THE LONDON AND EDINBURGH PHILOSOPHICAL MAGAZINE AND JOURNAL OF SCIENCE.

CONDUCTED BY

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AND

RICHARD PHILLIPS, F.R.S. L. & E. F.G.S. &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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ALERE FLAMMAM

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ADDENDA.

P. 320, last line, *for* 30·41 *read* 29·69

P. 397. It should have been stated that the notice on the "Desiccation of Silver," from the Phil. Trans., was extracted from a paper by Dr. Turner.

THE

LONDON AND EDINBURGH

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

JANUARY 1834.

I. *Notice of* Mr. Walker's *Communication on the Direction of the Mountain Chains of Europe and Asia. By the Rev.* W. D. Conybeare, *M.A., F.R.S., &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN your last Number my attention was attracted by a communication of Mr. A. Walker, in which he notices what he believes to be the prevailing tendency of the higher portions of our continents to their western boundaries, ranging north and south, with a steep westerly escarpment. This he ascribes to the effect of the rotation of the globe; and he states that he believes that these facts have never before "been brought into connexion, and that he is not aware that any one has assigned a cause for such remarkable coincidences." Now in order to show him that he has been completely forestalled in every point, I have only to refer him to our excellent old topographer Stukeley, in his *Itinerarium Curiosum*. This most worthy example of the Jonathan Oldbuck tribe did not merely confine himself to the antiquities of the country he traversed, but extended his observations to all the interesting natural phænomena which it presented. He collected many contributions towards the project, which Lister had before proposed, of a regular geological map (as we should call it) of the soils of the island *, and especially no-

* See Dr. Fitton's Notes on the History of English Geology, in Lond. and Edinb. Phil. Mag. vol. i. p. 153.—Edit.

ticed the course of our chalk hills and the subjacent sandy districts; and with regard to the present question, I have only to quote pages 3 and 4 of the first volume of the work referred to.

"If we cast our eyes on the geography of England, we must observe that much of the eastern shore is flat low ground, while the western is steep and rocky. This holds *generally* true throughout the globe as to its great parts, countries or islands, and likewise *particularly* as to its little ones, mountains and plains. I mean that mountains are abrupt to the west, especially north-west, and have a gentle declivity eastward or to the south-east.... Those that have gone about to demonstrate the problem of the earth's motion have neglected this proof, which is most sensible, and lies before our eyes every minute; for it is the property of matter, that when whirled round on an axis, the loose parts are thrown the contrary way in a tangent line.... By this means the elevated parts of the globe, as they consolidated, while yet soft and yielding, flew westwards, and spread forth a long declivity to the east." Thus far our antiquarian topographer, whose explanation, however, I have taken the liberty to abridge a little, by omitting a few of the repetitions which he judges necessary to elucidate such a subject; but I trust that what I have retained is amply sufficient to evince the exact identity of his views with those of Mr. Walker. Stukeley, in the progress of his journey, instances a great variety of examples of the local configuration of the chains which he traverses, always, as is undoubtedly true with regard to England, in favour of his own hypothesis.

Many geologists have at a later period speculated on these phænomena in a similar spirit. Bergman in 1773 remarked, that in mountains running north and south, the western declivity was most steep; and that in those running east and west, the southern face was most abrupt; and he enters into a large induction of the chains of the globe to confirm these statements. Buffon, in 1778, also insists that in continents, generally the most rapid declivity is to the west: and subsequently the subject was treated of by Herman, De la Metherie, Foster, and more especially by Kirwan, who collected the observations of the preceding geologists, and added many of his own. The inductive survey of the mountain chains of the globe by the latter is particularly copious. Kirwan, from his own peculiar views, refers the westerly elevated escarpments to the course of the tidal waves of the ocean, beneath which the strata were deposited, from east to west, as a consequence of the earth's rotation; and the similar abruptness which he likewise believed to prevail in the southern escarpments of such chains as run east and west, he ascribed to a tendency of the tidal currents towards vast abysses which he fancied to exist in the southern hemisphere. As the substance of Kirwan's Essay is given at much length in the *Encyclopædia Britannica*, (Article GEOLOGY,) I am surprised that a theory detailed in so popular a work should have escaped Mr. Walker's notice.

Having shown that such speculations were familiar at a much earlier period of geology, it can only be necessary for me to explain the reasons why they have been generally neglected, in the more advanced state of the science, as founded on a hasty and often erroneous induction.

The circumstance which invariably determines the position of the more abrupt escarpments of hilly chains, is really the direction of the great anticlinal lines of elevation. All the lateral chains have necessarily—to borrow expressive terms, commonly understood, from the art of fortification,—an abrupt counterscarp facing towards the central anticlinal ridge, and the gentle slope of a glacis, as it were, declining outwards. If, therefore, the anticlinal ridge range north and south, the lateral chains on the eastern side will have their steepest escarpment towards the west, and, *vice versâ*, the western lateral chains will have it towards the east; or if the anticlinal ridge should range east and west, the steep escarpments will face respectively to the south in the northern lateral chains, and to the north in the southern chains. This being the case, it is easy to find examples enough of steep escarpments in any direction to fill up a sufficient induction in favour of any hypothesis. Stukeley, we see, favoured the north and west; Kirwan the south and west. Both were equally confident, and I believe both equally well or rather ill founded. Thus, it is certainly true that the general configuration of America, of India, of Great Britain and of Norway, favours the idea of the greatest line of elevation being towards the west; but if we look at the great chain of the Ural, we see the Asiatic valley of the Tobol and Oby far nearer to its base than the European valley of the Volga. It is clear also that the chains skirting the north-east of Asia, and extending to the borders of the Chinese empire, in which the Lena and Yenisee have their source, are most abrupt towards the east, where along the sea of Ohkotsk they nearly line the coast. Little as we know of the interior of Africa, enough has been ascertained to show that there the principal elevations certainly do not tend towards the west; and the same remark may be made concerning the vast continent, rather than island, of Australia. Thus much for geology; and if Mr. Walker will turn to the German works of Adelung or Klaproth, or, among our own authors, to Dr. Prichard's Physical History of Man,—the most admirable view yet offered to the public of everything that is known concerning the history and migrations of the various races of our species,—I apprehend that he will find his opinions on these subjects very materially altered.

I remain, &c., your old Correspondent,

W. D. Conybeare.

Addition to the Notice of Mr. Walker's Communication on the Direction of the Mountain Chains of Europe and Asia.

Mr. Walker, admitting that, *primâ facie*, the extension of the old continent of Europe and Asia appears to be contrary to his theory, namely, from east to west, is desirous, by taking in Australia, to *correct* its longitudinal bearings to north and south; but had he studied Humboldt's admirable Essay on the Asiatic Mountain Chains, he would have seen demonstrative proof that the bearing of all those chains, individually and collectively, is undoubtedly from east to west; indeed, I myself feel convinced that we may safely generalize still further, and assert that one grand central range of elevations pervades both Asia and Europe,—for the mountain groups of the latter seem clearly, if viewed in a general light, to be prolongations of that grand line of elevation which traverses the former, and has its culminating point in the colossal Himmaleh, deserving, far more than any other mountain chain, the imposing title of the Girdle of the world.

Humboldt, on the authority of Chinese documents collected by M. Klaproth, traces the continuity of the Himmalayan chain from the Chinese coast on the east,—where, he observes, the island Formosa, elevated more than 12,469 feet, appears to be a prolongation;—thence the chain proceeds westwards along the confines of Fuhkien, Keangsi and Hokwang, and having penetrated the province of Yun-nan, quits China for the northern part of Ava, circles the sources of the Brahmaputra, bounds Assam to the north, and attaining its greatest height, incloses the sources of the Ganges and Indus; on the north of this point it inosculates with the Kwan-lun, a mountain chain traversing Thibet, and proceeds westwards by the Hindu Kho, and by the prolongations of the latter may be traced skirting the whole southern coast of the Caspian Sea. There is here, I believe, some interruption of continuity; but we must surely look on the subject in a more general point of view, and regard the general arrangement of nearly contiguous groups as sufficient evidence of the prevailing direction of grand lines of elevation without requiring the absolute continuity of a single unbroken chain. In this immediate neighbourhood the range of the Caucasus is parallel, and probably a lateral dependency on the great Himmalayan line, of which, however, the ranges of the Taurus, and the general mountain country of Asia Minor, present the most direct prolongation; and as these are separated only by a narrow strait from the great ridge of the Balkan (on which we may consider the Grecian highlands as dependencies), we thus trace the system into Europe. It here becomes, indeed, more broken and flexuous; but it is still impossible to cast an eye on any accurate map without being struck by the mutual relation and dependence of the several chains.

Crossing the Danube, the Eastern Carpathians of Transylvania take up the line, which is here considerably curved, but resumes its western direction in the Western Carpathians skirting Hungary; and this is prolonged through the Riesengebirge and Erzegebirge. In circling the basin of Bohemia, another great flexure occurs; indeed, we can compare the whole configuration of this district only to those vast crateriform depressions, surrounded by a mountainous border, which the telescopic appearance of the moon presents; still, largely viewed, we must undoubtedly consider the whole of this circular mountain zone as a single system of elevations. This system, by its south-eastern branch the Bohemer Wald, approximates close to the Alps; and although, on the west, the valley of the Rhone separates these latter summits by a broad interval from those of the Cevennes, still the general relief, as I may call it, of the whole surface of France, forbids us to consider these groups as more than distinct summits of a common system of elevation. The Cevennes in like manner conduct us to the Pyrenees; and the whole of Spain can be considered as little else than a general mass of highlands, divided into distinct groups (all of which have a tendency to range east and west,) by the valleys of the principal rivers.

W. D. C.

II. *On the Power of Glass of Antimony to reflect Light.* By R. Potter, *Jun., Esq.**

IN my paper on Comparative Photometry, which I read before the Physical Section of the British Association for the Advancement of Science at the Meeting at Oxford, and which is published in the Philosophical Magazine and Journal of Science for Sept. 1832, I have given a table of the quantities of light reflected by diamond for several incidences. I have since repeated the experiments for a low incidence on another larger specimen, a triangular larke diamond, which Sir David Brewster politely lent me for the purpose. These experiments indicate that this diamond also reflects at 2° to 3° incidence equally with crown glass at 64°, from which the various trials differed very little.

The small surfaces which we have on diamond cannot be expected to be sufficiently plane to enable us to get results for high incidences on which we may place entire confidence, and the smallness of the surfaces, also, is an obstacle, even if they were truly plane, to our obtaining quantities sufficiently exact for determining the curve which these reflections form, when we take the sine of incidence for the abscissa in rectangular coordinates.

These considerations induced me to make another attempt (having failed in a former one,) to give a perfect polish to glass of antimony, which has a refractive power but little inferior to that of diamond.

I used in this instance, for polishing powder, the fine prepared oxide of iron which I have described the method of procuring in a paper on the polishing, &c., of specula in the Edinburgh Journal of Science. By using this powder on a polishing bed of soft pitch, I succeeded completely; and as I had previously got the surfaces plane on a white German hone, which had been very carefully prepared for making the small specula of the Newtonian telescope, I had now a high refracting substance, of which I could determine the reflective power for any requisite incidences. The deep colour which glass of antimony possesses is also an advantage for these experiments, where we want only the reflection at the first surface.

I have made two sets of trials, which are given in two Tables below. Those in the second Table were obtained on a rather more favourable morning for using the photometer than the others. The incidences at which crown glass gave illuminations equal to those at the required incidences for glass of an

* Communicated by the Author.

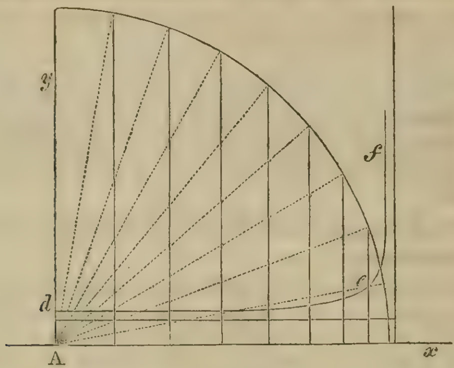
timony, I noted down (as usual) only to degrees and tenths of degrees; and where minutes are set down in the Tables otherwise than such even tenths, it has arisen from averages of different trials.

Incidence on Glass of Antimony.	Incidence at which Crown Glass gives an equal Reflection.	Glass of Antimony reflects of every 100 Rays incident,
10°	61° 48′	8·58
20	64 18	9·65
30	65 15	10·13
40	66 12	10·66
50	67 54	11·76
60	69 30	13·00
70	74 54	19·61
80	81 0	36·16
85	85 0	56·20
3	59 58	7·95
10	61 10	8·35
20		
30	63 48	9·41
40	64 54	9·94
50	66 27	10·81
60	69 24	12·92
70	74 18	18·63
80	80 45	35·17
85	85 6	56·77

Having now a regular series of reflections, most probably equally accurate with any which could be obtained by the infinitely more tedious and laborious method of lamps, we may examine the curve which those reflections indicate, and find the equation which will give the intensity of the reflection for glass of antimony at any required incidence.

Accordingly, taking the abscissa x of rectangular coordinates equal to the sine of incidence to radius (r) as 100, which is the number of rays we suppose incident, and the ordinate y, equal to the intensity of the reflection at that incidence, we have the formula I formerly discovered for glass, as follows, namely, $y = a + \frac{c^2}{r+b-x}$, which is the equation of a rectangular hyperbola, becoming similar to $d\,c\,f$ in the figure, by adopting for the constants a, b, c these values, $a = 7{\cdot}4$,

$b = 1{\cdot}25$, and $c = 9$. We thus obtain the intensities in the second column of the next Table; and comparing them with



those determined by experiment, we find as close an agreement through a long range of incidences as could be expected by those who have any idea of photometrical experiments.

Angle of Incidence.	Reflected of every 100 Rays incident, according to the Formula.
0	8·20
10	8·36
20	8·60
30	8·98
40	9·59
50	10·68
60	12·93
70	18·52
80	36·65
85	57·07
90	72·20

We find here again also the palpable inadequacy of the formulæ deduced from the undulatory theory of light, according to which glass of antimony should have reflected, at a perpendicular incidence, a quantity of light more than half as much again as what it really does, namely, 13·33 rays of every 100 rays incident. The polarizing angle for this specimen of glass of antimony appears to be about 65°, so that we may take the refractive index to be somewhere between 2·1 and 2·2 for rays of mean refrangibility. Such rays are however absorbed in refraction in specimens of ordinary thickness, from the tendency of the substance to transmit only the rays of the red end of the spectrum.

III. *Remarks on a recent Statement by* Berzelius *respecting the Use of Chemical Formulæ. By the Rev.* W. Whewell, *M.A., F.R.S., Fellow and Tutor of Trinity College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

As the subject of chemical nomenclature has been already discussed in your pages, perhaps your readers may be willing to see the view taken of the present state of the question by one of the persons whose opinion will naturally be most attended to on this matter.

In Berzelius's "Annual Report on the Progress of the Physical Sciences," read before the Swedish Academy on March 31, 1832, the following passage occurs:

"Some opinions concerning the chemical formulæ have been published in English Journals. The learned of that nation, little acquainted with foreign languages, do not acquire till late any knowledge of the progress of science in other countries, and always find excuses in defence of this slowness, among which not unfrequently occurs this, that the thing in question is *foreign*, and must be either extremely important, or must have become somewhat old, in order to obtain a more general notice. In the Annual Report for 1824, I have mentioned the objections which have been made against the chemical and mineralogical formulæ introduced by me: these have been again brought forward by Whewell, who urges principally that these formulæ are like algebraical ones, without being constructed according to algebraical rules,—on which account they must be disagreeable to any one who knows a small matter of algebra; and that they are also not a simple representation of the results of analysis, but affect to exhibit certain determinate modes of combination. Now he, it appears, has discovered a system of chemical signs which has not the same fault." I shall not trouble your readers with the comparison which he gives, and to which he adds, "*tot capita, tot sensus!*". "Whewell concludes his dissertation," he continues, "with an admonition to the English chemists 'to purify and improve the *foreign* system.'...Prideaux has made some objections against Whewell's remarks*, and rightly endeavoured to show, that by means of a formula an idea of the composition of a body is more easily expressed."

I am by no means going to attempt to open up again this subject, of which a general discussion, independent of some particular object, is not likely to be very useful or instructive. I will only observe, that in the note to the Report of 1824, to which he refers, Berzelius has not, I think, even attempted to answer the objections which he quotes in the above passage. In that note he endeavours to show the convenience of *some* chemical formulæ, without attempting to compare different systems. And both there and elsewhere he views his formulæ merely as modes of expressing his own opinion of certain compositions, briefly and clearly. I consider that chemical formulæ are capable of doing more than this,—of expressing the analysis, without adopting any one's hypothesis of the mode of composition; and of showing how different analyses, and different views of composition, are necessarily related to each other. And this can only be done by using algebraical formulæ constructed according to algebraical rules†. For Berzelius's purpose, the use of the sign + is a wanton and superfluous violation of analogy.

Those chemical formulæ are the best which best answer their purpose; and therefore our judgement of what is best must depend on our views with regard to the purpose which these formulæ are to answer. It is easy to make formulæ simple enough, if we want them to mean little. Berzelius compares his formula for garnet, $fs+As$, which does not express the quantity of oxygen combined with the bases, and which does not admit of being put into a form independent of his assumptions, with a formula which was intended to possess these advantages. A person who was contented to express somewhat less than Berzelius, might have "discovered" a simpler formula still, and might have denoted garnet by a single letter *g*. But such simplicity would probably not be considered as a merit of a high order.

Believe me, Gentlemen, &c. &c.

W. Whewell.

Trinity College, Cambridge,
Nov. 21, 1833.

[* See Phil. Mag. and Annals, N.S., vol. x. pp. 104 and 405 note.—Edit.]

† Dr. Turner, in the last edition of his Chemistry, has been led by this very consideration "to employ chemical symbols in strict accordance with the rules of algebra." Preface, p. vii.;—and in pages 399, 403, 409, of the same work may be seen examples of the use of such a mode of employing symbols.

IV. *On the Reduction of* Mr. Faraday's *Discoveries in Magneto-Electric Induction to a general Law. By the Rev.* William Ritchie, *LL.D., F.R.S., Professor of Natural and Experimental Philosophy in the Royal Institution of Great Britain, and in the University of London.*

To Sir David Brewster.

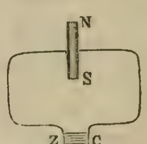
My Dear Sir,

IN a lecture which I had the honour of delivering before the British Association at the Oxford Meeting, I first pointed out, and illustrated by experiment, a *general law*, which connects together the splendid discoveries made by Mr. Faraday on the subject of magneto-electricity. As one of the most essential parts of science is the reduction of facts to general laws, and as the views which I then developed have only been published in the Abstracts of the Royal Society*, I have ventured to request you to give them a place in your widely extended Journal. I am, my dear Sir, yours truly,

Wm. Ritchie.

The law is founded on the universal principle that action and reaction are equal. Thus, if voltaic electricity induce magnetism under certain arrangements, magnetism will, by similar arrangements, react on a conductor and induce voltaic electricity. Instead of stating the law of magneto-electric induction and then illustrating it by experiment, it will be more instructive to arrive at it by a process of induction.

1. If a piece of soft iron, N S, be made to *approach* rapidly a voltaic conductor, and at right angles to its direction, the soft iron will be converted into a magnet having its poles developed at N S, according to the direction of the voltaic influence. If the battery, Z C, be removed, and the ends of the wire connected with the zinc and copper plates brought into metallic contact, and if the piece of soft iron be again *converted* into a magnet by means of a permanent horse-shoe magnet, the wire will have the *same* voltaic state induced on it as it had when connected with the battery.



If a temporary magnet be merely a piece of soft iron having the electricity essentially belonging to it arranged in a particular direction, it is obvious that the *motion* of the atoms of the electric fluid will take place in the *opposite* direction when the iron is ceasing to be a magnet or returning to its natural state. Hence the direction of the voltaic influence induced

* See Lond. and Edinb. Phil. Mag., vol. iii. p. 37.—Edit.

on the conductor will, as Mr. Faraday has shown, take place in the opposite direction.

2. If two conductors, having electricity induced on them in the *same direction* by means of an elementary battery, be placed parallel to one another, they will be mutually *attracted.* Hence, if we remove one of the batteries and connect the ends of the wires as in the first experiment, and then imitate *attraction*, by making the wires rapidly approach each other in a parallel position, the *same* electric influence will be induced on the closed circuit as it had when connected with the battery. If the conductors, when connected with the batteries, have their electricities induced in *opposite* directions, they will mutually repel each other. Hence, if we remove one of the batteries, connect the wires as before, and *imitate repulsion*, by *removing* the wires rapidly from each other, the electric influence or current will be induced in the *same* direction as it was when the wires were connected with the battery.

3. If a voltaic conductor be made to revolve round the pole of a magnet, as in Mr. Faraday's first experiment on rotation, and if the battery be removed and the ends of the conductors brought into metallic contact, the *same electric state* will be induced on the closed circuit, by turning the wire rapidly round the pole of the magnet by mechanical force.

4. If we produce all the rotations described in works on electro-magnetism, and if we remove the battery, and bring the ends of the conductors into metallic contact, and then *continue* the rotation by *mechanical means*, the *same* electric state will be induced on the conductor which it had when connected with the battery.

In a paper of mine read before the Royal Society on the 21st of March 1833*, I first described the method of making a piece of soft iron or electro-magnet, either straight or in the form of a horse-shoe, revolve *rapidly* round its centre, either by the action of the earth or of a horse-shoe magnet, by *changing* its poles *twice* in every revolution. Hence from the general law it follows, that if the soft iron be made to revolve by *mechanical force*, the *same* electric state will be induced on the conductor as it had when the ends of the wires were connected with the battery.

These facts were known, and the experiments publicly exhibited, months before the large revolving electro-magnet was exhibited in the Adelaide Rooms. The general law at which we have arrived may be thus expressed: "*If a wire conducting voltaic electricity, produce by its action on magnets or conductors certain motions, as attractions, repulsions, or continued*

* See Lond. and Edinb. Phil. Mag., vol. iii. p. 145.—Edit.

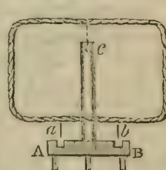
rotation; and if the battery be removed, the ends of the wires brought into metallic contact, and the same motions be produced by mechanical means, the conductor will have the same electric state induced on it, as it had when connected with the battery."

V. *On the continued Rotation of a closed Voltaic Circuit, by another closed Circuit. By the Rev.* William Ritchie, *LL.D., F.R.S., Professor of Natural and Experimental Philosophy in the Royal Institution of Great Britain, and in the University of London**.

M. AMPÈRE has demonstrated that when a *closed* circuit (or a conductor of voltaic electricity returning into itself so as to form a complete circuit,) is acted upon by another closed circuit, there is a determinate position in which *stable* equilibrium takes place. Hence the impossibility of producing continued rotation by the mutual action of two closed voltaic circuits. Hence also the impossibility of producing continued rotation by the mutual action of two *permanent* magnets†. But though continued rotation cannot be produced by the action of closed circuits, when the voltaic influence is exerted in a particular direction, I have succeeded in producing such rotation by changing the direction of the voltaic influence; a short account of which may not be unacceptable to the readers of the Philosophical Magazine.

The description of the method will be easiest understood by reference to the annexed figure.

Let A B represent the section of a circular piece of wood, having a groove measuring about an inch in its inner diameter, and half an inch broad, for the purpose of holding mercury. The groove is divided into two compartments by small slips of wood fixed diametrically opposite to one another. These compartments may be connected by means of wires with the plates of an elementary battery.



A glass rod, having a small cup, *c*, at the top, is cemented into the centre of the sole of the apparatus. A fine copper wire, covered with silk, is formed into a rectangular coil or closed circuit, as in the figure, the ends of which, *a b*, dip into the mercury contained in the compartments. The lower horizontal branch of the rectangle has the wires separated so

* Communicated by the Author.

† This property was not known a few years ago, and hence an ingenious Scotch shoemaker contrived to gull the most eminent philosophers in Scotland, by a pretended perpetual motion, alleged to be produced by the mutual action of magnets.

as to form an opening for the glass rod to pass through it, in order that the rectangle may hang perpendicularly. A similar rectangular closed conductor is supposed to be connected with the poles of another battery.

If the last conductor be placed above that represented in the figure, but not parallel to it, as in the case of stable equilibrium, the moveable conductor will turn round till that position be gained. But the moveable conductor being put in motion will pass this position at the moment the ends of the wires, *a b*, pass above the two divisions in the groove, so that the direction of the voltaic influence is changed, and the moveable conductor forced round another semicircle, when the direction of the influence is again changed; and so on, producing continued rotation. By using a magnet instead of the closed conductor, a more powerful and rapid rotation may be produced. By placing the divisions in the magnetic meridian, the closed conductor may be made to revolve by the action of the earth.

This experiment, if I mistake not, will afford an interesting illustration of the mutual action of voltaic conductors, and of the striking analogy between a permanent magnet and a closed circuit conducting voltaic electricity.

VI. *On the* Membrana versicolor *of the Eyes of Animals, in reply to* Sir D. Brewster. *By* G. H. Fielding, *Esq., M.R.C.S., Curator of Comparative Anatomy to the Hull Literary and Philosophical Society.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN your Number for October, I observe a communication from Sir David Brewster, which requires a few remarks from me by way of reply. It is entitled, "Notice respecting certain Changes of Colour in the Choroid Coat of the Eye." The principal object of the paper seems to be to claim for Dr. Drummond of Belfast the merit of being the first to notice the curious fact (which I mentioned in the Number of your Magazine for August last, as a discovery of my own,) of the disappearance by drying, and reproduction by wetting with water, of the brilliant colours of the choroid membrane of certain animals.

I was certainly not aware of the fact, as I had not seen the article "Optics," of the Edinburgh Encyclopædia, nor ever heard of the experiment alluded to. The claim, however, is indisputable, and I with pleasure cede the palm of priority to Dr. Drummond.

My experiments, however, would seem to be fuller and more complete than the original as mentioned in the Encyclopædia, as I had injected the choroides minutely, dissected it out, and washed it carefully to remove the pigmentum. This, however, is of no moment, excepting as regards the object I had in view.

If Sir David Brewster has happened to see the Essay I published last year, on the subject of a new membrane in the eye, he will find that my object has been to prove that the anatomy of the choroid membrane in animals has not been hitherto thoroughly understood; that the brilliant colours we meet with in the eyes of various animals, which we have been accustomed to consider as appertaining to a peculiar secretion of the choroid tunic, and which, it may be easily seen, are so stated to be in standard anatomical and physiological works* even to this day,—that these brilliant colours are not owing to any such secretion, but arise from the peculiar construction of the anterior lamina of the choroid membrane, which I have ventured to term the *Membrana versicolor.*

That the membrana versicolor is perfectly distinct from the choroides is, I think, satisfactorily shown by direct experiment, and perhaps particularly so by the one above mentioned, in which the eye of the sheep was minutely injected.

In conclusion, I may remark that this curious fact, as regards the experiment first named, does not invariably hold good. We find the colours disappear by drying, and reappear by moistening, in the sheep, ox, &c., but it will not do so, completely, in the cat, and probably not in the fox or the lion. The reason of this is the much greater thickness of the membrane in these animals.

I hope shortly to have some remarks on Vision to offer.

I am, Gentlemen, yours, &c.

Hull, Oct. 14, 1833. GEO. H. FIELDING.

VII. *Experimental Researches regarding certain Vibrations which take place between Metallic Masses having different Temperatures. By* JAMES D. FORBES, *Esq., F.R.SS. L. & Ed. Professor of Natural Philosophy in the University of Edinburgh*†.

1. ON the 17th of January 1831, Mr. Arthur Trevelyan communicated to the Royal Society of Edinburgh a

* As Bell, Fyfe, Bostock, Cuvier, Hunter, &c.

† From the Transactions of the Royal Society of Edinburgh, vol. xii. This paper was read before the Society on the 18th of March and 1st of April 1833.

paper, entitled "Notice regarding some Phænomena observed during the Cooling of certain Metals placed in contact with Lead." This was the first account published of the remarkable discovery made by that gentleman, of a most curious class of phænomena, which till then was unknown to the scientific world. This paper was afterwards published, with some additions, in the 12th volume of the Transactions of that body, under the title of "Notice regarding some Observations on the Vibrations of Heated Metals*."

2. Mr. Trevelyan had, in February 1829, first observed the phænomena just alluded to, which consist in certain tremulous motions accompanied by sounds, often highly musical, excited in many metals while hot, placed in contact with lead or tin, at a lower temperature. The method of rendering these conspicuous will be understood from fig. 1, where A represents

Fig. 1.

a block of lead, and B a bar of some other metal, such as brass or copper, which is made of such a form as to vibrate readily upon two points of support, formed by the solid angles of a ridge left on its lower surface, which is bevelled away on either side. The narrower this ridge, of course the more easily the equilibrium is disturbed. No sooner is the bar of copper, iron, or other hard metal, placed upon the lead-block, the former being heated to a moderate temperature, than visible vibrations commence, the bar oscillating upon its horizontal axis. Musical notes are not always produced, but generally under the circumstances shortly to be noticed. Soon after Mr. Trevelyan's communication to the Royal Society of Edinburgh, the subject was taken up by Professor Leslie and Mr. Faraday, both of whom explained the vibrations upon recognised principles, and did not conceive that any new mode of action was concerned in their production. Doubts which I ventured to entertain as to the conclusions of these eminent individuals, led me to investigate the subject experimentally, by which these doubts were strongly confirmed. Facts increased in number, and I was forced to abandon several suc-

* See also Lond. and Edinb. Phil. Mag., vol. iii. p. 321.

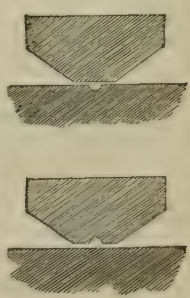
cessive hypotheses. The real difficulty of the subject, and the singular conclusions deducible from several of my experiments, led me to delay putting together the facts which I had accumulated, into the form of a paper. Nearly two years having now elapsed since the commencement of my experiments (which were almost all made in the summer of 1831), and no one else having taken up the investigation, I have resolved to publish the conclusions at which I have arrived, though such as are purely theoretical I offer with all the diffidence which a speculation connected with some of the most unexplained processes in natural science requires.

3. I propose to divide this paper into three sections,—first, on the Phænomena of Sound, as those which earliest presented themselves, and the consideration of which will pave the way to further inquiries; second, on the Phænomena of Vibration; and, third, on the Theory of these Phænomena.

I. *Phænomena of Sound.*

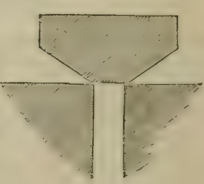
4. Musical sounds do not always accompany the vibrations above described. There is one condition or modification of the apparatus which generally ensures their production. If a groove be made either in the bar or block, as shown in the sections, fig. 2, in the direction of the axis of the bar, and separating the points of contact with the block, upon which the bar oscillates, we shall rarely fail in producing the sound. These sounds generally commence with a deep base note, which rises as the experiment proceeds, and as the equilibrium of temperature of the two metals approaches; sometimes rising suddenly an octave in the most fitful manner, and occasionally redescending. Mr. Trevelyan, in his paper just alluded to, has treated of the sounds thus produced, and seems to consider the phænomena introduced by the condiion of the groove, of an essentially different class from the others. He assumes that the only effect of the groove is, that it may allow a current of heated air to pass through it; yet he admits that this current is not sufficient *alone* to produce musical notes, because no such occur when vibrations do not take place; nor yet, according to him, do the vibrations suffice, since they require the supposed current of air introduced by the groove to complete the effect. In his enumeration of the sources of musical notes, Mr. Trevelyan has omitted to

Fig. 2.



state that the mere accumulation of impulses of any kind up to a certain number in a second, produces alone a musical sound depending upon that number. This Mr. Faraday readily showed to be the true cause of the musical sounds produced in this experiment, namely, the number of contacts of the hot with the cold metal in a second, and he illustrated the fact by putting a cold bar of metal in vibration by means of the corresponding expansions and contractions of a pair of sugar-tongs connected with it, when sounds quite analogous to those just alluded to were produced*. So far Mr. Faraday completely established the dependence of these sounds upon the vibrations. He did not, however, include in his explanation the action of the groove in producing them, which therefore became an object of my attention. To show that there was no action such as the impulsion of air through an orifice, which seemed to have been contemplated by Mr. Trevelyan, I caused a hot bar to vibrate upon two pieces of lead, the two striking parts or solid angles of the bar impingeing upon *different* masses of lead, at a distance of about a quarter of an inch, fig. 3; the effect was precisely similar to that of a groove cut of the same breadth in a single piece of lead, and of the smallest possible depth. In order to show that the motion of air had nothing what ever to do even with the tone, I allowed the instrument to acquire a steady note with the two masses of lead just described, and then carefully closed the edges of the space between the masses with adhesive paste, yet not the slightest change of note was perceptible: this was several times repeated with the same results. We must therefore, in its fullest extent, deny the influence of any imaginary current of air in the production of the sound.

Fig. 3.



5. What, then, is the influence of the groove? The answer is simple, and easily proved by experiment, merely that the rapidity of the vibrations is in some way increased by its presence. When both surfaces are smooth, the vibrations are comparatively slow, and might almost be counted: sometimes, with the form of bar I have used, they do not exceed twenty in a second. In fact, the phænomena essentially depend upon the form of apparatus employed. Mr. Trevelyan practically found the influence of throwing the mass of matter to the sides of the bar, so as to render its equilibrium more unstable, and

* Lecture at the Royal Institution April, 1831, and Royal Institution Journal, No. IV. N.S.

thus lengthening the period of its vibrations; and Mr. Faraday well observed, that the pressure of the finger upon the upper part of the bar, while in a state of vibration, by shortening the period of oscillation, raised the note to a great extent, and rendered the sounds musical, when before they were not so. In the bars which I have generally employed, the back was hollowed out, as shown in the section fig. 4, which served both to diminish the quantity of matter near the axis, and to contain a portion of mercury, the bright surface of which I found a convenient test of the existence of oscillations too minute to be readily perceived by the eye, as was generally the case when the sounds were musical. They could always be detected likewise by approaching the point of the finger gently to the bar. This will even detect vibrations which produce no visible tremor in a clear globule of mercury.

Fig. 4.

6. I trust I have now shown that all the phænomena of sound are ultimately resolvable into those of simple vibration. There are one or two facts connected with, and indicated by, the musical tone, which we shall more particularly consider in treating of the modification of the vibrations. These are especially the sudden changes in the note of the instrument, which generally *rises* as the experiment proceeds, and the influence of the groove in raising the note, that is, rendering the vibrations more frequent.

7. We have only a remark to make upon the *absolute* velocity of these oscillations, which the sound produced affords the only accurate way of estimating. With one of the ordinary bars, such as I have been in the habit of using, the highest note which I have distinctly observed and compared, was A above the middle C of the pianoforte, which corresponds to 430 vibrations in a second. From this velocity (and it is often very much greater,) the tone descends through all the lower notes down to the smallest number of vibrations producing a musical note, even to about 20 in a second.

8. From what has been now said, we are prepared to maintain that the phænomena of sound are all referable to phænomena of vibration, and we must seek an explanation of the modifications observable in the former, in those of the latter.

II. *Phænomena of Vibration.*

9. When a rocking bar, such as has been above described, is put in motion upon a block of metal, the temperatures being equal, the vibrations gradually diminish in extent, and by the simple action of gravity are very speedily annihilated.

That this may not be the case, as we find it is not under some circumstances already alluded to, namely, where certain different metals, and at different temperatures, are employed, we must admit the existence of some impulse which prolongs the time during which the oscillations are kept up. This impulse can only be received during the successive contacts at the two bearing points of heterogeneous metals, and may safely be assumed to depend in some way or other upon the propagation of heat, since the effect does not take place unless the temperatures be different, nor is it indifferent which of the two kinds of metal has the highest temperature. The impulse, of whatever kind it be, resembles that derived by a pendulum from the pallets of a timekeeper, which in fact is the *sustaining power* of the mechanism.

10. The arcs of vibration of course depend, other things being equal, upon the intensity of the impulse communicated to the bar.

11. We have already noticed, that various circumstances tend to modify materially the character of the vibrations, particularly as to their distinctness and duration: a little practice is required to distinguish between the mere mechanical oscillations of a bar once made to vibrate, and a true vibration depending on an impulse received at each successive contact. There is, in general, this difference, even where the sustaining power is very feeble, that whereas the oscillations arising merely from gravity rapidly diminish from the very first, if there be a true sustaining power, they will rather increase in energy and distinctness for some time, from the accumulated effect of successive though small impulses.

12. We proceed to examine the influence of contingent circumstances upon the intensity of these vibrations.

1. *Relation to the Specific Characters of the Substances employed.*

13. The first general law of these vibrations may be considered to be, that *they never take place between substances of the same nature*. This is probably quite general. An exception is noticed by Mr. Trevelyan, who thinks that he observed a vibration of a copper bar upon a copper block; I am inclined to think that he had mistaken the oscillations connected with the simple law of gravity for a true vibration, as I have in vain endeavoured to repeat the experiment.

14. The second general law is, that *both substances must be metallic*. I have never seen a single exception to this law*;

* I need hardly state it as an exception that I have used *galena* (the sulphuret of lead) as a block, with success, instead of metallic lead.

Mr. Trevelyan, however, thinks that he observed a vibration upon glass. It is a remarkable fact, substantiated by experiments which will presently be mentioned, that *all* metals do not possess the property alluded to. It was natural to divide the metals into two classes, one of which might form the heated bar, the other the cold block; and it was also natural to suppose that metals excluded from the one class belonged to the other. I have, however, discovered, that at least two metals are perfectly inert, in either situation, namely, antimony and bismuth.

15. At an early period I found (as Mr. Trevelyan also did independently,) that the position of the bar and block were convertible; I mean, that the metal commonly used for the block might be employed for the bar, and *vice versâ*, provided always, that the same metal was always hotter or colder (as experience showed to be necessary,) than the other. Thus, Mr. Trevelyan announced that a bar of *hot* iron or copper placed on a block of *cold* lead or tin, produced vibrations; but we shall still have the same phænomena, provided we use a *cold bar* of lead or tin placed upon a *hot block* of iron or copper.

16. Mr. Trevelyan having observed that lead and tin were the metals which required to be *cold*, and that metals which he designates as " hard", such as iron and copper, must be *hot*, he naturally draws a division between two classes of metals quite distinct, each of which require certain conditions to produce the vibrations. Mr. Faraday having taken up the subject, found that hot silver vibrated on cold iron, a fact observed by silversmiths, thus forming a link between the classes, and showing that a metal which requires to be *cold* relatively to a second metal, must be *hot* relatively to a third. Some theoretical views, which we shall presently advert to, and to which experience did not seem to be opposed, led Mr. Faraday to the conclusion that the arrangement of the metals with regard to their power of vibrating with one another, was *directly* as their conducting power for heat, and *inversely* as their expansibility. The metal standing highest on the scale of metals thus formed, being necessarily the *hot* one relatively to the other, which stood lower on the same scale. These observations of Mr. Faraday were given in a lecture at the Royal Institution in April 1831, and were published in the Journal edited there.

17. Mr. Faraday having pointed out the arrangement of metals alluded to as a theoretical result, though confirmed in some points by experiment, I conceived that the only true way of arriving at an explanation of the phænomena, would be to classify the metals by experiment in the order of their

intensity and distinctness of vibrating power. In this inquiry I found many difficulties, chiefly arising from the apparently capricious nature of the effects produced, which for a long time seemed almost to baffle an attempt at classification; and it was only by reiterated series of experiments, at different times, and made in different ways, that I could satisfy myself of the degree of accuracy to which my results were entitled.

18. I soon found that the conditions of vibration depended simply on the distance between the places of the two metals employed for the bar or block, in a certain scale required to be determined. The remarkable case of iron already observed by Mr. Faraday made me very desirous to extend such a law; here we have a metal which must be placed towards the middle of such a scale, since a metal above it in the scale vibrates upon it when hotter than the iron, whilst iron itself vibrates upon cold lead, which, therefore, must be placed lower in the scale; and as the intensity of vibration may be expected to be proportional to the interval in the scale, so we actually find that silver vibrates on lead much more actively and steadily than iron does.

19. I first prepared bars similar to those which have been already described, of copper, zinc, brass, iron, tin, lead, antimony and bismuth. My earliest experiments demonstrated the small number of pairs of metals between which vibrations took place. The superiority of lead to all other metals, as the cold substance, was manifest; and in order to establish, in relation to it, the intensity of vibration of the different heated metals, it was necessary to obtain some ready means of employing them all at a fixed temperature. Without entering minutely into the actual difference of temperature between the two metals requisite for producing the effect, it was sufficient to discover that the heat of boiling water answered every practical purpose; it was therefore resorted to. The temperature of the lead being 65°, we conclude that a difference of 150° between the metals is sufficient to produce the effect in the most decided manner.

Block at 65°.	Bars at 212°.
LEAD.	Zinc; vibrates briskly and steadily.
	Brass; nearly the same as zinc, but not quite so steady.
	Iron; decidedly less than brass.
	Tin; does not vibrate so well as iron, but the difference is inconsiderable.
	Antimony; not at all.
	Bismuth; not at all.

20. In pursuing these experiments, I varied them in every way I could devise, but almost always got precisely the same arrangement of vibrators. Employing a lead *bar*, I used also blocks of the different metals heated to 212°. With silver, gold and platinum, I found it difficult to procure considerable masses of sufficient purity; and when small ones taken out of boiling water were employed and placed in a vice, I found that the loss of heat they experienced was so rapid as to destroy the comparability of the experiment. The plan I adopted for procuring an approximation to a uniform temperature, and which from reiterated trials I found susceptible of great accuracy, was the following: The piece of metal under experiment being firmly held in a vice, a drop of water was placed upon it, and a spirit-lamp applied below, so as to heat at once the metal and the vice, until the water was rapidly dissipated in the act of ebullition, at which instant the cold bar of lead was placed upon it, and the vibrations encouraged by a gentle oscillation. In this manner I went over not only the metals which could not be conveniently tried in the other way, but also those of which I already had bars.

21. Pursuing the relations of the metals to COLD LEAD, I found, in the first place, that the position of platinum is not very different from that of tin. The mass used weighed about 7 ounces, and was kindly lent to me by Dr. Hope; its form prevented its being used as a bar, and its small thickness and angular corners did not fit it for retaining a high temperature, or performing well the part of a block. When held in a vice and heated by a spirit-lamp, the vibrations of a cold lead bar were very active. From some experiments made with iron, tin and platinum, at a temperature of 350°, I conceived that they stood as vibrators in the order just named: I had not then, however, fallen upon a method of operating upon small masses with accuracy; and subsequent experiments, often repeated, with a small thick mass lent me by Professor Jameson, and heated till water boiled on the surface (as above explained) led me to the conclusion, that, in the other mode of experimenting, platinum had been placed too low, as might have been anticipated, and that it is at least equal to iron in vibrating power. It appears at the same time that there is little difference between platinum, iron and tin.

22. From numerous experiments with *antimony* and *bismuth*, these metals when heated appeared to have no vibratory action with cold lead. This experiment was tried at a great range of temperatures, and, notwithstanding the low melting point of bismuth, I raised it by a particular arrangement to a temperature of 350° without obtaining any vibration.

23. The next experiments were made with *silver*. Even with very unfavourable apparatus it appeared remarkably active as a vibrator. A small block of heated silver placed in a hot vice, gave indications of being at the very top of the series of vibrators, a cold lead bar being used; a result fully confirmed by subsequent experiments, in which the temperature of 212° was carefully ensured.

24. My first observations upon *gold* led me to the conclusion, that it is much inferior to silver in the scale of vibrators. Subsequent experiments led me to place it third in the scale. Standard gold was employed.

25. *Zinc* vibrates with great facility and certainty upon lead, when it has a temperature of 212°; when hotter, it is subject to some irregularities. Zinc and *brass*, to which it is closely allied, seem to stand next to gold, being very superior to platinum and iron, but considerably inferior to silver.

26. *Iron* is very nearly allied to platinum, but, from very careful experiments, is, we have seen, to be placed somewhat lower. *Tin* stands decidedly below iron, and stands in the scale next to lead, the *cold* metal employed; yet still its vibrations are very sensible.

27. From accidental circumstances, *copper* was one of the last metals I tried; when I had not the means of experimenting accurately upon silver, I had no hesitation in placing copper at the very top of the scale, so steady, forcible and sustained were its vibrations. I finally placed it below silver, but the difference is not great. Besides the direct mode of observing the intensity of vibration on lead, I had an indirect way of confirming the results which will immediately be noticed. The arrangement of metals in relation to their intensity of vibration with lead, determined by a great series of experiments, of which I have now given the principal results, becomes the following:

Standard Silver (best).
Copper.
Standard Gold.
Zinc.
Brass (nearly the same as zinc).
Platinum.
Iron.
Tin.
Antimony; does not vibrate.
Bismuth; ditto.

28. This arrangement indicates very distinctly the order in which the metals possess the property or properties essential to vibration with regard to lead. But a very important inquiry

is immediately suggested: Is the property of lead as the cold metal peculiar to it? or does it only require a certain space between any two metals in this scale to produce the effect? For example, lead being placed in the arrangement between tin and antimony, platinum is the third metal above it; the question is, Would platinum used as the cold metal bear the same relation to gold, the third metal above it, as lead does to platinum? This is the principle, though of course we are not bound to suppose that the energy is proportional to the number of metals interposed in the list; since we have already seen that the vibrating property in relation to cold lead is almost the same in several consecutive metals. The observation stated by Mr. Faraday, forcibly suggests the idea that a certain interval in the scale of metals is alone required to produce the effect; the lowest metal being necessarily the coolest.

29. The following are some cases of decided vibrations obtained among numerous experiments. Upon COLD TIN, heated *silver*, *copper*, *gold* and *iron* vibrate in the order just stated, silver being the most intense. Upon COLD IRON, *silver* vibrates decisively: my experiments, therefore, confirm the statements of Mr. Faraday. With hot *copper* I have likewise obtained distinct vibrations, though it is not possible at all times to repeat the experiment in a satisfactory manner. With hot *gold* the vibrations were dubious. Upon COLD ZINC, no hot metal except *silver* has been observed to vibrate. In this case, however, the effect is decided. I have in no case observed that copper, silver, or gold, have any action when cold upon other metals heated.

30. It is well worthy of notice, that, from multiplied experiments with ANTIMONY and BISMUTH, and at a great range of temperature, I have been led to the conclusion, that among all the metals under trial none have any vibrating energy with antimony and bismuth, whether hotter or colder than these metals, whether used as bars or blocks. There is only a single experiment in my note-book which offers any exception. On one occasion very hot *brass* was observed to vibrate on COLD ANTIMONY; on another occasion, however, no such effect was produced. This solitary experiment appears to be one of those anomalies which have frequently exercised my patience, and retarded my progress in this delicate inquiry.

31. These experiments, it will be observed, aid us in giving a definitive arrangement to the metals in the Table before given, and which, in fact, have been resorted to in order to determine with accuracy the position of metals, of which the vibrating power with regard to lead might be somewhat doubtful. This was particularly the case with some of the

metals highest on the scale: An example will illustrate the mode of operation.

32. 30th July 1831.—Experiments made with masses of silver, copper and gold, placed in a vice, and heated with a spirit-lamp until a globule of water evaporated in violent ebullition from its surface. The cold metals were employed as bars.

Hot Silver.

Cold Lead. Vibrates perfectly well.
—— Tin. Apparently as well as Lead.
—— Zinc. Vibrates very well.
—— Iron. Vibrates distinctly; but apparently less than Zinc.
—— Brass. Not decisively.
—— Copper. Not at all.

Hot Copper.

Cold Lead. Vibrates very well.
—— Tin. The same.
—— Zinc. Not at all; even where the temperature of the Copper is raised much above 212°.
—— Iron. Vibrates very imperfectly. It appears, however, more active than Zinc. I have formerly observed a decisive vibration of Hot Copper upon Cold Iron (11th July).
—— Brass. No Vibration.

Hot Gold.

Cold Lead. Vibrates quite readily. Nearly as with Copper.
—— Tin. Nearly as with Copper.
—— Zinc. Ditto.
—— Iron. No distinct vibration, but with considerable heat of the lamp approaches to it; it is rather more disposed to vibrate than Zinc.

By experiments analogous to these, we may see how the position of any metal is fixed by a variety of tests, which afford mutual confirmation.

33. In the course of forming the classification of metals, I was naturally led to compare it with Mr. Faraday's hypothesis, that the vibration between two metals depended on the difference of their conducting powers for heat directly, and of their expansion inversely. Finding considerable deviations from this law, I was led to look for some simpler analogy.

34. The first arrangement which presented a striking similarity was that of the conducting powers of the metals for electricity. The further examination to which this remark

led me, pointed out another, and perhaps not less important, analogy, which appeared not to have been before observed, namely, that when the best data are collected, the order of conducting powers of the metals for *heat* and for *electricity* is the same. I did not adopt this conclusion till after a mature examination of existing statements, and an extensive series of experiments upon the conducting powers of the metals for *heat*, made with Fourier's thermometer of contact, which enabled me, where discrepances occurred between previous observers, to ascertain the truth, and to add some new metals to the list. In the case of electricity, I was a good deal surprised to find observers more at one, than in that of heat. The result of these inquiries, which for a time withdrew my attention from the immediate subject under consideration, is contained in a paper read to the Royal Society of Edinburgh on the 7th of January 1833*.

35. The general conclusion at which I then arrived is thus stated in the paper alluded to: *That the arrangement of metallic conductors of heat does not differ more from that of those of electricity than either arrangement does alone under the hands of different observers.* I shall here quote the *provisional* arrangements which I have given in that paper, and compare them with the order of vibrations which we have recorded above.

Conductors of Heat.	Conductors of Electricity†.	Vibrators.
Gold,	Silver,	Silver,
Silver,	Copper,	Copper,
Copper,	Gold,	Gold,
Brass,	Zinc,	Zinc,
Iron,	Brass,	Brass,
Zinc,	Iron,	Platinum,
Platinum,	Platinum,	Iron,
Tin,	Tin,	Tin,
Lead,	Lead,	Lead,
Antimony,	Antimony,	Antimony,
Bismuth,	Bismuth,	Bismuth.

* The analogy to which I allude was observed by me in autumn 1831; and the experiments described in the paper just quoted were made between that period and February 1832.

† On the subject of the conducting powers for electricity, a beautiful illustration of the application of new discoveries in science to branches already known, has occurred to me since forming these lists. Mr. Faraday has shown, that, according to his beautiful Theory of Magnetism by rotation, the Transient Magnetic Energy (as it was formerly termed) of different metals, should bear a relation to their conducting power of metals for electricity. This is most remarkably confirmed by the following Table, given by Mr. Harris in the Philosophical Transactions for 1831, which most

The analogy is exceedingly striking, more especially when it is stated that each arrangement has been compiled alone solely upon the evidence of experiment, and the ground upon which each metal has its place assigned to it, is fully stated in the paper alluded to for the two first columns, and in the present paper for the third. It is also worthy of remark, that the same metals seem to be most allied to one another in each of the three series. The various observers agree in treating of their respective subjects, that gold, silver and copper are nearly allied in all: and it is probable that platinum and iron are in equally close connexion*. The observations on all three points are at one in proving that there is a decided breach of continuity between lead and antimony, so marked is the change of property of the two lowest metals in the list.

36. We may now venture to enunciate a third and most important law of these phænomena; That *the vibrations take place with an intensity proportional* (*within certain limits*) *to the difference of conducting power of the two metals employed for heat* (*or electricity*),—*the metal having the least conducting power being necessarily the coldest.* I have stated that the difference of conducting power must be *within certain limits*, because the anomaly of antimony and bismuth seems to be caused by this exception; and it is on the same account probably that the class of bodies which possess the vibrating property is confined to the metals; other matter being destitute of the requisite conducting power. Here antimony and bismuth almost want this characteristic property of the other metals examined. My experiments with the thermometer of contact prove their very low rank as conductors of heat, as Mr. Harris of Plymouth, in reporting to me some experiments which he had kindly undertaken at my request, with regard to their power as electrical conductors, states in regard to bismuth, that nothing in "the form of a metal can be much worse."

[To be continued.]

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strikingly confirms the arrangement of conductors which I have given in the text.

Transient Magnetic Energy of the Metals.

Rolled Silver	39
——— Copper	29
Cast Copper	20
Rolled Gold	16
Cast Zinc	10
—— Tin	6·9
—— Lead	3·7
—— Antimony	1·3
—— Bismuth	0·45

* It is most probably from the great *specific heat* of iron that it stands so high in the first column.

VIII. *A Catalogue of Comets. By the Rev.* T. J. Hussey, *A.M., Rector of Hayes, Kent.*

[Continued from vol. iii. p. 199.]

[The Chronology employed is that of Petau or Petavius.]

A, the comet of 1680. B, that of 1652. C (Halley's), that of 1682. D, that of 1759. E, that of 1661. F, that of 1677. G, that of 1556.

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
224	891	E	April, May.....	U.Maj.,Bootes	Chinese Records Annal. Sax.,&c.	
225	892	...	Apr. May, June	Scorpio	Ch. Rec., Chron. Andr.,Ch. Mall.	
226	——	...	November.....	Sagit., Capric.	Chinese Records.	
227	——	...	December	To the S.E.	Chinese Records.	
228	893	...	May	U.Maj.,Bootes	Chinese Records.	Seen 37 days.
229	894	...	February	Gemini.........	Chinese Records.	
230	904	...	December......	......	Constant. Porph. Leo.Bas. &c.&c.	
231	905	...	May, June.....	Gem.,Ur.Maj., Leo, Virgo...	Ch. Rec., Chron. Flor. &c. &c.	
232	906	...	......	......	Chron. de Mailros. Matt. West.	Seen nearly 6 months.
233	912	...	May, June.....	Hydra	Chin. Rec., Leo. Grammat. &c.	
234	912 or 913	...	......	......	Haly in Centil.	
235	923	...	October........	Cancer.........	Chinese Records.	
236	928	...	December	Capricornus ...	Chinese Records.	Seen during 3 nights.
237	930	C?	......	Cancer.........	Miz. Lubien. &c.	
238	936	...	September	Aquar., Pegas., Capricornus	Chinese Records.	
239	939	...	......	......	Luitprand.	
240	941	...	September.....	Hercules	Chin. Rec., Anon. Gnezn., &c.&c.	
241	942	...	October........	......	Chron. Andegav. Chron. Mall. &c.	
242	943	...	November	Virgo	Chinese Records, Chron. Sith.	
243	945	...	......	......	Frodoard. Chron. Con. Urs. Chr. &c.	
244	956	...	March	Orion...........	Chinese Records.	
245	959	...	Oct., Nov.	......	Constant. Porph. Sim. Log.	
246	975	G	Aug.,Sep.,Oct.	Hydra, Cancer, Pegasus	Chinese Records, Ced. Glyc., &c.	
247	981	...	Autumn........	......	Burkhardt, Hepi	dan.

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
248	985	...	......	......	Platin. Simonet.	
249	989	...	February	......	Chin. Rec. An. Sax. &c.	
250	—	...	August	Gemini, Virgo	Elmacin.Ch.Rec.	Elements com-
251	990	...	August	In the West...	Chin. Rec., Romuald., &c.	puted by Burkhardt*.
252	995	...	August	......	Ch.Rec. Florent. Vigorn. &c.	
253	998	...	February	Pegasus......	Chinese Records.	
254	1000	...	December	....	Balderic.&c.&c.	Seen during
255	1003	...	February	....	Ann. Hepidanni.	9 days.
256	—	} ?	November....	Can., Gemini, Taurus,Orion	Chinese Records.	Seen 30 days.
257	1004		Jan., Feb.......	......	Chron. Vet. Cell. Ursperg.&c.	
258	1005	...	Sept., Oct.	To the North	Chin.Rec. Alpert. Chro.Rem.&c.	
259	1006	C	April...........	Scorpio	Cardan. Mem. Hist.Palmer.&c.	
260	1010	...	......	......	Ann.Sax.Ch.Qu.	
261	1012	...	......	To the South	Ann. Hepidanni.	
262	1015	F ?	......	......	Protosp. Chroni.	
263	1017	...	......	......	Sigeb. Nangis. Balderic. &c.	Seen during 4 months.
264	1018	...	July, August...	Ursa Maj.,Leo, Cor. Hydræ	Ditmar. Chron. Qued.&c.C.Re.	Seen 37 days.
265	1023	...	Autumn........	Leo	Ademar.	
266	1024	...	......	......	Dugloss.Cureus.	
267	1033	...	March	......	Chi. Rec.Cedren. Helgald.&c.	
268	1034	...	September....	Hydra, Crater	Cedren. Glyc. Chin. Records.	
269	1035	...	September....	Hydra..........	Chinese Records.	Seen 12 days.
270	—	...	November.....	Pisces	Chinese Records.	
271	1041	...	......	......	Glycæ Annales.	
272	1042	...	October........	......	Cedrenus Glycas.	
273	1046	...	......	......	Godelli Chronic.	
274	1049	...	March..........	Aquar., Equul.	Chinese Records.	Seen 114 days.
275	1056	...	September	Orion,Cor.Hy.	Chinese Records.	
276	1058	...	......	......	Henn. Cromer. Dugloss. &c.	
277	1060	...	......	......	Will. Malm. Herman. &c.	

* Passage through the perihelion in mean time at Greenwich: September $11^{d}\,23^{h}\,50^{m}\,39^{s}$.—Long. of the perihel. on the orbit of the comet, $8^{s}\,24°\,0'\,0''$.—Long. of the ascending node, $2^{s}\,24°\,0'\,0''$.—Angle between the perihel. and the node, $6^{s}\,0°\,0'\,0''$.—Inclination of the orbit, 17°.—Perihel. distance, 0·568000.—Log. of the mean motion, 0·328200.—Direction R.

[To be continued.]

IX. *On the Comparative Strength of Salt Springs at different Depths.* *By* Charles G. B. Daubeny, *M.D., F.R.S., Professor of Chemistry in the University of Oxford.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

ON turning, the other day, over the pages of a work on the Rocks of Wirtemberg by Alberti, published at Stuttgard in the year 1826, I met with some statements relative to the comparative strength of the salt springs of that country at different depths, which I do not remember to have seen quoted in any English work. I am desirous, therefore, of giving them publicity, as tending in some degree to elucidate the problem with respect to the manner in which rocks of salt may have been formed from water holding it in solution, which has exercised the ingenuity of many naturalists, and especially of Mr. Lyell in the first volume of his Principles of Geology.

I say, it may tend in some degree to elucidate that problem, because, after all, it does not seem clear in what way a deposition of salt can take place from a strong or even a saturated brine, without the operation of some assisting cause, such as pressure, heat, or the like.

Nevertheless, it is something to be able to show the probability that at the bottom of the sea the brine may be in that state of concentration which is but one remove from the condition in which it would begin to deposit salt; and this will, perhaps, appear from the following statements.

Well of Salt Water at Friedrickshall, No. 1.

From the surface to the depth of

14 measures	4 ⅌ ct. of salt.	183 measures	4 ⅌ ct. of salt.
61 ———	3	184 ———	7½
71 ———	2½	186 ———	16
82 ———	2	189 ———	23¾
91 ———	1½	191 ———	24½
117 ———	½	195 ———	25
140 ———	1	196 ———	26
157 ———	1½	197 ———	27
180 ———	2¼		

From thence to 201 measures there was no increase of salt*.

* The measure employed was equivalent to 3¼ feet.

The same Well, after remaining 8 hours without being drawn upon:

16 measures	$2\frac{1}{2}$ ⅌ ct. of salt.	55 measures	$\frac{1}{2}$ ⅌ ct. of salt.
17 ———	$2\frac{1}{4}$	58 ———	$\frac{3}{4}$
18 ———	2	91 ———	1
19 ———	$1\frac{1}{2}$	92 ———	3
20 ———	1	93 ———	$7\frac{1}{4}$
21 ———	$\frac{3}{4}$	94 ———	13
22 ———	$\frac{1}{2}$	95 ———	19
29 ———	$\frac{1}{4}$	96 ———	22
47 ———	0	97 ———	23
48 ———	0	98 ———	26
49 ———	$\frac{1}{8}$	99 ———	27
54 ———	$\frac{1}{4}$		

Well, No. 3, after remaining 43 hours without being drawn off:

12 measures	$2\frac{3}{4}$ ⅌ ct. of salt.	165 measures	1 ⅌ ct. of salt.
13 ———	$2\frac{1}{2}$	166 ———	$1\frac{1}{4}$
14 ———	2	167 ———	$2\frac{1}{2}$
15 ———	$1\frac{1}{2}$	168 ———	$4\frac{1}{2}$
16 ———	1	169 ———	7
18 ———	$\frac{3}{4}$	170 ———	$9\frac{1}{2}$
19 ———	$\frac{1}{2}$	171 ———	$11\frac{3}{4}$
22 ———	$\frac{1}{4}$	172 ———	$15\frac{1}{4}$
24 ———	$\frac{1}{8}$	173 ———	16
26 ———	0	174 ———	$17\frac{1}{2}$
39 ———	$\frac{1}{10}$	176 ———	19
51 ———	$\frac{1}{8}$	178 ———	$19\frac{1}{4}$
56 ———	$\frac{1}{4}$	179 ———	$19\frac{1}{2}$
58 ———	$\frac{1}{2}$	180 ———	20
133 ———	$\frac{3}{4}$		

The same Well after remaining 6 days undisturbed:

16 measures	$4\frac{1}{2}$ ⅌ ct. of salt.	36 measures	$\frac{1}{10}$ ⅌ ct. of salt.
17 ———	$4\frac{1}{4}$	101 ———	$\frac{1}{8}$
20 ———	3	124 ———	$\frac{1}{2}$
21 ———	$2\frac{3}{4}$	125 ———	$\frac{3}{4}$
22 ———	$2\frac{1}{2}$	126 ———	1
24 ———	$2\frac{1}{4}$	127 ———	2
25 ———	2	128 ———	$\left\{\begin{array}{l}2\frac{1}{2}\\3\end{array}\right.$
26 ———	$1\frac{3}{4}$	129 ———	$\left\{\begin{array}{l}4\\5\frac{1}{2}\end{array}\right.$
27 ———	$1\frac{1}{2}$	130 ———	$\left\{\begin{array}{l}7\\8\frac{1}{2}\end{array}\right.$
28 ———	$1\frac{1}{4}$	131 ———	$\left\{\begin{array}{l}10\\11\frac{1}{4}\end{array}\right.$
29 ———	1		
30 ———	$\frac{3}{4}$		
31 ———	$\frac{1}{2}$		
33 ———	$\frac{1}{8}$		

132 measures	13½ ⅌ ct. salt. 14¾	136 measures	24½ ⅌ ct. salt. 25
133 ———	17 18	137 ———	25¼ 26
134 ———	20 21	138 ———	26¼ 26¾
135 ———	22½ 23	147 ———	27

The diminution of the quantity of salt in the spring *down* to a certain depth is a phænomenon even more difficult of explanation than its subsequent increase below this point; and it would seem very desirable to have similar observations made in the case of other salt springs, with a view of ascertaining whether the same law holds good generally; the total absence of saline impregnation at a certain depth seems, indeed, hardly credible in the present state of our knowledge on the subject. In the hope, therefore, of directing the attention of men of science to the question, I have been induced to communicate to you the above details; and now beg leave to subscribe myself, Gentlemen, yours, &c.

Oxford, Dec. 16, 1833. CHARLES DAUBENY.

X. *On the Brachystochronous Course of a Ship.* By the Rev. J. CHALLIS, *Fellow of the Cambridge Philosophical Society**.

THE following theoretical considerations respecting the most advantageous course of a ship are offered to the attention of mathematicians chiefly as presenting some peculiarities of analysis, the author not being aware that this one of the class of brachystochronous problems has been attempted in the manner here proposed.

I suppose the wind to be unfavourable, or the ship to be obliged to *tack*, as the contrary case needs no inquiry. The wind remaining of uniform intensity, we may assume the velocity of the ship to be given, when the direction of its course makes a given angle with the direction of the wind.

Let O (fig. 1.) be a fixed point, OW a fixed line parallel to the direction of the wind, which is supposed to blow from W towards O. Then if the two straight lines POP′, QOQ′, make supplementary angles, P′OW, Q′OW, with OW, the wind will have equal effect whether the ship sail in the direction POP′ or QOQ′. If, therefore, O be the origin of rectangular axes, OW the axis of x reckoned positively from O towards W, and $p =$ the tangent of the angle (θ), which the direction

* Communicated by the Author.

in which the ship is moving at any point of its course makes with the axis of x, the velocity will be some function of θ, and therefore of p, of such a nature that it will retain the same value when θ is changed to $\pi - \theta$, or, what is equivalent, when p is changed to $-p$. We may therefore say, that the velocity, or $\frac{ds}{dt} = f(p^2)$. Hence $dt = \frac{ds}{f(p^2)}$, and $t = \int \frac{\sqrt{1+p^2}\,dx}{f(p^2)}$. Referring now to the general equation for determining maximum and minimum functions according to the method of the calculus of variations, viz.

$$\mathrm{N} - \frac{d(\mathrm{P})}{dx} + \frac{d^2(\mathrm{Q})}{dx^2} - \&c. = 0,*$$

in the instance before us this reduces itself to $\frac{d(\mathrm{P})}{dx} = 0$, because p only is involved. Hence integrating $\mathrm{P} = \mathrm{C}$. The quantity P is the differential coefficient of $\frac{\sqrt{1+p^2}}{f(p^2)}$ with respect to p. Hence

$$\frac{p}{f(p^2)\sqrt{1+p^2}} - \frac{2pf'(p^2)\sqrt{1+p^2}}{(f(p^2))^2} = \mathrm{C}. \;\ldots\ldots\; (\mathrm{A})$$

This equation, as it contains only p, must belong to a system of straight lines; which proves that the brachystochronous course is not curved, but rectilinear. Let, therefore, the brachystochronous course from A to B (fig. 1.) be in a system of straight lines AE, EF, FG, GB. Then the brachystochronous course from A to F is in the *two* lines AE, EF. We might show that these two lines make supplementary angles with OW, by solving the following problem as a question in maxima and minima of two variables:—Supposing the course from one given point to another to be in two straight lines, and the velocity to be given when the angle which the direction of the motion makes with a fixed straight line is given, required the positions of the two lines. It would appear that the resulting equations are satisfied by supposing the two lines to make supplementary angles with the fixed line. But perhaps the following reasoning will be deemed sufficient. The case in which the wind is favourable, and blows directly from A towards B, is involved *analytically* in the preceding investigation, because to show that in this case the swiftest course is in the straight line joining A and B, requires calculation pre-

* See Airy's Tracts, 2nd Edit. p. 232.

cisely the same as that given above, and leading to the same equation (A). Hence this equation is satisfied by $p = 0$, and consequently $C = 0$. Hence the equation,

$$\frac{1}{f(p^2)\sqrt{1+p^2}} - \frac{2f'(p^2)\sqrt{1+p^2}}{(f(p^2))^2} = 0,$$

$$\text{or,} \quad f(p^2) - 2f'(p^2)(1+p^2) = 0, \ldots\ldots\ldots \text{(B)}$$

belongs to the straight lines which answer the condition of minimum we are seeking. It appears from the nature of the equation (B), that if any positive value of p satisfy it, an equal negative value corresponding to the supplementary angle will also satisfy it.

Let $f(p^2) = V$; then $f'(p^2) = \frac{1}{2p} \cdot \frac{dV}{dp}$. Substituting these values in (B), there results, $V p\, dp - dV(1+p^2) = 0$, which gives by integrating, $V^2 = C(1+p^2) = C. \frac{ds^2}{dx^2}$. Consequently $\frac{dx^2}{dt^2} = C$, showing that the resolved part of the velocity parallel to the direction of the wind is always the same. This will be the case if the direction of the ship's course always make a given angle, or the supplement of that angle, with the direction of the wind. In fact, if we take *any* point

Fig. 1.

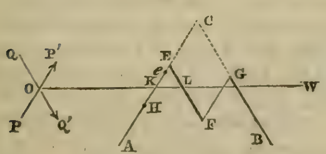
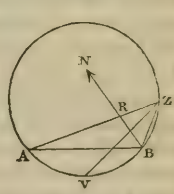


Fig. 2.



H in AE, the shortest course from H to F must be in HE, EF, otherwise AE, EF is not the shortest course from A to F. Therefore when the bearing of the two places from each other is altered, the angle which the ship's course makes with the direction of the wind remains the same. If the point e be very near to E, the shortest course from e to F is principally in EF, which differs but little from the straight line joining e and F. Hence the angle which EF makes with OW is that which is made by the ship's course with the direction of the wind, when it first begins to be of advantage to tack instead of proceeding in the direct course between the two places.

This angle, which can only be known by experiment, let us call α. As AE, EF is the shortest course from A to F, if the angle AKO be α, the angle ELW is $\pi-\alpha$. The same may be said of EF, FG, the shortest course from E to G, and of FG, GB, the shortest course from F to B. If AE, BG be produced to meet in C, FC is plainly a parallelogram, and the brachystochronous course from A to B is also AC, CB.

From all that precedes we may derive the following simple construction for the brachystochronous course, when the angle α is known. Let it be required to sail from A to B, (fig. 2.) in the shortest time possible, the wind blowing from B towards N. Upon AB describe a segment of a circle AZB, containing an angle equal to $\pi-2\alpha$. Complete the circle and bisect the arc AVB in V. Draw VZ perpendicular to BN and join AZ, ZB. Then AZB is the course required; for as AZB $= \pi-2\alpha$, each of the angles ZRB, ZBR is equal to α.

Papworth St. Everard, Dec. 13, 1833.

XI. *On the Properties of the* Dædaleum, *a new Instrument of Optical Illusion. By* W. G. Horner, *Esq.**

THE ingenious and amusing invention of M. Plateau, by superadding the resources of art to those of science, has rendered an instructive experiment exceedingly popular. But neither the Professor nor any of his imitators have added anything to the mathematical principles, which remain hitherto in exactly the position in which Mr. Faraday left them nearly three years ago, in the Journal of the Royal Institution. The repose of one portion of the spectrum, the residual motion apparent in the advance or retrogression of others, and the blending of variation of action with identity of subject, have been traced to their causes, both by Mr. Faraday and Dr. Roget, most satisfactorily; nor does it appear that any phænomenon observable in the relative motions of a wheel and a system of detached bars, or of a pair of perforated disks, has escaped the notice of one or other of those gentlemen. One set of phænomena, derived from a still more simple apparatus, has, however, been left unnoticed, as far as I can discover, by all; and my design in submitting the present paper to the readers of the Philosophical Magazine and Journal of Science is to familiarize both the principle and the exhibition of an experiment involving all the interesting illusions of the phantasmascope, but capable of being performed without a

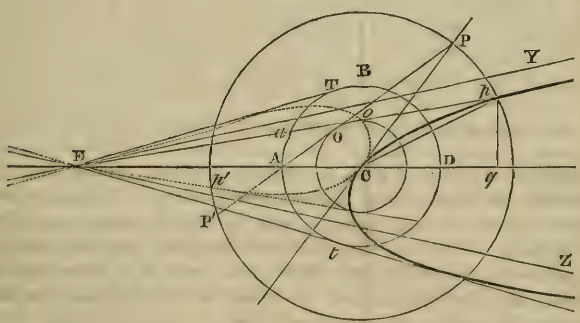
* Communicated by the Author.

mirror or any second instrument, and of being displayed to unlimited numbers at once.

The apparatus is merely a hollow cylinder, or a moderately high margin, with apertures at equal distances, and placed cylindrically round the edge of a revolving disk. Any drawings which are made on the interior surface in the intervals of the apertures will be visible through the opposite apertures, and if executed on the same principle of graduated action, will produce the same surprising play of relative motions as the common magic disk does when spun before a mirror. But as no necessity exists in this case for bringing the eye near the apparatus, but rather the contrary, and the machine when revolving has all the effect of transparency, the phænomenon may be displayed with full effect to a numerous audience. I have given this instrument the name of *Dædaleum*, as imitating the practice which the celebrated artist of antiquity was fabled to have invented, of creating figures of men and animals endued with motion. The peculiarities incident to this form of construction, some of which demand the careful attention of artists, will appear from the following investigation.

Let ABD in the annexed diagram (fig. 1.*) represent a cir-

Fig. 1.



cular section of the cylinder, C its centre, E the place of the observer's eye, A an aperture brought on the line AC. For the sake of a more general investigation, let any circle, Oo, Pp,

* It is necessary to remark here, that by a mistake of the engraver the circles in the above diagram are all of nearly equal strength, instead of the middle one ATD, representing the section of the cylinder, being the strongest, and the outer and inner ones both faint, as in Mr. Horner's drawing. —Edit.

be described from the same centre C, and either interior or exterior to the circle ABD.

From A draw AP to any point in this second circle, and from centre C describe a circle touching AP. From E draw E $a\,p$, touching the same circle, and meeting the second circle in p. Then it is manifest that $a\,p$ = AP, and that the object P, which to a spectator at A would be apparent in the situation P, can only be seen from E when it has arrived in the situation p, the aperture A having then arrived at a.

Let an indefinite line be drawn through B and C, and join p C. Put ϑ = PCD the angular distance of P from the point D diametrically opposite A; $\phi = p$ CD, the like distance of the projected point p; ψ = PAC, the angular distance of P from D as apparent at A. Also let a = AC, d = EC, x = CD, $y = p\,q$, coordinates; and u = PC = p C. Then it will be seen that $u = \frac{a \sin \psi}{\sin(\vartheta - \psi)}$, $x = u \cos \phi$, $y = u \sin \phi$; likewise that $\phi = \vartheta - \psi + \sin^{-1}\frac{a \sin \psi}{d}$. From these general premises we may deduce all the particulars we have occasion for.

(1.) Wherever the observer is placed, whether near the cylinder or remote from it, *all the objects* depicted on the inner surface of the cylinder are visible to him at one and the same time. This is a highly paradoxical phænomenon; but the explanation of it is involved in the deductions already made. For, if we assume P to be coincident with A, or make $\vartheta = \pi$ and $\psi = \frac{1}{2}\pi$, we have $\phi = \frac{\pi}{2} + \sin^{-1}\frac{a}{d}$, which is equivalent to the arc TBD, contained between the tangent ET and the diameter AD. Consequently the point T is the projected place of the point A, and TBD, the portion concave toward the eye, is the projection of the whole semicircle ABD. And so of the other semicircle AtD, which is projected on t D. But the thickness of the material will obstruct the view of a small portion adjacent to T and t.

(2.) Wherever the eye is placed, the divisions of the projected picture are sensibly equal in breadth, especially toward the central parts. For since $\sin^{-1}\frac{a \sin \psi}{d} = \frac{a\psi}{d} - \frac{a(d^2 - a^2)\psi^3}{6\,d^3}$ &c., if we neglect the second and following terms, which are evidently small, we have $\phi = \vartheta - \psi + \frac{a\psi}{d}$ exceedingly near;

where, ϑ remaining constant, ϕ will vary, differentially, as ψ varies. The greatest effect of the neglected portion occurs when $d = a\sqrt{3}$, which is therefore the worst situation for observing the phænomenon.

(3.) If the eye is brought *close* to the revolving cylinder, d becomes $= a \therefore \phi = \vartheta$, or each projected division occupies exactly the same breadth as each of the originals.

(4.) If the spectator removes to an *indefinite distance*, $\frac{a}{d}$ approaches 0 as its limit $\therefore \phi = \vartheta - \psi$ or $\pi + \vartheta - \psi$; and ϕ varies differentially as ϑ. Therefore the projected divisions will be exactly equal to each other. As these last conditions are those which a lecturer will require, artists should keep them especially in view. And since in this projection all the divisions round the *outer circle* are projected upon just a *semicircle*, all figures and objects depicted should be considerably *exaggerated in breadth*, nearly in the proportion of 2 to 1 when compared with the natural breadth.

(5.) Objects connected with the effect of those in the cylindrical border may be drawn in corresponding sectors on the disk itself. But the projections of these sectors will of course appear considerably curved if extended quite to the centre. The curves produced, however similar in appearance to those observed by Dr. Roget, are, notwithstanding, of quite a distinct class. A few of their properties it may be worth while to unfold.

(α.) If $\psi = \pm \frac{\pi}{2}$, $u = \mp \frac{a}{\cos \vartheta}$ gives the two points where the tangents ET, Et are likewise *tangents* to the curve.

(β.) If $\psi = \vartheta$ or $\pi + \vartheta$, $u = \infty$ while $\phi = \pm \sin^{-1} \frac{a \sin \vartheta}{d}$, indicating two *asymptotes*, which are tangents from E to the circle whose radius is $\sin \vartheta$.

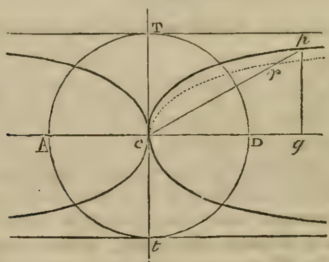
(γ.) If $\phi = 0$ or π, $\therefore u = -d$, $x = -d$, and $y = 0$, indicating a *nodus* at E.

The entire curve has then four branches of the hyperbolic species, two of which have double curvature, touching given lines, and then returning to become asymptotes to lines interior to those.

When E is at an infinite distance the curves are of a more simple kind, and the family to which they belong becomes apparent. The most simple of all is when $\vartheta = \pm \pi$, giving $x = \frac{a \sin \psi^2}{\cos \psi}$, or $a \sin \psi \tan \psi$ and $y = a \sin \psi$. Eliminating the circular parts, this gives $x^2 = \frac{y^4}{a^2 - y^2}$, which bears a striking analogy to the equation to the common cissoid. In fact, if we assume $x^2 = \frac{y^{n+2}}{a^n - y^n}$ as the type of the cissoid family, that of Diocles is of the first order, and this of the second. The general polar equation will be $u = \frac{a \cos \phi^{\frac{2}{n}}}{\sin \phi}$

Hence the immediate relation between the first and second cissoids is apparent. For, since $u_1 = \frac{a \cos \phi^2}{\sin \phi}$ and $u_2 = \frac{a \cos \phi}{\sin \phi}$, we have $u_1 = u_2 \cos \phi$; or the *radius vector* (from the vertex) of the cissoid of Diocles is equal to the corresponding *abscissa* of the optical cissoid. (See fig. 2.)

Fig. 2.



A relation not very dissimilar exists between the latter and the Apollonian parabola, whose radius vector (always taking the vertex for the pole) is $U = \frac{a \cos \phi}{\sin \phi^2}$; whence $u_2 = U \sin \phi$; or, the *radius vector* of the second cissoid is equal to the corresponding *ordinate* of the parabola.

If we assume $x^m - \frac{y^{m+n}}{a^n \mp y^n} \therefore u_n = \frac{a \cos \phi^{\frac{m}{n}}}{\sin \phi \, (\sin \phi^m \pm \cos \phi^m)^{\frac{1}{n}}}$, for the more general type of the cissoids, as $x^m = \frac{y^{m+n}}{a^n}$ $\therefore U_n = \frac{a \cos \phi^{\frac{m}{n}}}{\sin \phi^{\frac{m}{n}+1}}$ is of the parabolas, it becomes apparent that the latter form a genus intermediate to the two genera into which the former resolve themselves. This relation is comparable to that which the common parabola bears to the ellipse and hyperbola, whose *vertical* equations are

$u = \frac{t c^2 \cos \phi}{t^2 \sin \phi^2 \pm c^2 \cos \phi^2}$. But these remarks are beyond the purpose of the present occasion. I shall conclude with observing, that the equation $u_2 = a \cot \phi$ suggests a much readier process for outlining the second cissoid than that which was indicated by its optical development.

P.S. I have not thought it requisite to give a more particular description of the instrument, having communicated every needful part of the detail, some weeks ago, to a respectable optician of Bristol, Mr. King, jun. The publication seems, however, to have met with some impediment, probably in the sketching of the figures.

XII. *Remarks on Mr.* Phillips's *Observations on the Use of Chemical Symbols. By Mr.* John Prideaux, *Member of the Plymouth Institution.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE observations of Mr. Phillips will, it is to be hoped, have their effect; if not that so much desired by him, yet in inducing chemists to conform as nearly as possible to the symbols already extensively known and employed. Perfection is as unattainable in a system of symbols, as in any other; and although improvement may be desirable here, as in other cases, one should be well convinced, before attempting innovations, that their effects and inconveniences will be less than those of what they are proposed to supersede.

In the case in point, however, the confusion is really not so great as it appears.

There is but one known combination of phosphorus with 5 atoms of oxygen; in which many chemists (chiefly in this country) apprehend the phosphorus to enter as a single atom, whilst others regard it as double, from its requiring 2 atoms of alkali. The first class will write the P simple; the others will bisect it: and as this difference of opinion is generally known, the 5 atoms of oxygen, whether represented by dots or initials, immediately distinguish the symbol as that of phosphoric acid.

With hydrogen, the case is different: $\dot{H}$ may signify, according to our atomistic notions, either pure or oxygenated water; and considering the great number of compounds into which water enters in definite proportions, it were to be wished that Berzelius had adhered to his original decision of writing it Aq, whereby this ambiguity was avoided. No practical difficulty, however, is likely to result from the change, until oxygenated water becomes a subject of more extensive chemical interest.

If this were all, the interposition or omission of the sign + and the mode of expression of oxygen, would lead to little embarrassment in the use of chemical symbols, (amongst which the mathematical notation of Whewell and Brande can hardly be classed); but in the specimen given (at p. 445), there is confusion of another kind, originating, apparently, in the specimen itself. The common ingredient oxygen, is represented in very different proportions.

Berzelius and Graham are made to give it;

	In the Soda.	Acid.	Water.
	as 2	5	24
Rose and Johnstone ...	1	5	24
Turner	1	$2\frac{1}{2}$	24

which, if ever written by chemists of such reputation, could only have been by accident or inadvertence. Rose and Johnstone would doubtless place 2 before the symbol of soda, and Turner would write 12 Aq, when they would clearly correspond with Berzelius. Warrington's, *as there printed*, appears to signify quadroxide of potassium, soda, and 24 water.

My name has no claim to a system of symbols, I having always advocated those of Berzelius, and only modified a few of them for a specific purpose, where they are accompanied by an explanation. But nothing there resembles the quoted specimen. Crystallized phosphate of soda stands on my scale $\dot{S}o^2\,\underline{\dddot{P}h}\,Aq^{24}$.

Thus corrected, or rather restored,

Rose.................... $2NaO+PO^5+24\,\underline{H}O$.

Turner.................. $\dot{S}o+P+2\frac{1}{2}O+12\,aq$.

Johnstone .... $\dddot{P}+\dot{S}o+24\,\dot{H}$.

My scale .... $\dot{S}o^2\,\underline{\dddot{P}h}\,Aq^{24}$, they all harmonize with

Berzelius .... $\dot{N}a^2\,\underline{\dddot{P}}+24\,\underline{\dot{H}}$, and are a common language of science, applicable to all nations, and easily acquired.

Mr. Graham's symbol, $\dot{N}a^2\,H\dddot{P}$ (the original subject of Mr. Phillips's observations), shows that phosphate of soda, deprived of its water of crystallization, still retains an atom of combined water.

And this concise expression

$$\text{Phosphate } \overset{\cdot}{\mathrm{Na}}^2\,\overset{\cdot}{\mathrm{H}}\,\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}}$$

$$\text{Pyrophosphate } \overset{\cdot}{\mathrm{Na}}^3\,\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}}$$

will convey to the French, German and Italian, as well as to the English chemist, what Mr. Graham has found to be the effect of a red heat in modifying the properties of phosphoric acid.

I hope Mr. Phillips may, in time, be induced to revoke his unsparing sentence; and am, yours, &c.

Plymouth, Dec. 3, 1833. JOHN PRIDEAUX.

XIII. *Observations on the Vision of the Retina.* By Mrs. MARY GRIFFITHS, *New York.**

HAVING for several years been attentive to the movements of the eye, and of the action of light on the optic nerve, I was led to investigate several very curious phænomena connected with it. I thought with others that the retina was assuredly the seat of vision,—that from that part the image of all objects was conveyed to the sensory; but I have now satisfactorily proved that the retina is *not* the seat of vision. I place this proof before you, and it is the first intimation on record that the subject is capable of being brought to actual experiment in the living eye.

On lying in bed one morning later than usual, quite awake, *but not yet having opened my eyes*, the servant opened the shutters very suddenly, and a bright glare of light fell directly on my eyelids. To my great surprise, I saw, very distinctly, the whole of the retina! This extraordinary spectacle remained visible to my mind for a few seconds, showing the changes of light and shade which are always perceptible when objects are thrown on the *open* eyes, having a strong light for the back ground,—a phænomenon frequently noticed by all writers on optics. At one moment the meshes of the network were of a dull brick-dust colour, and the spaces between were of a pale dingy yellow; and in the next moment the case was reversed, the meshes or intersections were of this pale dingy yellow, and the spaces or interstices were of a dull brick colour.

My surprise was so great at first that I could do no more than mark the outline and general appearance of the spectrum, or retina. I had, likewise, the fear that it was an illusion—that it might be the window-sash itself which had been thus con-

* Communicated by the Author.

veyed to my sight; but I soon recollected that my eyes were shut, that the lids had not once been opened, and that my face was directed to the ceiling; the squares, too, of the spectrum were placed diamond-wise and were of a different colour from that of the window-sash so often seen by me. I could not make the object return that morning, although I had the room made dark and closed my eyes again.

I inferred from this that during sleep the whole apparatus of the eye is relaxed, and that there is a suspension of all effort, consequently the fluids are not so abundantly present. While in this state, the retina is taken by surprise, as it were, and so becomes *visible* to the mind. If the lids had been open and the senses on the alert before the shutters were opened, the small portion of light always perceptible in a room not particularly closed against it, would be sufficient to stimulate the nerves and keep the retina in a state of tension ready for use.

Subsequent and repeated experiments by myself and others have not only reduced this fact to a certainty, but they have elicited others, all confirming the opinion that the retina is *not* the seat of vision. In several instances I discovered that the retina had the power to contract and expand, for the squares of the meshes were at times wider apart than usual; and when thus expanded the lines or intersections were thinner and paler. The meshes were generally the fifth of an inch asunder, varying with the health of the body, the quantity of light thrown on the closed lids, and the period of the day when the closed eye was exposed to the glare of light.

When the eyes are covered in a dark room after dinner, and we lose ourselves for a few moments in sleep, and the shutters are suddenly opened, *before we open our eyes or rub them*, then a different thing takes place. We shall not see the intersections or threads of the retina, but the squares or interstices between them, which have the appearance, first of a dark, and then light colour. In the centre of each square, while the colour is light yellow, a bright star will be often seen, about the size of the head of a large pin: as soon as the light yellow colour changes to black, the star disappears; but when the light colour returns, the star is again seen, or rather the place where the star stood is now occupied by a faint light. When the stars are first seen, they twinkle or scintillate; and finally, if the glare of light has been strong, on covering the eyes suddenly with the hands, the whole vanishes in wavy lines, which are certainly the undulations of the meshes of the retina, for we can easily infer that the retina can be raised or depressed. Throughout the whole appearance the *place* of the pupil is open and distinct, sometimes contracting and sometimes dilating, but always *contrary to the contraction and dilation of the retina itself.*

Does not this fact prove that the meshes of the retina must be of an elastic nature? Perhaps its use in some way is to keep the pupil, or the opening of the pupil, of a certain dimension. Nothing can be more beautiful than the appearance of the spectrum thus seen when the eyes are closed; the diamond beetle is not more brilliant: but I have observed that two things are requisite to our seeing this phænomenon in perfection;—one is a sudden and broad glare of light, and the other is the *rest* which all eyes have by sleep and closed lids. Even after a doze in the afternoon, and with the precaution of keeping the eyes shut before the light is thrown on the closed lids, the appearance is not so brilliant if the eyes have been rubbed or the lids opened: practice, however, will soon enable others to see all the variations which take place.

The wavy lines of which I speak are only seen under particular circumstances,—when the eyes are a little depressed by the hands in the afternoon, when the light is first thrown on them. The peculiar wavy appearance certainly arises from the irregular compression of the meshes or intersections of the retina. When these meshes are first seen, they are composed of straight lines forming squares, and these squares are narrowed off, as anything of a reticular or net-work-like form would be, or would have the appearance of being, to the eye, if seen painted on a vaulted or concave surface. The unnatural pressure of the eyeballs by the fingers forces the net-work out of its regularity, and two sides of the square bulge out and form a curve, the convexity being outward: the other two sides are wavy, being bent in two regular curves, thus elongating the squares.

This must now confirm the opinion that *the seat of vision does not exist in the eye at all*; that the eye does no more than modify and transmit to the sensory the light which emanates from the surface of all bodies; and that no image, *while the eye is living*, can be found on the hinder part of the eye. The retina, too, *from its peculiar shape*, being square, would not seem to be suited to the purpose of conveying rays of light of different density even if it was not now certain that we can see it; and as to the appearance of an inverted image on the hinder part of the eye when dead, that of itself would be conclusive against the supposition that the retina is the seat of vision, for the camera obscura, and several other optical instruments, show the image inverted likewise, and there is nothing in these glasses of the nature or character of the retina. If we approach a concave or convex mirror in a certain direction, our image is seen inverted on the margin of the glass, but on going nearer to the centre, the figure is right again.

One thing, however, is now brought to a decision. If *something else* sees the retina, then that *something else* must convey the impression of outward objects to the mind or sensory. My opinion is, that as the whole apparatus of the eye is merely to modulate and convey light, there is no occasion for any intermediate agent to form a regular image to assist the mind in its conception or apprehension of outward forms and qualities. All that the mind can ever know of outward objects and their qualities is from the rays of light themselves, which by their density—meaning by density their various lengths, the different lengths answering to different colours—act upon the nerves of vision far away from the eye itself.

Independently of the certainty, which the above facts bring to the mind, that the retina is not the seat of vision, I have elsewhere proved that the vitreous humour is not the place where images are formed, for by a reference to page 306 of a work, entitled "Neighbourhood," it will there be found that we can see part of the interior of our own eye. At the time that was written I fully believed that the retina was the seat of vision, and I made that belief the base of my deductions from that curious phænomenon; I could not perceive then that any other part of the eye was adapted to the purpose of conveying the appearance of bodies to the mind, for the use of the lens was fully understood; and as to the choroides, that part could not for one moment be supposed the seat of vision, as the office of all dark colouring matter is simply to decompose light.

MARY GRIFFITHS.

Observations by the Editor.

Although some of the conclusions in the preceding paper, and especially those about the seat of vision, are not correct, yet as we believe, on the authority of direct experiment, that most of the phænomena described may be distinctly seen, we have thought it right to lay the experiment before our readers, and establish Mrs. Griffiths's claim as the first observer of a fact so curious.

In repeating the experiments under different modifications, we have observed several curious facts, which will be communicated in a subsequent paper. D. B.

XIV. *Reviews, and Notices respecting New Books.*

Abstracts of the Papers printed in the Philosophical Transactions *of the Royal Society of London, from* 1800 *to* 1830 *inclusive. Printed, by order of the President and Council, from the Journal Book of the Society.* London 1832, 2 vols. 4to and 8vo: vol. i. pp. 516; vol. ii. pp. 448.

THE Philosophical Transactions contain the details of a series of discoveries and other developments of scientific truth, which, if not superior to those made public in any similar collection produced in foreign countries, especially in the results of pure induction, is unquestionably equal to any, and of much greater value than most. They comprise the records of nearly all the more important mathematical and physical investigations which have been made by the philosophers of Britain, from the æra of Boyle and Wren, through that of Newton, Flamsteed and Halley, down to the æra just passed of the first Herschel, Wollaston, and Davy.

In the Report of the Council of the Royal Society for 1832, given in our Magazine for May last, (Lond. and Edinb. Phil. Mag. vol. ii. p. 374,) we are informed that the Council had directed the printing of an edition of the Abstracts made by the Secretaries, and entered on the Journal Book of the Society, of such papers as had been read to the Society and ordered for publication in the Philosophical Transactions, from the year 1800 to the present time. The motive for the intended publication is stated to be the opinion of the Council that a collection of these Abstracts, which, it is observed, possess in themselves much intrinsic value, would form a useful sequel to the Abridgment of the Philosophical Transactions, (by Dr. Hutton, Dr. Shaw and Dr. Mason Good,) of which the public are already in possession, but which, it is added, does not extend to a later period than the end of the last century.

Agreeing entirely with the Council of the Royal Society in our opinion of the value of the Abstracts, and of the propriety of their publication, we conceive that they have performed an important and acceptable service to the Society, and to all who are engaged in the cultivation or promotion of science, by the production of the volumes now before us. The period through which the Abstracts extend is one of the proudest in the annals of science; for during it were effected, or first made public, as appears from these records, —the hitherto unrivalled analytical researches in chemistry of Davy;—many of the most stupendous and sublime investigations in sidereal astronomy of Dr. Herschel;—the exquisite applications of combined knowledge and skill, whether to the discovery of new elements or to the improvement of the means of observation and research, of Wollaston;—the laborious, exact, and refined investigations of the all-but-universal Young;—the bulk of the papers on comparative anatomy and physiology by Home, comprising also the results of the accurate dissections of Clift, and the almost unrivalled microscopical observations, dissections, and drawings of Francis

Bauer;—the mathematical investigations of the attraction of spheroids by Ivory;—the experiments on the pendulum of Kater;—the researches in analytical chemistry of Hatchett;—the principal results of the chemical labours of Faraday down to the Bakerian Lecture on the Manufacture of Glass for Optical Purposes;—and many others of commensurate value; besides in single papers, the masterly examination of meteorites, by Howard and Bournon;—the experiments and observations on the crystallization of slowly-cooling fused earthy matter, of the lamented Gregory Watt; (both the papers last mentioned, it may be remarked, being the foundation of nearly all that has hitherto been made known on their respective subjects;)—the identification of the celebrated solar eclipses mentioned by Herodotus and Diodorus Siculus, of Baily;—and the account of the Kirkdale cave of Hyænas, of Buckland:—together with many more of a high degree of interest and importance.

Of all these contributions to the knowledge of nature, then, amounting to upwards of seven hundred and fifty in number, we are presented in these volumes with the condensed results. The proof sheets, as we gather from the Report of the Council already cited, have been read over by Mr. Lubbock and Mr. Children, and no alterations have been made except for the correction of errors obviously arising from inaccurate transcription. They are printed uniformly with the "Proceedings of the Royal Society" now in course of publication, which will form a regular continuation of the Abstracts, down to the date of each number of the former.—To add any further recommendation of these volumes to the public is impossible.

XV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

1833. Nov. 6.—THE Society assembled this evening for the Session. A paper was first read "On a Band of Transition Limestone, and on Granite Veins, appearing in the Greywacké Slate of Westmoreland, near Shap Wells and Wastdale Head," by the Rev. Adam Sedgwick, V.P.G.S., and Woodwardian Professor in the University of Cambridge.

The author began by stating, that his communication was a short supplement to a former paper, in whch he described the range of a band of transition limestone from the south-western extremity of Cumberland through a portion of Lancashire and Westmoreland. He there had stated that this limestone was cut off by the Shap granite, and did not reappear on the north side of it. During the past summer, however, he ascertained, by the help of some new artificial sections laid bare near Shap Wells, that the band of limestone does reappear, nearly in its original line of direction; and that it passes, along with the slate rocks, unconformably under the terrace of old red sandstone and mountain limestone. The phænomena are noticed in detail: and a mineral spring is described as rising among these beds, in near connexion with a protruded mass of porphyry.

The paper then describes some granite veins in the same neighbourhood, which rise from the central granite near the farm called Wastdale Head, and penetrate the grauwacké slate. Near the junction of the granite and slate, the latter puts on the character of the killas of Cornwall. The change extends to some distance, but gradually disappears, and the slate then returns to its common type, and contains organic remains. The author considers these facts as proving (in this instance) the posterior origin of the granite, and the protrusion of the granite veins into the preexisting slate rocks.

A paper was then read entitled "A Notice respecting some Points in the Section of the Coast near St. Leonard's and Hastings," by William Henry Fitton, M.D., V.P.G.S. &c.

The improvements in the neighbourhood of St. Leonard's which have rendered it necessary to cut down the face of the cliffs from Hastings to that place, have brought to light several portions of the strata, previously concealed. The object of the present paper is to describe some of these details; and a great part of it, consequently, is not susceptible of abridgement.

Several rocky ledges run out obliquely from the shore, both on the east and west of Hastings; these are analogous to the ledges which occur in the equivalent of the Hastings Sands, on the south coast of the Isle of Wight; and for the greater part consist of concretional grit, including especially fresh-water shells, of the genera Cyclas, Paludina, and Unio; others again are composed of a pisolitic sand-rock, inclosing numerous grains of reddish brown oxide of iron, which is found all along the shore from the Lover's Seat to the west of Bopeep. With the rocks above mentioned beds are found to alternate,—of sand-rock varying in colour and degrees of hardness, clay, and fuller's-earth. In proceeding westward from Hastings, the strata are observed to decline gradually towards the west as far as the gate of St. Leonard's; but at a very short distance beyond that point, they rise towards the west, and the same strata are found to recur, but in a reversed order. This appearance, which might at first be ascribed to some derangement, is produced, in fact, by a slight projection of the shore at the eastern point of the Marina at St. Leonard's, where the range of the beds coincides with the direction of the coast; the strata which come up from the sea at a small angle towards the interior, and are continued in the cliffs on the east and west, thus rising in different directions.

Among the strata which have recently been disclosed in the cliffs, a continuation of the remarkable group of the White-rock is one of the most conspicuous, and can be traced from its emergence in the sea under the White-rock to the cliff within the New Brewery. Beneath, at an interval of about 30 feet, the well-known bed of white sand-rock which forms the cliff of the Castle Hill at Hastings, rises on the shore, and being continued to the north-east, may be traced in the upper part of the East-cliff, and thence nearly to the summit of Fairlight Down.

The group of the White rock contains a subordinate stratum, in which numerous specimens of *Endogenites erosa* have been found; and the large number of specimens exposed during the progress of the works, has brought to light some additional circumstances respecting this singular vegetable. The specimens, which were found lying horizontally, in a stratum composed of sand with alternate layers of clay, consist of two portions, perfectly distinct from each other: 1st, An external coating of lignite; within which is, 2ndly, A stony kernel or nucleus, the internal structure of which has been already described*. The general form of the whole appears to have been originally nearly cylindrical, and this has been modified by pressure, so that the transverse section both of the masses, and of the tubular cavities within, generally approaches to an oval figure. The specimens differ very much in size; being from less than one foot to nine feet in length; the stony matter within occupying, in the largest, about 5 feet, with a thickness of 6 to 9 inches, and a general width of about 1 foot. This stony nucleus was invested with a coating of coal, from $\frac{1}{10}$th to $\frac{1}{2}$ an inch in thickness, which was found to extend, at both extremities, 2 or 3 feet beyond the nucleus. The external surface of the coaly covering is uniform and smooth, of a light brown colour, and glistening: but neither in this surface, nor in the coal beneath, could any traces of organization be discovered. Thin polished slices of the nucleus were exhibited.

A ledge which is observable on the shore below St. Leonard's, may be traced thence in the cliffs, through the site of the church, and westward to the summit of the hill above the Sussex Hotel. In this group also, a specimen of Endogenites was found by Woodbine Parish, Esq.; by which and other circumstances it is identified with that of the White-rock ledge: and from its including also a thin band of siliceous conglomerate, abounding in the remains of animals like those of the well-known grit of Tilgate Forest,—the teeth and bones, especially, of the Iguanodon of Mantell,—there can be no doubt of its geological identity with some of the strata of that place.

The coast sections, described in this paper, will be useful in assisting to determine the order of succession in the Hastings Sands; a point of difficulty, from the great similarity, both in the rocks, and the included fossils, of the several members composing that formation: and the author thinks it deserving of inquiry, whether the Ashburnham group, which has hitherto been referred to the lower portion of the Hastings Sands, may not be identical with some of these groups upon the shore,—and, consequently, may not belong in reality to the upper part of the formation.

A letter was afterwards read from Woodbine Parish, Esq., addressed to George Bellas Greenough, Esq., P.G.S., accompanying a collection of fossils made by Mr. Parish during the last summer at St Leonard's.

These fossils Mr. Parish states were principally found in a layer of very compact conglomerate varying from 1 inch to 3 inches in thick-

* Geol. Trans., 2nd Series, vol. i. p. 423: and Mantell's Tilgate Fossils.

ness, and forming a crust upon a stratum of sandstone which extends from the new church to the western extremity of St. Leonard's. They consist of remains of the Iguanodon, and other Saurians, and of the *Lepisosteus Fittonii.*

Mr. Parish also describes, in his letter, a submarine forest, which he traced at low water, from the western extremity of St. Leonard's to the headland at Bulverhithe, and he is of opinion that it is a continuation of the submarine forest which occurs off Hastings. The trees, he says, are chiefly oak, and appear to have fallen towards the sea.

In the peat forming part of the deposit he found hazel nuts, a variety of seeds, and the remains of beetles and other insects. No tradition has been preserved of the irruption of the sea by which the forest was submerged.

Nov. 20.—A paper was read entitled, "Notes on the Geology of the North Coast of the River and Gulf of St. Lawrence, from the Mouth of the Saguenay (Long. 69° 16′) to Cape Whittle (Long. 60°)," by Captain Bayfield, R.N., and communicated by George Bellas Greenough, Esq., P.G.S.

The line of coast surveyed by the author, and described by him in this memoir, includes above 500 miles. It is traversed by ranges of round-backed hills, rarely exceeding 1000 feet in height, and towards the eastern termination of the district sinking nearly to a level with the sea. In some parts of the coast the hills approach close to the shore; but in others they recede to a distance from it, and the country presents a succession of flats or extensive peat bogs.

The formations of which the main land and adjacent islands consist, are granitic and syenitic compounds, limestone, a deposit of clay, sand and gravel, and modern alluvial accumulations.

The granitic and syenitic rocks compose the whole of the hilly districts, with the exception of a tract opposite the Mingan Islands. True granite was noticed only in one place, the prevailing rocks being formed of felspar, quartz, hypersthene and hornblende. Porphyry, passing into syenite, was observed at the falls of the Maniton river; and veins of trap were occasionally noticed traversing the syenite. Magnetic iron was found in great abundance along the whole line of the coast, either as a constituent of the rocks or as beds of sand accumulated on the beach.

The limestone forms the Mingan and Esquimaux Islands, and it occurs on the adjacent main land, reposing in horizontal beds on the syenite. It composes also the whole of the island of Anticosta, which lies to the southward of the Mingan Islands, as well as Cape Gaspé on the south shore of the St. Lawrence. It varies considerably in its characters, being sometimes compact, at others earthy, arenaceous, shaly, or crystalline; and it generally abounds in fossils, which agree with those found in the limestone of Lake Huron and near Quebec. The strata, except at Cape Gaspé, dip at a very low angle towards the S.W.

The deposit of clay, sand and gravel forms a series of horizontal strata, sometimes 500 feet thick, in the valleys and basins between the syenitic hills. The clay invariably occupies the lowest portion, and the gravel generally the highest. No shells were noticed, though the water-courses of the rivers cut through the deposit.

The modern alluvial accumulations are of great extent, and in some parts of the coast are rapidly increasing. In Outard Bay (100 fathoms deep) the surface of the water was highly charged with earthy matter, which the surveying vessel cut through in her course, and displayed beneath the pure sea water.

The peat bogs occur towards Cape Whittle, the eastern part of the district examined, and rest upon the syenite.

During his investigations, the author noticed many evidences of change having taken place in the relative level of land and water. He mentions, that in the Mingan Islands he traced a succession of shingle beaches, the most distant from the shore and covered with trees, being 60 feet above the level of the highest tides. In the Bay of the Seven Islands, and in almost every other bay, and at the entrance of the valleys near the sea, he observed parallel ridges of sand, sometimes attaining a height of 100 feet, and occasionally containing shells analogous to those now inhabiting the St. Lawrence. This change the author conceives has been produced, not by successive depressions of water, but by successive elevations of land; and he supports his opinion by showing, 1st, that no permanent depressions could have taken place in the water of the River and Gulf of St. Lawrence, without corresponding ones in the Atlantic; and 2ndly, that the beach now forming on the Mingan Islands presents the same characters as the beaches which he traced at a distance from the shore; that the water-worn pillars of limestone which accompany each beach, bear evidence of having been worn or scooped out at different periods, the successive action of the water agreeing in level with the successive ridges of limestone shingle; and he states that the distance between these marks of action of water on the limestone pillars, exactly agrees with the rise of the present tidal waves of the St. Lawrence. He also proves, by a minute description of the alluvial accumulations now forming on the shore of the main land, and a careful comparison of them with the parallel ridges of sand already mentioned, that an identity of character exists.

In conclusion, the author briefly refers to the geological structure of the south shore of the St. Lawrence, between the meridian of the Saguenay and Cape Gaspé, and states that it consists of alternating strata of slate and grauwacké, overlaid conformably, at the latter point, by limestone, containing fossils analogous to those of the Mingan Islands and Lake Huron.

LINNÆAN SOCIETY.

Nov. 19 and Dec. 3.—A memoir was read, entitled, "On the Degree of Selection exercised by Plants with regard to the earthy Constituents presented to their absorbing Surfaces." By Charles Daubeny, M.D., F.R.S. L.S., Professor of Chemistry, Oxford.

The author states, that he was first led into this train of experiment with the hope of ascertaining by more decisive experiments than had been hitherto done, whether plants are able under any circumstances to form those earthy and alkaline matters which they usually contain, when not supplied with them from without.

With this view, he planted a known weight of the seeds of certain vegetables in earths of known composition, introduced in a finely divided state into boxes cased internally with sheet zinc. One box, containing each kind of earth, was placed in a garden exposed to rain and dust, and a corresponding one of each kind in a greenhouse protected from both.

The earths employed were, washed sea-sand, Carrara marble and sulphate of strontian.

The crop obtained from each of the boxes was separately burnt, and the ashes weighed and examined chemically. That from the boxes placed in the garden was greater than that from those in the greenhouse; but in both cases an increase of earthy matter was observed beyond that which existed in the seeds from which they had sprung.

Having remarked, however, that the plants grown in strontian contained none of that earth, he resolved to try whether this circumstance might be owing merely to the insolubility of the sulphate in water, or to some specific power belonging to the plant of rejecting the earth in question.

He therefore varied the experiment the succeeding year, by planting the seeds in four different soils, namely, sand, marble, sulphate of strontian and flowers of sulphur, and watering them with a weak solution of nitrate of strontian. In every instance there was an increase of calcareous matter, beyond that present in the seeds, greatest in the plants that had grown in sulphate of strontian and in Carrara marble, least in those planted in sulphur; but the largest quantity of strontian ever detected by chemical means from their ashes did not exceed 0·4 of a grain. From these and similar experiments, detailed in the memoir, the author concludes, that the absorbing surfaces, or *spongioles*, of the roots of plants either do not admit strontian earth at all, even in a state of solution, or at least receive it much less readily than they do calcareous matter.

He details an experiment to show, that the absence of strontian from the solid parts of the plants was owing to its remaining unabsorbed by the roots, not to its being excreted by them; and accounts for the difference between what happened in the instance of the strontian, and that which he had himself observed in common with Mons. de Saussure, as holding good with regard to solutions of substances more directly injurious to the plant, by supposing, in the latter instance, the *spongioles* to be disorganized by the poisonous quality of the substance, and consequently to have allowed the solution to be absorbed by capillary attraction. In this latter case he observed, that before the plant is destroyed, a portion of the poisonous substance will be excreted again by the spongioles of the roots.

Upon the whole he concludes, that his experiments lend no countenance to the idea that plants can form their earthy constituents when not supplied with them from without, although they do not altogether demonstrate the reverse.

They seem, however, to show more decisively that plants do, to a certain extent at least, possess a power of selection, and that the earthy constituents which form the basis of their solid parts are determined as to *quality* [*kind?*] by some primary law of nature, although their *amount* may depend upon the more or less abundant supply of the principles presented to them from without.

The conclusion of Mr. Westwood's Monograph on the Genus *Diopsis* was also read. The subject of this paper is a remarkable group of two-winged flies, having the sides of the head produced into two long and slender processes, at the extremity of which the eyes and antennæ are placed. In this paper the author has given the characters of the genus at great length, has more than doubled the number of species, and has noticed the analogous structure of other pedunculated-eyed animals. He has divided the genus into four sections. A. wings with an abbreviated stripe near the tip, including, 1. *D. ichneumonea*, Linn., Tropical Africa; 2. *D. collaris*, Westw. Senegal; 3. *D. pallida*, Westw., habitat unknown; 4. *D. nigra*, Illiger, Sierra Leone.—B. wings with a terminal spot. 5. *D. apicalis*, Dalm., Sierra Leone; 6. *D. tenuipes*, Westw., Senegal; 7. *D. indica*, Westw., East Indies; 8. *D. assimilis*, Westw., habitat ? 9. *D. abdominalis*, Westw., habitat ? 10. *D. fumipennis*, Westw., Senegal.—C. Wings without spots. 11. *D. signata*, Dalm., Sierra Leone; 12. *D. fasciata*, Gray, habitat ? 13. *D. macrophthalma*, Dalm., Sierra Leone; 14. *D. thoracica*, Curt. MSS., Westw., Africa; 15. *D. obscura*, Westw., Sierra Leone; 16. *D. confusa*, Weid., Angola.—D. Wings with several entire fasciæ; 17. *D. Dalmanni*, Weid., Java; 18. *D. Sykesii*, Gray MSS. Westw., East Indies; 19. *D. brevicornis*, Say, North America.

ZOOLOGICAL SOCIETY.

July 23. (*continued from* vol. iii. p. 375).—The Secretary read a communication from M. Geoffroy-Saint-Hilaire, entitled "New Observations on the Nature of the Abdominal Glands of *Ornithorhynchus*," in which the author states it to be his purpose to reply to the observations of Mr. Owen on that subject, contained in the Proceedings of the Society, under date of the 12th of March in the present year *.

"The question no longer regards merely the simple fact, *whether*, decidedly and absolutely, the *Monotremata* are *viviparous*, or *oviparous*; whether we should reason upon them according to the rules of *the past*, and apply to them the entire character of *Mammalia*; or whether we are not compelled to see in them sufficient anomalies to embrace them in views *of progress*.

"Let us state the case more precisely. There is but one single consideration to be discussed; viz. whether the gland on each side of the *abdomen* is mammary and lactiferous (as Mr. Owen thinks), or whether it is not a gland of a different kind (as I, for my part, believe). I call it a gland *sui generis*, and have lately proposed to denominate it *Monotrematic*, as it attains its *maximum* of composition among the *Monotremata*.

* See Lond. and Edinb. Phil. Mag., vol. iii. p. 64.

"Is it a mammary gland? Mr. Owen's concessions militate strongly against this conclusion; for it is not conglomerate, it is not invested with an erectile tissue, and it is without nipples. In Meckel's time the appearance of the latter was hoped for, the nipples being frequently developed under the action of sucking; but at present this can no longer be anticipated. Females have been seen in full nutritive action, in New South Wales, by Lieut. Lauderdale Maule and Mr. James M'Arthur, and at London by Mr. Owen himself; and each observer has insisted on the circumstance that there were *no nipples*.

"Thus the fact of a decidedly assimilated *structure* is wanting: the gland of *Monotremata* is not in its composition comparable with a mammary gland. But I observe that I am answered here by a fact of an assimilated *function*. Lieut. Maule and Mr. M'Arthur speak of an abundant secretion, *milky* according to one, of a *milky appearance* according to the other. It is therefore inferred that there remains at least this character (the *function*) in common, to prove *mammary* a gland of a different structure. But, I may reply, begin by being certain that the product of the secretion is a true *milk*; do not introduce an *unknown* to characterize a new organ of a structure hitherto equally *unknown*. What! the organ is not in its composition *mammary*, and yet its secretion is *lacteal*! What would become, then, of the principle, *Such as the organ is, such necessarily is its function?*

"The vascular system does not go the length, as in conglobate glands, of folding itself round, of mutually anastomosing, and of penetrating itself, in obedience to the law of affinity of *self for self* (*de soi pour soi*); whence, at the proper period, a compound fluid,—*milk*. But this vascular system, as in mucous membranes, extends its terminating branches into cavities with an external exit. From this more simple apparatus I expect a fluid in itself more elementary—*mucus*, as I suppose.

"But I do more than believe this by way of conjecture: I offer this demonstration of the fact. On the 3rd of June I laid before the Academy of Sciences, of which I am this year President, a paper on the existence of a gland in all respects similar to that which is described and figured (Phil. Trans. 1832, pl. 17, fig. 2 and 3,) by Mr. Owen in the *Echidna*,—a *Monotrematic gland* consequently, which I have observed in the *Water-Rat* (*Mus amphibius*, Linn.). I subjoin the figure of this gland magnified, and invite a comparison of this drawing with that of Mr. Owen's plate.

"I begged of our learned chemist, M. Dumas, Member of the Academy of Sciences, to analyse the product of the secretion of the monotrematic gland of the *Water-Rat*; his researches determine that it is not milk. M. Dumas has obtained this result still more positively by microscopic observations. Each of these products is invariable in its form: milk has the appearance of perfectly spherical globules; while the matter from the gland of the *Water-Rat* exists under the form of thin flakes strongly angular at the edges. The mucus of the *saliva* presents the same aspect, except that the edges of the flakes are not so deeply indented. The result is, that the glandular secretion furnished by the *Water-Rat* appeared to us to be *mucus* mixed with a small proportion of fatty odorous matter; and there can be no doubt that the same is the case with the secretions of the glands of the *Shrews*.

"Now there remains an experiment to be made by the Zoological Society, but principally by Mr. Owen, animated like myself with zeal for scientific truth; and which I invite my colleagues to make. Alcohol does not alter the form of the elementary molecules, either of milk or of *mucus*. Mr. Owen has deposited in the Museum of the College of Surgeons his anatomical preparations; it is easy, without damaging the preparations, to take from the monotrematic apparatus a small quantity of its secretion, and to place it in the field of a microscope. An answer will thus be obtained, of which I admit beforehand all the consequences.

"The negative characters indicated above, (*no conglobate tissue, no erectile tissue, no nipples*,) are remarkable concessions on the part of Mr. Owen. He might have advanced still further in the same direction, and not have accepted, for example, from Lieut. Maule his milky fluid only, reserving himself to combat afterwards what that observer says *de visu* of the shells in the nest, and rejecting also the opinion of the country in favour of the oviparous character of the *Monotremata*.

"But I will not return here to all the accessory points of the controversy: I pass to Mr. Owen's observations in reply.

"Firstly, To destroy the effect of the analogy of the glands of the *Shrews*, to which I had referred the glands of the *Monotremata*, Mr. Owen cites the authority of Von Baer, who in the Archives of Anatomy and Physiology, published at Leipzig in 1827, p. 168, had combated my views, in order to support the opinions of his friend and fellow-countryman Meckel, remarking that, proceeding from analogy to analogy, that of the *Cetacea* must also be taken into consideration. Von Baer says that the structure of the glands of the *Ornithorhynchus*, as described and figured by Meckel, reminded him in all particulars of the mammary glands of the *Cetacea*; and actually refers to a similar arrangement in the *Porpoise*. Now, adds this learned anatomist, 'it has never entered into the mind of any man to deny the *Porpoise* to be a lactiferous animal.' It is true that nobody has hitherto raised a doubt on this point; but it would not be by any means extraordinary if we were obliged to do so now, if it were certain, as I believe, that the monotrematic glands of the *Ornithorhynchus* give rise to a new mode of nutrition as regards the young. For if this were the fact, the *Cetacea* would participate in this new mode, in these new functions, which it will become ulteriorly necessary to determine better, inasmuch as offering an intermediate generation, viz. between that proper to the *Monotremata*, and a third sort, that of the *Ovovivipara*, (that is to say of the *Sharks* and *Rays*,) the eggs of which are hatched either within or without the body of the mother, they would furnish facts of the same rank as those of the vipers and other snakes, and would not offer such important characteristic differences between all these animals, as have hitherto been uniformly believed to exist. I refrain from proceeding further in order not to overpass the boundaries of analogies and of truth; but it might happen that the objection proposed by Von Baer should lead to this result; not that the *Monotremata* should be thrown back into the centre of the *Mammalia*, but that the *Cetacea* should be separated from among them. The affinity of structure, if it be such as the German physiologist announces, may lead to an idea that the mode of nutrition which I have sketched for the *Monotremata* may be equally adapted to the *Cetacea*. Formerly one mode only was known, and it was supposed *à priori* that the *Cetacea* must have passed through it. At all events it is necessary to revise the doctrine of the nutrition of the *fœtus* of *Cetacea*.

"Secondly, Mr. Owen points out the contradictoriness of my two opinions in two papers published at an interval of less than a month, and this is fair play in his capacity of critic. Nevertheless I had scarcely touched on the fact relative to the egg-shells in my first paper, proposing to return to it again. This I actually did some weeks afterwards, when I conceived a system complete in itself, well connected, opening out new views to research, and of which I frankly declare that I had not the smallest idea a few days before I became attached to it. Let it not, however, be believed that I present either my old or my new conjectures as facts, the solidity of which I decidedly maintain. In the absence of facts, I venture to recur to presumptions, which may become motives for research; but if I calculate certain probabilities, I merely desire to have applied to them the criterion of observation. I know well that the mind of no man is endowed with the faculty of imagining with regard to substantial bodies, of distinctly conceiving the idea of a form. What has been seen of this kind is thenceforth known. Seriously admitting the truth of this proposition, I merely wish to play a useful part, restricting myself to the duties of a naturalist having the privilege of age, confident in the experience of ancient studies, and acquainted with the possible extent of the diversities of the acts of nature, in order to assist observers less practised than myself in the study of natural history, so that if there should exist in the most distant part of the globe, organic conditions which we are interested in becoming immediately acquainted with, I may say to them 'There is a chance that it is A, or B, or C; see what is the fact; instruct us with regard to it.'

"Thirdly, The monotrematic glands follow the *phases* of the development of the sexual apparatus: like the mammary, they form part of it, being large only in the females. To this I answer that it is presuming too much with regard to the resources of nature, (which shows on the contrary a tendency, as well as the most ingenious means of execution, for a diversity of forms,) to fall into absolute rules. What do we know of it? On the contrary, let us better understand our duties; let us constantly restrict ourselves to the consideration of facts. It is a means of exposing ourselves to grave mistakes, if we so easily and so precipitately determine with regard to functions. In fact the *Shrews* alone share with the *Monotremata* this fact of resemblance, viz. that the monotrematic glands are more developed in the female during the period of heat. The circumstances are different in the *Water-Rat*, which possesses the same gland at all seasons and in both sexes.

" Fourthly, What are we to infer from the distinction drawn from the nature of the localities, aquatic as regards the *Ornithorhynchus*, dry with reference to *Echidna*? And why might it not happen that the function should be modified according to the nature of the ambient medium? Let us not establish a general thesis on facts which are not accurately known. To acquire a knowledge of these facts is our object, and our uncertainty with regard to them forms the problematic part of our controversy. We are dealing with a new fact; let us wait till we have seen and learned it before coming to a definitive conclusion. The *Shrews* offer us another useful piece of instruction: they consist, in fact, of several species, all having the same gland, but not inhabiting the same localities. Some do not quit the lowlands and take freely to the water; while others are met with on the dry soil of upland plains."

The reading of M. Geoffroy-Saint-Hilaire's Paper having been concluded, Mr. Owen addressed the Society. He spoke of the glands adverted to by M. Geoffroy, as differing essentially from those of the *Monotremata* : in the *Water-Rat*, the glands exist in both sexes, and at all seasons; in the *Shrews*, they exist in the female only, and are developed in the season of heat; in the *Monotremata*, they exist also in the female alone, but their development is at the period of bringing forth the young. To these important discrepances is to be added one still more important—the glands referred to in the *Water-Rat* and in the *Shrews* are additional to those for the nutrition of the young, and their function is wholly different: in the *Monotremata* only one set of glands exists, and these are admitted by M. Geoffroy, in his later hypothesis, to be for the secretion of nutriment for the young.

As regards the glands of the *Cetacea*, Mr. Owen adduced various testimonies to show that their secretion is milk, of a very rich quality, approaching to that of cream. Simplicity of structure, in a secreting organ which is usually complicated, cannot therefore be relied on as affording proof of a difference of function. All glands are in their lowest condition, simple tubes, which become, in the more highly developed forms of the gland, complicated in various degrees, conglomerate or conglobate. Such is the case with the organs for the secretion of bile, which commence in *Insects* in the form of simple tubes, and passing through various stages of complication, become in the higher classes condensed into a liver. Such is the case also with the pancreatic organ; a case more in point, as it exhibits, within the compass of a single class, that of *Fishes*, all degrees of complication. In some it seems to be altogether wanting; in others it is rudimentary, consisting of one or two minute *cæca* appended to the *pylorus*; and these, in others, increase in extent, in number, in complication, by becoming branched, and eventually form, in the *Cartilaginous Fishes,* true conglomerate glands. To the class of *Mammalia* mammary glands are peculiar; and it might almost have been expected *à priori* that in that class these organs should be found in the various degrees of simplicity or complication of which they are capable. Such appears to be the case; in *Cetacea* they are simple *cæca* (and in this respect the glands of *Monotremata* agree with these mammary glands); in higher forms they are conglomerate, and cannot be misunderstood.

Mr. Owen added, with reference to the microscopic test of the nature of the secretion which was proposed by M. Geoffroy, that he had not been able to procure either from the glands themselves or the openings of their ducts any portion of their secretions to which the test could be applied; globules of oil alone offering themselves to his observation, and these existed also in the spirit in which the animals were preserved.

September 10*.—A letter was read, addressed to Mr. Vigors by B. H. Hodgson, Esq., Corr. Memb. Z. S., and dated Nepâl Residency, February 23, 1833. It referred to the zoological specimens which the writer had forwarded to Calcutta, to be thence transmitted to England, some account of which, as contained in a letter from Mr. Prinsep, was read at the last Meeting.

The Secretary called the attention of the Society to several recent acquisitions to the Menagerie; including a specimen of the *red-handed Tamarin Monkey, Midas rufimanus,* Geoff., presented by J. Christopher, Esq.; of the *crested Porcupine, Hystrix cristata,* Linn., which had recently been brought forth there, being the first instance of such an occurrence in this species, and respecting which he added, that observation of the young while sucking confirmed the correctness of M. Blumenbach's statement that the nipple is nearly axillary; of the *purple-crested Touraco, Corythaix porphyreolopha,* Vig., presented by J. J. Audubon, Esq.; and of the *Platycercus Novæ Hollandiæ,* Vig., *Psittacus Novæ Hollandiæ,* Lath., a species which appears not to have been seen since the time when it was originally described until very recently, when a living specimen for the Menagerie, and skins for the Museum, were obtained nearly simultaneously.

Mr. Bennett also called the attention of the Meeting to a living *Lemur,* forming part of the Society's collection, and pointed out the distinguishing marks which induced him to consider it as the representative of an undescribed species, for which he proposed the name of Lemur *rufifrons.*

At the request of the Chairman, Mr. Gould exhibited a series of specimens of the genus *Malurus,* Vieill., including the whole of the species previously known, together with one, forming part of the Society's collection, which he regarded as hitherto undescribed. He characterized it as the Malurus *pectoralis.*

Mr. Gould also exhibited specimens of the male and female of the *Trogon pavoninus,* Spix: the latter, he stated, has hitherto

* The Proceedings of August 13 and 27 will be found in the Lond. and Edinb. Phil. Mag. for November last.

escaped the observation of ornithologists. It has recently been acquired for the Society's collection.

The female rather exceeds the male in all her proportions. Her bill is black instead of yellow: her crest is shorter, and has bronzy reflections. The whole of the under surface is of a brownish grey, with the exception of the under tail coverts, which are scarlet. The outer tail feathers, which in the male are white with black shafts, are in the female barred, except at the base, where they are dull black. The colours of the upper surface are similar in both sexes; but the plumes which spring from the rump, and which in the male attain so remarkable a length, scarcely extend in the female beyond the tip of the tail.

A "Description of *Perdix Lerwa*," by B. H. Hodgson, Esq., Corr. Memb. Z. S., was read. It was accompanied by a coloured drawing of the bird, which inhabits the northern region of Nepâl, and forms, by its half-plumed *tarsi*, a sort of link between the *Partridges* and the *Grouse*. Its habits assimilate with those of the latter genus. It is found close to the permanent snows, among rocks and low brushwood, and sustains itself upon aromatic buds, leaves, and small insects. It is characterized as Perdix *Lerwa*.

The great comparative expanse of the wing; the diminution of its rounded form by the second quill feather being the longest; the increased length and strength of the tail; and the extent of the feathering of the *tarsi*, are very remarkable characters, which give to this species a peculiar interest. Its dimensions, as compared with several allied birds, are given by Mr. Hodgson in the following table:

	Perd. Lerwa.	*Perd. Chukar.*	*Perd. gularis.*	*Perd. Francolinus.*
Length, from the tip of the bill to that of the tail	1·2$\frac{3}{4}$	1·1$\frac{1}{2}$	1·2$\frac{1}{4}$	1·2
Length of the bill	1	1$\frac{1}{8}$	1	1$\frac{1}{16}$
Basal height of ditto	$\frac{3}{8}$	$\frac{11}{16}$	$\frac{7}{16}$	$\frac{4}{8}$
Basal breadth of ditto	$\frac{5}{8}$	$\frac{12}{16}$	$\frac{9}{16}$	$\frac{5}{8}$
Length of the tail	4$\frac{5}{8}$	3$\frac{1}{2}$	4$\frac{1}{8}$	3$\frac{3}{4}$
Expanse of the wings	1·11$\frac{1}{2}$	1·8	1·9$\frac{1}{4}$	1·8
Length of the *tarsi*	1$\frac{7}{8}$	2$\frac{3}{16}$	2$\frac{5}{8}$	2$\frac{1}{16}$
Length of the central toe and nail	1$\frac{5}{8}$	2$\frac{9}{16}$	2$\frac{1}{2}$	1$\frac{3}{8}$
Weight	1lb. 2oz.	1lb. 2oz.	1lb. 2oz.	1lb.

A paper "On the Anatomy of the *Cheetah*, *Felis jubata*, Schreb.," was read by Mr. Owen. It commenced by remarking on *Felis* as a truly natural genus, and by observing that the anatomical structure of the animals composing it offers even fewer differences than their outward forms. The principal deviation from the common type is that which obtains in the organs of voice of the *Lion* (and, as Mr. Martin has observed, in those of the *Jaguar* also), where the *larynx* is situated at a considerable distance from the posterior margin of the bony palate, the soft palate and the tongue being proportionally increased in length, and thus a gradually expanding passage is formed, which leads from the *glottis*, where the air is rendered sonorous, to the mouth. This structure may contribute, in the *Lion*, to produce the peculiar roar of that animal.

In the *Cats* generally, the connexion of the *os hyoides* to the *cranium* is not by a long elastic ligament, as in the *Lion*, but by an uninterrupted series of bones. This latter structure exists in the *Cheetah*. The *Cheetah* has also the circular pupil of the *Lion*, *Tiger*, *Leopard*, and *Jaguar*, and is perhaps the most diurnal of the genus.

In the form of the *œsophagus*, and in the transverse *rugæ* of its lower half, the *Cheetah* agrees with the *Lion*; and, as in it and in the other *Feles*, the *œsophagus* is not prolonged into the *abdomen*, but terminates immediately after passing through the diaphragm in the stomach. This organ in the *Cheetah* has all the peculiarities which are found in the genus *Felis*. The intestines also agree in character with those of that group; and the *cæcum*, as usual in it, is simple, having none of the convolution which is found in the *Dog*. The liver, *pancreas*, and spleen, resemble those of the *Cats* generally; as do also the kidneys in the arborescent form of their superficial veins: a form, however, equally common to the *Viverridæ* and the *Felidæ*, which also agree in having *spiculæ* on the tongue.

The *viscera* of the *thorax* in the *Cheetah* agree with those of the *Cats*. The *lytta*, or rudiment of the lingual bone, so conspicuous in the *Dog*, is reduced in it, as in the other feline animals, to a small vestige.

There is, as in the *Feles* generally, no bone of the *penis*; and the *glans*, as usual in them, has retroverted *papillæ*.

The elastic ligaments of the ungual *phalanges* exist in the same number and position as those of the *Lion*; they are, however, longer and more slender, their length alone occasioning the incomplete retraction of the claws as compared with the rest of the *Felidæ*.

Mr. Owen concluded by observing that in the circulating, respiratory, digestive, and generative systems, the *Cheetah* conforms to the typical structure of the genus *Felis*.

September 24.—A collection of skins of *Birds*, sixty-four in number, formed in the Himalayan Mountains, and presented to the Society by Lady William Bentinck, was exhibited. It included several species apparently new to science, and was particularly rich in the interesting *Pheasants* of the Himalaya. The collection was remarkable on account of the fine condition of the specimens, which generally surpassed in beauty those previously contained in the Society's Museum.

A series of eighty skins of *Birds*, selected from a collection formed in India by H. B. Hillier, Esq., and presented by that gentleman to the Society, was exhibited. It comprised specimens of many species in fine or interesting plumage.

Mr. Bennett called the attention of the Meeting to a *Monkey* which had been for some time living at the Society's Gardens, and which, from a comparison of the figures and descriptions of recent authors, he had regarded as entirely new, until Mr. Ogilby pointed out to him its identity with the *Malbrouck* of Buffon, a very different animal from that figured under the same name by M. Fréderic Cuvier. The *Simia Faunus*, Linn., to which Buffon referred his *Malbrouck*, is wholly founded on a figure given by Clusius in his 'Exotica,' which represents, if correctly drawn, a species nearly related to the *Simia Diana*, Linn. (not F. Cuvier); and the *Simia Cynosurus*, Scop., with which

M. Geoffroy and others have since identified it, is so imperfectly figured and described as to apply with almost equal justice to any of the related species. It became necessary therefore to give a new name to the true *Malbrouck*; which, as its characters appear to have been of late completely misunderstood, even in France, seemed also to require a new description to assist in its recognition. Buffon's figure, and the accompanying description by Daubenton, were taken from a female; the Society's specimen is a male.

CERCOPITHECUS TEPHROPS. *Cerc. suprà fusco-virescens, infrà albidus; artubus externè grisescentibus; facie pallidè carneá, naso, genis, labiorumque marginibus pilis brevibus fuliginosis conspersis.*

The colour of the upper surface resembles that of the *Green Monkey, Cerc. Sabæus*, Geoff., having the separate hairs ringed with black and yellow; on the outsides of the legs it has more of a greyish hue, the lighter rings on the hairs having little of the yellow tinge. The under surface is nearly of a pure white, and this extends to the insides of the limbs and to the sides of the neck anteriorly, where the hairs do not attain a sufficient length to constitute moustaches. The naked parts of the hands, and the nails, are black; the ears dusky; and the face is of a light flesh colour, with short black hairs, giving a sooty tinge to the nose, cheeks, and edges of the lips, from which a circle round the eyes and the space surrounding the nostrils are free. There is a narrow light bandeau traversing the forehead above the superciliary ridges. The tail, in its mutilated state, is nearly as long as the body, and is of the same colour as the latter above, and lighter beneath. The length of the body appears to be about 18, that of the tail 16 inches.

A paper entitled "Further Illustrations of the *Antilope Hodgsonii*, Abel," by B. H. Hodgson, Esq., Corr. Memb. Z. S., was read,—an abstract of which is given in the "Proceedings" of the Society.

A "Description of the *wild Dog* of Nepâl," by B. H. Hodgson, Esq., Corr. Memb. Z. S., was read. Its local name is *Búánsú*. It is characterized as the

CANIS PRIMÆVUS. *Can. dentibus molaribus in maxillá inferiore utrinque sex; palmis plantisque pilosis; auribus erectis; suprà saturatè rubiginosus, infrà flavescens; caudá insigniter comosá, rectá, mediocri.*

The very remarkable peculiarity in the number of the molar teeth of the lower jaw, indicated in the specific character, has been verified by Mr. Hodgson on the examination of the *crania* of three adult, two mature, and one young individual of the race. The deficient number is occasioned by the absence of the second tubercular tooth. All the other teeth exist in the ordinary number and positions.

At the commencement of his paper, Mr. Hodgson remarks on the uncertainty that prevails as to the primitive stock of the *familiar Dog*, and rejecting, with most modern zoologists, the claim of the *Wolf*, the *Jackal*, and the *Fox* to rank as its prototype, he also argues against regarding as such the half-reclaimed *Dingo* of Australia. He thinks that he has detected this original race in the *Búánsú* of Nepâl, the eastern and western limits of whose range appear to be the Sutlege and the Burhampootra, and which seems to extend, with some immaterial differences, into the Vindyia, the Ghauts, the Nilgiris, the Casiah Hills, and in the chain passing brokenly from Mirzapore through South Bahar and Orissa to the Coromandel Coast.

Of this race, although so wild as to be rarely seen, Mr. Hodgson has succeeded in obtaining many individuals; some of which lived in confinement many months, and even produced young, having been pregnant when they reached him. He is consequently enabled to describe not only the form and colours, but the manners also, which he does in great detail. The form he compares particularly with that of the *Indian Jackal* and the *Indian Fox*, short notices of which he gives as an Appendix, and comparative figures of which with the *Búánsú* he also forwards with his paper. The paper is also accompanied by comparative figures of the *crania* of these several species; and the description given of this important part of the animal structure is also comparative.

The *Búánsú* preys by night as well as by day, and hunts in packs of from six to ten individuals, maintaining the chase rather by its powers of smell than by the eye, and generally overcoming its quarry by dint of force and perseverance. In hunting, it barks like a hound; but its bark is peculiar, and equally unlike that of the cultivated breeds of *Dogs* and the strains of the *Jackal* and the *Fox*.

Adults in captivity made no approach towards domestication; but a young one, which Mr. Hodgson obtained when it was not more than a month old, became sensible to caresses; distinguished the dogs of its own kennel from others, as well as its keeper from strangers; and in its whole conduct manifested to the full as much intelligence as any of his sporting dogs of the same age.

October 8.—A letter was read, addressed to the Secretary by W. A. Wooler, Esq., and giving an account of a *wild Dog* from the Mahablishwar Hills, now known as Malcolm's Pate, in the Presidency of Bombay: its local name is *Dhale*. The habits of this *Dog*, in a state of nature, are described by Mr. Wooler: they accord with those of the *Búánsú* of Nepâl, as detailed by Mr. Hodgson in a paper read at the previous Meeting of the Society.

A specimen was exhibited of the *hairless Egyptian* variety of the *familiar Dog*, which had recently died at the Society's Gardens. The exhibition was made principally with the view of illustrating the apparent connexion between teeth and hair. In this animal, so remarkable for its deficiency of hair, a corresponding deficiency of teeth was observed; there being neither incisors nor canines in either jaw, and the molars being reduced to one on each side, the large tubercular tooth being the only one remaining.

Mr. Yarrell stated in further illustration of the subject, that he had examined the mouths of two individuals of the same variety still living at the Gardens, in both of which he found the teeth remarkably deficient. In neither of them were there any false molars; one was entirely destitute of canines also, these teeth being in the other short of the usual number; and the incisors were also in both deficient in number.

He also exhibited from his collection the *cranium* of a hairless *Terrier*, in which the false molars were wanting.

A letter was read addressed to the Secretary by M. Savi, For. Memb. Z.S., and dated Pisa, July 22, 1833. It accompanied a collection of the works of the writer, which he presented to the Society, together with specimens of most of the zoological objects which he had added to science. These specimens were exhibited.

In bringing them severally under the notice of the Society, the Secretary continually referred to those writings of M. Savi which related to them, and explained from thence the most interesting particulars connected with each of the specimens submitted.

A collection of skins of *Mammalia*, obtained from the Frankfort Museum, was exhibited. The whole of them were from Abyssinia, where they were procured by M. Rüppell, in the 'Zoological Atlas' of whose 'Travels in Northern Africa' many of them were for the first time described and figured. They included thirteen species new to the Society's collection, and were severally brought under the notice of the Meeting by the Secretary.

October 22.—A letter was read, addressed to the Secretary by Sir R. Ker Porter, Corr. Memb. Z. S., and dated City of Caracas, August 14, 1833. It described a *Bear* now living at that place and brought from the Andes, which differs in the marking of its face both from the individual of *Ursus ornatus*, figured by M. F. Cuvier, and from that which forms at present a part of the Society's Menagerie. The yellowish white of its face begins on the bridge of the nose between the eyes, and describes under each eye a semicircle, whence it extends over the whole of the muzzle, taking rather a greyish hue, until it ends in pure white, covering the whole throat and chest, and forming a point between the fore legs. The rest of the animal is jet black, the hair being silky and shining. It is smaller by far in size than the *Bears* of the Northern countries of Europe, and is more compact in form.

Sir R. Ker Porter also enters into various details respecting the *Curassows* or *Powies* of Caracas. Of a pair kept by him in confinement, the female laid an egg without making any provision for its reception or paying it any subsequent attention.

He adds that he has obtained a specimen of a bearded *Capuchin Monkey* from the Rio Negro, which he intends forwarding to the Society in the spring.

Mr. Cox stated that he had at present in his possession a living *Mocking-bird*, which he had recently obtained from North America, and to which he invited the attention of the Members.

A specimen was exhibited of the female *Antilope Bennettii*, Sykes, which had been presented to the Society by the President, Lord Stanley. It had lived in his collection for about a month, and was believed to be pregnant, which was ascertained on examination after death to be the fact.

Drawings were exhibited of two *Fishes* taken in Mount's Bay, Cornwall. They were communicated by Dr. Henry Boase, and one of them was accompanied by a short description. It appears to be the *Capros Aper*, La Cép., *Zeus Aper*, Linn., a Mediterranean species which has not before been noticed as occurring on our shores, unless it be the fish included by Mr. Couch in his list of the *Fishes* found in Cornwall, (Linn. Trans., vol. xiv. p. 81.) under the name of *Stone Basse*; the reference to Ray, however, made by the latter author is to a species of *Gerres*, Cuv. Dr. Boase's drawing agrees well with the figure published by Rondelet.

The other drawing represents a *Tetrodon*, evidently identical with that obtained from the same coast by Pennant and by Mr. Donovan.

Mr. Gray gave some account of the reproduction of *Cirrhipeda*, founded on observations made by him on *Balanus Cranchii*, Leach, during a recent visit to the coast of Devonshire. In illustration of his remarks he exhibited an adult of that species with the eggs attached to the body at the base of the shell, and the young *in ovo*. He also exhibited numerous very minute individuals of *Bal. vulgaris* affixed to rock.

He described the mode of reproduction as ovoviviparous. On opening under water, after they had been preserved in spirit, the eggs attached to the body of the adult, each was found to contain a perfectly developed animal, which occupied nearly the whole of its cavity. The form of the young *Barnacle* at this period of its existence is ovate, rather tapering above, and truncated and ciliated at the tip: it is furnished with three pairs of arms along the sides, the base of each arm being two-jointed; the lower pair of arms has only one elongated process, while each of the two upper pairs has two fusiform, thick, articulated and ciliated processes, similar to those of the anterior part of the perfect animal, but less elongated. From the adult it differs chiefly in having a smaller number of feet and in the less development of the hinder part. It is also destitute of shelly covering, which is probably not formed until the young animal becomes fixed. In very small attached individuals of the *common Barnacle* the shell is rather soft, transparent and horn-coloured.

In the absence of shell from the animal in the egg, an additional evidence is furnished of the affinity of the *Cirrhipedes* to *Crustacea* rather than to *Mollusca*: the fœtus in the latter class being covered by a shell at a very early stage of its embryo growth. The existence in the young animal of a smaller number of arms than that found in the adult is also analogous to the corresponding fact which has been observed in several of the *Branchiopodous Crustacea*. A similar fact has recently been noticed by Dr. Nordmann as occurring in *Lernæa*.

Mr. Gray remarked that he had been the more induced to call the attention of the Society to the subjects which he exhibited, on account of his observations being at variance with those recorded by Mr. J. V. Thompson in the fourth Memoir of his 'Zoological Researches.' The young of *Balanus* is there described as being, when $\frac{1}{10}$th of an inch in length, a free swimming animal, resembling *Cyclops* in its general form, and having pedunculated eyes: and it is stated that it then throws off its bivalve-shell-like envelope together with the greater part of the black colouring matter of the eyes, becomes fixed and covered with calcareous matter, and is changed into a young *Barnacle*, such as is described by Pennant as *Balanus pusillus*, the arms at the same time acquiring the usual ciliated appearance. In Mr. Gray's specimens of the young, on the contrary, the general form of the adult is found, and the arms are ciliated while it is still in the egg, its total length being less than $\frac{1}{50}$th of an inch. Of this length it is also by no means uncommon to find *common Barnacles* attached.

Mr. Gray added that on examining the eggs which are found around the base of the animals of *Pentalasmis*, Leach, and *Otion*, Ej., he had observed indications of the existence of young similar to the adult. They were not, however, sufficiently developed to enable him to describe them with precision.

Mr. Gray also called the attention of the Society to a fact connected with the history of some of the marine *Gasteropodous Mollusca*, which he had observed on the same occasion with the young of the *Balani*. It is well known that the animals of terrestrial shells are torpid during the winter in cold and temperate climates, and during the dry season or summer in tropical regions; but it had not been previously remarked that a similar state occurs in those of marine shells. Mr. Gray found that many individuals of *Littorina petræa*, and some of *Litt. rudis*, were in this condition during his stay at Dawlish. They were attached to the rocks several feet above the reach of the highest autumnal tides; their foot was entirely retracted; and a membranous film was spread between the rock and the edge of the outer lip of the shell: the gills were only moist, the branchial sac being destitute of that considerable quantity of water which exists in it in those of the same species which are adherent to the rock by their expanded foot. In this torpid condition, the individuals observed by Mr. Gray continued during the whole of his stay, which lasted for more than a week. On removing several of them and placing them in sea water, they recovered in a few minutes their full activity.

Mr. Gray further stated that he had on the same occasion observed that the animal of *Rissoa parva* has the power of emitting a glutinous thread, by which it attaches itself to floating sea-weeds, and is enabled, when displaced, to recover its previous position. A similar property, he remarked, was long since observed in one of the land *Mollusca*, a species of *Limax*, Linn.; and it has recently been recorded by M. Sander Rang as occurring in a marine genus of *Mollusca*, to which he has given the name of *Litiopa*. Mr. Gray added his belief that it would probably be found to be common to many species of marine *Mollusca*.

PHILOSOPHICAL SOCIETY OF CAMBRIDGE.

A Meeting of the Cambridge Philosophical Society was held on Monday, November 25, the Rev. Dr. Clark, V.P., being in the chair.

Mr. Hopkins gave an account of some important points in the geology of Derbyshire. The toadstone beds of the limestone district in that county give a definiteness to the stratification which makes it impossible for any false generalizations to stand for an instant the test of close examination. It soon became evident to Mr. H. that Farey's views on this subject were totally untenable, leading in some cases to absolute geometrical impossibilities; and in many cases to results which, it was conceived, ought to be regarded as geologically impossible. The mode of investigation adopted by Mr. H. for the purpose of ascertaining the exact disposition of the toadstone beds, was the obvious one of tracing their basseting edges, which he was able to do with clearness and certainty, and thus to arrive immediately at demonstrable results with respect to all the important points of the subject. One of these is furnished by the well known hill, Masson Lowe, near Matlock. There is decisive proof that the toadstone capping this hill belongs to the *second* or *lower* bed observable in the High Tor; and the above mode of investigation proved that that which is found at the village of Bonsal, many hundred feet lower than the top of the hill, and close at its foot, belongs to the *first* or *upper* bed in the High Tor, the basseting edge of this bed being distinctly continuous from the Tor round the northern side of the Lowe to Bonsal. It afterwards skirts the Via Gellia Dale, and passes by Ible and Grange Mill, being, in fact, the bed which Farey, and it is believed every other geologist, has regarded as the *third* toadstone. Masson Lowe must consequently have been elevated by an enormous fault, of which indeed there is direct evidence to be found in Bonsal Dale.

The same mode of investigation showed immediately that the toadstone near the top of Priestcliff Lowe (stated by Farey to be the *second* bed) belongs to the same bed as the *upper* toadstone in Fin Copt Hill, which is unquestionably the *first*; and that that which is observed at the foot of the Lowe, in the road from Taddington to Miller's Dale and Tideswell, (considered by Farey as the *third* bed,) belongs to the same bed as that which is found nearly at the top of the hill. The whole of the toadstone from Taddington, Blackwell, &c., to the Wye, is thus shown to belong to the *first* or upper bed. Its basseting edge extends southerly by Chelmerton, and northward by Wormhill towards Castleton, being in fact that which Farey has described as that of the *third* bed. This result exactly harmonizes with that obtained at Masson Lowe as above described.

The short time which Mr. H. was able to devote to these investigations last autumn, prevented his entering into a detailed examination of other parts of this limestone district. After the determination of the points above mentioned, however, he did not conceive that the investigation of others necessarily subordinate to them could present the slightest difficulty.

It is manifest from the above statement, that the western portion of this district, instead of being composed of the *fourth* limestone, as described by Farey, is composed of the *second*. Hence it appears how much smaller in elevation any *faults* along the western boundary must be than is represented by Farey. Mr. H. stated that he should be much surprised if, on completing his investigations, he should find any fault coextensive with that boundary.

Mr. H. proceeded to describe the curious formation of the valley of the Wye, and the striking parallelism of the numerous faults in its vicinity. These faults follow the same law as the principal mineral veins in this district, forming themselves in many cases rake veins of this description. Mr. H., in fact, considers these faults and veins to be phænomena of one class, and consequently that they are to be referred to the same *mechanical cause.* Now he conceives that faults such as those described may be shown to be, if not the absolutely necessary, at least the very probable consequences of those forces which are believed by geologists to have produced general elevations of the strata, (as distinguished from merely local ones, with which faults are connected,) similar to that observable in the limestone of Derbyshire. Hence he believes that those fissures also which in the limestone so frequently become mineral veins, are referrible to the same general elevating forces just alluded to. Of the truth of this opinion, Mr. H. stated, that he believes he shall be able to offer much stronger evidence than has yet been brought forward.

This communication led to various observations from several of the Members present.

A Meeting was also held on Thursday, December 5, Dr. Clark being in the chair. Professor Farish gave an account of a large meteor of the nature of a falling star, which he observed on the 10th of September last. Professor Sedgwick afterwards gave an account of the results of his examination of Charnwood Forest, in Leicestershire, in the course of the last summer. The communication was divided into three parts.

In the first part were considered the relations of the Forest rocks to those of the neighbouring districts. The phænomena were illustrated by sections. It was shown, that the coal fields on the west side of the Forest were under the new red sandstone; that the Forest is almost surrounded by a plain of red marl and sandstone, resting unconformably on the edges of the older strata; and that the lias and inferior oolites overlie the red marl in regular order, forming a remarkable feature on the east side of the county. Hence the author asserted, that the works carried on at Billesdon Coplow in search of coal, had been undertaken in entire ignorance of the stratification of the country; and that in a published report by a mineral surveyor, the marlstone and lower oolites (though lying over the regular lias terrace and full of fossils,) had been mistaken for the new red sandstone.

In the second part the author described the rocks of the Forest, showing them to be composed of greenish slate (like the great middle group of Cumberland), alternating with, and seeming to pass into, great tabular masses of porphyry and compact felspar. It was stated that the porphyry abounded at the N.W. corner of the district, almost to the exclusion of the slate. Besides these masses of rock, which are more or less perfectly stratified, there are at several places on the outskirts of the Forest large protuberances of syenite, sometimes passing into true granite. Similar masses break out of the red marl at Enderby, Stony Stanton, and four other places about ten miles south of Charnwood Forest. What the author considered most important was the establishment of a single anticlinal axis, ranging from the neighbourhood of Bradgate Park in a direction about N.W., through the longest diameter of the district. On the opposite sides of this line the beds have opposite dips, and the whole Forest ridge is therefore composed of a single saddle, the sides of which are generally inclined at a considerable angle.

In the third part, Professor Sedgwick briefly considered the date of the elevation of the Forest ridge, and the effects produced by it on the neighbouring country. In the first place, he showed by sections that the forest ridge had been elevated, and that many of the valleys belonging to its actual configuration existed, prior to the deposit of the new red sandstone; secondly, from the position of five highly inclined masses of mountain limestone appearing on a line drawn from Breedon to the N.W. corner of the Forest ridge, that the movement of elevation was posterior to the deposit of the carboniferous series. In short, the five masses above mentioned were stated to dip under the Ashby de la Zouch coal field, and their position to be perfectly accounted for by the prolongation of the anticlinal line above mentioned. Still further to the N.W. the forces of elevation connected with the anticlinal line seem to have produced no sensible effects, as the limestone at Ticknall in Derbyshire is nearly horizontal. Lastly, the author expressed his opinion that the coal field of Nuneaton had been elevated by an undulation of the lower strata, parallel to, and probably of the same date with, Charnwood Forest; and that both these elevations were probably of the same epoch with the disturbing forces which threw up the transition limestone and coal measures of Staffordshire, and produced the configuration of the great coal fields in the south-western parts of England.

UNIVERSITY OF LONDON.

At the commencement of the first Conversazione for the season, Dr. Lindley delivered a lecture upon the nature of the ancient plants, by the remains of which the beds of coal at Newcastle have been formed. His object was to prove, by an explanation of the nature of some of the more remarkable of the plants now found buried in the shale of the coal measures, the truth of the modern opinion, that the scenery of the North of England must in ancient days have been enriched with Palms, Tree-ferns, gigantic Cacti, tropical Coniferæ, and other enormous plants, which are characteristic of the stately vegetation of equatorial latitudes at the present æra. These singular remains were considered sufficient by themselves to establish the correctness of the theory, without having recourse to the apparent preponderance of ferns over all other tribes,—a circumstance which it was stated would probably be found susceptible of an explanation widely different from that usually given. The doctrine of an excess of carbonic acid in the atmosphere, at the time when the coal was deposited, being the cause of the very large quantity of carbon which coal now contains, was alluded to; but difficulties were said to attend that hypothesis, which by many is considered to be more plausible than correct. After the lecture the visitors adjourned to the museum of anatomy, where specimens were exhibited of the anatomical structure of coal coniferæ, and of coal itself, seen under the microscope, together with a spirited sketch by Mr. Hardy of the supposed appearance of the vegetation of Newcastle at the period of the coal deposit.

ROYAL SOCIETY OF EDINBURGH.

Keith Prize.—At the meeting of the Society on Monday, December 2, this biennial prize, consisting of a gold medal and piece of plate, was presented to Mr. Graham for his paper on the Diffusion of Gases.

XVI. *Intelligence and Miscellaneous Articles.*

COMPOSITION OF OIL OF BITTER ALMONDS.

[Continued from vol. iii. p. 392.]

Bromide of Benzoyle.—THIS compound is prepared by mixing hydruret of benzoyle (oil of almonds,) with bromine. The mixture becomes hot, and gives vapour of hydrobromic acid, which, as well as the excess of bromine, is expelled by heat. This bromide is a brownish semifluid crystalline mass at common temperatures: a gentle heat reduces it to a yellowish brown liquid. Its smell resembles that of the chloride, but it is much weaker, and slightly aromatic. Exposed to the air it smokes a little, but when heated intense vapours are given out. It is inflammable, and burns with a sooty flame.

It is slowly decomposed by water: when heated under water, it remains there in the form of a brownish oil. By long boiling it is converted into hydrobromic acid and crystallizable benzoic acid. It dissolves readily in alcohol and æther without being decomposed, and it may be separated by evaporation in the form of a crystalline mass.

Iodide of Benzoyle.—This does not appear to be obtainable by direct action: it is readily procured by heating iodide of potassium with chloride of benzoyle. It distils in the state of a brown liquid, which on cooling becomes a crystalline mass of the same colour: it then retains iodine in solution. When pure it is colourless, crystallizes in tables, fuses readily, but each time it decomposes and yields a little iodine. Its smell, action upon water and alcohol, and combustibility resemble the preceding.

Sulphuret of Benzoyle.—It is procured by distilling chloride of benzoyle with well powdered sulphuret of lead. It has an oily appearance, and becomes a yellow soft crystalline mass. Its smell resembles that of sulphur. It does not appear to decompose by boiling water: when treated with a boiling solution of potash it slowly yields benzoate of potash and sulphuret of potassium. It is inflammable and burns with a sooty flame, sulphurous acid being formed. Alcohol does not decompose it.

Cyanuret of Benzoyle.—Hydruret of benzoyle dissolves some cyanogen and possesses its smell; but the cyanogen is expelled by a gentle heat, without decomposing the benzoyle. The true compound is obtained by distilling chloride of benzoyle and cyanuret of mercury. It has an oily appearance and a golden yellow colour: chloride of mercury remains in the retort. When fresh rectified and pure it is a colourless liquid, but it soon becomes of a yellow colour. Its smell is strong and penetrating and occasions tears. It bears a distant resemblance to oil of cinnamon. Its taste is biting and sweetish, and its after-taste like prussic acid.

It is heavier than water, sinks in it like an oil, and is quickly changed into benzoic and hydrocyanic acids. If a drop remains on the surface of the water, it is found the next day converted into radiating crystals of benzoic acid. Boiled in water it quickly becomes benzoic and hydrocyanic acids. It is inflammable and burns with a very sooty flame.

Benzamide.—When dry ammoniacal gas is passed over pure chloride of benzoyle, heat is given out, gas is absorbed and the liquid is converted into a white solid mass. It is composed of muriate of ammonia and a new substance, *benzamide,* so called on account of the analogy which exists between it and oxamide in their composition and reactions.

It is difficult to saturate the chloride completely with ammoniacal gas, because the solid mass which is produced at the commencement of the action prevents the remainder of the liquid from coming into contact with the ammonia. It is necessary frequently to withdraw the mass from the vessel to press it, and to submit it again to the action of the gas. In order to separate the benzamide, the white mass is first washed with cold water, and the remaining benzamide is dissolved in hot water, and the solution is suffered to crystallize. If the ammoniacal gas is not perfectly dried, benzoate of ammonia is procured, and the formation of benzamide proportionally prevented. When a boiling solution of benzamide is suddenly cooled, it crystallizes in brilliant crystals, like chloride of potash; but if it be slowly cooled, the crystals are needle formed, and have a silky lustre, like those of caflein: after a day or more, great cavities are formed in the crystalline mass, in which there are formed well determined crystals, into which the silky crystals have been changed: this change takes place gradually throughout the mass.

The form of the crystals is a right rhombic prism, subject to variation; they are transparent, of a pearly lustre, and float upon water as if they were unctuous; slightly soluble in cold water, the solution has but little taste; they are very readily soluble in alcohol; boiling æther also dissolves them, and the solution yields regular crystals. At 239° Fahr., benzamide melts into a limpid liquid, which on cooling becomes a coarsely-foliated crystalline mass.

When strongly heated it boils and distils without alteration; its vapour smells rather like oil of almonds; it burns with a sooty flame.

A solution of potash produces no effect upon benzamide while cold, but when they are heated together ammonia is evolved in quantity: cold it has no action upon any metallic salt, but when heated with a solution of iron, subbenzoate of iron is precipitated.

When benzamide is dissolved in a boiling powerful acid, it disappears, benzoic acid crystallizes, and an ammoniacal salt is formed. When concentrated sulphuric acid is employed, the benzoic acid formed sublimes; but when diluted no action takes place.

Benzamide was found to consist of such proportions of its elements as indicated its composition to be

14 atoms carbon	......	107·0118	69·73
12 — hydrogen	....	8·7360	5·69
2 — azote	......	17·7036	11·53
2 — oxygen	....	20·0000	13·05
			100·00

If the vapour of benzamide be passed through a narrow red-hot tube, a small portion of it is decomposed without depositing any charcoal. The greater part goes over without decomposition, mixed with a quantity of sweet oil already mentioned.

Chloride of Benzoyle and Alcohol.—Chloride of benzoyle mixes with alcohol in all proportions; the mixture becomes gradually warm, and eventually so hot as to boil, and evolves dense vapours of muriatic acid. When the reaction is over, by the addition of water an oily body separates, which is heavier than the fluid, and which possesses the aromatic odour of fruit. By washing it in water and treating it with chloride of calcium, it is separated from the water, alcohol, and acid, which render it impure. This fluid was found to be benzoic æther, composed of

Carbon	72·529
Hydrogen	6·690
Oxygen..............	20·781
	100·

	C.	H.	O.
or, 1 atom anhydrous benzoic acid =	14	10	3
and 1 atom æther..................	4	10	1
	18	20	4

and this was found to be the composition of benzoic æther, prepared by distilling a mixture of muriatic acid, benzoic acid, and alcohol. The analysis of benzoic æther by Dumas, differs greatly from this with respect to the hydrogen.

Benzoïne.—This substance has already been noticed by Stange: it is the body mentioned in chemical works by the name of camphoride, or camphor of oil of bitter almonds.

This substance is formed, under certain circumstances, in oil of almonds: 1st, by rectifying the oil with caustic potash; it then remains on the surface of the potash; 2nd, in large quantity by leaving oil of almonds for some weeks in contact with a concentrated solution of potash; 3rdly, by dissolving oil of almonds in a weak solution of potash; in a few days benzoine begins to deposit in crystalline delicate needles. The colour which it has when thus prepared is got rid of by solution in boiling alcohol, and treating it with animal charcoal: by repeated crystallizations it is obtained perfectly pure in brilliant colourless prismatic transparent crystals. Benzoine has neither smell nor taste: it is insoluble in cold water, and but slightly so in hot; and separates on cooling in small crystalline needles. It is more soluble in hot than in cold alcohol. At about 300° Fahr. it melts into a colourless liquid, which on cooling becomes a mass of radiated crystals; when more strongly heated it boils and distils: it burns readily with a sooty flame.

Neither concentrated nitric acid nor a boiling solution of potash acts upon it: sulphuric acid forms with it a violet blue solution, which soon becomes brown, and assumes when heated a deep green colour; but it then disengages sulphurous acid, and the mass soon becomes black: in these properties it resembles the hydruret of benzoyle, and appears to be an isomeric modification of it; and it also gave by analysis

Carbon	79·079
Hydrogen	5·688
Oxygen	15·233
	100·

which is the same atomic constitution.

This substance could not be changed into oil of bitter almonds; but when fused with hydrate of potash, it gave, as the oil does, benzoic acid and hydrogen gas.

The authors of this paper consider the substance which they have called benzoyle, the formula for which is $14\ C + 10\ H + 2\ O$, as a fixed compound element, which preserves its nature and composition when combined with other bodies: thus, combined with an atom of oxygen, it forms anhydrous benzoic acid; and with an atom of oxygen and one of water, the crystallized acid; with two atoms of hydrogen it is the oil of almonds deprived of prussic acid; by exposure to the air it takes two atoms of oxygen, one of which forms benzoic acid with the radical, and the other combining with two atoms of hydrogen, forms the proportion of water.

Chlorine, bromine, iodine, sulphur, cyanogen, may take the place of the hydrogen in the oil, and in the benzoic acid that of the oxygen; and the bodies which result, comparable with the corresponding combinations of those simple bodies with phosphorus, all form when decomposed by water an hydracid and benzoic acid.—*Ann. de Chim. et de Phys.*, li. 273.

The *Annales de Chimie et de Physique*, tome li. p. 312, contains a letter from Berzelius to the authors on the subject of the above paper, in which he considers the discovery of this "compound element" as the commencement of a new æra in organic chemistry.

SCHEELE'S ARTIFICIAL MALIC ACID—OXALHYDRIC ACID.

In our Number for March last we have briefly noticed M. Guérin's experiments on artificial malic acid. The author has continued his experiments, and the results are, that the acid produced by the action of nitric acid and gum differs from vegetable malic acid, and for a reason which will presently appear, the author has called it *oxalhydric acid.* We have given the author's original mode of preparation, but he appears to have improved it, and now states it as follows: Mix one part of gum arabic with two parts of nitric acid of density 1·339, diluted with one part of water; heat the mixture in a retort, the capacity of which is equal to four times that of the mixture, with a tubulated receiver adapted to it; apply heat gently until the gum is dissolved. When nitrous vapours appear, remove the fire, and a copious disengagement of nitric oxide ensues. When this ceases, boil the liquor slowly for an hour, dilute it with four times its weight of water, and saturate with ammonia; then add a solution of nitrate of lime to precipitate the small quantity of oxalic acid formed; the reddish yellow liquor being filtered, precipitate it with acetate of lead; the precipitate is to be thrown upon a filter and washed till the liquor ceases to blacken sulphuretted hydrogen; this precipitate is afterwards decomposed by a current of washed sulphuretted hydrogen gas, or by sulphuric acid diluted with six times its weight of water.

The acid thus obtained is of a yellow colour; it is to be evaporated with a gentle heat, and when sufficiently concentrated, to be neutralized by ammonia, and again evaporated till crystallization begins; the crystals, of a very deep brown colour, are to be purified by animal charcoal. The colourless liquor is to be precipitated by acetate of lead, and again treated, either with sulphuretted hydrogen or diluted sulphuric acid, as already directed. The solution of the acid thus procured is to be evaporated nearly to the consistence of a syrup, and the evaporation is to be finished *in vacuo.*

The properties of the acid are, that it has the consistence of a strong syrup, is colourless, of a taste much like that of oxalic acid. Its density is 1·416. It dissolves in water and in alcohol in all proportions. In æther, whether cold or boiling, it dissolves sparingly; it is very deliquescent, precipitates lime, barytes, and strontia water, but the precipitates are redissolved by an excess of acid. It precipitates the salts of lead and of silver in bulky colourless flocks. By long exposure crystals of oxalhydric acid were obtained, which had properties similar to those of the syrupy solution.

The following are the comparative constitutions of oxalhydric acid by M. Guérin, and vegetable malic acid by Liebig;

	Oxalhydric acid.	Malic acid.
Oxygen	6 atoms	5
Carbon	4 —	5
Hydrogen	6 —	2

One part of oxalhydric acid being mixed with three parts of nitric acid, in a month there were formed, without employing heat, fine crystals of oxalic acid: this acid was afterwards also obtained by slight boiling.

One part of the acid, one of water, two of peroxide of manganese, and two and a half of concentrated sulphuric acid and two of water, when mixed and heated in a retort, yielded formic acid.

The oxalhydric acid forms with:

1st. Ammonia, a neutral uncrystallizable salt, and a bisalt, consisting of 2 atoms acid, 1 atom base, and 1 atom water, and crystallizing in quadrangular prisms with dihedral summits.

2nd. Potash, two salts, one neutral, the other acid, both crystallizable.

3rd. Soda, uncrystallizable salts.

4th. Barytes, ditto.

5th. Strontia, a neutral uncrystallizable salt, and a bisalt, which crystallizes in transparent prisms grouped in crosses.

6th. Lime, a neutral uncrystallizable salt, a bisalt, which crystallizes in quadrangular transparent prisms.

7th. Oxide of zinc, a sesquisalt, containing two atoms of water.

8th. Oxide of lead, a neutral anhydrous salt.—*Journal de Chimie Médicale*, July 1833.

TELLURIUM, ITS PREPARATION, ATOMIC WEIGHT, &c.

Berzelius extracts tellurium from the bismuthic tellurium of Schemnitz by the following process: Reduce the ore to powder and wash it to separate the foreign oxides and earthy substances, which, as they contain tellurium, are not however to be rejected. Then mix the powder with twice its weight of carbonate of potash, and make the mixture into a paste with olive oil, and put the whole into a well-covered crucible. Heat, at first cautiously, and then to full redness; and when flame ceases to appear between the crucible and its cover, allow it to cool. The cooled mass is not fused, but is porous and of a dark brown colour: powder it, and wash it on a filter with water which has been well boiled and suffered to cool. The powder remaining on the filter consists principally of charcoal and bismuth, with some tellurium. The filtered liquor is a solution of an alloy of tellurium and potassium in water: it is at first opake and purplish red. When air is blown into it by the bellows, the potassium is oxidized and the tellurium precipitated. There remains in the alkaline liquor a small quantity of sulphuret of tellurium and seleniuret of tellurium, which may be precipitated by muriatic acid. The deposited tellurium is then washed with boiling water, dried and fused: it is then to be put into an oval porcelain vessel, placed in a tube of the same material: it is to be heated to redness and a current of hydrogen gas directed upon it: it is afterwards to be distilled.

M. Berzelius found the specific gravity of tellurium greater than hitherto observed. The reason he states to be, that the metal contracts much on cooling, when it is rapidly effected, and the surface will then support the pressure of the air: there are thus formed cavities containing no air, which are seen when the metal is broken.

This property it possesses in common with selenium. If the tellurium is allowed to cool slowly, the air usually penetrates the upper surface, and in the centre there is a large cavity, from around which fragments may be taken that contain none. The mean of six trials gave M. Berzelius 6·2445 as the specific gravity; and he found the atomic weight to be 802·121.

Tellurium combines with two proportions of oxygen to form tellurous and telluric acid: the former is already known as oxide of tellurium. It has two isomeric modifications: one of them is produced at a moderate heat, and the other by the actions of the alkalis upon it. Berzelius distinguishes them by the letters A and B: the latter differs from the former in dissolving readily in acids, in ammonia, and the alkaline carbonates, from which it expels the carbonic acid. Tellurous acid forms peculiar salts; but M. Berzelius, at present, knows only those formed by the modification B. They are of various degrees of saturation, that is to say, they contain an atom of base, combined with 1, 2 and 4 atoms of acid: the last are most readily formed; those with alkaline bases crystallize.

Telluric acid is very imperfectly formed in the humid way with aqua regia; but it is produced in the dry way, by treating tellurous acid with nitre. That it has not been before discovered, is owing to the circumstance that the tellurate formed offers an unexpected isomeric modification, and is partly transformed into tellurite, when too strongly heated. The following is the best method of preparing telluric acid: Fuse a mixture of equal parts of potash and tellurous acid: dissolve the salt in water, and add hydrate of potash equal in quantity to that employed, or rather more: pass a current of chlorine into the liquor, until the turbidness and precipitate at first occasioned are redissolved, and the liquor smells of chlorine. Add a little muriate of barytes, and filter if any precipitate be formed; this is either sulphate or seleniate of barytes: then pour ammonia into the solution, until it is slightly in excess, and precipitate the tellurate of barytes [potash?] by muriate of barytes. The precipitate is at first bulky, but soon becomes granular and falls to the bottom of the vessel: if this does not occur it is because it contains tellurite of barytes. The salt is then washed, and digested with a quarter of its weight of concentrated sulphuric acid previously diluted with water: filter the liquor, evaporate the greater part of it in a salt-water bath, and afterwards spontaneously: the acid crystallizes in flat hexahedral prisms, terminated by a flat four-sided pyramid. When the sulphuric acid is in excess, the crystals are more perfect: this acid may be removed by concentrated alcohol.

In this state telluric acid contains 23·5 per cent. of water, which it retains at 212°; at a higher temperature, but below redness, it loses two thirds of the water, without destroying the crystalline form: the acid is then difficultly soluble in water, but it gradually dissolves by long boiling. At a high temperature it loses the last third of its water, and there then remains a lemon yellow powder, which is insoluble in any fluid: it is an isomeric modification of telluric acid, corresponding with the modification A of tellurous acid; it differs from it in giving peculiar and very different salts from those yielded by the acid soluble in water, or modification B.

When telluric acid is exposed to a still higher temperature, it is decomposed, gives oxygen gas, and leaves tellurous acid as white as snow. Tellurous acid consists of one atom of radical and two atoms of oxygen, and telluric acid of one of the former and three of oxygen, or 72·78 of metal and 27·22 oxygen. Like the tellurous acid, it has a tendency to form salts with one, two and four atoms of acid to one of base, and the salts of modification B, are at first converted by heat into those of A: they then, at a red heat, give out oxygen, and become tellurites.

Telluric acid is not decomposed by sulphuretted hydrogen; it may consequently be obtained by decomposing tellurate of lead by sulphuretted hydrogen and water: but if a weak solution of telluric acid in water, be saturated with sulphuretted hydrogen, and kept well stopped in a warm place, sulphuret of tellurium is deposited, which usually covers the interior of the bottle with a blackish grey crust of a metallic lustre: it is easily detached.—*Journal de Pharmacie*, Nov. 1833.

NEW SUBSTANCE IN OPIUM.

M. Pelletier has announced the discovery of a new crystalline substance in opium, which is isomeric with morphia, and which he calls *Paramorphia*. This substance differs essentially from morphia in its chemical properties, although its composition is similar: it cannot be confounded with codein, nor any of the other crystalline bodies found in opium: its taste is analogous to that of pyrethrum. It is infinitely more soluble in æther and alcohol than narcotine is; it differs also from the last mentioned by its fusibility and crystallization. It has a very marked action on the animal œconomy; and in very small doses it kills a dog in a few minutes. M. Magendie has shown that it acts upon the brain and occasions convulsions.—*Journal de Chimie Médicale*, Mars 1833.

DISCOVERY OF FOSSIL FISH, THE TOOTH OF A SAURIAN REPTILE, AND OTHER REMAINS, IN THE LIMESTONE OF BURDIEHOUSE, NEAR EDINBURGH: WITH REMARKS BY THE REV. W. D. CONYBEARE.

Dr. Hibbert has recently read before the Royal Society of Edinburgh his description of the limestone bed of Burdiehouse, about four miles to the south of Edinburgh, which forms an inferior bed of the coal measures in the neighbourhood of Loanhead. This limestone was shown to differ materially from the common carboniferous limestone of marine origin, and to form a species of deposit hitherto undescribed by geological writers, being not of a marine but of a fluviatile character. While proofs were thus adduced that the limestone bed of Burdiehouse indicated the existence of a lake, or of some fluviatile expanse within which calcareous matter was elaborated, it was likewise explained that its animated tenants were fresh-water fish, resembling the Cyprinidæ. An interesting fragment of one of these fish, first discovered by Dr. Hibbert, was exhibited to the Society, which, in its entire state, could not have been less than a foot in length. There also appears to be in this deposit an immensity of very minute crustaceous and shell animals. One species of the crustaceous kind Lord Greenock conceived to resemble the *Cypris Faba*; but there seems to be more than one description of these minute animals which, like the Cypris, are referable to the Entomostraca of fresh-water lakes and marshes.

Besides these animals, a remarkable variety of fossil plants, imbedded in the limestone, were exhibited, similar to such as are discovered in coal fields, and indicative of the vegetation of a tropical country. Of these the most abundant appeared to be the *Sphenopteris affinis*, first found by Mr. Witham, in the quarries of Gilmerton; and another plant, resembling the *Lepidostrobus variabilis* of Professor Lindley and Mr. Hutton; but it is to be hoped that the more perfect specimens of this latter plant which are to be found in the Burdiehouse limestone, may serve to decide its hitherto dubious botanical character. Mr. Witham's attention has been invited to this circumstance, as well as to some other vegetable remains, apparently monocotyledonous.

These were the principal results communicated to the Royal Society of Edinburgh, relative to this most interesting fresh-water limestone. But we have now the satisfaction to make known a subsequent and still more remarkable discovery which has taken place. On the morning after this communication was made, Dr. Hibbert, in company with Mr. Witham, revisited the quarry, and in the course of this visit one of the workmen accidentally found inclosed in a fragment of the rock a tooth, two inches and a quarter in length, of a large reptile, evidently referable to the Saurian order; this relic being in the most beautiful state of preservation, and having an enamel shining as if perfectly fresh. It was also observed that the limestone abounded with substances resembling coprolites, which gave encouragement to the expectation that many more remains of these Saurian animals will turn up during the process of quarrying. On this account we cannot refrain from recommending to the Royal Society of Edinburgh, or to the Patrons of the College Museum, that every encouragement should be given to the labourers of Burdiehouse quarry, to preserve in a state as entire as possible any further relics of this kind which may be discovered during the process of quarrying.

This discovery is one of the most important which has been lately made in geology. It refers the existence of reptiles allied more or less to the crocodile, to a period much earlier than has been generally supposed by geologists, and at the same time shows that these immense animals must have existed coeval perhaps with the very earliest vegetable state of our globe.

We cannot close these remarks without congratulating the student of natural history upon the discovery of a deposit of such interest as that of the Burdiehouse limestone. It is not exceeded in importance by any other ossiferous bed which has yet been described, and it gives new features to the striking geology of the vicinity of Edinburgh.

Mr. Conybeare, in transmitting an account of this discovery, observes, that the formation here described may very possibly be closely related to the bituminous schist, containing shells, apparently fluviatile, and fish, discovered in 1830 by Messrs. Murchison and Sedgwick, in Caithness, and by them considered as occupying the relative position of the carboniferous group. This formation has been fully described by them in the Geological Transactions; and the fish discovered are there carefully figured, with which it will be interesting to compare those of Burdiehouse. With regard to the discovery of Saurian remains in rocks of this age, Mr. Lyell has already observed, (vol. i. p. 129,) that amongst other fossils collected from the mountain limestone of Northumberland, the Rev. Charles V. Vernon had been fortunate enough

Unius sese dominum fecisse lacertæ,

having found there a Saurian vertebra.

Although in England the lias is the oldest formation which has presented any very decided or abundant specimens of Saurian animals, still, on the Continent, especially around the Thuringer wald, they are commonly found in the cupriferous schist coeval with our own magnesian limestone: these remains were referred by Cuvier to the genus Monitor, and here also they were associated with the remains of fresh-water fish.

As the whole formation of Zechstein (our magnesian lime) is immediately next in order of age to the carboniferous group,—as the cupriferous schist is its lowest member, and the fossils of the formation generally present an intermediate link between the inferior carboniferous and superior muschelkalk and lias, we cannot be much surprised by any new instance of analogy between the said schist and the coal measures.

SCIENTIFIC BOOKS.

A Geological Manual, (Third Edition). By H. T. De la Beche, F.R.S. V.P.G.S. &c.

Mathematical Tracts. By J. W. Lubbock, Treas. R.S.

The Transactions of the Zoological Society, Part I. vol. i.

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Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. R. HOCKING at Penzance, and Mr. VEALL at Boston.

Days of Month, 1833.	Barometer.					Thermometer.					Wind.			Rain.		
	London.		Penzance.		Boston	London.		Penzance.		Bost.	Lond.	Penz.	Bost.	Lond.	Penz.	Bost.
	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	8½ A.M.						
Nov. 1	30·072	29·814	30·054	29·910	29·12	62	41	56	48	60	W.	NW.	calm	...	...	...
2	30·043	29·705	30·140	30·066	29·50	57	45	54	44	49	SW.	NW.	calm	...	...	...
3	29·986	29·740	30·366	30·059	29·03	53	33	55	43	49	W.	NW.	NW.	...	...	...
4	30·295	30·152	30·392	30·378	29·60	51	29	56	45	41·5	W.	W.	calm	...	...	...
☽ 5	30·304	30·126	30·146	29·852	29·68	55	48	55	42	43	SW.	NW.	calm	0·10	...	...
6	30·017	29·990	29·672	29·358	29·35	57	41	55	42	54	SW.	W.	NW.	...	...	0·32
7	29·982	29·511	29·449	29·152	29·29	46	32	56	45	45	SW.	NW.	W.	1·02	0·260	...
8	29·908	29·711	30·045	29·856	29·30	46	36	50	44	36	NW.	SW.	N.	...	·460	...
9	30·136	30·019	30·055	30·048	29·58	50	30	55	46	36	W.	NW.	N.	...	...	...
10	30·156	30·111	30·025	29·831	29·63	53	32	55	51	43·5	SW.	SW.	calm	...	·175	·02
○ 11	30·113	30·067	29·916	29·849	29·55	50	40	58	44	48·5	SW.	W.	W.	·03	·410	...
12	30·277	30·236	30·209	30·109	29·43	50	39	55	47	44	S.	SW.	calm	...	...	·03
13	30·309	30·249	30·152	29·955	29·83	50	39	54	45	42	E.	SW.	calm	·02	...	...
14	30·184	30·152	30·048	29·988	29·72	43	30	50	43	41	SE.	W.	E.	...	...	...
15	30·078	29·955	29·968	29·874	29·70	45	33	55	44	42·5	S.	S.	calm	...	...	...
16	29·936	29·894	29·725	29·728	29·45	50	34	50	43	38·5	S.	S.	calm	...	...	...
17	30·176	30·031	29·988	29·841	29·35	55	44	50	47	47	S.	SW.	calm	·02	...	·12
18	30·295	30·277	30·248	30·178	29·33	53	47	53	48	47	SW.	S.	calm	...	...	...
☾ 19	30·264	30·114	30·122	30·108	29·64	51	37	55	46	49	SW.	E.	calm	...	...	...
20	30·141	30·040	30·108	30·068	29·62	49	41	51	46	41	W.	E.	calm	...	...	...
21	30·055	29·973	30·055	29·828	29·55	59	48	56	45	44	S.	SW.	calm	...	...	...
22	29·676	29·458	29·718	29·691	29·05	55	48	55	50	52	SW.	W.	W.	·02	...	·02
23	29·733	29·716	29·778	29·734	29·12	48	37	56	43	43	SW.	W.	calm	...	·430	...
24	29·715	29·460	29·637	29·540	29·14	51	31	51	44	42	SW.	NW.	calm	·61	·520	...
25	30·001	29·705	30·043	29·796	29·25	41	24	50	40	36	W.	NW.	calm	...	·230	...
26	30·186	30·116	30·076	29·909	29·74	44	31	48	38	31	S.	W.	calm	...	...	...
● 27	29·894	29·632	29·553	29·346	29·53	50	30	51	40	39·5	S.	S.	S.	·03	·670	...
28	29·424	29·175	29·343	29·129	29·09	52	43	53	40	38	S.	SW.	calm	·53	·690	...
29	29·715	29·231	29·853	29·616	28·65	46	35	55	42	45	SW.	W.	W.	...	·820	·35
30	30·135	30·043	30·100	30·073	29·52	51	46	50	44	41	SW.	W.	calm	...	...	...
	30·309	29·175	29·963	29·828	29·41	62	24	58	38	43·6				2·38	4·665	0·86

Remarks.

London.—November 1—3. Fine. 4. Clear and frosty. 5. Windy with rain. 6. Cloudy: fine. 7. Very heavy rain, falling to the depth of upward of an inch. 8, 9. Fine. 10. Overcast. 11. Rain. 12. Dense fog. 13. Foggy. 14. Frosty: fine. 15. Overcast. 16. Fine. 17. Foggy. 18—20. Foggy. 21. Fine. 22. Cloudy and windy. 23. Fine. 24. Fine: heavy rain. 25. Frosty. 26. Sharp frost: fine. 27. Overcast. 28. Fine in the morning: stormy and wet at night. 29. Clear. 30. Fine: overcast.

Penzance.— November 1. Fair: showers. 2—4. Fair. 5. Misty: fair: showers. 6. Heavy showers. 7. Showers: faiir: rain at night. 8. Fair: misty. 9. Rain: fair. 10. Rain. 11. Fair. 12. Fair: showers. 13, 14. Fair. 15. Clear. 16. Fair. 17. Misty. 18. Misty: fair. 19. Fair: slight showers. 20. Fair. 21. Fair: foggy. 22. Fair: rain at night. 23. Showers: heavy rain at night. 24. Rain: fair. 25. Hail and rain. 26. Fair: rain at night. 27. Rain: very stormy. 28. Very stormy: rain. 29. Fair: showers: stormy. 30. Fair: rain.

Boston.—November 1. Cloudy: stormy P.M. 2. Cloudy. 3. Stormy. 4. Fine. 5. Rain. 6. Fine. 7. Cloudy. 8, 9. Fine. 10. Cloudy: rain early A.M. 11. Cloudy: rain A.M. 12, 13. Foggy. 14. Fine. 15, 16. Cloudy. 17. Cloudy: rain early A.M. 18, 19. Cloudy. 20. Foggy. 21. Cloudy. 22. Cloudy: rain rain early A.M. 23—27. Fine. 28. Cloudy: rain P.M. 29. Stormy. 30. Fine.

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XVII. *Notice of an Instrument for ascertaining various Properties of Terrestrial Magnetism, and affording a permanent Standard Measure of its Intensity in every Latitude.* By Robert Were Fox*.

INTERESTING as is the subject of terrestrial magnetism, our acquaintance with many of its phænomena is very limited, owing principally to the imperfection of our instruments, and the time and minute attention which the best of them require, to enable us to obtain results which can be considered even as approximations to accuracy. Impressed with these defects, I have directed some attention to the subject, and have succeeded in constructing an instrument, which I think will be found highly useful in the prosecution of magnetical researches in any part of the world.

It consists of a dipping-needle with a transverse axis, and pivots working in jewel holes, like those used for the balance of a chronometer: the needle is contained in a vertical box of the usual form, having a concentric brass plate inserted in the back, which admits of its being turned round on its axis independently of the box. The jewels are set precisely in this axis, one in the concentric plate itself, and the other in an arm connecting the axis with the plate. This contrivance admits of the needle's taking any direction without interruption by the arm, which is moveable by pins affixed to the back of the plate. In the construction of this instrument, every precaution usually taken for the true adjustment of a dipping-needle should be observed, the friction of the pivots

* Communicated by the Author.

being overcome by a slight vibration of the concentric plate. This vibration may be produced by gently tapping the plate at the back, or the latter may be indented, so that the passing of a brass rod once or twice rapidly over its surface may, by the friction produced, effect the desired object. After a little practice I have found that this plan causes the needle to take its natural bearing in the most satisfactory manner, and at least to equal in delicacy any other method of mounting it. The perpendicular support of the box rests on a horizontal plane or base, and can be turned round on its axis in any direction, and the needle be made to face any point of the compass without altering the base of the instrument, which is of course furnished with levels and adjusting screws in the usual manner. Besides the graduated circle on the face of the box, I use another, coinciding with it, which is placed immediately within the glass to direct the line of sight, and to enable the observer to ascertain with great minuteness the place of the needle. For example, if the outer circle be twelve times more distant from the indicating mark on the needle than the latter is from the inner circle, it is evident that any visual lines crossing at the mark, will be separated from each other at the circles in the ratio of the relative distances of the latter from the mark; so that a degree on the outer circle will in this way represent one twelfth, or five minutes of a degree on the inner one. This mode of division need only be limited by our means of observation.

The plane of the magnetic meridian may be easily found without a horizontal compass, by moving the face of the instrument round till the needle becomes vertical (after vibration), which will be its position when it is exactly opposite the magnetic north or south, so that the needle will be in the plane required when turned 90° from that point. If the base of the instrument be furnished with a graduated scale round the circular edge of the support or axis on which the box revolves, and the axis itself with a vernier, or *vice versâ*, it will enable us to ascertain the variation of the magnetic from the true meridian with great precision. If from defects in the construction of the needle (to which all are liable,) its magnetic axis and centre of gravity should be so untrue as to prevent its being vertical when placed at right angles to the plane of the magnetic meridian, the error will be shown, and its extent detected, by repeating the observations with the face of the instrument turned towards the opposite quarter; for in this case, if the azimuth distance, when the needle again becomes vertical, should exceed or fall short of 180°, the difference will show the correction that must be applied. This property of self-correction, which may be relied upon for the certainty of its results, gives this mode of taking the magnetic variation a decided advantage over the common method with the azimuth compass.

The true meridian may, of course, be found in the same manner as in the azimuth compass; but as the vertical box has a graduated circle at its back agreeing with those within it, and a fixed pivot, the axis of which is in the common centre, (for reasons immediately to be stated,) I intend to adapt to it a tube on the principle of those used in theodolites, having a broad flanch at each end, the solar rays being transmitted through slits at right angles to each other in the flanch at the object end, and received by the other at the opposite end, which is faced with ivory to render the lines of light more distinct; and I may fix to one or more of these slits the segment of a glass cylinder, as adopted by Capt. Kater in his azimuth compass, for the purpose of increasing this effect. This tube, being fitted to a bar which turns on the central pivot, and furnished with verniers at each end, suited to the graduated circle, will enable the observer to find the sun's *meridian* and *altitude* at the same time. The true adjustment of the tube may be ensured, or any needful corrections ascertained, by taking the observations with the face of the box turned in opposite directions. The inside of the tube may be provided with cross wires and glasses, so as to render the instrument a good theodolite, if desired.

But the most valuable property of the instrument, because the most wanted, is the facility with which it will indicate the intensity of the earth's magnetism in every latitude. To accomplish this object, steel magnets are employed to deflect the needle from its natural dip, the greater or less intensity existing at the place of observation being determined by the extent of the deflection. I have employed two magnets, each three inches in length (or about one half that of the needle), and after having been exposed to the heat of 150° at least, I have inclosed them in two brass tubes. These are made to slide in a larger tube, six inches in length, on the principle of the spy-glass, one being fixed at each end, so that the magnetic poles alternate with each other. The large tube is perforated through the middle, and exactly fits the central pivot at the back of the box, where it may be confined in its place by means of a nut and screw at the end of the pivot, or taken off at pleasure. This arrangement admits of the tube or deflector being turned round in a vertical plane parallel to that in which the dipping-needle moves, and of its being firmly fixed in any required position within that plane.

The deflector is furnished with verniers at its extremities. To ascertain the magnetic intensity, let the deflector (with the tubes closed, for example,) have its poles turned round so as to coincide as nearly as possible with the dip of the needle at the place of observation; the latter will be deflected either to the right or left (the amount of this deflection should not, I think, be much less than 60° or 70°); produce vibration and then note the place of the needle, observing it at both ends. Draw out one of the small tubes for a moment, till the needle swings back beyond the line of the dip; then quickly return the tube to its former position, and the needle will be deflected in the opposite direction: half the sum of these deflections will represent the intensity of the terrestrial magnetism at the place of observation. This should be repeated with the face of the box turned the opposite way, the deflector being made to coincide with the dip as before. The small tubes and the deflector being turned round 180°, the observations should be again repeated in like manner, and will tend to correct the previous results. In this case it is evident that the poles of the deflecting magnets are opposed to the direction of the terrestrial magnetism, whereas before they coincided with it, and its influence on the force of the magnets becomes hereby compensated. The deflectors may be employed in attracting instead of repelling the needle, by being placed at right angles or at any other given angle to the dip; but there are many reasons, I think, for preferring the former. The deflector may also be used with great advantage in correcting the observed dip, inasmuch as the force of the earth's magnetism on the needle increases in the ratio of the sine of the angle of deflection. This it may effect in different ways; but a small deflection appears to me to give the most satisfactory and decisive results; and the force of the deflector may in this case be modified by drawing out both tubes, more or less, and adjusting it at any given angle on each side of the dip. The effect of temperature on the deflector and dipping-needle may be easily ascertained by covering the instrument with a double metallic case, containing water at any required temperature, an opening being left in it for the purpose of observation, which should be covered with glass. It will, however, be sufficient for the maker, to ascertain what corrections should be made, as it respects each instrument, on this account. I have employed such an apparatus, and found that a needle repelled by a deflector at an angle of 61° 30′ from it when the temperature was 52°, stood at 61° 15′ when the temperature was raised to 95°, and returned to 61° 30′ on cooling; so that the amount of deflection was reduced only $\frac{1}{243}$th part by an increase of temperature equal to 43°. It may here be remarked, that the effect of temperature on the reciprocal influence of the deflector and needle, is in part compensated by a corresponding change of the effect of the earth's magnetism on the latter. I am about to apply to my instrument a simple contrivance for limiting the distance to which the magnetic tubes may be drawn out or pushed in, according to the temperature existing at the time; but its influence is so inconsiderable, and corrections can be so easily applied to the results, that it does not seem necessary to describe it. It is always desirable to note the temperature at the time of observation, and a small thermometer is affixed to the face of the box for this purpose.

I am aware that a vibrating needle or any moderately light body in constant motion, is liable to be much affected by temperature in consequence of the mechanical* action of the air, in which currents are produced by the application of warmth; thus experiments on the oscillations of the needle, and even of the pendulum in a small degree, are liable to more or less uncertainty on this account, the anomalous and fluctuating results obtained from the former being often very remarkable.

When the apparatus is not in use, the ends of the needle are received into sockets attached to brass springs fixed to the side of the box, and which may be pushed forward by screws passing through it. The deflectors at the same time should be turned round so as to be parallel to the needle, but with its poles reversed. With this precaution, I have reason to believe that the magnetic forces will, within a moderate time, become sufficiently permanent for making satisfactory comparisons relative to the magnetic intensity in different parts of the world: at any rate, if there should be any diminution of these forces after the instrument has been completed twelve months, it will, (protected as the magnets are,) I believe, be very inconsiderable, and its ratio continually diminishing†.

* This source of error having been referred to by others, I think it proper to say that I believe I was the first person to announce it, early in 1832. There is also a paper of mine on the subject since inserted in the Lond. and Edinb. Phil. Mag. vol. i. p. 310.

† In corroboration of this opinion, I have found that a horizontal magnetic needle, or rather bar, coated with sealing-wax, and suspended by unspun silk in a box, in my underground cellar, nearly three years ago, has suffered no sensible change of intensity in the last twelve months; or at least, the time in which it performs a given number of vibrations does not differ so much as *one second* in 1434″. The temperature there fluctuates very little, and it was at 52° when these comparative results were obtained.

The instrument in question will, however, admit of the application of an unequivocal test of any change in its magnetic force.

If the dipping-needle be made to assume a horizontal position, by the proper adjustment of the deflector, or by weights applied to its southern arms, it will retain very nearly this position, whatever way it may be turned; and it would quite do so, were it not for the action of the terrestrial magnetism on the deflector and needle, which tends to produce some modification of their forces, in a greater or less degree, according to their relative directions: for example, if the north pole of the needle be deflected upwards from its natural dip in this latitude when in the plane of the magnetic meridian, by raising the north pole of the deflector under it (with the deflecting tubes closed,) till the needle is perfectly horizontal and pointing towards the north, both the needle and deflector will have their forces augmented by the action of the terrestrial magnetism; and the latter more so than the former, because it more nearly approaches the direction of the magnetic currents. Turn the face of the instrument round 90°, when the terrestrial magnetism will have no effect in modifying the force of the horizontal needle, though it will still add to that of the deflector; in this case the needle will have a tendency to suffer a slight depression. Let the instrument be turned 90° further round till it is again in the plane of the magnetic meridian, but with the north end of the needle pointing towards the south; the earth's magnetism will in this case tend to diminish the force of the needle and of the deflector also, if it be at a greater angle from the horizontal line than the complement of the dip, and the depression of the north pole of the needle will probably not be less, and may be more considerable than when the instrument was at right angles to the plane of the magnetic meridian *. This source of error may, however, be compensated by reversing the direction of the deflector, and drawing out the tubes: so that the mean results obtained in both these ways, when the needle is at right angles to the plane

* The modifying effect of the terrestrial magnetism is rendered more apparent by attaching small weights to the north pole of the needle, and raising the deflector till the needle becomes horizontal.

Does it not seem to follow that the results obtained by the vibrations of a magnetic needle are influenced, not only by the attraction of the terrestrial magnetism, but also by an adventitious force, which it imparts to the needle in a greater or less degree in proportion as their relative directions are more or less coincident with each other? I do not know whether this question has been duly considered, but it appears to me to be of some importance that it should be taken into account in making comparative observations in different latitudes with the horizontal vibrating needle.

of the magnetic meridian, may be considered as independent of any modification arising from the earth's magnetism.

Let the north pole of the needle be deflected above the horizontal line, the deflector being fixed at some given angle from it: suspend minute weights from the northern arm at any given point, say, for instance, midway between the centre and extremity of the needle, till it is reduced to the horizontal position. The weights required to effect this will represent the force of the deflection *minus* the directive influence of the earth's magnetism on the needle so circumstanced. Remove the weights, and raise the north pole of the deflector as many degrees above the horizontal line as it was before below it, when the deflecting force will cooperate with that of the terrestrial magnetism, and the weights required to render the needle horizontal, if applied to the corresponding side on the southern arm, will be more than in the former case at every place north of the magnetic equator. One half the *excess* or *difference* in the weights will represent the directive force of the terrestrial magnetism, acting on the needle in question when horizontal, and half the *sum* of the weights employed on both arms of the needle will indicate the force with which it is deflected under the actual circumstances of the case. The latter will be a constant quantity, (*i.e.* the mean of the results obtained with the deflector turned in opposite directions and the tubes closed, and afterwards drawn out, as before described,) whilst the forces of the needle and deflector remain unchanged; and if they should have undergone any modification, its extent may be immediately detected by taking one half the weights, however they may be divided, between the two arms. The former quantity or difference of weights on the two arms will be ever varying with the obliquity of the earth's magnetism, and its increased or diminished intensity. The deflecting force, therefore, becomes, I conceive, a standard of comparison for every change which may occur, and the causes of which may hereby be distinguished from each other. Suppose, for instance, the deflecting forces to remain unchanged, and the directive influence of the terrestrial magnetism on the needle to be increased, the southern arm will require an increase, and the northern a diminution of weight; but if the terrestrial force should be diminished, the case will of course be reversed as it respects the weights, their sum remaining the same. If the deflecting forces only, and not the terrestrial magnetism, be increased or diminished, the weights must be increased or diminished on each arm, the proportions of which would be equal, were it not that the directive force of the terrestrial magnetism on the needle will undergo some corresponding change, which will bear a constant ratio to the change in the deflecting forces, the extent of which is known.

If the forces of deflection and of the terrestrial magnetism should both change, then, from what has been said above, it appears that the relative amount of each can be estimated.

In the foregoing examples, I have assumed that the deflector and needle suffer equal changes of intensity; and this, I think, they will do (if they change at all), after some time, when they have found their level: but it is well to provide for any contingency; therefore, to detect any alteration which may take place in their relative forces, it is only necessary to substitute the deflecting magnets for the needle, and to allow the latter to deflect the former; and this can be accomplished by inserting the ends of the magnets in two sockets, opposite to each other, and furnished with a transverse axis like that of the needle, so that when balanced on the jewels, the combined magnets may become a dipping-needle, and the amount of its deflection, caused by the needle when properly adjusted in connexion with the verniers at the back, will indicate any change in its force, if such should have occurred.

It may be remarked, as it respects these combined magnets, as well as the dipping-needle itself, that any error in their construction will be always constant when their position is horizontal, and that therefore it will not affect the value of their indications.

It is now more than a year and a half since Watkins and Hill, No. 5, Charing Cross, made my first dipping-needle with a deflector; and I showed it to several scientific individuals in London, and also at Oxford, when I attended the meeting of the British Association in that city. I afterwards had a larger one made in Cornwall, and Watkins and Co. are now constructing another, in which I intend to have some improvements made in the method, above described, of attaching the deflector, &c., to the back of the instrument.

It is evident that needles, balanced with knife-edges resting on agates, may be used in the manner before mentioned, for obtaining a permanent standard measure of the magnetic intensity; but although this method may be preferred by many to the chronometer one, for local use, I think that it will not be deemed so eligible for *locomotive* purposes.

XVIII. *Remarks on* Prof. Moseley's *Principle of least Pressure. By* S. Earnshaw, *B.A., Fellow of the Cambridge Philosophical Society.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

NOT being fortunate enough to comprehend fully the demonstration of the new statical principle, distinguished as "the Principle of least Pressure," which Mr. Moseley communicated in your Journal of last October, I have been waiting since its appearance in the hope that some gentleman in a subsequent Number would have taken notice of it: but this not having happened, and there appearing in your Magazine of the present month a further communication from Mr. Moseley in continuation of the same principle, but not containing any additional elucidation of the previous demonstration, I have ventured, not wishing to remain longer in doubt of the truth of a principle so curious and interesting, to make a few observations on the subject, in the hope of eliciting from Mr. Moseley such a reply as may clear both the principle and its demonstration from the obscurity with which, to me at least, it appears to be somewhat clouded. The remarks I have to make are principally upon the following sentence of Mr. Moseley's first communication: "Now each force of the system C, under these circumstances, *just* sustains and is equivalent to the pressure propagated to its point of application by the forces of the system A; or it is equivalent to that pressure, *together with* the pressure propagated to its point of application by the other forces of the system C." Now it appears to me that, speaking generally, it is impossible that *each* force of the system C shall just sustain *only* such pressures as are propagated to it by the system A; unless either that the system C consists of but *one* force, or that all the forces of which it consists are *parallel*. For if there be more forces than one in the system C, and if they are not parallel, their actions *must* of necessity mutually propagate pressures. Wherefore in either of these cases (which to me appear the only ones to which the former part of the paragraph quoted can apply,) it is manifest that the remaining part of the paragraph cannot apply; for there is no pressure propagated by the forces of the system C, under those suppositions, to each other. As what I have quoted from Mr. Moseley contains the premises of the syllogism of which his demonstration consists, if what I have said upon it be found to be correct, the problem of pressures must remain in as great mystery as heretofore. Admitting, however, that I have taken a wrong view of the matter, (which I confess is not improbable, since what I have written is so very obvious that it must have occurred to Mr. Moseley and been considered by him invalid,) I cannot comprehend *why* we are to consider the forces as *functions of the coordinates of the points at which they act.* If there be a reason, it most certainly does not appear on the face of the demonstration of the principle.

In Mr. Moseley's communication to your Journal of this month, he mentions, as a valuable instance for showing the incorrectness of the notions which have hitherto been held on the subject of pressures, the case of a mass supported on three props, at the angular points of a triangle, whose centre of gravity is in the same vertical line as that of the given mass. The pressure upon each prop in this case, as is well known, is equal to one third of the weight of the body supported. "Now," says Mr. Moseley, "this condition continuing to be satisfied, let us suppose the third point of support to move into the same with the other two. The fraction $\frac{0}{0}$ expressing the evanescent ratio of each elementary triangle to the whole triangle, will then *manifestly* have the value $\frac{1}{3}$. And three points of support in the same straight line will each of them sustain the *same* pressure." I am, however, of opinion that the conclusion from this reasoning is not so manifest as Mr. Moseley seems to think it is; for let A, B, C be the angular points of the triangle in its *finite* state, then if we inquire into the cause of the pressure at any one of them (suppose C), we shall find that it arises from the circumstance that the side AB is in a certain respect a *fixed axis*, about which the body has a *tendency* to move, and about which it is only prevented from moving by the pressure of the prop at C: as soon, therefore, as the points of support are brought into one line, AB, in which of course, from the nature of the hypothesis, the centre of gravity is situated, the body *ceases* to have a tendency to move round AB, and the office of C is abolished, and the case of the triangle is not applicable to this.

To put this idea in another view, let the body be supported on three props, A, B, C, of which A, B are so situated that the line joining them passes through the centre of gravity, and C is situated without. Then the body will balance upon AB, and there will be *no pressure* at all upon C; for if C exerted any pressure, it would overthrow the body by turning it round AB. Let now C come into the line AB, and have the precise situation assigned it by Mr. Moseley in the instance mentioned by him, and (if we may be allowed to stretch the reasoning of a finite state to an evanescent state,) there will still be *no pressure* on C; which is directly at variance with Mr. Moseley's results.

In confirmation of my views, I beg leave to lay before your readers the following principle, which I have had numerous opportunities of confirming. If y, x be two quantities such that $y = \phi x$, and if θ be an *independent* quantity which comes into the *investigation* of the equation $y = \phi x$ of necessity, although it does not appear in the resulting equation itself, then, notwithstanding the equation $y = \phi x$ is true in *general*, there may happen to be values of θ which cause some of the steps of the demonstration to vanish or become impossible; and in such cases the equation $y = \phi x$ is not necessarily true, though it may happen to be so. This principle I have found to be most frequently necessary when ϕx = constant. I shall add two instances.

1st, If a plane be immersed in a fluid at an angle, θ, with the surface, the coordinates of its centre of pressure are constant, whatever be the inclination. But if the inclination be diminished without limit, that is, when the value of θ is 0°, the coordinates of the centre of pressure are no longer of the same constant value as before, but become those of the centre of gravity. If we inquire into the reason of this, we shall find that although θ does not enter into the expressions for the coordinates of the centre of pressure, yet it is a quantity necessarily introduced into the demonstration; and when θ is made equal to 0, some of the steps of the investigation become nugatory.

2nd, The equation $d_a^2 V + d_b^2 V + d_c^2 V = 0$ for the attraction of a mass upon a particle, furnishes us with another instance; for although the left hand member is in general equal to zero, yet when the attracted particle becomes one of the particles of the attracting body, its value suddenly changes from zero to -4π; which circumstance will be found, as before, to arise from one of the steps of the demonstration becoming nugatory in the latter case.

St. John's College, Cambridge,
Dec. 20, 1833.

XIX. *On certain Metallic Cyanurets.* *By* Frederick *and* Edward Rodgers*.

BEFORE the description of the experiments relative to the alkaline cyanurets, it is necessary that the processes should be considered by which these compounds have hitherto been prepared.

The process recommended by Berzelius for the preparation

* Communicated by the Authors.

of the cyanurets of potassium, sodium and barium, is to calcine the anhydrous ferrocyanurets in covered porcelain crucibles. He gives no process for the preparation of the cyanuret of strontium; but he mentions that the cyanurets of calcium and magnesium may be prepared by dissolving the hydrates of lime and magnesia in dilute hydrocyanic acid.

Thénard mentions two general processes for the preparation of the alkaline cyanurets. He prepares them by saturating the bases with hydrocyanic acid, or by heating the anhydrous ferrocyanurets to redness in close vessels.

It is also necessary to state that Dr. Nimmo, in his process for the preparation of hydrocyanic acid, recommends that the cyanuret of barium should be prepared by decomposing the solution of the sulphuret of barium by the bicyanuret of mercury.

However, even the cyanuret of potassium was not obtained in a pure form until recently; for, according to Mr. Laming, the cyanuret obtained by heating the anhydrous ferrocyanuret of potassium to redness in earthen vessels is always more or less impure. Although this statement does not agree with the experiments of other chemists, it must be admitted that the cyanuret obtained by this process is frequently of a yellow colour, and consequently very impure. In consequence of Mr. Laming's observations, we endeavoured to obtain the cyanuret of potassium by some other process; the results of our experiments on this subject, together with those on some other cyanurets, are contained in the following paper. In all probability these processes cannot be employed to prepare these compounds for commercial purposes, but it may prove useful to state the new modes by which they may be obtained.

1. It is stated in most chemical works that the solution of the sulphuret of potassium is decomposed by the bicyanuret of mercury, cyanuret of potassium remaining in solution, and the cyanuret of barium was obtained by Dr. Nimmo by the analogous process; but no other protocyanuret has hitherto been obtained by such a process. The following experiments show that other cyanurets may be prepared by a similar mode.

On the addition of the solution of the bicyanuret of mercury to a solution of the protosulphuret of potassium, sodium, barium, strontium, calcium, or magnesium, the bisulphuret of mercury is precipitated, and the pure protocyanuret remains in solution. One equivalent of the bisulphuret of mercury, and two equivalents of the protocyanuret, result from the decomposition of two equivalents of the protosulphuret and one of the bicyanuret of mercury. The sulphurets of potassium, sodium, barium and strontium should be prepared for this experiment by heating the anhydrous sulphates to redness with half their weight of charcoal; and the sulphuret of calcium by transmitting a current of sulphuretted hydrogen gas through lime, suspended in recently boiled distilled water previously allowed to cool in close vessels. As the protosulphuret of magnesium cannot be prepared by heating sulphate of magnesia with charcoal, it must be prepared by transmitting sulphuretted hydrogen gas through magnesia suspended in water, or by decomposing the solution of the sulphate of magnesia by means of sulphuret of barium. In order to obtain a pure protocyanuret by this process, great care must be taken, otherwise an excess of the alkaline sulphuret will redissolve some of the precipitated bisulphuret of mercury, and an excess of the bicyanuret of mercury would also cause serious mistakes. The proportion of one equivalent of the bicyanuret to two equivalents of the protosulphuret must be rigidly adopted, and before filtration the solution should be tested to ascertain that it is pure.

It is not advisable to attempt to crystallize the protocyanurets obtained by this process in the open air, owing to the rapid decomposition produced by the carbonic acid existing in the atmosphere, and to the tendency which the cyanurets have to resolve themselves into new compounds.

All the solutions have an alkaline reaction and evolve the odour of hydrocyanic acid.

2. The cyanurets of potassium, sodium and magnesium may be readily obtained by decomposing the solution of the cyanuret of barium by a solution of the sulphates of potash, soda, or magnesia; for sulphate of baryta is immediately precipitated, and a very pure cyanuret of potassium, sodium, or magnesium remains in solution. Should an easy mode of preparing the cyanuret of barium be discovered, this process will probably afford the most easy mode of preparing a solution of the cyanuret of magnesium; but the solution cannot be evaporated by the aid of heat without decomposition.

3. The following process is not applicable to the formation of large quantities of the cyanurets of potassium and sodium; but it is interesting in a theoretical point of view, being analogous to the reduction of chloride of silver by fused carbonate of soda.

On projecting powdered bicyanuret of mercury (dried by a water-bath,) by small portions upon carbonate of potash, heated to a temperature just approaching low redness, the carbonate of potash turns black and enters into a state of semifusion; cyanogen is not evolved, but metallic mercury is sublimed, oxygen and carbonic acid gases are disengaged with effervescence, and at length the mixture becomes white. On the addition of a fresh portion of the bicyanuret of mercury, the same phænomena take place; but the mixture then enters into complete fusion.

The bicyanuret of mercury employed in this experiment must be quite pure and well dried, otherwise the cyanuret of potassium has a yellow colour. Atomic proportions may be employed; but as it is better to allow the alkaline carbonate to be in slight excess, the best proportions are 120 grains of the dry bicyanuret of mercury to 70 grains of anhydrous carbonate of potash. When the mass is cold, the cyanuret of potassium may be separated from any undecomposed carbonate of potash by digestion in boiling rectified spirit, and the cyanuret may be obtained in crystals by concentrating the alcoholic solution; but this is not often necessary, as cyanuret of potassium is not very soluble in alcohol.

The process may be performed in a green glass tube, or in a clean Hessian crucible. It cannot be performed at a full red heat; for, under certain circumstances, cyanuret of potassium appears to be volatile in an open crucible at that temperature; thus, on projecting dry bicyanuret of mercury into fused carbonate of potash, cyanogen gas is copiously evolved and inflames, whilst the crucible becomes filled with a kind of mist; but immediately after this appearance cyanogen ceases to be evolved. Sometimes the whole of the matter is dissipated; but usually after the crucible is cold a yellowish mass is found at the bottom, much less in weight than the cyanuret obtained at a low red heat, and the aqueous solution of this substance does not evolve the odour of hydrocyanic acid, but effervesces strongly with acids. Finally, the process cannot be performed in platinum vessels, for platinum is attacked. It is evident that in this process both the bicyanuret of mercury and the carbonate of potash are decomposed, the metallic mercury being sublimed, and the carbonic acid and oxygen gases expelled, whilst cyanogen unites with potassium.

Cyanuret of sodium may be obtained by a similar process, substituting about 54 grains of anhydrous carbonate of soda for 70 grains of carbonate of potash. The process requires to be performed at a temperature rather higher than a low red heat. The tube employed must be made of green glass, or the process may be performed in the bottom of a Florence flask; for a common glass tube is rapidly attacked, blackened, softened, and finally destroyed.

The changes which take place during the formation of this cyanuret are similar to those already described. The cyanuret of sodium is separated from the undecomposed carbonate of soda by treating the white residuum with boiling rectified spirit, and may be obtained in crystals from the solution. It is not very soluble in alcohol. The carbonates of lime, baryta and strontia cannot be decomposed by the bicyanuret of mercury at a red heat. The bicyanuret is decomposed by heat, as usual, with the evolution of cyanogen; but the carbonate does not undergo any change.

4. *Cyanuret of potassium* may be prepared by exposing a mixture of anhydrous carbonate of potash and anhydrous ferrocyanuret of potassium to a moderate red heat, in a covered porcelain crucible, for about twenty minutes. When the crucible is quite cold, a dark mass is seen, which adheres rather firmly to the bottom; on treating this dark mass with hot alcohol, a colourless solution of cyanuret of potassium is readily obtained. The aqueous solution produces no change in the solutions of the persalts of iron, but it immediately throws down the orange protocyanuret of iron from the solutions of the protosalts. The proportions employed should be nearly one equivalent of anhydrous carbonate of potash to one equivalent of the anhydrous ferrocyanuret of potassium. The process is very productive, for the weight of the cyanuret of potassium obtained by this process is much greater than the weight of the cyanuret obtained by heating ferrocyanuret of potassium alone, owing to the cyanogen of the cyanuret of iron uniting with the potassium contained in the carbonate of potash; and moreover the process occupies less time. Some carburet of iron always remains in the crucible.

5. *Cyanuret of potassium* may be obtained by heating a mixture of anhydrous carbonate of potash and pure Prussian blue to redness in covered crucibles. It appears that ferrocyanuret of potassium is generated, and that the cyanuret of potassium results from the subsequent decomposition of this salt. The proportion should be about 242 grains of pure Prussian blue, and 315 grains of anhydrous carbonate of potash. The cyanuret of potassium may be separated by means of alcohol from the undecomposed carbonate of potash. After the cyanuret of potassium has been dissolved by alcohol, and the undecomposed carbonate of potash by water, the black residue dissolves with effervescence in dilute muriatic acid. The gas generated during the solution has a most offensive odour.

Cyanuret of sodium may be obtained by an analogous process, substituting 243 grains of anhydrous carbonate of soda for 315 grains of carbonate of potash. The same changes occur when the mass is heated; and the cyanuret of sodium may be separated from any undecomposed carbonate of soda by means of boiling rectified spirit.

The quantity of the cyanurets obtained by this mode is extremely variable: the difference appears to depend upon the more or less rapid application of heat.

6. *Cyanuret of barium* may be obtained in a pure form by the process described by Berzelius and Thénard; viz. by heating the dry ferrocyanuret of barium to redness in covered porcelain crucibles. The cyanuret of barium is very soluble in water, but it also dissolves in boiling rectified spirit.

7. The cyanurets of the common metals, and the double cyanurets formed by the union of these compounds with the cyanurets of the bases of the alkalies and alkaline earths, are described in the system of Berzelius; but the following experiments appear to show that the latter compounds may be formed by processes different from those mentioned by that distinguished chemist. It appears that Gay-Lussac procured the double cyanuret of iron and potassium by digesting the black oxide of iron in a solution of cyanuret of potassium. The cyanuret of potassium dissolved a portion of the oxide, but the solution continued alkaline: on the addition of hydrocyanic acid a fresh portion of the oxide was dissolved and the solution became neutral. Gay-Lussac also obtained the double cyanuret of silver and potassium by digesting cyanuret of silver in a solution of cyanuret of potassium; part of the cyanuret of silver was dissolved, and the solution became neutral on the addition of hydrocyanic acid. Berzelius, who describes these compounds as the discovery of Ittner, does not mention the latter important circumstance.

The following experiments appear to show that cyanuret of potassium is capable of dissolving many other oxides.

Hydrated protoxide of copper, prepared by precipitating the sulphate by pure potash, was digested, whilst moist, in a solution of cyanuret of potassium, which dissolved a certain portion, but continued alkaline. On the addition of hydrocyanic acid, the oxide assumed a yellow colour, an additional quantity was dissolved, and the solution, which had a red colour, became quite neutral. The presence of copper cannot be detected by any of the usual reagents, until the solution is boiled with a little sulphuric acid. Berzelius states that the solution prepared by Ittner was of a yellow colour, and gave little yellow prisms on cooling, whilst Gmelin prepared a double cyanuret of copper and potassium in colourless rhombohedral prisms.

The solution prepared by the process just described is usually red, but in one experiment it was *quite colourless.* Another solution, prepared by precipitating the acetate of copper by means of cyanuret of potassium, and redissolving the precipitate in an excess of the latter salt, was of a *beautiful purple* tint, similar to the salts of permanganic acid; but this solution was instantly changed to *yellow*, and finally decolourized by the application of heat.

The double cyanuret of copper and potassium, like the cyanuret of potassium, throws down a yellow precipitate from the solution of the protosulphate of copper; but it is easily distinguished by its neutrality, and by causing a *white* precipitate in the solution of the protosulphate of iron, whilst the cyanuret of potassium throws down an orange precipitate from the same salt.

Berzelius describes the double cyanuret of copper and lead, on the authority of Ittner, as a green precipitate; but the red solution throws down a French white precipitate from the solutions of the acetate and nitrate of lead. From nitrate of silver a white flocculent precipitate, with a slight tinge of pink, is thrown down, which soon changes to lead colour.

Cuprocyanic acid cannot be prepared by means of tartaric acid, for this acid produces a *pink* precipitate in the solutions of the double cyanuret of copper and potassium. In that peculiar variety of the double cyanuret which forms a colourless solution, tartaric acid produces a *white* flocculent precipitate. Probably cuprocyanic acid may be prepared by decomposing the double cyanuret of copper and lead by a current of sulphuretted hydrogen, or the solution of the double cyanuret of copper and barium by dilute sulphuric acid; but the elements of the hydracid do not appear to have much affinity for each other, for although the double cyanuret of copper and barium is not precipitated by ferrocyanuret of potassium, the copper is immediately precipitated if the baryta is thrown down previously by dilute sulphuric acid. The double cyanuret of copper and potassium cannot be generated by fusing metallic copper with carbonate of potash and isinglass. When the heat rises sufficiently high, the copper fuses, but suffers no other change.

A double *cyanuret of zinc and potassium* may be prepared by dissolving the hydrated oxide of zinc in a solution of cyanuret of potassium, and neutralizing the solution by means of hydrocyanic acid. The solution is colourless. It appears that *zincocyanic acid* may be prepared by adding tartaric acid to the concentrated solution of this salt, for bitartrate of potash is then precipitated.

A double *cyanuret of nickel and potassium* may be formed by digesting the hydrated oxide of nickel in a solution of cyanuret of potassium, adding hydrocyanic acid as before. The excess of oxide of nickel assumes a dusky yellow colour. The solution is yellow, and is not affected by any of the reagents which detect the presence of oxide of nickel. The solution throws down from the sulphate of copper a pale yellow gelatinous precipitate, soluble in ammonia, forming a colourless solution; but if the salt of copper is in excess, the solution is blue: the precipitate, somewhat changed, is thrown down from the ammoniacal solution by the nitric, muriatic and sulphuric acids. A white flocculent precipitate is produced in the solution of the nitrate of silver, which is soluble in ammonia, insoluble in nitric acid, and unchanged by the solar rays. From the protosulphate of iron a *white* precipitate is thrown down, which changes to a very light blue after standing some time. The solution precipitates the persalts of iron *reddish yellow*, and the precipitate, which is rather bulky, becomes redder after standing some time, but never becomes blue. The solution throws down a white precipitate, with a slight tinge of blue, from the solution of pure sulphate of nickel, and a flocculent white precipitate, soluble in nitric acid, from the acetate of lead.

When a current of chlorine is transmitted through the solution, a white precipitate takes place. The filtered solution is still neutral, but no longer precipitates the salts of lead.

A double *cyanuret of cobalt and potassium* may be formed by adding pure hydrated oxide of cobalt to a solution of cyanuret of potassium: the oxide is immediately dissolved, and the solution becomes yellow, but continues alkaline; the colour of the solution soon changes to a brownish hue. On the addition of hydrocyanic acid an additional quantity of the oxide is taken up, the solution returns to its yellow colour, and is then quite neutral. No change is produced in the solution by the reagents employed to detect oxide of cobalt. The solution precipitates the sulphate of copper *pale blue*, the pure sulphate of cobalt *pink*, pure sulphate of nickel very *light green*, sulphate of zinc *white*, nitrate of silver *flocculent white*, and protonitrate of mercury, sulphate of manganese and protosulphate of iron also *white*; but it produces no change in the solution of the persulphate of iron.

L. Gmelin prepared a double cyanuret of cobalt and potassium, analogous to the red cyanuret of iron and potassium, by dissolving the carbonate or the cyanuret of cobalt in solution of potash, and neutralizing the solution with hydrocyanic acid. Perhaps the double cyanuret prepared by dissolving the oxide is also analogous to the same salt, for the solution suffers no change when a current of chlorine is passed through it.

A double *cyanuret of manganese and potassium* cannot be prepared by digesting the hydrated oxide with cyanuret of potassium, owing to the oxide of manganese passing into a higher state of oxidation, and to the rapid decomposition of the double cyanuret itself. No experiment was made with the anhydrous protoxide of manganese; but, as it has been lately shown that ferrocyanuret of potassium is generated when the anhydrous protoxide of iron is digested in a hot solution of cyanuret of potassium, it is probable that the anhydrous protoxide of manganese is soluble in the same solution.

The *hydrate of the oxide of chromium* does not dissolve in the solution of the cyanuret of potassium until hydrocyanic acid is added; the colour of the oxide is then changed to a reddish brown, and a very small portion is dissolved. The solution continues strongly alkaline. Acetic acid throws down a brown precipitate from the solution. The solution of the oxide is not assisted by digestion in the alkaline cyanuret. It appears from these experiments that the sesquioxide of chromium has very little tendency to form a double cyanuret when digested with an alkaline cyanuret.

It appears that the sesquicyanuret of uranium has very little disposition to unite with cyanuret of potassium. It dissolves very sparingly in the solution of the cyanuret: the solution, which is alkaline, has a light yellow colour, and the undissolved sesquicyanuret of uranium retains its beautiful yellow tint. On the addition of hydrocyanic acid, both the cyanuret of uranium and the solution assume a darker tint, but the odour of hydrocyanic acid does not disappear, which always happens on the addition of hydrocyanic acid to the alkaline solutions of the oxides of nickel, cobalt and copper in the solution of the cyanuret of potassium, unless the hydrocyanic acid is added in excess. Hence, it would appear that the double cyanuret of uranium and potassium does not exist. The sesquicyanuret of uranium was employed in this experiment instead of the peroxide, because it is easily obtained in a pure state by decomposing the nitrate of uranium by a solution of cyanuret of potassium. It is soluble in nitric acid.

8. The double cyanurets may be formed by other processes: for example, by the addition of a metallic sulphate to a solution of the cyanuret of barium; thus ferrocyanuret of barium is generated on the addition of protosulphate of iron to a solution of cyanuret of barium. At the first moment the precipitate is *white*, but a precipitate soon takes place which rapidly becomes blue. On examination the solution is found to precipitate the protosalts of iron light blue, and the persalts dark blue.

The *red cyanuret of iron and potassium* may be generated by adding a solution of persulphate of iron and sulphate of potash to a solution of the cyanuret of barium.

The double *cyanuret of copper and barium* may be formed by adding a solution of sulphate of copper to cyanuret of barium. The presence of copper in the solution cannot be detected until it is digested with a little sulphuric acid; after filtration, the copper may be precipitated by ferrocyanate of potash. When a double cyanuret is prepared, by decomposing cyanuret of barium by a soluble sulphate, it is absolutely necessary to add the solution of the sulphate to that of the cyanuret. It appears that a metallic cyanuret and sulphate of baryta are generated; the former then unites with an excess of cyanuret of barium, and the latter is precipitated. On the addition of a fresh portion of the sulphate, the hydracid is precipitated in combination with the oxide, or the base of the hydracid in combination with the metal.

9. For experiment, the solutions of the double cyanurets may be readily prepared by the following process, which is a mere modification of that recommended in the work of Berzelius, who recommends the cyanurets to be precipitated, washed, and then dissolved in a solution of cyanuret of potassium. During this process certain cyanurets—for example, copper—appear to undergo some change.

It is better, therefore, to precipitate sulphate of copper by means of cyanuret of potassium, and immediately redissolve the precipitate by an additional quantity of cyanuret of potassium. The solution thus obtained has a yellow colour, similar to the solution described by Ittner: it is quite neutral.

A double *cyanuret of cobalt and potassium* may be prepared by precipitating the nitrate of cobalt by means of cyanuret of potassium, and dissolving the precipitate as before.

A double *cyanuret of nickel and potassium* may be obtained by decomposing sulphate of nickel in a similar way: the solution is yellow.

If required, the double cyanurets may be separated from the sulphate of potash by evaporation and crystallization.

10. The experiments mentioned in this paper have as yet entirely related to the alkaline and double cyanurets; but the following facts seem to be worth recording, although they refer to the pure cyanurets of the common metals, which are much better known.

The *protocyanuret of lead* may be readily thrown down as a dense, white precipitate, from the solution of the acetate, by means of hydrocyanic acid. It is soluble in nitric acid. Hot water changes the white colour of the cyanuret to a faint flesh colour, and then dissolves it: the filtered solution is colourless. A small portion of the cyanuret appears to remain undissolved, even in a large quantity of water. The cyanuret of lead is sparingly soluble in cold water.

The *protocyanuret of copper* cannot be conveniently precipitated from the acetate by means of hydrocyanic acid. The solution of the acetate becomes turbid, and remains in that state for a considerable time. The acid employed in this experiment was that prepared according to the process of Mr. Laming.

The *sesquicyanuret of chromium*, according to Berzelius, has not yet been obtained in an isolated form; but it seems that it may be prepared by the following process. When a solution of cyanuret of potassium is added to a solution of the double sulphate of chromium and potash, in *cold* distilled water, an immediate grayish white precipitate is obtained, which falls in distinct masses. The precipitate is broken up by agitation, and then subsides rapidly in the form of a rather dark green powder. On the addition of another portion of cyanuret of potassium, a fresh precipitation takes place. Finally, after all the oxide of chromium is precipitated, the solution evolves the odour of hydrocyanic acid. Owing to the latter fact, and to the precipitate appearing like the hydrated oxide, it might be mistaken for the oxide, and the behaviour of the cyanuret of potassium in the solutions of the oxide of chromium might be compared to that of the sulphuret of potassium. But the precipitate dissolves *slowly* in distilled water acidulated with nitric acid, and a rather copious precipitate of cyanuret of silver is thrown down on the addition of nitrate of silver to the solution. The experiment, however, was performed on rather small quantities of the materials, and requires, therefore, to be carefully repeated.

Cyanuret of barium throws down a copious precipitate from the solution of the double sulphate of chromium and potash: the precipitate consists of sulphate of baryta mixed with the supposed cyanuret of chromium. The supernatant solution smells strongly of hydrocyanic acid, especally if the operation is continued after the oxide of chromium is precipitated.

11. Berzelius describes an hydracid, analogous to the persalts of iron, which is prepared by decomposing, by dilute sulphuric acid, the peculiar kind of ferrocyanuret of lead which is generated when the red cyanuret of iron and potassium is added to a solution of a salt of lead. It is, perhaps, the best mode of obtaining it in a perfectly pure form; but when this is not required, the following process may be employed. The

aqueous solution of the crystallized red double cyanuret of iron and potassium may be readily decomposed by an alcoholic solution of tartaric acid: the bitartrate of potash is completely thrown down, and a yellow solution is obtained, which has a strong acid reaction. 65·89 grains of the crystallized salt are exactly decomposed by 90 grains of crystallized tartaric acid; but as the red cyanuret is very insoluble in strong alcohol, it is better to use a slight excess of the cyanuret. The red cyanuret employed must be quite free from chloride of potassium. The tartaric acid should be dissolved in alcohol and then added to the saturated aqueous solution of the double cyanuret. The filtered solution of the hydracid must be preserved in green glass bottles, well protected from the light; but, whatever care is taken, the solution gradually becomes darker, then assumes a dark green tint, and is at length completely destroyed.

The solution produces in metallic solutions precipitates similar to those produced by the red double cyanuret itself. Like that salt, the hydracid produces a deep blue precipitate in the solutions of the protosalts of iron. The hydracid immediately darkens the colour of the solutions of the persalts of iron; but the solution of the red cyanuret produces no immediate change in moderately dilute solutions: gradually, however, it changes the colour to a deep brownish red, and ultimately produces a *green* precipitate. When the hydracid or a solution of the red cyanuret is added to a strong solution of the persalts of iron, the final precipitate is a mixture of blue and green. The green precipitate becomes blue at the temperature of 212°. If washed and dried without exposure to heat, it becomes a *green powder*, which is readily decomposed by a dilute solution of potash; peroxide of iron subsides, and the supernatant yellow solution throws down a *green* precipitate from the persalts, and a *blue* precipitate from the protosalts of iron.

Perhaps the hydracid just described may be called the perferrocyanic acid, as that term will express the analogy which exists between this compound and the persalts of iron. It is difficult to imagine in what manner the double cyanuret of iron and potassium, which produces green precipitates in the solutions of the persalts of iron, differs from the red double cyanuret.

12. In a paper published in the Glasgow Medical Journal in the year 1831, Mr. Clarke recommended that hydrocyanic acid should be prepared for medical use by the decomposition of cyanuret of potassium by tartaric acid. This process, slightly

modified in the details, has been lately recommended to the notice of the medical profession by Mr. Laming. In the original process a small portion of bitartrate of potash always remained in solution, so that the acid was always distilled when required for chemical use. The alcohol employed by Mr. Laming to precipitate the bitartrate of potash renders the acid even more unfit for chemical purposes, although it is highly useful to add a small portion in the preparation of the *medicinal acid.*

However, *pure* hydrocyanic acid may be obtained at once by the decomposition of a solution of the cyanuret of calcium by oxalic acid. Heat is developed during the decomposition. The oxalate of lime is completely precipitated in a few hours, leaving the supernatant acid quite pure; but if slight traces of oxalate of lime are held in suspension, one filtration is sufficient, whilst in the process of Proust two filtrations are required. The liquid is quite pure, as the oxalate of lime is insoluble in hydrocyanic acid. In this process, as well as in Mr. Clarke's, the use of a mineral acid is avoided. The chief objection to its adoption for the preparation of hydrocyanic acid for chemical purposes, is the difficulty of procuring the cyanuret of calcium; but it is probable that an easy mode of obtaining that cyanuret will soon be discovered. The proportions required are one equivalent of oxalic acid and one equivalent of the cyanuret of calcium.

XX. *On some Meteorological Phænomena observed in the Mount's Bay, Cornwall. By* W. J. Henwood, *F.G.S. Lond. and Paris, Hon. M.Y.P.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

On the 18th of November, at about 4^h P.M., as I was walking on the beach between Penzance and Newlyn, my attention was drawn to a very dense black cloud (*a*) hanging over the plateau of Paul Hill (*b*). Towards the sea it shaded off, and immediately over the beach in that direction it terminated, a few small clouds appearing at intervals in the otherwise clear sky.

On continuing to observe it for some time, I found that although the mass of cloud was in motion before a somewhat brisk breeze from the south-east, yet, as one portion receded, another in taking its place became equally dark.

In fact, as successive portions of air, which, whilst *over the sea, were transparent,* approached the land, small light spots of cloud began to be formed; and when the *same* mass of air was brought *over the land*, these light clouds *thickened, and united into one dense and dark one.*

In company with Mr. John Phillips, I noticed this for about half an hour, and so far as we could judge, it continued after evening fall. On the following day both Mr. H. Thomas, to whom I am indebted for the accompanying sketch, and myself observed similar appearances on the same spot, which forms the western boundary of the bay, and over the Lizard Point (*c*), which terminates it on the east.

I have several times subsequently observed the like.

Mr. Harvey * has described similar phænomena occurring in the neighbourhood of Plymouth: in this part of the country I believe they are by no means unfrequent.

I have the honour to remain, &c.

W. J. Henwood.

Geological Society, Penzance,
Dec. 4, 1833.

XXI. *On the Mode of detonating a Mixture of Oxygen and Hydrogen by a Spark induced by a small horse shoe Magnet.* By the Rev. William Ritchie, *LL.D., F.R.S., &c. Professor of Natural and Experimental Philosophy in the Royal Institution of Great Britain, and in the University of London.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In a paper read before the Royal Society nearly two years ago, I first described the method of obtaining a spark from a small magnet sufficient to detonate a mixture of oxygen and

* Edinburgh Journal of Science, Jan. 1829, First Series, vol. x. p. 148.

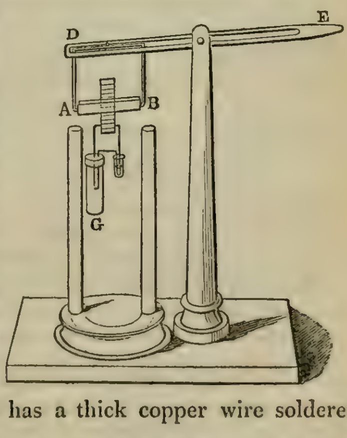
hydrogen, thus establishing for the first time one of the physical properties of the magneto-electric spark*. As the experiment is thus rendered what may be called a good class-experiment, and as the account of it has only been partially published, I have ventured to request you to give it a place in the Philosophical Magazine.

I am, Gentlemen, yours, &c.

WILLIAM RITCHIE.

Take a short lifter, either round or square, of the worst English iron, a little shorter than the whole length across the poles of a horse-shoe magnet capable of carrying fifteen or twenty pounds, and roll a ribbon of fine copper round the middle of it, metallic contact being prevented by a thin tape interposed between the spirals. The length of the copper ribbon may be about ten or twelve feet. To the ends of the coil two thick copper wires are to be soldered, in order to form a complete metallic circuit when the lifter is in contact with the poles of the magnet. The magnet is mounted on a base or sole of wood, having a pillar with an arm or lever passing through a mortice in the top of it for the purpose of removing, by a sudden jerk, the lifter from the poles of the magnet.

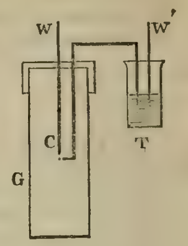
The description will be best understood by reference to the annexed figure, in which A B is the lifter of soft iron, D E the lever for the purpose of suddenly raising the lifter by a smart blow given with the palm of the hand at E. In front of the magnet a glass tube G, is placed, having its top closed by a cap of box-wood, with a hole in the centre, to allow one of the copper wires to pass through it. The other wire or end of the coil is made to play up and down in a brass tube filled



with mercury. This tube has a thick copper wire soldered

* The readers of your Journal are perfectly aware that Mr. Faraday first obtained the spark from a temporary electro-magnet, and after his researches had found their way to Italy, SS. Nobili and Antinori obtained it from a permanent magnet. [See Phil. Mag. and Annals, N.S. vol. xi. p. 401.—EDIT.]

to it and passing through the cap in the top of the glass tube, the lower end of the wire being flattened and bent at right angles as in the annexed figure, in which G is the glass tube, W one of the wires resting on C, which is connected with the brass tube T. The other end of the coil, W′, dips into the tube T, filled with mercury. The ends of the wires and the flat piece at C should be well amalgamated and covered with a clean surface of mercury. If the end of the lever be struck rapidly with the palm of the right hand, the left pressing on the short end at D, the sparks at C may be made to follow each other in rapid succession.



If a mixture of two volumes of hydrogen and one of oxygen be introduced into the tube by means of a bent or flexible tube, and the spark made to appear at C, the mixture will be exploded. If the lever be gently struck, the spark still may be made to strike off in the mixture without exploding it, whereas with a smart blow the experiment never fails.

XXII. *Reply to* Mr. Phillips's *Observations on the Use of Chemical Symbols. By* Thomas Graham, *Esq., M.A., F.R.S.E., Lecturer on Chemistry in the Andersonian Institution, Glasgow.*

To Richard Phillips, Esq., &c.

Dear Sir,

IN reference to your objections to the notation employed in my paper on phosphoric acid, allow me to make the following observations.

The system of notation which I follow is that last proposed by Berzelius; and convenient as that system is, and as it is generally adopted on the Continent, I think the introduction of any other at present calculated rather to retard than to advance the progress of chemistry. You are therefore entitled to ask why, in the paper referred to, one atom of water is represented by $\dot{H}$, while in the tables of Berzelius it is represented by $\underline{\dot{H}}$. My answer is, that in common with Gay-Lussac, and all the chemists of this country who have lately published, I consider water as composed of one atom of oxygen and one atom of hydrogen, a constitution expressed by $\dot{H}$ *in the symbolic language of Berzelius.* Berzelius himself uses the expression $\underline{\dot{H}}$, because, from theoretical considerations, which everybody knows, he halves the combining proportion of hydrogen, and therefore makes water to consist of one atom of oxygen united with two atoms of hydrogen.

The same observations apply to the expression used by me for phosphoric acid, namely, $\overset{:\cdot:}{P}$. I view the phosphorus in this acid as one atom, just as I view the nitrogen united with five atoms of oxygen in the case of nitric acid, or with three atoms of hydrogen in the case of ammonia, as one atom: Berzelius views it as two, and therefore expresses phosphoric acid by $\underline{\overset{:\cdot:}{P}}$. In the original paper, I thought I had rendered this evident, by always stating first in words what was afterwards expressed by a formula, and particularly by adopting the admirable plan of Berzelius of stating the relation between the oxygen in the acid and that in the base or water; but in this your remarks prove that I have been unsuccessful.

In the tabular exhibition of the formulæ of various authors to express crystallized phosphate of soda which you give, you set down to me $\dot{N}^2\,\dot{H}^{24}\,\overset{:\cdot:}{P}$, which is not exactly the expression I would have used. The formula which I would give is $\dot{N}^2\,\dot{H}\,\overset{:\cdot:}{P} + \dot{H}^{24}$, *i. e.* the crystallized salt consists of two atoms of soda and one atom of basic water united to the atom of phosphoric acid, together with twenty-four atoms of water of crystallization. In his formulæ for the salts, Berzelius arranges the symbols so that the most positive ingredient stands first. Now, from reasons which are explained in my paper, I presume that, besides the soda, one atom of water is positive to the acid, and not twenty-four, as you make me say.

I remain, dear Sir, yours, &c.

Glasgow, Dec. 16, 1833. THOMAS GRAHAM.

XXIII. *On the Former Extent of the Persian Gulf, and on the comparatively recent Union of the Tigris and Euphrates. By* CHARLES T. BEKE, *Esq.**

WHATEVER may be the opinion of profane historians, founded on tradition, we have no warrant from the Scriptures themselves for the conclusion, that the City and Tower of Babel†, the Babel of Nimrod‡, and the Babel or Babylon of Nebuchadnezzar§, were identical. I do not, of course, intend to infer that their sites were different simply from the

* Communicated by the Author; being an extract from a Work on the Geography of Sacred History, now in the press.

† Gen. xi. 4. 9. ‡ Gen. x. 10. § Dan. iv. 30.

silence of the Scriptures as to their identity; but, from other circumstances, I am fully persuaded,—and the silence of the Scriptures unquestionably aids the conviction,—that these three places, although bearing the same name, were totally distinct from each other.

I proceed to detail my reasons for this conclusion:—The City of Babylon, of which Nebuchadnezzar was king, is generally supposed to have been situate at Hillah, on the Euphrates, about 200 miles, in a direct line, from its junction with the Tigris, and rather more than 300 miles from the point at which the united rivers empty themselves into the Persian Gulf. What the nature of this country is at the present day, will be best shown by the following extract from the late Mr. Rich's "Memoir on the Ruins of Babylon," 2nd edit. p. 13. The Euphrates, he informs us, when at its height, "overflows the surrounding country, fills the canals dug for its reception without the slightest exertion of labour, and facilitates agriculture in a surprising degree. The ruins of Babylon are then inundated so as to render many parts of them inaccessible, by converting the valleys among them into morasses. But the most remarkable inundation of the Euphrates is at Felugiah, twelve leagues to the westward of Bagdad, where, on breaking down the dyke which confines its waters within their proper channel, they flow over the country, and extend nearly to the banks of the Tigris, with a depth sufficient to render them navigable for rafts and flat-bottomed boats. At the moment I am now writing, (May 24th, 1812,) rafts, laden with lime, are brought on this inundation almost every day from Felugiah, to within a few hundred yards of the northern gate of Bagdad, called the Imam Mousa Gate." Felugiah, it may be observed, lies in a direct line across from Bagdad, that is, about sixty miles higher up the Euphrates than Hillah.

If such be the present state of the country in the neighbourhood of Babylon, what may we not consider to have been its state during the first ages after the Flood?

We behold continually, in all parts of the world, the formation of countries through which great rivers take their course; and we know that the whole of the alluvial soil about the lower parts of those countries has, at some time or other, been brought down and deposited by the rivers. There can be no difficulty, therefore, in asserting, that the low lands of the Euphrates and Tigris, for a considerable distance from their mouths, could not have existed in early ages, but must have been gradually formed by the encroachment, on the Persian Gulf, of the alluvial soil brought down and deposited by those mighty rivers. The extent of this alluvial soil can of course be easily ascertained, and we may obtain, consequently, the means of determining what was the extent northward of the Persian Gulf at some former period; though, from our inability to calculate the rate at which the new land has been formed, we may not be able to arrive at any certain conclusion as to the period when the change from sea to land of any portion of the country actually took place. Mr. Lyell remarks*, that "The union of the Tigris and the Euphrates must undoubtedly have been one of the modern geographical changes on our earth." By this expression it would almost seem that Mr. Lyell meant it to be inferred that the union of the two rivers has not taken place within the historical æra, as have those of the Ganges and Burrampooter, and other rivers instanced by him; but this could hardly, I should think, have been his intention, when we consider that Pliny expressly tells us that "between the mouths of these two rivers where they fall into the sea, were counted, in old times, twenty-five miles, or, as some would have it, but seven †." Indeed, if the estimation made by Nearchus of the distance from the mouth of the Euphrates to Babylon be correct, not merely were these two rivers separate, at a comparatively late period, but the increase of the land at the head of the Persian Gulf must have taken place at an extraordinarily rapid rate. The distance mentioned by him is only 3300 stadia‡, or little more than 200 miles; whilst the actual distance, at the present time, from Hillah, where the ruins of Babylon are now supposed to be situate, is in a straight line at least 300 miles. But the statement of Pliny § with respect to the city of Charax, on the confluence of the Tigris and Eulæus, not merely

* Principles of Geology, vol. i. p. 252, 1st edit.

† Holland's Pliny, book vi. ch. xxvii.

‡ Voyage of Nearchus, ch. xli. Dr. Vincent, in a note on this passage in his translation, says "3300 stadia [of sixteen to a mile: see his Preface, p. xi.] make little more than 200 miles English; the real distance by the river is more than 400. But may not Nearchus calculate this distance by stadia of eight to a mile?" The accuracy, however, of the mode thus adopted by the learned translator, and by Geographers generally, of reconciling apparent discrepances in the works of ancient writers, by varying the standard of measurement, may legitimately be questioned.

§ "Charax oppidum Persici sinus intimum, a quo Arabia Eudæmon cognominata excurrit, habitatur in colle manufacto inter confluentes, dextra Tigrin, læva Eulæum, iii. mil. pass. laxitate. Conditum est primum ab Alexandro Magno: qui...Alexandriam appellari jusserat....Prius fuit a litore stadiis x., et maritimum etiam ipsa inde portum habuit: Juba vero prodente, l. mil. pass. Nunc abesse a litore cxx. mil. legati Arabum nostrique negotiatores qui inde venere, affirmant. Nec ulla in parte plus aut celeestablishes the fact that "nowhere were new lands formed more quickly or in greater quantities," but would also seem to determine the actual rate at which the Persian Gulf had been filled up during the 400 years immediately preceding his time:—Alexandria (on the site of which Charax afterwards stood,) having been built by Alexander the Great, at the distance of ten stadia only from the sea; whilst in Juba's time it was 50 thousand paces, or about 50 miles, and in Pliny's own time as much as 120 thousand paces, or about 120 miles, from the sea*.

But whatever may have been the actual rate of advance on the Persian Gulf of the alluvial tract thus formed, I think that—taking into consideration the present state of the country in the neighbourhood of Babylon, as described by Mr. Rich, and keeping in mind the changes which, from the statements of the historians above mentioned, must indubitably have taken place in it,—the legitimate inference is, that in the first ages after the Flood, the state of the country which subsequently became the site of Babylon, was such as to have rendered it totally inapplicable to the use of man; so that at the period of the building of the Tower of Babel and the commencement of Nimrod's kingdom, *there*, under any circumstances, could not have been that plain in the land of Shinar where "the whole earth" dwelt, and where they wished to erect a city and a tower whose top might reach unto heaven. Indeed, if the calculation of Nearchus and the statement of Pliny are to be depended upon, we are justified in concluding that, in the period immediately subsequent to the Flood, the Persian Gulf extended so far to the northward as actually to occupy the present supposed site of Babylon; so that it was physically impossible for the Tower of Babel to have been erected, at or near the spot where its remains have been imagined to exist.

Divesting our minds, indeed, of the authority of the tradition which connects Babylon with Babel, and considering the degree of probability which may be attached to the idea that the founders of the human race, when they had before them the choice of all the world, would have pitched upon a low,

rius profecere terræ fluminibus invectæ. Magis id mirum est, æstu longe ultra id accedente, non repercussas."—*Hist. Nat.* curâ Harduin, lib. vi. c. xxvii.

* This distance of Charax from the sea is totally at variance with the notions commonly entertained respecting its site; the map of the late Major Rennell, and also that by Professor Long recently published by the Society for the Diffusion of Useful Knowledge, placing it at a distance of little more than fifty miles from the sea shore.

swampy and unhealthy country, subject to periodical inundations, we shall speedily find that it is not merely improbable, but morally impossible, that they should have done so. Even at the distance of 2500 years from the present time, at which period we know the city of Babylon to have been in existence, the country in its natural state was as totally inapplicable to the use of mankind as it was 2000 or 2500 years previously. But the state of society at the later period was widely different from that of the first ages of the world: and, as we have instances in ancient Egypt, and also in modern Holland and Venice, how spots, in themselves perfectly uninhabitable, have, from the necessities of mankind, or from their peculiar local advantages as places of commerce or of defence, been selected, and by artificial means made habitable, and rendered the seats of mighty cities; so may we understand how, in the time of Nebuchadnezzar, and even earlier, Babylon should have been reclaimed, as it were, from the waste in which it was situate, and made the seat of empire and "the Glory of Kingdoms." It would, however, in the time of Nimrod, have been perfectly unnecessary, if even, indeed, from the paucity of inhabitants, it had not at that early period been actually impossible, (supposing the physical state of the country to have allowed it,) to raise those mighty embankments and walls which were the only safeguard and protection of Babylon from the floods of the Euphrates, and by the destruction of which that mighty city has again become "a desolation among the nations."

It is thus evinced that Babylon could not have occupied the site either of the Tower of Babel or of the Babel of Nimrod; but nothing has as yet been advanced to prove that the two latter places were not identical. The tradition on this subject is that the Tower of Babel, whether built by the whole of mankind or by Nimrod's family alone, was commenced at the instance and under the direction of that "mighty hunter before the Lord"; and that at the time of the Dispersion, Nimrod with his family remained on the spot, and became the founder of the Babylonian or Chaldean Dynasty. For this tradition, the only support appears to be that text which has so often been the subject of discussion: "And the beginning of his (Nimrod's) kingdom was *Babel*, and Erech, and Accad, and Calneh, in the land of Shinar *." But this authority only proves that Nimrod (or not improbably his descendants, since it seems almost too much to imagine that in those early times one man should have built four cities,) founded a city,

* Gen. x. 10.

the name of which was similar to that of the tower and city from whence the dispersion had taken place; whilst the inference that the two places were identical, deduced from the mere similarity of their names, is entirely rebutted by the following words of Scripture: "So the Lord scattered them abroad from thence upon the face of all the earth: *and they left off to build the city**:" from which the only legitimate conclusion to be drawn is that the City and Tower of Babel were left unfinished, and probably were altogether deserted. The ground of the tradition which has thus improperly connected these three places, may without difficulty be discovered in that vanity with which the history of all nations (that of the Jews, from the nature of its records, forming the solitary exception,) is replete.

XXIV. *Facts relating to Optical Science.* No. I. *Communicated by* H. F. Talbot, *Esq., M.P., F.R.S.*

1. *Microscopic Cleavages in Talc or Mica.*

If a thin transparent film of talc or mica is held in the exterior flame of a candle, it speedily becomes white and opake, like chalk. This opake portion is separated from that which remains transparent by a sort of penumbra. Now if we examine this penumbra with a powerful microscope, it is seen to consist of myriads of minute fractures, each of which exhibits the form of a cross. These microscopic crosses are all similarly disposed, and pointing in the same direction. This direction indicates, therefore, the line of least cohesion, or of most easy cleavage, in the crystal, while the opposite arm of the cross indicates another line of a similar nature. Two opposite angles of the cross contain spaces which are a good deal darker than the other two; which shows, if I am not mistaken, that the *plane* of the cleavage is oblique to the eye. This, however, disappears if the crystal is immersed in oil, because the refraction is thereby considerably diminished. It would be desirable to ascertain the position of these cleavage lines with respect to the axis of the crystal.

2. *Optical Properties of Chromium.*

Sulphate of chromium is a liquid which is green by daylight and reddish by candle-light, as was first noticed, I believe, by Sir David Brewster. The cause of this peculiarity is pleasingly exhibited by the following experiment. If a hollow prism of a small refracting angle, as five or ten degrees, is

* Gen. xi. 9.

filled with this liquid and a candle is viewed through it, instead of one, two candles are seen, one of which is red and the other green. It is a very striking experiment, for the rest of the spectrum being wholly absorbed, it in a manner imitates double refraction. Now in ordinary cases, as when this liquid is examined in a bottle, the two images are superposed, and in the day-light the green tint prevails over the red, while the reverse takes place by candle-light, because the green rays are *proportionally* more abundant in day-light than in candle-light.

3. *Purple Crystals from a green Liquid.*

If to a solution of common bichromate of potash be added a mixture of sulphuric acid and alcohol, it is well known to chemists that the liquid changes its colour from yellow to dark green. The compound thus formed is an impure sulphate of chromium. After standing a few hours it deposits an abundant crop of small, roundish, *purple* crystals. It is impossible not to recognise in this variety of colour the same peculiar action of chromium upon light which was the subject of the last experiment, but modified by circumstances, inasmuch as here both colours are exhibited by day-light.

4. *A Body in rapid Motion, yet apparently at rest.*

Let a small object be firmly fastened in front of a mirror; a thread, for instance, tied round the mirror, will do very well for the purpose of exemplification. Let the mirror be made to revolve rapidly, and let the thread be nearly parallel to its axis of revolution, at a little distance from it, however, on one side. Under these circumstances the thread will of course disappear. Now let a candle be placed some feet in front of the mirror, and in the band of light which it causes on the mirror, the thread will be plainly discerned, and apparently at rest.

This phænomenon, which I discovered in the year 1826, caused me at the moment, I acknowledge, the greatest surprise. Yet the explanation of it is easy; for it is only at one fixed period in each revolution that the image of the candle appears to pass behind the thread, and makes it visible. At that period it can, therefore, be distinguished, but only then, for during all the rest of its revolution it is projected against a dark ground, and invisible from its rapid motion. Is not this experiment capable of being applied to some useful purpose? There are many cases in which it might be desirable to see what passes in the interior or immediate vicinity of a body during the time of its being subjected to a swift motion.

Remark.—I have used the revolving mirror for many years as an instrument of optical and photometrical research; but the idea of applying it to determine the velocity of electricity belongs entirely to Mr. Wheatstone. The electrical experiment which I proposed in this Journal was distinctly stated to be founded upon his experiment exhibited at the Royal Institution, only proposed to be tried upon a much larger scale, (*viz.* with a wire of some miles in length,) but since Mr. Wheatstone says in his letter * (which I have only lately seen, having been abroad for some months,) that he had already tried an experiment upon the same principle, I most willingly yield to him the priority of it. It is a trifle, not worth a moment's hesitation.

5. *On the Flame of Lithia.*

Lithia and strontia are two bodies characterized by the fine red tint which they communicate to flame. The former of these is very rare, and I was indebted to my friend Mr. Faraday for the specimen which I subjected to prismatic analysis. Now it is difficult to distinguish the lithia red from the strontia red by the unassisted eye. But the prism displays between them the most marked distinction that can be imagined. The strontia flame exhibits a great number of red rays well separated from each other by dark intervals, not to mention an orange, and a very definite bright blue ray. The lithia exhibits one single red ray. Hence I hesitate not to say that optical analysis can distinguish the minutest portions of these two substances from each other with as much certainty, if not more, than any other known method.

6. *On the Flame of Cyanogen.*

For the opportunity of examining the optical characters of this flame, I am likewise obliged to the kindness of Mr. Faraday, who showed it both to Sir J. Herschel and myself at the Royal Institution. When viewed with a prism, this flame presents a very distinct and peculiar character, separating the violet end of the spectrum into three portions, with broad dark intervals between. But the most remarkable fact is this, that the last of these portions is so widely separated from the others as to induce a suspicion that it may be more refracted than any rays in the solar spectrum, a question which I should be glad to have the opportunity of deciding by direct experiment. This separate portion has a pale undecided hue. I should hardly have called it violet, were it not situated at the violet end of the spectrum. To my eye it had a somewhat whitish or grayish appearance.

* See Lond. and Edinb. Phil. Mag., vol. iii. p. 204.—Edit.

XXV. *Observations on the supposed Vision of the Blood-vessels of the Eye.* *By* Sir David Brewster, *L.L.D., F.R.S.**

IN the Number of this Journal for September 1832, I had occasion to refer to the remarkable experiment described by Dr. Purkinje of Breslau, in which the blood-vessels of the retina are supposed to be exhibited; and though I had in vain tried to see this phænomenon, yet it had been so accurately described to me by Mr. Potter that I ventured to give an opinion respecting its cause. The paper which contained this explanation was read at the Physical section of the British Association at Oxford in June 1832, and Mr. Wheatstone, who was present, favoured the Meeting with some excellent observations on the subject. These observations have been printed in the Report of the Association for 1832, in the form of an Appendix to the abstract of my paper; and as they are highly interesting, and will form the groundwork of the following observations, I shall give them *verbatim*.

"After the reading of Sir David Brewster's paper, Mr. Wheatstone said, that having been the first person to introduce Purkinje's beautiful experiment into this country, and having repeated it a great number of times under a variety of forms, he would take the opportunity of stating a few particulars respecting it, which appeared not to be generally known. The experiment succeeds best in a dark room, when, one eye being excluded from the light, the flame of a candle is placed by the side of the unshaded eye, but so as not to occupy any of the central part of the field of view. So long as the flame of the candle remains stationary, nothing further occurs than a diminution of the sensibility of the retina to light; but after the flame has been moved upwards and downwards, through a small space, for a length of time, varying with the susceptibility of the individual on whom the experiment is tried, the phænomenon presents itself. The blood-vessels of the retina, with all their ramifications, exactly as represented in the engravings of Sœmmerring, are distinctly seen, apparently projected on a plane before the eye, and greatly magnified. The image continues only while the flame is in motion: directly, or soon after, the flame becomes stationary, it dissolves into fragments and disappears.

"Mr. Wheatstone dissented from the ingenious explanation of this appearance offered by Sir David Brewster, and also from that opinion stated to be the generally received one; and begged to repeat the solution he had published, and which he had not since been induced to relinquish. Mr. W. observed, that there was no difficulty in accounting for the image; it evidently was a shadow resulting from the obstruction of light by the blood-vessels spread over the retina; the real difficulty was to explain why this shadow is not always visible. To account for this, Mr. W. adduced several facts, which tended to prove *that an object, either more or less luminous than the ground on which it is placed, when continuously presented to the same point of the retina, becomes invisible; and the rapidity of its disappearance is greater as the difference of luminous intensity between the object and the ground is less; but by continually shifting the place of the image of the object on the retina, or by making it act intermittently on the same point, the object may be*

* See Lond. and Edinb. Phil. Mag., last Number, p. 43, and also vol. i. p. 318.

rendered permanently visible. To apply this explanation to the phænomenon in question, Mr. W. observed, that whenever the flame of the candle changes its place, the shadows of the vessels fall on different parts of the retina; which is evident from the motion of the figure while the eye remains still, which is always in a contrary direction to that of the flame. Hence the shadow, being thus made to change its place on the retina, remains, according to the law above stated, permanently visible; but instantly the flame is at rest, the shadow also becomes stationary, and consequently disappears.

"Mr. Wheatstone then exhibited an instrument for showing an original variation of this experiment: it consisted of a circular plate of metal, about two inches in diameter, blackened at its outer side, and perforated at its centre with an aperture about as large as an ordinary gun-hole; to the inner face was fixed a similar plate of ground glass. On placing the aperture between the eye and the flame of a candle, and keeping the plate in motion, so as to displace continually the image of the aperture on the retina, the blood-vessels will be seen distributed as before, but will now appear brighter, and the spaces between the ramifications will be seen filled with innumerable minute vessels, anastomosing with each other in every direction, which were invisible in the former experiment. In the very centre of the field of vision there is a small circular space, in which no traces of these vessels appear. Mr. W. remarked, that the absence of these minute obstructions to light will probably account for the greater distinctness with which small objects are there seen, and also for the difference of colour observed by anatomists in that spot of the retina."

In this experiment Mr. Wheatstone has described, 1st, the common method of seeing the blood-vessels, and 2ndly, an original variation of the experiment by which the blood-vessels are seen much more distinctly and completely.

As Mr. Wheatstone was so kind as to lend me, when at Oxford, his plate of metal, &c., I was enabled to see the very phænomenon which he saw, and I have repeated the experiment fifty times since under many modifications. I have therefore no hesitation in asserting that the *ramifications* exhibited by Mr. Wheatstone's apparatus *are not blood-vessels*, but are nothing more than the ramifications described in my paper already referred to (Lond. and Edinb. Phil. Mag., vol. i. p. 170. §. 1.). If we throw aside the ground glass in Mr. Wheatstone's apparatus, and look at a luminous surface through the circular aperture when moved as he describes, the same phænomenon will be seen; and if we substitute a rectilineal aperture, and make the line of motion perpendicular, or nearly so, to its longest sides, the phænomena will be seen still more distinctly; and if we look through one or more narrow slits, as in my experiment, the effect will be the same. In short, the edges of the circular aperture in Mr. Wheatstone's apparatus perpendicular to the line in which the aperture is moved, perform the part of the rectilineal slit or slits in my experiment. Mr. Wheatstone will have no difficulty in recognising the perfect identity of the two experiments, and he will therefore see that the ramifications are nothing more than the new forms given to the luminous and dark parallel lines produced by the action of light upon the retina. In order to demonstrate this, let us use Mr. Wheatstone's own apparatus. The general direction of the ramifications is invariably perpendicular to the direction in which the aperture is moved. If we change this direction from a horizontal to a vertical line the ramifications change their direction also, so that we can give them any inclination we please. They cannot, therefore, be pictures or representations of any blood-vessels in the eye.

This unequivocal result would have induced me to believe that the ramifications seen by the common method had a similar origin, and were owing to the action of the rectilineal sides of the flame upon the retina, had I not succeeded in seeing this phænomenon with my own eyes. At the Observatory of Cambridge, last summer, Sir John Herschel pointed out to Mr. Airy and myself the method by which he saw the ramifications, and we were all successful in observing the same phænomenon. This method scarcely differed from that described by Mr. Wheatstone, but the ramifications which I saw were *toto cœlo* different from those produced by Mr. Wheatstone's apparatus: they had not, indeed, one property in common but that of *ramifying*. The one was seen with great difficulty and occasionally in the middle of a brownish red light, which *did not proceed directly from the candle*; while the other was distinctly and continuously seen in the middle of condensed light *proceeding directly* from the candle or other luminous body.

Regarding, therefore, the phænomenon as real, and the ramifications as occasioned by a blood-vessel of the retina, I shall proceed to examine the different explanations that have been given of it.

The explanation given by Mr. Wheatstone is exceedingly ingenious; and the principle which he lays down, and which is printed in Italics, is in every respect well founded. This property of the retina, by which it is unable to maintain the continued visibility of an object seen *obliquely*, or at a distance from the axis of vision, was communicated by me to the Royal Society of Edinburgh on the 19th of January 1818, and has since appeared in several elementary works on optics; and it is a necessary corollary from the law of oblique vision, that any movement of the object must restore its visibility by removing the cause of its disappearance, namely, the continued action of the light upon the retina.

So far, therefore, Mr. Wheatstone's explanation is unimpeachable; but when he states that the motion of the flame causes *the shadows of the vessels to fall on different parts of the retina*, we can no longer follow him. Unless the blood-vessel is placed at a certain distance *in front of the retina*, and consequently in the vitreous humour, it can have no *moving* shadow; and unless it is within the refracted cone of rays which proceed from the candle, it can have no shadow either moveable or stationary. If the shadow here referred to, be the shadow produced from the direct light of the candle, then the blood-vessel would appear across the visible flame of the candle, and not at the side of it in the reddish brown light. But independent of these objections to the application of the optical principle previously laid down, there are two facts which appear to be conclusive against the explanation: the one is, that the blood-vessels of the retina are not at a distance from it; and the other, that the ramifications may be seen distinctly when the candle is not in motion *. There is one objection more to this explanation, which appears to me a formidable one: the ramifications ought to be distinctly and readily seen when the light which forms the shadow is reduced to the same state of dilution, and the same colour, as the reddish brown light on which they appear. This experiment I have repeatedly made with light of all degrees of dilution and divergency, but I have never been able to see a trace of the ramifications.

If the ramifications in question are the representation of a blood-vessel, it becomes very interesting to ascertain the cause to which their visibility is owing. The first step in the inquiry is obviously to determine the origin of the reddish brown light in which the phænomenon is seen. It is quite clear that the brown light is no part of the cone of refracted rays that proceed from the candle: it is equally clear that it is not produced by two or more reflections from the curved surfaces which bound any of the humours of the eye, because in this case it would be of the same colour with the light of the can-

* The force of this last objection will depend on the circumstances of the case. Mr. Wheatstone says that the image "continues only while the flame is in motion," and that "directly, or soon after, the flame becomes stationary, it dissolves into fragments and disappears." Now if this is a phænomenon of oblique vision, the image ought not to disappear permanently. One part of it should disappear while another part remains visible, and the whole may for a short time continue invisible; but it will soon reappear, because it would require great steadiness, both in the hand and head of the observer, to keep the shadow on the same part of the retina, though even this would not ensure its permanent invisibility. If this, therefore, were a phænomenon of indirect vision, the difficulty would consist in losing sight of the ramifications, whereas the difficulty really consists in seeing them; and this difficulty is so great with me, that I have never been able to see them again since I saw them at Cambridge.

dle: and I have besides ascertained that it has no focus; for if it had, it would expand and contract by any variation in the distance of the candle. It cannot proceed from any imperfect transparency in any of the coats or humours of the eye, because it is seen in eyes that have the most pure and perfect vision. It must, therefore, be light produced by a physiological action, or light propagated from, or induced by, the direct image of the candle upon the retina; and if this is the case, the explanation which I formerly gave of the phænomenon is likely to be the true one. The blood-vessels touching the retina will deaden, as it were, the part of the retina which they touch, or make it less sensible to the propagated light, and hence the blood-vessels will appear delineated in a fainter light than that which surrounds them. The distinctness with which the ramifications will thus be seen will vary with the intensity of the brown light, with the ever changing sensibility of the retina, and with the varying pressure of the blood-vessels themselves. If I could command the vision of these ramifications as Mr. Wheatstone can, it would not be difficult to institute experiments by which the preceding explanation could be cross-examined; and I therefore hope that he will resume the subject in reference to the facts and views which I have ventured to state.

Before concluding this notice I may mention, as connected with the subject, some curious phænomena which appear when we throw a condensed beam of light upon the retina so as to fill the whole eye. This may be done by holding near the eye a convex lens, about an inch in diameter, and an inch or so in focal length, so as to see its whole area filled with the light of a candle or lamp. If we move the lens backwards and forwards quickly, looking steadily at one point of the field, we shall see on each side of the axis of vision the ramifications described in my former paper and in the preceding pages. There are none of them visible within a certain space round the axis of vision; but in the axis of vision there is an irregularly illuminated or shaded circular spot, obviously corresponding to the *foramen centrale* of the retina; and in this spot, and for some distance round it, is seen a sort of network pattern, delineated in dark lines. This pattern* has sometimes all the regularity of one formed geometrically, with dark spots in the centre of each area, and the ground on which the pattern is seen is generally of a faint purple colour. But, what is more remarkable, the luminous field is crossed by ex-

* The very same phænomenon is seen, though less distinctly, when we look steadily at the moving or *flaring* summit of the flame of a candle.

ceedingly faint bands of red and green light perpendicular to the direction of the motion.

When the eye has not been fatigued by light, the luminous ground on which these phænomena are seen has a minutely granular appearance; and by the continued action of the light an apparent effervescence appears over the whole, as if each grain of light, or the minute spaces between the grains, were becoming more or less luminous in succession.

Belleville, Dec. 18, 1833.

XXVI. *Descriptions of some nondescript British Species of May-flies of Anglers.* By JOHN CURTIS, *Esq.*, *F.L.S.*, *&c.**

AS few insects are more remarkable in their œconomy or more employed for the amusement of men than the *Ephemeræ* and *Phryganidæ*, I hope the characters of some new genera and species may induce others to pay attention to these curious and interesting tribes of insects.

The following descriptions are scarcely more than the essential characters: the numbers refer to Mr. Curtis's Guide to an Arrangement of British Insects, in which a list of the species has been given.

Order NEUROPTERA. Fam. EPHEMERIDÆ.

Gen. 734. EPHEMERA *Linn.*

7. fusca *Curt.*

$2\frac{1}{4}$ lines long: dull piceous, the space between the eyes and the base of the anterior legs ferruginous, the others ochreous; filaments longer than the insect, pale lurid, dotted; the articulations long: wings transparent, superior with few transverse nervures and the longitudinal ones most distinct; inferior very small.

Gen. 735. BAETIS *Lea.*

A. The wings very much reticulated.

2. dispar *Curt. Brit. Ent. pl.* 484.

The Pseudimago† of this insect may be the *E. venosa* DeGeer.

7. costalis *Curt.*

5 lines long: slender, pale castaneous, sides of thorax, apex of abdomen and legs ochreous, the joints of tarsi fuscous at the articulations; filaments very long, pale yellow, each joint with the apex black: superior wings with the costa brownish yellow.

6. elegans *Curt.*

$4\frac{1}{2}$ lines: bright ochre, abdomen inclining to pale chestnut; filaments whitish dotted with fuscous, tarsi with all the joints tipped with black: wings beautifully opalescent, stained very pale yellowish brown, the costa darker, especially towards the apex.

* Communicated by the Author.

† By Pseudimago I designate the fourth state of the *Ephemeridæ*.

5. mellea *Curt.*, a beautiful Pseudimago.
5½ lines: bright ochreous, eyes black, segments of abdomen edged with brown, with a trigonate brown mark on the back of each; and the spiracles forming a double row of black dots down each side; filaments longer than the insect, dotted with brown: wings pale yellow, costa a little darker, nervures yellow and brown.

5ª. straminea *Curt.*, a Pseudimago.
4 lines: deep straw-colour, eyes black, segments of abdomen edged with brown, wings fuscous-ochre, tarsi fuscous at the apex, filaments whitish dotted with brown.

8. flavescens *Curt.*, a Pseudimago.
Like the last, but only 2½ lines long.

8ª. lateralis *Curt.*
3½ lines: piceous, shining, an orange spot before the wings; abdomen dull ferruginous, the sides paler, with the spiracles piceous; filaments very long: wings with the 3 costal nervures rosy or ferruginous; legs lurid, excepting the anterior, which are ochreous at the base.

9. semicolorata *Curt.*
3 lines: ochreous, thorax variegated with brown, abdomen banded with the same colour, filaments very long and slender, knees and tarsi brown, superior wings with the basal half and inferior entirely pale yellowish brown.

9ª. carnea *Curt.*
3½ lines: ochreous, with a pink tinge, abdomen darker, filaments twice as long as the insect, pale unspotted; wings elongated, with the costa slightly tinged, the nervures reddish brown.

B. Wings very little reticulated. Pseudimago with the wings ciliated.

11ª. vernus *Curt.*
3½ lines: pale ferruginous; head and thorax piceous above, with an ochreous dot on each side the collar and several beneath the wings, filaments twice as long as the insect, pale and dotted; legs dirty ochre; wings with the costa tinged.

11ᵇ. autumnalis *Curt.*
2 lines: pale castaneous brown; margin of eyes and sides of thorax ochreous; filaments thrice as long as the insect, whitish; legs pale lurid; wings with the anterior margin slightly tinged. Pseudimago more ochreous.

Gen. 736. Cloëon *Lea.*

1. dipterum *Linn.*, marmoratum *Curt.*

8. obscurum *Curt.*
The Pseudimago, I believe, of *C. dipterum*: the wings are fuscous and ciliated.

4. unicolore *Curt.*
4 lines: reddish ochre; filaments lost; legs ochreous; costa pale brown towards the tip; nervures reddish ochre.

6. dimidiatum *Curt.*
3 lines: castaneous brown; collar with an ochreous dot on each side; abdomen, especially at the base, banded with ochre; filaments very long and white, remotely dotted with black; legs straw-colour; nervures of wings very faint.

Gen. 736[a]. Brachycercus *Curt.*

Head short; eyes small and remote, at least in one sex; thorax large and ovate; abdomen not longer than the thorax, terminated by 3 very short setæ, thick at the base: wings 2, rather short but ample, the form of a Musca, with many longitudinal nervures and 3 transverse ones between the costa and disc; legs short, at least in one sex.—*Obs.* These characters, although imperfect for want of specimens, are sufficient to distinguish this group for the present.

1. Harrisella *Curt.*—*Harris's Exposition, tab.* 6. *f.* 3 *&* 1.
5 lines long.

2. Chironomiformis *Curt.*
2½ lines: ochreous shining; eyes black; wings milk white, costa fuscous; legs whitish. This insect somewhat resembles a large female Chironomus.

3. minima *Curt.*
1¼ line. My specimen, taken in Norfolk twenty years since, is much injured; the head and collar are blackish, the body white, legs and filaments pale ochre; wings similar to the last.

Order TRICHOPTERA. Fam. Phryganidæ.

Gen. 748. Limnephilus *Leach.*—*Curt. Brit. Ent. v.* 11. *fol.* 488.

A. Posterior margin of superior wings emarginate.

1. basalis *Curt.*
Expansion of wings 16 lines: pale dirty ochre; superior wings freckled with brown, leaving an oblique plain spot on the disc, with a larger one connecting it with the apex; stigma and a sinuated oblique line at the base piceous, 3 pale spots on the posterior margin; tips of inferior wings ochreous variegated with brown; body green.

2. emarginatus *Curt.*
17 lines: dull ochre; superior wings mottled with a deeper colour, a pale oblique spot on the disc unconnected with one beyond it, and a short narrow one approaching the posterior margin, which has three spots, and the edge of the angle as well as the stigma piceous; apex of inferior wings brownish ochre; body green.

B. Superior wings truncated obliquely at the apex.

3. Strigosa *Gmel.*
20 lines: dull ochre; superior wings more or less freckled with brown, having a dark longitudinal line at the apex of each wing, sometimes obliterated in the superior.

5. binotatus *Curt.*
16 lines: superior wings ochreous yellow, deepest towards the base, with three pale spots on the disc and a large one covering the transverse nervures, a large brown spot at the posterior angle; stigma large ovate and piceous.

6[a]. discoidalis *Curt.*
15 lines: superior wings pale brown, with the costa (as far as the stigma), a large spot below the disc connected with a large one covering the transverse nervures, and numerous dots semitransparent; apical margin of inferior wings fuscous.

8. marmoratus *Curt.*
14 lines: superior wings dull pale ochreous, variegated with brown, leaving an oblique transparent spot below the centre, a large one covering the transverse nervures (which are brown) and several dots surrounding them; stigma brownish.

9. nebulosus *Curt.*, probably a var. of the next.
14 lines: superior wings yellowish ochre, the posterior margin brown, with a pale lunule on the edge, the inferior margin brown also, with the disc pale, forming 2 lobes below; stigma deep ochreous.

10. apicalis *Curt.*—*rhombica Ahr.*, *fas.* 9. *pl.* 13.
14 lines: superior wings dull ochreous, the lower portion variegated with bright brown, leaving an oblique subreniform transparent spot on the disc, a larger one covering the transverse nervures (which are piceous) with various dots, and a lunule on the posterior margin; stigma piceous.

10b. Stigma *Curt.*
14½ lines: superior wings yellowish ochre; stigma piceous.

10c. lunatus *Curt.*
14½ lines: superior wings brown, the costa, a spot on the disc, and a large sublunulate spot beyond it semihyaline, there are four pale rays between this and the posterior margin which is spotted brown and whitish; stigma large and piceous.
A single specimen of this fine species was taken last July at Whittlesea Mere by J. C. Dale, Esq.

12. fenestralis *Curt.*
10 to 15 lines: superior wings dirty white freckled with pitchy brown, generally leaving a plain spot near the base, an oblique one on the disc, a sublunulate one beyond the transverse nervures connected with another on the inferior margin; stigma piceous and spotted pale.

13. bipunctatus *Curt.*
11 to 13 lines: superior wings pubescent, pale brown, darkest towards the apex, very much freckled with ochreous white, the costa plain, as well as a lunulate spot on the disc, two smaller ones beyond it forming an ∂, a pale spot on the posterior margin; stigma and some of the longitudinal nervures piceous dotted with white.

14. affinis *Curt.*
11 lines: superior wings more or less pubescent, fuscous freckled with pale dull ochre; stigma, most of the nervures and the inferior margin spotted with piceous.

23. sparsus *Curt.*
11 to 13 lines: superior wings very silky brown freckled with ochre, with an ochreous mark on the inferior margin towards the angle, inferior wings pale fuscous, brown at the apex; antennæ brown, annulated with ochre; anterior tibiæ annulated fuscous and ochre.

23b. tenebricus *Curt.*
13 lines: superior wings very pubescent, dark brown more or less freckled with ochre, the costa often very ochreous, a spot of the same colour before the stigma and another opposite to it on the posterior margin; stigma piceous; inferior wings slightly fuscous, with the apex brown.

24. cœnosus *Curt.*
11 lines: ochreous brown; superior wings rather short and broad; stigma

scarcely visible, with a pale spot on each side the posterior angle; inferior wings paler except at the tips.

A single specimen in Scotland.

25. Vinculum *Curt.*

9½ lines: superior wings deep fuscous; the stigma a little darker with numerous pale dots, and a larger one before the stigma, two opposite on the inferior margin and another upon the transverse nervures; apex of inferior wings fuscous.

15. obscurus *Curt.*

11 lines: superior wings fuscous ochre; the stigma brown, a pale dot at the disc and 2 interrupted transverse lines of dots beyond it; inferior wings tipped with fuscous.

11. Auricula *Curt.*

10 lines: superior wings pubescent, dark ochre with a hyaline spot on the disc and a larger ear-shaped one covering the transverse nervures; stigma obscure; tips of the inferior wings pale brown.

16. ochraceus *Curt.*

11 lines: superior wings pale fuscous yellow, freckled with yellowish spots; stigma obscure; inferior wings slightly ochreous at the apex.

18. bipartitus *Curt.*

10½ lines: superior wings pale ochreous, very much freckled with brown, excepting the costa, the sixth or apical longitudinal nervure forming a brown line; apex of inferior wings tinged with ochre.

19. Consobrinus *Curt.*

11 lines: superior wings ferruginous-ochre freckled with brown excepting the costa, the posterior angle brown freckled with ochre; apex of inferior wings ochreous.

21. terminalis *Curt.*

11 lines: superior wings ferruginous ochre, the inferior portion freckled with brown, leaving a large plain subtrigonate spot on the posterior margin; apex of inferior wings pale ochreous.

22. centralis *Curt.*

11 lines: superior wings brown freckled with ochre, the costa entirely ochreous, a pale spot on the disc, a smaller one beyond it, and a large semi-orbicular one on the posterior margin; inferior wings ochreous at the tips.

17. incisus *Curt.*

10 lines: wings scarcely longer than the body, superior ochreous, the inferior margin and the nervures freckled with brown, the discoidal cell very long; inferior wings deeply notched beneath the apex, which is slightly ochreous.

17[b]. elegans *Curt. Brit. Ent. pl.* 488.

26. luridus *Curt.*

13 lines: superior wings dirty reddish ochre freckled with paler dots, the nervures darker; inferior wings with the apex tinged with the same colour.

27. nervosus *Leach.—Sam. pl.* 7. *f.* 3.

14 to 15 lines: superior wings ochreous brown, with a pale lunulate whitish spot near the centre and a dot at the base of the third marginal cell.

C. Superior wings broad and rounded at the apex.

28. radiatus *Leach.*

19 to 22 lines: pale dirty ochre; superior wings with brown spots and lines variegating the cells, the transverse nervures surrounded with the same colour, the posterior margin brown, with a long pale stripe down each cell, with a brown line in the centre.

29. hieroglyphicus *Curt.*

2 inches: ochreous; superior wings with numerous brown markings (excepting a broad portion next the costa) forming irregular pale spots.

30. Vibex *Curt.*

14 to 20 lines: ochreous; superior wings mottled with pale brown, excepting the costa, forming innumerable ochre dots.

31. latipennis *Curt.*

19 lines: pale ochreous, silky; superior wings with the edges of the nervures very pale fuscous, forming indistinct rays towards the apex.

32. stellatus *Curt.*

16 to 17 lines: superior wings very pubescent fuscous ochre, with pale lines at the base and centre of the discoidal nervures, 2 or 3 small spots at the base, a bilobed one near the centre, 2 dots by the transverse nervures and a curved series of pale streaks beyond them; inferior wings fuscous ochreous, very pale at the base.

D. Wings short, rough or hispid.

33. brevipennis *Curt.*

11 lines: superior wings scabrous tawny ochre, a pale dot at the base of the third marginal cell, another below the centre, and a third near it towards the posterior angle, posterior edge dark, dotted pale.

34. villosus *Fab.*

8 to 11 lines: superior wings hispid, being clothed with bristly erect hairs, ochreous brown, with a pale dot on the inferior margin towards the posterior angle and another above it.

[To be continued.]

XXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

1833. Nov. 21.—A PAPER was read, entitled, "Historical Notice to the supposed Identity of the large mass of Meteoric Iron now in the British Museum, with the celebrated Otumpa Iron described by Rubin de Celis, in the Philosophical Transactions for 1786." Communicated in a letter from Woodbine Parish, jun., Esq., F.R.S., to Charles Konig, Esq., Foreign Secretary of the Royal Society.

The mass of iron in question was transmitted to Buenos Ayres, for the purpose of being manufactured into fire-arms, at the period when the people of that country declared themselves independent of Spain; but a supply of arms having in the meanwhile arrived, it was deposited in the Arsenal, and afterwards given to Mr. Parish, who transmitted it to England. Its identity with the mass of iron described by De Celis, though probable, is not exactly determined.

A paper was also read, entitled, "Observations of Nebulæ and

Clusters of Stars, made at Slough, with a Twenty-feet Reflector, between the Years 1825 and 1833." By Sir John F. W. Herschel, K.H., F.R.S.

This paper contains the results of observations begun in 1825, and assiduously prosecuted till the commencement of the present year, for the purpose of reviewing the nebulæ and clusters of stars discovered by his father, the late Sir William Herschel, and also of extending his discoveries, and enlarging our knowledge of the nature and physical constitution of those remarkable and mysterious bodies. Since the recent improvements in the achromatic telescope, and the increased diligence of astronomers in surveying every part of the heavens, and detecting the passage of comets, the want of an extensive list of nebulæ has become continually more urgent; and hence the author was induced to supply, as far as he was able, that deficiency, which he has now attempted by simply stating the results of his own observations, in preference to waiting until he could present them to the Society in the more complete form of a general catalogue of nebulæ and clusters visible in this latitude. All the observations here given have been reduced to a common epoch, and arranged in the order of right ascension: and in every case where the same object was observed more than once, all the observations relating to it have been collected together; by which means they not only can be used as a catalogue for reference, but each result carries with it its own weight and evidence.

Great and various are the difficulties attending inquiries of this nature. Many of the nebulæ present a surface so large and ill defined, that it is not always easy to determine where the centre of greatest brightness is situated. Vast numbers of the nebulæ, indeed, are so extremely faint, as to be with difficulty perceived, till they have been some time in the field of vision, or are even just about to quit it; so that the observations become hurried and uncertain. In those parts of the heavens where they are most crowded, their prodigious number, as well as their variety, and the interest they excite, render it scarcely possible to proceed with that methodical calmness and regularity which are necessary to ensure numerical correctness. It is also to be recollected, that it is only during the months of March, April, and May, that the richer parts of the heavens can be advantageously observed, and then only in the complete absence of the moon and of twilight. From all these causes conjoined, it will be readily understood, that a much greater latitude of error is incident to observations of nebulæ than to those of stars.

The observations registered in this paper comprise 2500 nebulæ and clusters of stars,—a number equal to that of those observed by Sir William Herschel: only about 2000, however, are common to both collections, the remaining 500 of the author's being new. Of these last, by far the greater proportion are objects of the last degree of faintness, only to be seen with much attention, and in good states both of the atmosphere and the telescope. The author generally made a sketch of any remarkable nebula that presented itself; and these drawings accompany the paper. Among these are representations of some very extraordinary objects, which have not hitherto sufficiently engaged the attention of astronomers, and many of which possess a symmetry of parts, and a unity of designs, trongly marking them as systems of a definite nature, each complete in itself, and subservient to some distinct, though to us inscrutable, purpose.

In an Appendix, the author enters into a detailed account of the manner in which the reductions have been executed, and how the numbers set down in the catalogue are concluded from those registered at the moment of observation. For effecting these reductions, he pursued a method materially different, and much more convenient and exact, than he employed to reduce his earlier catalogue of double stars.

Various remarks are next made on the figured nebulæ. It often occurred to the author, to notice a peculiar state of the atmosphere, which is quite independent of fog or haziness, in which all large stars above the seventh magnitude appear surrounded with photospheres, of a diameter of two or three minutes, or even more, and exactly resembling those about some of the finer specimens of nebulous stars. These appearances come on suddenly, seldom last long, and disappear as unexpectedly as they come: hence the inference is drawn, that the true cause of this phenomenon is atmospheric, and that it is perhaps connected with some highly rarefied material, disseminated in cloud-like, though invisible, masses in the very highest regions of our atmosphere, and possibly the same with that which, when ignited by the passage of electric currents, gives rise to many, if not all, the phenomena of the aurora borealis. Frequent instances occur of the proximity of minute stars to nebulæ; an appearance which naturally suggests the idea of their composing planetary systems: for the enormous magnitude of the nebulæ, and its consequent probable mass, may, notwithstanding the rarity of its material, give it a gravitating energy, capable of retaining, in orbits three or four times their own diameter, and in periods of great length, small bodies of a star-like character.

Lastly, the author offers some remarks on the constitution of nebulæ which have an elongated or elliptical form, of those which are double, and of those to which the epithets of *hairy* or *filamentous* have been applied; and considers their relations to ordinary physical laws.

Anniversary Meeting, Nov. 30th.

His Royal Highness the Duke of Sussex, K.G., delivered the following Address:—

Gentlemen,

The third anniversary of my election to this Chair affords me again the opportunity of expressing my grateful thanks for the kindness which I have continued to receive from you. I would willingly enlarge upon a topic which is so grateful to my feelings, were I not conscious that by so doing I should merely vary the form of phrases which the natural expression of my sentiments prompted me to use when I have before had the pleasure of addressing you, whilst the sentiments themselves remain not merely unchanged, but, I trust, likewise unchangeable. If I am thus brief, therefore, Gentlemen, in the public declaration of my acknowledgements, from a fear of being tedious by their too frequent repetition, I hope that you will not upon that account consider them the less sincere, or that the long experience which I have had of your support and co-operation has made me less sensible of their value.

When I last had the honour of addressing you, it was a source of pride and happiness to me to be empowered to announce to you the gracious intentions of His Majesty to continue to the Royal Society the Annual Grant of two Gold Medals, which had been previously conferred on the Royal Society by his Royal Predecessor.

It must be well known to you, Gentlemen, that these Royal Medals were not adjudged during the two first years that I presided over the Royal Society; and as there exist many circumstances connected with the original grant and distribution of those Medals, as well as causes leading to their temporary discontinuance, with which the Fellows may not be generally acquainted, I trust that I may be allowed to enter into some details respecting them.

His late Majesty King George the Fourth announced, towards the close of the year 1825, through the medium of the Secretary of State for the Home Department (Sir Robert Peel), his gracious intention of founding two Gold Medals, of the value of Fifty Guineas each, to be annually awarded as honorary premiums, under the direction of the President and Council of the Royal Society, in such a manner as should, by the excitement of competition among men of science, seem best calculated to promote the objects for which the Royal Society was originally instituted. This munificent gift of the Patron of the Royal Society was of course accepted by the President and Council with every expression of gratitude for so valuable an addition to their means of promoting the interests of science; and it was resolved that, in conformity with His Majesty's Commands, the Royal Medals should be adjudged for the most important discoveries or series of investigations completed and made known to the Royal Society in the year preceding the day of their award; that their presentation should not be limited to British subjects; and that His Majesty's effigy, if such should be the Sovereign's pleasure, should form the obverse of the Medals; and that two Medals from the same die should be struck upon each foundation, one of gold and the other of silver.

Upon proceeding to the distribution of the Medals, it was found that the limitation of time which these Resolutions fixed was of such a nature as to interfere most materially with the proper observance of the object proposed to be secured by their foundation; and the period was therefore, with His Majesty's sanction, extended to five years: in accordance with this arrangement the Medals continued to be awarded until the year 1830, inclusive, when the demise of His late Majesty took place, and in which year I had the honour of being elected to fill the Chair of the Royal Society.

Mr. Chantrey, to whom, in conjunction with Sir Thomas Lawrence, was intrusted the selection of the subject for the Medal, furnished the cast for the medallion of the head of His late Majesty, which was to form the obverse of it, while Sir Thomas undertook to compose the design for the reverse. Unfortunately, that distinguished artist, either from over-delicacy or over-anxiety to produce a work of art worthy of the object for which it was intended, or from that spirit of procrastination which was unhappily too common with him, delayed its execution from year to year, and died without leaving behind him even a sketch of his ideas respecting it, though the character of such a design as would be at once classical and appropriate to the purpose, was the subject of frequent conversation, and even of favourite speculation with him. From these and other causes, to which it is not necessary for me now to advert, it arose, that, at the demise of His late Majesty, although the adjudication of ten Medals had been formally made and announced from the Chair of the Royal Society, not even the dies, much less the Medals, were forthcoming for the purpose of distribution to the various distinguished persons, some of them foreigners, to whom they had been awarded.

It cannot be necessary for me to impress upon you, Gentlemen, that the non-completion of an engagement so solemnly entered into with the whole republic of men of science, would have brought discredit not merely upon the Royal Society, but upon the personal honour of a Monarch of this country, whose name it is our especial duty as Fellows of the Royal Society, to hand down unsullied to posterity, as our munificent Patron and benefactor; and as no funds had been placed at the disposal of our Treasurer, nor in the hands of any other ostensible person to meet the very heavy expenses which must be incurred for cutting the dies and furnishing the Medals already awarded, I felt it to be my duty, when I succeeded to this Chair, to recommend to the Council the suspension of any further adjudgment of the Medals until I could have an opportunity of ascertaining the nature of the commands which had been issued concerning them by the late Sovereign through his official advisers or otherwise, and also of taking the pleasure of His present Majesty respecting their continuance in future, and the conditions to which they should be subject. These inquiries terminated in the most satisfactory manner. On a proper application to those who were intrusted with the ultimate arrangement of His late Majesty's affairs, prompt measures, as far as lay in their power, were adopted for the immediate fulfilment of every pledge which it was conceived had been given to the Royal Society and to the public at large in the name of George the Fourth.

The dies for the Medals upon the old foundation are now completed, and ready for distribution; they bear upon the one side the likeness of His late Majesty, while the reverse represents the celebrated statue of Sir Isaac Newton, which is placed in the chapel of Trinity College, Cambridge, with such emblematical accompaniments as seemed best calculated to indicate the magnificent objects of the researches and discoveries of that great philosopher, whose peculiar connexion with the Royal Society forms the most glorious circumstance in its annals.

After having settled that part of the business, and apprized the King of my success, I then ventured to petition His Majesty for the continuance of that protection and munificence which the Royal Society had ever experienced from His Illustrious Predecessors. The Sovereign, with that just and enlightened zeal for the promotion of every object allied with the honour and prosperity of this country, which as a loyal subject I acknowledge with gratitude, while as an affectionate brother I recognise it with pride, acceded at once to my request, accepted the charge devolved upon him by the demise of the late King, and ordered, in consequence, that a fresh die should be cut, and that his effigy should form the obverse side of the medal. This work also is completed. All the dies have been executed by Mr. Wyon with such boldness of outline, depth, and delicacy of finish, as do him the highest credit: and I trust that the medals will be considered in every way worthy of the exalted rank and dignity of the Illustrious Personage in whose name this mark of Royal favour is intended to be conferred.

I am well aware that a diversity of opinion exists respecting the advantages which are likely to be conferred upon Science by a frequent distribution of medals. It is said that they must either confirm or contradict the judgement which has been either already pronounced, or which posterity will most certainly hereafter pronounce, upon the merits, pretensions, and influence of the discoveries or series of investigations which such medals are designed to commemorate: that in the first case they can confer no additional honour upon their author, whose rank has already been ascertained and fixed by the sentence of a higher tribunal, while, in the second, they can only tend to compromise the character of the scientific body by whose advice they are conferred. It is true that I would not claim infallibility for the united judgement of any association, or of any body of men, however eminent their scientific rank may be: but it is the peculiar privilege of the great masters of Science, (and more particularly so when acting or speaking as a body,) to be able to anticipate, though not without the possibility of error, the decision of Posterity, and thus to offer to the ardent cultivator of Science that highest reward of his labours, as an immediate and well assured possession, which he might otherwise be allowed silently and doubtingly to hope for, but never be permitted to see realized: and though some powerful minds might be content to entrust the complete developement of their fame to the fulness of time, and might pursue their silent labours under the influence of no other motives but such as are furnished by their love of truth, the gratification derived from the discovery of the beautiful relations of abstract science, or from the contemplation of the agency of a Divine Mind in the harmonies and constitution of the physical world, yet it is our duty and business to deal with men as we find them constituted, and to stimulate their exertions by presenting to their view honourable distinctions attainable by honourable means; to assure them that the result of their labours will neither pass unnoticed nor unrewarded; and that there exists a tribunal to which they may appeal, or before which they can appear, whose decision is always for honour, and never for condemnation.

It is for these reasons, Gentlemen, that I feel myself justified in expressing my opinion that the power possessed by your Council of conferring honorary rewards is a most salutary power, provided it be exercised boldly, impartially and diligently; and that it may greatly promote the taste for scientific pursuits in this country, by presenting a more immediate prospect than would otherwise exist, of a public and distinguished recognition of any valuable discovery, or of the completion of any important and laborious course of investigation.

I had occasion, Gentlemen, when I had last the honour of addressing you, to remark that there were many circumstances in the constitution of society in England, and perhaps in the form and working of our Government, which were unfavourable to the cultivation of Science as a distinct and, as it were, a Professional employment. Though many of the causes of this evil, if so it may be considered, are too deeply seated to be reached by any legislative enactment, and though its existence may be the result of a system, the general effects of which are favourable to the interests and happiness of society at large, yet I think it is the duty of a wise Government to neglect no opportunity of promoting, by liberal encouragement, the developement of the intellectual as well as of the physical resources of a nation. Without venturing to give an opinion from this Chair, which it would ill become me to do, whether the various Administrators of the Government of this country, for more than a century past, have adequately fulfilled this duty, by animating individuals to the cultivation of Science by all the influence at their command, I rejoice and feel proud at finding myself at full liberty to give free utterance to the language of my feelings when speaking of the Royal Patron of the Royal Society, who has shown himself in this as in every other capacity, the Friend, the Protector, and the Promoter of whatever is dignified with the name and character of Science in this country. The King, Gentlemen, is the Fountain of Honour; and although His Majesty has been graciously pleased to authorize the President and Council of the Royal Society to act as his Official Advisers, in awarding his Royal Medals, he will not on that account regard them as less worthy of being considered as the immediate gifts of his Royal bounty, and as the honourable symbols of his Royal approbation.

It will be my first duty, Gentlemen, to distribute the Ten Royal Medals which have been already adjudged during the life-time of His late Majesty, to Philosophers who are amongst the most illustrious in this country or in Europe: they form a glorious commencement of a philosophical chivalry, under whose banners the greatest amongst us might feel proud to be enrolled; and though it may appear presumptuous in me to hope that a constant succession of associates can be found, either at home or abroad, who shall be considered worthy of being ranked with those noble Founders of this Order, yet I am confident that the Council of the Royal Society will feel an honourable pride in maintaining the character of the Body whose Members are to be constituted by their choice.

In proceeding now, therefore, to call your attention, Gentlemen, to the series of great men to whom those Medals have been awarded, I shall not presume to state in detail the specific grounds upon which the decisions of your Council were founded, but confine myself to little more than their enumeration in the order of time, feeling that it would be unbecoming in me to attempt to assign them those stations which they either have taken, or are destined hereafter to take, in the temple of fame.

The first name upon the list is that of Dr. John Dalton, a venerable Philosopher, whose developement of the Atomic Theory and other important labours and discoveries in physical science have, at the eleventh hour, (I blush to own that it was not earlier,) first abroad, and secondly at home, secured him that public recognition of his scientific rank to which he has long been entitled. With him, Gentlemen, *posterity* may be said to have already commenced, and though full of years and honour, I rejoice to hear that he still retains the same zeal and vigour in the pursuits of science which have carried him forwards from his earliest youth in his career of discovery, in spite of all the discouragements of confined means and of the most laborious and depressing employments. It gives me great pleasure to learn that His Majesty has lately expressed his Royal approbation of his services to science by the grant of a pension, if not commensurate with his services, at least as considerable as the severity of existing regulations will allow; though I cannot refrain from expressing on this occasion my regret at the very narrow limits within which the munificence of the King and the generosity of the Nation should be confined.

The second Medal for the same year was awarded to Mr. Ivory, the first of our mathematicians who transplanted to this country the profound analytical science which LaGrange, Laplace, LeGendre, Gauss and others upon the continent, had applied to the most important and sublime physical inquiries. The dignity of such investigations has not suffered by the association of Mr. Ivory's name with them, and the Transactions of the Royal Society present frequent and honourable records of his valuable labours. It is, however, a gratifying circumstance to find that Mr. Ivory is no longer a solitary cultivator of these sublime sciences; but that an English School, of which he may be considered as the Father, is now rising, and must continue to rise, whilst it boasts of such masters as our Herschels and Airys, our Lubbocks and Hamiltons, and looks forward to such disciples as they are likely to form.

The Medal which was awarded to Sir Humphry Davy was a tribute of respect to that great Philosopher towards the conclusion of his labours. He had already retired from the Chair of the Royal Society, under the admonition of those infirmities which were destined too speedily to terminate his valuable life; and the Council availed themselves of the first opportunity of marking their sense of the honour which he had conferred upon his country by his brilliant electro-chemical and other discoveries, by awarding to him, as a Fellow, that Medal which, from natural feelings of delicacy, they could not have offered to their President.

In the following year a similar tribute of gratitude and respect was paid to Dr. Wollaston, who had so long honoured the Royal Society by his services and his scientific contributions, and who, towards the close of his life, had augmented its means of usefulness by his liberality.

The fame of these two illustrious men is established upon too firm a basis to require or receive additional strength or permanence from any honours which we can pay to their memories; but there are some who were connected with them by the tenderest ties of kindred and affection, who are in part the depositories and inheritors of their honours: these may cherish the possession of such monuments, as recording the reverence and respect of their contemporaries and fellow-labourers. To their hands, therefore, we commit them, as our last public offering to their memories. *Illi habeant secum, serventque sepulchro.*

The two other Medals for the corresponding years were awarded to two distinguished foreign Astronomers. The first, to Professor Struve, of Dorpat, who is so justly celebrated for his numerous and valuable observations of double stars,—a department of astronomy which is daily acquiring an increase of interest and importance, from the new and extensive views which it is beginning to open to us of the constitution of the remoter parts of the universe, and of the laws which seem to govern some at least of the periodical changes which they are undergoing. The second, to Professor Encke, of Berlin, the greatest of modern astronomical calculators, who first determined and predicted the motion of the comet which is justly signalized by his name, with an accuracy approaching to that which before belonged to the ephemerides of the planets only; and who still more has subjected the discrepancies between its tabulated and observed places to so accurate an analysis as to make them the foundation of the most novel and unexpected speculations respecting the existence of a resisting medium, which is capable of sensibly affecting the motions of those extraordinary bodies which obey the laws of gravity, at the same time that they seem to present few or none of those characters with which our notions of matter and substance are commonly associated.

The Medals for the years 1829 and 1830 were adjudged to Sir Charles Bell, to Professor Mitscherlich of Berlin, to Sir David Brewster, and to M. Balard of Montpellier.

To the first, for his elaborate experiments and discoveries relating to the nervous system, which place him in the highest rank of the physiologists and anatomists, not merely of this country, but of Europe.

To the second, for his theory of isomorphism, one of those great generalizations in the sciences of chemistry and crystallography which are reserved for men of large and extensive views, and which may be considered as constituting a great epoch in their history.

To the third, for his discoveries relating to the polarization of light, the most important laws of which he determined; forming one of those great series of experimental investigations relating to the properties of light and the optical properties of crystals which are unrivalled, since the time of Newton, for their variety, their delicacy, and perhaps also for their theoretical importance.

To the last, for a singularly successful and well developed example of chemical analysis, which terminated in the discovery of a new, and hitherto undecompounded body, Bromine.

I now come to the consideration of the Medals upon the Foundation of His present Majesty; and it is the King's pleasure that the President and Council of the Royal Society should be considered as his official advisers, in the award of an honour which emanates immediately from himself. His Majesty has also been graciously pleased to prescribe the general Rules and Principles which shall regulate their distribution hereafter. The King has therefore commanded that they shall be adjudged annually, and that the award shall be announced on the day of the Anniversary Meeting of the Royal Society; that the Memoirs which shall be entitled to receive them, whether composed by Foreigners or by Englishmen, shall be communicated to the Royal Society; and that the *general* subject matter of such Memoirs shall be prescribed and announced by the Council at least three years preceding the day of their award: and also, that for the present and the two following years, the principle of their distribution shall be the same as that which has hitherto been adopted, with the additional condition, that the succession of branches of science which shall be selected as entitled to these rewards, shall be the same as that which shall be hereafter followed when the cycle of their regular distribution begins.

The selection of the subjects which should compose this cycle was left to the Council of the Royal Society, who have made such a choice as seemed to them best calculated to comprehend every department of science and to prevent the jealousies which might arise from the recurrence of similar subjects in immediate or too close succession: the subjects themselves and their periodical order (determined by lot) are as follow:—

1. Astronomy.
2. Physiology, including the Natural History of Organized Beings.
3. Geology and Mineralogy.
4. Physics.
5. Mathematics.
6. Chemistry.

In conformity with these Regulations, which form the existing law for the distribution of the Royal Medals, they have been awarded for the current year to Professor de Candolle, of Geneva, for his numerous and valuable researches and investigations in Vegetable Physiology, as detailed in his Work, entitled "Physiologie Végétale," published in the year 1832; and to Sir John Frederick William Herschel, for his Paper "On the Investigation of the Orbits of Revolving Double Stars," inserted in the Fifth Volume of the Memoirs of the Royal Astronomical Society.

The science of Vegetable Physiology has at all times presented extraordinary difficulties, and although it has employed the talents and the industry of a great number of philosophers, from the earliest period, little progress has been made in obtaining an exact knowledge of the minute organization of plants, and of the mode in which their functions are exercised, at least, when compared with the great advance which has taken place in the analogous sciences which relate to the comparative anatomy and physiology of animals.

The structure of vegetables, in consequence of its minuteness and intricacy, is involved in the greatest obscurity; its investigation requires the application of powerful microscopes, and is liable to all the fallacies peculiarly incident to such observations: and the greater part of vegetable physiology being dependent on the full and accurate knowledge of that organization, is exposed to the same causes of uncertainty. But the progress of this department of science has suffered less from the want of accurate and sufficiently multiplied observations, than from the absence of a well-compacted and consistent theory to connect them together; and it was chiefly with a view to supply this great deficiency that the admirable work of Professor de Candolle was written, which has been selected by the Council as justly entitled to one of the Royal Medals. There is, in fact, no branch of botanical science which has not been greatly benefited by his valuable labours: his *Théorie Élémentaire de la Botanique* and his *Organographie Végétale* have made most important additions to our knowledge of descriptive botany, whilst in his *Physiologie Végétale*, by a most careful analysis and examination of the influence both of external and internal physical agents upon the organs of plants in the great functions of their nutrition and reproduction, by tracing them throughout the whole course of their operations, and by connecting their results with the well-known and well-established deductions of chemistry and other sciences, he has shown that he is also entitled to claim the rank and distinction of an inductive philosopher of a very high order.

The mention of the name of the second of these distinguished Philosophers to whom the Royal Medals for the present year have been adjudged, recalls my attention to the circumstances under which he has recently quitted his home and his country to pursue his labours in another hemisphere. He has devoted himself, as you well know, for many years at least, as much from filial piety as from inclination, to the examination of those remote regions of the universe into which his illustrious father first penetrated, and which he has transmitted to his son as an hereditary possession, with which the name of Herschel must be associated for all ages. He has subjected the whole sphere of the Heavens within his observation to a repeated and systematic scrutiny. He has determined the position, and described the character of the most remarkable of the nebulæ. He has observed and registered many thousand distances and angles of position of double stars; and has shown, from the comparison of his own with other observations, that many of them form systems whose variations of position are subject to invariable laws. He has succeeded, by a happy combination of graphical construction with numerical calculations, in determining the relative elements of the orbits which some of them describe round each other, and in forming tables of their motions; and he has thus demonstrated that the laws of gravitation, which are exhibited as it were in miniature in our own planetary system, prevail also in the most distant regions of space: a memorable conclusion, justly entitled, by the generality of its character, to be considered as forming an epoch in the history of astronomy, and presenting one of the most magnificent examples of the simplicity and universality of those fundamental laws of nature by which their Great Author has shown that He is the same to-day and for ever, here and everywhere.

A discovery like this, which we are this day called upon to commemorate, forms a noble, but I trust only temporary termination to Sir John Herschel's European labours. He has long contemplated a voyage to the Cape of Good Hope, as a favourable station for observing the constellations of the Southern Hemisphere, and the magnificent nebulæ which it contains; and when we consider the space-penetrating power of his instruments, such as has never yet been brought to bear upon them; his skill and long experience and systematic diligence as an observer; his perfect familiarity with the class of phenomena which are to be observed; his sagacity in interpreting and disentangling the most complicated appearances; and his profound knowledge of physical as well as practical astronomy, we may look forward to a harvest of discoveries, such as will not only extend the existing boundaries of science, but add to the lustre of a name which is known and reverenced in every region to which European civilization has reached.

It has been said that distance of place confers the same privileges as distance of time, and I should gladly avail myself of the privilege which is thus afforded me by Sir John Herschel's separation from his country and friends, to express my admiration of his character, in stronger terms than I should otherwise venture to use; for the language of panegyric, however sincerely it may flow from the heart, might be mistaken for that of flattery, if it could not thus claim somewhat of an historical character: but his great attainments in almost every department of human knowledge, his fine powers as a philosophical writer, his great services and his distinguished devotion to science, the high principles which have regulated his conduct in every relation of life, and, above all, his engaging modesty, which is the crown of all his other virtues, presenting such a model of an accomplished philospher, as can rarely be found beyond the regions of fiction, demand abler pens than mine to describe them in adequate terms, however much inclined I might feel to undertake the task. That he may live to accomplish all the objects which have induced him to transport himself to another continent, and that he may long survive his return to witness the respect, reverence and gratitude of his countrymen, is my earnest prayer, in which I am quite sure that you, Gentlemen, will cordially join.

It now becomes my painful duty to call your attention to the names of those Fellows and Foreign Members whom the Royal Society has lost during the last year.

Sir John Malcolm was born in the year 1769, a year remarkably fertile in the production of great men *. He was one of a family of seventeen children, which enjoyed the singular distinction of having three of its members created Knights of the Bath in the same year. At the early age of thirteen he was sent to India as a Cadet, and learnt his first lessons of military service in the celebrated wars of the Mysore; and during an almost uninterrupted residence of nearly forty years, he was employed both in civil and military duties, frequently of great importance and difficulty, in almost every part of Central India; and it was chiefly owing to the opportunities afforded by this long intercourse with the natives of all classes and nations, aided by the system of carefully recording his observations of their manners and customs, and by his perfect knowledge of their languages, that he was enabled to acquire the most intimate acquaintance with their habits, their feelings and their prejudices, at the same time that he secured, in a very uncommon degree, their confidence and respect by his strict impartiality, and by his considerate attention to their wants and their interests.

He was twice sent as Ambassador to Persia, where he conducted negotiations of great delicacy and difficulty in such a manner as to maintain the honour, at the same time that he secured the interests of the Government which he represented: he was, in fact, eminently qualified for the discharge of such a duty by his profound knowledge of the Persian language and literature, and by the conformity of his own manners with those of that lively and polished nation. Nor were the fruits of his mission political merely, inasmuch as they led to the production of his *History of Persia*, a work of great research and of standard value; to his *Persian Sketches*, so remarkable for their wit and vivacity, and, I believe, likewise for the truth of the pictures of manners which they furnish; and also to a volume of Poems, which display no inconsiderable powers of versification.

Sir John Malcolm was a voluminous writer, and amongst other works may be particularly mentioned his *Political History of the Government of India, from the year* 1784 *to the Present Time*; his very interesting *Sketch of the Sihks*, and his *History of Central India*. In all his writings he has shown himself to be the friend of the native population, and the zealous advocate of a system of government such as would reconcile the interests of the governed with those of the governors: and though he has very clearly demonstrated that our Indian Empire must be progressive in order to be permanent, and that external attacks upon it must not only be repelled, but the means of renewing them either greatly weakened or altogether removed, yet he stigmatizes with just reprobation the commencement or continuance of wars of conquest merely, which are not rendered necessary by previous and adequate provocation. Upon all such subjects Sir John Malcolm was eminently entitled to pronounce an authoritative opinion, from his great experience, both military and civil, and from his almost unequalled knowledge of the

* Napoleon, Wellington, Cuvier, &c.

political interests and relations of all the various nations who compose or border upon our Indian Empire.

Sir John Malcolm returned to England in 1822: in 1827 he was appointed Governor of Bombay and Central India. He retained this important situation for three years, when he was recalled for the purpose of taking part in the discussions which were likely to arise upon the renewal of the East India Company's Charter. He was shortly after his return elected Member of Parliament for Launceston; but the questions which almost entirely absorbed the attention of Parliament and of the public at that period were not calculated for the favourable display of his peculiar powers. His last public address was made at a meeting in London in honour of his illustrious countryman Sir Walter Scott, of whose genius and writings he was an enthusiastic admirer: on the following day he was attacked by paralysis, from which he never recovered; and he died at his house in London on the 31st day of May last.

Sir John Malcolm was tall and commanding in his person; his manners were remarkably free and unconstrained, and his conversation rapid and animated; and notwithstanding his long and intimate intercourse and association with Oriental people and Oriental languages and with scenes of life altogether different from those in which his earlier boyhood had been passed, yet he continued to speak with the accent of his countrymen, and to remember their national traditions with all the vividness and to recite their national poetry with all the enthusiasm, which characterize our earliest and deepest impressions. As a father, a husband and a brother he was eminently kind and affectionate; and few persons have been more generally beloved by their friends for their social virtues, or more respected and revered for their great talents and attainments and for their eminent public services.

I observe with pleasure that a monument, from the chisel of Mr. Chantrey, is to be erected to his memory in Westminster Abbey, for which ample funds have been provided by the almost spontaneous contributions of his friends; and it is worthy of remark that amongst the subscribers is to be found the name of an Eastern Potentate, the Pacha of Egypt, the founder of a great empire, and still more distinguished for his triumphs over Eastern prejudices, who became acquainted with Sir John Malcolm upon his return from Bombay, and who has most gladly availed himself of this opportunity of expressing his respect for the memory of his friend.

Mr. William Morgan was the author of several papers in our Transactions, chiefly upon the subject of the value of reversions contingent upon different cases of survivorship. For two of these papers, printed in 1788 and 1789, he received the Copley Medal. He was one of the first authors who rejected altogether the hypothesis of the equal decrements of life which had been introduced by De Moivre, partly from the want of correct tables, and partly for the purpose of simplifying the formulæ employed in the calculation of contingent reversions; and he showed in what manner such questions could be practically solved with reference to the real probabilities of life. Mr. Morgan was the nephew of the celebrated Dr. Price, whose memoirs he has written, and some of whose works he has edited; and he partook largely, at one period at least, of some of the political and financial opinions of that ardent character, particularly relating to the dangers of a national bankruptcy from the rapid increase of our National Debt. He was appointed early in life, chiefly by his uncle's influence and recommendation, to the situation of Actuary of the Equitable Assurance Company, which he continued to hold for nearly sixty years; and the unexampled wealth and prosperity of that great establishment may be in a great degree attributed to the confidence inspired by the correct principles of calculation and of management which he introduced: and though he was exposed towards the close of life to many attacks and much opposition, in consequence of his too rigid adherence to a system which might be calculated to do injustice to some classes of insurers, yet no small indulgence is due even to the prejudices of a man who had done so much service to society, by establishing upon a firm basis the security of establishments which act as safeguards against the fluctuations and vicissitudes of life, and which thus encourage habits of providence and of foresight amongst the higher and middle classes of the community.

Mr. Thomas Allan, an eminent citizen of Edinburgh, was the author of a work on Mineralogical Nomenclature, and of several papers on geology and mineralogy in the Transactions of the Royal Society of Edinburgh, and elsewhere. He was greatly distinguished for his accurate knowledge of mineral species and their varieties, and of all the delicate and minute distinctions of external characters by which they are separated from each other; and his collection of minerals has been justly celebrated for its great extent and perfect arrangement. In the year 1812 he joined Sir George Steuart Mackenzie in an Excursion to the Faroe Islands, where he greatly enriched his collection, particularly in zeolites. This expedition was undertaken for the purpose of ascertaining whether, in a Trap Country, where no traces of *external* volcanoes existed, anything similar to the peculiar features of the rocks of Iceland was to be found; and his Account of the Mineralogy of these Islands, in which his object has been to describe, without relation to theory, whatever appeared to him interesting in a geological point of view, was read before the Royal Society of Edinburgh in the beginning of the following year, and printed in the seventh volume of their Transactions. He adopted in early life the opinions of Dr. Hutton, though his papers on some points in geology in the neighbourhood of Edinburgh, and in the environs of Nice, show him to have been an accurate and an unprejudiced observer. He was a person of active habits and character, a liberal supporter of public charities and useful institutions, and an ardent and even enthusiastic friend of all the schemes for the improvement and decoration of his own magnificent and picturesque metropolis.

Dr. William Babington was a distinguished physician in the City of London. He was formerly a lecturer on materia medica and on chemistry at Guy's Hospital, and he was the author of a Systematic Arrangement of Minerals, founded upon a joint consideration of their chemical, physical and external characters; and also of other works, of less importance, upon mineralogical arrangement. He was the active and disinterested friend of science and of men of science, from the time of Priestley to that of Sir Humphry Davy; and though the absorbing duties of a laborious profession prevented his taking a leading part in original inquiries, he was well acquainted with the existing state of knowledge, particularly in geology, physiology and chemistry. He was one of the first founders of the Geological Society; and the earliest meetings of that distinguished body, which has contributed so powerfully to the advancement of geological knowledge, were held at his house. He was a person of great simplicity of manners, a warm and active friend, zealous in the promotion of objects of charity and usefulness, and in the practice of his profession singularly kind to the poor.

The death of Lord Dover in the course of this year excited an unusual degree of public sympathy and sorrow, from his youth and high birth, his domestic virtues, and perhaps also his domestic happiness, his unsullied public character, his cultivated taste for the arts, and his liberal and enlightened patronage of artists, and most of all from the promise of the highest literary distinction afforded by his very interesting historical memoirs and other literary productions. Such qualities and attainments, whilst they give dignity to all who possess them, acquire a peculiar grace and lustre when found in those classes of society in which the possession of rank and wealth separate altogether the pursuit of knowledge and of fame from all taint of a suspected union with the desire of mere personal aggrandizement.

The Rev. Bewick Bridge, Fellow of St. Peter's College, Cambridge, obtained the highest mathematical honours in his own academical year. He was for many years Mathematical Professor in the East India College at Haileybury, and was the author of several elementary works on different parts of mathematics, which are remarkable for their judicious adaptation to the capacities of ordinary students, by the union of simplicity and fulness in the developement of first principles,—a species of merit which those only can duly estimate whose experience in education has shown it to be very rarely attained. Mr. Bridge was a person of great benevolence, who devoted his life and fortune to the promotion of objects of charity and public utility, and whose purity of character and kindness of heart secured him the affectionate attachment of a large circle of friends.

Captain Lyon became first known to the public from his having accompanied the late Mr. Ritchie in his journey into the interior of Africa. His companion died at Moorzouk, and after encountering the ordinary succession of sufferings and dangers which characterize the melancholy records of African discovery, he succeeded in effecting his return, and published a very modest and interesting journal of his travels. He afterwards accompanied Captain Parry in the second voyage to the Arctic Regions, as commander of one of the two ships which composed that expedition. After his return he was chosen, from a knowledge of his enterprising and energetic character, to conduct a party of English miners to Zacatecas and Bolaños in Mexico, and to undertake the management of the first of these mining establishments: and though he continued there for a short time only, being compelled by domestic circumstances to return to England, his services were of such a kind as to produce the most important results. His Mexican adventures form a narrative full of interesting, amusing and instructive details. He was afterwards chosen by the Brazilian Company to superintend the celebrated gold mines at Gongo Soco, in the province of Minas Geraes, which under his management became so productive, as fully to vindicate and redeem the character of South American mining speculations. Upon quitting their service he engaged in mining adventures of his own; and it was in returning to England, in consequence of an accidental injury which he received in the course of his operations, that he died at sea, in the thirty-seventh year of his age.

Mr. Joshua Brookes was for more than forty years a distinguished teacher of anatomy, and it is said that during the course of his life he had superintended the anatomical education of more than seven thousand pupils. He had formed a Museum of human and comparative anatomy, which was second only in extent and value to the Hunterian Collection, and to which he gave the most ready and liberal access both to his pupils and to the public. To the completion of this museum, and to the instruction of his pupils, he devoted the whole of his time and of his income; and it was a melancholy circumstance that he should have been compelled towards the close of his life, when his health, and with it his sources of income were declining, from the pressure of pecuniary difficulties, to consent to the sale of his museum. The dispersion of this collection was to him a source of the most poignant distress; and the latter years of a long life which had been devoted with singular disinterestedness to the public service, were imbittered at once by the pressure of poverty and the despondency occasioned by the annihilation of those hopes of having raised a lasting monument to his fame, which had formed the great object of his ambition.

Lieutenant-Colonel John Baillie went to India as a Cadet in 1791, and from the commencement of his residence he devoted himself with great diligence to the study of the Oriental languages. Upon the establishment of the College of Fort William, in 1800, he was appointed Professor of the Arabic and Persian languages, and of the Muhammedan law, a situation which he continued to fill with great credit and distinction for several years. He was the author of *Tables elucidatory of a Course of Lectures on Arabic Grammar*, of *A Collection of the original Texts of the five most celebrated Grammars of the Arabic Language*, and of *A Translation from the Arabic of a Digest of the Muhammedan Law*, of which one volume only out of four was published. His Oriental studies appear to have terminated upon his appointment as Resident at Lucnow, where he continued for several years. He quitted India in 1818, and in 1823, he was appointed a Director of the East India Company. Colonel Baillie was one of the founders and most active supporters of the Royal Asiatic Society; and he represented his native town, Inverness, and its contributory burghs, in two successive Parliaments. His collection of Persian, Arabic, and other Oriental Manuscripts is said to have been one of the most extensive and valuable that was ever brought to this country.

Mr. Joseph Whidbey was for nearly fifty years a Master in the Navy, and had been one of the companions of Vancouver in his voyage round the world. He was a person of great practical knowledge and skill, and possessed of more than ordinary general attainments; and he was in consequence selected by the Government to superintend, under the direction of the late Mr. Rennie, the execution of that great national work, the Breakwater at Plymouth. He was the author of three papers in our Transactions: one on the means adopted for raising the Dutch frigate Ambuscade, which had been sunk at the Nore; and the other two on certain fossil bones discovered in the limestone quarries at Oreston, near Plymouth.

Adrien Marie LeGendre, one of our Foreign Members, and one of the most illustrious analysts in Europe, was born in Paris in 1752, and died on the 10th of January last, in the eighty-first year of his age. After the completion of his studies at the Collège Mazarin, he devoted himself to mathematical and scientific pursuits, which he continued, with singular perseverance and industry, for the remainder of his life. At the age of thirty he gained the two prizes proposed by the Academies of Berlin and Paris; the one for a memoir on the motion of projectiles in a resisting medium, and the other for a memoir on the attraction of spheroids upon any external point whatever. It was this second memoir which gained him, in the following year, a place in the Academy, as the successor of D'Alembert, and which attracted in a peculiar degree the attention of mathematicians. The problem which it treated was one of the greatest importance and difficulty, particular cases only of which had been successfully treated by Newton, MacLaurin and Clairaut, but which he attacked in all its generality, and mastered its difficulties "sword in hand," to use the expressive language of Lagrange, when speaking of this admirable memoir. An important proposition discovered by Laplace led to a second, and a happy substitution, proposed and applied by Mr. Ivory, to a third resumption of this problem, which has finally terminated in such an organized system of approaching its difficulties, that it has lately been reduced to the order of those propositions which are included in the higher class of elementary books *.

It was in the course of his researches upon the attraction of spheroids that his attention was first drawn to the subject of elliptic integrals, concerning which his first memoir was published in 1786. He continued to pursue this most interesting and difficult branch of analysis in a succession of works, for a period of nearly forty years,

* Poisson, (*Traité de Mécanique*, second edition,) who has obtained an expression for the attraction under a finite form.

and had finally collected his entire labours upon it in two volumes quarto, which he published in 1827, forming a vast treasure of analytical knowledge. He had hitherto laboured in this field without a colleague and without a rival, when two young analysts of singular genius and boldness, M. Abel, of Christiania in Norway, and M. Jacobi, of Königsberg, announced, almost simultaneously, the discovery of propositions which have led to an immense extension of this theory. LeGendre, with a nobleness of character which can only result from the most disinterested love of truth, was the first to welcome the appearance of these illustrious strangers upon his own territories, to make known the full importance of their discoveries, and to develope all their consequences; and although already arrived at an extreme old age, he commenced and finished, with all the vigour and activity of youth, a third volume, expressly devoted to the discussion and classification of these *ultra-elliptic* functions, and to point out their analogy with, and relation to other classes of transcendents which he had himself already considered, or to which they would naturally lead.

M. LeGendre was the author of a justly celebrated treatise or essay on the Theory of Numbers, which first reduced the numerous and disconnected discoveries of Fermat, Euler and Lagrange to systematic order. He was the proper author, amongst many other discoveries, of the *law of reciprocity* between any two prime numbers, one of the most fertile and important in this theory, though its complete establishment was reserved for Gauss, whose work on this subject has gained him so just a reputation. Notwithstanding, however, the labours of these great men, this most important department of analysis still continues to be too much insulated, both in its form and its treatment, from the other branches of algebra, though much has been done to reunite them by the very valuable and original researches of that distinguished analyst M. Libri, of Florence, who has been recently naturalized in France, and who has succeeded M. LeGendre in his place in the Institute.

The work of M. LeGendre, on Geometry, has enjoyed a singular reputation, and has been most extensively used, particularly on the continent of Europe, in the business of education. It may be doubted, however, whether this work has altogether merited the high character which it has obtained: it has rather increased than cleared away the difficulties of the theory of parallels, which have so long embarrassed the admirers of ancient geometry and of the Elements of Euclid; and it has not succeeded, at least in any essential degree, in adding to the simplicity of the demonstrations, or to the clear and logical connexion and succession of the propositions of that unrivalled and unique elementary work, which has alone maintained its place amongst all civilized nations for more than two thousand years. It is proper, however, to observe that the notes appended to this work are full of valuable and original remarks, and are justly celebrated for the elegance of the demonstrations which they furnish of many important propositions.

M. LeGendre was the author of many other works and memoirs, containing many valuable series of investigations, and very important discoveries. He first attacked the great problem of the determination of the orbits of comets by general methods, which display all the resources of his analysis; though astronomers have not found it expedient to make use of his methods in the actual calculation of their elements, which is the only proper test of their practical value, though it may not be decisive of their theoretical perfection. He was the author of the method of *the least squares of the errors,* for the purpose of determining the most probable mean amongst the results of a great number of observations, of which such extensive use is now made in practical astronomy: a celebrated and most useful theorem in geodesy goes by his name; and there are few departments of analysis or of dynamics which have not been benefited by his labours.

M. LeGendre was associated with Méchain and Cassini in the operations which were instituted in 1787, and finished in 1790, for the junction of the meridians of Paris and London. He was one of the three Members of the Council nominated for the purpose of introducing the new metrical system into France in 1795, and he constructed the formulæ employed for the calculation of the tables for the centesimal division of the quadrant. He was nominated, both during the Imperial and subsequent Government, to various public employments, chiefly, however, of an honorary nature, requiring no great sacrifice of time or attention,—a fortunate circumstance, when it is considered to what important labours the leisure of his long life appears to have been devoted.

The next name which I feel called upon to notice is that of Francisco de Borja Garção Stockler, Baron da Villa da Praia, a Lieutenant-General in the Portuguese army, and formerly Secretary of the Academy of Sciences of Lisbon: he was the author of several Papers in the Transactions of the Lisbon Academy, chiefly on subjects connected with the developement of functions, and also of a volume of Poems. In 1795 he published his *Methodo dos Limites,* and in 1824 his *Methodo inverso dos Limites*. In this latter work, written late in life, he adopted the opinions of the well-known Hoene de Wronski, which led to its rejection by the Academy of Lisbon, upon the report of two Academicians, when it was offered to them for publication. His works are not of a kind to exercise much influence upon the progress of science, and some of them are examples of the danger of dealing with formulæ of such great generality that their proper import and derivation are not very clearly understood by those who use them.

Of the five Foreign Members whose names appear in the lists of the additions which the Royal Society has received during the last year, it is with deep regret that I observe those of two of them also in the record of its losses: the first is that of Professor Meckel of Halle, the second that of M. Desfontaines of Paris.

Dr. John Frederick Meckel, Professor of Anatomy in the University of Halle, was the third member of a family singularly illustrious in the annals of physiological and anatomical science. His grandfather, at the beginning of the last century, was probably the greatest anatomist of his age, and was the founder of that collection which has become, by the additions of his son and of his grandson, the richest and the best arranged in Germany. His father was likewise an eminent anatomist, and greatly distinguished for his success in the practice of physic and of surgery, and for his general attainments. It was for the purpose of enriching the great collection which he inherited, and of completing those departments of it in which it was deficient, that young Meckel first directed his whole attention to comparative anatomy; but the results of his labours were not confined to his museum: he published a German translation of the Anatomie Comparée of Cuvier, which was enriched with many valuable notes. This was followed by his Contributions to Comparative Anatomy; by his *System der vergleichenden Anatomie,* which he did not live to complete; his *Tabulæ Anatomico-pathologicæ*; his *Handbuch der pathologischen Anatomie*; his work On Human Monsters, and several memoirs relating to this branch of medical science, which display a remarkable union of laborious research with the most profound and original views relating to the phenomena of animal life. He devoted a great portion of his time to the publication of the *Archiv für Anatomie und Physiologie,* one of the most valuable and instructive periodical publications on medical and physiological science which appeared in Germany. One of his last works, on the Lymphatic System, which is upon a magnificent scale, was dedicated to the celebrated Sömmerring, upon the completion of his fiftieth year from the period of his inauguration as Doctor in Medicine, as a tribute of respect to one who had been his own preceptor, the fellow-student of his father, the follower and pupil of his grandfather, the intimate friend of his family for three generations, and who was also one of the few of his living rivals in the sciences which he cultivated.

Meckel was only fifty years old at the time of his death: he united in a very remarkable degree the power of correct and philosophical generalization with the most profound and accurate knowledge of anatomical details; and though he may have left in his own country some who may equal or even surpass him in particular departments of human and comparative anatomy or physiology, there is no one of his countrymen, if, perhaps, Tiedemann be excepted, who can be considered as having made such important additions to our general views in those sciences.

René Louiche Desfontaines, Professor of Botany at the Jardin du Roi, and one of the most distinguished botanists in Europe, was born at Tremblay in 1752. In the course of the years 1782 and 1783 he travelled, for the purpose of forming botanical collections, to the North of Africa, penetrating as far as the range of Mount Atlas; and his *Flora Atlantica,* which was published in 1798, a splendid and richly decorated work, contains the principal results of his labours. It was in the same year that his celebrated memoir on the Organization of Monocotyledonous Plants was read to the Institute, in which he demonstrated the different manners in which the ligneous fibres are developed in plants with simple and double cotyledons, and thus laid the foundation of two great and fundamental divisions in the vegetable kingdom *. He was the author of the *Tableau de l'École de Botanique du Muséum d'Histoire Naturelle,* of the *Histoire des Arbres et Arbrisseaux qui peuvent être cultivés en pleine terre sur le Sol de la France,* of a *Manuel de Cristallographie,* according to the system of Romé de l'Isle, of many elaborate articles in the Dictionnaire des Sciences Naturelles, and other similar publications; and of a great number of Memoirs, chiefly in the Annales du Muséum d'Histoire Naturelle, which were for the most part descriptive of new genera and species of plants cultivated in the Jardin du Roi, the management of which had devolved upon him conjointly with MM. de Jussieu and Thouin.

M. Desfontaines was a person of mild and inoffensive manners, and perfectly free from those feelings of jealousy which tend to provoke either opposition or controversy. For a considerable period before his death he laboured under the affliction of total blindness, and was thus debarred from the continuation of those pursuits which had constituted at once the delight and the business of his life: and it was a fortunate circumstance that a visitation of Providence, which under ordinary circumstances would have produced a spirit of repining and discontent, was deprived of more than half its bitterness and severity by the spirit of contentment and resignation with which it was met.

At the conclusion of my Address to you, Gentlemen, last year, I felt called upon, at once by my subject and my feelings, to pass from the notice of the *certain* losses which the Society had sustained during the preceding year, to one which circumstances at that time rendered too probable. The long absence of Captain Ross and his companions, the perilous enterprise upon which they were engaged, the fearful alternative of shipwreck or famine which seemed their almost inevitable fate, had left few elements for hope, except in those who steadily trust in the unlimited resources of Providence to accomplish its ends, however remote and wonderful. I rejoice at the unlooked-for accomplishment of that hope, and I know that you, Gentlemen, one and all, will equally participate with me in these feelings. Captain Ross and his brave companions were "lost, and are found;" and I trust that the enthusiastic welcome which has met them upon their return will convince them that the heart of their country is that of a parent.

I forbear, Gentlemen, to mix up other topics with the expression of those feelings to which this happy event naturally gives rise, and however important may be the contributions to geography or to science which these perilous and painful adventures may have produced, I consider them, in the present condition of my feelings, but as dust in the balance, when compared with the knowledge of the important fact of the recovery of our long lost brethren.

Permit me then, Gentlemen, in your name as well as in my own, to offer to Captain Ross, whom I rejoice to see amongst us, our most

* Traces of this distinction in the structure of Monocotyledonous and Dicotyledonous plants may be found in the writings of Grew, Malpighi, and Daubenton, though its full developement was reserved for M. Desfontaines.

cordial congratulations upon his happy return, and to express our hope that the sympathy and respect of his countrymen which he has already experienced, and which, I trust, he will retain for the remainder of his days, will form one of the best compensations for the long sufferings which he has endured, and for the incomplete success of an enterprise presenting difficulties from the certain operation of the laws of the physical world, which not merely baffle, but almost annihilate, the powers of the bravest, the strongest, and the most persevering of men.

The Secretary reported, on the part of the Council, that they had received an application from the Lords of the Treasury, for their opinion respecting the construction and mode of applying an instrument for ascertaining and charging the duty on spirits: in compliance with which they appointed a Committee to conduct the investigations required for that purpose. The Committee, after bestowing considerable labour and pains on this subject, agreed in a Report, which was adopted by the Council, and transmitted to the Lords of the Treasury, and for which they have received their thanks, for the labour and attention they have given to this subject. They have lately also received an intimation from the Treasury, that their further assistance will be requested to superintend, examine, and assist in the construction of the instruments and tables which will be required.

The Treasurer then reported the Number of Fellows, the State of the Finances, and the Receipts and Payments of the Society during the preceding year; all which are given at large in the printed "Proceedings of the Society," No. 14.

The Society next proceeded to the election of the Council and Officers for the ensuing year, when the following was declared to be the list:—

President: His Royal Highness the Duke of Sussex, K.G.—*Treasurer:* John William Lubbock, Esq., M.A.—*Secretaries:* Peter Mark Roget, M.D.; John George Children, Esq.—*Foreign Secretary:* Charles Konig, Esq.

Other Members of the Council: Francis Baily, Esq.; Peter Barlow, Esq.; William Thomas Brande, Esq.; Benjamin Collins Brodie, Esq.; Mark Isambard Brunel, Esq.; William Clift, Esq.; Rev. James Cumming; Michael Faraday, Esq.; Davies Gilbert, Esq.; George Bellas Greenough, Esq.; Rev. Philip Jennings, D.D.; Rev. George Peacock; William Hasledine Pepys, Esq.; Rev. Baden Powell; Rev. Adam Sedgwick; Captain William Henry Smyth, R.N.

GEOLOGICAL SOCIETY.

Dec. 4.—A letter from Hugh E. Strickland, Esq., addressed to George Bellas Greenough, Esq., was first read.

This letter was accompanied by a manuscript map, on which were laid down, with greater accuracy than had before been attempted, the boundaries of the red marl and lias in the districts adjacent to Pershore, Evesham, Bitford, Alcester, Droitwich and Worcester.

Mr. Strickland points out also, for the first time, a line of fault ranging from a little north of Bredon Hill in Gloucestershire, to Inkberrow, north of the road from Alcester to Worcester. By this fault the relative position of the red marl and the lias has been affected, the former constituting a valley of elevation, bounded on each side by the latter. Mention is also made of bones and teeth of the Hippopotamus and of a Deer having been found in the gravel near Cropthorne, between Evesham and Pershore. Mr. Strickland likewise alludes to the occurrence, on Shotover Hill near Oxford, of fossils which he believes to belong to the fresh-water genus Paludina; but the specimens which he procured are imperfect casts. These shells, he adds, were first discovered by the Rev. H. Jelly of Bath, in a sand-pit on the brow of the hill, much higher than the pit at which the Portland strata occur.

A paper on the Strata of Quainton and Brill in Buckinghamshire, by James Mitchell, LL.D., F.G.S., was then read.

In this communication the author confines his observations almost solely to an enumeration of the beds belonging to the Portland stone presented at the two localities of Quainton and Brill.

The principal quarries at the former place are composed of the following strata:

	Feet.
Top Vegetable mould	3
Clay	—?
Iron sand (lower green sand) containing a layer of Fuller's earth	6 to 8
Hard sandstone	1 to 2
Clay	2
Soft, calcareous sandstone (Pendle stone) abounding in fossils	2
Building stone,—numerous fossils	2
Soft, white limestone	2½
Sand	6
Rubble stone,—abundance of fossils	3
Sand	6
Coarse, soft, blue stone	1½

Besides the fossils common to the Portland stone, the author procured caudal vertebræ of a Plesiosaurus and a Crocodile.

The strata at Brill are then enumerated in the following order:

	Feet.
Vegetable mould	4
White, soft limestone, with fossils	7
Sand	3
Rubble, with fossils	4
Sand and clay, with nodules of blue stone in the lower part	8
Coarse, white sand	2
Blue clay	2

In the lower part of this quarry is an abundance of green sand.

The upper beds of stone in the Quainton quarries, the author adds, are wanting in those at Brill; and the lower beds at each locality are stated to be nearly the same, though not agreeing precisely in all their details.

A paper was next read, entitled "Observations on the Cliff at Reculver in Kent," by James Mitchell, LL.D., F.G.S.

The object of this paper is twofold: 1st, It describes the geological structure of the cliff; and 2nd, It gives a chronological account of the changes which have taken place on this part of the coast of Kent since the period of the Roman dominion.

The cliff, described in the memoir, is situated between Reculver and Herne Bay, and is about two miles in extent. The upper part, where the beds are fully displayed, consists of about 35 feet of mottled, brown and red clay; and the lower part of about 50 feet of sand, containing a layer of masses of sandstone. Fossils are stated to be found only in the sand, and to belong chiefly to a species of Venus. Sections are given of different parts of the cliff, and it is shown that the strata dip gradually towards the west, the sandy portion of the series sinking beneath the level of the shore, and being replaced entirely by the clay.

In tracing the history of the change on the line of the coast, the author first draws attention to the present hydrography of the bed of the Thames, and gives his reasons for concluding that many of the sand banks now dry at low water, were formerly islands; and in additional support of this opinion, mentions the large island which is laid down in Ptolemy's map in the position of the present Margate sands.

The author then states that historical documents, and inscriptions on altars, prove that Reculver, or Regulbium, was at the period of the Roman dominion a military station and a sea-port, and that the Isle of Thanet was at that æra separated from the rest of Kent by a navigable channel; that at the period of the Norman Conquest the district of Reculver was one of the hundreds of Kent, though it now forms only an obscure portion of the hundred of Bleangate; that in the reign of Henry VII. the channel between the Isle of Thanet and Reculver was so far filled up as to permit a bridge to be built,—but according to Leland, in the beginning of the reign of Henry VIII., Reculver was then half a mile from the sea, or, in proportion to other distances mentioned by him, about one mile; that in the year 1780, the wall of the Roman castrum, distant 80 yards from the church, had been only lately taken down; and lastly, that about the beginning of the present century, the church itself was abandoned as a place of worship, and would in all probability have long since disappeared, but for the precaution taken by the Trinity House to defend the cliff from further destruction.

Dec. 18.—A paper, entitled "Notes on the Geology of the Brown Clee Hill in Shropshire," by Rumley Wright, Esq., employed in the Ordnance Survey, was first read.

The base of the Brown Clee Hill is stated to consist of old red sandstone, and the upper part of coal measures surmounted by basalt. The top stratum of the sandstone is a conglomerate, and the same formation contains two beds of nodular limestone or cornstone, the lower of which is about 12 feet thick. The strata are said to dip regularly towards the centre of the hill at an angle of about 7°.

The coal-field is represented to have the form of the figure 8. The strata are said to be about 150 feet thick, and to dip towards a common centre at an angle of from 3 to 5 degrees. Three beds of coal have been discovered, varying from 1 foot 7 inches to 2 feet 6 inches in thickness, but the coal is of inferior quality to that of the Titterstone Clee Hill.

Three faults are described, and stated to range nearly parallel to each other, and to traverse the coal measures in a north easterly direction. One of them, the author observes, is marked by a dyke of basalt connected with an overlying mass of the same rock. It is 13 yards in horizontal thickness, and though the wall of the dyke is so hard as to require to be blasted, yet the coal is not in the least charred.

The overlying basalt is shown to form the two highest points of the hill, one of them being 1800 feet above the level of the sea, and the other 1600.

LINNÆAN SOCIETY.

Jan 21, 1834.—Read the description of a new species of the genus *Chamæleon*, by Mr. Samuel Stutchbury, A.L.S., Curator of the Bristol Institution.

This new species of *Chamæleon*, to which Mr. Stutchbury has given the name of *cristatus*, from its peculiar dorsal crest, supported by the spinous processes of the vertebræ,—by which character the animal approaches the Basilisks,—is from the banks of the river Gaboon, in the western region of equinoctial Africa, and was presented, along with specimens of other reptiles from the same country, to the museum of the Bristol Institution, by Messrs. King and Son of that city. The colour of the body is ash-grey, with a dark patch upon the anterior and superior part, giving off inferiorly two or three bands : the posterior part marked with orange and dark-coloured reticulate lines : the edge of the dorsal crest and tail spotted with the same dark colour. Mr. Stutchbury gives the following differential character of the species.

C. cristatus, superciliari occipitalique carinâ elevatâ et crenulatâ, caudæ anteriori parte dorsique apophysibus elongatis formâ cristæ dorsalis, squamis ferê rotundis subæqualibus.

Read also part of a paper by Mr. Robert H. Schomburgh, entitled “ A Description of some Trees, remarkable for their size or age, in all parts of the world, but with particular respect to a Silk-cotton Tree (*Bombax pentaphyllum*) near the town of the Island of St. Thomas in the West Indies.”

A fine specimen of the Squacco Heron (*Ardea comata*), shot lately in Hampshire, was exhibited at the meeting, as well as specimens of a number of rare birds from Senegal, Mexico and Chili. A copy of a large work on the plants of the country around Rio de Janeiro, entitled “Flora Fluminensis,” was presented for the Society's Library by General Oliveira, F.L.S.

MARYLEBONE LITERARY AND SCIENTIFIC INSTITUTION.

This Institution is rapidly advancing to a state of permanent prosperity, and may confidently be expected to become one of the most important and interesting establishments of the kind in the metropolis. The Committee have taken spacious and commanding premises, situate No. 17, Edwards-street, Portman-square, where the business of the Institution is now conducted. A lecture room is to be built in the rear of the premises capable of containing at least 600 persons. Until then the handsome and spacious suite of rooms on the first floor, which have been thrown into one for the purpose, will be occupied for the delivery of lectures.

The audiences are numerous and highly respectable, and the lectures delivered during the last six months have been eminently calculated to command them. Among those who have given instruction in different branches of science or literature at this Institution are Dr. Lardner, Dr. Ritchie, Dr. Copland, Dr. Southwood Smith, Sir A. Carlisle, Professor Burnett, Messrs. John Taylor, T. Phillipps, Wallis, Brayley, Jun., and Hemming, the latter of whom is President of the Institution.

The Committee are now forming classes for the instruction of members in chemistry, languages and music. The following list of the officers of the Marylebone Literary and Scientific Institution will sufficiently prove the estimation in which it is held, while it is a guarantee for the continuance of its prosperity.

President: John Hemming, Esq.—*Vice-Presidents:* E. W. Brayley, Esq., F.S.A.; Sir Anthony Carlisle, F.R.S.; James Copland, M.D., F.R.S.; Raikes Currie, Esq.; T. H. Holdsworth, Esq., F.G.S.; Rev. Dr. Lardner, F.R.S., &c.; Robert M'William, Esq.; Basil Montague, Esq.; Richard Taylor, Esq., F.L.S.; G. Ainslie Young, Esq.—*Treasurer:* Thomas Bridgman, Esq.

XXVIII. *Intelligence and Miscellaneous Articles.*

TEST FOR HYDROCYANIC OR PRUSSIC ACID, AND METHOD OF APPRECIATING THE QUANTITY.

WE are informed by Mr. John T. Barry that the nitrate of silver, in common with other salts of that metal, is so extremely delicate a test of the presence of hydrocyanic acid, that its detection is not difficult in a drop of water containing far less than *the ten thousandth part of a grain* of that poisonous agent. For instance, if one minim of the dilute medicinal solution be mixed with a pint of water, its presence may be demonstrated in a single drop of the mixture. But what is of more consequence is, that although the mixture be contaminated with various organic substances, such as those contained in articles of diet, milk, coffee, tea, porter, wine and soups, so far as is yet known the test retains its sensibility unimpaired. Mr. Barry, however, thinks that this extreme sensibility, while it renders the evidence of the silver test conclusive as to the *absence* of prussic acid, will be of more limited service in establishing its *presence*, for, without adverting to the possibility of other volatile substances being hereafter discovered to have a similar effect on solution of silver, it is to be borne in mind that this reagent indicates the existence of prussic acid in some esculent substances where it had previously been found, as well as in some new ones. Upon this branch of the subject medical jurists will probably think it right to collect information.

The application of the solution of silver is simple. The suspected fluid is to be acidulated by the addition of acetic acid, but so as to redden litmus paper in only the *slightest degree.* If excess of acid be already present, it is to be *not quite* neutralized by carbonate of soda. These precautions are adopted to retard the interference of ammonia or muriatic acid. Two or three drops, quite cold, are then put into a watch glass, and immediately covered by a plate of glass, whose under-surface, to the breadth of a pea, is moistened with solution of nitrate of silver, formed by dissolving one grain lunar caustic in 100 grains distilled water:—

If the inverted drop of silver solution retain its transparency unaltered, the *absence* of prussic acid is established; for had it been present, the silver solution would in a few moments have become clouded by the formation of a *white* precipitate, an effect which, indeed, is almost instantaneous when the prussic acid is not excessively diluted. If, on the other hand, the precipitate appear, the conclusion must not be drawn that it is *cyanuret* of silver, until identified as such by two properties:—first, its speedy *re-solubility*, as denoted by the clouded drop becoming again clear, when *placed over* a vessel of caustic ammonia, in which respect it differs from the silver compounds of iodine and bromine:—and secondly, in retaining *unchanged* its pure white colour after exposure a few minutes to the sun's rays, or for a longer time, to day-light. As this property essentially distinguishes it from the compound of silver with chlorine, it is important to establish it by a separate experiment upon a somewhat larger portion of the precipitate, which should be obtained, by candle-light, by successively placing the inverted drop of nitrate of silver over renewed portions of the liquid in a saucer: as soon as the precipitate separates into distinct curd-like particles, it is ready for exposure to the solar rays.

Another property which distinguishes the cyanide (or cyanuret) of silver from the chloride, is, that upon being ignited in an open short glass tube, the cyanogen burns with a flame of the usual colour, leaving the metal pure, if sufficiently heated,—a quality the more valuable as it furnishes an index to the *proportion* of prussic acid it represents, which upon ordinary occasions may be estimated as equal to one fourth the weight of residual silver.

When, acting upon this principle, it is desirable to ascertain the *entire* quantity of prussic acid, it is to be obtained by slowly distilling over, in nearly filled close vessels, about an eighth of the acidulated mixture under examination; rectifying it; re-acidulating by acetic acid; precipitating by *slight* excess of nitrate silver; washing with distilled water, only so long as the washings affect litmus paper; drying at 212°; weighing:—and lastly, igniting and re-weighing.

The medicinal solution above referred to (as to be diluted for experiment in the proportion of one drop to the pint of water,) contains, in round numbers, nearly a sixteenth of its own weight of anhydrous prussic acid, or rather less than four grains in the drachm, being the article (commonly designated "of Scheele's strength,") as manufactured by some respectable houses in London. We understand that Messrs. William Allen and Co., by means of silver as a reagent, have uniformly concentrated it to this degree since the year 1820, when Mr. Barry introduced the use of that metal to determine and regulate its proportion of absolute prussic acid by the formation of cyanuret of silver. The method being one which admits of extreme precision, will deserve the attention of the College of Physicians, if prussic acid be inserted in the next Pharmacopœia. It is to be recollected that this preparation, like those of alcohol or æther, is subject to variation, notwithstanding any superiority of formula, or care on the part of the operator. Hence the necessity of means for assaying the final product and for reducing it to a uniform standard. With regard to the employment of cyanuret of potassium for the occasional formation of hydrocyanic acid, it is a question which at least deserves very serious consideration. Its disposition to absorb atmospheric moisture, and always to become more or less converted into carbonate, while its cyanogen (united to hydrogen,) to an uncertain extent is dissipated, especially when this beautiful salt is much disintegrated, constitute formidable difficulties. But a still greater objection will present itself at the counters of apothecaries and chemists where medicines are made up, from the possibility of this intensely poisonous salt being sometimes mistaken for other substances, in the frequent extemporaneous production of prussic acid.

OXALATE OF CHROME AND POTASH AND PROTOSULPHATE OF IRON AND MAGNESIA.

M. Gregori finds that double oxalate of chrome and potash, when seen by reflected light, is black; when placed between the eye and the light it is blue, and when powdered it is green: when dissolved in water, the solution in some points of view is green, and in others red. He also finds that the double sulphate of magnesia and protosulphate of iron is of a bright green, and crystallizes in oblique octahedrons. The composition of these salts has not yet been ascertained.—*Journal de Chimie Médicale*, March 1833.

NEW ACID IN NUX VOMICA.

M. Coriol has discovered an acid in nux vomica which forms a very soluble salt with lime. It crystallizes in a granular mass in the aqueous extract of nux vomica, treated with alcohol; the acid may be separated from the lime by oxalic acid. He has also found another acid in the nux vomica, which he is about to examine.—*Ibid.*

ON CUSPARIA FROM ANGUSTURA BARK.

M. Saladin obtains *cusparia* from angustura bark by the following process: Digest seven parts of the bark in 20 parts of alcohol, of sp. gr. 0·795, without heat: let the spirit evaporate gradually at a very low temperature. After some days have elapsed, there will be observed very slight mammellated crystalline appearances, in a magma of colouring and extractive matter, &c. &c., and also deposited as acetates upon the upper part of the sides of the vessel. When carefully separated from the liquor, pressed and freed by a small quantity of water from the greater part of the foreign substances mixed with it, the crystalline form appears regular, though not very distinct.

By fresh treatment with alcohol, of sp. gr. 0·8349, and successive agitation with a little hydrate of lead freshly precipitated and æther, it separates, though with difficulty, from the fatty and colouring matters with which it is mixed; and it presents, even after some hours' exposure to a freezing mixture, acidulous crystals, united in concentric groups, the greater part of which are tetrahedrons and various modifications of this form.

These crystals dissolve in alcohol, and more rapidly when moist than after drying; the solution is bitter and slightly acrid: it does not act either as an acid or as a base. It dissolves readily in acids, and forms during their concentration, especially in acetic and muriatic acids, a white flaky deposit, which retains acid strongly even after a number of washings: it appears, however, to be merely a hydrate of cusparia.

It is insoluble in the volatile oils and æther, but dissolves in small quantity in water, according to its temperature: 1000 parts of water, at 55° Fahr., dissolve 5·45 parts; at 212°, 11·04 parts. Alcohol of 0·8356 at 60° dissolves 37-100dths of its weight.

Nitric and fluoric acid at common temperatures render cusparia yellowish green; sulphuric acid turns it reddish brown: neither iodic nor muriatic acid produces any visible alteration: the salts and the protoxides of iron and tin, the acetate and subacetate of lead do not precipitate its solutions. These characters are sufficient to distinguish it from brucia; like brucia and salicine, however, it possesses the property of being reddened by pernitrate of mercury.—This test will serve to detect the smallest admixture of salicine with sulphate of quina, even $\frac{1}{74}$.

Tincture of galls precipitates cusparia abundantly both from water and alcohol: the alkalies dissolve it partially without altering it. Chlorine, iodine and bromine in the gaseous state colour it; the first of a straw yellow, and the two last brown. In the first case, the cusparia becomes more soluble in the state of a peculiar acid: the washings contained only traces of chlorine.

When heated to 130° Fahr. and gradually higher, cusparia first melts, and then loses 23·09 per cent. of its weight. It does not appear to suffer any alteration until it is heated to about 270° Fahr.; it then burns, without leaving any appreciable residue, and without subliming or phosphorescing: its vapour at a lower degree does not indicate azote as one of its elements; and by this it is also distinguished from brucia. The only circumstances which require notice in the angustura bark are, that it contains rather a large quantity of pectine and no copper.

Cusparia is not poisonous, even in large doses; its properties appear to approach those of quina, gentianine and salicine. When the watery and acid extract of the bark is treated with animal charcoal, alcohol, &c., there may be obtained by crystallization about 13-1000dths of the weight of the bark employed.—*Journal de Chimie Médicale*, July 1833.

SEPARATION OF OSMIUM AND IRIDIUM.

M. Person gives the following as a simple and easy process for separating osmium and iridium: Mix one part of the insoluble residue of platina (osmium and iridium) with two parts of carbonate of soda, and two and a half parts of sulphur: calcine the mixture, and lixiviate the product. He obtains by this process sulphurets of osmium and iridium, which are to be mixed with three times their weight of sulphate of mercury, and the whole is to be put into a retort, furnished with an adopter and receiver, and the retort is to be made red hot. All the osmium volatilizes, and passes into the receiver in the state of a blue oxisulphuret; part of it also remains in the neck of the retort, combined with mercury and oxygen. By heating both these compounds in hydrogen, metallic osmium is obtained: the iridium remains in the retort in the state of oxide. Sometimes it retains traces of osmium which are to be separated by potash: from the pure oxide iridium is easily obtained.—*Ibid.*

ACTION OF BISULPHATE OF POTASH AND CHLORIDE OF POTASSIUM ON CERTAIN METALS.

M. Person states that a remarkable action takes place with bisulphate of potash, an alkaline chloride, and such metals as form chlorosalts, such, for example, as those contained in the ore of platina: the sulphuric becomes sulphurous acid, its oxygen combines with the metal of the chloride, and the chlorine is evolved; then combining with the metal present, it forms an acid chloride capable of combining with the undecomposed portion of the alkaline chloride, and a double salt is formed.—*Ibid.*

PERMANGANESIATE OF POTASH.

M. Wöhler gives the following process for preparing this salt: Fuse chlorate of potash in a platina crucible over a spirit lamp: dissolve a bit of hydrate of potash in it, and add some peroxide of manganese; this also dissolves, producing a very fine green colour. There are then formed green manganesiate of potash and chloride of potassium. The mass is to be dissolved in boiling water; the green colour changes to a brilliant purple, because the manganesiate becomes permanganesiate. Decant, for the solution will not filter, and evaporate it. Small black opake crystals, with a greenish tint and a metallic lustre of permanganesiate, are obtained, mixed with chloride of potassium.

This salt is isomorphous with the heptachlorate of potash: they crystallize together in all proportions, without changing form. A few hundredths of permanganesiate of potash, mixed with a hot solution of heptachlorate of potash, give crystals of a fine ruby colour.—*Journal de Chimie Médicale*, July 1833.

SPARK DURING THE FREEZING OF WATER BY ÆTHER.

M. Julia Fontenelle states that M. Pontus, Professor at the Royal College of Cahors, has communicated to him the following observation. It is well known to chemists that if a phial, terminated by a small tube one to two centimetres long, be filled with water as well as the tube, and surrounded with cotton moistened with æther, the water freezes during the evaporation of the æther under the receiver of the air-pump. On repeating this experiment, M. Pontus remarked, that some moments before the congelation occurs, a spark, visible in daylight, escapes from the small tube which terminates the phial. This phænomenon is so generally true, that every time that he perceived the spark, he concluded the congelation was about to take place; and, on the contrary, when he did not see it, he presumed that the congelation was not near. M. Pontus was never disappointed in his conclusions. M. Fontenelle states that he also has seen the spark, and that M. Becquerel had remarked to him a similar effect at the moment of the formation of crystals in solutions.—*Ibid.*

SEPARATION OF BARYTES AND STRONTIA.

M. Liebig states that iodate of soda is an excellent reagent for separating barytes from strontia: the latter is not at all precipitated by it, while the former is completely thrown down by it from neutral solutions, so that no remaining trace of it can be detected. The precipitate is flaky.—*Annales de Mines*, April 1833.

COMPOSITION OF SERUM—SEROLIN.

M. Boudet remarks, that except the saline constituents of the serum of human blood, and the extractive matters, imperfectly known by the names of ozmazome, impure lactate of soda, muco-extractive matter, &c., the only well defined substances shown by analysis to exist in the serum are albumen, the fatty matter of the brain, urea, and an oily matter.

M. Boudet did not examine the extractive matters dissolved by water from dried serum, but only those products which were obtained from dried serum by boiling alcohol, after the water had dissolved such as were soluble in it.

Having obtained a considerable quantity of serum, dried it, and dissolved all that was soluble in boiling water, it was again dried, and treated with boiling alcohol: the mixed alcoholic solutions were colourless. The mixture became turbid by cooling, and deposited, though very slowly, white flocks, which were separated by the filter; these had a fatty pearly lustre: they were not crystalline, but small and slightly translucid plates. In M. Boudet's opinion these plates constitute one of the principles of serum, and he calls it *serolin*. This substance, when examined by the microscope, appeared to be formed of small filaments, with small white opake globules, which gave them the appearance of strings of beads; it fuses at about 134° Fahr.; does not act upon test papers, but, on the contrary, like cholesterine, it reddens concentrated sulphuric acid. It does not make an emulsion with cold water; and if it be heated, it floats like a colourless oil on its surface. Sulphuric æther dissolves it easily, even when cold; on the contrary, alcohol of 0·837 dissolves a trace of it when boiling, and none when cold. Heated for six hours in a solution of potash it suffered no change, and muriatic acid added to the liquor produced no turbidness.

Neither muriatic nor acetic acid, whether cold or hot, produced any change in this substance: when long heated with nitric acid it was not dissolved, but became soluble in a solution of potash and rendered it brown.

Distilled by the lamp in a small glass tube, it emitted a very characteristic odour, gave alkaline vapours and a light coaly residue: part of it seemed to volatilize. The small quantity obtained did not allow of performing more experiments; but the above, M. Boudet thinks sufficient to establish serolin as a new immediate principle, and to justify the name which he has given it.

The alcohol from which the serolin had been separated by the filter was distilled in a salt-water bath, and when reduced to one fourth of its bulk, the distillation was stopped and the liquor allowed to cool. It soon became turbid, but no material deposit was formed.

By continuing the evaporation in a capsule, a slightly yellowish brown residue was obtained: it was of the consistence of turpentine, and formed an emulsion with cold water. Its taste was acrid, and its smell similar to that of the phosphorized fatty matter of the brain.

This residue triturated with alcohol of sp. gr. 0·837, rendered it yellow, and attached itself to a tube like a soft resin. Fresh alcohol was added until it ceased to acquire colour, and two products were thus separated; one, soluble in alcohol, was of course the oily matter already alluded to, and the other was the fatty matter of the brain; this was insoluble in cold alcohol, but dissolved by it when boiling, and in æther, except a very small portion of a rosy matter, which was too minute for examination. It crystallized in brilliant laminæ, did not act upon coloured test papers, was unalterable by alkalies, made an emulsion with cold water; and its properties were perfectly similar to the cerebral fat, as described by Vauquelin and Chevreul.

The alcoholic solution, exposed for some time to spontaneous evaporation, gave small crystalline plates resembling cholesterine in appearance. On comparing their properties, it was found that they agreed nearly in their fusing point at about 278° of Fahr.; but they differed in this respect, viz. that pure cholesterine is crystalline, while this substance was in flocks, and had no crystalline splendour: this difference, however, appeared to depend upon its retaining a little phosphorized fatty matter, the odour of which it retained; and on mixing pure cholesterine with this substance, the mixture had the same properties as the supposed cholesterine of the blood. M. Boudet admits, however, that further experiments are required to ascertain positively that the serum of the blood contains cholesterine.

After the separation of this supposed crystallized cholesterine, the alcoholic solution was evaporated to dryness: it furnished a viscid residue, of an acrid taste, and soluble in alcohol. It still, however, retained cerebral fatty matter, which was separated as much as possible by alcohol, of sp. gr. 0·915, this appearing to have scarcely any action upon it: lastly, it was dissolved in æther, which separated from it traces of saline matter.

Thus purified, this new product was soft, somewhat transparent, of an acrid and soapy taste, slightly altered by phosphorized fatty matter, very soluble in alcohol and æther, and sensibly dissolved in water either cold or hot, and rendered it frothy like a true soap. Lastly, it restored the blue colour of litmus reddened by an acid; it appeared therefore to be a true soap. To decide this question, it was dissolved in hot water, and a few drops of muriatic acid were poured into the solution: abundant flakes were separated from a transparent liquid, and melted at the surface with the appearance of an oil. This oil after washing with hot water, retained no muriatic acid, reddened moistened litmus paper strongly, made no emulsion with water, and dissolved rapidly in alcohol and æther, rendering them acid: it immediately combined with soda, and reproduced a solution resembling common soap: it was probably a mixture of oleic and margaric acid.

M. Boudet remarks, that numerous attempts have been made to discover bile in the blood, and he further states that the existence above described of an alkaline soap, and probably of cholesterine, show that bile, or the various substances of which it is composed, are actually found in the blood.—*Ann. de Chim. et de Phys.*, t. lii. p. 337.

ROYAL OBSERVATORY OF OUDE.

It will give pleasure to our readers, and to all who view with interest the progress of science in India, to learn that the King of Oude is about to found an Observatory in his capital of Lucknow. The building was commenced in November 1832, and the work is altogether under the superintendence of an English gentleman, Captain Herbert.

The principal instruments for this establishment, which Messrs. Troughton and Sims are now constructing in London, are a mural circle of six feet diameter and a transit instrument of eight feet focal length. The apartment in the Observatory intended for their reception is fifty-four feet long, twenty-five wide, and twenty-five high.

FRESH-WATER LIMESTONE BETWEEN THE SEAMS OF COAL IN THE NEIGHBOURHOOD OF SHREWSBURY.

Mr. Editor,

With infinite pleasure I learnt, through the medium of your Journal, of the important discovery made by Dr. Hibbert, of a fresh-water limestone in the coal-measures at Burdie House near Edinburgh. The organic remains, and their high state of preservation, will secure to the limestone of that locality a marked attention, particularly if the tooth of a *Saurian* animal be included in these relics. In announcing this discovery, my friend Mr. Conybeare has stated, that "the limestone was shown to differ from the common carboniferous limestone of *marine* origin, and to form a species of deposit hitherto undescribed by geologists, being not of a marine but of a fluviatile or lacustrine character."

Now, Sir, without wishing to detract in the slightest degree from the merits of Dr. Hibbert's researches, I must state that the existence of a thick band of lacustrine limestone fairly included between seams of coal was pointed out by myself in March 1833, the discovery having been made in the year 1831, and confirmed in 1832, during repeated investigations of the country round Shrewsbury. In my communication* I distinctly asserted that "this limestone is similar in mineral aspect to the lacustrine limestone of Central France, and contains minute shells, referrible to fresh-water genera." At the same time it was shown that the associated beds of shale contained many of the published plants of the coal measures.

This Salopian fresh-water limestone is by no means confined to a single spot, but is found in many situations, distant from each other, where coal is worked, as at Pontesbury, Uffington, Le Botwood, &c., and always in the same position, separating the upper from the central seam of coal.

I certainly attached some importance to what I conceived to be a singular and an undescribed species of deposit; and I enlarged upon its importance in my memoir read before the Society, although the abstract simply announced the fact.

I remain, Sir, yours, &c.

RODERICK I. MURCHISON.

SCIENTIFIC BOOKS.

Curtis's British Entomology. Vol. x.

Transactions of the Society of Arts, Manufactures and Commerce. Part II. vol. xlix.

Parkes's Chemical Catechism. 13th Edition.

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Hooker's Botanical Miscellany.

Inquiries concerning the Medium of Light and the Form of its Molecules. By the Rev. John G. Macvicar.

A Century of Birds from the Himalayan Mountains. By John Gould, F.L.S.

A Popular Introduction to Experimental Chemistry. By Francis Watkins.

Analecta Anglo Saxonica: a Selection, in Prose and Verse, from Anglo-Saxon Authors of various Ages; with a Glossary. By Benjamin Thorpe, F.S.A.

* See Proceedings Geol. Society, No. 31. [Lond. and Edinb. Phil. Mag. vol. iii. p. 225.—EDIT.]

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. R. HOCKING at Penzance, and Mr. VEALL at Boston.

Days of Month, 1833.	Barometer.					Thermometer.					Wind.			Rain.		
	London.		Penzance.		Boston	London.		Penzance.		Bost.	Lond.	Penz.	Bost.	Lond.	Penz.	Bost.
	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	8½ A.M.						
Dec. 1	30·141	30·061	30·091	29·934	29·15	55	44	55	44	52·5	sw.	w.	calm	0·05	0·600	...
2	30·121	30·045	30·00	29·937	29·64	50	40	51	46	41·5	w.	w.	calm	·01	...	0·30
3	29·993	29·962	30·087	30·050	29·35	54	47	55	47	45	w.	w.	w.	...	·185	...
☾ 4	29·683	29·510	29·728	29·587	29·10	55	46	56	43	53	sw.	sw.	w.	·13	·145	...
5	29·537	29·495	29·577	29·567	28·98	49	38	49	39	43	w.	sw.	calm	·13	·185	...
6	29·670	29·559	29·766	29·746	29·03	55	41	55	42	39	w.	w.	w.	·15	·525	...
7	29·887	29·229	29·767	29·490	28·80	56	35	52	43	52	sw.	w.	w.	·03	·690	·05
8	30·113	29·610	30·123	29·896	29·55	55	41	55	45	40	nw.	w.	w.	·16	...	·25
9	29·752	29·568	29·795	29·781	28·97	56	40	56	45	56	sw.	w.	w.	·04	·445	·06
● 10	29·966	29·697	29·891	29·854	29·48	48	39	50	41	38·5	sw.	nw.	w.	·01	·180	...
11	29·725	29·699	29·926	29·859	29·11	44	33	49	40	39	w.	nw.	nw.	·06	...	·06
12	29·993	29·785	29·985	29·976	29·40	41	31	48	42	34	nw.	nw.	calm	...	·255	·13
13	30·182	30·094	30·157	30·132	29·70	49	38	51	44	32	w.	nw.	calm	...	...	...
14	30·162	30·043	30·143	30·060	29·58	50	43	47	44	42	w.	w.	calm	·01	...	·09
15	30·011	29·980	29·982	29·735	29·52	52	51	51	45	47	sw.	w.	calm	·06	...	·05
16	29·746	29·457	29·621	29·597	29·20	53	42	55	45	53·5	sw.	sw.	w.	·12	·710	·13
17	29·724	29·381	29·613	29·525	28·66	55	42	56	43	46	w.	sw.	w.	·11	·285	·08
18	29·813	29·494	29·642	29·621	28·88	54	44	57	45	51	w.	w.	nw.	·12	...	...
☽ 19	29·801	29·439	29·597	29·542	29·22	55	45	56	44	55	sw.	w.	w.	·14	...	...
20	29·517	29·395	29·613	29·601	28·71	49	35	54	45	48	sw.	w.	nw.	·76	·325	·17
21	29·852	29·355	29·512	29·506	28·80	47	35	53	45	42	sw.	w.	calm	·06	...	...
22	29·567	29·319	29·721	29·713	29·10	54	43	55	44	38	sw.	w.	calm	·11	·735	·06
23	29·516	29·241	29·496	29·431	28·99	54	42	54	43	41·5	ne.	sw.	calm	1·13	...	·20
24	29·415	29·312	29·531	29·358	29·	52	42	55	42	46	s.	w.	calm	·26	1·250	·07
25	30·068	29·526	29·721	29·719	28·97	45	28	53	44	44	sw.	w.	ne.	...	...	·11
○ 26	30·191	29·877	29·863	29·851	29·80	47	34	52	41	31	sw.	w.	calm	·20	...	·03
27	29·825	29·695	29·981	29·832	29·43	49	33	53	42	40	sw.	w.	calm	·21	...	...
28	30·011	29·810	29·732	29·565	29·42	54	37	54	41	37	sw.	w.	calm	·05	0·140	...
29	29·821	29·758	29·753	29·632	29·15	53	48	55	43	51	w.	w.	w.	·03	...	·03
30	29·890	29·631	29·812	29·683	29·20	56	43	55	42	46	w.	w.	calm	·05	·660	...
31	29·715	29·558	29·821	29·583	28·95	52	38	55	41	45·5	w.	w.	w.	·10	·465	·11
	30·182	29·229	30·157	29·431	29·18	56	28	57	39	44·1				4·29	7·780	1·98

Remarks.

London.—Dec. 5. Fine: clear with lightning at night. 8. Fine: strong wind with heavy rain at night. 9. Rain. 11. Clear and cold: lightning at night. 13. Frosty: clear and cold. 21. Windy. 23. Dense fog with heavy rain. 26. Frosty: cloudy, so that the total eclipse of the moon could only be seen indistinctly at intervals. 28. Overcast. 31. Stormy and wet.—The depth of rain was nearly 2 inches above the average of this month.

Penzance.—Dec. 3. Showers. 4. Misty: showers. 5. Showers, hail and rain: heavy rain at night. 6. Showers, hail and rain: rain at night. 7. Very stormy: showers: fair. 8. Fair: rain. 9. Rain: fair. 10. Fair: showers: stormy: hail: showers at night. 11. Showers, hail and rain: stormy at night. 12. Hail showers. 16. Shower: rain. 17. Heavy showers. 18. Showers. 19—21. Showers: rain at night. 22—24. Rain. 25, 26. Fair: showers. 27. Rain: fair. 28. Fair: showers. 29. Rain throughout. 30. Showers: rain.

Boston.—Dec. 6. Fine: rain early A.M. 7. Stormy: hurricane with rain P.M. 9. Cloudy: rain early A.M. 11. Stormy: rain early A.M.: rain, hail and snow P.M. 13. Fine: rain P.M. 15. Cloudy: rain early A.M. 16. Cloudy and stormy: rain early A.M. 17. Stormy: rain early A.M. 18, 19. Stormy. 20. Cloudy and stormy: rain early A.M. 21. Cloudy: stormy with rain P.M. 22. Cloudy: rain A.M. 24. Cloudy: rain early A.M. 25. Cloudy: rain early A.M. and P.M. 28. Fine: rain P.M. 31. Rain: rain early A.M.: blew a hurricane P.M.

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MARCH 1834.

XXIX. *On the probable future Extension of the Coal-fields at present worked. By the Rev.* W. D. Conybeare, *M.A., F.R.S., &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN the application of geological science to the development of the mineral resources which constitute so material a part of the wealth of nations, no subject can possibly claim a higher statistical importance than the investigation of the relations of our principal coal districts, with a view to point out the lines in which we may look with the greatest probability for the future profitable extension of their workings, when the immense and increasing demand for that mineral shall threaten to exhaust our present supplies; a period which some apprehensive geologists have predicted to be within a few centuries. Although I myself incline to be more sanguine, still every one must allow the discussion to be one of the very first œconomical interest. I have already in my former geological publications often alluded to it; but I am persuaded that a short connected view of what is already known may at the present moment be useful, and may very probably tend to elicit fresh information in the points where it will thus be shown to be most important.

I shall first hastily survey with this view the great coal-fields of Durham and Yorkshire, on the eastern skirt of the great chain of the Penine hills, which traverses, as a back-bone, our northern counties. I shall then examine the detached fields scattered through the plains of the midland counties to the south of the expiration of that chain, and thus proceed to those on its western skirt, extending thence to the borders of

Wales, and still more to the north, skirting the Cumbrian or Lake Mountain group. Lastly, I shall speak as to the coal-fields of our south-western districts of Somerset, Gloucester and South Wales.

I. The Northumberland and Durham coal-field is well known, extending from the mouth of the Coquet on the north to the banks of the Tees on the south. The dip of the strata is along the northern edge to the south-east, and through the middle of the field due east; but at its southern extremity along the Tees, near Bishop's Auckland, the line of bearing appears to curve, and they change their dip, first to north and then to north-west. Along the south-eastern border they are uniformly overlaid by a terrace of magnesian limestone; but Mr. Sedgwick has satisfactorily shown that they are capable of being profitably pursued beneath this limestone, and actually have been so in many instances. Here, therefore, is an extension of the field apparently limited only by the expense of deep drainage. But the most important point connected with our inquiry presents itself on the south of the Tees, where, in consequence of the curvature which we have mentioned of the line of bearing of the coal strata to the east and north-east, the magnesian limestone, extending in its regular course to the south, overlies unconformably their edges, and comes in contact with the more unproductive subjacent strata of millstone grit, &c. Now it ought carefully to be ascertained whether this curvature near Bishop's Auckland be more than a merely partial inflection; and whether the main coal strata do not speedily resume their southerly bearing, beneath the covering of the magnesian limestone, under such circumstances that they might still be profitably worked.

From the Tees to the Wharfe the eastern terrace of magnesian limestone in its course through northern Yorkshire appears almost immediately in contact with the more barren inferior strata; but Mr. Smith, in his Geological Map of Yorkshire, has indicated a thin zone of carboniferous measures as accompanying the greater part of its course; and Mr. Sedgwick has indicated coal as worked in this interval near the banks of the Gore at Winksley, and on the right bank of the Nid near Bilton. Now if in pursuing these indications eastward in the direction of the dip beneath the magnesian limestone the main seams were recovered, and traced throughout the interval between the Tees and Wharfe, a district fully equal to those already worked might be laid open; and as the seams of the Somersetshire coal-field are worked under a still thicker covering of superincumbent strata of magnesian limestone, red marl, and lias, I do not doubt that these might be worked with equal ease; and I believe I have often heard Mr. Sedgwick express his opinions to the same effect.

South of the Wharfe the main coal-seams again emerge, and continue exposed throughout the great field of South Yorkshire and Derbyshire, as far as the banks of the Derwent; but here they are again overlaid by the superincumbent strata, for the red marl, sweeping round to the west in this direction, overflows, as it were, all the extensive plains of our midland counties, effectually concealing all the subjacent strata.

I do not believe that any judicious endeavours have been made to trace the probable prolongation of the coal-beds of Nottingham beneath this covering to the south of the Derwent; and this is one of the points which most demands, and would ultimately best repay, such researches.

I am thus conducted to the scattered coal-fields of our midland counties, which may probably be regarded as partial indications of the southern extension of the fields already traced; but as these will require a more minute detail, I must reserve them for a future communication. Your old correspondent,

W. D. Conybeare.

[To be continued.]

XXX. *Trigonometrical Height of Ingleborough above the Greeta at Ingleton.* *By* John Nixon, *Esq.**

BAROMETRICAL observations of the altitude of Ingleborough above Ingleton, instituted with a view to improve the formula of calculation, might be obtained under many advantages. Ingleton adjoins a great (coach) road, and possesses more than one comfortable inn, situated within a short distance of an eligible inferior station, more than 2000 feet below Ingleborough. On the summit of the hill the substantial tower lately erected would afford complete shelter to the observer, and enable him to fix his barometer unexposed to sun, wind, or wet. The descent is gradual, little obstructed by rocks or bogs; and at one third of the distance to the village is a farm-house, where the instruments might be left with perfect safety. The indispensable *datum* of the trigonometrical height of the mountain above some fixed standard at its base, I have attempted to supply, and with, I trust, success, by the various measurements of which the details are now given.

At Ingleborough and at Hunt's Cross the angles for the distances were measured by the 12-inch repeating circle described in the Lond. and Edinb. Phil. Mag., vol. i. p. 340. At the latter station, and also at the base line measured near Ingleton church, the vertical angles were obtained by the horizon sector.

* Communicated by the Author.

At Colonel Mudge's station on Ingleborough, which is 197 feet east of the centre (axis) of the round tower erected on the site of the beacon hut, the angle between the tower and his signal on Whernside measures 87° 20′. The distance from the station to Whernside being 22,435 feet, that between the centre of the Tower and Whernside (our base line,) will be 22,445 feet. The Tower, of which the base, resting on an elevated flagged terrace, is on a level with the ground at the station, measures 22 feet 3 inches in height, and 57 feet in circumference.

About a mile from Thornton church, on the road into Kingsdale, lies, at a short distance to the left, a steep rounded knoll, called Hunt's Cross. On the fence wall which crosses the crest of the hill, a pile of stones, presenting a triangular front towards Ingleborough, was erected and whitewashed. At a station a few yards east of this signal the following *oblique* angles were obtained.—(N. B. Of the two objects subtending the angle, the one to the *left hand* is *first* mentioned.)

At Hunt's Cross Station.

	°	′	″
(I.) Between Whernside and Ingleborough Tower, *left side* at base. (Multiple of repetition 12.)	60	4	7
(II.) Between Whernside and Ingleborough Tower, *right side* at base. (Multiple 10.)	60	7	39

(III.) In the same plane the angle between Hunt's Cross signal and the centre of Ingleborough Tower measured 165° 1′ 24″; and the horizontal distance from the signal to the centre of the circle, 84 feet.

(The horizontal angle* at Hunt's Cross station, between Whernside and the centre of Ingleborough Tower, will be 60° 12′ 5″, (= 60° 5′ 53″ + 6′ 12″, the correction for obliquity).)

	°	′	″
(IV.) Between Ingleborough Tower, left side at base, and the top of the north-west pinnacle of Ingleton church tower. (Mult. 11.)	62	57	12
Semidiameter of Ingleborough Tower		—1	44
	62	55	28

(V.) In the same plane the angle between Hunt's Cross

* For the zenith distances at this station consult the register of the vertical angles; those at Ingleborough may be found from the given differences of level and the distances.

The telescope lies about 1·5 inch to the left of the vertical axis of the circle, but a correction on this account would be too trivial to be regarded.

signal and the centre of Ingleborough Tower measured 164° 46′ 32″.

(The horizontal angle at Hunt's Cross station, between the centre of Ingleborough Tower and the top of the north-west pinnacle of Ingleton church tower, will be 62° 22′ 58″ (= 62° 55′ 28″ + 32′ 30″ obliquity).)

(Having reduced to the horizon the two measures, (III.) and (V.), of the angle between Hunt's Cross signal and Ingleborough Tower, their mean value*, with the distance, 84 feet, will give 4′ 14″ for the angle at Ingleborough Tower centre between Hunt's Cross station (to the left) and the signal on its wall (to the right).)

At the North-west Side of Ingleborough Tower.

Angles taken by the repeating circle.

(VI.) Between Hunt's Cross signal and Whernside. (Multiple 10.) ... ... } 76° 51′ 10″

(VII.) In the same plane the angle between the centre of Ingleborough Tower and Hunt's Cross signal measured 126° 39′ 0″, and the distance to the nearest point of the Tower 7 horizontal feet, or 16 feet to its centre.

(The horizontal angle at the centre of Ingleborough Tower, between Hunt's Cross station and Whernside, will be 76° 49′ 46″ (= 76° 51′ 10″ −2′ 10″ obliquity; −3′ 28″ reduction to centre of Tower; and +4′ 14″ reduction from Hunt's Cross signal to its station).)

	°	′	″
(VIII.) Between the north-west angle of Ingleton church tower at top, and Hunt's cross signal. (Multiple 6.)	27	36	13
Reduction to north-west pinnacle top, 1 foot			+ 13
	27	36	26

(IX.) In the same plane the angle between the centre of Ingleborough Tower and Hunt's Cross signal measured 126° 13′ 40″.

(The horizontal angle at the centre of Ingleborough Tower, between the top of the north-west pinnacle of Ingleton church tower and Hunt's Cross station, will be 27° 28′ 57″ (= 27° 36′ 26″ − 2′ 16″ obliquity; − 0′ 59″ reduction to the centre of Ingleborough Tower; − 4′ 14″ the angle between Hunt's Cross station and its signal).)

* The two horizontal angles should come out the same; but as the telescope did not point in either instance at the exact height of the signal, marks were substituted, set up at the proper heights, as nearly in a line with the signal as the eye could estimate. The slight difference in these angles will not materially alter the one of 4′ 14″.

*Horizontal Distances**.

Base; Whernside to Ingleborough Tower centre, 22445 ft.

Whernside	(42° 58′ 9″)	... 25184·6 ft.
Ingleborough Tower centre ...	76 49 46	... 17629·7
Hunt's Cross station	60 12 5	
Ingleborough Tower centre ...	27 28 57	... 15621·0
Hunt's Cross station	62 22 58	... 8135·7
Ingleton church tower, top of north-west pinnacle	(90 8 5)	

Vertical Angles.

As the levels of the sector have graduated scales, it becomes superfluous to bring the bubble at every observation exactly to the *reversing* point, (or that degree of the scale to which the bubble constantly returns on inverting the telescope within its Ys.). At Ingleton the reversing points, determined from the average of three independent trials, were 74°·8 for the level of the arc G, and 72°·5 for that of the arc L. In the register there is given for every observation the degree of the scale at which the middle of the bubble became stationary; the difference between which degree and the reversing point, converted into seconds, (at the rate of 1″·82 per degree for the level of G, and 2″ per degree for that of L,) being additive to, or subtractive from, elevations, according as the registered degree exceeds or falls short of the reversing point. For depressions the signs must be changed.

Each vernier being what is termed *double*, two readings of the fractional part of the angle are obtained, both of which are given.

At Hunt's Cross a short horizontal filament, found adhering to the vertical wire, was substituted, in pointing the telescope, for the cross lines made use of at the station on the base near the church. In both cases the error of collimation will be found in the last column but one.

In calculating the differences of level, the angles of elevation, marked E, were augmented by the instrumental error of 20″, the (angular) height of the eye, and curvature; the refraction, which was estimated at $\frac{1}{17}$th of the contained arc, being subtractive. For depressions, marked D, all the signs were changed.

* These distances were confirmed by using the oblique (or observed) angles, and diminishing the distances thus obtained in the ratio of radius to the cosine of horizontal inclination.

At Hunt's Cross Station.

November 13, 1833, between 11 A.M. and 2 P.M.;—the hills remarkably clear, but the valley rather hazy. The sector stood on a firm board, screwed to the substantial tripod of the repeating circle. Eye 2 feet 8 inches above the highest part of the hill (between the station and the signal), and 1 foot 6 inches below the base of the signal on the wall. The observations are numbered in the order in which they were made.

			Arc G.	Level.	Arc L.	Level.	E. of Coll.	Diff. of Level. Feet.
			° ′ ″	°	° ′ ″	°	′ ″	
Ingleborough Tower, top, nearest point	(1)	E.	3 58 22½	70·5	3 53 52½	76·0	−2 9	+1223·0
			27½		50			
	(2)	E.	3 58 22½	71·0	3 53 52½	73·3	−2 12	
			27½		52½			
	(5)	E.	3 58 18	73·0	3 53 42½	74·0	−2 14	
			25		50			
Ingleton church tower, top of N.W. pinnacle	(4)	D.	4 48 10	90·5	4 52 37½	71·0	+2 32	−683·7
			2½		40			
	(7)	D.	4 47 32½	59·3	4 52 50	71·0	+2 30	
			17½		50			
Ingleton church tower, top at N.W. angle...	(9)	D.	4 48 42½	59·0	4 54 32½	71·5	+2 42	−687·5
			35		30			
Ingleton church tower, highest ledge at N.W. angle.........	(8)	D.	4 51 27½	64·0	4 57 30	78·0	+2 49	−693·8
			17½		32½			
	(10)	D.	4 51 32½	62·5	4 57 30	82·0	+2 40	
			25		30			
Whernside, ground at signal (?)	(3)	E.	2 48 52½	80·8	2 44 55	73·0	−2 5	+1243·0
			57½		52½			
	(6)	E.	2 49 7½	78·5	2 44 42½	75·0	−2 11	
			2½		47½			
Base of Ingleborough Tower			(1223·0 −22·3 height of tower =)					+1200·7

Height of Ingleton Church Tower above the Greeta.

A base line having been marked out on a gentle and sufficiently even slope near the east bank of the Kingsdale stream, (just above Thornton bridge,) a straight deal rod, about 2 inches square and 6 feet long, was driven perpendicularly into the ground at each end of the base. The distance between the rods appeared to be 898 feet 4 inches, or 898 feet 9 inches, according as it was measured in lengths of 50 or of 60 feet each of an excellent 66 feet tape. Corrected for the error of the tape, which had been compared, under the usual degree of tension, with an eight-feet levelling staff, the base line would be 898 feet 3 inches, or if we reject the fraction on account of some slight inequalities in the ground, 898 feet.

The angles taken at each end of the base were by a pocket sextant (by Allan) reading off to 1′ or less, of which the index error, scarcely appreciable, had not sensibly varied in the course of two years. Both rods being about 5 feet above the ground, the observations were made with great nicety by supporting the sextant on the top of the rods.

Rod at north end of base 37° 1′ 0″ } 1037·5 inclined, or
Rod at south end of base 83 38 45 } 1034·0 horizontal feet.

Ingleton church tower, top of N. W. pinnacle } (59 20 15)

The distances from the north end of the base to the top of the church tower, and to the ground at its west base, may be estimated severally at 1034 feet; but that to the projecting ledge should be called 1033 feet.

Vertical Angles measured by the Sector at the north end of the base. Height of eye 4 feet 10 inches.

	Arc G.	Level.	Arc L.	Level.	E. of Coll.	Diff. of Level.
	° ′ ″	°	° ″	°	″	
Top of N.W. pinnacle of Ingleton church tower.....	E. 4 45 27½ / 35	77·5	4 45 32½ / 30	71·0	−8	+91·03
Top of Ingleton church tower near N.W. angle	E. 4 32 52½ / 33 0	72·5	4 33 15 / 12½	64·5	+6	+87·21
Ingleton church tower, highest ledge at N.W. angle................	E. 4 12 0 / 7½	73·5	4 12 7½ / 12	65·5	−6	+80·80
Ingleton church tower, ground at west point.....	E. 1 40 0 / 0	85·5	1 40 47½ / 40	64·5	+8	+35·16
Top of rod at south end of base........	D. 1 16 22½ / 27½	68·5	1 16 17½ / 17½	64·0	−2	−15·07
Ground at south end of base	(15·07 + 4·83 height of rod =)					−19·90

The fall from the ground at the south end of the base to the *spring* of the west end of the north side of the great or eastern arch of Thornton bridge, (a distance of about 35 yards,) was found by the 8-feet levelling staff, and a 12-inch telescopic level, (adjusted on the wires of the sector,) to be 6·38 feet. Having measured by the sector, placed at an intermediate distance, the elevation of the south end of the base and the depression of the spring of the arch, and obtained by the tape the corresponding distances, the difference of level appeared to be 6·47 feet, the mean of the two measurements being 6·44 feet. The fall from the bridge to the confluence of the Kingsdale and Chapel-le-dale streams (a distance of 150 yards,) was ascertained by the levelling staff and sector to be 11·93 feet.

The data, now complete, are presented so arranged as to exhibit at one view the final process of calculation.

		Feet.Tenths.
Hunt's Cross above church pinnacle	683·70	... 774·73
Pinnacle above base at north end......	91·03	
Hunt's Cross above church tower top	687·50	... 774·71
Tower above base at north end.........	87·21	
Hunt's Cross above church tower ledge	693·80	... 774·60
Ledge above base at north end.........	80·80	
Hunt's Cross above ground at north end of base ...		774·68
North end of base above south end		19·90
South end of base above spring of arch...		6·44
Hunt's Cross above spring of arch...		801·02
Ingleborough above Hunt's Cross...		1200·70
Ingleborough above spring of arch of Thornton bridge		2001·72*
Fall from arch to confluence of the two streams, or head of the Greeta		11·93
Ingleborough above the Greeta		2013·65

The height of Whernside above the Greeta will be 2056, and that of Hunt's Cross 8·13 feet. Reciprocal observations at Ingleborough and Whernside give 41·3 for their difference of level, or one foot less than by the measurements at Hunt's Cross. At the latter place the signal on Whernside did not, however, appear of its usual height, the ground in front of it probably intercepting the view of its base.

Possible Errors of Measurement.—Admitting the angles observed for the distances liable to an error of 10″ each, we may make the distance from Hunt's Cross to Ingleborough tower *side* 17623 instead of 17621 feet, and that to the church tower 8137·5 instead of 8135·7. Now increase their corresponding vertical angles 10″ each, and we shall get an augmentation in the height of Ingleborough above the church of 1·5 foot. If we *omit* the correction for refraction, we add 6 inches to the error, now become 2 feet. There cannot well be any material error in the height of the church above the

* Thornton bridge would be an excellent inferior station for measurements by the barometer of the height of Ingleborough, Whernside, Hunt's Cross and Greygreth.

south end of the base beyond what might be introduced by a faulty measurement of the latter, or such an error as might be committed in measuring 100 feet by a tape of a corrected length. About two years ago the height of the church tower was found by observations with a 4-inch theodolite to be 115·6 feet above the water under Thornton bridge. The base line was 318 feet in length, and the vertical angles taken at both ends of it gave the same height within one inch. Supposing the stream, noted as extremely low, to have been one foot below its level in November last, (or two feet below the spring of the arch,) then will the two measurements agree *exactly*.

Since the *possible* errors, though all tending one way, do not amount to more than two feet, we may certainly claim for the measurement a degree of accuracy equal to *one* foot.

Leeds, Dec. 16, 1833. JOHN NIXON.

XXXI. *On the* Zimb *of* Bruce, *as connected with the Hieroglyphics of Egypt. By the Marquis* DI SPINETO*.

AT the top of the cartouche which incloses the mystic titles of the Pharaohs, we invariably find two hieroglyphics,—a crooked line and the figure of an insect,—which M. Champollion has interpreted to signify, *king of the obedient people*; for he takes the first, a sort of crooked line, as a phonetic character expressing the letter *s*, and an abbreviation of the word ⲥⲟⲧⲛ (*soten*), which means *king*; and considers the figure of the second, an insect, to be that of a *bee*, and upon the authority of Horapollo, the symbol of an *obedient people*. "Si nous tenons compte du témoignage formé d'Horapollon, l'abeille exprimait en écriture hiéroglyphique, λαον προς βασιλεα πειθηνιον, 'peuple obeissant à son roi'....L'abeille, caractère purement *symbolique*,—*l'abeille*, insecte industrieux, au quel une vie laborieuse, et dirigée par un instinct admirable, donne une apparence de civilisation qui dut en effet le faire considérer comme l'emblême le plus frappant d'un peuple soumis à un ordre social fixe, et à un pouvoir régulier." (*Précis du Syst. Hiero.* p. 184–5.)

In my Lectures on Hieroglyphics, published by Rivington, although I could not see why the Pharaohs should boast of commanding over an obedient people, and thus pass an eulogy upon the nation rather than upon themselves, admitting the supposition that the figure of the insect was that of *a bee*, I adopted the same idea; but in pursuing my researches on

* Communicated by the Author.

the antiquities of Egypt, to ascertain, if possible, the origin of polytheism and the worship of animals, I found in Bruce the description and the figure of a terrible insect, very much resembling the bee in its appearance, but totally different from it, inhabiting within the limits of the black fat earth which is found on the marshy grounds of the Nile, and so mischievous as to drive both the inhabitants and their cattle from their habitations, and, indeed, from a whole district. This insect is a native of Abyssinia, and has no specific name. Mr. Bruce asserts that the Arabs call it *Zimb*, which, he says, means *Fly* in general. But I apprehend there must be a mistake in the spelling. There is no such word, nor anything like *Zimb*, in the whole Arabic language. The term by which the Arabs distinguish the *fly* is *Thubab*; and it seems that a corruption of it has supplied the Chaldee version with the name of *Zebub*, which Mr. Bruce quotes as having the same general signification of *a fly.*

In deference to Mr. Bruce, the name of *Zimb* has been adopted in the Bible published by D'Oyly and Mant, and even by Mr. Kirby in his interesting work upon Entomology, for indeed they had no better name to supply,—an example which I shall follow.

Amongst the Greeks it received the appellation of κυνόμυα, or κυνόμυια, which signifies the *dog-fly*; and the Church of Alexandria, in correcting the Greek copy to make it conformable to the Septuagint, imitating the Greeks, has called it *Tsaltsalya Kelb*, which means the same thing. But the fact is, the Greek word itself is but a translation of the old name by which the ancient Egyptians distinguished this insect. They called it ⲁϥ ⲛ̀ ⲟⲩϩⲟⲣ (*af an ouhor*); which is a combination of ⲁϥ (*af*) a fly, ⲛ̀ (*an*), the article or mark of the genitive case, and ⲟⲩϩⲟⲣ (*ouhor*) a dog. Professor Rosenmüller, without knowing this, asserts that in Exodus viii. 21. as well as in two of the Psalms, this fly is called *Arob* [quære *Oreb*], and says that this word *Arob* cannot signify the *dog fly*, but the *blind fly*; but this is a mistake, for the name of *Arob*, or rather *Oreb*, literally means *a devouring fly*. Professor Rosenmüller says that he has adopted that opinion in consequence of the description which Philo gives of this insect. But this is a second error; for from the description he gives of the devastation produced by the *Arob*, it can never be the blind fly, the 'fly' of Moses, the *Zimb*, which is infinitely more mischievous and terrible. The Professor says, that " it flies with great noise, and is extremely troublesome both to man and beast. It is especially quite intolerable to the large herds of cattle in the woods of Hungary, and even to the shepherds themselves. Wherever it settles itself, it cannot be taken off again: it gluts itself with blood and makes bloody boils; the large ones are painful."

He considers it to be the same insect which is "very plentiful in the Levant, which makes a very painful bite, and when it multiplies uncommonly, can become a serious plague; for it not only torments men and cattle, but it also gnaws household furniture and clothes, and eats all sorts of provisions."

Hence, he says, "it has been conjectured that *Arob* signifies *Karcherlake* or *Küchenschabe*," which is in fact the *Blatta*. But this is another error. The similarity of the devastations made by the *Karcherlake* and the *Zimb*, even on the supposition of their being equally terrible and extensive, no more proves that these two insects are alike, than the fondness of devouring dead carcasses establishes the similarity between a rat and a pig. And in the present case, another fact militates against the position, for, however troublesome and mischievous the *Arob* may be, and to whatever degree its devastations may be carried, they are nothing in comparison to those of the *Zimb*. They may be considered to resemble, but in a greater proportion, those of a species of *gad-fly*, which is so very abundant in Lapland during the summer, as to oblige the natives, for the sake of preserving the rein-deer, to emigrate to the mountains; though even the damage done by this insect is nothing in comparison with that which Bruce describes of the *Zimb*; and which corresponds exactly with the account we have from Moses. Here are his words.

"Small, insignificant and ugly as this insect is in appearance, when we consider its history and its powers, we are obliged to acknowledge that those huge animals, the elephant, the rhinoceros, the lion, and the leopard, are vastly his inferiors: the very appearance, nay, the very sound of its approach, occasions more trepidation, movement and disorder, both in the *human* and *brute creation*, than whole herds of the most ferocious wild beasts in a tenfold greater number than they are. As soon as this plague appears, and their buzzing is heard, all the cattle forsake their food, and run wild about the country till they *die, worn out with fright, fatigue and hunger.* Even elephants and rhinoceroses, which, by reason of their enormous bulk and the vast quantity of food and water they daily need, cannot shift to desert and dry places, roll themselves in mud and mire, which coats them over like armour, and enables them to stand their ground, though not always, against this winged assassin; for when once attacked by this fly, their body, head and legs break out into large bosses, *which swell, break and putrify, to their certain destruction.*

"It seems that Providence has confined the habitation of this terrible insect to one species of soil, a black fat earth, such as is found in the marshy parts of the Nile; and this fortunate circumstance enables the natives, with their cattle, to leave the country, and hasten down to the sands of Atbara, and there to remain while the rains last, this cruel enemy never daring to pursue them further." This is no partial emigration, but a general one of the whole country; nor is there any alternative, though a hostile band were in the way, capable of despoiling them of half their substance.

From this description it seems evident that this terrible insect, this *Zimb* of Bruce, must have been the fly which formed the fourth plague that God sent upon the Egyptians, and which, in the language of Scripture, would "put a division between them and the Israelites," and sever the land of Goshen, "where these latter dwelt, from the land of Egypt." For this land, the possession of the Israelites, was a land of pasture, neither tilled nor sown, because not overflown by the Nile: but the land inundated by that river was the black earth of the valley of Egypt; and as by nature the *Zimb* never leaves this black earth, it followed that no fly could be seen in the sand or pasture of the land of Goshen, because this kind of soil had ever been the refuge of all cattle emigrating from the black earth round the Nile to the lower region of Atbara. The prophet Isaiah, in fact, (vii. 18, 19,) has given an account of this insect, and the manner of its operation: "The Lord shall *hiss* for the fly that is in the uttermost part of the rivers of Egypt....and they shall come, and shall rest all of them in the desolate valleys:" which, if my interpretation be correct, cannot mean anything else than the fly *shall cut off from the cattle their usual retreat*, by taking possession of those places which are the refuge of the cattle; and perhaps this fly, or some species related to it, was the prototype of the Philistine idol, the god of Ekron, worshiped in the form of a fly under the name of Baal-zebub, which literally means 'the fly of Baal', and was no doubt the corruption of *Thubab*, the Arabic name of a fly.

Upon these considerations it seems reasonable to suppose that the figure of the insect at the top of the cartouche inclosing the mystic titles of the Pharaohs is not that of a *bee*, but of the *Zimb* of Bruce; and if so, it cannot be the symbol of an *obedient* people, but must necessarily be the symbol of *Lower* Egypt and the valley of the Nile. Many reasons concur in favour of this surmise. In the *first* place, the number of wings: the bee has four, the insect at the top of the cartouche only two. In the *second* place, the figure of the body, which resembles more that of the *Zimb* given by Bruce than that of the bee. *Thirdly*, the position of the antennæ. In the bee they diverge, in the *Zimb* they are parallel and lie straight forward, which last position, in the hieroglyphics, has been changed into a vertical position, for the evident sake of saving room. In addition to these considerations it may be noted, that as far as I have been able to ascertain by monuments hitherto discovered, the figure of this insect seems to have been introduced among the hieroglyphics describing royal titles, during the reigns of the Pharaohs of the eighteenth dynasty, that is, after the expulsion of the Hyk-shos; and therefore, as the *Zimb* was confined within, and never went beyond the limits of the black earth in the valley of the Nile, its figure might have been reasonably taken as the symbol of Egypt in general, or perhaps more properly of Lower Egypt, the place of residence of the Hyk-shos, and thus offer to the Pharaohs the boast of ruling over that country, which at one time was wrested from them by these shepherd intruders. That the Pharaohs felt a pride of this sort, is evident from the introduction of one of these Hyk-shos, in the most abject position and submissive attitude, in every one of the monuments of triumph of each and all the Pharaohs who *succeeded* Mishra-Thoutmosy, the *first* prince of the eighteenth dynasty, who had expelled them from the country.

To this original cause of boasting, the Exodus of the Israelites may have added another; for as the fly was the fourth plague with which God visited Egypt, so it might gratify the vanity of the Pharaohs to exult at ruling over that country, which at one time was so infested by that insect, on account of the hated Israelites. That such hatred existed, is evident from the inspection of historical monuments, in which the figure of a Jew is invariably seen in an humble and dejected position, either among the prisoners following the car of the Pharaohs, or serving as a foot-stool to their throne.

Upon these considerations, I think that the figure of the insect at the top of the cartouches above mentioned, is not that of a *bee*, but of the *Zimb* of Bruce, and that, together with the crooked line which precedes it, it means, not *king of an obedient people*, but King of Egypt, or, more specifically, *King of Lower Egypt*. Nor are we to be surprised at M. Champollion taking it for the figure of a bee, for Bruce, who had given the description and the drawing of it, openly asserts that *it resembles the genus of the bee very much.*

But here it may be asked, what sort of insect this *Zimb* is? To what class, to what genus, to what species does it belong? Is it to be found in different parts of the world, or is it exclusively confined to Egypt? To these questions I answer, that this insect belongs to the order of the *Diptera*; it is a sort of gad-fly, or rather an undescribed species of the *Œstrus*, confined to the black fat land of the marshy grounds round the Nile, and was known to the ancient Greeks by the appellation of κυνόμυια, a translation of the Egyptian name *af an ouhor*, which means 'a dog's fly.'

In order to establish this opinion, I have to observe, that the first who gave the description of this insect was Bruce. "In its size," he says, "it is very little larger than a bee, *the genus of which it resembles very much*, though its proportions are thicker, with wings more broad, but placed separate like those of a fly. These wings are of a pure gauze, without colour or spot. The head is large; the upper jaw is sharp, and has at the end of it a strong pointed hair of about a quarter of an inch long. The lower jaw has two of these pointed hairs; and this pencil of hairs, when joined together, makes a resistance to the finger nearly equal to that of a strong hog's bristle. Its legs are serrated inside, and the whole is covered with brown hair or down. It has no sting."

From this description it is evident that this insect belongs to a species not known in Europe: for it has several of the properties of the *Bombylius*, the *Tabanus*, the *Œstrus* and the *Hippobosca*, without belonging to any of them. In some of its generic and even specific characters it is like the *Bombylius* and the *Œstrus*, in others like the *Hippobosca* and the *Muscidæ*; in a few like the *Tabanus* and the Dog-fly, whilst in the aggregate it differs from every one of these insects. Mr. Bruce says that it *resembles very much the genus of a bee*; but this is a mistake. It may be so in appearance, but it is not so in reality. The bee has four wings, and the figure which he has published of his *Zimb* has only two, though it cannot be denied that in appearance, as far as the form of its body goes, it may resemble the bee, and might be mistaken for it, yet it has no sting.

The *Zimb*, in fact, belongs to the *Diptera*, closely allied to the *Muscidæ*, and in this respect is like the *Bombylius* and the *Œstrus*. It has hairs long and sharp, which perform the office of the taper trunk of both the *Bombylius* and the *Œstrus*, without, however, being inclosed by two horizontal valves, as those of the *Bombylius*. Its body, though apparently like that of a bee, is shorter and thicker than that of the *Bombylius*, which is slender; but it is larger than that of the *Œstrus*, which is small, and, like it, is covered with a thick brown down, which in the *Bombylius* is yellow. Its wings are throughout transparent, while those of the *Bombylius* are more so next to the anterior, but less so towards the posterior margin. It has six legs; they are rather thick and of a bright colour, not so long as those of the *Tabanus* or of the *Bombylius*, which are slender and black, but quite as long as those of the *Œstrus* and the bee. It has no sting, and its motions are more rapid and sudden than those of a bee, and not dissimilar to those of the *Œstrus*. In flying it makes a sort of buzzing or jarring noise, like the *Tabanus* and the *Œstrus*, together with a humming like a bee: this, Mr. Bruce observes, proceeds in great measure from a vibration made with the three hairs at its snout.

It is to be lamented that Mr. Bruce, in giving the short description he has of this insect, has neglected to observe the use to which the three hairs are destined. That they are piercers there is not the least doubt, and most probably perform the office of suckers, simply because it seems that it does not lay its eggs in the flesh of animals; for, according to the account which Bruce gives of the evils attending the attacks of this fly, the bosses which are produced swell, break and putrify, to the certain destruction of the animal, but never exhibit any larvæ or maggots; and in this respect also it differs considerably from the *Œstrus*, for according to the excellent essay of Mr. Clark, *on the Bots of Horses and other Animals*, the *Œstrus* does not pierce the skin of the animal, but only glues its eggs to it, an operation which it performs in a few seconds, during which the animal attempts to lash it off, as it does other flies, with its tail; an evident proof that this infliction is not attended with much pain, though after it the animal may be seized with a kind of phrensy; while the very sound of the *Zimb* terrifies whole herds. Add to this that the larvæ of the *Œstrus* live in wood, which does not seem to be the case with the *Zimb*, for the country in which it lives is for miles deprived of wood, though it cannot be denied that there may be sufficient underwood to harbour them.

Mr. Kirby, who copies the whole account given by Bruce, considers this fly, both from its habits and figure, to *belong to the Tabanidæ*, and thinks it *congenerous with the Œstrus of the Greeks*. He has, no doubt, adopted this opinion on the authority of Mr. W.S. MacLeay, who, in a learned paper published by the Linnæan Society, has shown, that " the μύωψ and the οἶστρος of Aristotle were insects extremely near to each other in affinity;" and that the former has always been "considered to be the *Tabanus*, and the latter the *Œstrus*; that they both are most inimical to cattle; that the οἶστρος is one of the largest flies, having a strong sting in its mouth, and uttering a particular kind of harsh humming noise, while the μύωψ is like the fly called by the Greeks κυνόμυια; and although it makes a louder hum, it has a smaller sting*." Mr. Kirby, therefore, imagining that the characters of both these insects are united in the *Zimb*, considers it to belong to the genus of both.

But, however true this may be in regard to the *Œstrus*, I do not know how it can be proved in regard to the *Tabanus*. The mouth of this insect is formed into a fleshy proboscis terminated by two lips: it has a rostrum furnished with two pointed palpi, which are very short, and placed on each side of the proboscis. It is marked (at least the *Tabanus bovinus*, which infests the cattle, and in this respect resembles the *Zimb* of Bruce, is marked) down the back by a series of large whitish triangular spots, pointing downwards; and on each side, also, is an approach to a similar appearance, though less distinct, than those of the dorsal row. The body of the *Zimb* is considerably thicker and shorter; has no such spots, nor, indeed, any spots; its lips are very large and very thick; it has no antennæ; and of the three hairs at its snout, one is attached to the superior, and two to the inferior lip: they are very long, and of the same dimension and character, and it would be difficult to say whether all or any of them perform the office of a proboscis, which is not probable; or whether the under alone is grooved like a channel to convey the poison; or whether all of them, merely performing the office of piercers, inflict the wound, into which the poison is afterwards injected from the mouth, for it is quite clear that it does not inflict the wound for the sake of depositing its eggs.

From these facts I may venture to conclude, that the *Zimb* of Bruce is the κυνόμυια of the Greeks, the *af an ouhor* of the ancient Egyptians, an undescribed species of the *Œstrus* of Aristotle, partaking of the different characters both generic and specific, of the *Hippobosca*, the *Bombylius*, the *Tabanus*, and even of the *Pangonia* and the *Nemestrina*, without properly belonging to any of them. It is exclusively confined within the fat black earth round the Nile; and its figure was adopted amongst the hieroglyphics as the symbol of Egypt,

* Mr. W. S. Macleay's paper here referred to will also be found in the Phil. Mag. vol. lxv. p. 43: see further Phil. Mag. and Annals, N.S. vol. iii. p. 283.—Edit.

or perhaps more particularly of Lower Egypt, the place of its residence and ravages.

It is, however, necessary to observe that the insect to which modern entomologists have given the name of *Œstrus*, is totally different from the fly which Aristotle distinguished by that name. The modern *Œstrus* shuns the water, it is no blood-sucker, has no proboscis, and scarcely any mouth, and in flying makes no noise; while the *Œstrus* of the Greeks possesses many of the specific characters and habits which Bruce ascribes to the *Zimb*.

These considerations are submitted to the attention of naturalists with hesitation and doubt. But having had the good fortune of directing the studies of one of the gentlemen whom the Pacha of Egypt has sent to this country to be educated, the writer hopes soon by his means to obtain specimens which will enable naturalists to clear up this interesting and doubtful point.

XXXII. *On the* Gopher-wood *of the received Version of the Scriptures. By* W. G. Carter, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN a letter from Mr. Beke in your Number for August last, on a paper by Mr. Drummond Hay on certain Plants of Marocco, and the Cedar of the Ancients, it is said that the word גֹּפֶר (*Gopher*), used to express the wood of which the Ark of Noah was built, is probably the same as כֹּפֶר; that *Kopher* means pitch, and that the tree in question would therefore appear to be a pitch tree.

This change has, I believe, never been before suggested, but the change of letters of the same organ is common in Hebrew as in other tongues. That it has been made in the particular instance seems highly probable, and the correction very admissible. Admitting, then, that *Gopher* is identical with *Kopher*, I still think it is very questionable that *Kopher* means pitch, and *atzei Kopher*, pitch trees, for the following considerations.

Though the word is of very frequent use in the Hebrew Scriptures in the sense of cover over, atone for, expiate, and is the word constantly employed, particularly in the Pentateuch, to express that meaning, not one passage, I believe, can be found in which it has the former import. The nearest to it, and the only one I can find having the like sense, is Isaiah xxviii. 18. " Your covenant with death shall be disannulled:" and there we need not infer the meaning of cancelled by daubing with pitch, but that of covered from sight, put away, not allowed to be enforced.

The pitch obtained from wood could scarcely have been known at that early age, in a country where, probably, as now, whole pools of it existed, and it flowed spontaneously from the earth, and when boats and ships (for which is its more important use) could be very little known.

Two other words are furnished by the language to express the various kinds of pitch or bitumen then employed, the more general term *hemer* signifying, among other things, red earth, soil, or cement (from חָמַר, *chamar* or *hamar*, to redden), and זֶפֶת (*zepheth*). Sulphur was expressed by גָּפְרִית (*gaphrith*). The two latter words occur in Isaiah xxxiv. 9., and sulphur and pitch or bitumen, are clearly, as expressed, severally intended; "The streams shall be turned into pitch, the *dust* into brimstone." Dense fumes charged with sulphur are sent forth during volcanic eruptions, and it is found in abundance in volcanic deposits. The elder Pliny was killed by such fumes. *Hemer* is in a few passages employed to signify some other tenacious soil or cement. Moses's ark of bulrushes was covered with both *hemer* and *zepheth*; but it was also used to express bitumen. The pits in the vale of Siddim (now the Dead Sea), into which the King of Sodom and others fell, were of *hemer*, and pits of bitumen are still found there (Gen. xiv. 10). The same was used (xi. 3.) in building the Tower of Babel in the plain of Shinar to the east of Babylon, and bitumen is known to have been used in the buildings of Babylon. (Dion. Cass. lib. 68.) The notion of red might be taken from the lighter petroleum, which is of a reddish brown, appearing on the surface.

The Hebrew word which expressed to atone, atonement, expiation, and also the mercy-seat of pure gold over which the Divine glory appeared, were forms of the same word *Kopher*. There is in Hebrew no paucity of terms having the like import of a covering; and that a word should have been selected for these great and revered objects, with the meaning of a sliming over of pitch, seems far from probable.

Trees that we know yield pitch are, moreover, very often referred to in the Hebrew Scriptures. Of fir, בְּרוֹשׁ *verowsh*, or בְּרוֹת *verowth*, (the שׁ, *sh*, being, as in the instance of *Gopher*, changed to another letter of the like organ, ת, *th*,) as well as cedar trees, a very large number were obtained for Solomon's Temple; and that *verowsh* was truly fir wood, appears highly probable from the use made of it. Solomon built the walls and the floor with cedar, אֶרֶז (*erez*), but covered the floor of the house with planks of fir (*verowsh*). 1 Kings vi. 15. We find fir wood the most suitable for musical instruments: *verowsh* also was so employed; "David and all the house of Israel played on all manner of fir wood (*verowsh*) instruments." 2 Sam. vi. 5. In a few passages we have the tree תִּדְהָר (*tidhar*). Isa. xli. 19. "I will set in the desert the fir tree (*verowsh*) and the pine (*tidhar*):" probably affording the etymon of the Latin *teda*, the heart of a pine, a torch.

The cedar, אֶרֶז (*erez*), seems particularly well defined, and is often named in the Old Testament. It was eminently the tree of Lebanon. It was fragrant: "the smell of Lebanon," 4 Cantic. It was a noble lofty tree. Ez. xxxi. "A cedar in Lebanon, with fair branches and with a shadowing shroud, and of an high stature. The fir trees were not like his boughs, nor any tree in the garden of God was like unto him in his beauty." When Maundrell visited Lebanon there were not many left, but enough to identify the tree. A few were of immense size. "There are some of them," says Mr. Buckingham (Travels), "ten or twelve feet diameter in the trunk, with branches of corresponding size, each like a large tree, extending outwards."

Then if it were neither the fir, the pine, nor the cedar, what was the *Kopher* tree? Dr. Hales considers it was the cypress. "Probably," says Taylor on this word in his Hebrew Concordance, "the cypress, a tree with a straight, smooth, long stem, and every way fit for building the ark." There are, however, two varieties of the *Cupressus sempervirens*, and one of them is a spreading tree. But that the cypress was the tree, can we at this remote period expect much better assurance than in the name having been continued from the Hebrews through the Greeks to the present time? *Kopher*, or *kowpher*, (the *p* and *ph* being in Hebrew the same letter,) would be κυπρος, or with *ss* servile, κυπαρισσος, in Greek, whence the Latin *cupressus*, and thence our 'cypress'. And the cypress tree was, as Taylor observes, every way fit for the purpose. The wood has a healthful odour, is extremely hard, yet elastic, it resists the worm, and is considered even superior to cedar. It is a tree of a warm climate, and is met with in China and many parts of Asia, and in the Levant. Thucydides (near 500 years B.C.) mentions (l. 2.) a public funeral at which the bones of the warriors were placed in λαρνακας κυπαρισσινας, chests or coffins of cypress, and the coffins of the Egyptian mummies are generally of this material. The cypress-wood doors of old St. Peter's at Rome, placed there by Constantine, were said to have shown no signs of decay when the brazen ones were substituted by Pope Eugenius IV., a period of 1100 years. Indeed its unequalled durability serves much to identify the tree, for the ark is considered to have been no less than 120 years in building, and if made of a more perishable material, one part would have decayed ere the other were completed.

The *Kopher* is thrice mentioned in the Canticles. In 7th chap. "Come my beloved, let us lodge in the villages" (*bakipharim*), "among the cypress trees" would be more in keeping with the context. In the 1st and 4th chap. where it is called 'camphire,' 'camphire spikenard,' 'a cluster of camphire,' the fragrant shrub and its precious oil mentioned by Pliny (lib. 12. cap. 24.) as *cupros*, and which grew in abundance in Egypt, Cyprus, Ascalon, &c., are understood, and were doubtless intended. There seems to have been no very marked distinction of name between the tree and the shrub. Pliny is certainly treating of the tree in lib. 16. cap. 33., though he refers to Cato De Re Rustica, who noticing the *Cupressus*, mentions the sowing and weeding of it, (cap. 48 & 151). The difficulty, perhaps, may be thus explained. The cypress probably grew in plantations to the common size of a branch, or was trained to shoot out small branches. These were used on funeral occasions, as well at the house entrance as in the procession, at the pile and at the tomb, and were so valuable that a fall of cypress, made once in thirteen years, sufficed for a daughter's portion (*ibid.*).

The same name having been given in both languages to the shrub, and the same or a similar name by the Greeks to the tree, favours the inference that for some reason common to both people, though not manifest to us, the like term was used in each language for the two productions.

I think it results from the foregoing remarks that we should be justified in reading the passage under consideration thus: "Make thee an ark of cypress wood (*Kopher*): thou shalt make rooms in the ark, and with cypress (*Kopher*) shalt thou cover it within and without." A Hebrew critic has lately proposed to render the latter *Kopher* 'atonement'. It will certainly bear at least as well the construction of 'covering' only, and would then be read (though I prefer the former reading), with a slight change of words, but with the same import, "thou shalt make it of cypress wood, and cover it, &c., with a covering," *i. e.* "thou shalt thus completely or surely cover it;" a mode of expression often occurring in the Hebrew Scriptures, as in Isaiah xxii. 17., where it is found in both parts of the verse: "The Lord מְטַלְטֶלְךָ טַלְטֵלָה (*metaltelka taltielah*), will carry thee captive into captivity," *i. e.* will surely carry thee captive, and ועטך עטה (*we-oteka atah*), "cover thee with a covering," *i. e.* surely, or completely, cover thee. And it may be noticed, that a particular command thus to cover the ark, seems to have been more needed than a command to daub it with pitch, for navigation in early society is usually performed in boats made of a single tree. It is after some progress in the art, that men begin to cover within and without with a covering of wood.

I have made the above observations, assenting to the opinion to which many would object, that a reference to the existing productions of the climate in which the ark was built, may not be irrelevant to such an inquiry, and believing it can be satisfactorily shown from the Mosaic history of the Deluge, that, in the opinion of its author, the upper strata of the earth have not undergone those great and universal changes which some attribute to the event, and that the present appearances of the earth's surface are in correspondence with the history.

I am, Gentlemen, yours, &c.

Temple Chambers, Jan. 1834. W. G. Carter.

XXXIII. *Experimental Researches regarding certain Vibrations which take place between Metallic Masses having different Temperatures. By* James D. Forbes, *Esq., F.R.SS. L. & Ed. Professor of Natural Philosophy in the University of Edinburgh.*

[Concluded from p. 28.]

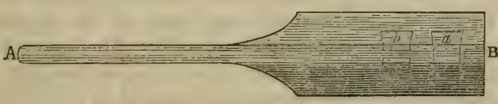
2. *Influence of Figure upon the Vibrations.*

37. I HAVE already noticed the form of the apparatus which I have generally employed. The time of the oscillations and their magnitude depend upon the figure of the vibrating mass, which seems to act just as in the case of a pendulum, or rather of a rocking-stone, the impulse which it receives at each vibration appearing to be given at whatever instant of time the contact of the vibrating edges with the block is effected. This, however, must be understood within certain limits. There must be a decisive interval of time between the two contacts, for if the surface, instead of having two solid angles, as in the bar described above, merges into a continued curve, the vibrations will not take place. If, by any means, however, the period of contact of two portions of the curved surface with the block be prolonged, the impulse will be obtained; as in the case of a silver spoon, used as a bar, where the bowl of the spoon rests upon the block. No vibrations

will take place if the *handle*, which is the other point of support, terminate with a round end: should it, however, terminate with an ornamental device, which affords two points upon which it can rock, the necessary impulse will be gained. We presume, therefore, that *the time of contact of two points of the metals must be longer than that of the intermediate portions.* This condition is readily fulfilled by a vast variety of forms of apparatus, and the rudest masses of metal, such as a poker, when duly heated and placed upon lead, will produce active vibrations. The variations of tone produced upon the apparatus by mechanical interference is easily explained: if a slender rod, with metallic balls at its extremities, be placed across a vibrating bar at right angles to its axis, the time and the arcs of oscillation will be extended, the matter being thrown more to the sides; hence the note will become much lower, and vibrations previously quite insensible will become visible. Again, if while a bar is in a state of active vibration, it be gently pressed from above, the extent of its vibrations will be diminished, and the time will be reduced; hence the note will rise.

38. As it appeared essential to the experiment that the vibration should take place between two points which were longer in contact with the block than the other portions, it seemed important to determine whether the connexion of these points was essential. With a view to determine this, I constructed a bar of lead of the same figure as those which I usually employed, such as A B, fig. 5. I let into it a stud of copper *a*, of which the surface corresponded with that of

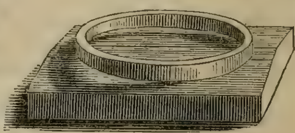
Fig. 5.



the rest of the bar, and similarly two small ones *b c*, forming the two solid angles upon which the bar was to vibrate, but totally distinct from one another. Whether upon the complete bridge *a*, or upon the divided bridge *b c*, the bar, when heated, and placed upon a block of cold lead, vibrated precisely *as if the entire bar had been made of copper.* In an early part of this paper I described an experiment, in which the points of the block upon the bar impinged, were completely distinct pieces of metal: see fig. 3. We therefore conclude that *the impulse is received by a distinct and separate process at each contact of the bar with the block, and that* *in neither case is the connexion of these points in any way essential.*

39. The use of bars made entirely of the different metals is therefore quite unnecessary. A convenient form of experimental apparatus is suggested by the following construction, which I have employed with success. A heated ring of brass or copper, three or four inches in diameter, being placed sideways upon a ridge of solid lead, with two solid angles, upon which the ring may vibrate (the plane of the ring being horizontal), the action will be extremely energetic, the impulse being given simultaneously at two points, as shown in fig. 6. If we had the means of firmly clamping two slips of any metal under experiment to the two points of the ring in contact with the block, by means of tightening screws, so as to substitute the material required for that of which the ring is made, we should have a convenient apparatus, requiring very small pieces of the metals to be tried, and therefore well adapted for experiments on gold, silver, &c.

Fig. 6.

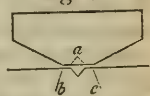


40. The influence of the thickness of the metals employed, and of the extent through which the impulse may be given, early attracted my notice; and I found that thin films of metals of superior conducting power, in the form of leaf, burnished upon the lead block, did not annihilate its characteristic property. The same result in regard to simple gilding was announced by Mr. Trevelyan.

41. We have now to resume the consideration of an important point connected with figure, referred to in an early part of this paper; I mean the *groove* in the bar or block, which frequently appears essential to the production of a musical note. We have already dismissed the supposition that it has any connexion with the passage of air through that groove, and referred the effect solely to the actually observed increase of velocity in the oscillations: it still remains to explain this result. After a very careful consideration of the phænomena, I am disposed to account for it entirely upon the diminution of the surfaces in contact. It may at first sight be thought that the adhesion of two metallic surfaces must be too small to influence sensibly the time of an oscillation: when the enormous velocity of these oscillations is, however, considered, there can be no room for astonishment. We have shown that there are frequently more than *five hundred* contacts and separations in a single second. The most minute

adhesion must therefore clog the energy of the impulse in a way nearly insensible to our ordinary modes of impression: yet cases are not awanting where such adhesion is abundantly sensible, and especially when a metal so soft as lead is one of those employed. It is not difficult to perceive how the position of the groove or separation of pieces (for we have seen that the effect is absolutely independent of the form of the groove, provided the contact of the bar and block for a certain space be avoided) is the most favourable for producing the vibrations. The separation of surfaces may either be in the block, as fig. 7, or in the bar, as indicated by the dotted lines at *a* in the same figure: the surface of contact will thus be reduced, as there shown, to about one half. If, instead of this, the space between the solid angles *b* and *c* had been reduced to one half, the stability of the bar would have been materially changed, and the requisite distance between the *points d'appui* for producing an active vibration would have been deranged. By cutting out the interior space of contact, the other conditions remain unimpaired, and the adhesion is diminished to almost any required extent; in fact, the note has been most clear and steady when the two points of contact of the block had almost the whole intermediate space removed. The sudden changes of note before alluded to have been very satisfactorily accounted for by Mr. Robison, as arising from a sudden movement of the bar, which, by changing its points of bearing, of course alters the velocity of vibration. The rise of tone which is usual towards the end of the experiment, depends on the diminished impulse received at each stroke, and consequent diminution of the arc of vibration.

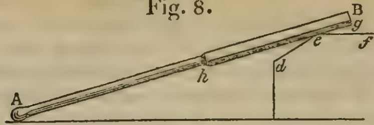
Fig. 7.



42. Indeed the success of the whole experiment depends mainly upon the careful exclusion of adhesion between the two bodies. When merely tried under the most favourable circumstances, as when copper vibrates on lead, the experiment can hardly fail to succeed. With metals less distant from one another on the scale, more delicacy is requisite, and it is then absolutely necessary to avoid any extent of contact in regard to the length as well as breadth of the bar. The form employed originally by Mr. Trevelyan was well adapted for this effect, though the cause seemed not to be attended to: the bar A B (fig. 8.) rested upon an obtuse angle of the lead-block C: had the bar been so inclined as to have touched the whole plane *d e*, in many cases no vibration would have taken place, and I have always been at pains to place the bar so that the angle *g e f* should be nearly equal to *h e d*. If, as

has been sometimes the case, I used a block of hard metal,

Fig. 8.



with an angle much more acute than that shown at *e*, and placed a bar of lead upon it, the effect was less favourable than when the angle was more obtuse, and the contact might seem to be greater. The truth, however, was, that in this case the lead, from its softness, was *cut* by the harder metal, and a new adhesion produced, as in the action of a wedge.

43. These and many other experiments have proved to me, that, to facilitate the vibrations as much as possible, we must have a *minimum of adhesion*; thus their frequency will be increased and the note raised. Mr. Trevelyan states, that if the surfaces in contact of the two metals be highly polished, no vibration will ensue: this manifestly depends upon the same principle, the adhesion between two perfect planes being well known to be great in amount. I have not met with so strong a case in the course of my experiments.

44. We may conclude this head by noticing that the interference of any foreign matter between the metals (with the exception of the metallic pellicles already mentioned) seems fatal to the experiment. Dust, amalgam, a coating of oxide, or even oil-gilding, stops the vibration. The action of mercury is probably by increasing the adhesion.

3. *Influence of Temperature.*

45. We have seen that the metal of greatest conducting power must have the highest temperature in the combination of two required to produce a vibratory motion. Not merely is there no action between two metals when the temperature of both is the same with air of an apartment, but likewise when *both* are raised to any higher temperature, for example, that of boiling water.

46. I have not ascertained what is the smallest difference of temperature requisite to produce the effect. It varies, of course, with every different pair of metals. With lead and copper, for example, the vibrations will continue much longer than with lead and tin, although in the former case the temperatures tend more rapidly to an equilibrium.

47. A difference of temperature of 150° seems to be sufficient for all practical purposes. Being anxious to investigate

the properties of some metals at a definite higher temperature, I heated several bars in a cast-iron vessel full of sand, along with a thermometer, having a very long scale: this vessel was placed in another containing oil, and when the temperature had risen to 350°, the bars were placed upon cold lead. On one occasion I employed copper, brass, iron and antimony; on another iron, tin, platinum and bismuth. I did not find, however, that the additional temperature thus gained facilitated my inquiries, and it was, in the first place attended with considerable practical difficulties. The experiments, however, confirmed a fact which I had previously suspected, and which forms an exception to what may be considered the general law, namely, *that the intensity of vibration is proportional to the difference of temperature of the metals*; I found that at 350° iron was far more sluggish in its vibrations than at 212°. I cannot say that I remarked this in the case of copper, brass, or platinum. The fact, however, hardly admits of doubt. At an early period I had been much perplexed with some anomalies in the vibration of iron. When first taken out of a hot open fire, and just cool enough not to melt lead, its action with that metal appeared very unsatisfactory. This effect was so sensible, that I have frequently repeated with success a singularly paradoxical experiment. A bar of iron heated, suppose to 212°, being placed on a lead block, and the vibrations commenced, if a spirit-lamp was applied to the lower portion of the bar, the vibrations *are completely stopped*, and may actually be restored by immersing the lead, to which the lamp had been applied, in cold water: these singular effects I have been able to produce several times in succession during one experiment.

48. The same effects, though less striking, have been produced with zinc instead of iron, which vibrates with considerable difficulty when the temperature is raised above 212°. I have been disposed to consider that every metal has its own most favourable temperature, though on what principle it is not so easy to explain.

49. It is probable that the softening of the heated metal diminishes the resiliency of the two bodies when impact takes place. I do not think that it is attributable to the softening of the lead, for I have found that iron is more disposed to vibrate on platinum when at a moderate temperature, than when red hot. The effect may, however, be connected with the theory of the vibration.

50. Having now discussed the phænomena of sound, and of the vibrations to which we have shown these sounds to be referrible, we shall next consider

III. *The Theory of the Phænomena.*

51. It is a curious fact how imperfectly the interest attached to the phænomena observed by Mr. Trevelyan, seems to have excited enlightened curiosity. Indeed, an explanation of great simplicity, and which appeared to account for the more conspicuous phænomena, was pretty generally acquiesced in, and seems to have acted as a barrier to further examination. It was, I believe, first thrown out by Sir John Leslie, on considering the simple facts which were brought to light by Mr. Trevelyan's experiments, that they might be explained by the expansion of the cold metal at the instant of contact with the warm one, which might be supposed to give a sufficient impulse for sustaining a new vibration. Even at first sight it does appear very difficult to conceive how, when the vibrations are increased to 500 or more in a second, a process depending upon so slow an operation as the conduction of heat, should cause the metal to expand and contract successively by a finite quantity. The effect has every appearance of being one of active and almost instantaneous repulsion, and bears no resemblance whatever to the slow mechanical elevation of the surface by the process of expansion. But such inferences are often erroneous; it became, therefore, most important to inquire how far the hypothesis was applicable to various forms of the experiment, particularly to the different properties in this respect of various substances.

52. This more difficult task was undertaken by Mr. Faraday; and in a lecture on the subject which I was fortunate enough to hear at the Royal Institution in April 1831, he freed the subject, (as we have already seen,) from many of the difficulties with which it had been surrounded, and illustrated the theory which he supported in that happy style for which he is so remarkable.

53. The principle which he adopted was fundamentally the same as that of Sir John Leslie, but he added an explanation of the influence of the properties of different metals upon the phænomena. According to his view, the *hot* metals should have a higher conducting power, and a smaller expansion by heat, than the *cold* one, and the arrangements of the metals as vibrators depend, according to him, upon this principle. To employ the official statement of his views contained in the Royal Institution Journal *, " the superiority of lead, as the cold metal, was referred to its great expansive force by heat, combined with its deficient conducting power, which is not a fifth of that of copper, silver, or gold; so that the heat ac-

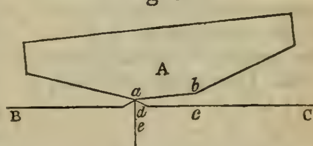
* New Series, No. IV. pp. 119, 122.

cumulates much more at the point of contact in it, than it could do in the latter metals, and produces an expansion in that respect proportionally greater."

54. I certainly approach with deference any opinion expressed by a philosopher of the reputation and acuteness of Mr. Faraday, and nothing but a strong conviction, entertained chiefly upon the *general* grounds already alluded to, could have induced me to spend my time in an investigation which he considered decided upon some of the simplest principles of physics. My dissatisfaction with the explanation increased the more I thought of it, and the more closely I analysed the natural process which he had traced out. I consider it essential to point out on what grounds I dissented from a theory supported by two of the first names in British science, before I proceed to give any opinion of my own, which may perhaps be liable to equally strong objections, but the data of which are not the less valuable as physical facts.

55. Waving all minor objections, I conceive that the process of the communication of heat, and consequently its effects, would be very different from what has been stated in the passage just quoted. Let fig. 9. represent on an exaggerated scale the presumed state of the apparatus in the middle of an oscillation: the hot bar A, whilst performing its vibration upon one of the solid angles *a*, has expanded a portion of the cold block BC into a hillock at *d*: when the semi-vibration is completed, the angle *b* of the bar will touch the block, and raise a new hillock at the corresponding point *c*, whilst the elevation at *d* subsides; and so on alternately. Let us conceive that *d e* is the finite depth to which heat is communicated in the minute portion of time occupied by a semi-vibration, a depth so small as to be inappreciable by the senses, and insignificant compared to the distances between the points of impact *d c*. The elevation or height of the hillock *d a* is the amount of expansion of the element *d e*, by the accession of temperature received during a semi-vibration: the question is, what relation will this expansion, or acquired vantage-ground for the commencement of a new vibration, bear to the nature of the block BC, considering the nature and temperature of the bar A, and the initial temperature of the block, to be constant? It surely requires no elaborate demonstration to prove that the amount of caloric which passes into the block

Fig. 9.



must increase with the conducting power of the material. Upon the very fundamental axioms of the theory of heat, the amount of caloric which passes from a molecule A into a molecule B, in an infinitely short interval of time, is proportional to the difference of the temperatures of the molecules* combined with the conducting power of B (that of A being considered constant), and with the element of the time. Or, putting the temperature of A $= \alpha$, that of B $= \beta$, and its conducting power $= K$, and the element of time $= dt$, the proportion of caloric transmitted will be

$$(\alpha - \beta) . \text{K} . dt.$$

56. It appears to me indubitable, that whether the time be short or long, the quantity of caloric transmitted, and the consequent amount of expansion, must increase with K. The idea of an accumulation of heat at the surface producing more effect than a rapid communication with the interior, is obviously an oversight. For if the heat be accumulated at the surface, the temperature of that surface, rapidly approaching to that of the source of heat A, will, in the same ratio, diminish the amount of heat received; and it can require no demonstration to prove, that the expansion depends solely upon the amount of temperature acquired above its initial temperature by the prism of metal, which by its expansion is to raise the bar from d to a, (fig. 9.) modified of course by the amount of expansion proper to any substance employed †. Consequently this amount, or $d\,a$, will be proportional to

$$\frac{\text{E}}{c} (\alpha - \beta) . \text{K} . dt,$$

E being the measure of expansibility, c the capacity for heat of the substance, taken by *volume* not by *weight*.

* For such small differences of temperature the Newtonian law may be viewed as absolutely accurate.

† In fact, let A B represent the surface of the body, receiving heat at the point a; and let $a\,b$ be a line normal to the surface, consequently the expansion of which is to produce the elevation at the point a. The ordinates of the curve $c\,d$ may represent the acquired temperatures, and the total acquired temperature will be denoted by the area of the curve, to which likewise the expansion will be proportional without regard to its particular form, (the distribution of heat,) which will vary with the conducting power; and although it is very possible that the ordinate $a\,c$ of the curve may be greatest in a bad conductor, it is very easy to see that the total area never can.

57. Hence it appears to be quite obvious, that as far as conducting power is concerned, *both bar and block* should have it in the highest possible degree. It would be quite essential, too, upon this explanation, that the cold metal should expand more than the hot one, otherwise the loss of elevation by the contraction of the warm metal will equal or exceed the vantage-ground for the new vibration gained by the expansion of the cold one. By both these criteria Mr. Faraday's theory seems to be deficient: I need only point out the position of zinc, which, with greater expansibility than lead or tin, occupies so high a place in the list of vibrators, and cannot be used as the cold metal with any other except silver: according to the theory, zinc ought to vibrate far better upon zinc than upon lead or tin. Silver, again, vibrates upon cold iron, although its expansibility is a half greater. Such facts as these seem absolutely unaccountable upon the hypothesis of expansion.

58. The objections which I took *in limine* to the explanation of Sir John Leslie and Mr. Faraday, (which was adopted by Mr. Trevelyan in the paper printed in the Edinburgh Transactions,) were strengthened, and I may say rendered decisive, by my subsequent experiments, the results of which have been detailed in a previous part of this paper. For nearly two years I have been constantly expecting to see some systematic examination of these curious facts; but the public seems to have rested satisfied with the ascription of them to a simple and acknowledged effect of heat. They have hardly been noticed in the Journals, and foreigners complain of the few data afforded by English works on the subject. An article by Professor Müncke of Heidelberg, in *Poggendorff's Annalen**, consists chiefly of the translation of a very brief notice, which I had published in the Edinburgh Journal of Science†, but contains no new observations. Having shown the reasons which led me to dissent from the opinions at first proposed, I shall now explain the views which I have been led to entertain from the study of the phænomena.

59. I shall first recapitulate the general laws at which we have arrived.

1st, The vibrations never take place between substances of the same nature. Art. 13.

2nd, Both substances must be metallic. Art. 14.

3rd, The vibrations take place with an intensity proportional (within certain limits) to the difference of the conducting powers of the metals for heat (or electricity), the metal having the least conducting power being necessarily the coldest. Art. 36.

* 1832, No. III., p. 466. † New Series, No. XI.

4th, The time of contact of two points of the metals must be longer than that of the intermediate portions. Art. 37.

5th, The impulse is received by a distinct and separate process at each contact of the bar with the block, and in no case is the metallic connexion of the bearing points in the bar, or those of the block, in any way essential. Art. 38.

6th, The intensity of the vibration is (under certain exceptions) proportional to the difference of temperature of the metals. Art. 47.

60. In order to satisfy these various conditions, we shall find that the range of hypotheses is not great. During my experiments I was for a long time attached to the idea of a thermo-electric action. The hypotheses which I assumed to explain the steps of it I was forced successively to abandon, and the total want of connexion of the order of the metals as vibrators with their thermo-electric properties (and especially the absolute inertness of antimony and bismuth), convinced me, after a long series of experiments, undertaken with this view, that I was wrong.

61. The strict and simple connexion with the conducting powers of the metals for heat and electricity afforded a firm basis for speculation, and I was soon forced to consider heat as the sole agent in the case, all idea of electricity being necessarily abandoned, as soon as it was established that *thermo*-electricity had no share in the action. The general laws above quoted seem to be all resolvable into this, " That there is a repulsive action exercised in the transmission of heat from one body into another, which has a less power of conducting it." These repulsions only take place between bodies having a certain amount of conducting power, below which some metals fall; it must be excitable in a most minute space of time; and is energetic in proportion to the difference of conducting power of the substances, and to their difference of temperature.

62. It seems most probable, therefore, that the repulsive action alluded to, depends on the internal motions of heat itself. It were easy to frame a hypothesis which would be sufficiently plausible, and represent the phænomena. I forbear, however, from doing it at present, because our ignorance of the internal constitution of bodies, and the mechanical process of the conduction of bodies, is such as to render hypothetical reasoning upon such data almost useless. That repulsion does exist in the case of heat can hardly admit of a doubt. The reason that we cannot render it visible in ordinary cases, is no doubt that the repulsion of the heat in two approximate molecules of bodies is to small too be weighed in our balances.

Consequently, two bodies equally heated and placed together, manifest no sensible repulsion. In such a case every portion of heat is kept in equilibrium by the equal and opposite repulsions of the molecules on each side of it, which is the case when heat is uniformly diffused through a body, and which is manifested by that universal tendency to diffusion. Hence the element of heat is in a state of equilibrium, and the only force which could be excited successfully to produce a separation, would be between the heat residing in the *last* molecule of one body, and the *first* of a separate one in contact with it, but not bound to it by cohesive attraction. Suppose, however, this second or free body cooler than the other, a current of heat will be immediately created, which, as it is more or less easily received by the cold body than parted with by the hot, will create a stagnation, or a rarefaction of the elements of heat, respectively; in the former case producing a repulsive action, or recoil through the whole string of elements set in motion; in the latter we are led to anticipate that the action would be attractive. If this view be correct, (and being theoretical, I do not attach great importance to it,) it is easy to see why repulsion takes place only when the cool body has less conducting power than the hot, and why the repulsive energy depends on the difference of these conducting powers. In the case of very bad conductors, such as antimony and bismuth, I conceive that the current has not had time to establish itself.

63. In the case of electricity, a remarkable similarity of effect is observable, depending on the conducting power of the material through which it passes. All those remarkable repulsive actions which produce destructive effects in the case of lightning, take place during the accumulation of impulses in bad conductors*.

64. I have been led to entertain the idea of a new species of mechanical agency in heat, not from a love of introducing novel principles, but after having been driven by experiment from the hypotheses to which I was at first entirely attached. Although the mechanical effect of the repulsive power of heat cannot be said ever to have been demonstrated, experiments are not wanting which seem to be quite inexplicable without its aid, or some other principle not yet recognised in science.

65. Several ingenious French experimentalists have fur-

* I might point out another analogy in the sudden and forcible action of the hydraulic ram, where the accumulated effect of small impulses produces sudden and intense results, but I am afraid of extending unwarrantably such speculative analogies. The two preceding paragraphs of this paper have been somewhat modified since it was first read.

nished us with facts, which, though not completely established as belonging to any peculiar class of phænomena, and therefore not generally admitted into systematic works, are not the less worthy of notice. Those which bear most directly on our present speculation were observed by M. Fresnel*; namely, the repulsion of disks of mica, of which one was placed at the extremity of a delicately suspended needle *in vacuo*, and when the disks were in contact, heated by means of a ray of solar light concentrated by a lens. M. Saigey† has also described a class of similar phænomena observed by himself, with a considerable number of metals, which after rejecting the influence of aerial currents, of electricity, magnetism, &c., he ascribes to the repulsive action of radiant heat at sensible distances. There are several other experiments on record which seem to require a similar explanation, but I apprehend that the present are the first to establish the existence of some species of mechanical repulsion in the propagation of heat, a principle which can hardly fail to be applicable to the explanation of many natural phænomena.

Greenhill, Edinburgh, Feb. 19, 1833.

XXXIV. *Reply to* Mr. Earnshaw's *Remarks on the "Principle of Least Pressure."* *By the Rev.* H. Moseley, *B.A., Professor of Natural Philosophy in King's College, London.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN submitting to the consideration of Mr. Earnshaw the following answer to his remarks on my paper purporting to contain the demonstration of a new statical principle, which I have called that of least pressure; I beg to suggest to that gentleman that the obscurity of which he complains is perhaps in some respect attributable to the nature of the subject under discussion. The theory of statical resistances has always been considered among the most difficult and complicated of those subjects which lie within the range of exact science, yielding itself most unwillingly and ungraciously to the control of analysis, and such, in its nature, as *cannot be submitted to the test of experiment.*

It is nevertheless a subject entering, more or less, into the complete discussion of almost every question‡ that can occur

* *Annales de Chimie et de Physique*, xxix. 57. and 107.

† See several successive articles in the *Bulletin des Sciences Mathématiques*, tom. ix. See also Pouillet, *Elémens de Physique.*

‡ There is practically scarcely any case of equilibrium, among the forces

in the practice of statics; and one which, although it has never yet received a complete and satisfactory discussion, yet lies on the very *threshold* of the science of mechanics.

The demonstration which I have given of the principle of least pressure is shortly this.

"Let there be conceived a system of forces of which a certain number are *given* in magnitude and direction, and the rest are supplied by the *resistances* of as many fixed points. Also let the *points of application* of the forces of the system be *supposed* to be given.

"Let A designate the *given* forces of the system, B the resistances, and C any other system of forces which, being applied to the same points with the forces of the system B, would maintain the equilibrium. Also let the system C be supposed to replace the system B.

"Now each force of the system C, under these circumstances, *just* sustains and is equivalent to the pressure propagated to its point of application by the forces of the system A; or it is equivalent to that pressure, *together with* the pressure propagated to its point of application by the *other* forces of the system C.

"In the former case it is identical with the corresponding *resistance* of the system B. In the latter case it is *greater* than it.

"The sum of the forces of the system B, each being considered *a function of the coordinates of its point of application*, is, therefore, a MINIMUM."

Mr. Earnshaw's first remark upon this is, that, "speaking generally, it is impossible that *each* force of the system C shall just sustain *only* such pressures as are propagated to it by the system A; unless either that the system C consists of but *one* such, or that all the forces of which it consists are parallel. For if there be more forces than one in the system C, and if they are not parallel, their actions *must* of necessity mutually propagate pressures. Wherefore," &c.

Now to this objection I have the following answer to make. Each force of C sustaining as well the pressure propagated from the system A, as that arising out of the mutual action of the forces of its own system, let it be resolved into two others, one equal and opposite to the former pressure, and the other to the latter. Call C′ the system of forces thus equal and opposite to the pressures propagated from A, and

composing which, there do not enter two or more resistances. The *general* solution of that case cannot be effected by the known conditions of equilibrium.

C$''$ those equal and opposite to the pressures mutually propagated in C.

Now it is manifest that if the system C$''$ be withdrawn, the equilibrium *will remain.* And yet the forces of the system C$'$ mutually propagate *no pressure*; and are equivalent, severally, only to the pressures propagated to their points of application from the forces of the system A. It is therefore possible, &c. &c.

Were this *not*, as I hold it to be, a *demonstration* that the forces of the system C *may*, under any conceivable circumstances, sustain those of A, and yet mutually propagate no pressures, yet I should decidedly object to Mr. Earnshaw's position that any portion of a system of forces not parallel must of *necessity mutually* propagate pressures.

Suppose we apply to a solid body any number of equal and opposite forces; these may certainly have directions however oblique to one another, and yet not mutually propagate pressures. Or, to take a case bearing more immediately upon the point at issue, suppose that to the different points of application of the system A, we apply forces having for their resultant *one* of the forces C, and then to the same points forces having for their resultant *another* of the forces of the system C, and so on until we have thus obtained all the forces of the system C. Now, under these circumstances, the systems of forces concerned will have become precisely analogous to the systems A and C; but by the principle of the *superposition of forces*, it is manifest that no pressures will be mutually propagated among the forces of C.

Mr. Earnshaw admits that he has urged this objection without much confidence in its validity. The explanations given above will, I think, induce him to yield to me *this much* of my demonstration.

And here I would call his attention to the fact, that this admitted, *the principle* for which I contend is established. It follows rigidly that each pressure is a minimum subject to the conditions imposed by the equilibrium of the whole. What remains has reference, not to the *demonstration* of the principle of least pressure, but to the *expression* of its conditions in the language of analysis, and to its *application.*

This remark bears especially upon Mr. Earnshaw's next objection. He states that he is "unable to comprehend why we are to consider the forces as functions of the coordinates of the points at which they are applied."

My answer to this is, that there appears to me the same reason for fixing upon these as the variables upon which those functions depend, as for assigning the variables to any other function or functions on the determination of which a question of physics may be made to depend.

The resultant of the resistances must be equal and opposite to the resultant of the other forces of the system. Now suppose that, by the variation of these last forces, the position of their resultant is altered, the position of the resultant of the resistances will then be altered; but the position of this resultant cannot be altered unless the resistances themselves be altered; the resistances, then, vary with any variation in their position in respect to the resultant of the other forces of the system: they are, therefore, functions of their positions in relation to that resultant.

Or we may reason thus. We must admit the resistances upon different points not symmetrically situated with respect to the given forces to be *different*, otherwise the resultant of those resistances would have a position *determined* by the positions of the points, and independent of, and therefore not opposite to, the resultant of the other forces of the system.

Also, the resistances being different from one another, that difference must arise from some cause. Now the nature of the resisting surfaces being supposed to be everywhere the same, or if it be different, being supposed a function of the coordinates of the surface, there is no other cause of difference in the resistances than that resulting from the different positions of the points of resistance.

Taking, for instance, any one point of resistance; as long as the coordinates of that point and those of all the other points remain the *same*, we cannot understand how the pressure upon it should vary; but if we cause the coordinates of that point to *vary*, we can as little understand how the resistance should remain the same: in fact, it *cannot* manifestly remain the same; for if it were, the resultant of the resistances would cease to have its direction opposite to that of the other forces of the system.

Thus, then, the resistance upon any point remains the same so long as the coordinates of that point remain the same, and it varies when they vary; it is therefore *a function* of those coordinates.

It appears to me that this proof is precisely of the same kind with that upon which it is *usual* to assume one variable to be a function of certain others; the proof, for instance, on which we ground the assumption, that if two forces act upon a point, their resultant is a function of the magnitudes of these forces and of the included angle.

Mr. Earnshaw's next remark has reference to the case of a mass supported upon three points, forming a triangle, whose centre of gravity is in the vertical passing through the centre of gravity of the mass. In this case the pressure will be equally divided between the points; and this equal division will continue whatever form we suppose the triangle to assume, provided only the centre of gravity of the mass alter its position so as continually to satisfy the condition that the vertical through it passes always through the centre of gravity of the triangle. Hence, therefore, it is concluded, as it appears to me, rigidly enough, that this equal division obtains also in the *limit* when one point passes into the line joining the other two. Mr. Earnshaw thus argues in opposition to this conclusion: "Let A, B, C be the angular points of the triangle in its *finite* state; then if we inquire into the cause of the pressure on any one of them (suppose C), we shall find that it arises from the circumstance that the side A B is in a certain respect a *fixed axis*, about which the body has a *tendency* to move, and about which it is only prevented from moving by the pressure of the prop at C: as soon, therefore, as the points of support are brought into one line, A B, in which of course, from the nature of the hypothesis, the centre of gravity is situated, the body *ceases* to have a tendency to move round A B, and the office of C is abolished, and the case of the triangle is not applicable to this."

Now, *assuming* Mr. Earnshaw's definition of the *office* of the point C, and calling K_1 and K_2 the perpendiculars from the centre of gravity and from C, upon A B, it follows that

$$\frac{\text{pressure upon C}}{\text{weight of mass}} = \frac{K_1}{K_2}.$$

If now the centre of gravity and the point C pass both into the line A B, K_1 and K_2, both of them vanish, and

$$\frac{\text{pressure upon C}}{\text{weight of mass}} = \frac{0}{0}.$$

It matters not whether we suppose one of the quantities K_1 and K_2 to vanish first, and *then* the other, or suppose them both, as above, to vanish together; the fraction, *in either case*, assumes the indefinite form $\frac{0}{0}$.

Mr. Earnshaw puts this idea under another form. "Let the body be supported upon three props A, B, C, of which A, B are so situated that the line joining them passes through the centre of gravity, and C is situated without. Then the body will balance upon A B, and there will be *no pressure* upon C; for if C exerted any pressure, it would overthrow the body by turning it round A B. Let now C come into the line A B, and have the precise position assigned to it by Mr. Moseley in the instance mentioned by him, and (if we may be allowed to stretch the reasoning of a finite state to an evanescent state,) there will still be no pressure upon C; which is directly at variance with Mr. Moseley's results."

The answer to this argument is analogous to the preceding. Let G be the centre of gravity of the triangle, then

$$\frac{\text{pressure upon C}}{\text{weight of mass}} = \frac{\triangle\, \text{A G B}}{\triangle\, \text{A C B}}.$$

Now when G is in the line A B, $\triangle$ A G B is evanescent;

$$\therefore \frac{\text{pressure upon C}}{\text{weight of mass}} = \frac{0}{\triangle\, \text{A C B}}.$$

In this case, therefore, as Mr. Earnshaw observes, there will be no pressure upon C, *provided only* the $\triangle$ A C B have a *finite* value. But if we give to this triangle *also* an evanescent value, the conditions of the case will be altered, and we shall have

$$\frac{\text{pressure upon C}}{\text{weight of mass}} = \frac{0}{0};$$

from which no conclusion can be drawn, unless we know the evanescent value of the fraction $\frac{\triangle\, \text{A C B}}{\triangle\, \text{A G B}}$. Now supposing the point G to take up the position which I have assigned it, the evanescent value of this fraction is $\frac{1}{3}$.

The evanescence of the pressure upon C arises out of a particular application of the general principle of the equality of moments; it is not therefore *absolutely*, but only *relatively* evanescent; a fact which Mr. Earnshaw does not appear to have considered.

I have stated in one of my former papers that the result which I have obtained in the case of parallel forces is verified by the known condition of the question when the points of support are *two* only. It has occurred to me to seek for a similar verification in the case which I have just been discussing of a pressure equally divided between *three* points of support in the same right line.

As above, let C and G come into the line A B,

Let D be the bisection of A B. Then D G $= \frac{1}{3}$ D C.

$$\therefore \text{D G} = \tfrac{1}{3}(\text{A C} - \tfrac{1}{2}\text{A B})$$

$$\therefore \text{A G} = \tfrac{1}{3}\text{A C} - \tfrac{1}{6}\text{A B} + \tfrac{1}{2}\text{A B}$$

$$\therefore 3\,\text{A G} = \text{A C} + \text{A B}.$$

Let $\text{A G} = a$

$\text{A B} = b$ M = weight of mass

$\text{A C} = c$

Let also X be the point about which the moments of the pressures upon A, B, C are equal. Let these pressures be represented by A, B, C; also let α represent a constant quantity, and let $\text{A X} = x$:

$$\therefore \text{A} = \frac{\alpha}{x}$$

$$\text{B} = \frac{\alpha}{x-b}$$

$$\text{C} = \frac{\alpha}{x-c}$$

The two equations of equilibrium are, therefore,

$$\left.\begin{aligned} \frac{\alpha}{x} + \frac{\alpha}{x-b} + \frac{\alpha}{x-c} &= \text{M} \\ \frac{\alpha b}{x-b} + \frac{\alpha c}{x-c} &= \text{M}\,a \end{aligned}\right\}$$

$$\left.\begin{aligned} \therefore \alpha\,\frac{(x-b)\,(x-c) + x\,(x-c) + x\,(x-b)}{x\,(x-b)\,(x-c)} &= \text{M} \\ \alpha\,\frac{b\,(x-c) + c\,(x-b)}{(x-b)\,(x-c)} &= \text{M}\,a \end{aligned}\right\}$$

$$\left.\begin{aligned} \therefore \alpha\,\frac{3\,x^2 - 2\,x(b+c) + b\,c}{x\,(x-b)\,(x-c)} &= \text{M} \\ \alpha\,\frac{x\,(b+c) - 2\,b\,c}{(x-b)\,(x-c)} &= \text{M}\,a \end{aligned}\right\}$$

$$\therefore \frac{3\,x^2 - 2\,x\,(b+c) + b\,c}{x\{x\,(b+c) - 2\,b\,c\}} = \frac{1}{a}$$

$$\therefore 3\,a\,x^2 - 2\,a\,x\,(b+c) + a\,b\,c = x^2(b+c) - 2\,b\,c\,x$$

$$\therefore x^2(3\,a-b-c) - 2\,x(a\,b+a\,c-b\,c) = -a\,b\,c$$

$$\therefore x = \frac{a\,b+a\,c-b\,c}{3\,a-b-c} \pm \sqrt{\left(\frac{a\,b+a\,c-b\,c}{3\,a-b-c}\right)^2 - \frac{a\,b\,c}{3\,a-b-c}}.$$

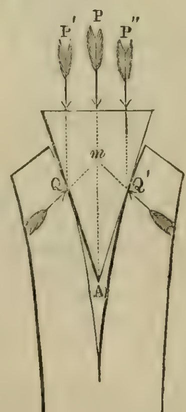
But in the case we have supposed

$$3a = b + c$$
$$\therefore\ 3a - b - c = 0$$
$$\therefore\ x = \text{infinity}.$$

Hence, therefore, it follows that the point X, about which the *moments* of the pressures are equal, is at an infinite distance. The pressures *themselves*, therefore, are *equal*, as they should be.

It will be observed that there are here obtained precisely the same results, in a somewhat complicated case of resistance, by methods of investigation which are throughout entirely different; and I beg to suggest to Mr. Earnshaw, that he must allow to both methods of investigation, that authority and confirmation which they mutually derive from this coincidence.

The theory of the WEDGE presents an application of the principle of least pressure under its simplest form, leading to a result which is of some practical importance, and, as I believe, altogether *new*.



Let P be the force acting upon the back of the wedge, Q and Q′ the *resistances* upon its sides. Now by the principle of least pressure Q and Q′ should be the least possible subject to the condition that their resultant shall be P. It is manifest that to satisfy this condition these forces must have a direction *parallel* to the direction of P, or one inclined *as little as possible* to that direction.

If, therefore, the surfaces in contact at Q and Q′ are such as are capable of supplying resistances at those points *parallel* to P, then the system will be one of *parallel* forces, and the points Q and Q′ being similarly situated with respect to PA, each will sustain one half of the force P. But if, by reason of the *nature* of the surfaces in contact at Q and Q′, these be incapable of supplying resistance in directions parallel to PA*, then will the

* The following is a principle of statics, of great practical importance, from which the possibility of this supposition will be evident. Let p be a force pressing two *surfaces* together *obliquely*, and let θ be its inclination to the normal at the point of contact; then $p \sin \theta$ and $p \cos \theta$ are the resolved parts of p, parallel and perpendicular to the surfaces at their common

directions of Q and Q′ be those which the surfaces will supply *nearest* to the direction of PA.

Now, as is shown in the note, there is a certain direction between which and the normal at either point, if any force be applied, the surfaces will supply a resistance opposite to that force, but if the force be applied further from the normal than this direction, then *no* resistance will be afforded by the surfaces in an *opposite* direction. The angle which this direction makes with the normal may be called the *limiting angle of resistance.* The resistances Q and Q′ will manifestly have their directions inclined to PA at the *least possible* angles, when they are actually in the directions spoken of above, and make each with the normal at its point of application an angle equal to the limiting angle of resistance. Such, then, by the principle of least pressure, are the actual directions of the pressures at Q and Q′.

Now let us consider what are the conditions of the equilibrium resulting from this conclusion.

Let α = the limiting angle of resistance,

2ι = the angle A of the wedge.

The angle which Q makes with the side of the wedge is

$$\frac{\pi}{2} - \alpha.$$

Hence, therefore, the angle Q*m* A, which it makes with PA, is

$$\frac{\pi}{2} - \alpha - \iota.$$

Hence, therefore, the resolved part of Q in the direction PA is

$$Q \cdot \sin(\alpha + \iota),$$

and the wedge being symmetrical about PA, the resolved part of Q′ is the same. Hence

$$2\,Q \sin(\alpha+\iota) = P;$$

$$\therefore\ Q = \frac{P}{2\sin(\alpha+\iota)}.$$

point of contact. Let the coefficient of friction equal the tangent of a certain angle, which call α. Therefore the actual amount of the friction, being the product of the *perpendicular* pressure by the coefficient of friction, is represented by $p \cos\theta \tan\alpha$. Now this power of resistance acts in a direction *parallel* to the surfaces at their point of contact, and in a direction opposite to the horizontal part of the force p, which is $p \sin\theta$. Hence, therefore, there will be an equilibrium or not, according as

$p \sin\theta$ is, or is not, less than $p \cos\theta \tan\alpha$,

or according as $\tan\theta$ ——————— $\tan\alpha$,

The surfaces will, therefore, not supply a resistance whose direction is inclined to the normal at an angle θ, greater than that angle α whose tangent is the coefficient of friction.

If $$\alpha + \iota = \frac{\pi}{2},$$

$$Q = \tfrac{1}{2} P.$$

This is the case spoken of before, in which the directions of Q and Q′ are parallel.

Now the above results may be arrived at by another and an entirely independent process of reasoning.

Let P′ and P″ each equal one half of P, and let them be applied immediately above the points Q and Q′; they may then be made to replace P without in the least altering the circumstances of the equilibrium. Now if the direction of P′Q be *within* the limits of the resistance of the surfaces at Q, the pressure P′ will be *wholly* sustained by that resistance, and the direction of the force Q will be in the same straight line with P′Q; the wedge sustaining no pressure whatever laterally or in a direction perpendicular to P A. But if the direction of P′Q be *without* the limits of resistance at Q, then some other force must be supplied at Q, in order to maintain the equilibrium. That force can only result from the action of the force P″ at Q′. It acts, therefore, in the line Q′ Q, and therefore in a direction perpendicular to P A. Also, this force, resulting from the tendency of the wedge to motion on the point Q″, is only just equal to that tendency, or in other words, it is equal to the least force which would keep that point at rest. Since, then, it is equal to the least force which would keep the point Q′ at rest, it is also equal to the least force which would keep the point Q at rest: now the least force which would keep Q at rest is manifestly that which will bring the direction of the resistance at Q just within the limiting angle of resistance at that point. Thus, then, it appears that the directions of Q and Q′ are inclined to the normals at those points at angles each of them equal to the limiting angle of resistance. This is precisely the result which is given us at once, by the principle of least pressure.

There are numerous applications of the principle of least pressure in the theory of statics which admit of a verification similar to the above. Take, for instance, the theory of the arch. The pressures upon the surfaces of the abutment and keystone should, by that principle, be each a minimum, subject to the condition that they should be sufficient to sustain the semiarch, if it formed one continuous solid, and that the pressure on the key should be *horizontal*. Now the weight of the semiarch being given, as the pressure upon the key diminishes, that upon the abutment also diminishes. Also, the pressure upon the key tending to support either semiarch results from the tendency of the opposite semiarch to motion, and is just equal to that tendency. It is therefore equal to the *least force* which would support that semiarch, and therefore to the least force which would support the first-mentioned semiarch; or it is a minimum subject to the conditions, and therefore the pressure upon the abutment is also a minimum. And the positions of the resultants of the pressures upon the different points of the key and abutments, as well as the magnitudes of those resultants, are subject to this law*.

Here, then, we have verified another application of the principle of least pressure, and it is shown to constitute a most important element in the true theory of the arch, determining the position of its line of pressure, and thence all the conditions of its equilibrium.

I have now answered the objections of Mr. Earnshaw at considerable length; and I beg in conclusion to assure him of the respect which I feel to be due to any observations which he may think fit to make on subjects connected with the science of mechanics, and of the pleasure which I shall have in answering any other inquiries which may suggest themselves to him on the subject at issue between us. The question of statical resistance is *beset* with difficulties. Those which Mr. Earnshaw has suggested are by no means the only, nor even the greatest difficulties which have occurred to me, and which I have carefully considered. It will, however, give me great pleasure to know that, so far as these are concerned, the explanations I have given, appear to that gentleman satisfactory. In the mean time I beg leave to state, that it is my intention on some early occasion (should my other occupations permit it,) to *resume* the discussion of the subject, and to endeavour to apply the theory of *statical resistances* to some of that *vast variety* of practical questions which depend upon it.

I have the honour to be,
Gentlemen, yours, &c.
H. Moseley.

King's College, London,
February 8, 1834.

* It is evident, that an infinite number of different forces might be applied at an infinite number of different points of the keystone, each of which would support the semiarch; of these one only is that which will *just* sustain it. I have discussed this subject fully in a paper read before the Cambridge Philosophical Society during the last term.

XXXV. *A Catalogue of Comets. By the Rev.* T. J. Hussey, *A.M., Rector of Hayes, Kent.*

[Continued from p. 30.]

[The Chronology employed is that of Petau or Petavius.]

A, the comet of 1680. B, that of 1652. C (Halley's), that of 1682. D, that of 1759. E, that of 1661. F, that of 1677. G, that of 1556. H, that of 1665. I, that of 1585.

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
278	1066	F? H?	April, May.....	Pegasus, Ursa Maj., Bootes	Chin. Rec., Matt. Par. &c.&c. ...	Elements computed by Pingré*.
279	1067	...	May	......	Chron. Andegav. Protosp.	
280	1071	...	......	......	Curop. Cæs. Alst.	
281	1075	...	November.....	Corvus, Taurus	Chinese Records.	
282	1080	...	July, August...	Coma Beren., Leo, Taurus	Chinese Records.	
283	—	C	August, Sept..	Hydra	Chinese Records.	Seen from Aug. 27 to Sep. 14.
284	1096	...	October........	To the South	Annalista Saxo.	
285	1097	...	October........	Libra, Hercules	Ch. Rec., Sigeb. Chron., &c. &c.	Elements computed by Burckhardt*.
286	1098	...	June............	......	Robert. Hist. Hieros. &c.	
287	1100	B?	......	......	Ekkeard.	
288	1101	...	January........	......	Chinese Records.	
289	1106	A	Febr., March..	Pisces, Andro., Aries, Taurus	Chin. Rec., Sim. Dunelm. &c.&c.	
290	1109	F?	December	......	Hoveden, Sim. Dunelm., &c.	
291	1110	...	May, June, July	Andro., Pisces, Aries	Ch. Rec., Hoveden, &c. &c.	
292	1113	...	May	......	Matt. Par., Matt. West.	
293	1114	...	May	......	Hen. Hunt., Mat. Par., West.	
294	1115	...	August.........	Aries............	Annal. de Margan., Chin. Rec.	
295	1125	...	......	......	Dubravius.	
296	1126	...	June, July......	Herc., Ur. Maj.	Chin. Rec., Ann. Bosov., &c. &c.	
297	—	...	December......	......	Chinese Records.	
298	1132	...	January.........	......	Chinese Records.	
299	—	...	October........	Musca..........	Chinese Records.	Seen 7 nights.
300	1138	...	August.........	......	Chinese Records.	
301	1142	...	December	......	Chinese Records.	
302	1145	...	Apr. May, June	Scorpio, Virgo	Ch. Rec., Fab., &c.	

* See note, page 207.

Number.	Year of Appearance A.C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
303	—	...	June, July	Orion	Chinese Records.	Of a pale blue colour.
304	1146	...	......	......	Annal. Hirsaug., Matt. Par., &c.	
305	1147	E ?	January.........	Aquar., Pegas.	Chinese Records.	
306	—		February	Capricornus ...	Chin. Rec., Hist. Ep. Virdun.	
307	1155	C	May	......	Chron. Monast. Admontensis.	
308	1156	...	August	Gemini.........	Chinese Records.	
309	1162	...	November....	Pegas., Andro., Aquar., Cetus	Chinese Records.	
310	1165	...	August	To the North.	Chron. Mailr., Hoveden, Boet.	
311	—	...	August	To the South.	Boeth., Hoveden, Chr. Mailr.	
312	1181	...	July, August...	Cassiopeia......	Cavitel. Ann. Chr. Mailr., Ch. Rec.	Seen 156 days.
313	1198	...	November.....	......	Coggesh. Chron.	Seen 15 days.
314	1204	...	......	......	Sicardi Chron.	
315	1208	...	......	......	Chron. Weichen. Cæs. Heisterb.	
316	1211	...	May	......	Cromer. Dlugoss. &c. &c.	
317	1214	...	March	......	Annal. Hirsaug., Crusius.	
318	1217	...	Autumn........	......	Ursperg. Annal. Trevir., &c. &c.	
319	1222	...	Aug., Sept.	Bootes, Libra, Scorpio	Ch. Rec., Chron. Sic. Pad. Est. &c.	
320	1223	...	July............	......	Chr. Franc., Chr. de Nangis. &c.	
321	1230	C	......	......	Dubravii Hist.	
322	1231	...	......	Cygnus, Lyra, Capricornus	Chinese Records.	Elements computed by Pingré*.
323	1232	...	Oct., Nov.	South of Virgo.	Chinese Records.	
324	1239	...	......	......	Chinese Records.	
325	1240	...	Jan., Feb........	Pegasus.	Chin. Rec., Matt. Par.. West, &c.	
326	1250	...	December	....	Arch. Trevir., Chron. Pict.	
327	1254	...	End of the year	....	Polyd. Virg. Chr. Pictav. &c.	
328	1262	...	......	......	Crus. Ann. Suev.	
329	1264	G	Summer........	Hydra, Cancer, Gem., Orion	Ch. Rec. Pachym, &c. &c........	Elements computed by Dunthorn and Pingré*.
330	1265	...	Autumn........	......	Chron. Mellic., Riçobaldus, &c.	
331	1266	I	August	Taurus	Ch. Rec., Nangis. Nic. Greg., &c.	

* See note, next page.

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
332	1269	...	Aug., Sept.....	To the South.	Boeth., Cardan., Centur. Mag.	
333	1273	...	December....	Taurus, U.Maj. Bootes......	Chinese Records.	Seen 21 days.
334	1274	E?	......	......	Guill. de Thoco. Antonin.	
335	1277	...	March	To the N.W.	Chinese Records.	
336	1285	...	April............	......	Ptolem. Hist., Berthold.	
337	1293 or 4	}...	Jan., Feb.......	In the North.	Chinese Records.	
338	1295	...	February	......	Annal. Flandr.	
339	1298	F	......	......	Ann.Trev., Matt. West. &c. &c.	Seen 12 nights.
340	1299	...	Jan. to March	Taurus.........	Chinese Records. English MSS..	Elements computed by Pingré*.
341	1301	...	September.....	Cancer, Scorpio	Chinese Records. Pachym., Chron. Nangis. &c.&c.	Elements computed by Burckhardt and Pingré*.
342	——	...	December......	Aquar., Pisces.	Siffr. Ricobaldus.	
343	1304	...	January, &c...	Peg., Cepheus	Chinese Records.	Seen 3 months.
344	1305	C	April............	......	Chron. Bothon., Crusius, &c.	
345	1313	...	April.........	Gemini	Ch.Rec., Mussat. &c. &c.	
346	1314	...	October	Virgo...........	Paul. Lang., Villani, &c.&c.	
347	1315 –16	}...	Dec., Jan., Feb.	Leo, Virg., Corvus, Peg....	Ch.Rec., Steron. Mussati, &c.&c.	
348	1316	...	May	In the East....	Chron. Rotom., Steron. &c.	
349	1334	...	August.......	......	Chinese Records.	
350	1337	...	May, June, July, Aug.	Taurus, Gemi.	Ch.Rec., Villani, Gregor. &c.	Elements computed by Halley and Pingré*.
351	——	...	July, August..	Cancer.........	Villani, Bucelin, &c. &c.	

Year.*	Passage of the Perihelion in Mean Time at Greenwich.	Longitude of the Perihelion.	Longitude of the Ascending Node.	Angle between the Perihelion and the Node.	Inclination.	Distance in the Perihelion.	Logarithm of the Mean Motion.	Direction.	Names of the Computers.
1066	May 30 or 31	$4^s\ 0^\circ\ 0'$	$7^s\ 20^\circ\ 0'$	$3^s\ 20^\circ\ 0'$	70° or 80°	0·34000	0·665000	R.	Pingré.
1097	Sept. 31 $8^h\ 51^m$	11 2 30	6 27 30	4 5 8	73 30′	0·73850	0·157648	D.	Burckhardt.
1231	Jan. 30 7 13	4 14 48	0 13 30	4 1 18	6 5	0·94776	9·995081	D.	Pingré.
1264	July 6 7 51	9 21 0	5 19 0	4 2 0	36 30	0·44500	0·487588	D.	Dunthorn.
	15 23 51	9 2 30	5 25 30	3 7 0	30 25	0·43000	0·509924	D.	Pingré.
	17 6 1	9 5 45	5 28 45	3 7 0	30 25	0·41081	0·539668	D.	Pingré.
1299	Mar. 31 7 29	0 3 20	3 17 8	3 13 48	68 57	0·31793	0·706633	R.	Pingré.
1301	Sept. beginning	6 0 0	2 0 0	4 0 0	80 0	0·33000	0·682360	D.	Burckhardt.
1301	Oct. 21 23 51	9 0 0	0 15 0	3 15 0	70 0	0·45700	0·470000	R.	Pingré.
1337	June 2 6 26	1 7 59	2 24 21	1 16 22	32 11	0·40666	0·546274	R.	Halley.
	1 0 31	0 20 0	2 6 22	1 16 22	32 11	0·64452	0·246268	R.	Pingré.

XXXVI. *Remarks on* Mr. Christie's *Bakerian Lecture, published in the Philosophical Transactions for* 1833, *Part* I. *By the Rev.* William Ritchie, *LL.D., F.R.S., &c. Professor of Natural and Experimental Philosophy in the Royal Institution of Great Britain, and in the University of London.*

To Sir David Brewster.

Dear Sir,

HAD this lecture appeared as a simple communication by the author, I should have done nothing more than point out the discrepancies between the results and those which I obtained about the same time for voltaic electricity. But as my communication is printed in the same volume of the Transactions as that which has been thus selected and rewarded for its scientific excellence, and as *both* communications cannot be true, I consider myself called upon either to acknowledge the fallacy of the conclusions at which I arrived, or to point out the errors into which the author of the Bakerian Lecture has fallen. As I am still as much convinced of the truth of my communication as when it was read before the Royal Society, I have only one course left to pursue, namely, to point out the fallacy of the conclusions stated in the Bakerian Lecture, which I shall do without the intention or the fear of giving offence either to the author or to the Council of the Royal Society. We are all engaged in the same disinterested warfare, in forcing reluctant Nature to confess the truth: we contend for *Truth*, not for *Victory*.

I shall feel obliged to you and the other Editors if you will give this communication a place in your excellent Journal.

I have the honour to be, Dear Sir, yours, &c.

William Ritchie.

In the first part of the Bakerian Lecture for 1833*, the author has satisfactorily shown that copper is a better conductor of magneto-electricity than iron; which Mr. Faraday in his first trials had not detected, in consequence of the imperfection of his apparatus; but which he afterwards found to be the case, without any knowledge of Mr. Christie's experiments. From the entire paper it is also clearly made out that a short copper wire conducts better than a long one, and that a thick wire conducts better than a fine one (facts which were well known to every person who ever performed a single experiment on the subject); but it appears to me that the laborious investigations, both experimental and mathe-

* An abstract of the Bakerian Lecture for 1833 will be found in the Lond. and Edinb. Phil. Mag., vol. iii. p. 141.—Edit.

matical, prove nothing beyond these general results. Now as the object of the paper was obviously to determine the strict laws of conduction, it is in this point of view that its claims ought to be scrutinized.

In accumulating a current of voltaic electricity so feeble as to produce scarcely any permanent deflection on a needle, the method uniformly employed was to make the needle oscillate, or swing, by repeated impulses given at the moment it returned to the conducting wire.

Mr. Faraday was the first who employed the method of estimating the intensity of electric forces by comparing the arcs through which a magnetic needle was made to oscillate by single electric impulses. As he viewed the forces proportional to the *arcs* themselves, instead of the *sines* of *half* the arcs, it is obvious that he employed this mode as an approximation, his object being merely to ascertain general results*. This principle, when employed as Mr. Faraday has done, as a simple and obvious mode of exhibiting the differences of electric intensity, is exceedingly valuable; but when extended, as Mr. Christie has attempted, to investigations of the most delicate and accurate kind, may lead to general conclusions exceedingly wide of the truth.

Mr. Christie sets out with assuming that a magnetic needle suspended by a fibre of silk directly above a conducting wire, and parallel to it, will, when acted upon by currents of magneto-electricity, *begin* to move with a velocity proportional to the intensity of the current. "The intensity of the current will therefore vary as the velocity with which the needle is impelled at the commencement of its motion, and this velocity will be the same as that which it would acquire in descending from its highest point by the force of terrestrial magnetism acting upon it.

"Let V be the angular velocity with which the needle begins to move; A the whole arc described by the needle; v the angular velocity corresponding to any arc θ; and m the magnetic intensity acting on either pole in the direction parallel to the wires of the galvanometer, and reduced to the distance 1. Then, since the force in the direction of the tangent is $m \sin \theta$, we have

$$v\,d v = -m \sin \theta\, d \theta.$$

Integrating from $\theta = 0$ to $\theta = \mathrm{A}$

$$\mathrm{V} = 2 \sqrt{m} \sin \tfrac{1}{2} \mathrm{A} \ldots\ldots\ldots\ldots\ldots \quad (a)\text{"}$$

"Let I′, I″ be the intensities of the currents corresponding

* Philosophical Transactions for 1833, Part I. p. 50.

to the lengths of L′, L″ of the conducting wire on each side, from the helix to the galvanometer, and to the arcs A′, A″; and suppose that the intensity varies inversely as L^n;

$$\text{then} \quad \frac{L''^n}{L'^n} = \frac{I'}{I''} = \frac{\sin \frac{1}{2} A'}{\sin \frac{1}{2} A''}.\,"$$

This principle requires to be carefully examined. It is well known that the velocity of a pendulum at the lowest point is proportional to the chord of the arc (or to the sine of half the arc) through which it has descended, and that the same law holds with *terrestrial* magnetism acting on a horizontal needle. "En général, le parallélisme de direction des forces magnétiques terrestres qui sollicitent les divers points d'une aiguille aimante permet d'appliquer à tous ces phénomènes les considérations usitées dans la théorie de la pesanteur*." Now Mr. Christie has assumed, *without a shadow of proof*, that a needle placed directly above a wire, and acted upon by a *series of successive impulses* so as to deflect it a certain number of degrees, will have the same velocity at the commencement of its motion as if it had descended from that point by the force of terrestrial magnetism.

It is a well known principle that a body at rest being put in motion must pass through all the degrees of velocity from 0 up to the maximum velocity, and that it cannot gain this velocity till the end of a definite portion of time *after* the commencement of the motion. This definite portion of time may be longer or shorter according to the nature of the action. If the action be instantaneous, the time may be neglected, as being indefinitely small; but this is far from being the case in the present instance. The needle, instead of acquiring its maximum velocity at the commencement of the motion, may not reach that velocity till it has moved through *several* degrees from its position above the conducting wire.

This conclusion is in perfect accordance with the curious properties of electro-magnets which I communicated to the Editors of the Lond. and Edinb. Phil. Mag. last summer, and which are published in vol. iii. p. 122. I there showed that magnetic induction on soft iron is not instantaneously produced, nor does the induced magnetic state cease as soon as the inducing power is removed; but that it depends on the length of the soft iron magnet employed. By employing soft iron lifters of different lengths, the point at which the needle acquires its maximum velocity may be made to vary at pleasure from the zero of the scale. By employing needles of different *weights*, the point at which the needle gains its greatest velocity

* Biot, *Traité de Physique*, tom. iii. p. 20.

will also vary. The strength of the magnet, too, will also have considerable influence in determining this point. The fundamental *equation*, viz. $\frac{I'}{I''} = \frac{\sin \frac{1}{2} A'}{\sin \frac{1}{2} A''}$, on which the whole rests, having no existence in reality, must vitiate the whole of the mathematical reasoning into which it enters.

At page 126, the author first discovered the necessity of taking into consideration the length of the wire forming the galvanometer coil. In a paper of mine, published in the Journal of the Royal Institution several years ago, I first pointed out the necessity of taking into account the length of the wire forming the coil, which ought to be avoided entirely in investigations of this nature*. But though the author has taken into consideration the length of the coil forming the galvanometer, he has entirely neglected the length of the wire forming the helix. Now it is equally necessary to take into consideration the length of this wire as the length of the other, since the effect has to be communicated through the *whole* length of the closed circuit. Hence by employing helices of the same wire, but of different lengths, the apparent laws of conduction will be found to vary with every new helix which is employed.

Another source of error has also been overlooked by the author, namely, the *breadth* of the galvanometer coil. The investigation proceeds on the supposition that the coil is a mathematical line, whereas it may have been an inch broad in that actually employed. The results will therefore vary with the breadth of the coil.

From certain "aberrations" from the law which had been *assumed* as the true law, the author suspected, what he might have known from the beginning, viz. that the contiguous coils of the galvanometer acted by *induction* on each other. After investigating this part of the subject he comes to the following conclusion. "It is therefore clear from all these results that the galvanometer wire acts as a wire of less than its real length, that is, that there is an increase of intensity in it, in consequence of the action of the coils on each other." Now this conclusion is in direct opposition to one of Mr. Faraday's most beautiful results. That philosopher has proved, in the most satisfactory manner, that when a current is induced on a wire parallel to a conductor, the induced current is always in the *contrary* direction to the inducing current. The mutual action of the wires forming the galvanometer would therefore *diminish* the intensity insteadof *increasing* it.

The conclusion at which the author arrives is the follow-

* Journal of the Royal Institution, last series, vol. i. (Oct. 1830,) p. 36.

ing, viz. That the conducting power of wires of the same metal varies as the *squares* of their *diameters* directly, and as their *lengths* inversely. "Or we may say, that the intensity or conducting power varies as the mass or weight directly, and the square of the length inversely." The law thus obtained for magneto-electricity is exactly the same as that obtained by M. Becquerel for voltaic electricity.

I have formerly shown that this law of M. Becquerel does not give even an approximation to the truth for voltaic electricity*, and as all are agreed about the identity of the electricity from these two sources, the law thus obtained for magneto-electricity must be equally remote from the truth.

I have proved, also, that had the conducting power of liquid conductors varied inversely as their lengths, the voltaic battery could not have existed †. In like manner it may easily be shown that had the conducting powers of wires varied inversely as their lengths, the *multiplier*, or galvanometer, could have had no existence. For suppose a galvanometer coil be made with a wire 10 feet long, having 10 folds, and another similar coil made with a wire 40 feet long, having 40 folds, the latter coil would, on that supposition, be exactly equal to the former. If the conducting power vary nearly as the square root of the length, the power of the second would be *double* that of the former, which will be found by experiment not far from the truth.

The very existence, then, of the multiplier, *as an accumulator of feeble voltaic electricity*, is a glaring proof of the fallacy of the conclusion, that the *conducting power varies inversely as the length*.

I shall not enter on the examination of the third part of the Bakerian Lecture; for since the method employed has been insufficient for detecting the laws of conduction in the *same* metal, it is equally inapplicable to determine the ratio of the conducting powers of different metals.

XXXVII. *Descriptions of some hitherto nondescript British Species of May-flies of Anglers.* By J. Curtis, *Esq., F.L.S., &c.*

[Concluded from page 125.]

Gen. 750. Phryganea *Linn.*

6. minor *Curt.*

11 lines: brown, antennæ annulated; superior wings variegated with numerous ochreous dots, forming two large spaces on the costa, two abbre-

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* See Phil. Trans. 1833, Part II. p. 313—316; or Lond. and Edinb. Phil. Mag. vol. iii. pp. 145, 146.—Edit.

† Philosophical Transactions for 1832, Part II. p. 289.

viated oblique lines on the inferior margin and another parallel to the posterior margin, the edge of which is spotted brown and yellow; on the centre is a minute white dot: inferior wings pale fuscous, darkest towards the apex.

Gen. 751. PHILOPOTAMUS *Lea.*

A. Antennæ rather short and stout.

4. trimaculatus *Curt.*

6 lines: fuscous with an ochreous or coppery tinge; similar to *P. variegatus* Fab., but the superior wings are more regularly sprinkled with ochreous dots.

5. conspersus *Curt.*

12 lines: pale fuscous; palpi, legs and antennæ tawny, the latter annulated with white; superior wings thickly and minutely spotted with ochre, leaving brown markings on the costa and inferior margin, and two forked ones on the disc, the margin spotted yellow and brown from the stigma to the posterior angle.

6. dorsalis *Curt.*

12 lines: ochreous; superior wings obscurely clouded and freckled; stigma pale brown, inferior margin fuscous, leaving three pale spots, the anal one minute, forming when the wings are closed three suborbicular spots down the back; the first pair of spurs below the middle in both pair of legs.

6b. longipennis *Curt.*

13 to 14 lines: pale dirty ochre; antennæ and legs fulvous, the former short and annulated; superior wings long, narrow and lanceolate, fuscous with innumerable yellow dots; stigma long and fuscous, bearing three yellow spots; first pair of spurs on intermediate tibiæ considerably below the middle.

B. Antennæ long and slender.

7. instabilis *Curt.*—maculatus *Don.*? *pl.* 548, 2.

Similar to Donovan's figure; but as I believe Olivier's *P. maculata* is different, it became necessary to give another name to our insect. The intermediate tarsi are dilated.

8. pellucidulus *Curt.*

15 lines: head, thorax and abdomen blackish; antennæ very long, ochreous, spotted fuscous; wings semitransparent, superior obscurely freckled with pale fuscous and ochre, margin spotted with ochre from the stigma to the posterior angle, and there are longer ochre spots on the inferior margin; legs ochreous, intermediate tarsi dilated.

10. lanceolatus *Curt.*

13 lines: wings fuscous, superior slightly hooked, clothed with shining ochreous pubescence, slightly freckled, the posterior margin slightly spotted; intermediate tarsi but little dilated.

9. angustipennis *Curt.*

10 to 13 lines: antennæ slightly serrated and annulated; head, thorax and abdomen blackish; wings fuscous, superior with an ochreous tint, a large brown trigonate stigma and an ochreous oblong spot before, and another round one at the posterior angle; intermediate tibiæ and tarsi compressed and dilated in the female; legs ochreous, darker at the base.

9b. fulvipes *Curt.*

13 lines: black; antennæ annulated with fulvous, neck clothed with gri-

seous hair, edges of abdominal segments white; wings pale black, superior obscurely freckled with ochre; legs fulvous, thighs fuscous.

Gen. 753. Leptocerus *Lea.*

A. Superior wings with two furcate apical cells.

11. cinereus *Curt.*

12 lines: antennæ very long, black annulated with white, clothed with griseous hairs; wings fuscous, superior clothed with grey pubescence; costa slightly ochreous, as well as some indistinct marks, a small portion of the base of the inferior wings pale ochreous; tibiæ and tarsi of the same colour.

B. Superior wings with a line of transverse nervures forming the apical cells.

12. ochraceus *Curt. Brit. Ent.* v. 2. fol. 57.

13. testaceus *Curt.*

8 lines: reddish ochre, very pubescent; antennæ long; eyes black; cilia fuscous; inferior wings transparent, subfuscous at their tips.

C. Wings very narrow, with few nervures.

15. bicolor *Curt.*

8 lines: bright ferruginous ochre; antennæ very long, black annulated with white; abdomen and inferior wings blackish; legs fulvous.

16. tineiformis *Curt.*

6 lines: antennæ 4½ lines long, white annulated with black; cinereous pubescent; superior wings very lanceolate, the nervures dotted with fuscous: legs ochreous white.

Gen. 754[a]. Molanna *Curt.*

Antennæ stout, as long as the wings; abdomen short; wings long, narrow and rounded at the apex, superior with a short furcate cell at the apex, a long one below and an oblique nervure above it; discoidal cells very long; legs long and pubescent; first pair of spurs on four posterior tibiæ considerably below the middle.

1. angustata *Curt.*

3 lines long, 12 broad: very pubescent and silky, tawny; antennæ spotted fuscous; head, thorax and abdomen fuscous; nervures dark; inferior wings pale fuscous; legs fulvous.

Gen. 755. Odontocerus *Lea.*

2. maculipennis *Curt.*

16 lines: pubescent black; head and thorax griseous, scutellum ochreous; superior wings ochreous; nervures, margin and cilia black, a spot on the costa, the stigma, another beyond it, a large one at the posterior angle and several small ones round the disc pale black; legs fulvous, anterior pair and thighs dusky.

I took a single specimen of this beautiful insect, flying over a stream in the New Forest, in June.

Gen. 758. Sericostoma *Lat.*

Maxillary palpi recurved, densely hairy, compressed and forming an obtuse rounded front to the head in the males; elongated and distinctly articulated in the females.

1. Latreillii *Hal.*

♂ 10 lines, ♀ 14: fuscous; base of antennæ with orange scales in the male; head, excepting an ochreous spot on the crown, thorax and abdomen black; superior wings with a cupreous tint; legs ochreous, thighs griseous.

Gen. 757. Silo *Curt.*

The maxillary palpi in the males divaricate, and sparingly clothed with long subclavate hairs; the four posterior tibiæ have two pair of spurs on each.

1. pallipes *Fab. Ent. Syst.* 2. 76. 7.

Gen. 759. Goëra *Hoff. MSS.*

Maxillary palpi recurved, close to the head in the males.

A. Superior wings with one of the cells dilated and circular near the posterior angle.

4. flavipes *Curt.*

11 lines: silky-ochreous; thorax, abdomen, inferior wings and underside, also the coxæ and thighs fuscous; antennæ brown, basal joint clothed with ochreous hairs.

1. pilosa *Fab. Ent. Syst.* 2. 76. 6.

2. irrorata *Curt.*

7 lines: ochreous, pubescent; antennæ piceous, slightly annulated with ochre, the basal joint long, stout and very hairy; wings very pale fuscous, superior subochreous, with numerous indistinct whitish dots.

B. Superior wings with the same cell short.

6. hirta *Fab. Ent. Syst.* 2. 80. 26.

This may be the female of the next species; and if so, it must be removed to my genus Mormonia.

Gen. 759b. Mormonia *Curt.*

Resembling *G. hirta*, but the maxillary palpi are porrected, subclavate and densely clothed with short hairs or scales in the males; three cells on the inferior margin are free from transverse nervures, excepting towards the base, where on the costa there is a densely hairy patch.

5. gracilicornis *Curt.*

10 lines: ♂ fuscous very pubescent; maxillary palpi and eyes black; antennæ, legs and sometimes the body ochreous, the former dotted fuscous; wings with an ochreous tint.

3. maculicornis *Curt.*

10 lines: ♂ pale black, very pubescent; antennæ ochreous annulated with fuscous, labial palpi, underside of abdomen and legs tawny.

Gen. 759c. Brachycentrus *Curt.*

Antennæ remote at the base, shorter than the wings, stout in the male, slender in the female; maxillary palpi short, divaricating and very hairy in the male: wings rather broad and rounded, superior with a small discoidal cell, from which spring three recurved nervures; four posterior tibiæ with minute spurs at the apex and a smaller pair just above them.

3. subnubila *Curt.*

10 lines: black; head and thorax with woolly griseous hairs; wings pale fuscous, the nervures piceous, especially in the superior, which bear several pale yellow spots on the stigma, the discoidal cell and one between each nervure on the posterior margin; tibiæ and tarsi ochreous.

Gen. 759[d]. Thya *Curt.*

Antennæ shorter than the wings, basal joint porrected, long, stout and hairy; maxillary palpi long, stout and hairy in both sexes; wings rounded, very pubescent, nervures indistinct; tibiæ spurred at the apex longest in the middle pair, posterior with a minute pair above the apex.

7. pullata *Curt.*

6 lines: black; tarsi shining whitish; wings with a blueish tint, superior with a few very obscure whitish spots.

8. pygmæa? *Fab. v.* 5. 202. 31.

The spurs in my specimens are longer than in the type, but the posterior tibiæ only have two pair.

4. Maurus *Curt.*

5 lines: black; wings narrower and more lanceolate, with a slight ochreous tint; legs shining dirty ochre.

Gen. 759[e]. Glossosoma *Curt.*

Male: wings with a subscutiform discoidal cell in each, superior with a knot or fold at the base clothed with rigid hairs above, concave beneath; first pair of spurs on the intermediate tibiæ at the middle, those on the posterior near the apex; a large horny subdepressed lobe attached to the underside of the sixth segment of the abdomen, with a smaller one on the following joint; *female*, with the intermediate tibiæ and tarsi very much dilated.

1. Boltoni *Curt.*

10 lines: antennæ, head, thorax and abdomen fuscous castaneous, the former annulated with and the latter tipped with ochre; wings pubescent, pale fuscous, superior with the callous lump at the base in the male, brown, clothed with black hairs; stigma and a spot opposite on the inferior margin fuscous, and each nervure terminated by a spot of the same colour on the margin alternating with ochreous spots, with an indistinct row behind them, and two or three near the disc of the same colour; inferior wings gray and transparent at the base.

Gen. 760. Tinodes *Lea. MSS.*

Maxillary palpi long and alike in both sexes; antennæ shorter than the wings and slender; abdomen short, acuminated and horny in the female; four posterior tibiæ with two pair of long spurs on each, first pair near to the base in the intermediate, and considerably below the middle in the posterior tibiæ.

A. Wings long and rather narrow, superior with a small discoidal cell and four furcate ones on the posterior margin.

5. luridus *Curt.*

9 lines: dirty ochreous; head, thorax and abdomen castaneous brown; nervures of superior wings dark brown, inferior wings hyaline and iridescent; legs fulvous.

B. Wings shorter, more rounded at the apex, superior very pubescent, the nervures very indistinct.

4. pusillus? *Fab. Ent. Syst.* 2. 81. 33.

Gen. 760[b]. Anticyra *Curt.*

Antennæ rather longer than the body, porrected, not slender; head very hairy on the crown; maxillary palpi long and stout; abdomen short, with two large horny lobes at the apex in the male; wings long and narrow, four posterior tibiæ with two pair of long spurs, first pair approaching the base in the intermediate, and below the middle in the others.

1. gracilipes *Curt.*

5½ lines: pale fuscous; antennæ whitish, annulated with black; head griseous; eyes black; wings lanceolate, superior glossy ochreous; legs slender, pale dull fulvous.

2. latipes *Curt.*

6 lines: similar to the last, but the wings are narrower, the superior more rounded, the inferior more pointed; abdomen carneous; intermediate tibiæ and tarsi dilated.

Gen. 760[c]. Agapetus *Curt.*

Antennæ not longer than the body, stout and divaricating; abdomen of male with a long incurved spine arising from the centre of the belly; apex acuminated in the female; wings comparatively short and rounded; four posterior tibiæ with two pair of strong spurs, one pair at, the other above the apex.

1. fuscipes *Curt.*

5 lines ♂: black; superior wings and tips of inferior ochreous fuscous, iridescent, nervures darker; head and thorax with shining griseous hairs; legs fuscous; trochanters ochreous.

2. ochripes *Curt.*

5½ lines ♀: similar to the last; the antennæ are rather longer and more slender; the head, thorax and body are subcastaneous, and the legs pale ochre, basal joint of intermediate tarsi dilated elliptical.

3. funereus? *Oliv. Geoff. Lat. Hist. Nat. v.* 13. *p.* 93. 30.

Gen. 760[d]. Agraylea *Curt.*

Antennæ not longer than the body, rather stout and filiform; head rather broad; abdomen short; wings long, narrow and pubescent; anterior legs stout, the others slender; four posterior tibiæ with long spurs at the apex, intermediate with *one* at the middle, hinder with a pair above the apex.

1. 6-maculata *Curt.*

4 lines: ochreous; antennæ fuscous, except at the base; superior wings pale fuscous, with two whitish spots on the costa, two bands towards the apex, and two oblong spots on the inferior margin of the same colour.

2. multipunctata *Curt.*

4 lines: fuscous; crown of head griseous; superior wings with a long ochreous spot beyond the stigma, with several small ones on the posterior margin and along the centre to the base, two oblong ones on the inferior margin and two at the base of the cilia; face, abdomen and legs fulvous.

Gen. 761. Hydroptila *Dal.*

4. sparsa *Curt.*

3 lines: fuscous black; antennæ and legs fulvous; head gray; superior wings with a silvery white spot on the middle of the costa, another opposite, the margin between this and the base with an interrupted whitish line, and several minute dots of the same colour round the apex.

2. Vectis *Curt.*

3½ lines: fuscous; head whitish or ochreous; superior wings with an ochreous spot near the base, an angulated band across the middle, a spot on the cilia at the stigma, and seven round, and one at the apex; legs and belly dull silvery.

3. costalis *Curt.*

3 lines: pale ochreous shining; superior wings variegated fuscous, with a dot on the disc, the costal cilia long and black, with a long pale space at the centre.

Jan. 1st, 1834.

XXXVIII. *Reviews, and Notices respecting New Books.*

Mathematical Tracts. By J. W. Lubbock, Esq., F.R.S.

THIS volume, which forms an important addition to the libraries of the astronomer and the analyst, consists of three tracts, the objects of which we proceed to state, *seriatim.*

1. *On the Theory of the Moon, and on the Perturbations of the Planets.*

Mr. Lubbock's object in this memoir is to give a connected but brief sketch of, and to improve, a portion of his researches in physical astronomy, printed in the Philosophical Transactions. Laplace adopted the analytical investigation of the theory of the moon proposed and employed by Clairaut, but carrying the approximation much further: "it has since been pushed to an almost incredible extent by M. Damoiseau, without any alteration, however, in the method employed.

"Laplace," Mr. Lubbock states in his Preface, "insists particularly upon the necessity of having recourse to the equations in which the true longitude is the independent variable; but notwithstanding the respect which is due to so great authority, I am convinced, after much reflection, that the method which I have submitted to the Royal Society, and which forms the groundwork of this essay, is on many accounts to be preferred. According to this method, the expressions for the parallax, and the true longitude obtained by the reversion of series in the method of Clairaut, are arrived at directly."

It being desirable to introduce into the science of physical astronomy a uniform system, by employing, if possible, methods which embrace the motions of all the heavenly bodies*, Mr. Lubbock has shown how the perturbations of the planets and those of the satellites of Jupiter may be obtained from the equations employed in his theory of the moon; and how also the table of the arguments which occur in the theory of the moon may be used in the planetary theory. The resulting expressions for the planetary perturbations differ in form from those of the *Mécanique Céleste,* Mr. L. obtaining the inequalities of the reciprocal of the radius vector, by the method of indeterminate coefficients, directly from an equation, without the intervention of an auxiliary variable, while he obtains those of longitude from an equation differing from that employed in the *Mécanique Céleste,* and other works in which the same methods are adopted.

* On this subject see also our Report of the Proceedings of the Royal Society, in the present Number, p. 216—217.

"The stability of a system of bodies in motion subject to the law of mutual gravitation which obtains in nature, is a question," Mr. Lubbock observes, "of great interest. It appears to me," he continues, "that it may be established through considerations different from those hitherto employed, namely, by observing that the quantities which have been called c and g in the Lunar Theory are *rational*, so that no imaginary angles or exponentials are introduced in the expressions for the coordinates of the body in motion, which are thus functions of periodic quantities. In the theory of the moon I apprehend this theorem is certainly independent of the direction of the moon's motion."

2. *On the Determination of the Distance of a Comet from the Earth, and the Elements of its Orbit.*

This essay is intended to convey an exposition of different methods which have been proposed for the solution of one of the most important problems in physical astronomy. The author having proposed, in the Memoirs of the Astronomical Society, a new method, which results from a combination of the equations of Legendre and Lagrange, by which the distance of the comet from the sun is eliminated, and the question of the determination of the distance of a comet from the earth is reduced to the solution of a quadratic equation, thus superseding the necessity of having recourse to repeated trials; gives, in the present tract, details which could not enter into the plan of that paper, and also a new method of obtaining the perturbations of a comet, which consists in determining directly the perturbations of the rectangular coordinates.

3. *Account of the "Traité sur le Flux et Réflux de la Mer," of Daniel Bernoulli; and a Treatise on the Attraction of Ellipsoids.*

This tract, we believe, was the first of the series of memoirs (continued in the Philosophical Transactions, &c.) in which Mr. Lubbock has discussed the phænomena and theory of the tides, and which have had an influence so advantageous in the direction to that subject of the minds of British mathematicians and observers, as well as in calling the attention of public bodies to the importance of instituting regular and comparable observations of tidal phænomena.

The Essay of Bernoulli which forms the subject of the first part was published in the Jesuits' edition of the *Principia*: it shared the prize proposed by the Academy of Sciences at Paris in 1738, and adjudged in 1740, with those of Euler, Maclaurin, and Cavalleri; but the tables contained in it, Mr. Lubbock remarks, "have solely been employed hitherto in calculating the times and the heights of high water." Mr. L. observes, in the Preface, "that the effect attributed to changes in the moon's distance in the tables for finding the time of high water in Mackay's "*Complete Navigator*," p. 58, in Riddle's "*Navigation*," p. 257, and in Dr. Inman's "*Nautical Tables*," p. 5, is directly contrary to that which is given by the table of Bernoulli, p. 165, (upon which they otherwise appear to be founded,) and to observation. In order to remove this error it is necessary to reverse the heading which gives the one the moon's parallax, and in the others the moon's semi-diameter."

In the short Treatise upon the Attraction of Spheroids which constitutes the second part of this tract, the author has endeavoured to treat that subject in as elementary a manner as possible. He has made continual references in it to the authors who have written upon the figure of the earth: it may serve therefore, in some measure, to illustrate the first chapter in the fifth volume of the *Mécanique Céleste*. He shows, in the Preface, that the first addition to the Newtonian theory of the figure of the earth was made, not by Clairaut, as affirmed by Laplace, but by our countryman Stirling, in his paper "Of the Gravity of the Earth, and the Variation of Gravity on the Surface," which was published in the Philosophical Transactions for 1735, two years prior to the appearance, in the same collection, of the memoir in which Clairaut proved the approximate expressions which Stirling (as evinced in his paper) must already have been in possession of. The investigation of the latter, Mr. Lubbock thinks, is a great addition to what is contained in the *Principia*.

The following are the contents of this treatise:—Attraction of a prolate spheroid upon a particle within it—Attraction of an oblate spheriod upon a particle within it—Attraction of an ellipsoid upon a point within it—Attraction of an ellipsoid upon a point without it—On the figure of the earth—On the figure which the ocean would assume if at rest—Index to some of the symbols which are used.

XXXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Dec. 12.— 1833. AN account of some experiments made in the West Indies and North America, to determine the relative Magnetic Forces, in the years 1831, 32, and 33. By the Rev. George Fisher, M.A., F.R.S.

The experiments of which the results are given in this paper were made by Mr. James Napier, late Master of H. M. S. Winchester. The needles were precisely similar to those used in the experiments described by the author in a former paper; and the observations were made with great care, and repeated several times at the same places; by which it appeared that the intensities of the needles continued unchanged during the whole period of the experiments; and the mean of all those made at one place was taken as the result. From these the relative forces at different places were computed, and stated in the form of a table.

Reports were read from Sir John Herschel, Professor Airy and Captain Smyth, on the performance of the refracting telescope, constructed with fluid lenses, on the plan proposed by Mr. Barlow.

"On the Theory of the Moon." By John William Lubbock, Esq., V.P. and Treas. R.S.

M. Poisson, in a memoir which he has lately published on the Theory of the Moon, expresses the three coordinates of her path, namely, her true longitude, her distances, and her true latitude, in terms of the time. The author observes that the reasons for so doing adduced by M. Poisson, are the same as those which led Mr. Lubbock also to deviate from the course which had previously been always pursued by mathematicians, and to employ equations in which the true longitude is the independent variable. Instead, however, of integrating the equations of motion by the method of indeterminate coefficients, as the author had proposed, M. Poisson recommends the adoption of the method of the variation of the elliptic constants. In the present paper, Mr. Lubbock states the reasons which have determined him not to employ the latter method, founded chiefly on the advantages of obtaining complete uniformity in the methods used in the theories of the moon and of the planets, and also in that of a greater rapidity of approximation by the improvements introduced in these methods.

Laplace, in the Mécanique Céleste, alludes to an equation of long period, of which the argument is twice the longitude of the moon's node, plus the longitude of her perigee, minus three times the longitude of the sun's perigee; and M. Poisson has shown that the coefficient of the corresponding argument in the development of the disturbing function equals zero: but the author shows that the same result may be arrived at very simply, by means of the method of developing the variation of the disturbing function.

Dec. 19.—"On the Quantity and Quality of the Gases disengaged from the Thermal Spring which supplies the King's Bath, in the City of Bath." By Charles G. B. Daubeny, M.D., F.R.S., Professor of Chemistry in the University of Oxford.

The author, pursuant to an intention expressed in a former paper read to the Society, undertook a series of experiments, for the purpose of measuring the gas evolved from the thermal springs at Bath during a period of time sufficiently long to enable him to determine with tolerable precision its average amount, and to ascertain whether any great diurnal variations in its quantity can be detected. He also kept during the same period a corresponding register of the conditions of the atmosphere, as to temperature, humidity and pressure, in order to learn whether any connexion could be traced between these conditions and the quantities of gas evolved. The supplies, both of water and of gas, from the hot bath and the Cross bath being insignificant compared with those from the King's bath, the author confined his inquiries to the last of these, and chiefly to the gas arising from the apertures within its central area, which is about twenty feet in diameter; the other apertures without this circle from which gas issued being carefully stopped up. The gas was collected by a funnel-shaped apparatus, constructed of several sheets of iron riveted together, and the seams rendered airtight by white lead, supported on a frame, with contrivances for raising and lowering it as occasion might require. The observations were made during periods of from five to fifteen minutes, and continued daily from the 17th of September to the 18th of October inclusive. The average quantity of gas evolved per minute, as deduced from the mean of all the observations, is 267 cubic inches, giving a total daily volume of 223 cubic feet.

The author, by referring to the accounts on record of other thermal waters, concludes that the evolution of gas is a phenomenon as intimately connected with the constitution of these waters, as the presence of a definite quantity of certain saline ingredients, or the possession of a particular temperature; both of which probably continue unaltered for periods of indefinite duration, compared with the records of any human history. He considers this phenomenon to be explicable, by supposing that a large volume of these gases is pent up in some cavern of rock, at a great depth below the surface of the earth, which, at some former period, had been heated by volcanic action, and which, by the gradual cooling and consequent contraction of its external portions, exerts a continued pressure on the gaseous contents of its cavity, and determines the uniform flow of a stream of gas through crevices towards the surface.

It appears from the observations of the author that the quantities of gas disengaged, in a given time, from the King's bath are somewhat variable; for the differences between the results obtained on successive days are too considerable to be ascribed either to errors of manipulation or to variations in the amount of gas escaping by other avenues. These fluctuations in quantity cannot be traced to have any connexion with those of the atmospheric pressure. Variations likewise were observed in the proportional quantities of carbonic acid contained in the gas evolved at different times, which latter variations the author thinks may perhaps be dependent on the former.

The author remarks, in conclusion, that the immensity of the volume of nitrogen gas which is disengaged from these thermal springs, and the entire absence of carburetted, sulphuretted and phosphuretted hydrogen, seems to afford additional presumption against the truth of the opinion that the nitrogen gas which escapes from volcanoes and from these springs is derived from atmospheric air, held in solution by the water, and deprived of the greater part of its oxygen by animal and vegetable putrefaction. He is disposed to ascribe the deficiency of oxygen to some process of combustion, during which it unites with some base, forming a compound not easily volatilized by heat; and to account for the presence of carbonic acid, by the calcination of earthy carbonates, rather than by the combustion of coal or bitumen.

"On the Position of the North Magnetic Pole." By Commander James Clark Ross, R.N., F.R.S.

The author remarks that the discordances in former observations made with a view to determine the position of the magnetic pole, have arisen partly from the irregularity of distribution in the earth of the substances which exert magnetic power, and partly from the great distances from the magnetic poles at which these observations have been made. The latter cause of uncertainty has been now, in a great measure, removed, by the numerous and accurate observations made during the late arctic expeditions. The object of the present paper is to put on record those which were made in the last voyage of Captain Ross, in which a spot was reached corresponding to the true north magnetic pole on the surface of the earth. The nature of the instruments, and the difficulties encountered in their practical employment, under the circumstances of the expedition, are fully stated. Having arrived, on the 1st of June, at north latitude 70° 5′ 17″, and west longitude 96° 45′ 48″, the horizontal magnetic needle exhibited no determinate directive tendency, and the dipping needle was within a minute of the vertical position, a quantity which may be supposed to come within the limits of the errors of observation; hence the author concludes that this spot may be considered as the true magnetic pole, or as a very near approximation to it, as far, at least, as could be ascertained with the limited means of determination of which he was then in possession.

A table of the observations, including those on the intensity of the magnetic force at various stations, is subjoined.

Jan. 9, 1834.—"On the empirical Laws of the Tides in the Port of London, with some Reflections on the Theory." By the Rev. William Whewell, M.A., F.R.S., Fellow and Tutor of Trinity College, Cambridge.

The present state of our knowledge of the tides is represented by the author as extremely imperfect, and at variance with the scientific character which Physical Astronomy is supposed to have attained; for although it be the universally received opinion that they are the direct results of the law of gravitation, the exact laws by which the phenomena are actually regulated with regard to time and place have never been strictly deduced from this general principle. The tide tables that have been given to the world are calculated by empirical methods, which are frequently kept secret by those who employ them; and the mathematical solutions of the problem hitherto attempted have been confessedly founded on hypotheses which are in reality very remote from the real facts; and accordingly it is doubtful whether they give even an approximation to the true result. The comparison of the results of theory with extensive series of observations had not been attempted previously to Mr. Lubbock's discussion of the tides of the port of London, recorded in the Philosophical Transactions for 1831. The establishment, on theoretical grounds, of rules for the calculation of tide tables, has been attempted by Bernoulli and by Laplace: the methods recommended by the former are probably the foundation of those at present used by the calculators of such tables, that of Laplace being complicated, and too laborious for practice. Original tide tables are very few; none, with which the author is acquainted, deserving that title, except those which are published for Liverpool, and those for London. The former, which are calculated according to rules obtained from Mr. Holden, from the examination of five years of observations, made at the Liverpool docks by Mr. Hutchinson, at that time harbour-master, are remarkably correct. Several tide tables for London are annually published; but they vary considerably from one another. The method generally practised in England for the construction of tide tables for other places, has been to add or subtract some constant quantity, according to the place, assuming as a basis the tide tables either of London or of Liverpool; but this assumption of a constant difference is shown by the author to be, in various instances, incorrect. Much, therefore, remains to be done, before we can hope to arrive at a scientific solution of this problem.

The author then proceeds to examine the empirical laws of the tides of the port of London, deducible from the records of the nineteen years of observations which have been discussed by Mr. Dessiou, under the direction of Mr. Lubbock, and which include 13,073 observations. His first object is to determine the manner in which the time of high-water is affected by the following conditions, namely, the right ascensions, declinations and parallaxes of the sun and moon; for which purpose he considers at some length, first, the establishment; secondly, the semimenstrual inequality; thirdly, the corrections for lunar parallax; fourthly, the lunar declination; and lastly, the solar parallax and declination. He next discusses the empirical laws of the height of high-water; which he observes will be affected in the same manner as the periods of the tides, by a semimenstrual inequality, by corrections for lunar parallax and declination, and by a solar correction; and concludes by giving a formula for computation which comprehends all these elements. He then enters into a comparison of the results thus obtained with the theory of Daniel Bernoulli, according to which the waters of the ocean assume nearly the form in which they would be in equilibrium under the actions of the sun and moon, on the supposition that the pole of the fluid spheroid follows the pole of the spheroid of equilibrium at a certain angular distance; and that the equilibrium corresponds to the configuration of the sun and moon, not at the moment of the tide, but at a previous moment, at which the right ascension of the moon was less by a constant quantity. The author thinks, however, that it would not be safe to attempt to deduce from the preceding investigations any general views concerning the laws of the tides, for it is not likely that the discussion of observations at any one place should exhibit clearly the true principles of the theory, especially as, in the present case, it so happens that the phenomena of the tides at London are in some measure masked by a curious combination of circumstances, namely, by the mouth of its river being on the side of an island, turned away from that on which the tide comes, and so situated that the path of the tide round one end of the island is just twelve hours longer than round the other.

In consequence of the time required to transmit to any port the general effect of the tide-producing forces being different from the time required to transmit to the same port the effects of particular changes in these forces; or, in other words, from the epochs of the changes due to parallax and declination being different from the epoch of the semimenstrual inequality, it follows that although the general form of the terms, and the variable part of the arcs on which they depend, may be deduced from the theory of equilibrium, yet the constant epoch which occurs in each of these arcs, and which determines when the inequality vanishes, and reaches its maximum, will probably have to be determined, in all cases, by observation.

In conclusion, the author gives a statement of what appears to him to be the most important steps from which any great improvement to our knowledge on the subject of the tides may be hoped; and recommends the discussion of extensive collections of observations made at a variety of places, in a manner similar to what has been done by Mr. Dessiou with regard to those at London; and the comparison with one another of the empirical laws resulting from their separate investigation. Very valuable materials for this purpose, he expects, will hereafter be furnished by the observations now making, on a judicious system, at the St. Katharines' docks.

Jan. 16, 1834.—"On a new property of the Arcs of the Equilateral Hyperbola." By Henry Fox Talbot, Esq., M.P., F.R.S.

By an analytical process, the author arrives at the following theorem, namely, if three abscissæ of an equilateral hyperbola are materially dependent by reason of two assumed equations, which are symmetrical with respect to these three abscissæ, the sum of the arcs subtended by them is equal to three quarters of the product of the same abscissæ, or only differs therefrom by a constant quantity. In order to satisfy himself of the correctness of this theorem, the author calculated various numerical examples, which entirely confirmed it. This simple result is essentially a relation between three arcs of the equilateral hyperbola, and is by no means reducible to a relation between two; and therefore is not reducible to the celebrated theorem of Fagnani, concerning the difference of two arcs of an ellipse or hyperbola, nor to any other known property of the curve.

Appendix to a Memoir, lately read to the Society, on the Quality and Quantity of the Gases disengaged from the Hot Spring of the King's Bath, in the City of Bath. By G. B. Charles Daubeny, M.D., F.R.S.

The author has lately examined two tepid springs, which, since the setting in of the wet weather, have broken out at the foot of St. Vincent's rocks, Clifton, immediately below the Cliff, against which the suspension bridge over the Avon is designed to abut. The temperatures of the springs were 72° and 66° respectively; and the gas consisted of 92 parts of nitrogen, eight of oxygen, and three of carbonic acid. The author deduces from these facts arguments in confirmation of the views he has stated in the paper to which this is an appendix.

GEOLOGICAL SOCIETY.

Dec. 18, 1833.—A memoir "On the Geology of the Banks of the Indus, the Indian Caucasus, and the Plains of Tartary to the Shores of the Caspian," by Lieut. Alexander Burnes, was then read.

The author has endeavoured in this paper to embody the geological observations which he made on a journey during the years 1831 and 1832, up the river Indus and across the lofty range of Hindoo Koosh to the Caspian Sea.

He first describes the province of Cutch, situated near the eastern mouth of the Indus. He states that it is mountainous; that the soil is either rocky or sandy, with masses of lava scattered over its surface; and that sulphur, coal, iron and alum are found in the district.

Nummulites occur in a ridge near the right banks of the Indus. The delta of the river is composed of a succession of beds of earth, clay and sand of different colours, sometimes parallel, and sometimes having one stratum dovetailed into another. The sea is described as being discoloured to a distance of three miles by the detritus carried down by the river, with regard to which it may be stated that the base of the triangle of the delta is above 125 miles.

After mentioning a range of hills called the Hala Mountains, which extends in a northerly direction from the sea-shore westward of the mouths of the Indus, and terminates to the N.W. of Cabool in the Hindoo Caucasus, and which consists, in part of compact nummulitic limestone, the author proceeds to describe the principal geological features which he observed on the banks of this great river. The town of Hydrabad, he states, is built on a finely grained, shelly limestone. At Schwan in lat. 26° 22′ and at Curachee, are hot wells; and the island of Bukhur, in lat. 27° 42′, consists entirely of flint. On the eastern bank of the river, opposite this island, is a precipice of flint, 40 feet high, on which the village of Roree is built. In lat. 28° 55′ the rivers of the Punjab fall into the Indus. Still higher up, in lat. 33°, at Kara Bagh, the river cuts through a range of hills, described by Mr. Elphinstone as the salt range. The salt is found in layers of about a foot in thickness, separated from each other by thin strata of clay. With the exception of this range of hills, which is estimated to be about 1800 feet above the level of the sea, the district of the Punjab is uniformly flat; but the hilly district is intersected by numerous defiles, presenting vertical strata, which terminate in peaked points. Between the river Sutlege and Lahore the country consists of indurated clay, sometimes gravelly.

At Attoch, much higher up, the rocks by which the Indus is confined consist of a dark coloured micaceous slate, which is said to extend to the southward until it meets the salt range above mentioned. Near this place gold is washed out of the sand of the river.

At Lahore, in February 1832, the author experienced a very violent shock of an earthquake. Several valleys were choked up by the masses of rock thrown down from the overhanging precipices, and a great part of the population of Badakhshan was destroyed. In crossing the Punjab the author observed that several buildings of the Mogul Emperors were decaying from the foundations, and were encrusted with an efflorescence of nitre. Proceeding to the westward from the Indus, he found bituminous coal at Cohat, and that the salt range above mentioned extended across the country into this district. The river of Cabool flows through a very narrow defile, the rocks of which rise to a height of 2000 feet, and consist of sandstone, quartz rock and mica schist, the strata of the latter being vertical. Cabool is situated 6000 feet above the sea. The neighbouring hills are covered with rounded pebbles of all sizes, sometimes loose, at others forming a conglomerate. A beautiful white marble is found near Cabool, and the rocks are occasionally covered with asbestus.

From Cabool the author crossed the Hindoo Caucasus to Balkh and the plains of Tartary. This range of mountains is the prolongation of the Himalaya to the westward of the Indus.

Hindoo Koosh is, properly speaking, the name given to the highest peak in the range, the only part of which that is covered with perpetual snow is the Koh-i-Baba, between Cabool and Bameean, from which latter place the waters flow northward into the Oxus. In some of the defiles through which the author passed, the sides rose to a height of 2000 or 3000 feet. The loftiest peak which he observed between Cabool and Hajeeguk consisted of gneiss or granite, sometimes deeply impregnated with iron. These formations were succeeded by blue slates and quartz rock, and precipices of micaceous schist. From the summits of the precipices masses of green granite and other rocks had been hurled into the valley below. Further down is a calcareous conglomerate, succeeded by cliffs of reddish and purple coloured clay, and by ridges of indurated clay mixed with bands of a harder nature. In this ridge great idols are carved and caves excavated, for it is easily worked. The neighbourhood of Bameean is described as producing gold, lead, copper, tin, antimony, sulphur and iron.

The lower passes of Hindoo Koosh consist principally of a light brown splintery limestone, of great hardness, and susceptible of a high polish. This formation is followed by sandstone rocks, in one of which round flint stones are imbedded at regular intervals. The real peak of Hindoo Koosh lies about a degree to the eastward of this route, and the difficulty of crossing it is very great.

From Khooloom, whence the author descended into the plains of Toorkistan, the country slopes gradually towards the Caspian. It is generally flat and is watered by the Oxus.

The author then describes the course of the Oxus, from its source in the high plain of Pameer until it is lost in the sea of Aral, after passing through a low and swampy district. He does not believe that the Oxus ever terminated in the Caspian Sea, and concludes that what are called the dry river beds between Astrabad and Khina are the remains of ancient canals. The natives pretend that the waters of the Aral pass by a subterranean communication into the Caspian Sea, and that at a place called Kara-goombuz, between the two seas, the water may be heard gushing beneath. It is, however, remarkable, that in the sandy ridge near this place, water is found near the surface, although further south it cannot be had within a hundred fathoms. The author then fully describes the navigation, course, rise and fall, and inundations of the Oxus; and he mentions that it is frequently frozen over.

The author then notices the effects of the great earthquake of 1832 in the valley of Badakhshan. The roads in this valley were blocked up for several days by the falling of stones and cliffs, and this place seems to have been the centre of the convulsion. Badakhshan is famous for its rubies, which are found imbedded in limestone.

The country which extends from the north of the Oxus towards Bokhara is next described. It consists of a succession of low ridges of soft yellowish limestone, sometimes oolitic, with a superficial coating of loose gravel, alternating with plains of hard clay. Sand hills of greater or less extent, raised by the winds, also occur in several places on this plain, and in some of the valleys are saline rivulets and deposits of salt.

The author afterwards offers some remarks upon the inhabitants, and on the meteorological phenomena which he observed in the neighbourhood of Bokhara; and concludes his memoir with a description of the sandy desert of the Turcomans, between the Oxus and the Caspian Sea.

Jan. 8, 1834.—A paper was read by Roderick Impey Murchison, Esq., F.R.S., F.G.S., "On the Old Red Sandstone in the Counties of Hereford, Brecknock and Caermarthen, with collateral Observations on the Dislocations which affect the north-west margin of the South Welsh Coal-basin."

This memoir is presented as the first of a series of communications resulting from researches made during the last summer.

A short sketch is given of the structure of that portion of the carboniferous limestone of the South Welsh coal-field, which, in Brecknock and Caermarthenshires, is contiguous to those older formations, which were the peculiar subject of the author's examination.

After noticing some features which are common to the mountain limestone in other districts, such as an oolitic structure, and the existence of caverns and funnel-shaped cavities, attention is specially called to a portion of the limestone near Gwinfe in Caermarthenshire, the exterior of which exhibits a high polish. As these polished beds protrude from the edge of a turf bog, it is suggested that such effects may have been produced by the long-continued action of a weak vegetable acid issuing from the morass, and altering the surface of the rock.

I. Old Red Sandstone.—The old red sandstone is divided into three groups.

a. "Conglomerate and sandstone." *b.* "Cornstones and marl." *c.* "Tile-stones."

a. The uppermost of these groups, occupying the loftiest summits the country described, such as the Brecon and Caermarthen Fans, is uniformly capped by a band of conglomerate, underlaid by a vast thickness of sandstone. Neither calcareous beds nor organic remains have been discovered in this group.

b. The central group is spread in undulating masses over the greater part of Herefordshire. The argillaceous red marls of which it consists, contain many beds of concretionary limestone or cornstone, with some strata of sandstone.

Remains of Crustacea have been found in this group, together with defences of fish, &c.

c. The tile-stones are best exhibited in a remarkably rectilinear escarpment, extending from the north-western extremity of the Mynidd Eppint to near the mouth of the Towey, a distance of about thirty-five miles.

These beds contain fossils in Caermarthenshire, and also in their north-eastern prolongation into Shropshire: among them are Lingula, Avicula, three or four species of univalves, a small species of Orthoceratite, &c. These fossiliferous tile-stones constitute the beds of passage into the "*Ludlow Rock*," or highest member of the grauwacke series.

The limits of certain detached basins of the old red sandstone, partially described during the last session, and which are spread over the area of the inferior grauwacke rocks, have this year been extended westward to the source of the Teme, and twenty-five miles to the north-west of the ancient line of demarcation. The absence of all vegetable remains, with the exception of a few small fragments, notwithstanding the full exhibition of the mineral structure of all the groups of the formation afforded by many natural deep sections, is insisted upon as demonstrating the hopelessness of ever finding coal in the old red sandstone of this part of the kingdom.

The maximum thickness of the whole formation is estimated to be from 10 to 12,000 feet.

II. Outliers of Carboniferous Limestone, &c.; Dislocations of the Old Red Sandstone.

A very remarkable outlier of carboniferous limestone and millstone grit is pointed out as occupying the summit of a mountain of old red sandstone to the south of the town of Crickhowell. This mass, called Pen Cerrig Calch, is distant from the main escarpment of carboniferous limestone from four to five miles, and is separated from it by the deep valley of the Uske. It is shown, by the position and slight inclination of the beds, that the limestone of Pen Cerrig Calch must have been connected with that of the main escarpment anterior to the excavation of the intermediate valley, and the case is cited as one of the deepest and most extensive denudations which has come within the author's observation.

Numerous complicated dislocations of great extent occur in that segment of the extensive margin of the South Welsh coal-basin which extends from the Caermarthen Fan to the latitude of Llandello. The largest of these breaks is the great upcast of Fan Sirgaer, by which the old red conglomerate is thrown up about 700 feet from its regular horizon at Cerrig Ogof. The greatest downcast has taken place at the spot marked by the occurrence of the glazed limestone; but the most extraordinary of all these disruptions is that which has given rise to the position of the singular outlier of carboniferous limestone called Castel Cerrig Cennen. This outlier, by a violent elevation of the old red sandstone, has been dismembered from its parent rock, and left insulated, with the dip of its beds reversed, in the centre of a valley of the old red sandstone.

By these great elevations and subsidences large masses of carboniferous limestone are (as it were,) thrown out *en echellon* from the circumference of the coal-field into the area of the old red sandstone.

This portion of the memoir concluded with a comparison between the local and violent disturbances which have broken up the mountain limestone and old red sandstone on the margin of the coal-field, and that much greater movement from north-east to south-west which threw up, in Brecknockshire and Caermarthenshire, the mural chain formed of the lower member of the old red sandstone, and the upper member of the grauwacke. It is also shown that this chain, though it passes in one part of its course within three miles of those disturbances, still preserves in an unbroken line its true strike and direction, as if unaffected by those subsequent convulsions of the coal measures.

ROYAL ASTRONOMICAL SOCIETY.

November 8, 1833.—The following communications were read:—

I. Report on Captain Foster's Pendulum Experiments. By Francis Baily, Esq.

During the recess, Mr. Baily has presented to the Council his Report on the Pendulum Experiments made by the late Captain H. Foster, which Report has been approved by the Council, and forwarded to the Lords Commissioners of the Admiralty, at whose request the inquiry was undertaken. Their Lordships have since been pleased to order the same to be printed at the public expense, and it will form a part of the seventh volume of the *Memoirs* of this Society. The following is a brief abstract of the contents of the Report.

Captain Foster took out with him four different pendulums, two of which were of the kind called Kater's invariable pendulum; but the other two were of a new construction, recommended by Mr. Baily, the one of iron and the other of copper, each of which was furnished with two knife-edges; so that, in fact, Captain Foster might be considered as having taken out six different and independent pendulums. The iron and copper pendulums are the property of this Society: the brass ones belong to Government. The places at which Captain Foster swung these pendulums are fourteen in number: viz. London, Greenwich, Monte Video, Staten Island, South Shetland, Cape Horn, Cape of Good Hope, St. Helena, Ascension, Fernando de Noronha, Maranham, Para, Trinidad, and Porto Bello. At all these places (with the exception of the experiments with one of the brass pendulums at South Shetland,) the results are very accordant, and show that the pendulum, even with its present imperfections, affords an accurate measure of the relative force of gravity at different latitudes. But Mr. Baily has detected some minute sources of error in this instrument which were never before suspected, and an attention to which will render future experiments much more valuable, and comparable with each other.

As it would be impossible in a short abstract, like the present, to enter into a comparative examination of the results at each place, it will be sufficient here to state the general result of all the six pendulums, at the several stations above mentioned. For the purpose of deducing these results with the least probable chance of error, Mr. Baily has adopted the usual method of minimum squares; whereby he finds that if we make

v = the number of vibrations at the equator,

g = the increase of the force of gravity,

L = the latitude of any other place on the globe,

V = the number of vibrations made by the same pendulum there,

the general formula

$$V = v\,(1 + g \sin^2 L)^{\frac{1}{2}}$$

becomes, in the present case, and according to Captain Foster's experiments,

$$V = (7441507482 + 38666418 \sin^2 L)^{\frac{1}{2}}.$$

The following table will show the observed mean value of all the pendulums at each station, as well as the computed value from the above formula, together with the error, or difference, thence arising: and it exhibits the *final result*; deduced from the whole of the experiments.

Stations.	Latitude.	Vibrations.		Difference.
		Observed.	Computed.	
Para	° ′ ″ 1 27 0 S.	86260·61	86264·30	−3·69
Maranham	2 31 35 —	86258·74	86264·60	−5·86
Fernando de Noronha	3 49 59 —	86271·20	86265·16	+6·04
Ascension	7 55 23 —	86272·25	86268·44	+3·81
Porto Bello	9 32 30 N.	86272·01	86270·32	+1·69
Trinidad	10 38 55 —	86267·24	86271·84	−4·60
St. Helena	15 56 7 S.	86288·29	86281·06	+7·23
Cape of Good Hope	33 54 37 —	86331·33	86333·90	−2·57
Monte Video	34 54 26 —	86334·36	86337·52	−3·16
Greenwich	51 28 40 N.	86398·90	86401·24	−2·34
London	51 31 17 —	86400·00	86401·40	−1·40
Staten Island	54 46 23 S.	86415·22	86413·58	+1·64
Cape Horn	55 51 20 —	86417·98	86417·54	+0·44
South Shetland	62 56 11 —	86444·52	86441·72	+2·80

The values in the last column indicate most clearly that the pendulum is powerfully affected by local circumstances; since the differences between the observed and computed results, in most of the cases, far exceed the probable errors of observation: and all the pendulums agree in their indication of the degree of intensity at the several places. This fact, however, is rendered more striking by combining all the experiments hitherto made with the invariable pendulum, by the several English, French and Russian voyagers. In this manner Mr. Baily has obtained fifty-one different places, where the pendulum has been swung: and at several of these stations it has been swung by more than one experimentalist. The results therefore being, in several instances, confirmed either by the experiments of various persons, or by various pendulums swung by the same person, show most decidedly that there is some local influence on the pendulum, at such stations, with the exact nature of which we are unacquainted; and which baffles all our efforts to deduce the true figure of the earth from pendulum experiments made at a *few* places only; for the results deduced from such experiments will vary according to the selection which is made of such stations. And it is a remarkable circumstance that the force of gravity seems to be greater in islands situate at a distance from the main land (such as St. Helena, Ascension, &c.) than it is on continents.

The compression of the earth, deduced from Captain Foster's experiments, is $\frac{1}{289\cdot48}$; Captain Sabine made it $\frac{1}{288\cdot40}$; the mean of the French and Russian experiments is $\frac{1}{267\cdot23}$; the mean of the whole combined is $\frac{1}{285\cdot26}$.

The total number of coincidences made by Captain Foster was more than 20,000; and occupied upwards of 3180 hours. The total time occupied by Captain Sabine was only 598 hours: the total time occupied by Captain Freycinet was only 367 hours: and the total time occupied by Captain Duperrey was only 256 hours. So that Captain Foster's experiments are five times more extensive than Captain Sabine's, and full two and a-half more extensive than the whole of the above experimentalists united. They have also the advantage of having been made with a greater variety of pendulums; and cannot fail to be duly appreciated in all inquiries connected with that important subject (the true figure of the earth) which they were intended to elucidate.

II. On the introduction of the Copernican Theory into England. By the Rev. Joseph Hunter. Communicated by W. Frend, Esq.

The work of Copernicus was published in 1543; the Ephemerides of Rheticus in 1551, and the Canons of Reinholdt in 1554. In 1556, the first English *Copernican* work was published, under the title of, "Ephemeris, anni 1557 currentis, juxta Copernici et Reinholdi Canones fideliter per Joannem Field, Anglum, supputata ac examinata ad meridianum Londinensem, qui occidentalior esse judicatur a Reinholdo, quam sit Regii Montis, per Hor. I. scr. 50. Londini, MDLVI. Sept. xii." Various particulars of this work and of its author were given in the paper.

III. Some particulars relative to the Life and Writings of the late Mr. Flamsteed, never yet published. By F. Baily, Esq., President of the Society, &c. These are given at large in the Monthly Notices of the Society.

IV. Transits of the Moon with Moon-culminating Stars observed at Cambridge Observatory, in the months of July, August, September and October 1833. By Professor Airy.

XL. *Intelligence and Miscellaneous Articles.*

PROF. STEVELLY'S MODE OF DETERMINING THE VARIATION AND DIP OF THE MAGNETIC NEEDLE.

THE mode in which Professor Stevelly uses the sextant in determining the dip and variation of the needle is as follows. The sextant is placed horizontally on its stand, with the centre of the mirror precisely over a point in a meridian line, and the telescope directed towards the compass needle placed at a short distance from it. The needle has at each extremity a fine point let into it, directed vertically, and is also placed on a stand, which has a screw elevating collar, and a second screw acting horizontally, and at right angles to the line of direction of the needle. By a little adjustment, the two points on the needle are made to coincide in the field of view of the telescope of the sextant; and the radius is then moved until the meridian mark (at such a distance as to make the parallax of the instrument inappreciable,) coincides with them. The degrees read off give twice the horizontal divergence of the needle. If an east point can be easily procured, a means of correcting this result will readily suggest itself. This mode may prove of great importance in experiments on diurnal variation, and as it requires but trifling expense, the investigation is opened to many who were deterred by the price of the apparatus ordinarily in use. With the dipping needle the process is precisely similar, the axis being so constructed that the points can be observed in conjunction, and a horizontal mark at a distance previously obtained.

(From a Correspondent.)

ON THE CAUSE OF THE METEOROLOGICAL PHÆNOMENA OBSERVED IN THE MOUNT'S BAY BY MR. HENWOOD.

To the Editors of the Phil. Mag. and Journal of Science.

Gentlemen,

The occurrence to which Mr. Henwood has alluded in your last Number (p. 103.), namely, the formation of clouds on Paul Hill, while the rest of the atmosphere was almost free from clouds, is easily accounted for, on the principle that vapour exists in the atmosphere in a quantity proportional to its temperature. If the temperature of the atmosphere be high, in comparison with what it had been, the heavens will be clear; if it become lower, vapour will be visible; or if still lower, rain will descend. The temperature of the sea in winter (as is well known) is usually higher than that of the land, especially in the beginning of winter, and in the evening*; clouds, therefore, at these times are frequently seen forming on the hills as the breeze passes from the sea. The sea gives out heat, which preserves what would otherwise be clouds in the form of transparent vapour: the land withdraws heat from the vapour, and causes the formation of clouds.

Of course, the relative temperature of places varies; consequently, clouds are sometimes seen over the sea, while the land is clear.

I remain, Gentlemen, respectfully,

February 4, 1834. J. F.

[Our Correspondent's explanation is substantially correct: no difficulty, nor any difference of opinion, however, could exist upon the subject.—Edit.]

ON LACTIC ACID. BY MM. J. GAY-LUSSAC AND J. PELOUZE.

Various opinions have at different times been entertained as to the nature of lactic acid. It was discovered by Scheele in 1780,

* Mr. Henwood's observations were made in November, in the evening.

and has been examined since, principally by Braconnot and Berzelius: the former obtained an acid from rice and beet-root, which he supposed to be peculiar, and gave it the name of nanceic acid, which has been since found to be identical with lactic acid.

Bouillon-Lagrange and L. Gmelin having suggested that lactic acid was merely impure acetic acid, it appeared probable to Berzelius that it might be a compound acid, analogous to the sulphovinic: he left the subject, however, in a state of uncertainty, admitting that the lactates in a pure state are unknown.

MM. J. Gay-Lussac and J. Pelouze exposed a considerable quantity of beet-root juice to fermentation, in a stove the temperature of which was constantly between 77° and 81° Fahr. After some days had elapsed, the tumultuous motion, known by the name of the *viscous fermentation*, took place throughout the mass: hydrogen gas, mixed with carburetted hydrogen gas, was abundantly disengaged. In about two months the fermentation ceases; the liquor becomes fluid again, and it is then to be evaporated to the consistence of a syrup. Crystals of mannite are then diffused through the liquor, and these, when washed with cold water and pressed, are extremely pure: the mass contains, besides, a sugar which had all the properties of grape sugar. In the fermentation of beet-root juice, it appears that the cane sugar is first converted into grape sugar and then into mannite; for when the fermentation is long continued, mannite only is obtained without grape sugar.

The product of evaporation is to be treated with alcohol, which dissolves the lactic acid, and precipitates some matters, which were not examined: the alcoholic extract is treated with water, which leaves another deposit. The solution is afterwards saturated with carbonate of zinc, which occasions a more abundant precipitation than the others. After concentration, the lactate of zinc crystallizes: it is then to be dissolved in water and heated with animal charcoal purified by muriatic acid. The solution is to be filtered while boiling, and on cooling, perfectly colourless lactate of zinc crystallizes: the crystals are to be washed with boiling alcohol, in which they are insoluble. By treating them successively with barytes and sulphuric acid, lactic acid is separated, which is to be concentrated *in vacuo*: by agitation with sulphuric æther, traces of flaky matter are separated.

Lactic acid thus obtained is quite colourless: if it should not be so, which is the case when the last crystallizations of the lactate of zinc have been operated upon, it is to be converted into lactate of lime, which is to be boiled with water and purified animal charcoal. The crystallized salt obtained is afterwards treated with boiling alcohol, which dissolves it; it is then to be redissolved in water and decomposed by oxalic acid. Thus prepared, it is always white and pure. A large quantity of milk, suffered to ferment long, and treated in the same manner, yielded an acid and salts which perfectly resembled the preceding and its compounds.

M. Corriol has lately discovered that a watery infusion of nux vomica, after having fermented for some days, deposits lactate of lime, which requires only to be successively treated with water and alcohol to be perfectly white. According to M. Corriol, nux vomica yields from 2 to 3 per cent. of this salt, and it also contains lactate of magnesia. These salts were purified with the greatest facility, and furnished an acid identical with the lactic acid of beet-root, rice and milk.

When concentrated *in vacuo* until it loses no more water, lactic acid is a colourless liquid of the consistence of syrup: its density at about 68° Fahr. is 1·215. It is inodorous: it is extremely acid. Exposed to the air it attracts moisture: water and alcohol dissolve it in all proportions; sulphuric æther takes it up more sparingly.

When boiled with concentrated nitric acid, it is converted into oxalic acid. Two drops, added to about 1500 grains of boiling milk, immediately coagulated it; but a larger quantity had no effect on cold milk: a small portion also coagulates albumen. It rapidly dissolves bone phosphate of lime. When boiled with acetate of potash it expels acetic acid: if added to a concentrated cold solution of acetate of magnesia, it soon occasions a granular white precipitate, which is lactate of magnesia, and a smell of vinegar is produced.

Lactic acid also gives a precipitate of lactate of zinc when poured into a strong solution of acetate of zinc. Lactate of silver is decomposed by acetate of potash, and acetate of silver is deposited in abundance. Lactic acid does not occasion any turbidness in lime, barytes or strontia water.

Of all the properties of lactic acid, the most remarkable, and which would be alone sufficient to characterize it, is the phænomenon of its sublimation. When the acid, of the consistence of a syrup, is gradually and cautiously heated, it first becomes very fluid, soon afterwards coloured, and yields inflammable gases, vinegar, a coaly residue, and a great quantity of a concrete white substance, the taste of which is both acid and bitter. This substance, pressed between folds of blotting paper, and thus freed mechanically from an odorous substance, is very soluble in boiling alcohol, from which it precipitates on cooling in the form of rhombic tables, which are of a brilliant white colour. These crystals are inodorous; their taste is acid, but much less so than the liquid lactic acid, which is probably owing to their slight solubility. They fuse at about 225° Fahr., the liquid resulting does not boil at a lower temperature than 482°, and emits white irritating vapours: on exposing a cold body to them, they condense in the same crystalline forms as those which produced them. These vapours are inflammable, and burn with a pure blue flame. If the operation is carefully conducted, there is no residue in the vessel in which the sublimation is effected: all the acid rises without alteration. These crystals repeatedly fused and sublimed do not lose any water.

The tendency of the sublimed lactic acid to crystallize is extremely remarkable, especially in the dry way. Thus, when it is fused in a glass tube, although great agitation be used, the acid cannot be prevented from crystallizing in perfectly well-formed crystals.

These crystals dissolve very slowly in water, and they could be obtained by concentrating the solution *in vacuo*. The solution remained limpid, and gradually became thicker, and had at last precisely the same appearance as the concentrated lactic acid obtained in the humid way.

The liquid acid, taking the mean of two analyses, was found to consist of nearly,

Carbon	41·	=	6 atoms.
Hydrogen	6·95	=	6 —
Oxygen	52·05	=	6 —
	100·00		

The sublimed or crystallized acid yielded nearly, taking the mean of three experiments,

Carbon	49·83	=	6 atoms.
Hydrogen	5·60	=	4 —
Oxygen	44·57	=	4 —
	100·00		

From these analyses it appears that the two acids differ merely in the fluid one containing two equivalents of water, while the crystals are anhydrous. Four analyses were performed, employing lactates instead of the liquid and crystallized acid. They differed but slightly; one gave,

Carbon	44·64	=	6 atoms carbon.
Hydrogen	6·36	=	5 atoms hydrogen.
Oxygen	49·00	=	5 atoms oxygen.
	100·		

By analysing the lactates, the equivalent of this acid was found to be 1019·7; and supposing, as the above results would indicate, that it is a compound of 6 atoms of carbon, 5 hydrogen and 5 oxygen, its equivalent would be 1021, and its composition would be,

Carbon	44·90
Hydrogen..................	6·11
Oxygen	48·99
	100·

It appears, therefore, that fluid lactic acid loses an atom of water when it combines with bases, and the concrete acid acquires one under the same circumstances.

Their composition may be thus stated:

Liquid acid.. = C 6, H 6, O 6; or C 6, H 4, O 4 + 2 water.
Acid in salts. = C 6, H 5, O 5; or C 6, H 4, O 4 + 1 water.
Sublimed acid = C 6, H 4, O 4.

Ann. de Chim. et de Phys., tome lii. p. 410.

MORPHIA IN POPPY SEEDS.

M. Accarie, an apothecary of Valence, infused six pounds of poppy seeds (*Papaver somniferum*) in boiling water; he obtained 250 grammes of extract of a pillular consistence: it had a weak smell of opium. It was boiled in water with magnesia: the precipitate, washed and dried, was treated with boiling alcohol, which dissolved the morphia, and 30 grains were obtained. M. Fontenelle remarks that if these experiments should be confirmed, it will prove the error of rejecting poppy seeds when the capsules are employed for making decoctions. —*Journal de Chimie Médicale,* July 1833.

ANALYSIS OF THE BLUE ARSENIATE OF COPPER OF CORNWALL.

According to M. Wachmeister, the powder of this mineral is of an olive-green colour: by gradual calcination it becomes of a pure blue and loses half of its water; then losing the rest of its water it becomes of a dark bottle-green colour.

To analyse this mineral it was fused in a platina crucible with three times its weight of carbonate of soda; washed with water, the residue treated with muriatic acid, excess of ammonia precipitated the iron and alumina, and dissolved the copper. The alkaline solution was saturated with muriatic acid: and the arsenic precipitated in the state of sulphuret by sulphuretted hydrogen gas: after this carbonate of ammonia gave a precipitate of phosphate of alumina, which was analysed by fusing with one part and a half of silica and six parts of carbonate of soda, &c. The results of the analysis were

Arsenic acid	20·79
Phosphoric acid	3·61
Oxide of copper	35·19
——— iron	3·41
Alumina	8·03
Silica	4·04
Quartz, silicate, &c.	2·95
Water	22·24
	100·26

The iron is probably in the state of protoxide, and the alumina only combined with water.—*Annales des Mines,* June 1833.

ORIGIN OF AZOTE IN ANIMAL SUBSTANCES.

MM. Macaire and Marcet have ascertained that the chyle and the blood of herbivorous and carnivorous animals are nearly of similar composition: this fact seems to show that, whatever be the nature of the food, animals, by means of their digestive powers, extract chyle of similar composition. The following was found to be the composition of the chyle of the

	Dog.	Horse.
Carbon	55·2	55·
Oxygen	25·9	26·8
Hydrogen	6·6	6·7
Azote	11·	11·
	98·7	99·5

And it was afterwards found that the similarity of composition existed with respect to the blood of these animals. On analysing the arterial and venous blood of the rabbit, the results obtained were,

	Arterial Blood.	Venous Blood.
Carbon	50·2	55·7
Azote	16·3	16·2
Hydrogen	6·6	6·4
Oxygen	26·3	20·7
	99·4	99·0

The annexed were the results of a comparative analysis of the excrement of the

	Dog.	Horse.
Carbon	41·9	38·6
Oxygen	28·	29·
Hydrogen	5·9	6·6
Azote	4·2	0·8
Earthy matter	20·	25·
	100·0	100·0

The authors observe, that the excrement of the dog contains much azote, while the small quantity contained in that of the horse may be attributed to an admixture of animal fluids, as the bile, mucus, &c.

MM. Macaire and Marcet have arrived at the following as the results of their experiments:

1st, That the elementary chemical composition, and especially with respect to the azote, is similar in the chyle of herbivorous and carnivorous mammifera.

2nd, That arterial blood contains as much azote, but less carbon than venous blood.

3rd, That the elementary chemical composition of the blood of graminivorous and carnivorous animals is identical, and the substances which they contain have also the same composition.

4th, That when equal weights of the two fluids have been perfectly dried, the blood of a mammiferous animal, whatever may have been the mode of its nourishment, contains more azote than its chyle.

5th, That the excrement of carnivorous animals contains more azote than that of herbivorous animals.

6th, That neither carnivorous nor herbivorous animals can exist upon food entirely free from azote.

7th, and lastly, That unless it be allowed that azote may be formed by the process of vitality, it must be admitted that what the chyle contains is supplied from the food, and that in the two classes of mammifera examined, respiration supplies the complement of that which is found in the blood.—*Ann. de Chim.*, tom. li. p. 371.

ACTION OF HEAT ON IODIDE OF AMIDINE.

M. Lassaigne remarks, that in making some experiments on the combination of amidine with iodine, which is easily obtained by gradually pouring an alcoholic solution of iodine into the solution obtained from the starch extracted cold from bruised grain, he found its fine deep indigo blue colour gradually disappear by the action of heat, and at about 175° to 195° of Fahr. it entirely disappeared, the fluid remaining transparent.

It was at first supposed that the iodide of amidine had been decomposed by the heat; but this was not the case, for on cooling, the blue colour gradually reappeared, and eventually became as dark as at first. This experiment of the alternate destruction and reproduction of colour may be several times repeated, provided the heat be not continued longer than is required to decolourize the liquor; and a few minutes' boiling beyond this point destroys the power of reproduction. In this, however, it did not appear that the iodine was volatilized by the vapour of water, as might be supposed; for it is found, in part, in the decolourized liquor in the state of hydriodic acid, mixed with a portion of undecomposed amidine; and the addition of a few drops of a weak solution of chlorine explains why the blue colour is reproduced by this chemical agent.—*Journal de Chimie Médicale*, August 1833.

COMPOSITION OF ATROPIA.

M. Liebig found that 38·2 parts of very white atropia, dried by exposure to the air, gave 97·9 of carbonic acid and 28 parts of water: 31·2 parts absorbed 5·9 of dry muriatic acid gas, 100, therefore, would take 18·59.

At the instant the acid came into contact with the atropia it melted, and yielded a yellow transparent mass, very soluble in water, and which reddened litmus. This salt by evaporation crystallized in brilliant star-shaped groups. According to this, the atomic weight of atropia is 2·400. If the quantity of azote which it contains be calculated by that of the muriatic acid which it absorbs, we obtain, according to the above analysis,

Azote	7·519
Carbon	70·986
Water	8·144
Muriatic acid	13·351
	100·

Calculated according to its atomic weight, it will be found to consist of

2 atoms of	Azote	177·036	7·55
22 ——	Carbon	1681·162	71·68
30 ——	Hydrogen	187·144	7·98
3 ——	Oxygen	300·	12·72
		2345·392	99·73

When atropia is heated with a solution of potash, ammonia is abundantly evolved.—*Ibid.* August 1833.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. R. Hocking at Penzance, and Mr. Veall at Boston.

Days of Month. 1834.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Penzance. Max.	Barometer. Penzance. Min.	Barometer. Boston $8\frac{1}{2}$ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Penzance. Max.	Thermometer. Penzance. Min.	Thermometer. Bost. $8\frac{1}{2}$ A.M.	Wind. Lond.	Wind. Penz.	Wind. Bost.	Rain. Lond.	Rain. Penz.	Rain. Bost.
Jan. 1	30·084	29·893			29·35	44	36			40	NW.		W.	...		...
☾ 2	30·414	30·305			29·83	45	30			36	NW.		NW.	0·04		...
3	30·114	29·911			29·63	51	44			39	W.		calm	·03		0·08
4	30·188	30·021			29·55	51	39			42	N.		calm	...		...
5	30·084	30·003			29·58	45	49			44	SW.		calm	...		·19
6	29·822	29·403			29·34	50	36			44·5	S.		calm	·20		...
7	29·632	29·528			29·12	46	35			39	SW.		calm	·06		·07
8	29·349	29·217			28·94	46	37			43	E.		E.	·26		·10
● 9	29·416	29·172			29·02	44	38			43	S.		calm	·20		·65
10	29·257	29·112			28·90	49	41			43	S.		calm	·10		·11
11	29·440	29·201			28·95	51	46			46	S.		calm	·14		·03
12	29·165	29·071			28·77	51	41			47·5	S.		calm	·09		·19
13	29·691	29·636			29·20	52	47			42	SW.		calm	·07		·03
14	29·669	29·493			29·13	50	42			48·5	S.		SW.	·09		·06
15	29·616	29·302			28·93	56	42			46	W.		calm	·10		·11
16	29·615	29·124			29·07	55	42			46·5	S.		calm	·31		·07
17	29·421	29·367			28·82	52	43			52	SW.		SW.	·11		·17
☽ 18	29·765	29·566			29·00	50	38			[illegible]	S.		W.	·02		·03
19	29·684	29·590			29·24	46	37			37·5	W.		SW.	·06		...
20	30·053	29·978			29·52	52	46			38	SW.		calm	...		...
21	29·933	29·869			29·38	53	44			48	SW.		W.	·01		...
22	29·968	29·617			29·11	54	44			53	W.		SW.	·57		...
23	29·897	29·784			29·22	58	53			33	SW.		W.	·01		·04
24	29·997	29·884			29·35	55	47			53·5	SW.		W.	·04		...
○ 25	30·172	30·098			29·52	54	37			46	W.		NW.	·06		...
26	29·942	29·869			29·26	55	46			52	SW.		W.	·05		·04
27	29·871	29·697			29·30	54	48			46	SW.		calm	·07		...
28	29·749	29·489			28·90	56	33			52	W.		calm	·06		·15
29	30·449	30·224			29·75	39	28			33·5	NW.		NW.	...		·29
30	30·360	30·197			29·92	47	36			34	[illegible]		calm	·12		...
31	30·195	30·146			29·67	48	34			44	S.		calm	...		...
	30·449	29·112			29·26	58	28			44·3				2·87		2·41

Remarks.

London. — Jan. 1, 2. Clear, and very fine. 3. Slight rain. 4. Cloudy. 5. Hazy: overcast, and fine. 6. Cloudy: heavy rain. 7. Fine. 8. Cloudy, heavy rain: foggy. 9. Foggy: hazy: rain. 10. Cloudy: fine. 11. Fine: rain. 12. Rain. 13. Stormy and wet. 14. Overcast and fine. 15. Heavy rain: fine. 16. Cloudy: rain at night. 17. Heavy rain: fine, with lightning at night. 18. Stormy showers. 19. Hazy: rain. 20. Overcast. 21. Cloudy and cool: rain: very fine at night. 22. Cloudy: heavy rain: very fine. 23. Slight rain. 24. Fine: rain. 25. Clear and fine. 26. Cloudy and drizzly: slight haze. 27. Hazy, with rain. 28. Rain: fine. 29. Clear and frosty. 30. Overcast. 31. Slight showers: overcast and fine. — The winter has been so remarkably mild, that towards the end of this month the Almond trees in many places about London were to be seen in blossom.

Boston. — Jan. 1, 2. Fine. 3. Cloudy: rain early A.M. 4. Fine: rain P.M. 5. Cloudy. 6. Cloudy: rain P.M. 7. Fine. 8. Rain: rain P.M. 9. Cloudy. 10, 11. Cloudy: rain early A.M. 12. Cloudy: rain early A.M. & P.M. 13. Fine. 14. Rain: rain P.M. 15. Cloudy: rain P.M. 16. Fine. 17. Rain. 18—21. Fine. 22. Cloudy: (three degrees warmer than 31st of August last). 23. Cloudy: rain early A.M. 24, 25. Fine. 26. Cloudy: rain early A.M. 27. Cloudy: rain P.M. 28. Fine: rain and stormy P.M. 29. Fine. 30. Cloudy. 31. Fine.

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[THIRD SERIES.]

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XLI. *On the Influence of successive Impulses of Light upon the Retina.* *By Sir* DAVID BREWSTER, *LL.D., F.R.S., V.P.R.S. Ed.*

FROM the remarkable experiment of Mrs. Griffiths, described in a former Number*, it appears that the reticulated structure of the retina may be rendered visible by throwing light suddenly on the closed eye when in a state of repose, and especially in the morning, before the retina has been subjected to the action of any other light.

In repeating this experiment, I have found that a remarkable structure may be exhibited at any time, and whether the eyes are open or shut, by subjecting the retina to the action of successive impulses of light. If, when we are walking beside a high iron railing, we direct the closed eye to the sun so that his light shall be successively interrupted by the iron rails, a structure resembling a kaleidoscopic pattern, having the *foramen centrale* in its centre, will be rudely seen. The pattern is not formed in distinct lines, but by patches of reddish light of different degrees of intensity. When the sun's rays are powerful, and when their successive action has been kept up for a short time, the whole field of vision is filled with a brilliant pattern, as if it consisted of the brightest tartan, composed of red and green squares of dazzling brightness. The green colours prevail chiefly at the centre, corresponding to the *foramen centrale*, and here we observe the dark-lined *network* pattern which I have described in Number 20†, and which is totally distinct from the reticulated structure noticed by

* See Lond. and Edinb. Phil. Mag., vol. iv. p. 43.

† *Ibid.* p. 119, *note.*

Mrs. Griffiths. The brilliancy of the spectrum thus produced, and the beauty of its colours, exceed any optical phænomenon which I have witnessed, and so dazzling is its effect that the eye is soon obliged to withdraw itself from its overpowering influence.

The very same phænomena may be seen by looking at the sun through the distended fingers when they are made to move backwards and forwards, or rather from right to left, and from left to right, in front of the eye.

The colours of the spectrum above described have their origin in the *red* light transmitted through the eyelids, the *green* tints being the accidental or complementary colour of the *red*; but the phænomenon may be seen in a great measure without colour by opening the eye, and interposing between the eye and the sun any white transparent ground, such as thin white paper or ground glass, or by directing the eye immediately to a bright sky, or to the ground when covered with snow. In these different forms of the experiment the effect varies greatly with the intensity of the light and the state of the eye, but the following general description of the phænomena will be found tolerably correct. In order to make the light produce a series of successive impulses on the retina, and on the same parts of it, I look through the openings of the revolving disc of the phenakistiscope with one eye, and fix it steadily upon the same point of the luminous ground.

When the disc revolves with great velocity, a very *faint* and uniform light is seen over the whole luminous surface. As the velocity diminishes, the light becomes less uniform, and a flickering or wavering motion commences. Patches of a *bluish purple* colour appear in different parts of the field, forming a sort of network, the intersections between the meshes of which are of a faint *lemon-yellow* colour, the accidental colour of the bluish purple. The pattern of this network is related to the centre or point on which the eye is fixed, and seems to belong either to a hexagonal or octagonal division of the circle. The centre of the pattern, corresponding to the *foramen centrale*, is a square or lozenge, one of whose diagonals is vertical; but as the differently coloured patches or elements of the pattern are constantly changing their colour, their intensity of light, and even their form, owing to the unsteadiness of the eye and the revolving disc, I have never been able to draw the pattern, or to trace how the patches or interstices of the net-work spring from the sides and angles of the central lozenge. That the reticulated structure is related to this central square or to the central foramen of the eye is unquestionable; and I have no doubt that observers who have younger eyes than mine, and who shall have the courage to repeat the experiments with the direct light of the sun, and with a disc having narrow slits, and revolving upon a fixed axis so as to have its velocity uniform, will be able to obtain an accurate representation of the pattern in question.

Within and around the central lozenge, is seen with great distinctness the dark-lined network pattern already mentioned, and apparently unconnected with the larger pattern. As the spaces, however, or patches, which compose the larger pattern diminish in size towards the centre, it is possible that the dark-lined network, with dark specks in the centre of the figures, and having all the regularity of a geometrical figure drawn with ink, may be the central part of the larger patterns seen more distinctly by direct vision; but I cannot admit this notion, because under favourable circumstances a similar dark-lined pattern, with extremely small spaces between the meshes, appears throughout the rest of the field of view, especially in the external part of it where it first begins to show itself.

The colours which appear in the principal pattern are chiefly *bluish purple*, and its complementary colour *lemon-yellow*, but as the former increases in depth or approaches to blackness, the latter becomes more white. These different colours sometimes appear in the different patches of the pattern, and sometimes they appear in succession over a considerable part of the field. They are, however, most distinctly seen in the central lozenge, the inner part being sometimes *purple* and then *yellow*, while the outer part of it is first *yellow* and then *purple*. The central lozenge is sometimes marked out by whitish, and sometimes by greenish light, and I have frequently seen in its centre a *blush-red* of a very rare tint. The succession of colours in the lozenge is very beautiful, each colour advancing to the centre, replacing that which preceded it, and then disappearing.

The cause of the colours themselves is obvious. The action of white light on the retina renders it first insensible to the *red* rays*, and consequently a white object or ground appears *bluish purple* or *blue* in solar light, and *green* in candle light, the colour varying with the intensity of the exciting light, and with the distance of the image of the white object from the excited point. The other colour which appears in the preceding experiments is a faint *lemon-yellow*, which is the complementary colour of the *bluish purple*. It deserves also to be noticed that these colours are the very same as those

* See Lond. and Edinb. Phil. Mag., vol. iii. p. 169.

produced by the action of light falling on the retina at a distance from the axis of vision. When we look, for example, indirectly, or rather obliquely, at a candle for some time, the image of the candle itself becomes *bluish purple* surrounded with a nebulosity of *yellow* light, the accidental colour being the invariable companion of the primitive one.

In order to explain why no colours appear during a very rapid rotation of the disc, and why the primitive and the accidental colour succeed each other in the pattern, let us call

T the time in which the disc revolves,
n the number of apertures in its margin,
D the duration of the impression of direct light, and
d the duration of its complementary colour.

It is obvious that $\frac{T}{n}$ will be the time which elapses between each consecutive impulse of light on the retina, or the time during which the eye has the opake part of the disc opposite the pupil. When $\frac{T}{n}$ is very small, or the velocity very great, or when $\frac{T}{n}$ is very much less than D, (D is = eight thirds, or nearly one eighth of a second,) the ground will be uniformly luminous, because the direct impression of the one aperture has not begun to fade away before the succeeding aperture makes a new impression. When $\frac{T}{n}$, however, is nearly equal to D, the impression of the direct light is nearly gone, and hence arises the flickering or wavering appearance of the luminous ground, which becomes a maximum when $\frac{T}{n} = D$; for when this takes place the direct impression of the one aperture is just gone before the other aperture renews it. When $\frac{T}{n}$ is greater than D, the accidental colour of the direct impression begins to show itself; and when $\frac{T}{n} = D + \frac{d}{2}$, the accidental colour will be about its brightest, and will be seen to succeed the direct impression, the latter being now *bluish purple*, and the former *lemon-yellow*. When $\frac{T}{n} = D + d$, the opake space between the apertures will begin to be visible, and the phænomena will disappear.

As the reticulated pattern is marked out by different colours, and even by the same colour in different states of intensity, it follows that different parts of the retina have different degrees of sensibility to light. The lines which form the network are probably thicker than the interstices between them, and consequently less susceptible to luminous impressions. In like manner the interstices nearest to the *foramen centrale* are probably thinner than those more remote, and hence it is easy to understand why they exhibit a greater sensibility and a more rapid change of colour. If these views are correct, we not only obtain a general explanation of the phænomena which we have described, but of many others which have hitherto perplexed the optical physiologist, and among these we may enumerate the phænomena of oblique vision, and the superior distinctness of objects when they are seen directly along the axis of the eye.

In a former paper I had occasion to mention a very remarkable fact, which I had long ago discovered, that the intensity of a given light may be increased physiologically by causing it to act upon the retina by successive impulses of a given duration. Those who may repeat the preceding experiments will obtain an ocular demonstration of the truth of this new property of light. The maximum physiological intensity seems to take place when $\frac{T}{n}$ is nearly equal to $D + d$.

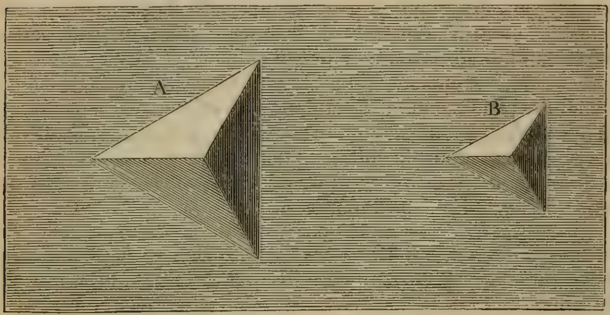
Belleville, Feb. 20, 1834.

XLII. *Account of a Rhombohedral Crystallization of Ice. By Sir* David Brewster, *LL.D., F.R.S., V.P.R.S. Ed.*

VARIOUS crystallizations of ice have been observed and recorded; but the primitive form of its crystal has not yet been determined. I have long ago shown that ice possesses double refraction; that it has only one axis; and that this axis is perpendicular to the surface of well crystallized plates of ice formed on the surface of still water. Hence it follows that the primitive form of this kind of ice must be either a rhombohedron, a right prism with a square base, or a pyramid with a square base. Crystals of ice of all these forms have been seen, I believe, by different individuals either in hoar frost or in icy caverns; but in no case has the optical structure of these crystals been submitted to examination.

On the forenoon of the 2nd of February, after a night of very slight frost, I observed a circular basin of water, which was protected from the agitation of the air by a stone parapet, covered with a sheet of perfectly transparent ice without a single air bubble. Upon examining it more narrowly I was delighted to observe the summits of two very flat rhombohedrons, hollow within, and raised above the general level of the ice, as shown at A and B in the annexed sketch, the axis of the rhombohedrons being nearly perpendicular to the surface

Fig. 1. Fig. 2.



of the plate of ice. When the plate of ice was exposed to polarized light, it exhibited, in a direction perpendicular to its surface, the positive uniaxal system of rings.

It was impossible to obtain even the rudest estimate of the angles of the rhombohedron, owing to the warmth of the day, and the distance which I had to carry the ice.

The crystallization now described exactly resembled specimens which I have seen of the *Chaux carbonatée basée* of Haüy, on the flat surface of which were formed several obtuse summits of the rhombohedron of calcareous spar, having their axes perpendicular to that of the plate.

Belleville, Feb. 19, 1834.

XLIII. *Additional Observations on the Use of Chemical Symbols.* *By* Richard Phillips, *F.R.S. L. & Ed., Lecturer on Chemistry in St. Thomas's Hospital.*

To Thomas Graham, Esq., M.A., F.R.S. Ed., &c.

Dear Sir,

YOUR letter contained in the Philosophical Magazine for February last does not, I regret to say, contain the information which I asked, nor dissipate the obscurities in which symbols have shrouded your facts. I take the liberty, therefore, of addressing you more particularly on the subject, for I am anxious to comprehend the steps by which you have arrived at the important conclusions contained in your paper on the arseniates and phosphates; and I shall avail myself of the present opportunity of replying also to Mr. Prideaux's "Remarks", published in the Magazine for January.

I remain, dear Sir, yours faithfully,

R. Phillips.

I presume that the following terms used in your paper in the Philosophical Transactions, all refer to the same salt, viz.

Page 253, "Phosphate of soda."
—— 255, "Common rhomboidal phosphate" of soda.
—— — "Common phosphate of soda."
—— 256, "A phosphate of neutral composition, such as the common phosphate of soda."

You also state (p. 255.) "that common phosphate of soda, for instance, is a phosphate of soda and of water, and that its symbol is $\dot{Na}^2\dot{H}\overset{\cdot\cdot\cdot\cdot\cdot}{P}$;" and I cannot here but remark, that even while asserting the system of notation which you adopt is that last proposed by Berzelius, you admit that two out of three symbols do not occur in it. His symbol for water is $\underline{\dot{H}}$, yours $\dot{H}$; he supposes it to contain two atoms of hydrogen, you admit but one atom; he represents phosphoric acid by $\underline{\overset{\cdot\cdot\cdot\cdot\cdot}{P}}$, you by $\overset{\cdot\cdot\cdot\cdot\cdot}{P}$; he supposes it to contain two atoms of phosphorus; you but one atom. On these, as well as some other grounds, I cannot agree with you that a system is "convenient" which requires so much patching, from the hand even of a professed admirer, to make it express his opinions both as to theory and as to facts.

But to proceed. In your letter to me you regard crystallized phosphate of soda as consisting "of two atoms of soda and one atom of basic water united to the atom of phosphoric acid, together with twenty-four atoms of water of crystallization." Now allow me to inquire in what sense the term *neutral* is applied to this salt in p. 256. of your paper? The above account of its constitution shows that it is a subphosphate, and consequently it cannot be *atomically* neutral; it turns turmeric brown, and therefore is not *chemically* neutral: on the other hand, you cannot consider it as a subsalt, though it contains two atoms of base to one atom of acid; for (p. 253.) you direct the subsesquiphosphate to be formed by adding to "phosphate of soda at least half as much soda as already in the salt." These directions would be quite proper for the conversion of a neutral into a subsesquisalt; but a salt which consists of two atoms of soda united to one atom of acid is a *disalt* and consequently already contains one third more soda than the subsesquisalt proposed to be formed by adding one half more.

This difficulty may, perhaps, be partly explained by referring to page 262 of your paper, where, alluding to a particular compound, you add in a note, " assuming the double atom of phosphoric acid $(\overset{\cdot\cdot\cdot\cdot\cdot}{P}) = 892{\cdot}3$." Now, I do not observe in the tables of Berzelius any mention of a *double atom*; and the atom is there represented, *not* by $\overset{\cdot\cdot\cdot\cdot\cdot}{P} = 892{\cdot}3$, as you state, but by $\overset{\cdot\cdot\cdot\cdot\cdot}{\underline{P}} = 892{\cdot}3$. Let, however, $\overset{\cdot\cdot\cdot\cdot\cdot}{P}$ in page 255 of your paper have the same meaning as in page 262, and then the salt will be neutral according to your notation, though not according to your statement.

In concluding this part of the subject, permit me to express a wish that you would furnish me with a table of acids and salts similar to that in page 282; but instead of containing an account of the atoms of oxygen in the soda, water and acid, I wish for a simple statement of their composition, and the atomic weight of each. I would save you the trouble if it were in my power; but I really cannot accomplish it with any degree of certainty or satisfaction, on account of the discrepancies which I have pointed out, and some others which your paper contains.

With respect to Mr. Prideaux's " Remarks," on my " Observations," I shall now show that they are inaccurate both with regard to the facts and fancies of symbolizing. He says " there is but one known combination of phosphorus with five atoms of oxygen; in which many chemists (chiefly in this country) apprehend the phosphorus to enter as a single atom, whilst others regard it as double, from its requiring 2 atoms of alkali." This is an admission that, according to most foreign chemists,—and Berzelius, be it remembered, is of the number,—there is no compound resulting from the union of a single atom of phosphorus with five atoms of oxygen; and consequently, while Berzelius symbolizes phosphorus by P, and two atoms by $\underline{P}$, he adds five dots to the latter, denoting its conversion into an atom of phosphoric acid $\overset{\cdot\cdot\cdot\cdot\cdot}{\underline{P}}$.

Mr. Prideaux further observes: " $\dot{H}$ may signify, according to our atomistic notions, either pure or oxygenated water." Undoubtedly it may, or anything else which anybody chooses that it should: with Berzelius, however, it is equivalent to half an atom of oxygenated water. If, therefore, those whose atomistic notions differ from his, employ it to signify mere water or oxygenated water, one of two things must occur, viz. that half an atom may be confounded with a whole one, or to prevent it, the innovation must be explained in words which will occupy more time and space than the meaning of the symbol written at full length.

It is singular that Mr. Prideaux should allow of such innovations in a system which he had previously represented as absolutely perfect, and as "exhibiting, in signs no less natural than compact, the entire constitution of the substance through all the grades of analysis."— Phil. Mag. and Annals, N.S. vol. x. p. 104.

After alluding to some embarrassment attendant upon the use of chemical symbols, Mr. Prideaux adds, "but in the specimen given (at p. 445) there is confusion of another kind, originating, apparently, in the specimen itself. The common ingredient oxygen, is represented in very different proportions," which Mr. Prideaux accordingly proceeds to prove. Now, as I am the author of the specimen alluded to, the confusion must have originated with me, and not "in the specimen itself." I think, however, Mr. Prideaux might have seen that my intention was not to describe a salt the composition of which was open to discussion, but an imaginary compound, as to the constitution of which all should agree. Still, however, I shall reply to Mr. Prideaux's objections to statements which I neither did nor intended to make. "Rose and Johnstone," he says, "would doubtless place 2 before the symbol of soda." Very likely; but then they would have written $\overset{\cdot\cdot\cdot\cdot\cdot}{P}$, while I have given $\overset{\cdot\cdot\cdot\cdot\cdot}{P}$ to express an acid "in which many chemists (chiefly in this country) apprehend the phosphorus to enter as a single atom," and therefore only one atom of soda to form phosphate of soda.

Mr. Prideaux then goes on to "correct, or rather restore," the symbols for crystallized phosphate of soda, which I have either mistaken or mutilated; and thus mended, they are stated to be as follows:

"Rose...... $2\,Na\,O + P\,O^5 + 24\,\underline{H}O.$

Turner.... $\dot{So} + P + 2\frac{1}{2}O + 12aq.$

Johnstone $\overset{\cdot\cdot\cdot\cdot\cdot}{P} + \dot{So} + 24\,\dot{H}.$

My scale $\dot{So}^2\ \underline{\overset{\cdot\cdot\cdot\cdot\cdot}{P}h}\ Aq^{24}$, they all harmonize with

Berzelius $\dot{Na}^2\,\underline{\overset{\cdot\cdot\cdot\cdot\cdot}{P}} + 24\,\underline{\dot{H}}$, and are a common language of science, applicable to all nations, and easily acquired."

If, as Mr. Prideaux asserts, these symbols exhibit harmony, it is to me of that variety which has been characterized as "all discord." And it is curious that Johnstone's symbol is the same when mutilated by me as when mended by Mr. Prideaux; and it is still more curious that with my view it was correctly employed, while with the *no* mending of Mr. Prideaux it is wrong. Now that I, who have never written symbols, nor read them when I could avoid it, should exhibit symptoms of a novice in attempting to employ them, ought to excite no surprise; but that a professed admirer of a system—and which, though perfect, he has attempted to improve—should err in correcting my blunders, is surely indicative of "something rotten" in the system itself: and I shall now show that Mr. Prideaux, in attempting to correct, or rather to restore *three* symbols, has committed *four* errors; for I maintain that, instead of the notation as above given by him, crystallized phosphate of soda would have been symbolized as follows: by

Rose......... $2\,\mathrm{Na\,O} + \underline{\mathrm{P}}\,\mathrm{O}^5 + 24\,\underline{\mathrm{H}}\,\mathrm{O}$.

Turner...... $\dot{\mathrm{S}} + \overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}} + 2\tfrac{1}{2}\mathrm{O} + 12\tfrac{1}{2}$ aq.

Johnstone... $\underline{\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}}} + 2\,\dot{\mathrm{So}} + 24\,\dot{\mathrm{H}}$.

From the use of the words "my scale," I find that Mr. Prideaux has published a scale of equivalents: and I regret that I did not remember it. My idea of his notation was taken from a paper already quoted, on account of the praise bestowed upon the system of Berzelius, and in which the only innovation that he professes to have made in it is that of "writing salifiable bases in inclined letters." In that paper Mr. Prideaux had no occasion to use the symbol either for phosphoric acid or soda; but as he employs S for sulphur and K for potash, I concluded, after what had been stated, that he also admitted with Berzelius P for phosphorus and $\dot{\mathrm{N}}$ for soda. I find, however, I was wrong; and I again express my regret at not remembering Mr. Prideaux's scale, for his accumulated innovations would have added variety and confusion to my illustrations of both.

Although Mr. Prideaux has so greatly altered Berzelius's notation, that, as the reader will perceive, all the constituents of his scale in crystallized phosphate of soda are totally dissimilar, yet Mr. Prideaux has observed that "one should be well convinced, before attempting innovations, that their effects [defects?] and inconveniences will be less than those of what they are proposed to supersede." I cannot discover the advantage which $\dot{\mathrm{So}}$ possesses over $\dot{\mathrm{Na}}$, $\underline{\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{Ph}}}$ above $\underline{\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}}}$, or Aq^{24} over $24\,\dot{\mathrm{H}}$; and yet Mr. Prideaux has made these alterations in a system which he formerly characterized as one "in which we see the acid and alkali, and the elementary composition of each: the bases expressed in their initial letters; and oxygen, the great modifier of chemical properties;—the pivot, as it were, round which chemical agency revolves,—is exhibited in a manner equally characteristic, prominent and compact, by dots, corresponding to the number of atoms placed over the symbol." Surely any alteration effected in such a system must be for the worse.

Mr. Prideaux, alluding to another set of symbols which I quoted in my observations, says, "Warrington's, *as there printed*, appears to signify quadroxide of potassium, soda, and 24 water." By the words *as there printed*, in italics, Mr. Prideaux intends, I presume, to insinuate that they are so printed nowhere else. Mr. Prideaux ought, however, to know that the privilege of changing symbols is not confined to himself and Berzelius. Mr. Warrington proposed a notation in the Lond. and Edinb. Phil. Mag., vol. i., Sept. 1832, in which ammoniacal alum is symbolized by $3(\dot{al}+\dddot{S}) + ((3\,H).+\dddot{S})$; but as a proof of the facility with which are

"Atoms and systems into ruin hurl'd,"

I may state that Mr. Warrington, dismissing his first system, has projected a new one, in a "Series of Chemical Tables," printed in the past year. In this the atom of alumina is represented by $\overset{o}{Al}$, sulphuric acid by S^{ooo}, and ammonia by N^{hhh}: how these would be arranged so as to symbolize alum, I shall not attempt to determine. On a microscopic examination of Mr. Warrington's new symbols, I am ready to confess that I did them, though quite unintentionally, some injustice; for I observe that the O following the P is rather larger than the OO over which it is placed, whereas they ought to have been of the same size; but if an O too large or placed too high up converts phosphate of soda with 24 atoms of water into quadroxide of potassium, soda, and 24 water, Mr. Prideaux will not, I presume, include my friend Mr. Warrington's system among those which "are a common language of science, applicable to all nations, and easily acquired."

In concluding, I must confess, that the language of symbols is to me

A Babylonish dialect
Which learned *chemists* much affect;
It is a party-coloured dress
Of patch'd and piebald languages:
'Tis English cut on Greek and Latin,
Like fustian heretofore on satin.

XLIV. *On the External Structure of Imperfect Plants. By the Rev.* Patrick Keith, *F.L.S.**

IF a plant of the imperfect class is taken and inspected minutely, at any season, it will be found to be defective, or apparently defective in one or other of the more conspicuous parts or organs of vegetables in general, such as the root, stem, leaf, blossom, seed. Yet in the class of imperfect plants, as in that of the perfect plants already surveyed, the eye readily recognises traces of a similitude or dissimilitude of external habit and deportment characterizing the numerous individuals of which it consists, and suggesting the idea of distinct tribes or families. Upon this principle botanists have instituted different divisions, more or less extensive, according to their different views of the subject; and some of their late alterations we regard as improvements. The student who wishes to make himself acquainted with the divisions and subdivisions that have been recently introduced into the class *Cryptogamia*, will find a very good view of them in the first numbers of Burnett's Outlines of Botany; though we cannot agree with the learned professor in thinking that the study of the simplest structures is that which the tyro in botany ought to begin with†. This is the converting of a pleasing and agreeable study into a difficult and disagreeable task. It is the seizing of the bull by the horns at the first onset. The study of the ascending scale is both delightful and instructive, when the mind is duly prepared for it. But the mind of the novice is not duly prepared for it, and cannot yet seize the rude and uninviting types of the lower grades of vegetables so as to enable him to study them either pleasurably or profitably, if we except not the singularly inviting type of the Tartarian Lamb. For as in the comparative anatomy of animals the student is always presumed to have made himself well acquainted, already, with the anatomy of the human body, so in the study of the comparative forms of vegetables, the student who has already made himself well acquainted with the structure of the higher grades, whose parts and organs are well developed, will find himself in the best capacity to enter upon the study of the structure of the lower grades, whose parts and organs are minute and microscopic. This it may be said is mere matter of opinion, and as such we are content to have it regarded. It detracts nothing from the value of the view which the professor exhibits. It relates merely to the description of students for whom it is best

* Communicated by the Author.

† Preface to Outlines, xiii.

adapted. The following brief sketch of the external structure of Imperfect or Cryptogamic plants embraces generalities merely, and does not profess to enter into detail. On this account we content ourselves with the old division by which they are distributed into *Filices*, *Musci*, *Hepaticæ*, *Algæ*, *Fungi*; or, in plain English, Ferns, Mosses, Liverworts, Flags, Mushrooms, without entering into the detail of their several subdivisions.

The Filices, or Ferns.—Ferns are herbaceous, and for the most part, stemless plants, dying down to the ground in the winter, but furnished with a perennial root, from which there annually issues a frond bearing the fructification. The favourite habitats of many of them are heaths and uncultivated grounds, intermixed with furze and brambles.

> *Neglectis* urenda *filix* innascitur *agris.*—*Hor.* lib. i. Sat. iii. 37.

But the habitats of such as are the most luxuriant in their growth are moist and fertile spots, in shady and retired situations, as on mossy dripping rocks, or by fountains and rills of water.

The root of the tribe of ferns assumes a great variety of different aspects in different species. In *Botrychium Lunaria* it is fibrous; in *Aspidium dilatatum* it is tuberous; and in *Polypodium vulgare* it is creeping and covered with scales. In *Pteris aquilina*, or common brakes, it is sometimes described as being spindle-shaped; yet this is not strictly the fact. If a frond is taken and pulled up with the hand, the lower portion of it is, indeed, spindle-shaped; but the real root from which you have thus detached the frond remains still in the soil, extending in a horizontal direction at the depth of from three to four inches below the surface; sometimes simple, and sometimes branched, but always furnished with lateral fibres. M. Du Petit Thouars regards this production, not as a root, but as a peculiar organ which he calls *souche souterrain**, the subterranean stock. Yet what is a *souche souterrain* but a different name for the *caudex descendens* of Linnæus, and consequently a real root; and if it is not a real root, where are we to find the root of this fern? After all, if botanists have agreed to call this under-ground production a *subterraneous stock*, rather than a root, I can have no objection to their nomenclature.

The trunk of ferns, if trunk it can be called, is a stipe supporting the frond, or rather the whole of the herbage is a frond, that is, an incorporation of stipe, leaf and fructifica-

* *Cours de Phyt.*, Séan. i. 78, 99.

tion. Yet in *Equisetum* the trunk is a jointed and upright stem; in *Pilularia* it is jointed and trailing; in *Lycopodium* it is lash-shaped and creeping. Ferns strictly so called have no branches; yet in the *Lycopodiaceæ* and *Equisetaceæ* branches are to be found. In the latter they issue in whorls; in the former they do not originate in any regular order. Among ferns, leaves are not to be met with, at least as distinct and separate organs, except in *Lycopodiaceæ* and *Marsiliaceæ*, which tribes, together with the *Equisetaceæ*, have so much that is peculiar to themselves respectively, that botanists now arrange them as distinct orders*.

It was for a long time thought that ferns were destitute of seeds and propagated nobody knows how; yet there is no botanist of the present day who doubts the reality of fern seed, or at the least of *sporules* from which new plants spring. Some botanists have even fancied that they had detected the parts of the antecedent flower †. But admitting that such detection is impracticable, the botanist can, at least, direct his attention to the mode of fructification, and to the fruit or *sporule* produced. In ferns strictly so called the fructification is dorsal, that is, scattered in patches or clusters—*sori*—on the back of the frond. These patches are generally accompanied with an integument called the *indusium*, which at the period of the maturity of the seed bursts open, sometimes towards the nerves, and sometimes towards the margin, but in plants of a similar habit uniformly in a similar manner. Hence its utility in determining natural genera, first discovered by Sir J. E. Smith ‡. When the *indusium* bursts, the fruit, now ripe, escapes, which is, for the most part, a capsule surrounded by an elastic and jointed ring, opening transversely, and discharging the inclosed seed or *sporule*, which is a small and minute globule, discoverable only by the aid of the microscope. In *Lycopodium* the fructification is axillary, in *Equisetum* it is terminal, and in *Isoetes* and *Pilularia* it may be said to be radical.

The Musci, or Mosses.—The mosses are a tribe of imperfect plants of a small and diminutive size, consisting often merely of a root, surmounted with a tuft of minute leaves, from the centre of which the fructification springs, but furnished, for the most part, with a stem and branches, on which the leaves are closely imbricated. Their most favourite habitats are bleak and barren soils, such as mountains, heaths, woods, where they are not only rooted in the earth, but attached also

* Hooker's British Flora, p. 438. † Hedwig, *Theor. Fruct. et Gener.*
‡ Smith's Tracts, 227.

to the roots and trunks of trees, and even to the bare and flinty rock.

> .... Ego laudo ruris amœni
> Rivos, et *musco circumlita saxa.*—*Hor.* lib. i. Epist. x. 6.

As they affect the most barren soils, so they thrive best also in the coldest and wettest seasons; so that even the chilling blast of winter, that deprives other plants of their foliage and threatens destruction to the race of vegetables, serves but to refresh and to revive the family of the mosses.

The root consists generally of a number of small and slender fibres, closely matted together, as in *Tetraphis viridula*; smooth, as in most examples; or covered with a fine and velvety down, of a dark or rusty colour, as in *Bryum ligulatum*, one of the most beautiful of all British mosses. Some mosses are altogether stemless, as in the case of *Phascum muticum*; but where a stem exists, it is generally like the root, weak and slender, though sometimes stiff and shrubby, as in *Hypnum alopecurum*. The branches are in their structure similar to that of the stem, and are distributed, in certain genera, according to some uniform and specific mode; in others, without any regular order. The leaves of the mosses are exceedingly minute, but are, at the same time, exceedingly elegant. The most frequent forms are the linear, the lanceolate, the oval, the concave. They are always, as I believe, sessile, though often decurrent, or sheathing, with the margin beautifully waved. Some are entirely smooth, as in *Hypnum splendens*; others are beautifully dotted, or reticulated, or streaked, as in *Hypnum striatum*. The *Buxbaumia aphylla* of Schmidel was thought to be wholly leafless, till Dr. Brown at last detected its leaves*.

The fructification of the mosses, though extremely elegant in its structure, is yet at the same time so extremely minute as to be but seldom noticed, except by botanists. The ancients regarded the mosses as a tribe of plants originating in the putrefaction of other vegetables, and consequently as producing neither flower nor fruit. But this doctrine began to give way along with the doctrine of equivocal generation. Dillenius seems to have been the first to catch a glimpse of the truth with regard to the fructification of the mosses, as we may gather from his appendix to his catalogue of flowers growing in the neighbourhood of Gisse†; but Micheli was the first of all botanists who obtained a complete view of their sexual apparatus ‡. Linnæus followed up the investigations

* Linn. Trans. vol. xii. p. 583. † *Gissæ*, 1719. 8vo.
‡ *Nov. Plant. Gen.* p. 108. tab. lix. 1729.

of his predecessors, but did nothing to elucidate the subject; while Hill, by sowing the powder of the little capsules, obtained as the result of his experiment a crop of young mosses. Finally, Hedwig, born, as it was said, to abolish cryptogamy, perceiving the disorder and obscurity in which everything lay relative to the fructification of the mosses, undertook the arduous but indispensable task of investigating everything *ab initio*, and ultimately arrived at the conclusion, that the mosses are universally furnished with everything necessary to the botanical notion of a flower, and that their flowers are chiefly diœcious, partly monœcious, and partly hermaphrodite.

According to Hedwig, the barren flowers are the stars, disks, or buds that terminate the branches and nestle in the bosom of the leaves. In their exterior they consist of leaves or scales, larger or more elegant than the other leaves of the plant, but never terminating in hairs. These Hedwig regards as constituting the calyx. In the interior they consist of a number of small thread-shaped and succulent substances, issuing from between the leaves, or occupying the centre. These he regards as stamens, furnished with an anther that bursts open when ripe and discharges a pollen*. The fertile flowers are of a most singularly curious construction. They consist of an urn-shaped capsule surmounted with a calyptra or veil, in the form of an extinguisher, and invested, at the base, with a membrane called a *perichætium* or fence. Some writers have called the fence a calyx, and the veil a corolla; but we believe there is no unanimity among botanists on this point. The urn is sessile as in *Phascum muticum*, or surmounted on a pedicle as in *Polytrichum commune*. When the veil falls, or is forcibly torn off, its mouth appears, covered with a lid or *operculum*; and when the lid falls, the mouth is found to be furnished with a circular row of fine and tooth-like substances called the *peristomium* or fringe. Within the urn, and in the direction of its longitudinal axis, there is situated a slender and cylindrical substance called the *column*. Its summit, which overtops the urn, Hedwig regards as the style, and the urn itself as the seed-vessel, which, when ripe, is found to contain a multitude of spherical granules, from the sowing of which Hedwig obtained a crop of young mosses†. Hence if Hedwig's theory should be even erroneous, as it is thought by many to be, it establishes at least one fact, namely, that the granules of the urn or column are capable of reproducing the species.

The Hepaticæ, or Liverworts.—The liverworts are a tribe

* *Fund. Hist. Nat. Musc.* chap. ix. † *Ibid.* chap. x.

of small and herbaceous plants resembling the mosses, but chiefly constituting fronds, and producing their fruit in a capsule that splits into longitudinal valves. Their name is derived from a Greek word, ἧπαρ, ἥπατος, signifying the liver; because, perhaps, some of them were formerly employed as a remedy in diseases of the liver, or exhibit a slight resemblance to the lobes of that organ. Their favourite habits are wet and shady spots by the sides of springs and ditches, and their vegetation is always the most rapid in cold and damp weather.

Many of them have no root, or at least no conspicuous root, as *Jungermannia asplenioides*; but where a root is present it is fibrous. In the greater number of them the herbage is frondose, though not upright as in the ferns, but creeping along upon the surface of the soil, and striking root as it extends. It is rather lobed than leaf-like, with the lobes overlapping, and exhibiting under the microscope a fine network of vesicles frequently transparent.

The fructification of the liverworts is analogous to that of the mosses. According to Hedwig, the barren flowers are either small and globular protuberances issuing from the summit of the plant, or small and minute granules imbedded in the body of the frond, or in target-shaped substances elevated on a conspicuous pedicle as in *Marchantia*. The fertile flowers, for the most part, but particularly in the genus *Jungermannia*, are furnished with a double envelope; the outer corresponding in some degree to the calyx, and the inner, which immediately invests the ovary, to the corolla of perfect plants. If the flower is left to ripen, the envelopes will, in the process of fructification, burst open at the top, and discover a small protruding globule, of a black or brownish colour, and of about the size of a millet-seed, which is by and by disengaged from the envelopes entirely, and elevated on a fine and thread-shaped pedicle from a line to an inch in length. This elevated globule is the ovary, which when ripe separates into four longitudinal valves, from the extremities of which a number of small spiral and elastic threads issue, to which the seeds or *sporules* are attached.

The Algæ, or Flags.—The term *Algæ*, which is of Latin origin, and which we translate Flags, seems, primarily, to have denoted any sort of plant or herb growing in sea-water.

...... Cras foliis nemus
Multis, et *alga* littus *inutili*,
Demissa tempestas ab Euro,
Sternet.—*Hor.* lib. iii. Ode xvii.

Yet botanists have extended its application to many plants that are not even aquatics, agreeing, however, in the common character of having their herbage frondose, and their frond for the most part without a root.

Where a root exists, it is merely a fibrous or scutate base, for the purpose, not of nourishment, but of attachment, as in the *Fuci.* The frond is not uniform in its appearance throughout the several divisions of the order. In the *Tremellineæ* it is gelatinous, as in *Palmella nivalis*—the substance that gives colour to the polar snows *; in the *Confervoideæ* it is jointed and filamentose; in the *Ulvoideæ* it is membranous; in the *Fuci* it is coriaceous, or leather-like; and in the *Lichenes*, which do not well associate with the rest of the *Algæ* it is powdery, crustaceous, gelatinous, or even shrub-like.

The fructification of the *Algæ* is less perfectly known than that of any of the preceding orders; and yet it has received, like them, considerable elucidation from the pen of Hedwig; but chiefly from that of more recent writers, among whom we may specify the names of Vaucher, Agardh, Hooker, Greville, Fries. "It consists merely of seeds or sporules in tubercles, or in processes issuing from the frond, or immersed or more or less scattered on the surface †." In the Lichens there issues from the edge or general surface of the frond—*thallus* —a number of small warts or tubercles, of the colour or contexture of the general herbage, and containing a multitude of small granules, which Hedwig regarded as particles of pollen. Later investigators regard them as being mere *propagula*, From a different part of the frond there issues also a number of cup-shaped or target-shaped substances—*apothecia*—containing multitudes of small and minute granules, which Hedwig regarded as seeds. They are now reduced to the rank of sporules. Several of the *Fuci* are edible and much relished by many people whether raw or dressed: the *Lichen pulmonarius* is employed in medicine, and the *Lichen Perellus* in the art of dyeing.

The Fungi, or Mushrooms.—The *Fungi* are a tribe of plants whose herbage is a frond of a fleshy or pulpy texture, quick in its growth and fugacious in its duration, and bearing seeds or gems in an appropriate, exposed membrane, or containing them interspersed throughout its mass. Plants of this order are usually regarded as the lowest in the scale of vegetable being, and as exhibiting a considerable resemblance to the animal tribe of zoophytes. The habitats which they affect differ with the species; some on the surface of the earth, some buried under it; others on stumps and trunks of rotten

* Hooker's British Flora, p. 454. † *Ibid.*

trees; others on decayed fruit or on decaying cheeses, as the Mucors; others on damp and wet walls; and others on animal ordure.

Many of the *Fungi* are altogether destitute of root, or at least of any conspicuous root, being attached to some appropriate basis of support merely by means of a flattened and adhesive surface. If any of them exhibits a root, it is nothing more than a few fibres. The frond is thin and flat, or globular, or bell-shaped, as in *Nidularia*, adhering to a basis without any pedicle. But in a variety of genera it is furnished with a stipe, solid or hollow, cylindrical or compressed, from the size of a crow-quill to an inch or more in diameter, and from being almost sessile, to six or eight inches in height. Of the stipitate *Fungi* many are surmounted with a sort of conical production denominated the *pileus*, or cap, as in *Agaricus campestris*, the Common Mushroom. Its upper surface is generally smooth, though sometimes wrinkled; generally of a white or yellow colour, but often of a beautiful red, as in the elegant example of *Amanita muscaria*, or Fly Fungus. The under surface is furnished for the most part with a number of thin and flat substances, resembling in their form the gills of a fish, and designated by the same name. Some have the additional appendage of a veil or curtain, inclosing the gills, in the early stage of their growth; and some have the appendage of a wrapper, enveloping the whole of the frond.

If the inner surface of the curtain is carefully examined with a good magnifier before the time of its natural detachment from the pileus, it will be found to be furnished with a number of fine and delicate threads supporting small globules: these Hedwig regards as the stamens. If the gills are next examined in the same manner and about the same period, the surface will be found to be furnished with a multitude of small, tender and cylindrical substances, surmounted with a small globule: these he regards as being probably the styles and summits. But however this may be, the gills do eventually, and in their ripened state, discharge spontaneously multitudes of small and minute granules, whether seeds or gems, from which the species may be propagated. Let any one put a sheet of white paper under a frond about the time that it reaches maturity, and he will soon find it covered with a fine and brown powder discharged from the gills. In *Boletus* this receptacle is the tubes, in the Mucors it is the globule surmounting the stipe, in *Peziza* it is the upper surface of the frond only, and in *Clavaria* it is the general superficies.

Some *Fungi* are extremely detrimental to our growing crops of wheat, barley and oats, lodging and vegetating in the leaf or ear, and causing smut, blight, mildew, rust; but others are of excellent use, whether in the arts or in dietetics. The powder of the Lycoperdons is used as a styptic; several of the *Boleti* make a very good tinder; the Truffle is much esteemed for the rich and delicate flavour that it imparts to soups and sauces; and the Mushroom, *Agaricus campestris*, is known to every lover of good things, not only for its esculent property, but also for its special utility in the preparation of catsup. But in the gathering of such as are esculent, great care ought to be exercised, as much mischief may arise from a mistake of the species, many of the *Fungi* being highly poisonous.

> .... *Pratensibus* optima *fungis*
> Natura est; aliis male creditur.—*Hor.* lib. ii. Sat. iv. 20.

Such is the brief sketch of the external structure of vegetables which we have thought it expedient to exhibit, with a view to elucidate the gradation by which plants descend from the highest and most perfect orders, to the lowest and least perfect orders—"from the cedar that is in Lebanon, to the hyssop that springeth out of the wall;" or, to reverse the order of our progression, and take it in the line of the ascending scale—from the meadow mushroom to the mountain palm. Throughout the whole of the continued climax, elevation of rank is uniformly connected with complexity of structure. This shows the correctness of the general views of Gærtner in his controversy with Hedwig respecting the fructification of the Cryptogamia, whatever we may think of his particular application of them to the point at issue. Hedwig affirmed that all plants whatever possess sexual organs, and produce seeds, not excepting even the lowest in the scale of vegetable being*. Gærtner said that some plants are destitute of sexual organs, and do not produce seeds, but merely gems†. The sum of his doctrine is as follows: When the species is propagated by gems only, without seeds, as in the lowest orders of vegetables, no sexual organs are perceptible. When the seed is inconspicuous, and seemingly nothing but a mere nucleus or embryo, then the female organs are perceptible, but not the male organs, and the plants are called Aphrodites. When the embryo is furnished with a radicle perceptible in the seed, then also the pollen appears, but the flower has no beauty; and when the embryo is found no longer constituting a mere *nucleus*, but surrounded with its cotyledons, then there is to be seen the apparatus both of flower and of sexual organs. The first class includes plants without sex, the *Con-*

* *Theor. Fruct. et Gener.* † *De Seminibus. Introd.*

fervæ, *Ulvæ*, *Fungi*; the second class includes the *Aphrodites*, the *Filices*, *Musci*, *Fuci*; the third class includes what are called ambiguous plants, such as *Zostera*, *Zamia*, *Cycas*; and the fourth class includes all plants whatever with conspicuous flowers. This gradation, if not strictly correct with regard to the examples adduced, is at least singularly beautiful with regard to the view exhibited, as well as clearly demonstrative of the existence of a continued and ascending scale, as founded on complexity of structure.

Ashford, Jan. 4, 1834. P. KEITH.

XLV. *Correction of an erroneous Statement respecting* Mr. Faraday, *which is contained in the last Edition of the "Panorama of Torquay."* *By* OCTAVIAN BLEWITT, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I AM desirous of addressing you on the subject of an erroneous statement respecting Mr. Faraday, which is contained in a letter addressed to me by Thomas Northmore, Esq., of Exeter, and published in the last edition of my "Panorama of Torquay." It appears that Mr. Northmore published an account of some experiments on the condensation of the gases in Nicholson's Journal for the years 1805–6, vols. xii. xiii. xiv., and supposing that other philosophers had taken advantage of these experiments without acknowledgement, he censures in his letter to me, on the Caverns of Devonshire, the conduct of Sir H. Davy, Mr. Faraday, and several other philosophers for withholding the name of the "first discoverer."

Now, Mr. Faraday has proved to me from the "Quarterly Journal of Science, Literature and the Arts, vol. xvi. 1823," that so far from treating Mr. Northmore with injustice, he has there spoken of his researches in these terms: "*The most remarkable and direct experiments I have yet met with in the course of my search after such as were connected with the condensation of gases into liquids are a series made by Mr.* NORTHMORE, *in the years* 1805–6.......*Mr. Northmore's papers may be found in Nicholson's Journal*, xii. 368, xiii. 232," &c.

I lost no time after Mr. Faraday had obligingly pointed out this fact, in acquainting Mr. Northmore with his error, and with the unpleasant position in which he had placed me, an involuntary agent in the transaction. He has expressed his vexation and regret for his ignorance of the circumstance, and has requested me to assure Mr. Faraday how highly he should have valued the honour done him by his notice, had he been at all aware that such a paper existed.

I have the greater pleasure in giving this public explanation on behalf of Mr. Northmore, since it affords me an opportunity of stating, that admiring as I do the genius and the discoveries of Mr. Faraday, I should have been the last person to give publicity to a single word to his demerit, if I had not been previously informed that the whole passage merited implicit reliance. From an impression, however, that it would be unjust to allow Mr. Faraday to remain uninformed of the terms in which he is there spoken of, I availed myself of the first opportunity afforded me to place the work in his hands, for the purpose of ascertaining the accuracy of Mr. Northmore's charge against him; and I am happy in being able to remove the conclusion to which that charge might lead. As Mr. Northmore's letters on the organic treasures of the caves of Devon are authenticated by his name, and as he is well known in that county as an ardent antiquary, and as a public character in other ways, I considered that he would be held responsible for their contents,—and that although published in my work, their insertion would not render me amenable for his sentiments and theories.

I trust, Gentlemen, that you will, in justice to Mr. Faraday and myself, oblige me by the insertion of this letter in your valuable periodical.

I am, Gentlemen, yours, &c.

London, Feb. 27, 1834. OCTAVIAN BLEWITT.

XLVI. *On the Autoptic Spectrum of certain Vessels within the Eye, as delineated in Shadow on the Retina.* By W. G. HORNER, *Esq.*

To Richard Phillips, Esq., F.R.S., &c., Co-Editor of the Philosophical Magazine and Journal.

Dear Sir,

THE paper on the Vision of the Retina, in your Number for this month (January) brought to my recollection a more explicit and diversified statement, which appeared in the first number of the late Journal of the Royal Institution, in the form of "Contributions to the Physiology of Vision." The author gives his initials C. W. (C. Wheatstone?).

On turning to that paper, my attention was fixed by the writer's description and views of an experiment performed by him, after Purkinje and Steinbuch, in which the experimenter is said to observe "the blood-vessels of the retina."

The results of my own procedure in repeating that experiment are too remarkable to be concealed, whether justice to myself or to the cause of physiological science be considered; and as Sir David Brewster announces an immediate communication on the same subject, it appears to me that courtesy to him requires me to offer mine through the same channel. Besides, it is possible that our researches may have lain in different directions; and even if they happen to have led to the same termination, Sir David's prior claim will be established, and yet the deductions of an independent, though less informed, inquirer may have their value.

1. C. W., like his predecessor Purkinje, employed a flame, held within two or three inches of the eye, and on one side of it, or below, to produce the spectrum. On trying this, and assisting others to do the same, it presently occurred to me that the image of the flame in the focus of a powerful lens must be more convenient and effective. This substitution was no sooner made than, on assisting another person to observe the appearances in his eye, I discovered that the effect was certain, only when the light fell *on the ball of the eye*, or the sclerotica, *without touching the cornea.*

2. It was likewise obvious to remark, that the lines in the spectrum were never so dark and well-defined as when the focal image was accurately formed.

3. Placing the flame at a distance from my eye equal to four times the distance of the geometrical focus from the lens, and holding the latter halfway between the eye and the flame, it is obvious that I obtained the maximum of illuminative power; and by applying such light to the exposed ball of the eye in different directions, corresponding portions of the vascular image were brought more distinctly under cognisance. Thus a light near the right-hand angle of the eye displayed the left-hand portion of the image, and so of the rest. The central parts, within the range of distinct vision, were developed, in every instance, with splendid accuracy and minuteness.

4. Towards the middle of a sheet of cardboard of a darkish tint, having cut an aperture about half an inch long by one tenth of an inch broad, I repeated the experiment by the direct light of the sun, with still superior effect.

5. In all experiments upon one eye, the comfort of the other eye contributes materially to success. A case should be bound over it, so as completely to darken it, without touching the eyelids. It has, by all observers, been experienced, that a distinct view is not to be maintained, unless the light is kept in motion. The lens or the cardboard must be moved

slowly backwards and forwards edgewise, so that the light may traverse the interval between the cornea and the angle or lid of the eye.

6. The drawing (fig. 1, p. 267.) exhibits, with as much accuracy as my slender graphic skill admits, the result of numerous and varied observations of the vessels of my *right eye*. The cross (+) indicates the *centre* of the field of view, or the point of direct vision. Beneath 0 is the *origin* of the larger vessels, which in its average situation falls within the range of the *punctum cæcum*. To exhibit the more minute vessels, which either from my perception improving by habit, or possibly from continued excitement, appeared much more numerous in my later than earlier trials, fig. 2. is an enlarged figure of the more central vessels, and of the peculiar appearance of the central portion of the ground of the picture.

7. At the centre (+) of the ground of the picture, which "corresponds to the projection of the *foramen centrale*," C. W. observed a crescent-like appearance, indicating in his opinion "a slight convexity or concavity in the retina at that point." In my own eye, whether the right or the left, no trace of such a crescent is found, but the appearance of a granulated texture in the level surface, like a number of exceedingly minute polished spherules collected within an obscurely defined circular space, as represented in fig. 2.

8. This circular spot is quite fixed, while the eye continues steady; and generally speaking, it is quite clear even of the minute vessels, which appear almost equally spread in every other part. But in waving the light over a considerable angular portion of the eye, some of those vessels would intervene between the sight and the central spot, indicating that the vessels and the ground on which they were projected were on *different surfaces*, contrary to what has been hitherto believed.

9. This important fact was corroborated by the phænomena observed about (0) the origin of the vessels, which was manifestly on different sides of the *punctum cæcum*, according as the light fell far on the inner or the outer angle of the eye. The situation of the head of the optic nerve was marked not only by the usual indication, which in these weak lights is less distinct than usual, but by the occasional brightness that flashed upon it, when a strong light was suddenly thrown quite in the angle of the eye.

10. But the most available proof of this fact, was the appearance of the entire spectrum when referred to some distant part of space. For, the eye being kept perfectly steady, the image, with a sensibly uniform motion of all its parts, followed the motion of the lens. This proved that the vessels which

produce the image occupy a surface *parallel to that which is the seat of vision.*

11. Every indication concurring to prove that a condensed light thrown on the sclerotica was able to traverse its substance, and that of the vitreous humour, with a concentration sufficient to cast the shadows of these fine vessels upon the retina, their distance from it, and their size, were thus determined. A scale of inches and parts being held along a wall, distant 15 feet from my eye, I observed that the projection of one of the principal vessels in fig. 2. occupied a breadth of about half an inch; and that they moved over the space of one foot, while the focus of a lens traversed the whole extent from one angle of my eye to the other. Admitting this to be 150°, and the distance of the retina from the optical centre of the eye $\frac{3}{4}$* of an inch, we have 15 feet : $\frac{3}{4}$ inch : : $\frac{1}{2}$ foot : $\frac{1}{40}$ inch, whence $\frac{1}{40} \cot \frac{150°}{4} = \frac{1}{30}$ inch, very nearly, is the *distance of these vessels from the retina*; and $\frac{1}{2}$ foot : $\frac{1}{2}$ inch : : $\frac{1}{40}$ inch : $\frac{1}{480}$ inch, very nearly, is the *diameter of the medium vessels.*

12. The agreement of this estimate of distance with the observation of Art. 9. is very satisfactory; for the whole exposable angular extent of the eye being about 240°, we have $\frac{2}{30} \tan \frac{240}{4} = \frac{2\sqrt{3}}{30} = \frac{23}{200}$ inch very nearly, for the entire range of the shadow of each vessel. Now that this exceeds the greatest breadth of the *punctum cæcum* in my own eye, I am assured by direct experiment. For I find that an oval, 12 inches in height by $10\frac{1}{2}$ in breadth, and whose vertical major axis is at the distance of 2 feet from a mark in a horizontal line cutting the oval at one third of its axis from the vertex, just disappears when the mark is viewed from a distance of 8 feet perpendicularly to the plane of the oval. Hence by a quadratic equation, viz. $(\cdot 22 + 4y)(\cdot 69 - 4y) = y^2$ or $(\cdot 10 + 4y)(\cdot 81 - 4y) = y^2$, according as Young's or Herschel's estimate of the optical centre is adopted, we find $y = \cdot 16$ or $\cdot 19$ inch = the distance of the centre of the optic nerve from the axis of vision. The breadth of the *punctum cæcum* will be at most (2 feet : $10\frac{1}{2}$ inches : : ·19 :) ·085 inch. D. Bernoulli's estimate is stated at $\frac{1}{7}$ the diameter of the eye, which, by Dr. Young's measurements, is ·13 inch. Priestley's account of Bernoulli's data is imperfect; so that the cause of this discrepancy is unknown to me, the Petersburgh Trans-

* Sir J. Herschel places the optical centre in the centre of the iris; Dr. Young at ·22 inch from the front of the cornea. I take the medium as sufficient for the object in view.

actions, which contain the original paper (vol. ii. p. 313.) not being attainable here.

My remarks being brought down to these numerical conclusions, my present purpose is accomplished. On the luminous fringes of the shadows, the cause of the peculiar apparent structure of the centre of the retina, the nature of the vessels themselves, as well as on phænomena connected with another kindred experiment of C. W.'s, and the relation which exists between these vascular images and the more regularly tessellated spectra observed by Purkinje, Mrs. Griffith and others, I may venture to offer an opinion hereafter, unless (as I confidently hope) my purpose shall have been anticipated by a more accomplished investigator of the phænomena of light.

I am, dear Sir, yours, &c.

Bath, Jan. 14, 1834. W. G. Horner.

P.S. In my paper on the *Dædaleum*, the following errata (*not* due, I believe, to the printer,) require correction. On p. 37, last line, *omit* "O o"; on p. 38, l. 15, *for* $x =$ C D *read* $x =$ Cq; on p. 39, l. 14, *for* "outer" *read* "entire."

Jan. 16.—T. S. D.

Additional Remarks. (*February* 1*st.*)

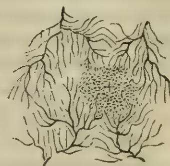
The contents of the preceding paper will sufficiently declare for what reason I send it in its original state, and countersigned by my friend Mr. Davies on the day when he perused it. You will perceive that, excepting the corrected calculations in Arts. 11. and 12. no alteration has been made since that day. For the purpose of correcting those calculations by reference to Dr. Young's publications, the paper was detained.

The new [February] Number of the Magazine and Journal has just now reached me, and I have the pleasure of perceiving that there is yet room for my contribution, partly as remarkably confirming some of Sir D. Brewster's statements, and partly as adding something new. By the aid, particularly of my 3rd and 4th remarks, I flatter myself that Sir David will find the difficulty mentioned in the note (p. 118.) to be completely obviated; and that the performance of the experiment being now reduced to a state of precision and certainty, my rough approximations will be corrected, and further conjectures superseded, by persons who have more available leisure for such inquiries.

Respecting the figures which accompany the paper, I ought to state that the vessels which are comparable to the larger ones in fig. 2. are in fact more numerous than could be represented in fig. 1. without confusion; and that some branches are omitted in fig. 2. to prevent perplexity in comparing it with fig. 1. The central region of the former was, however, as clear of all vessels as I have represented it.

Fig. 1.

Fig. 2.



Explanation of the Figures.

Fig. 1. *Vascular Spectrum*, as seen in the right eye; + being the point of sight in the centre of the field.

Fig. 2. The *Central Vessels* on a larger scale, exhibiting the ultimate vascules of the spectrum, and the granulated appearance of the *foramen centrale.*

In fig. 1. the picture has about *four* times the linear dimensions of the original, and in fig. 2. about *twelve* times.

By experimenting with strong sunlight, admitted through a large pinhole in a dark cardboard brought as close to the eye as possible, I find (1) that the appearance of the luminous surface is not so properly *granular* as *fibrous*, like plush, and has irregularities which bear a strong resemblance to those in Zinn Pl. I. fig. 2. From this appearance, and another circumstance presently to be noticed, I suspect that this luminous phænomenon is no other than the flocculent and delicately reticulated envelope of the internal arterial surface of the choroid, that portion of it, namely, which is included within the *punctum luteum.* And (2) that the minuter vessels of the spectrum are more variously dispersed than the figure exhibits them, and that those of the upper and lower branches actually appear to inosculate, as though one set were arteries and the other veins.

With regard to the granulated or silky spot, more exact observations have proved (3) that it is not fixed, as I at first believed, or at least that the principal mass of light which falls upon it is not fixed, but has a motion *contrary to that of the shadows.*

Another observation, which may prove essential to further researches, is this (4), that with sunlight admitted as above, although the peculiar structure of the spot in question is always distinctly enough seen, yet it never appears so bright as when the light falls on the verge of the cornea, in the region of the *ciliar circle.* On using a lens and candlelight, the silky texture is scarcely perceived, unless when the focus falls on the ciliar region of the eye. The shifting mass of light which, at this time, appears on the spot, would seem to have entered the crystalline lens laterally through the colourless portions of the integuments, and to be thence transmitted by reflection*.

The easiest way to obtain correct *measures* of the quantities of motion, would doubtless be by using a sunbeam fixed by a heliostat in a darkened room; but I have not the means at hand. It requires some practice to manage a lens, and the operation is at best complicated. Besides the apprenticeship to the use of the lens, the method itself of observing, namely, by indirect vision, is at first perplexing, and demands a constant effort to resist the habitual tendency of the eye to follow the object, and by that means exaggerate its apparent motion. The choice also of external objects of comparison is limited and nice: to fix the eye, they must be as few and as distinct as possible, and be surrounded with entire obscurity, since even a faint light in that situation would sensibly affect the distinctness of the shadows. At the hazard, therefore, of being thought triflingly minute, yet to prevent vexation to other observers and the unnecessary wear and tear of a delicate and invaluable organ, I will describe the method which, after numerous trials, has proved the most satisfactory in my own case.

A box, of adequate size, and capable of completely confining the light, yet provided with proper apertures to admit a supply of air, should have one of its sides substituted by a large dark-tinted cardboard prepared in the following manner. A sliding-piece of several inches in length being let into it, and two holes punched, one on the axis of the slider, and the other on its continuation upon the cardboard, a sheet of

* Probably from the white folds of the *corpus ciliare*: this will account both for the appearance described above (3), and for the variable *lunula* observed by Mr. Wheatstone (Art. 7.).—By the by, in Purkinje's experiment, are not the shadows entirely due to the light which permeates the ciliar region?

tissue paper should be wafered upon one side of the board. The box being set on an *elevated* stand at a distance from the observer, and a light placed within it, the cardboard, with the papered side towards the light and the slider horizontal, must be fixed against the box, and turned in the direction of the observer, who will then receive no light from that quarter, except through the two little apertures. He will seat himself, with due regard to firmness of attitude, close to a table in front of him, and on which is a single light, *below* the level of his eye, and at the distance of rather more than four times the geometrical focus of the lens he designs to apply, and having his unemployed eye comfortably cased and bandaged.

With one hand he draws down the lower lid of the open eye, so as to expose the sclerotica freely; while with the other he holds the lens at such a distance and position between the light and his eye, that the focus shall fall upon that angle of the eye which is opposite to the aperture in the *cardboard.* A mirror will materially assist in acquiring this adjustment at first; but to a practised observer its success is announced by the certain though dim appearance of the vascular shadows. Having succeeded in placing the focus, and at the same time keeping his sight fixed upon the aperture in the cardboard, if none of the shadows are situated vertically below the aperture, he will direct an assistant to remove the light slowly in the requisite direction, until one of the shadows has arrived at that position. Then carrying the lens sideways until the focus arrives at the other angle of the eye, his sight being steadily intent upon the aperture in the cardboard as before, but his attention being also carefully directed to the progress of the selected shadow, he will notice whether the latter vanishes when vertically below the hole in the *slider.* If not, an assistant must move the slider in the requisite direction until that effect is obtained. It then only remains to measure the distance of the observer's eye from the card and the mutual distance of the apertures.

As all the misleading causes tend to magnify the estimate, it will be found that my original statement of 1 foot to 15 feet is more than double of the actual quantity. The seat of vision will therefore prove to be less than $\frac{1}{60}$th of an inch behind the system of vessels. This locus of accurate convergency will probably be found to be the *posterior membrane of the retina.* It seems, however, to admit of doubt, whether the distance from the system of vessels is a permanent quantity.

To account, under this altered statement of the distance, for the phænomena of Art. 9., we must suppose that the origin of the vessels, and consequently the larger vessels themselves, are situate rather more internally than the medium and smaller vessels. And this supposition derives support from the fact that the trunks of the upper and lower branches, when narrowly observed, have a perceptibly greater motion than the general system has (Art. 10.); the excess being distinguished by an alternating movement, like that of a pair of shears, gently pressed and released by turns;—the perspective reason of which effect is very obvious. The estimated magnitude of the medium vessels also requires correction (Art. 11.). On comparing their shadows with a graduated system of bars at 15 feet distance, $\frac{5}{8}$ inch proves more correct than $\frac{1}{2}$; and this element introduced into the corrected calculation gives $\frac{1}{800}$ inch, or less, for the diameters of the larger vessels in fig. 2. Of these, and yet more delicate vessels, it is almost needless to remark, that not a trace appears in Sömmerring's designs; at least, in Schröter's copies of them. Nor is it, perhaps, to be expected that they can be anatomically demonstrated.

The appearance of the granulous spot in the experiment above detailed is interesting and splendid, occupying, at 16 or 17 feet distance, an area of a foot or more in diameter, and strongly resembling in its entire description the pyrotechnical exhibition of what is termed *golden rain*, when just beginning to descend.

The detail of these vessels must differ greatly with different observers, since a remarkable difference is found between those of my own right and left eye, the latter being more regularly distributed, and those of the upper and lower branches more decidedly intermingling their extreme and minute vessels. Yet the resemblance of the general structure to that exhibited in two of Sömmerring's figures, as Sir D. Brewster has already remarked, is too striking to admit a doubt to rest on the identity of the subjects. In fact, Zinn's description of the "arteries of the retina" is an accurate description of the very view of things to which my experiments have conducted by an independent route: "Arteriolæ retinæ unice in interiori retinæ facie decurrunt, nudæ fere, qua humorem vitreum contingunt; et teguntur, qua choroidem respiciunt, multa medulla nervi optici." (cap. x. § 4.)

The reflection that has most struck me, in reading, for the first time,this author's elaborate and beautiful description of the retina, its double structure, vascular and medullary, in distinct and parallel strata, and its by no means evanescent thickness, is this, that it is very surprising that so many writers who have recognised the retina as the seat of vision should have treated it as a mere geometrical superficies, or else have conceived of its irregular vascular inner surface as the locus of accurate convergency.

To persons who are timid, or have weak eyes, it may be interesting to know that these experiments succeed nearly as well on the closed as on the open eye, as far as concerns the distinct exhibition of the vascular spectrum, the difference being scarcely more than may be ascribed to the light which then reaches the cornea by diffusion through the lid. But in fact, if the air is mild, the mere examination of the vessels (Arts. 3, 4, 5.) is less trying to the eye, than continued reading, or exposure to a strong light.

W. G. H.

XLVII. *On* Prof. Moseley's *Explanation of the "Principle of Least Pressure" inserted in the Lond. and Edinb. Phil. Mag. for March. By* S. Earnshaw, *B.A., Fellow of the Cambridge Philosophical Society.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I AM fully aware of the great importance of the question of statical resistances, which Professor Moseley has undertaken. The subject is one of peculiar difficulty, and has defied the attempts of some of the ablest mathematicians to conquer it; it is therefore the more necessary that every theory which is now proposed should be scrutinized with very great care before it be received and acted upon. These considerations will screen me, I hope, from a charge of obstinacy when I inform Mr. Moseley that his explanation has had the effect of strengthening rather than of removing my previous objections to his theory. I shall therefore now set before him, at some length, my reasons for differing from him on this subject.

In the first place, Mr. Moseley denies "that if the forces of the system C are not parallel, their actions *must* of necessity propagate pressures." Now, for the sake of avoiding the complexity of reasoning in general terms, let us suppose the body acted upon by gravity only, and substitute the system C for the reactions B. Then the system A balances the system C. But the former = weight of the body, and acts in a *vertical* direction, and cannot, therefore, propagate *horizontal* pressures; for the resolved part of a vertical force is equal to zero in a horizontal direction. Also the forces C (being oblique to each other by hypothesis,) may be resolved into a vertical and a horizontal set; the forces of the former set exactly balance the forces A; but those of the latter are solely employed in balancing each other; and with regard to this set I have to observe:—

1st, That they are propagated by the forces of the system C, since no horizontal force can be propagated by the system A.

2ndly, That, though in equilibrium amongst themselves, they *cannot be removed* by the principle of the superposition of forces; for the forces C are oblique, and consequently produce these horizontal pressures of necessity: that is, no change in the *magnitudes* of the forces C can ever remove these horizontal pressures. It is not allowable to remove them in any other way than by changing the *magnitudes* of C; and any other method would change the *directions* of C, which cannot be admitted, because the directions are given. Wherefore it is impossible to remove them at all.

As I rest my objections to Mr. Moseley's theory principally on this point, I will vary the argument a little, lest Mr. Moseley should take some objection (though I certainly do not see one myself,) respecting the propagated pressures being horizontal, and not in the directions of the forces C. I suppose it will not be denied that a body cannot press with a force greater than its own weight. Now, as before shown, the weight of the body = the sum of the resolved parts of the forces C in a vertical direction; and as the forces C are oblique, their *sum* is greater than that of their resolved parts; and therefore the sum of the forces C is necessarily *greater than* the weight of the body. If, then, the forces C have to support a greater pressure than the weight of the body, whence comes the excess? I think there can only be this answer:—"That the forces C *must* of necessity mutually propagate pressures." I consider that Mr. Moseley has erred in his argument on this point in two particulars—

1. In assuming that each of the forces C can be *divided* (not *resolved*) into two parts, one of which is solely employed in sustaining the pressures propagated by the system A, and the other in sustaining the pressure mutually propagated by the system C:

2. In supposing that these latter forces *can* be removed by the principle of the superposition of equilibrium.

I come now to speak of the principle itself, that "the sum of the forces of the system B, each being considered *a function of the coordinates of its point of application*, is a MINIMUM." It is very usual (and I believe the method has never been objected to,) when a general principle is proposed, to try it in particular cases, the results of which are known beforehand from some independent source, and if it fail in any of these cases, it is either altogether rejected or its truth is looked upon as doubtful, until these cases are shown to be excepted in the general demonstration. Now suppose the body acted upon by gravity only, and supported on props acting vertically, then the forces of the system B are parallel, and their sum is *constant*, being equal to the weight of the body. In this case, then, it is plain the sum of the forces of the system B cannot be either a maximum or a minimum, and, therefore, Mr. Moseley's principle fails.

As a general objection to the principle of least pressure, I would beg to suggest to Mr. Moseley, that the enunciation of it belongs to the following problem, and does not contain a statical principle at all; viz. To find the *points of application* of a given number of reactions B, in order that they may sustain a given system of forces A, and have their sum a minimum. If Mr. Moseley disagrees with me on this point, I would ask him, how he would proceed to solve the problem I have enunciated?

I will now give my reason for saying that "I am unable to comprehend why we are to consider the forces as functions of the coordinates of the points at which they are applied." It is this. I believe that only the *directions*, and not the *forces themselves* (when there are more than three), are functions of the coordinates; and as Mr. Moseley has put the directions out of the question by saying that we are to take the *sum* of the forces, I was, and still am, unable to comprehend what the coordinates have to do with the matter.

With regard to the body supported on three parallel props at the angular points of a triangle, Mr. Moseley, by a certain hypothesis of construction, has shown that the pressure on one prop is $\frac{1}{3}$rd of the weight of the body, so long as the triangle is finite; and thence infers that when the triangle passes into an evanescent state, the pressure on that prop undergoes no change. This reasoning is certainly very natural; but, let it be observed, when by a different hypothesis of construction I have shown that the pressure on one prop is zero so long as the triangle remains finite, and come to infer, by the natural reasoning before mentioned, that it continues zero when the triangle is evanescent, Mr. Moseley thinks the conclusion not valid. I think, however, there ought candidly to be allowed as much validity to one as to the other, for they stand precisely upon the same footing. Mr. Moseley remarks on this head that the pressure on C "is not *absolutely*, but only *relatively* evanescent." To this I answer, that the pressure on C is as *absolutely* evanescent as if the prop did not exist, because the moments of the other two props about AB are *absolutely* evanescent by hypothesis. As Mr. Moseley has endeavoured to strengthen his arguments on this matter by a verification obtained from a different method of investigation (page 199 *et seq.*), it becomes necessary for me to remark, that no reliance can be placed upon it, for these two reasons:

1st, It does not appear that the assumption of a point X, about which the moments of the pressures upon A, B, C may be equal, is a possible assumption.

2ndly, Mr. Moseley shows that $3a-b-c=0$; and yet when, at the bottom of page 200, he arrives at the equation

$$x^2(3a-b-c)-2xa(b+ac-bc)=-abc,$$

he altogether overlooks the circumstance that the first term is evanescent, and that, consequently, the value of x is $\frac{\frac{1}{2}abc}{ab+ac-bc}$, which is not infinite. Wherefore, as Mr. Moseley's verification rests entirely upon x being infinite, it must fall to the ground.

As I am anxious not to extend this letter to an unnecessary length, I shall not make any remarks upon Mr. Moseley's application of his theory to the wedge and the arch; for I have not discovered in them anything which either militates against my arguments, or goes to confirm the theory in question. I have now, therefore, only to thank Professor Moseley for his polite reply to my former observations, and to assure him that as I write only for the investigation of truth, I shall feel a pleasure in confessing my opinion wrong as soon as he has satisfactorily answered the objections contained in this letter; and I am sure that he will pardon the trouble I am causing him, from a conviction that truth does not suffer, but gain, by every examination to which it is submitted.

I am, Gentlemen, yours, &c.

St. John's College, Cambridge, March 8, 1834. — S. Earnshaw.

XLVIII. *On the Influence of the Climate of Naples upon the Periods of Vegetation as compared with that of some other Places in Europe.* *By* John Hogg, *Esq., M.A., F.L.S. F.C.P.S., &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE geography of plants has of late years become a new subject of great interest to the naturalist, and is at the present day esteemed a most important and useful branch of natural science. Linnæus may be said to have first called the attention of botanists to this study; for in his writings are to be found many observations on the distribution of plants over the different regions of the world.

But the principal authors who have, within these few years, brought botanical geography again into notice, and have given to the world a vast deal of scientific information, collected in various parts of the globe, are Humboldt, Bonpland, De Candolle, Ramond, De Buch, Schouw, Mirbel, Tenore, Robert Brown, Winch, and Wahlenberg. Dr. Schouw published in 1822 a work in Danish, entitled "A Prodromus of the Universal Geography of Plants." It was translated into German, and published at Berlin in 1823.

How unfortunate it is that so much valuable learning on this subject remains locked up in an "unknown tongue," at least with respect to most of the botanists of England! I must therefore only hope that some German scholar will ere long be persuaded to translate it, either into French or English, since it is accounted by the botanists of Germany a work of general utility on this head.

But there is a part of botanical geography still less known than that which relates to the laws of the distribution of plants over both hemispheres; I allude to the influence which climate exerts upon vegetation in different places in the world. And, indeed, one of the most accomplished and learned men of our age* has said, that "the connexion of the law of the distribution of the generic forms of plants over the globe with the *laws of climate* constitutes one of the most interesting and important branches of natural-historical inquiry, and one on which great light remains to be thrown by future researches." Now it is with the view of introducing this particular subject to the readers of your Magazine that I have been induced to send to you the following sketch: and I earnestly wish that some of them may continue with zeal and care any inquiries that they may already have begun on the *periods of vegetation*; and at the same time they ought minutely to notice the different degrees of temperature and moisture according to the respective seasons of each year. For by a comparison of many nice observations, carried on in different parts of the world, at different elevations and in different seasons, we may ultimately be enabled to arrive at a more certain knowledge of the laws of climate.

The greater portion of this paper I have translated from the 8th chapter of an "Essay on the Physical and Bo-

* Sir John F. W. Herschel. See his "Discourse on the Study of Natural Philosophy," p. 345.

tanical Geography of the Kingdom of Naples *," by Signor Tenore; and the remainder consists of some comparative remarks on the vegetation of England, principally compiled from the "Naturalist's Calendar," published in White's works on natural history. My own observations are inclosed within brackets. I remain, Gentlemen, yours, &c.

London, Jan. 22, 1834. J. Hogg.

The great Linnæus, in different passages of his celebrated works, has not ceased from recommending botanists to keep an account of their observations on the influence which the difference of climates and seasons exercises upon the periods of vegetation. By adding, according to his custom, the example to the precept, in his *Philosophia Botanica*, under the title of "The Calendar of Flora," he has there given a series of researches instituted by himself on the vegetation of the vicinity of the city of Upsal, from whose celebrated University he dictated the rules that are essential in paying due attention to the study of nature.

Convinced of the utility of these inquiries, and taking for a model the work of Linnæus, and that of a similar kind compiled by M. Chavassieux d'Audibert on the vegetation of the neighbourhood of Paris, I have in different years applied myself in observing the times of vegetation in the environs of Naples, and I have not failed to keep an account of my own observations, and to compare them with those of the abovementioned authors.

I should have wished to extend this sketch to all the provinces of the kingdom of Naples; but as it is not possible that such researches can be carried on except by those who reside there permanently, so I could not change this plan until after some correspondents had been sent to live in the provinces who were charged to collect materials for the *Flora Napolitana*, and to send the plants to the Royal Garden at Naples. Observations on the periods of vegetation recommended to them in the instructions which were compiled for such purpose, and of which not a few important remarks made in all parts of the kingdom are about to be published in the *Giornale Enciclopedico di Napoli*, already begin to benefit the public by this useful undertaking; and indeed, unless by the postponement of that botanical correspondence, the compilation of this short work had better for the present been delayed.

In the mean time, flattering myself that the original part

* *Cenno sulla Geografia Fisica e Botanica del Regno di Napoli*, di Michele Tenore; *con due Carte Geologiche*. Napoli 1827. A very interesting work, deserving to be wholly translated into English.

of these remarks, although given in the second volume of my *Fitognosia**, may be able to throw some light on the subject which has so engaged me, I thought it could be reinserted in this place, and improved by dividing it into the *five* following articles, which correspond with the different periods of vegetation.

I. *Germination.*

The variations to which Nature has subjected the sprouting of seeds (*germinatio*) are generally known. Thus, for example, Millet, Wheat, and the greater part of the seeds of the *Plantæ Cereales* germinate in two or three days; the Lettuce, the Gourds, the Cress, in from five to seven days; the common Bean, the French Bean, the Onion, in about twenty, the Parsley in about forty days; the Columbine, the Almond, the Chestnut, the Pæony, the Filbert, the Cornel Tree, in from six to eight months; and finally, the Roses, after the first or second year.

In this article of botanical geography, we must not discourse on similar natural distinctions, but on the alterations alone which can be produced in the sprouting of seeds by the application of the varying causes of vegetation.

Among these, *heat*, being the more powerful, forces and exerts the greater and more direct influence upon germination; it can consequently be established, that this appearance of vegetation will be in constant relation to the different degrees of the temperature of the soil which surrounds the seeds.

[But, in the first place, I think it better to add here a few general notices† on the climate of the capital and vicinity of Naples.

Being situated between the Apennines and the sea, and therefore under the prevalence of the alternations of the south and north winds, the city of Naples must necessarily be exposed to those very rapid meteorological changes that render the weather extremely variable and unsettled.

In the spring, in the autumn, and in the winter, these variations are felt more sensibly; in so much so, that frequently the thermometer during the same day falls from 10° or 12° of Reaumur (54° or 59° of Fahrenheit) to 4° or 5° Reaum. (41° or 43° Fahr.), and *vice versâ*; and the state of the heavens alternates between calm, clouds, rain, hail and storm.

In general the spring is very short, and is often confounded with the summer, in consequence of the high temperature which is wont to take place at the end of the winter. Indeed, in some months of that season, and especially in May, the

* *Fitognosia* da M. Tenore; vol. ii. in 8vo. 2nd Edit. Napoli 1816.

† Partly translated from pp. 91, 92, 93, chap. vii. of Tenore's Essay.

changes of the atmosphere are so instantaneous that one may in the same day be overcome with heat, and so cold as to require a fire. The winter is mild and rather damp; but snow very rarely falls. In Naples the thermometer in winter has been known to descend so low as the *fourth* degree below zero of Reaumur, or *nine* degrees below the freezing point of Fahrenheit.

The sea-breezes and the west winds make the great heats of the summer to be endured in the capital. Then most violent thunder storms are very frequent, being attracted by the lofty range of the Apennines, which occupy the larger portion of the kingdom; for in that season the thermometer stands almost always between 20° and 22° Reaum. (77° and 81½ Fahr.), and for a few days it will even ascend to 23° and 25° Reaum. (84° and 88° nearly Fahr.).

Indeed, the general state of the weather during the twelve months successively will be more accurately comprehended from the following Table* of the *mean* meteorological observations taken at the Royal Observatory of Naples for five years, viz. from 1821 to 1825 inclusive.

From 1821 to 1825.	Thermometer.		Barometer.		Rain.
	Morn.	Even.	Morn.	Even.	
Months.	Degrees of Reaumur.		French Inches.		Centimetres.
January	4°6	8°4	27,71,72	27,71,47	8·82
February.......	4·6	9·4	8,88	8,62	3·10
March..........	5·7	11·2	7,50	7,50	11·76
April...........	8·0	14·4	7,47	7,47	5·88
May	11·4	18·9	8.50	8,48	2·35
June............	13·5	20·5	7,98	7,98	5·4
July............	16·4	23·1	8,70	8,66	1·56
August.........	16·0	23·7	8,82	8,70	1·90
September....	14·1	21·1	8,68	8,62	5·45
October	11·0	16·7	8,30	8,40	11·06
November....	7·6	12·4	9,46	9,12	7·57
December	6·5	10·5	8,46	8,37	9·46
Annual Means	9·8	15·9	27,8,36	27, 8,28	74·36

* Compare this with the Journals kept by the Royal Society in London, and published in the Philosophical Transactions for those years, and with the Meteorological Tables given in the Annals of Philosophy of the same years. Also with the tables of the weather kept near High Wycombe, Bucks, at p. 351, vol. v. of Loudon's Magazine of Natural History, wherein the years 1823, 1824 and 1825 are stated. I have not thought it necessary to reduce the French proportions used in Tenore's Table to our English scales.

In those five years the maximum heat was found to reach 30° Reaum. (100° nearly Fahr.) on the 7th of August 1824, and the minimum was −2°·8 Reaum. (about 26° Fahr.) on December 30th, 1822.]

Now to prove the truth of the principle before stated with regard to heat, it will be sufficient to observe that the seeds of the plants of hot climates, introduced into temperate climates, germinate very much later than they do in their native countries; whilst the germination of the seeds of cold climates is remarkably accelerated by their being transported into temperate climates. For the sake of an application to this same principle, we are obliged in the stoves of our botanic gardens to raise the heat to a very high point, in order to make the seeds of the equinoctial countries sprout; and on the contrary, for the growth of those of the northern climes, we must select the colder and more shady situations.

The different temperature which usually predominates in the same seasons in different years likewise exercises its great influence in retarding or accelerating the germination of seeds; inasmuch as we see that the seeds of the same plant sprout very much sooner when the spring begins early, and is preceded by a mild and rainy winter; and that they are later when the spring is kept backward by the severity of a long winter which has preceded it. Thus I have had occasion to observe in the Royal Garden at Naples, that the plants which I have caused to be sown in March or April have grown imperfectly at the very same period, that is to say, when the temperature rises between 12° and 15° Reaum. (or 59° and 66° nearly Fahr.); consequently, I have become quite indifferent whether they be sown a month sooner or a month later. This observation, repeated for many years in succession, the experiment being always made at the risk that the plants which had lately sprung up encountered, when they were exposed to the hoar frost, and to the cold nights in the beginning of spring, has induced me to decide very definitively, that in the Royal Botanic Garden the large seeds ought not to be sown before the last 15 days in April or the first 15 days in May*.

[To be continued.]

* It is worthy of remark what very little difference exists between the time here fixed, and the usual vernal seed-time in England.

XLIX. *Remarks on* Mr. Carter's *Paper on the* Gopher-wood. *By* Charles T. Beke, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I OBSERVE that my paper on the Gopher-wood of Scripture, which appeared in the Number of your Magazine for August last, has given rise to the observations upon the same subject, made by Mr. Carter in your last Number.

That gentleman justly states my argument to be, that the word גֹּפֶר (*gópher*) is probably the same as כֹּפֶר (*kópher*); that *kopher* means *pitch*; and that consequently the wood in question would appear to be that of a *pitch tree.*

The first of these propositions Mr. Carter allows to be "highly probable"; but at the same time that he admits *gopher* to be identical with *kopher*, and thus agrees with me in all that I really contended for, he thinks it very questionable that the latter word means pitch, as it is rendered in the received English and all other authorized versions, for the reason, principally, that the verb כָּפַר (*kaphár*), from which כֹּפֶר (*kopher*) is plainly derived, is nowhere used in Scripture in the sense of 'to cover or daub with pitch.'

But this reasoning must surely have been adopted too hastily; for Mr. Carter can never intend it to be his meaning that a derivative word is not to be used in a secondary sense, because the root to which it is to be traced cannot in every case have attached to it the same secondary meaning as that of its derivative*. Were it so, the use of *kapher* and its derivatives, in the sense of 'to purge away or pardon' (Ps. lxv. 3.); 'to atone' (Lev. iv. 35); 'to disannul or obliterate' (Isa. xxviii. 18.); 'a ransom' (Exod. xxi. 30.); 'a [covered?] bason' (1 Chron. xxviii. 17.), or even 'pitch';—all of which meanings have plainly a direct reference to the primary signification of

* It is scarcely necessary to adduce any illustration of the consequences of opposing what is, in fact, a fundamental principle of language: one example presents itself, however, which is so very apposite, that I cannot refrain from citing it. It is in the Latin verb *tegere*, to *cover*, which besides its derivatives *tegmen*, *tegimen* and *tegumen*, of which the meaning, generally, is a 'covering', whether signifying in particular a 'garment', a 'hide', a 'shade', a 'shelter', &c., has also another derivative, namely, *tegula*, which word is exclusively applied to the article used for the covering of dwellings: yet because not one passage in any author can be found in which the verb *tegere* has of itself the particular import of 'to tile', it would certainly never be contended that, *therefore*, *tegula* does not signify a *tile*.

the verb 'to cover,'—would be erroneous: and it may be added that (independently of all other objections to it,) Mr. Carter's suggestion that *kopher* means *cypress*, must, upon his own reasoning, be wholly untenable, for not one passage can be found in Scripture in which *kaphar* has the particular import of 'to cover with cypress'.

Indeed, if there be any word which has attached to it a fixed and unequivocal meaning, it seems to be this word *kopher* (Gen. vi. 14.), which, as far as my information extends, is not attempted to be rendered otherwise than by the word 'pitch', in any version of the Bible, excepting in that of Mr. Bellamy, —the authority of which version, however, I may be allowed unqualifiedly to dispute. But it is not necessary to depend solely upon the received translations for the determination of the true signification of this word, seeing that the Arabic كُفْر, the Chaldee כּוּפְרָא, and Syriac ܟܘܦܪܐ, are all employed to express the same word 'pitch'. Nor does the proof of its meaning rest even here; for we find the Hebrew word גָּפְרִית (*gŏphrith*), brimstone,—in Arabic كبريت, in Chaldee כּוּבְרִית, and in Syriac ܟܒܪܝܬܐ,—which word is evidently derived from גֹּפֶר or כֹּפֶר in its secondary signification of *pitch*, on account of the resemblance, however partial or indirect, of the one substance to the other, and not from its primary meaning 'a covering', with which brimstone cannot possibly have any connexion.

The process of the derivation of the word *gophrith* may be thus stated. The vegetable pitch with which Noah's ark was *covered*, was that substance to which the name of *kopher*, or *gopher*, was applied in the first instance: this name was afterwards (as, in fact, has been the case with its English representative 'pitch',) extended to the Asphaltus, or *mineral* pitch; and from the resemblance which brimstone bears to asphaltus, not only in its mineral origin, but also in the effect produced upon it by heat, the former substance thence derived its appellation, as being 'a substance like *gopher*'.

The opinion advanced by Mr. Carter, that mineral rather than vegetable pitch was the product of the country where the ark was built, and that, in fact, the pitch obtained from wood could scarcely have been known to Noah, is founded upon the assumption that the situation of that country was in the neighbourhood of Babylon, and also that society in that early time existed in a state of infancy as regards its culture and knowledge. On the latter point I will refrain from saying more for the present, than that I apprehend the evidence which we possess upon the subject ought to lead us to a very different conclusion; and with respect to the former assumption, I will refer to the paper "on the former extent of the Persian Gulf," which appeared in your Number for last month (February), from which, if the facts adduced in it are correct, the inference would appear to be that the ark could not possibly have been built anywhere in the neighbourhood alluded to by Mr. Carter.

I am not aware of any ground (beyond mere tradition, which is not to be depended upon,) for attributing any particular locality to Noah's antediluvian residence, excepting that, in the absence of all arguments for a contrary opinion, the most philosophical course is to assume that in the neighbourhood where the ark rested, there also it was built; in which case Armenia or the north of Mesopotamia has the greatest claim to be considered in that light.

It is not, however, intended to enter here upon the discussion of this subject, neither shall I attempt to determine to what particular species of tree the gopher-wood belonged; but agreeing as I entirely do with Mr. Carter, "that a reference to the existing productions of the climate in which the ark was built may not be irrelevant to such an inquiry," I will, in conclusion, merely remark, that the only wood which, in the present day, grows in any considerable quantities throughout the country of Armenia, from the frontiers of Persia as far as Asia Minor, is (as I have been lately informed) a species of white pine, the product of which, namely, pitch, is an article of great traffic among the natives: hence it would seem not unreasonable to imagine that Noah and his sons, after receiving the Divine directions, may have proceeded into the pine forests of their native country, and there built the ark of the wood of the trees עֲצֵי־גֹפֶר (*hatzé-gópher*), which furnished at the same time the pitch כֹּפֶר (*kópher*), or גֹּפֶר (*gópher*), which was necessary for covering and preserving it, and for caulking it and rendering it water-tight.

I am, Gentlemen, yours, &c.

March 8, 1824. CHARLES T. BEKE.

L. *On the Ancient and partly buried Forests of Holderness. By* JOHN PHILLIPS, *Esq., F.G.S., Professor of Geology in King's College, London**.

1. SUBTERRANEAN forests, as they are termed, abound so much on all the coasts of England, and have

* Read to the Yorkshire Philosophical Society on the 4th of March 1834; and communicated by the Author.

so much of a common character, that, except in cases where the trees appear under circumstances likely to elucidate the condition of the country at the time of their growth, there is little utility in adding to the mass of general description on this subject. The following remarks, suggested by two visits to a part of Holderness in which the excavation for a large drain, east of, and nearly parallel to, the river Hull, has laid bare a considerable number of buried trees and other vegetable accumulations at a level greatly below that of the tide of the Humber would therefore, not have required publication, but for their bearing on local, if not general, problems concerning the growth and inhumation of 'alluvial' forests.

2. It is well known that the tide rivers which unite in the estuary of the Humber flow through a vast extent of level country, a great portion of which is at present protected from the inroads of the sea by a general system of embankments. The surface of this level region is naturally either a thick deposit of fine silt or sediment left by repeated ancient inundations, or a sterile bed of vegetable *reliquiæ* called turf or peat moor. A large portion of the peat moors lying below the level of the tide, advantage has been taken of this circumstance to introduce the muddy water into particular situations where it is suffered to stagnate and drop its fertilizing sediment. Trees of several kinds, as oak, alder and fir, lie prostrate in the peat, and there are few questions belonging to the modern geological periods which it is more important to resolve than that which relates to the condition of the country where these trees grew.

3. The country of Holderness is entirely separated from the rest of the Yorkshire levels by the ridge of the chalk wolds: it is a large triangular district, bounded on one side by the German Ocean, on another by the estuary of the Humber, and on the third by the declining plane of the chalk. It is not properly a level, but rather a minutely undulated low district, full of insulated hills and devious ridges, which in one place on the coast swell to 130 feet in height. The winding hollows which embrace these hills in the southern part of Holderness are generally filled with sediment from the tide, which, if allowed free access, would cover them 5, 10, or more feet deep. Besides, there are many little hollows secluded amongst the hills, where formerly detached lakes existed, which have left various characteristic deposits. The title of 'Holderness and the Isles of Holderness', applied to this tract by some of our old writers, must at one time have been very appropriate.

The general basis of this whole region is a mass of clay of a brown or bluish colour, full of fragments of limestone, sandstone, slate, porphyry, granite, &c., derived from Northern and North-western Yorkshire, from Cumberland, and Westmoreland. In particular places a considerable quantity of chalk and flints, derived from the neighbouring wolds, lies in the clay or alternates with it: irregular layers of gravel and sand, containing shells of the neighbouring sea, likewise diversify the aspect of this diluvial deposit. It forms every hill and sinks under every hollow in the whole space between the chalk hills and the sea, and yields at several points Elephantoidal and other quadrupedal *reliquiæ.*

It is exclusively in the ancient tide channels and desiccated lakes that the vegetable accumulations occur. Here they are buried under lacustrine sediments at various levels, sometimes below low-water mark, but generally at greater heights; or covered by layers of sediment from the tide; or bare at the surface. The following descriptions embrace the two latter conditions.

4. In cutting the drain alluded to, it was found that the low lands sloped regularly, though very slowly, from the Humber bank to the northward, so that while the tide level was only 4 or 5 feet above the nearest marsh-land immediately within the banks, it was as much as 10 or 12 feet above that in the interior of the country. This is analogous to the well-known case of the marsh-lands near Lynn. A few littoral marine shells, as *Tellina tenuis*, &c., lay at particular places near the river in the silt, which varied in thickness according to the original level of the general basis of gravelly clay: sometimes the silt rested immediately upon the clay, but in many parts they were separated by a more or less considerable layer of black vegetable *reliquiæ.* Wherever a gravel or clay ridge was cut through, it was clearly proved that the mass of the ridge was continuous with that below the peat and silt, these latter being in every instance terminated against the higher and older land, by which some particular peat deposits were almost completely encircled. The top of the subjacent clay seemed in general of a lighter colour than the rest, and less pebbly, sometimes, indeed, very little pebbly: it was slightly and irregularly undulated, so as to form sinuosities and hollows, in which the vegetable matter was collected more abundantly than elsewhere.

In one place, about 3 miles from the Humber, hazel branches and portions of fir and oak were accumulated in a narrow space like the line of an old flood channel; and the loose nuts, acorn cups, and land shells found here appeared to show that the mass had been drifted to its present situation

No trace of the penetration of roots into the clay beneath was remarked along several miles of cutting: the trees were mostly in fragments, lying in confusion, and collected into a few spots, while the branches and decayed leaves, &c., were more generally spread. Phosphate of iron was frequently seen, but I obtained no bones.

5. These appearances were observed at intervals along a great length of the drain; and they show to a certainty that the term 'subterranean forest' is wholly inapplicable to these vegetable accumulations, if it be meant by this term to describe forests submerged in the place of their growth. On the contrary, the whole is clearly a water-drifted mass of fragmented and uprooted trees, bushes, branches, fruits, land shells, &c. It was interesting to observe that the quantity of trees and peaty matter, where no evidence of river channels could be seen, *increased toward the gravelly hills*; and I was much impressed with the probability that the trees and bushes had grown on these hills, and after falling by decay or violent tempests, had found resting places in the neighbourhood, and been gradually covered by sediment from the tide, then, perhaps, for the first time admitted. Only one layer of peat and trees was anywhere seen, and this observation is general for the low lands connected with the Humber.

6. Between the villages of Swine and Waghen, or Wawn, the drain crosses a remarkable flat morass, of about 100 acres in extent, and circumscribed by gravel knolls, called Wawn Turf Car. Toward the north-western margin of this morass a great number of stumps of trees rise 1 or 2 feet above the coarse grassy surface, and suggest to every beholder the notion of a humid forest, which had perished by violence of wind rather than by decay or the hand of man. They are nearly of equal and rather large size, *stand in attitude of growth*, with expanded roots, at distances such as old and large trees planted by Nature observe. As far as we could find, every one of these stumps belonged to the same kind of tree, viz. the Scotch fir, and the alteration of the interior wood is inconsiderable. If they were ever marked by fire or axe, the traces are obliterated. They are not blackened, except under the surfaces where the roots penetrate into a deposit of peat and buried timber 6 or 8 feet in thickness. From the excavation through this deposit vast numbers of trees were pulled out by main strength, from amidst the carbonaceous mass, and laid in heaps on the bank. These trees were found prostrated *in all directions*, generally with the branches and extreme roots broken off, but in other respects very little injured; they were mostly blackened within, but capable of use in forming con-

structions. They consisted of oak, yew, birch, alder, hazel and fir; none of them, except the yew trees, remarkable for great magnitude, though several of the oaks and pines were long and clean in the stem. It was noticed by the intelligent contractor of the works, that the "yew trees, which were the heaviest wood, lay at the bottom of the peat, the oaks above these, and the fir trees, which were the lightest wood, nearest the top." In consequence of thus lying at the top, the fir trees were found partly burnt along one side in the process of paring and burning to which this morass has been subjected. Amongst these trees many fir-cones, but very few nuts or acorn-cups, were found, and not a single land, or freshwater, or marine shell. Under the trees and peat was the same whitish blue clay, very little pebbly, which has been noticed before. I saw no trace of the passage of roots downward into this clay: the root masses when examined appeared entirely free from adhering earthy substances. The trees were confusedly implicated together; and in general it may be held that the whole mass of peat and trees had been accumulated by drifting from some short distance, after having been so long uprooted and exposed as to be free from earthy matter about the bases, as well as generally deprived of long branches and roots. In this manner we may, perhaps, understand how such a heterogeneous assemblage of trees, requiring different soils and circumstances for their growth, could be crowded into this narrow receptacle.

7. On looking round from this prostrate forest to the neighbouring hills, we are struck by the contrast of the vegetable forms. The most abundant of the trees in the neighbouring hedgerows are ash and oak, the latter of which may, perhaps, be considered as the native occupants of the soil; but where shall we seek for groves of indigenous yew, Scotch fir, and birch? There is probably not one natural wood of much extent in all Holderness, neither are the neighbouring chalk hills woody: it requiries care to rear plantations of selected trees in a country exposed like this to piercing east winds. As there is but one such layer of buried trees, may we not suppose that some great change has happened to the climate of Holderness since the æra of their growth? Do not the parallel deposits on the moors of Westmoreland and Scotland, at heights and in places which now for leagues around the hills are overspread with heath, lead us to think that these physical changes were of rather a general character?

8. I now return to the consideration of the subsequent growth of Scotch firs above the buried timber on a surface of peat which is now 10 or 12 feet below the high-water mark

in the Humber, where the rise of the tide may be taken at 24 feet. How can we explain the growth of trees at the level of half-tide? The notion appears to be general amongst geologists that phænomena of this kind require the admission of local or general changes of level of the land and sea. That such changes have often occurred is indisputable, and few parts of the country have not experienced them. But in this particular instance there seems no necessity for invoking any such operation: what is known of the action of the sea on the coast of Yorkshire and in the estuary of the Humber will give us a more simple and more applicable theory.

In order to see this, we have only to consider what would be the effect upon the movement of the tide over the levels of Holderness if its rampart of banks was removed. Undoubtedly the tide would reach to distances and rise to heights inversely proportional to the amount of the impediments which it had to overcome. The line of high water is not necessarily a *level* line, but, according to the form and depth and direction of the channel up which the tidal impulse is felt, it may be a *rising* line, as in some rivers, or a *declining* line, as in a system of imperfectly connected meres or lakes branching off from some limited tide stream. Now, remembering the description formerly given of the very intricate admixture of winding hills and hollows in Holderness, we can easily see how, over the tortuous, shallow, and often contracted course, at right angles to its main stream in the Humber, the tide water should be slowly and unequally propagated, so as to influence hardly at all the interior hollows of the country. The length of time during which the tide would flow, the distance which it could reach, and the height which it might attain, would all be dependent upon the configuration of the country,—as on the Yarmouth coast the flow of the water is retarded and lowered many feet by the shallow and intricate sand banks.

9. Again, if the aperture for the tide to enter were more contracted and further removed than at present, and if at the same time the interior basins were larger, it is obvious that the flow of the tide toward the shallow and intricate ramifications of these basins might be insensible, and thus hollows might exist and be drained though their surface was below the present tide level. Now it is well known that the whole of the coast of Holderness is subject to a regular and progressive annual waste, and that by this process the configuration of the mouth of the Humber has been greatly changed; large portions of land have been washed away both within and without the Spurn Point, and from the opposite coast of Lincolnshire about Grimsby; and the cliffs of Paghill are only the

remains of a ridge of shelly gravel, which once extended further to the south, and contracted the now broad estuary. We may therefore easily admit that the aperture for the entrance of the tide has been augmented. It is also certain that before the embankments the interior basins were very much, nay, a hundred times, larger than those which now receive the Humber flood. There are, perhaps, 300 square miles of surface bordering the rivers which are below the level of the tide, and therefore must at least in part have been flooded. It might then very easily happen that the land-locked basin of Wawn might be imperfectly drained, wholly free from marine irruptions, and capable of supporting a forest of pines upon earlier accumulations of drifted timber.

10. There is another consideration of some importance in discussions concerning the ancient levels of marshy and peaty countries. The peat bed which occupied perhaps 3 or 6 feet in thickness while wet and spongy, will shrink to half or one third the depth when drained; and if loaded with sediment, as in the process of warping, will be compressed to a few inches. In some cases, therefore, the difference of levels as now existing must be corrected by this rule to give the former difference.

In conclusion, it seems necessary to remark, that it is not intended to convey the impression that this mode of explanation will apply to every case of 'subterranean forests', nor that it is actually applied by me to the whole area of the buried forests of Yorkshire and Lincolnshire. I have here discussed a particular case leading to some general reflections, but am not in possession of data sufficiently minute and accurate for a more general investigation. Neither have I attempted to fix the *date* of the accumulation of the peat, or of the growth of the trees, relative to other geological monuments, further than by showing that it is clearly subsequent to the dispersion of the diluvium in Holderness, and therefore comparable to the æra of the many lacustrine deposits which I have described on the coast of Yorkshire.

I feel it a duty to remind geologists of their obligation to the late Dr. Alderson of Hull, for his early and valuable notices of the timber and peat deposits of Yorkshire, and to express my thanks to Mr. Stichney, of Ridgmont, near Hedon, who first drew my attention to the phænomena which have furnished the subject of this communication.

LI. *Facts relating to Optical Science.* No. II. *Communicated by* H. F. Talbot, *Esq., M.P., F.R.S.**

7. *On Mr. Nicol's Polarizing Eye-piece.*

IN the 20th volume of Jameson's Edinburgh Philosophical Journal (p. 83.), there is a paper by W. Nicol, Esq., entitled, "On a method of so far increasing the divergency of the two rays in calcareous spar that only one image may be seen at a time." The admirable contrivance by which this is effected does not appear to have become generally known, although published so far back as 1828. My own attention was first drawn to the subject from seeing a translation of Mr. Nicol's paper in a German journal, (*Poggendorff's Annalen*, 1833, No. 9. page 182.) the editor of which says, that although upon reading the original paper he did not anticipate any good result from the arrangement proposed, yet that upon making the trial he found that nothing could answer more perfectly than it did. He then goes on to say, that as Mr. Nicol had not attempted to explain the operation of the instrument, he would endeavour to do so, in which, however, I cannot say that I think he has been entirely successful.

Having read this testimony to the merits of the instrument, I was desirous of making trial of it, and I caused one to be constructed according to the directions given by the inventor. I found the performance of it fully justified the praises of the writer in the foreign journal. It may be described in a few words by saying that it possesses the powerful polarizing energy of the tourmaline united with perfect whiteness and transparency, and that thus it enables us to perceive the most delicate tints of the rings of crystals, which the green or brown colour of the tourmaline always injures and often destroys. But as, in my opinion, neither the inventor nor his German commentator has taken a right view of the causes which render this instrument so excellent, I will endeavour to do so.

To commence by a brief description of it.

It is formed by taking a rhomb of calcareous spar, about one inch in length, and one third of an inch in breadth and thickness; dimensions which may easily be given to it by splitting it according to its natural cleavage planes. It will have the form of a rhombic parallelopiped, with considerable obliquity in its planes. It must then be cut into two equal parts along the diagonal of one of its longer faces, and the two halves must then be again cemented together with Canada balsam, in

* No. I. of these Facts will be found in our Number for February, p. 112. —Edit.

the same position which they originally occupied. When the eye looks lengthwise through a piece of doubly refracting spar thus prepared (and therefore very obliquely through the Canada balsam), only one image is seen. If two such instruments are placed one before the other, and the observer looks through both, then, if they are in a similar situation, a printed book can be read through the combination with ease and distinctness; but if one of them be turned round 90°, total darkness is the consequence*.

Now it will be observed, that the inventor attributed the fact of the instrument's producing only one image to a great "divergency" which it causes in the images, throwing one of them aside, out of the field of view. The German writer follows the same idea, but adds, that in his opinion such divergency is caused by the Canada balsam, whose index of refraction being 1·549, is intermediate between that of the ordinary ray 1·654, and that of the extraordinary ray 1·483, which circumstance will (in his opinion) account for the rays being "thrown opposite ways". He adds, that any one "who was not afraid of the trouble" might easily calculate the path of both rays; a remark which shows that his idea was, that they were both transmitted, and diverging from each other. But I find that this great divergency does not in point of fact exist; for by inclining the instrument, a position may be found in which both the images are seen, and they are then very little separated, not more so than they were by the same piece of spar before its bisection and cementation. On gradually altering the position of the instrument, the second image is not seen to move away from the first. But at a certain moment it vanishes suddenly, without leaving the smallest trace of its existence behind. Having thus described the appearances as I have found them, I will give an explanation of them which I hope will be more satisfactory.

As long as the rays composing the two images are incident upon the Canada balsam at moderate obliquities, I will venture to say that it cannot exert any particular discriminating action upon them. But when the obliquity reaches a certain point, one of the images suffers total internal reflection, because the Canada balsam is (with regard to that image) a less refractive medium than Iceland spar. But with regard to the other image it is at the same moment a more refractive medium than the spar, and therefore it suffers that image to pass alone.

* This instrument is now made in considerable perfection by Mr. Watkins, optician, of Charing-cross.

LII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Professor Faraday's Recent Discoveries.

1834. Feb. 13.—PAPERS were read, entitled, "Experimental Researches in Electricity. Sixth and Seventh Series." By Michael Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry in the Royal Institution.

In the course of his experimental investigation of a general and important law of electro-chemical action, which required the accurate measurement of the gases evolved during the decomposition of water and other substances, the author was led to the detection of a curious effect, which had never been previously noticed, and of which the knowledge, had he before possessed it, would have prevented many of the errors and inconsistencies occurring in the conclusions he at first deduced from his earlier experiments. The phænomenon observed was the gradual recombination of the elements which had been previously separated from each other by voltaic action. This happened when, after water had been decomposed by voltaic electricity, the mixed gases resulting from such decomposition were left in contact with the platina wires or plates, which had acted as poles; for under these circumstances they gradually diminished in volume, water was reproduced, and at last the whole of the gases disappeared. On inquiring into the cause of this reunion of the elements of water, the author found that it was occasioned principally by the action of the piece of platina, which had served for the positive pole; and also that the same piece of platina would produce a similar effect on a mixture of oxygen and hydrogen gases obtained by other and more ordinary kinds of chemical action. By closer examination, it was ascertained that the platina, which had been the negative pole, could produce the same effect. Finally, it was found that the only condition requisite for rendering the pieces of platina effective in this recombination of oxygen and hydrogen, is their being perfectly clean, and that ordinary mechanical processes of cleaning are quite sufficient for bringing them into that condition, without the use of the battery. Plates of platina, cleaned by means of a cork, with a little emery and water, or dilute sulphuric acid, were rendered very active; but they acquired the greatest power when first heated in a strong solution of caustic alkali, then dipped in water to wash off the alkali, next dipped in hot strong oil of vitriol, and finally left for ten or fifteen minutes in distilled water. Plates thus prepared, placed in tubes containing mixtures of oxygen and hydrogen gases, determined the gradual combination of their elements: the effect was at first slow, but became by degrees more rapid; and heat was evolved to such a degree, indeed, as frequently to give rise to ignition and explosion.

The author regards this phænomenon as of the same kind as that discovered by Davy in the glowing platina; that observed by Döbereiner in spongy platina, acting on a jet of hydrogen gas in atmospheric air; and those so well experimented on by MM. Dulong and

Thenard. In discussing the theory of these remarkable effects, the author advances some new views of the conditions of elasticity at the exterior of a mass of gaseous matter confined by solid surfaces. The elasticity of gases he considers as being dependent on the mutual action of the particles, especially of those which are contiguous to each other; but this reciprocity of condition is wanting on the sides of the exterior particles which are next to the solid substance. Then, reasoning on the principle established by Dalton, that the particles of different gases are indifferent to one another, so that those of one gas may come within almost any distance of those of another gas, whatever may be the respective degrees of tension in each gas among the particles of its own kind, he concludes that the particles of a gas, or of a mixture of gases, which are next to the platina, or other solid body not of their own chemical nature, touch that surface by a contact as close as that by which the particles of a solid or liquid body touch each other. This proximity, together with the absence of any mutual relation of the gaseous particles to particles of their own kind, combined also with the direct attractive force exerted by the platina, or other solid body, on the particles of the gases, is sufficient, in the opinion of the author, to supply what is wanting in order to render effective the affinity between the particles of oxygen and hydrogen; being, in fact, equivalent to an increase of temperature, to solution, or to any of the other circumstances which are known to be capable of adding to the force of the affinities inherent in the substances themselves.

Some very curious cases of interference with this action of platina and other metals are next described. Thus, small quantities of carbonic oxide, or olefiant gas, mixed with the oxygen and hydrogen gases, totally prevent the effect in question; while very large quantities of carbonic acid, or nitrous oxide gas, do not prevent it: and it is remarkable, that the former of these gases do not affect the metallic plates permanently; for if the plates be removed from those mixtures, and put into pure oxygen and hydrogen gases, the combination of these elements takes place.

The author concludes by some general notice of numerous cases of physical action, which show the influence of certain modifications of the conditions of elasticity at the external surface of gaseous bodies.

The seventh series, which is a continuation of the subject of the fifth, namely, electro-chemical decomposition*, commences with a preliminary exposition of the reasons which have induced the author to introduce into this department of science several new terms, which appear to be required in order to avoid errors and inaccuracies in the statement both of facts and theories. As a substitute for the term *pole*, and with a view to express also a part of the voltaic apparatus to which that name has never been applied, although it be identical with a pole in its relation to the current, the author proposes to employ the term *electrode*. The surfaces of the decomposing body, at which the positive current of electricity enters and passes out, are denominated respectively the *eisode* and the *exode*. Bodies which are de-

* An abstract of Mr. Faraday's Fifth Series of Experimental Researches in Electricity will be found in Lond. and Edinb. Phil. Mag. vol. iii. p. 460

composable by the electric current are called *electrolytes*, and when *electro-chemically decomposed*, they are said to be *electrolyzed*; the substances themselves, which are evolved in such cases, being called *zetodes*, and the terms *zeteisode* and *zetexode* being applied, accordingly as the substance passes in one direction or the other. The propriety and the advantage of employing these new terms, the author observes, can be properly appreciated only by an experience of their uses and applications in the exposition of the theory of decomposition given in the fifth series of these inquiries, and of that of definite electro-chemical action advanced and supported in the present paper.

The first section of this paper is occupied with the consideration of some general conditions of electro-chemical decomposition. It has been remarked, that elements which are strongly opposed to each other in their chemical affinities are those most readily separated by the voltaic pile; and the discovery of the law of conduction, explained in the fourth series *, has led to a great augmentation of the number of instances which are in conformity with this general observation: but it is here shown, that the proportion in which the elements of a body combine has great influence on the electro-chemical character of the resulting substance; and that numerous instances occur where, although one particular compound of two substances is decomposable, another is not. It appears, that whenever binary compounds of simple bodies are thus related to one another, it is the proto-compounds, or those containing single proportions, which are decomposable, and that the per-compounds are not so.

The second section contains an account of a new instrument devised by the author, for exactly measuring electric currents, and which he terms the *volta-electrometer*. The current to be measured is made to pass through water acidulated by sulphuric acid, and the gases evolved by its decomposition are collected and measured, thereby giving at once an expression of the quantity of electricity which has passed. The principle on which this conclusion is founded is the new law discovered by the author, "*that the decomposing action of any current of electricity is constant for a constant quantity of electricity.*" The accuracy of this law was put to the test in every possible way, with regard to the decomposition of water, by making the same current pass in succession through two or more portions of water, under very different circumstances: but whatever were the variations made, whether by altering the size of the poles or electrodes, by increasing or lessening the intensity of the current or the strength of the solution, by varying its temperature or the mutual distance between the poles, or by introducing any other change in the circumstances of the experiment, still the effect was found to be the same; and a given quantity of electricity, whether passed in one or in many portions, invariably decomposed the same quantity of water. No doubt, therefore, remains as to the truth of the principle on which the volta-electrometer acts: but with regard to the practical application of the principle, several forms of the instrument are described by the author, and the mode of employing them, either as the measurers of absolute quantities, or as standards of comparison, are fully pointed out.

* See Lond. and Edinb. Phil. Mag. vol. iii. pp. 449, 450.

In the third section of the paper, the primary or secondary character of the bodies evolved at the electrodes is discussed. It is shown that they are secondary in a far greater number of cases than has usually been imagined; and that laws have been deduced with regard to the ultimate places of substances, from the appearance of the secondary products; so that certain conclusions, true in themselves, have hitherto been obtained by erroneous reasoning, since the facts which were supposed to support them have in truth, no direct relation with those conclusions. The methods of distinguishing primary and secondary results from each other are explained, and the importance of this distinction towards the establishment of the law of definite electro-chemical action is insisted upon by the author.

The fourth section is entitled, "On the definite Nature and Extent of Electro chemical Decomposition," and is considered by the author as by far the most important of this or indeed of the whole series of investigations of which he has now presented the results to the Royal Society. He adverts to the previous occasions on which he has already announced, more or less distinctly, this law of chemical action; and also to the instrument just explained as one of the examples of the principle about to be developed. He next refers to experiments described in another part, in which primary and secondary results are distinguished as establishing the same principle with regard to muriatic acid; the results showing, that not only is the quantity of that acid decomposed constant, for a constant quantity of electricity, but that, when it is compared with water, by making one current of electricity pass through both substances, the quantities of each that are decomposed are very exactly the respective chemical equivalents of those bodies. The same current, for example, which can decompose nine parts by weight of water, can decompose thirty-seven parts by weight of muriatic acid, these numbers being respectively the chemical equivalents of those substances, as deduced from the phænomena of ordinary chemical action.

Cases of decomposition are then produced, in which bodies rendered fluid by heat, as oxides, chlorides, iodides, &c., are decomposed by the electric current, but still in conformity with the law of constancy of chemical action. Thus the current which could decompose an equivalent of water, could also decompose equivalents of muriatic acid, of proto-chloride of tin, of iodide of lead, of oxide of lead, and of many other bodies, notwithstanding the greatest differences in their temperature, in the size of the poles, and in other circumstances; and even changes in the chemical nature of the poles or electrodes, and in their affinities for the evolved bodies, occasioned no change in the quantity of the body decomposed.

The author proceeds, in the last place, to consider a very important question with relation to chemical affinity, and the whole theory of electro-chemical action, namely, the absolute quantity of electricity associated with the particles or atoms of matter. This quantity he considers as precisely the same with that which is required to separate them from their combination with other particles when subjected to electrolytic action, and he brings many experiments to bear upon this point; describing one, in particular, in which the chemical action of 32·5 parts of zinc, arranged as a voltaic battery, was able to evolve a current of electricity capable of decomposing and transferring the elements of 9 grains of water, being the full equivalent of that number. The relation of electricity, thus evolved, to that of the common electric machine is pointed out in a general way, and the enormous superiority as to quantity, in the former mode of action, is insisted upon. In conclusion, the author refers to a statement which he has made in the third series of these researches*, in which he expresses his belief that the magnetic action of a given quantity of electricity is also definite; and he is now more confident than ever that this view will be fully confirmed by future experiment.

ROYAL ASTRONOMICAL SOCIETY.

December 13, 1833.—The following communications were read:

I. Observation of the Occultation of 63 *Tauri*, January 30, 1833, made at Saville Row, by R. Snow, Esq. Also, Observations of the Solar Eclipse of July 17, 1833. The immersion of 63 *Tauri* took place at $7^h\ 58^m\ 41^s{\cdot}00$ sid. time, corrected for clock error. The star started and glimmered just before its disappearance. Instrument, 42-inch refractor, power 80.

The eclipse of the sun was observed as follows:

July 16, 1833.

	h	m	s	
Beginning of eclipse	0	43	14·06	sid. time, rather uncertain.
A small solar spot in contact. .	0	54	43·06	——— well observed.
Ditto disappeared.	0	55	1·06	——— well observed.
End of eclipse.	2	28	39·06	——— excellent.

Latitude of place of observation, 51° 30′ 39″.

Longitude of ditto, $33^s{\cdot}01$ west.

"The eclipse was observed with a 42-inch refractor, power 40. The eye was protected with a medium composed of green and purple glass, which, without in the least distressing it, gives a perfectly *white* image of the sun, excepting that there is a slight tinge of yellow just round the edge of his natural disc; but no such yellowness was perceivable round the edge of the indentation formed by the encroachment of the moon's limb. Towards the end of the eclipse, there was not the slightest perceptible undulation; and so sharply was everything defined, that an intensely black, but excessively fine, outline marked the precise boundaries of the light and darkness in the two bodies. I have before observed the same kind of black band accompanying an extremely well-defined bright line viewed in a dark room."

II. Observation of the Solar Eclipse of July 17, 1833, at Herne Hill, four miles and a half south of St. Paul's. By Captain Horsburgh.

III. Observations of the Solar Eclipse of July 17, 1833, at Bedford. By Captain Smyth, R.N. In a letter to the President.

	Corrected Sidereal Time. h	m	s
Commencement	0	42	45·83
Appulse to a solar spot .	0	54	15·63
Occultation of ditto . . .	0	54	18·13
Illuminated part 9′ 44″ at	1	32	10·63
End	2	28	26·85

* See Lond. and Edinb. Phil. Mag. vol. iii. p. 358, *et seq.*

"The cusps were generally sharp, but were twice sensibly blunted, owing to irregularities in the lunar disc, independent of which their varying in direction was easily observable. There was no scattered light. The macula occulted was the only spot on the solar disc; and it disappeared in the largest valley on the edge of the moon, near the profile of a conical lunar mountain."

The morning was remarkably clear, with a light air from S.S.W.

In a postscript, Capt. Smyth observes: "In the paper by the Rev. W. R. Dawes, 'On the Adoption of a Standard of Optical Power by Observers,' (Lond. and Edinb. Phil. Mag. vol. iii. p. 291,) he has supposed that the emersions of Jupiter's satellites, which are inserted in the fourth volume of the Memoirs of the Astronomical Society, were observed by me with the large refractor, and reasons upon that assumption as a fact. In order that the conclusions may not be drawn from wrong premises, I beg to say that the observations which he cites were made with a five-feet achromatic, as stated in the letter sent with them, and therefore probably under an illuminating power very similar to that used at Ormskirk. Still, the acuteness of vision which Mr. Dawes possesses, as has been shown in several difficult trials, forms a substantial part of the argument for equalizing optical powers."

IV. Observations on the Method of calculating a Lunar Eclipse, with reference to that of December 26, 1833. By J. Freeman, Esq.

V. A portion of a paper was read, on the Construction of Astronomical Instruments. By F. R. Hassler, Esq., Associate of the Society.

FRIDAY-EVENING PROCEEDINGS AT THE ROYAL INSTITUTION OF GREAT BRITAIN.

January 24.—Mr. Faraday on the power of platina and other solid substances to determine the combination of gaseous bodies.—This was an experimental illustration of the sixth series of the author's Researches, an abstract of which is given at p. 291 of the present Number.

January 31.—Dr. Ritchie. Further developments in electro-magnetism and magneto-electricity.—This subject also formed a communication to the Royal Society, and has been reported upon in its place in our Journal.

February 7.—Mr. Dent on the effect of temperature on the balance-spring of time-keepers, and the means of compensating the errors.—Mr. Dent makes both his balance-wheel and spring of glass, and finds very great improvements in the going of the chronometer to result. These seem to be mainly attributable to the perfect elasticity of glass, and the comparatively imperfect elasticity of the malleable metals which have hitherto been used for these springs. The investigation, which is very laborious, is still in progress.

February 14.—Mr. Faraday on the principle and action of Ericsson's caloric engine.—The inventor of this engine has applied the expansion of hot air to produce an available motive power. A five-horse-power engine has been constructed, and appears to work well. A much larger engine is in progress, which will admit of the application of every test of its utility and advantage.

February 21.—Dr. Grant on the development of the nervous system.—Dr. Grant took a simultaneous view of the nervous system of animals, proceeding from the lowest to the highest order; and of the nervous system of man, from the earliest stage of his temporal existence to his full development. He showed that the former presented so many different stages of development, through all of which the human system was carried during its development, and that in any inferior animal the nervous system might be considered as a development of one common plan, up to a certain degree, accordant with the place of that animal in the scale of creation.

February 28.—Mr. Brande on the present state of gas manufacture in the metropolis.—The enormous extent to which this chemical manufacture had in a few short years arisen, was stated and illustrated by numerous statements of the quantities of gas made and coals used, and its increasing importance shown by an account of new manufactures almost daily rising out of it, and dependent on it for their supply of material.

March 7.—Mr. Faraday on electro-chemical decomposition.—This was an experimental illustration of the fifth series of Mr. Faraday's researches, the subject of which has already been noticed in our reports of the proceedings of the Royal Society.

March 14.—Mr. R. Phillips on the conditions and effects of chemical affinity.

March 21.—Mr. Hellyer. A day at Pompeii.—This was an account of the lecturer's personal observations at Pompeii during the past year, in which he compared the accounts of others with the actual state of the place, and brought before the Members many interesting points on the Fine Arts, manufactures and habits of the inhabitants of that town, and of the nation of which they formed a part, as it existed at the time of the destruction of Pompeii.

On the same evening Mr. Faraday called the attention of the Members to a Special General Meeting, to be held on the Monday following, for the purpose of receiving certain munificent gifts made by Mr. John Fuller to the Institution. He formerly gave 1000*l.* to the funds. He then invested 3333*l.* in the three per cents., to found a Chair of Chemistry, with a salary of 100*l.* per annum; of which Chair Mr. Faraday is Professor. He has now invested another 3333*l.* in the same manner, to establish a Chair of Physiology; to which Dr. Roget has been appointed. And he has also invested 3000*l.* three per cents., in the hands of trustees, as a permanent fund, which is to amount, by accumulated interest, additions, or otherwise, to 10,000*l*, and then be available for the service of the Institution. He has, therefore, devoted 10,000*l.* to the purpose of advancing science in this Institution alone.

ZOOLOGICAL SOCIETY.

Nov. 12, 1833.—A letter was read, addressed to the Secretary by M. Julien Desjardins, Corr. Memb. Z. S., and dated Mauritius, June 20, 1833. It was accompanied by an "Extrait du Troisième Rapport sur les Travaux de la Société d'Histoire Naturelle de l'Ile Maurice," of which Society M. J. Desjardins is the Secretary. This extract, containing an account of the Zoological Proceedings of the Mauritius Natural History Society, was read: an abstract of it is given in the 'Proceedings' of the Society.

The Secretary called the attention of the Society to several animals which had recently been added to the Menagerie. They included an *ursine Opossum*, *Dasyurus ursinus*, Geoff., an animal known to the colonists of Van Diemen's Land by the appellation of the *Native Devil*; a *Secretary Vulture*, *Gypogeranus serpentarius*, Ill., presented to the Society by Lieutenant-General Sir Lowry Cole; and two *crowned Cranes*, presented by the same distinguished officer, on his return from the government of the Cape of Good Hope.

Referring more particularly to the latter, he brought under the notice of the Meeting specimens from the Society's Museum of *crowned Cranes* from Northern and from Southern Africa, with the view of illustrating the characters which distinguish as species the birds from these several localities. Their specific distinction, he stated, on the authority of Professor Lichtenstein, had been pointed out, nearly thirty years since, by the Professor's father, who gave to the Cape bird the name of *Grus Regulorum:* this distinction has, however, not been generally known among ornithologists, although to those connected with the Society it has for some time been familiar, from observation both of numerous skins and of living individuals. In the bird of North Africa, for which the specific name of *pavoninus* will be retained, the wattle is small, and there is much red occupying the lower two thirds of the naked cheeks: in that of South Africa the wattle is large, and the cheeks are white, except in a small space at their upper part; the neck also is of a much paler slate colour than that of the North African species. He added that the latter characters had been observed to be permanent in an individual presented to the Society, in April 1829, from the collection of the late Marchioness of Londonderry, and which is still living at the Gardens: they exist also in both the individuals presented by Sir Lowry Cole.

The two species may be distinguished as *Anthropoïdes pavoninus* and *Anthrop. Regulorum.*

Mr. Gray took occasion to remark that the oval form of the nostrils in the *crowned Cranes*, added to other distinguishing characters which have frequently been pointed out, might be regarded as indicating a generic difference between them and the *Demoiselle* and *Stanley Cranes*, in which the nostrils have the lengthened form usual in the genus *Grus*, a genus from which they scarcely differ except in the comparative shortness of their bill. For the group including the *crowned Cranes* the name of *Balearica* might, he thought, be retained; and that of *Anthropoïdes* be appropriated to the one comprehending *Anth. Virgo*, Vieill., and *Anth. paradisœus*, Bechst.

A collection of *crania* and skins of *Mammalia* from Nepâl, presented to the Society by B. H. Hodgson, Esq., Corr. Memb. Z. S., was exhibited. It contained skulls of the *Buansu*, *Canis primævus*, Hodgs., remarkable for the absence of a second tubercular molar tooth in the lower jaw, as described in a communication by the donor read at the Meeting on September 24th, (see page 62); of the *Thibetan Mastiff*, and of the *Pariah Dog*: of the *Indian Jackal* and of the *Indian Fox*, both of which are regarded by Mr. Hodgson as belonging to species hitherto undescribed, the molar teeth of the latter having in their acute tubercles much of an insectivorous character: and of the *Felis Nepalensis*, Vig. & Horsf.

Among the skins were those of a *Sciuropterus*, F. Cuv., as large as *Sci. nitidus*, Ej., and apparently new to science: of a *Sciurus*, Linn., also apparently new: and of a *Herpestes*, Ill., which Mr. Hodgson at present regards as a small variety of the common *Indian Ichneumon*, *Herpestes griseus*, Desm.

At the request of the Chairman, Mr. Gould exhibited a specimen of a *Toucan*, hitherto undescribed, and which he had recently acquired. It is nearly related to *Pteroglossus Aracari*, Ill., and to *Pter. regalis*, Licht.; and Mr. Gould pointed out the characters which distinguish it from those and other species. He proposed for it the name of *Pteroglossus castanotis*.

Mr. Gould also exhibited a *Woodpecker*, inhabiting the Himalayan mountains and also the lower regions of India, which he regarded as new to science. He described it as *Picus flavinucha*.

Captain Belcher laid on the table several specimens of a *Barnacle*, the *Pentalasmis striata*, Leach, remarkable for the great length of their peduncles, which exceeded two feet.

Notes by Mr. Martin of a dissection of a *Puma*, *Felis concolor*, Linn., which recently died at the Society's Gardens, were read, from which the following are extracts.

" Among animals of the feline genus so few points of anatomical difference are found to exist, that the notes of the dissection of one species (allowance being made for relative magnitude,) are closely applicable to that of almost any other. We can therefore only expect to trace out minor differences in structure; and these not among organs essentially connected with the habits and general characteristics of the genus, but with habits peculiar and specific. Hence perhaps we find in this group the greatest difference to obtain in the organs of voice; a circumstance which might naturally be expected, as some according modification must necessarily produce the deep-toned roar of the *Lion*, the snarl of the *Jaguar*, and the hissing cry of the *Puma*.

" The distance between the base of the tongue and the *larynx* in the *Lion*, has been brought more than once under the notice of the Society*; in the *Jaguar*, this distance, comparatively speaking, is nearly as great; but in the *Puma*, an animal equal, or nearly so, in size to the *Jaguar*, the distance is reduced to an inconsiderable space, 1 inch or 1½, according as the tongue is more or less protruded. In addition to this, it is worthy of observation, that the circumference of the *larynx* in the *Puma* is also very inconsiderable: compare, for

* See Phil. Mag. and Annals, N.S. vol. xi. p. 461, and Lond. & Edinb. Phil. Mag., present vol., p. 60.—Edit.

example, the *larynx* of the *Jaguar* with that of the present animal, both natives of the wilds of the American continent. In the *Jaguar* we find a *larynx* indicating from its general magnitude considerable depth in the intonations of the voice, whereas in the *Puma*, if we take either its diameter, or its distance from the termination of the palate and base of the tongue, we are led to expect neither the roar of the *Lion* nor the growl of the *Jaguar*, but the shrill tones of an animal, ferocious indeed, but of all others of the genus perhaps the most stealthy and insidious. I am the more inclined to call attention to these differences, because I think that I have observed a kind of mutual correspondence between the voice and the habits of animals, a point well worthy minute investigation, and on which, on a future occasion, I design to offer a few observations.

Nov. 26.—Specimens were exhibited of a *Bat*, which had recently been obtained by the Society from the collection of the late Rev. Lansdown Guilding, Corr. Memb. Z. S.

Mr. Gray, in directing the attention of the Meeting to them, remarked on them as constituting the type of a new genus, for which he proposed, on account of the shortness of the nose-leaf, the name of

BRACHYPHYLLA.

Dentes incisores $\frac{4}{4}$, *superiorum* intermedii magni conici, approximati, externi minimi; *canini*, $\frac{1-1}{1-1}$; *molares* $\frac{5-5}{5-5}$, quorum anteriores duo utrinque utrinsecus spurii, superiores antici minimi.

Rostrum truncatum; *nasus* a facie sulco profundo sejunctus, *prosthemate* lato, plano; *labium* inferius excisum, excisuræ marginibus verrucosis.

Lingua elongata, undique verrucosa.

Cauda brevissima.

Patagium anale amplum, posticè profundè emarginatum, bi-tendinosum.

Genus *Glossophago*, Geoff., maximè affine.

"The cutting teeth are four in each jaw, of which the two upper central are large, conical and close together; the side ones very small, low and rudimentary; and the lower ones small, equal and closely pressed between the canines. The canines are large, the lower ones fitting before the upper; the upper ones very large, with a deep notch on the hinder side. The grinders are five on each side of either jaw, of which three are true and two false: the two lower false grinders on each side are equal; the front ones of the upper jaw are very small and rudimentary.

"The head is ovate: the face short and blunt: the end of the nose truncated, with a short broad flat nose-leaf, connected with the lips in front, and surrounded by a deep groove behind, separating it from the rest of the face; the groove is edged behind by a rounded callous ridge. The nostrils are ovate, rather large, open, and placed widely apart from each other, one being situated on each side of the middle of the nose-leaf. The lips are smooth, without any beard on the inner side of the angle of the mouth: the upper one is entire; the lower has a deep notch in the centre, which is bald, triangular, and edged with a series of close, short, rounded warts. The tongue is elongated, and is closely and minutely warty.

"The wings are large and broad. The thumb is long, two-jointed, free and sharply clawed; the index finger is composed of two, and the middle of four, bony joints.

"The interfemoral membrane is rather large, and is deeply notched behind.

"The tail is rudimentary, consisting of a single joint imbedded in the base of the interfemoral membrane. It has, in the female, a slight cartilaginous band extending beyond its tip, and separating behind into two diverging bands, one extending to the middle of each shin: in the male, these bands are distinct at their origin.

"The hinder feet are large; their toes are nearly equal, and are strongly clawed.

"This genus agrees with *Glossophaga* in most of its characters, and has the same warty-edged slit on the middle of the under lip, and the same elongated tongue: but it differs in the form and structure of the nose-leaf; in the tongue being covered with rough and closely set warts, which are not placed, as in that genus, in oblique plaits; and in the shape of the central upper incisors, which are elongated and conical, and not short and flat-topped and bevel-edged. In the form of its upper middle incisors it agrees with *Vampyrus soricinus*, Spix; but it is distinguished from that, and from all the other *Vampyri*, by the structure of its under lip and tongue, and by the hinder part of the nose-leaf being separated by a groove from the skin of the forehead. Its interfemoral membrane is somewhat like that of *Vamp. Spectrum*, Geoff., and has the same muscular bands.

Brachyphylla Cavernarum. *Brach. suprà badia, pilorum apicibus saturatioribus; infrà pallidè flavescenti-badia.*

Fœm. *pallidior.*

Long. *corporis* cum *capite*, 4½ unc.; *ulnæ*, 2½; *tibiæ pedis*que *postici*, in mare, 2½, in fœminâ, 2⅓; expansio *alarum*, 16.

Hab. apud St. Vincent's, Indiæ Occidentalis.

"The nose-leaf is oblong, transverse, notched and elevated behind. The *tragus* is triangular, elongated, crenulated on its outer and upper edge and 3-lobed. The face is rather bald in front, with scattered, rigid hairs; and there is a large convex wart, covered with rather rigid hairs, on the back part of the cheek just under the eyes. The wings are dark brown and bald; their front part and index fingers yellow, with a few scattered hairs on the outside of the thicker part near the loins and hinder members. The male is bay above, with the tips of the hairs darker; beneath it is pale yellowish bay. In the female the neck and wings are rather paler.

"This *Bat* inhabits caves in St. Vincent's according to the late Rev. Mr. Guilding, who proposed to call it *Vespertilio Cavernarum*."

Mr. Gray exhibited a drawing of a *Shell*, contained in the collection of Mr. Adamson of Newcastle. It was obtained from the base of the Parremo, near the Volcano of Tolyma, on the east slope of the Andes, and may be characterized as *Bulinus Adamsonii.* This shell approaches most nearly to *Bul. Phasianella*, Val.; it is distinguishable by its bands, the dark colour of its inner lip, and the straightness of its pillar in front.

A paper was read, entitled, "Descriptions of some new Species of Cuvier's Family of *Brachiopoda*, by W. J. Broderip, Esq., V.P.G.S. and Z.S., F.R.S., L.S., &c."

These new species are as follows: TEREBRATULA *Chilensis* and *Uva*, ORBICULA *lamellosa* and *Cumingii*, (approaching nearest to *Orb. striata*, Sow.,) LINGULA *Audebardii* and *Semen*. *Orb. lamellosa* was found by Mr. Cuming in groups, the individuals being in many instances piled in layers one over the other on a sandy bottom, at a depth ranging from five to nine fathoms. At Ancon they were found attached to dead shells, and also clinging to the wreck of a Spanish vessel of about 300 tons, which went down in the bay about twelve years ago. The sunken timbers (for the sheathing was gone to decay,) were covered with these shells, much in the same way that beams on land are sometimes invested with flat parasitic *Fungi*. At Iquiqui they were taken adhering to a living *Mytilus*.

In illustration of Mr. Broderip's paper the *Shells* described in it were exhibited; as were also drawings of them. They form part of the extensive collection made by Mr. Cuming on the western coast of South America.

Mr. Owen read a paper "On the Anatomy of the *Brachiopoda* of Cuvier, and more especially of the genera *Terebratula* and *Orbicula*."

The paper commences by a brief history of the formation of the order by Cuvier, and then refers to the anatomical particulars which have been recorded as regarding *Terebratula* by preceding writers. Among these Pallas seems to have given the best description of the animal. It is on one of this subdivision that the description given by Linnæus of the animal of his genus *Anomia* is founded.

Mr. Owen's materials for the anatomy of *Terebratula* consist of specimens of four species, three of which are inhabitants of the South Pacific Ocean (including one brought home by Mr. Cuming, and two by Captain P. P. King, R.N.); the fourth, *Ter. psittacea*, Brug., was brought from Felix Harbour, Boothia Peninsula, by Commander J. C. Ross, R.N.

The mantle adheres very closely to the valves: the lobe which corresponds to the perforated valve is traversed longitudinally by four large vessels; the opposite lobe is similarly traversed by two such vessels. Its margins are thickened, not as in the *Lamellibranchiate Bivalves* from contraction, but owing to a peculiar structure connected with respiration. They are puckered at regular distances, the puckerings being apparently caused by the insertions of delicate *cilia*, which pass as far within the mantle as they project out of it, but which are so minute as to be observable only by means of a lens. In the interspaces of the *cilia* the margin of the mantle is minutely fringed, and within the fringe is a canal, which extends along the whole circumference. From this canal the large vessels of the mantle lobes take their origin: they may be regarded as the branchial veins conveying the aerated blood to the two hearts, which are situated exterior to the liver, and just within the origin of the internal calcareous loop: they are accompanied in their course by much smaller vessels, probably the branchial arteries. Such is apparently the system of respiration in *Terebratula*.

The *viscera* occupy a very small space near the hinge. The alimentary canal commences by a small puckered mouth, situated immediately behind the folded extremities of the arms. It passes backwards, and expands into a membranous stomach, surrounded by the liver, a bulky gland of a green colour and minute follicular texture, which communicates with it by many orifices. The intestine passes down to the hinge, and then turns to the right side and terminates between the two mantle-lobes. No trace of a salivary gland was found.

The generation of *Terebratula* is that of the ordinary *Bivalves*. In two of the larger specimens the *ova* had insinuated themselves between the layers of the mantle, and partly surrounded the branchial vessels. When so far advanced they obscure the organization of the mantle which adapts it for respiration: this organization is consequently most satisfactorily observed in very young individuals.

Mr. Owen describes in detail the muscles, the arms, and the peculiar internal testaceous apparatus or loop connected with the hinge and supporting the arms. In the species which he examined, with the exception of *Ter. psittacea*, he finds that the loop possesses some elasticity, and when acted on by the muscles becomes in its reflected part sufficiently convex to press upon the perforated valve and separate it slightly from the opposite one; thus compensating for the absence of the thick arms of *Lingula*, which in their protrusion push open the valves, and also for that of the elastic fibres constituting the ligament of ordinary *Bivalves*.

The *Orbiculæ* examined by Mr. Owen consist of specimens of *Orb. lamellosa*, Brod.

Along the whole circumference of the valves shining *cilia* are seen projecting for an extent varying from 2 to 4 lines: they are consequently much longer than in *Terebratula* and in *Lingula anatina*, and are rather longer than in *Ling. Audebardii*, Brod. On examination under a high power they are observed to be beset with smaller *setæ*, which probably gives them greater power in determining the respiratory currents. The mantle is similarly vascular to that of *Terebratula*, there being, in the upper lobe, four principal trunks (comparatively, however, much shorter than in that genus); and two in the lower. These trunks terminate in sinuses, situated close to two strong tendinous membranes, which circumscribe the visceral mass, and to which the mantle-lobes firmly adhere. Here the veins of both mantle-lobes join, and the common trunk or sinus passes obliquely through the membrane, and may be plainly seen distributing *ramuli* over the liver and ovary.

The muscles and *viscera* form a rounded mass, situated in the posterior half of the shell. The mouth is seated between the base of the arms. The *œsophagus* passes obliquely through the tendinous wall of the *viscera* in a direction towards the upper valve: it becomes slightly dilated, and is then surrounded by the liver. The intestine is continued straight to the opposite end of the visceral cavity, is there again contracted, makes a sudden bend upon itself, and returns to the middle of the right side of the visceral belt, which it perforates obliquely, and terminates between the lobes of the mantle a little below the bend of the arm. The liver is of a beautiful green colour, and consists of a congeries of elongated follicles, closely compacted together, which communicate by numerous orifices with the stomach. As in *Terebratula,* there is no salivary gland.

In *Lingula Audebardii,* Brod., there is also no salivary gland; and Mr. Owen is therefore disposed to believe that the gland described as such in *Ling. anatina* by Cuvier, was only a portion of the liver, from which the colour had probably been removed by long maceration in spirit.

In the want of salivary glands the *Brachiopoda* would agree with the ordinary *Bivalves.* Destitute, like them, of any hard parts about the mouth for comminuting alimentary substances, glands for pouring in a fluid to blend with the food during that operation are not wanted.

The nervous system in *Terebratula* was not detected by Mr. Owen. In *Orbicula* two small *ganglia* were found on the side of the *œsophagus* next the perforated valve; from which two filaments, accompanying the *œsophagus* through the membranous wall, immediately diverge and pass exterior to the anterior shell muscles, proceeding with corresponding arteries to near the hearts, beyond which he could not trace them. A single small *ganglion* is situated on the opposite side of the *œsophagus,* but on a plane posterior to the preceding; this is probably the cerebral *ganglion* for giving off nerves to the free spiral extremities of the arms, close to the base of which it is situated.

Mr. Owen exhibited, in illustration of his paper, drawings of the several objects described in it.

The following Notes relative to the period of Uterine Gestation and the Condition of the new-born Fœtus in the *Kangaroo, Macropus major,* Shaw, were read by Mr. Owen.

"Perhaps there is no question in animal physiology that has given rise to more numerous and contradictory theories, and in which fewer facts have been well ascertained, than that which relates to the generation of the *Marsupial Animals.*

"In the present communication I propose to limit myself to the narration of some of the circumstances that have occurred in elucidation of this subject during a series of observations which have been made at the Gardens in Regent's Park during the past summer.

"All the *Kangaroos* at the Farm were for this purpose transferred from the Farm to the Gardens at the latter end of June. The whole stock consisted of two males and six females, all fully grown. The animals of different sexes were kept apart until they had become in some measure accustomed to the gaze of visiters, and reconciled to their new abode.

"It was to be expected that some accidents would occur in exposing so timid an animal, and one whose locomotion is of so violent a kind, to this change; and shortly after their arrival one of the females died in consequence of leaping against the wire fence. It is, however, probable, from the appearances observed on the *post mortem* examination of subsequent cases, that this, like the other individuals, was rendered highly excitable by great determination of blood to the brain. When the remainder had become more habituated to their new circumstances, the experiments were commenced, and the first step taken was to examine the pouches of all the females.

"The 1st female had previously been kept at the Gardens, and had a young one, which measured about 1 foot 2 inches from the nose to the root of the tail: this, of course, had quitted the nipple and the pouch, and now only returned occasionally to suck. There was no other young one in the pouch. The right superior nipple was the one in use; it was nearly 2 inches long and $\frac{1}{3}$rd of an inch in diameter, the gland forming a large swelling at the base. The other three nipples were everted, and about $\frac{1}{2}$ an inch in length.

"A 2nd female, from the Farm, had a young one attached to the lower nipple on the right side. It measured about 7 inches from the nose to the vent, was naked, with the skin of a bright pink colour, being still, in the language of M. De Blainville, a mammary fœtus. The nipple in use was $1\frac{1}{2}$ inch long from the gland to the mouth of the fœtus; the rest were everted, and about the size of those in the first-mentioned female.

"The 3rd female had a mammary fœtus, about 4 inches long from the nose to the vent, adhering to the left lower nipple, covered like the preceding with a naked vascular integument, which probably assists in oxygenating the blood. The eyes in this, as well as in the preceding, were closed. The other nipples were everted, but were not all of the same length, the right lower nipple being shorter than the right upper one. I could not ascertain when this female had been impregnated.

"The 4th and 5th females had no young in the pouch; all the nipples were everted.

"From this examination two facts were ascertained; 1st, that the *Kangaroo*, at least in a state of captivity, has no particular period or season for breeding; and 2nd, that the upper as well as the lower nipples are used both during the period of mammary gestation and for the young animal's subsequent supplies of nourishment.

"With respect to the female No. 2., the following facts relative to her gestation were obtained from Joseph Fuller, Head Keeper at the Farm. She received the male on the 14th of September 1832; but copulation might also have occurred previously. On the 14th of October of the same year Fuller observed her looking sickly, and when the male approached her she scratched and repulsed him.

He perceived much slime, like white of egg, passing from the *vagina*. This was about 3 p.m., when he was unfortunately called away on some business. In the evening, at 8 o'clock, suspecting that parturition had taken place, he examined her pouch, and found a young one attached to a teat: on being touched the young one dropped off to the bottom of the pouch. Next day he again examined her, and found the young one adhering to the nipple. It fell off a second time on being handled, and both Joseph and Devereux Fuller had the little one in their hands out of the pouch, and both assert that it was not more than 1 inch in length. It was again put into the pouch, and the mother was meddled with no more till the 3rd of November following. On that day Mr. Yarrell and myself visited the Farm, and on hearing this account we examined the female, and found the young one, now 3 inches long, adhering strongly to the nipple. On further questioning Fuller on the subject, he said, that when first he saw the young one it was covered with blood-clot or *coagulum*; but on the following day it was quite clean and dry, and moved its body vigorously. The mother still suckles one of the previous year.

"From Mr. Morgan's experiments it would appear that when the mammary fœtus has arrived at nearly the size of a fully grown *Norway Rat*, it will bear a separation from the nipple for two hours, and regain its hold. According to Fuller's statement it will bear a separation from the nipple, and again become joined to it, at what is now proved to have been a very short period after uterine gestation; and Mr. Collie's observations, in the 18th Number of the 'Zoological Journal', are in confirmation of the same opinion. It is still uncertain in what manner it regained the nipple, although in a subsequent experiment, where a similar fœtus was detached, the mother made many, but, as it appeared, unsuccessful, attempts to replace it.

"In order to ascertain precisely the period of gestation, as an essential guide to future experiments, the female No. 1. was selected, she being still suckling the young one of the previous year, and being known not to be impregnated. She was placed with the male only at such times as they could be watched.

"The *coitus* was observed on the 27th of August at 1 p.m. She was separated from the male the same day, and was kept in a distinct shed and paddock until parturition took place. In order to inure her to the examinations of the pouch when they should become indispensable, they were commenced six days after the copulation, and were repeated every morning and evening by James Hunt, the intelligent Keeper whose services were allotted to me by the Council during these investigations. At many of these examinations I was present, and the following are among my notes made on those occasions.

"Sept. 6th.—10th day of gestation. Pouch tolerably free from secretion; the right upper nipple about 2 inches long and $\frac{1}{3}$rd of an inch in diameter; the young one, which has left the pouch, still sucking occasionally; the other nipples as when first examined.

"Sept. 11th.—15th day. No alteration in the pouch or nipples; the young one still sucking occasionally.

"Sept. 30th.—34th day. The young one that was sucking is dead. The nipple in use by it has begun to shrivel, and the brown secretion to form.

"Oct. 4th.—38th day. Hunt observed the female in the afternoon putting her nose into the pouch, and licking the entry. He examined her at 6 in the evening; but a slight increase of the secretion was the only perceptible change, and there was no appearance in the nipples indicative of approaching parturition.

"Oct. 5th.—39th day. Hunt examined the female at 7 a.m. and found the young one attached to the nipple. No blood or albuminous discharge could be detected on the litter, nor any trace of it on the fur between the *vagina* and orifice of the pouch. As the birth took place in the night, the mother had probably had time to clear away all indications of it.

"I repaired to the Gardens the same day and examined the pouch. The young one was attached to the left superior nipple: it resembled an earth-worm in the colour and semitransparency of its integument, and adhered firmly to the point of the nipple. It breathed strongly but slowly, and moved its fore legs when disturbed. Its body was bent upon the *abdomen*, its short tail tucked in between the hind legs, which were about one third shorter than the fore legs, but the three divisions of the toes were distinct. The whole length, from the nose to the end of the tail, would not exceed 1 inch 2 lines. A linear longitudinal mark of the *umbilicus* was apparent.

"It has been asserted by Barton that the young of the *Opossum* immediately after birth are in a much more imperfect condition than that above described in the *Kangaroo*, being merely gelatinous corpuscles, comparable to a *Medusa*; but the later observations of Dr. Rengger on an *Opossum* (*Didelphis Azaræ*, Temm.,) nearly allied to the *Virginian* species (*Did. Virginiana*, Cuv.,) accord as to the condition of the new-born fœtus with what we have now been able to ascertain with accuracy is the condition of the new-born *Kangaroo*.

"Oct. 9th.—I again examined the pouch; the young one was evidently grown, and respired vigorously. I determined to detach it from the nipple for the following reasons: 1st, to decide the nature of the connexion between the fœtus and nipple; 2nd, to ascertain, if possible, the nature of the mammary secretion at this period; 3rd, to try whether so small a fœtus would manifest anything like voluntary action to regain the nipple; and, lastly, to observe the actions of the parent herself to effect the same purpose, as we might presume they would be instinctively analogous to those by means of which the fœtus was originally applied to the nipple, supposing that to take place through the agency of the mother.

"An organical connexion by vessels between the mammary fœtus and the nipple being a necessary consequence of the truth of Dr. Barton's assertion as to the condition of the product of generation at uterine birth, this has been much insisted upon; a discharge of blood has been described as a concomitant of marsupial birth; and even the anastomoses of the maternal vessels with those of the fœtus have been speculated upon. (See *Mém. du Muséum*, tom. ix. p. 393.)

"The dissections of the mammary fœtus of the *Kangaroo* by Mr. Hunter, showing the relation of the nipple to its tongue and mouth, the passage of the *larynx* into the posterior *nares*, the absence of the *urachus* and umbilical vessels, &c., tended indeed to disprove the theory of the vascular connexion; and the observations of Mr. Morgan and Mr. Collie, with the testimony of Joseph Fuller, were completely subversive of it. Nevertheless it was desirable to have ocular demonstration of the real state of the facts at this early period of the young animal's existence.

"It was removed from the nipple without the slightest trace of laceration of continuous vessels, or of any kind of connecting substance: but it adhered more firmly than I had been led to expect from Fuller. After it was detached, a minute drop of serous milk appeared on pressure at the point of the nipple: this was the smallest part of the nipple, and was not swollen or clavate; about half a line had entered the mouth of the fœtus.

"The young one moved its extremities vigorously after being detached, but made no effort to apply its legs to the fur or skin of the mother so as to creep along: it seemed perfectly helpless. It was deposited at the bottom of the pouch, and the mother was liberated and carefully watched. She immediately showed symptoms of uneasiness, stooping down to lick the orifice of the *vagina*, which she could easily reach, and scratching the exterior of the pouch with her fore paws. At length she grasped the sides of the opening of the *marsupium* with her fore paws, and drawing them apart, just as one would open a bag, she thrust her head into the cavity as far as her eyes, and could be seen moving it about in different directions. During this act she rested on her tripod, formed by the *tarsi* and tail. She occasionally lay down, but in that posture never meddled with the pouch: when stimulated to do so she immediately rose, and repeated the process of drawing open her pouch and inserting therein her muzzle, which she sometimes kept in for half a minute at a time. I never observed her put her fore legs, or either of them, into the pouch; these were invariably employed to widen the orifice, or in scratching the exterior. When she withdrew her head, she generally concluded by licking the orifice of the pouch and swallowing the secretion.

"After repeating the above act of insertion at least a dozen times, she lay down and seemed at ease. When she had rested quietly about a quarter of an hour we examined her again, and found the young one not at the bottom of the pouch, but within 2 inches of the nipple. It was moving its extremities, and respiring as vigorously as before. I attempted to replace it on the nipple, but without success; it was therefore left in the pouch, and the mother was released.

"My engagements prevented me from visiting the Gardens until the day but one after this examination, when at 10 a.m. I examined the *marsupium*; but the fœtus was gone. We searched very carefully every portion of the litter, &c., in the hope of finding it, but without success. I concluded, therefore, that the fœtus had died, and that the mother had probably eaten it.

"From what I observed of the mother after the separation of the fœtus, I should conclude that parturition takes place in the erect and not in the recumbent posture; and on perceiving the ease with which she can reach with her mouth the orifices of the *vagina* and pouch, a means adequate to the removal of the young from the one to the other became obvious. I should suppose the fore paws not to be used for the transmission of the fœtus, but to keep open the pouch ready for its reception, while the mouth would be the means by which it would be deposited therein, and perhaps held over a nipple till the mother felt the sensitive extremity grasped by the young one.

"This mode of removal is consistent with analogy. *Cats, Dogs* and *Mice* transport their young by the mouth.

"I ought, perhaps, to have forborne this hypothesis when an opportunity of actually observing the process may so soon be afforded; but it was suggested by observing the actions of the mother after an artificial separation of the fœtus from the nipple, and accords with the phænomena better, I think, than any that have previously been proposed. There is no internal passage; there is no power of bringing the mouth of the *vagina* in contact with that of the pouch, either in the living or dead *Kangaroo*, without lesion of the parts; the fore paws could not so effectually protect the tender embryo from the external air as the lips, nor so safely ensure its passage; and the young one itself did not by any of its actions give the idea of its having the power of creeping up along the fur to the pouch or nipple.

"Where, however, the structure of the pouch, as in *Perameles* and some South American *Opossums*, is different, the mother's aid may be less necessary; but the period of gestation being now ascertained, every endeavour will be made to clear up this part of the problem *ex visu*."

LINNÆAN SOCIETY.

March 4, 1834.—The following communications were read:

A description of a new species of Parrakeet from Western Australia belonging to Mr. Vigors's genus *Nanodes*: by Mr. William Tucker. This bird comes very near to *Nanodes undulatus* (*Psittacus undulatus* of Latham), but differs from it by the presence of a number of circular spots of an intense purple on the throat. Mr. Tucker has named this new species *Nan. formosissimus*.

A letter, addressed to the Secretary, giving an account of a new species of *Fungus* of the genus *Geastrum*, discovered in the Island of Tortola in the West Indies, by Robert H. Schomburgk.

A paper by John O. Westwood, Esq., F.L.S. on *Embia*, a genus of insects allied to the White Ants, with the descriptions of three species, one of them, hither to unnoticed by entomologists, from India, and named *Embia Saundersii*, after its discoverer William Wilson Saunders, Esq., F.L.S. The following are the characters of this new species:

E. Lutescenti-fuscescens, incisuris abdominalibus dilutioribus, alis pallidè fuscescentibus, vittis 5 angustissimis albis inter nervos longitudinales positis.

Long. corp. lin. $3\frac{1}{4}$; expans. alar. lin. $5\frac{1}{4}$.

The other two species are *E. Savignii*, figured by Savigny in the great French work on Egypt (*Neuroptera*, pl. 2. f. 9), and *E. Brasiliensis*, the *Olyntha Brasiliensis* of Mr. G. R. Gray in Griffith's translation of Cuvier's Animal Kingdom. The author regards these species as constituting the types of three subgenera, distinguished by the number of joints in the antennæ, and the disposition and ramification of the nervures in the wings.

March 18.—The following communications were read:

Additional remarks on the *Tropæolum pentaphyllum* of Lamarck, by Mr. David Don, Libr. L.S.

This paper is a supplement to Mr. Don's account of the plant forming the subject of his remarks, which was read before the Society on December the 18th 1832, and was noticed in the Lond. and Edinb. Phil. Mag. vol. ii. p. 67. In that paper he showed that this plant ought to be regarded as the type of a new genus, for which he proposed the name of *Chymocarpus*. In the present communication, which is accompanied by a drawing, and in illustration of which the flowers and fruit, preserved in spirits, were also exhibited to the Society, the author notices several interesting facts which an examination of living specimens in a more perfect state has enabled him to supply, and which greatly strengthen its claims to be regarded as the type of a distinct genus. Among these is the persistent nature of the calyx, so different from that of *Tropæolum*, which is strictly deciduous; and not only is the calyx persistent, but it undergoes considerable changes during the progress of the fruit towards maturity. Mr. Don attributes the greater regularity and shortness of the limb of the calyx in this plant to the almost total absence of petals, and to the diversion of a considerable part of the nourishment to supply the greater development of the tube. He concludes by making some additions to the technical part of his former description.

Remarks on some British Ferns, also by Mr. Don.

The author's attention having been lately directed to the examination of some species of Ferns more recently added to the British Flora, he lays the results of his investigation before the Society in this paper: they are briefly as follows:

Aspidium dumetorum, a species first proposed by the late President of the Society, Sir J. E. Smith, in the fourth volume of the English Flora, is made up of two plants; the one, from Cromford Moor, being a dwarf state of *A. dilatatum*; and the other, from Ravelston Wood, near Edinburgh, being nothing more, as is proved by a close inspection of the original specimens in the Smithian Herbarium, than an accidental variety of the same species *A. dilatatum*, arising from disease, which is shown by the sudden termination of the costæ and by the partial decay of the other segments. The distinctions derived from the fructification, given in the English Flora, are altogether fallacious, being dependent on age. It is clear, therefore, Mr. Don concludes, that the *Aspidium dumetorum* must be erased from the list of species.

Aspidium rigidum. This species has recently been introduced into the British Flora on the authority of a plant gathered on Ingleborough by the Rev. W. T. Bree, and by him communicated to Dr. Hooker, by whom it has been published in the Supplement to English Botany, and in the second edition of his British Flora. Mr. Don has not seen an authentic sample of the English plant; but the figure in the first of the works just named, which is unfortunately taken from a cultivated plant, does not well accord with foreign specimens of *rigidum*, and a sample in Mr. Forster's herbarium, stated to have been taken from a plant received from Mr. Bree, proves to be nothing but a dwarf state of *Nephrodium Filix Mas.*

Asplenium Filix fœmina. Of the two very marked varieties of this plant, that which has the segments of the more delicate texture, and the entire frond of a pale green, varies much in size according to soil and situation: in damp shady places it becomes the *filix fœmina* of English Botany, and in more open exposed situations the *irriguum*, but neither of these states, Mr. Don observes, is entitled to be regarded as a distinct form.

Cystea dentata. This plant appears to be peculiar to the Scottish Alps, on which it was discovered by Dickson and the late Mr. Don; for the author, after an attentive comparison of specimens from various stations, both in the Smithian Herbarium and in that of Mr. Forster, is satisfied that the Welsh plant is not in reality different from *fragilis*, from which *dentata* was first distinguished by Dickson. The Scottish plant is distinguished by its broader, rounded pinnæ, with short, blunt teeth, rather crenate than serrated, with the costæ more conspicuous and flexuose. The *angustata*, Mr. Don fears, must also be reckoned a variety of *fragilis*, as he can find no essential mark whereby to separate it.

Cystea regia. Dr. Hooker regards this and the *alpina* as identical, but to Mr. Don these plants appear to be essentially different; *regia* being distinguished from *alpina* by its more compact frond, by its shorter, broader, and cuneiform segments, and by the still more important characters of its more copious sori, and of its narrow and tapering indusium. It is hoped that some fortunate botanist will discover a British station for this plant, for the original one at Low Layton no longer exists, and the Welsh specimens belong to *fragilis*. The *alpina* is accurately represented by Schkuhr, Jacquin, Seguier and several other authors; but of the present species there is no authentic figure, except that in English Botany.

The real structure of this genus, Mr. Don remarks, has been misunderstood by most botanists. The *Cysteæ* are in fact *Asplenia* with abbreviated sori, the insertion and structure of the indusium being precisely similar in both genera.

Blechnum boreale. Mr. Don only notices this plant because he finds that it is still referred by most botanists to *Blechnum*, although it properly belongs to *Lomaria*, as Mr. Brown long ago suggested, agreeing remarkably with that genus, in habit, in having two kinds of fronds, in its marginal indusium, &c.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

A meeting was held on Monday evening the 17th of February, Dr. Clark, V.P., being in the Chair. Among the presents was a *Proteus anguinus*, offered by Mr. Lunn, with some observations on the history of our knowledge of the animal. Professor Miller communicated a notice of some optical experiments, by which it appeared that the lines seen in the vapour of bromine and iodine are identical in position; and that the vapour of perchloride of chrome exhibits lines apparently equidistant, much closer and fainter than the bromine lines, but occupying the same part of the spectrum. Mr. Whewell read a memoir on the nature of the truth of the laws of motion, tending to show that these laws may be demonstrated independently of experiment so far as their terms go; but that the meaning of the terms must be assigned by a reference to experiment.

A meeting was held on Monday evening, March the 3rd, Dr. Clark, V.P., being in the Chair. A memoir was first read by the Rev. J. Challis, containing new researches in the theory of the motion of fluids.

The Rev. T. Chevallier described experiments which he had made on the polarization of light by the sky. The general results were, that light is polarized by the clear sky; that the effect begins to be sensible at points thirty degrees distant from the sun; and that the greatest quantity of polarized light proceeds from points at ninety degrees distance from the sun, a fact which seems to indicate that the reflection, which occasions the polarization, takes place at the surface of two media as nearly as possible of the same density. It was also stated that though the light of the moon or of clouds shows no trace of polarization, a fog, when on the point of clearing off, lets polarized light through when its breaking up has not yet begun. Mr. Chevallier remarked that he had not detected any appearances of polarization by transmission, though, as was mentioned by another member, M. Arago has stated that he had observed, within a certain small distance of the sun, that the light was polarized in the opposite plane to that at a greater distance.

A meeting was held on Monday evening, March the 17th, Professor Airy, one of the Vice-Presidents, being in the Chair. Mr. Power gave an account of his views concerning the cause of the phænomena of exosmose and endosmose; which it appeared by his calculations may be accounted for by the effect of forces similar to those which produce capillary phænomena.

Professor Henslow gave an account of the speculations of M. Braun respecting the spiral arrangements of the scales on the cones of pines, illustrated by drawings and additional observations.

Professor Airy gave an account of experiments on the polariza- of light by the sky. It appeared that the light was polarized in a plane passing through the sun, and that the plane of polarization was not reversed in approaching the sun as had been formerly suggested by M. Arago. Professor Airy found that he could observe the polarization within nine degrees of the sun in a horizontal direction; but that above and below the sun the traces disappeared at a distance considerably greater. It was found in the course of these experiments that very rough surfaces, as a stone wall, a gravel walk, a carpet, produced some polarization by reflection; and that the plane of polarization in all cases passed through the point of reflection and the source from which the light came. This communication gave rise to observations from other members.

LIII. *Intelligence and Miscellaneous Articles.*

ACTION OF IODINE ON STARCH.

M. LEROY, of Brussels, has found that water is requisite to the production of the blue colour which is produced by the action of iodine on starch: in alcohol the iodine becomes merely of a dark brown colour, and water causes the blue colour to appear.

M. Chevallier has also remarked that farinaceous substances mixed with starch, which are always moist, when subjected to the vapour of iodine, acquired a brown colour, while potato starch became of a golden yellow merely. He concluded, from this circumstance, that iodide of starch is of a yellow colour, and that this, by absorbing water, became blue hydrate, and he found that when this yellow compound was touched with a moistened tube, it became instantly blue.—*Ann. de Chim. Médicale*, May 1833.

ANALYSIS OF OIL OF CLOVES, INDIGO, &c.

M. Dumas found that oil of cloves, even when perfectly transparent, may still contain much water, which may be separated by merely digesting it at a temperature of 140° to 175° with chloride of calcium: the solution of chloride subsides to the bottom of the vessel, and the pure oil floats upon it.

The atomic weight of the oil was attempted to be determined by combining it with soda; but definite compounds could not be obtained; for when the oil is heated with the alkali, a pearly mass forms on cooling, which contains an enormous quantity of free alkali, that cannot be separated.

Excess of ammoniacal gas was passed into the oil, and a compound was obtained which, estimating 2·125 as the atomic weight of the ammonia, would give 2·2 as that of the oil of cloves (or taking ammonia at 17, that of the oil would be nearly 18). The compound formed is in small very brilliant crystals; but they decompose immediately on coming into the air, and therefore their analysis was not attempted.

The following are the experimental and calculated compositions of oil of cloves.

	Expt.		Calculated.	
Carbon	70·04	or 40 atoms	1530·4	70·02
Hydrogen ..	7·88	— 26 ——	161·5	7·42
Oxygen....	22·08	— 5 ——	500·0	22·56
	100·00		2192·9	100·00

It will be observed that these results agree with that obtained from the ammoniacal compound. M. Dumas remarks that there are two circumstances which characterize the oils that are heavier than water, viz. the great quantity of oxygen which they contain, and the great resistance which they oppose to decomposition, oil of cloves requiring to be slowly passed in vapour over a column of 6 or 8 inches of oxide of copper.

A substance deposited from the distilled water of cloves, and which was crystallized in pearly scales, yielded by analysis,

Carbon	72·25	or 40 atoms	1530	73·55
Hydrogen ..	7·64	— 24 ——	150	7·21
Oxygen	20·11	— 4 ——	400	19·24
	100·00		2080	100·00

M. Dumas considers this as a peculiar compound requiring further examination: at first he was inclined to suppose it to be an isomeric state of the oil of cloves.

Caryophilline is a peculiar substance, which after some time is deposited in a crystalline form from oil of cloves: this was found by M. Dumas to consist of

Carbon	79·75	or 40 atoms	1530·4	79·27
Hydrogen ..	10·48	— 32 ——	200·0	10·36
Oxygen....	10·22	— 2 ——	200·0	10·37
			1930·4	100·00

This composition is identical with that of natural camphor.

M. Dumas analysed indigo without previous purification, and such as chemists have hitherto subjected to examination. He found it to consist of

Carbon	71·94	
Hydrogen	4·12	4·05
Azote	10·30	
Oxygen......	13·64	
	100·00	

These results differ but little from those previously given, and prove that they were sufficiently correct.

There were afterwards subjected to analysis: 1st, sublimed indigo purified by alcohol; 2nd, precipitated indigo purified by alcohol; 3rd, precipitated indigo, made hot and washed with boiling alcohol; 4th, the same, again washed with boiling potash, and then with boiling alcohol.

The results differed but very little; the mean gave,

Carbon	72·80	or 45 atoms	1721·7	....	72·34
Hydrogen	4·04	— 15 ——	93·7	....	3·97
Azote	10·80	— 15 ——	265·	....	11·13
Oxygen	12·36	— 3 ——	300·	....	12·60
					100·00

According to Berzelius, deoxidized indigo, when mixed with sulphate of copper, takes from it such a quantity of oxygen, that 100 parts of deoxidized indigo require 4·6 of oxygen to become blue indigo.

Indigotic acid, of perfect whiteness, and consequently free from carbazotic acid, which it contained when coloured, gave,

Carbon	48·23	or 45 atoms	1721·7	....	48·09
Hydrogen	2·76	— 1 ——	93·7	....	2·61
Azote	7·73	— 3 ——	265·0	....	7·40
Oxygen	41·28	— 15 ——	1500·0	....	41·90
	100·00		3580·4		100·00

M. Dumas remarks that indigotic acid is merely indigo highly oxidized, containing 5 times more oxygen than blue indigo. *Carbazotic acid* is the substance formerly known by the name of *Amer de Welter*. All animal matters treated with nitric acid may furnish carbazotic acid: it resists the action of the most powerful acids and forms detonating salts.

It is readily formed by treating indigotic acid with concentrated nitric acid: the carbazotic acid employed by M. Dumas was prepared from indigo, and was first converted into carbazotate of potash, which was carefully purified, then decomposed, and the acid repeatedly crystallized. The mean results of several experiments were

Carbon	31·8	25 atoms	940·4	....	31·3
Azote	18·5	6 ——	531·0	....	17·7
Hydrogen	1·4	6 ——	37·5	....	1·3
Oxygen	48·3	15 ——	1500·0	....	49·
	100·0		3008·9		100·0

The conversion of indigotic into carbazotic acid may be explained by the abstraction of the elements of oxalic acid and ammonia from the former, and the addition of nitric acid: this will give the exact composition of carbazotic acid. When carbazotic acid is boiled with a concentrated alkaline solution, much ammonia is evolved, and a salt of an intense red colour is obtained, much resembling that produced by the croconic acid of Gmelin.—*Journal de Chimie Médicale*, October 1833.

MODE OF OBTAINING AZOTE IN ANALYSES.

M. Dumas obtains the azote from substances containing it by the following process: The decomposing tube (in which oxide of copper is used) is disposed as usual; but at its closed end there are placed some grammes of carbonate of lead. After having exhausted the tube, a part of the carbonate of lead is to be decomposed, in order to expel the portion of air which remains in the apparatus and replace it by carbonic acid gas; then, after having expelled about 60 cubic inches of carbonic acid, exhaust a second time: the combustion is then to be performed in the usual way. The gases are mixed over mercury, in a receiver containing a strong solution of potash. When the decomposition is over, the carbonate of lead is to be again heated, and from 60 to 120 cubic inches of carbonic acid gas are to be formed, so as to drive all the azote from the apparatus, and to convey it to the receiver; by properly agitating which, all the carbonic acid gas is absorbed and pure azotic gas remains, which may be measured with precision. The only precaution to be taken is that of decomposing a quantity of matter capable of producing at least from 12 to 16 cubic inches of azotic gas.—*Journal de Chimie Médicale*, Oct. 1833.

ON THE ACTION OF GASES HURTFUL TO VEGETATION.

M. Macaire, in making some experiments, an account of which he gave to M. De Candolle, mentioned an accident which had delayed them, viz. the death of several plants by the exhalation of chlorine. M. De Candolle recommended that it should be tried whether this hurtful action occurred during the day or night, observing that chemists who had been consulted on the subject of the exhalations from manufactories, had almost always insisted, that according to their experiments the gas had no action on vegetables. M. De Candolle suspected that these experiments were probably made in the day, a time in which plants do not absorb gases, which would account for the different results obtained The following experiments were made at his suggestion.

Chlorine.—Plants of euphorbium, mercury, groundsel, cabbage, and sowthistle, which had well taken root, were placed in the morning in a large vessel, in which chloride of lime had been introduced. The roots were steeped outside the vessel: the quantity of chlorine disengaged was far from being sufficient to alter the vegetable tissue. In the evening the plants had not suffered, and the odour of chlorine remained the same.

The same plants, after having passed the night in the same vessel, to which no chlorine was added, were found quite withered in the morning, except the cabbage, which had resisted.

The smell of chlorine had entirely disappeared, and was replaced by that of a disagreeable acid.

This was repeated several times, making the disengagement of chlorine stronger, and the result was the same; and the plants in the daytime withstood a strong atmosphere of chlorine, while a much weaker dose killed them at night.

Nitric acid.—The experiment, began at night, with the vapour of nitric acid, showed that the plants were withered in the morning, and some leaves were rendered brown by the action of the acid. The same quantity of acid was tried in the daytime, and though several leaves were made brown, the others were not withered.

Nitrous acid gas.—This gas appears to be violently poisonous to plants, and at night it kills them in very small quantity; but in the daytime they do not appear to be sensibly altered, although the disengagement of gas is abundant.

Sulphuretted hydrogen.—The results of the experiments with this gas were precisely similar to those of the last. The plants left during the night in the same mixture of gases which did not at all injure them in the light, were quite withered in the morning, and the gas was absorbed. Cabbage only resisted.

Muriatic acid gas.—The results were similar to the last. The plants did not perish in the daytime, even when there was gas enough to render one or two leaves brown: they were completely dead in the morning, leaving the peculiar smell already mentioned. Cabbage was still an exception.

It appears, then, from these experiments, that several gases are hurtful to vegetation, but that their action occurs only during the absence of light, as M. De Candolle had foreseen.—*Annales des Sciences Naturelles*, April 1833.

LEDERERITE, A NEW MINERAL.

This substance was found by Dr. Jackson and Mr. Alger at Cape Blomidon, in Nova Scotia, beneath a precipice of basaltic rocks, from which it had recently fallen, with a large vein of stilbite, mesotype and analcime. The crystals are generally implanted in analcime or stilbite; some are colourless, transparent and extremely brilliant; others are of a salmon red colour, and translucent only: the colour being irregularly disseminated, it is evidently accidental. Its hardness is nearly the same as that of felspar, which it scratches with difficulty, being itself powdered by the friction. Specific gravity 2·169. The crystals have generally the form of low, six-sided prisms, terminated at each extremity by six-sided pyramids, which are replaced at their summits by little hexahedral tables. Some of the crystals have transverse striæ, which were at first supposed to indicate a rhomboid as the primary form; but the plane terminations indicate a six-sided prism, which, from the direction of the natural joints, made visible by heating the crystal, seems to be its primary form: no nucleus could, however, be obtained by cleavage. This mineral, named Ledererite, in honour of M. Lederer, Austrian Ambassador to the United States, yielded by analysis,

Silica	49·47
Alumina	21·48
Lime	11·48
Soda	3·94
Phosphoric acid	3·48
Oxide of iron	·14
Water	8·58
Foreign matter	·03
Loss	1·40
	100·00

Silliman's Journal, October 1833.

EXPERIMENTS WITH POTASSIUM AND SODIUM.

Dr. Ducatel, Professor of Chemistry in the University of Maryland, after referring to Serullas's experiments on the same subject, (See Phil. Mag. and Annals, N.S. vol. vi. p. 149.) repeated them with the following results:

1. A *mucilage* made with one drachm of powdered gum arabic, and half an ounce of water, will inflame sodium, most probably for the reasons assigned by Serullas, as stated above.

2. On *wood*, sodium most generally inflames in contact with a drop of *cold* water; the action being at the same time so violent as to cause the globule of metal to roll along the *dry* surface of the table with considerable rapidity, leaving a white streak of caustic soda over its path. This experiment, however, does not always succeed.

3. On a *pane* of *glass*, sodium will not inflame, when the glass is clean and smooth; but any particles of dust adhering to it will cause the firing of the metal, with scintillations.

4. On a *metallic* surface the sodium could in no instance be made to inflame.

5. On *charcoal*, which is not mentioned by Serullas, sodium never fails to inflame, with brilliant scintillations. This is the mode which I adopt with most confidence, for firing sodium in contact with *cold* water. It confirms the truth of the reason given by Serullas, why sodium will not inflame under the same circumstances as potassium; namely, the superior temperature which the latter acquires during its combination with the oxygen of the water: hence the necessity of placing the former on a bad conductor, in order to avoid the too rapid abstraction of caloric, which prevents a sufficient elevation of temperature for manifesting the phænomenon of combustion.

6. It is commonly stated, that in the decomposition of water by *sodium*, pure hydrogen is evolved. This is a mistake. A portion of the metal, as in the case of potassium, combines with the hydrogen, as may be shown by the following experiment. Take a globule of sodium, wrap it up in a small piece of paper, and introduce it under a small receiver provided with a stop-cock and jet, filled with water, and standing over the pneumatic trough. The decomposition of the water will be effected as usual, and *sodiuretted hydrogen* will be collected, which (on opening the jet attached to the receiver) being inflamed, burns with a characteristic bright yellow flame. *Potassiuretted hydrogen*, obtained under the same circumstances, burns with a rose-coloured flame fringed with blue. The potassium, in several repetitions of this experiment, always emitted light; the sodium did not.

7. A globule of *potassium* placed on a bath of mercury gradually amalgamates with the latter, without any rotary motion, if the atmosphere be dry; but when breathed upon, it immediately acquires, as observed by Serullas, a very rapid revolving motion, which continues for a long time. The surface of the mercury becomes tarnished, apparently by the accumulation of minute particles of the amalgam formed, which at intervals are seen to emerge from beneath the surface of the mercury, and at some distance from the large globule.

The surface of the liquid metal, within a circle of half an inch to an inch in diameter, retains its brilliancy. The minute particles of amalgam, which I suppose to be the cause of the tarnish, seem to be repelled by the large globule of potassium, and occasionally, as new accessions are made to them, they become singularly agitated, exhibiting somewhat of the appearance observed when a drop of vinegar, or of an acid, comes in contact with a drop of water.

8. Small pieces of sodium projected upon a bath of mercury were not found to exhibit the phænomena indicated by Serullas; that is, they were not thrown off with explosions accompanied with light and caloric. The effects are, however, curious. The amalgamation of the sodium takes place slowly, without any rotary motion; although sometimes when breathed upon, a motion of short duration is induced. When several pieces are put upon the bath at the same time, they show no disposition to come together, but rather the contrary. But when one piece is pushed towards another, there appears to be, within a certain distance, an attractive force exerted, which is immediately succeeded by a repulsive one of some comparative energy. Many pieces being accumulated in a small space, they become violently agitated, as if alternately attracting and repelling each other, until they finally separate.—*Silliman's Journal*, October 1833.

PLATINA IN GALENA.

M. Nöel d'Argy has discovered galena from two different places containing platina, and his experiments were confirmed by M. Gaultier de Claubry. The state in which it exists has not been determined, nor are the localities of the mineral mentioned.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

We are requested by the Council to announce that the next Meeting of the British Association for the Advancement of Science will be held at Edinburgh in the week commencing Monday, September 8th, 1834.

SCIENTIFIC BOOKS.

Researches in Theoretical Geology. By H. T. De la Beche, F.R.S., &c. Foolscap 8vo. Plates and Wood-cuts.

On the Connexion of the Sciences. By Mrs. Somerville.

Illustrations of the Botany and other Branches of the Natural History of the Himalayan Mountains, and of the Flora of Cashmere. By J. Forbes Royle, Esq., F.L.S. and G.S.

A Descriptive Catalogue of rare and unedited Roman Coins, from the earliest period of the Roman Coinage, to the extinction of the Empire under Constantinus Paleologus. By J. Y. Akerman, F.S.A.

An Elementary Course of Lectures on the Criticism, Interpretation, and leading Doctrines of the Bible, delivered at Bristol College: with an Essay on the general Grammatical Principles of the Semitic Languages. By W. D. Conybeare, M.A., Rector of Sully.

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. R. HOCKING at Penzance, and Mr. VEALL at Boston.

Days of Month. 1834.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Penzance. Max.	Barometer. Penzance. Min.	Barometer. Boston $8\frac{1}{2}$ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Penzance. Max.	Thermometer. Penzance. Min.	Thermometer. Bost. $8\frac{1}{2}$ A.M.	Wind. Lond.	Wind. Penz.	Wind. Bost.	Rain. Lond.	Rain. Penz.	Rain. Bost.
☾ Feb. 1	30·189	29·900			29·72	45	29			38·5	SE.		calm	...		...
2	30·025	29·955			29·65	46	36			35	SE.		calm	...		...
3	30·034	29·859			29·62	51	37			38	S.		calm	...		...
4	29·897	29·804			29·47	50	33			40	S.		calm	...		...
5	29·875	29·763			29·50	52	32			41·5	S.		calm	0·10		...
6	30·081	30·038			29·63	50	29			39	SW.		calm	...		0·15
7	30·196	30·146			29·75	42	27			35	SW.		calm	...		...
● 8	30·252	30·174			29·75	37	25			34	NE.		calm	...		...
9	30·482	30·411			30·00	40	27			33	SE.		calm	...		...
10	30·436	30·277			29·99	47	32			32·5	S.		W.	·03		...
11	30·146	29·997			29·65	47	31			41·5	NW.		calm	·07		·06
12	29·882	29·663			29·17	47	39			45	W.		W.	·07		...
13	30·287	30·026			29·60	49	31			37	NW.		calm	...		...
14	30·350	30·322			29·88	48	34			41	W.		calm	...		...
15	30·359	30·309			29·88	44	32			43	NE.		calm	...		...
☽ 16	30·484	30·421			29·96	45	16			40	NE.		calm	...		...
17	30·335	30·269			29·83	47	39			36·5	W.		calm	...		...
18	30·184	30·034			29·66	46	45			45	W.		calm	...		...
19	30·008	29·991			29·35	51	38			48	SW.		W.	...		...
20	30·194	29·967			29·66	51	37			40	SW.		calm	·03		·05
21	30·289	30·059			29·52	50	30			41	W.		NW.	...		·04
22	30·435	30·406			29·92	52	40			42	SW.		calm	...		·05
○ 23	30·300	30·258			29·74	52	47			43	SW.		SW.	..		...
24	30·207	30·080			29·54	54	32			49	S.		W.	·01		...
25	30·517	30·466			29·92	56	28			43·5	SW.		W.	...		...
26	30·477	30·286			29·88	56	31			40	S.		calm	...		...
27	30·162	30·143			29·60	56	45			49	S.		W.	·01		...
28	30·496	30·198			29·73	59	44			43	E.		E.	·05		·10
	30·517	29·663			30·41	59	16			40·5				0·37		0·45

Remarks.

London.— February 1. Slight frost: fine. 2. Frosty and foggy. 3. Clear and fine. 4. Fine. 5. Slight rain. 6. Foggy: very fine. 7—9. Frosty, with dense fog. 10. Slight frost and fog: fine: slight rain. 11. Rain: fine. 12. Showery. 13. Slight frost: fine. 14. Fine. 15. Foggy: fine. 16. Cold haze: clear and frosty at night. 17. Sharp frost: clear and fine. 18. Hazy and cold. 19. Cloudy. 20. Fine: cloudy and windy at night. 21, 22. Very fine. 23, 24. Cloudy. 25, 26. Clear and fine. 27. Cloudy. 28. Slight rain.

Boston.—February 1. Cloudy. 2, 3. Fine. 4. Cloudy. 5. Foggy: rain P.M. 6. Foggy. 7—9. Fine. 10. Fine: rain P.M. 11. Cloudy, 12. Stormy. 13. Fine. 14, 15. Cloudy. 16. Fine. 17. Cloudy. 18. Fine. 19. Stormy: rain P.M. 20. Fine: rain P.M. 21. Fine. 22. Cloudy: rain early A.M. 23, 24. Cloudy. 25, 26. Fine. 27. Cloudy. 28. Cloudy: rain early A.M.

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[THIRD SERIES.]

MAY 1834.

LIV. *Observations on* Mr. Graham's *Law of the Diffusion of Gases.* *By* THOMAS S. THOMSON, *Esq.**

THE object of Mr. Graham's paper is to establish, with numerical exactness, the following law of the diffusion of gases: "The diffusion or spontaneous intermixture of two gases in contact, is effected by an interchange in position of indefinitely minute volumes of the gases, which volumes are not necessarily of equal magnitude, being, in the case of each gas, inversely proportional to the square root of the density of that gas." For the detail of Mr. Graham's beautiful experiments I shall refer the reader to the original paper†, contenting myself with giving a brief sketch of his method of operating, and of the results of his observations, which undoubtedly amount to a rigorous demonstration of the above law. Mr. Graham subjects to examination the diffusive powers of different gases; but as the principle of operating is the same in all cases, it will be sufficient to consider his first and most fully developed example of the diffusion of hydrogen gas into the atmosphere. The instrument used for this purpose consists of a bulb of glass, two inches in diameter, blown on a tube of $\frac{4}{10}$ths of an inch. The upper extremity of the tube, above the bulb, was closed with a stucco plug of plaster of Paris, the porous medium adopted for the exhibition of the mutual diffusion of the gases. The instrument being filled with hydrogen gas, with due precautions, is placed in a glass jar, with water in the bottom; and in proportion as the water rises in the tube

* Communicated by the Author.

† Edinb. Phil. Trans.: and Lond. and Edinb. Phil. Mag., vol. ii. p. 175.

in consequence of the great diffusive velocity of hydrogen, the external level is maintained by fresh additions of water, so as to avoid the mechanical agency of an increasing atmospheric pressure. At the conclusion of the experiment, when the hydrogen has entirely escaped, and the level become stationary, the quantity of replacing atmospheric air is carefully measured, and compared with the known volume of hydrogen originally introduced into the instrument. The fraction $\frac{\text{original vol. of hydrogen}}{\text{replacing vol. of air}}$ = the diffusion volume of hydrogen referred to that of air as unity. The mean of five experiments gave 3·848 as the diffusion volume of hydrogen, in accordance with the law announced. For the specific gravity of hydrogen being 0·0694, the square root of which is 0·2635, we have the proportion 0·2635 : 1 : : 1 : 3·7947, the diffusion volume of hydrogen, a number approaching very closely to that indicated by experiment. Carbonic acid gas, chlorine, sulphurous acid gas, protoxide of nitrogen, and other gases, operated upon in the same manner, present similar results, all tending to show that their respective diffusion volumes are in the inverse ratio of the square roots of their densities.

With a view to explain some apparent inconsistencies in the results of experiments made under varied circumstances, Mr. Graham investigates the rate at which different gases, under the influence of mechanical pressure, flow through small apertures into a vacuum. Operating as in the former case, with a plaster of Paris plug as the porous medium, different gases were allowed to penetrate through it into an exhausted receiver; the comparative velocities of entrance were inferred from the indications of a mercurial barometer gauge attached to the apparatus. The same volume of different gases under the same pressure entered in the times expressed in the following Table, beginning at a pressure of 29 inches, and terminating in a pressure of 27 inches of mercury.

Air, dry	10^m	0^s
Air, saturated with moisture at 60°...	10	0
Carbonic acid	10	0
Nitrogen	10	0
Carbonic oxide	9	30
Olefiant gas	7	50
Coal gas....................................	7	0
Hydrogen...................................	4	0

The velocity of the same gas under different pressures was found to vary with the pressure, but not in direct proportion to it. Under twofold pressure the velocity was not quite doubled. Mr. Graham concludes with remarking that the law which he has discovered is not provided for or explained by any of the existing theories of corpuscular philosophy.

The object of the following remarks is to show that Mr. Graham's facts, so far from being inconsistent with every proposed theory of the mechanical relations of mixed gases, afford an elegant and striking confirmation of the truth of Mr. Dalton's hypothesis on this subject, which is, that the particles of one gas are not elastic or repulsive in regard to the particles of another gas, but only to those of their own kind. The most obvious and remarkable feature of Mr. Graham's law is, that the mutual diffusive velocities of gases are exactly proportional to those which theory assigns for their relative velocities of escape into a vacuum.

That portion of Mr. Graham's experiments last alluded to on this point are, I admit, at variance with the acknowledged law of gaseous mechanics, which pronounces that the velocities of different gases rushing into a vacuum are inversely proportional to the square roots of their densities. The demonstration of this law is, however, so rigorous and unexceptionable, as naturally to inspire a suspicion that there is in Mr. Graham's facts either some inaccuracy of observation, or, what is more probable, some defect in the principle of the mode of operating, which has led to an erroneous conclusion. That this is really the case, is rendered still more probable by the fact that there is a certain degree of accordance between Mr. Graham's observations and the proportions assigned by the theoretical law. For instance, he finds that the velocity of hydrogen flowing into a vacuum is considerably greater than that of common air under similar circumstances, but not quite so much so as theory would indicate. But a still more suspicious circumstance affecting the accuracy of this Table is, that the density of the gases, which in the case of their mutual diffusion is, according to Mr. Graham's own law, an element of the first importance, should in the case of their escape into a vacuum have little or no agency. According to Mr. Graham, the four gases, whose specific gravities are as follow:

Nitrogen	0·972
Common air	1·000
Oxygen	1·111
Carbonic acid	1·527

all flow into a vacuum with the same velocity! So singular an anomaly unavoidably creates a distrust of the principle of operating, or of the accuracy of the observation.

Relying, therefore, on the validity of this law of gaseous mechanics, we shall proceed to examine how, in cooperation with Mr. Dalton's law, it will affect the phænomena of gases under the same pressure, mutually diffusing through a porous intermedium. For this purpose we shall consider any two gases, g, g' representing their densities by d, d'; the velocities with which each would rush into a vacuum, under the same pressures, by ε, ε'; the volumes of each which would so escape in the same times by v, v'; and the comparative weights or masses of v, v', by m, m'. By the known law, their relative velocities into a vacuum are given by the proportion

$$\varepsilon : \varepsilon' :: \sqrt{d'} : \sqrt{d} \quad \ldots\ldots\ldots\ldots\ldots \quad (1.)$$

$$\therefore \qquad \varepsilon^2 d = \varepsilon'^2 d' \quad \ldots\ldots\ldots\ldots\ldots \quad (2.)$$

Now ε, ε' evidently vary as v, v'; and as the weight or mass is as the product of the density of each by its volume, we have the equations

$$v\,d = \varepsilon\,d = m \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3.)$$

$$v'\,d' = \varepsilon'\,d' = m' \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (4.)$$

Combining equations (1.) (2.) with these, we obtain

$$\varepsilon\, m = \varepsilon'\, m'.$$

Hence the mass multiplied into the velocity, of each issuing stream is the same for both gases, whatever be their respective densities, or, in other words, *the moving force of each issuing current is the same*; a law most remarkable for its simplicity and importance, and one which is not noticed in any of the treatises on gaseous mechanics that I have met with.

Instead of the gases issuing into a vacuum, let us now suppose that they are permitted to diffuse through each other by a small aperture, or system of apertures, such as is presented by a plug of stucco, or any other porous substance. Knowing so little as we do of the ultimate corpuscular constitution of gases, we cannot determine the precise manner in which the opposing currents will act upon each other; whether by percussion, by friction, or in what other possible mode of mechanical action. But one thing we may safely predict, viz. that a partial obstruction will take place, a retardation of the velocity of each gas will ensue; and since, from the equality of action and reaction, the quantity of motion lost on each side is the same, the resulting momenta of the currents will necessarily be equal, and consequently, by the converse of the equations (1.), (2.), (3.), (4.), the resulting velocities will be inversely proportional to the square roots of the densities. Hence it appears, if the data be correct, that the *initial velocities* of diffusion ought to be exactly in the proportion that Mr. Graham has determined by experiment.

It remains only to be shown that during the completion of the experiment the same equality of moving force exists, and that consequently the final volumes exchanged ought to be, as Mr. Graham finds them, proportional to the initial velocities. For this purpose we shall refer to the case before mentioned, of hydrogen escaping from the diffusion instrument into the atmosphere. At any period of the experiment, after diffusion has commenced, let us call h the quantity of hydrogen remaining in the instrument, and a the quantity of return air which has entered. The gaseous mixture being maintained at atmospheric pressure, by keeping the level of the water outside the instrument the same as that of the inside, its volume will be $a+h$, and calling atmospheric pressure unity, the tension of the hydrogen in the bulb will be $\frac{h}{a+h}$, and that of the air in the bulb will be equal to $\frac{a}{a+h}$. The diffusive tension or elasticity of the hydrogen is consequently proportional to $\frac{h}{a+h}$, and the impelling force of the atmosphere, which is the excess of its external pressure over the internal tension of the air, is proportional to $\frac{1-a}{a+h} = \frac{h}{a+h}$. Hence, since a and h are indeterminate, it follows that at every intermediate period between the commencement and the conclusion of the phænomenon, the impelling force of the air will be equal to the expelling force of the hydrogen; the quantity of motion lost on each side will be the same; the resulting momenta of the two currents will be equal; and, by reasoning similar to that already used, it follows that the *final volumes exchanged will necessarily be inversely proportional to the square roots of the densities.* This progressive decrease of elasticity or diffusive tension likewise explains why the rapidity of diffusion is so great at the commencement of the experiment, and gradually diminishes towards its conclusion.

Mr. Graham's observations, in short, extend to gases in motion; Mr. Dalton's theory, to their relations when in a state of equilibrium. One is the statical, the other the dynamical exhibition of the same law.

It is not easy to assign a reason why the results of Mr. Graham's experiments on the velocities of gases flowing into a vacuum should differ so considerably from those indicated by theory. Unfortunately, in the few experimental investigations hitherto made on this subject, sufficient attention has not been paid to the difference between a gas under pressure escaping into an atmosphere of another gas, and escaping into one of its own kind. For this reason, Leslie's experiment cited in the notes of his Treatise on Heat, and Mr. Faraday's on the Escape of Gases through Capillary Tubes, (Quarterly Journal of Science, vol. iii.) present results differing from each other, from Mr. Graham's, and from the deductions of theory. It is, I think, not improbable that when gases are subjected to pass rapidly through a porous medium, as is the case when they escape into a vacuum, instead of being retarded by the opposing atmosphere of another gas, the angular irregularities of the channels of communication may present greater obstruction to those gases which are disposed to move with great velocity, than to those whose motion is not so rapid. When, on the other hand, the gases mutually diffuse into and retard each other, it becomes a question of time and not of velocity; and the supposed inequality of obstruction may greatly diminish if not entirely disappear.

This supposition has at least the advantage of reconciling, in some degree, Mr. Graham's experiments with the theoretical deductions. But it is to be hoped that future researches may throw further light on this interesting question.

Primrose, near Clitheroe, April 12, 1834.

LV. *On Cask-Gaging.* *By* J. W. Lubbock, *Esq., V.P. and Treas. R.S.**

KEPLER was the first who endeavoured to reduce the art of gaging to accurate principles, in his work entitled, *Nova Stereometria Doliorum Vinariorum*, published in 1615. He gave in this work the solution of several new problems relative to the content of various solids, and he showed how the solution of others might be made to rest upon considerations more simple than those which had previously been employed. The art of gaging is one of such practical importance in all countries, but more especially in our own, in consequence of the immense duties annually levied by the Government upon various liquids, the quantity of which is ascertained by gaging separately the casks in which they are contained †, that I trust the following remarks will not be considered superfluous, although it is not in my power at the present time to do more than to show how the elementary principles of the mensuration of solids should be applied to the

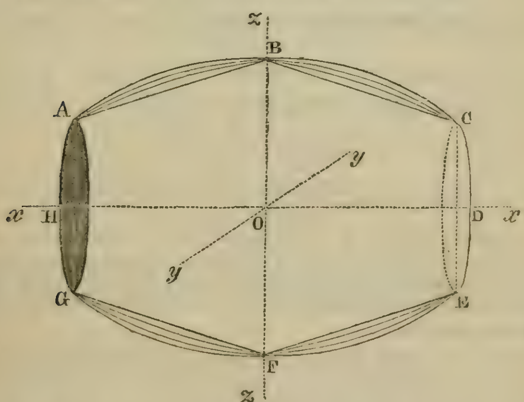
* Communicated by the Author.

† The number of casks gaged annually on the quays in this port alone, before they are deposited in store, amounts to more than 100,000. The duty on brandy is 22*s.* 6*d.* per gallon.

subject in question. I shall limit myself, in the following brief remarks, to the gaging of casks lying, not entering upon the question of the gaging of ships, or stills, or any other irregular figures, merely premising that this can only be done by quadratures, or, as it is termed in works on gaging, by the method of equidistant coordinates. A very material difference, however, exists between these problems and the one which I propose here to notice, and which consists in this circumstance, that in the one case extreme dispatch is a necessary requisite in the method employed; in the other, that is to say, in ascertaining the tonnage of ships, or the content of any fixed reservoirs, such as distillers' vats, &c., time not is so great an object; so that the peculiar difficulty of cask-gaging arises from the necessity of combining accuracy with celerity in the operation.

It has long been customary to divide casks into four varieties, which are thus defined:

1. The middle frustum of a spheroid.
2. The middle frustum of a parabolic spindle.
3. The frustums of two equal parabolic conoids.
4. The frustums of two equal cones.



I find these distinctions in a work, entitled "*Cosmographia*," by Dr. John Newton, published in 1679, and they appear to have continued ever since.

I place the origin of rectangular coordinates at the centre of the cask, and I suppose the cask to be a figure of revolution about the horizontal axis Ox, as in the diagram annexed. According to the preceding definitions, in the first variety the arc ABC is considered to be a portion of an ellipse of which BO is the semiaxis minor, the axis major coinciding in direction with Ox.

In the second variety the arc ABC is considered to be a portion of a parabola whose vertex is B, and whose axis coincides in direction with BO.

In the third variety the arc BC is considered to be a portion of a parabola whose vertex is in the line Ox beyond D.

In the fourth variety BC is a straight line.

In works on gaging, the second and third varieties are supposed to be intermediate between the first and fourth, as in the figure, which is copied from Symons's Practical Gager, p. 187; but this is incorrect, for the second variety, *as thus defined*, coincides very nearly with the first, and the third with the fourth.

Let BF (the internal bung diameter) be called b.
CE (the internal head diameter) be called h.
HD (the internal length) be called l.

In the first variety the equation to the curves ABC, GFE, in the plane xz, is

$$z^2 = 4\,b^2 - \frac{b^2-h^2}{l^2}\,x^2.$$

In the second variety the equation to the curve ABC is

$$z = 2b - \frac{b-h}{2\,l^2}\,x^2.$$

In the third variety the equation to the curves BC, FE, is

$$z^2 = 4\,b^2 - \frac{2(b^2-h^2)}{l}\,x.$$

In the fourth variety the equation to the curve BC is

$$z = 2\,b - \frac{b-h}{l}\,x.$$

In the first variety the content of the cask is easily found by means of the triple integral $\int\int\int dx,\, dy,\, dz$ taken between the proper limits,

$$= \frac{\pi l}{12}\left\{2\,b^2 + h^2\right\} \qquad (\pi = 3{\cdot}14159).$$

In the second variety the content

$$= \frac{\pi l}{20}\left\{\frac{8}{3}\,b^2 + \frac{4}{3}\,bh + h^2\right\}.$$

In the third variety,

$$= \frac{\pi}{8}\,l\left\{b^2 + h^2\right\}.$$

In the fourth variety,

$$= \frac{\pi}{12}\,l\left\{b^2 + bh + h^2\right\}.$$

These are the rigorous expressions for the content of the cask, considered of either variety, and rules founded upon them are given in Symons's Practical Gager, p. 193.

The preceding expressions give the result in cubic inches, cubic feet, &c., accordingly as the linear dimensions of the cask b, h, l are expressed in one or the other denomination. The Imperial gallon contains 277·273 cubic inches of water at 62°; and *if we consider a gallon of any other fluid to have the same volume at the same temperature*, and not as a measure of weight, when the dimensions of the cask, as is usual, are expressed in inches, the expression for the content must be divided by 277·273, in order to have the content in Imperial gallons.

The readiest mode of obtaining *accurately* the content of the cask, considered either of the first or of the third variety, appears to me to be by employing the Tables* given by Symons, p. 210 and following, which consist, the first of values of $\frac{\pi h^2}{12 \times 277{\cdot}273}$ and the second of $\frac{\pi b^2}{6 \times 277{\cdot}273}$ for different values of h and b. The second and fourth varieties may be rejected altogether, as coinciding with the first and third. Thus: required the content of a cask of the first variety when $b = 31{\cdot}2$, $h = 26{\cdot}1$, and $l = 46{\cdot}9$. I find by the Tables

$$\frac{\pi h^2}{12 \times 277{\cdot}273} = {\cdot}6432$$

$$\frac{\pi b^2}{6 \times 277{\cdot}273} = 1{\cdot}8382$$

$$\text{sum} \qquad = 2{\cdot}4814$$

$2{\cdot}4814 \times 46{\cdot}9 = 116{\cdot}37$ = content of cask in Imperial gallons *considered as of the first variety.*

The content of the third variety may be found in the same way.

$$\left.\begin{array}{l} \dfrac{\pi h^2}{12 \times 277{\cdot}273} = {\cdot}6432 \\[2ex] \dfrac{\pi b^2}{12 \times 277{\cdot}273} = {\cdot}9191 \end{array}\right\} \text{By the Table entitled "\textit{Areas of the Head Diameter.}"}$$

$$\text{sum} \quad = 1{\cdot}5623$$

$$\frac{3l}{2} = 70{\cdot}35$$

$70{\cdot}35 \times 1{\cdot}5623 = 109{\cdot}91$ = content of the cask in Imperial gallons *considered as of the third variety.* The extreme difference in the content, due to the variety, obtained by con-

* These tables are *headed* inaccurately.

sidering the cask, with the same length, bung and head diameter, is thus in this instance 6·46 gallons, or about 5·5 per cent.

The term *ullage* appears to be used indifferently to denote either the liquor contained in a cask, or the vacuity above the liquor. It has been seen that accurate, that is to say, *rigorous* and extremely simple expressions can be obtained for the entire content of a cask of either variety; but it seems to me impossible to find expressions sufficiently simple to be used in practice for the ullage of a cask *lying upon its bulge*, except in the case of the first variety, and when the *dry inches* do not exceed the half difference of the bung and head diameters. This difficulty arises from the limits of the integral which has to be employed.

The cask considered as of the first variety is a portion of the spheroid, of which the equation is

$$l^2 (z^2 + y^2) + (b^2 - h^2)\, x^2 = 4\, b^2\, l^2.$$

If this spheroid be intersected by a horizontal plane, the curve of intersection will be an ellipse, of which the semiaxes are

$$\frac{l \sqrt{4 b^2 - z^2}}{\sqrt{b^2 - h^2}} \text{ and } \sqrt{4 b^2 - z^2}:$$

the area of this ellipse $= \dfrac{\pi l (4 b^2 - z^2)}{\sqrt{b^2 - h^2}}$.

This quantity multiplied by $d\,z$ and integrated between the proper limits will give the ullage in the first variety; and I find that if d represent the *dry inches*, or the distance from the bung-hole at B to the surface of the liquor,

the ullage or vacuity $= \dfrac{\pi\, l\, d^2}{3 \sqrt{b^2 - h^2}} \left(\dfrac{3}{2} b - d \right)$,

when the dry inches do not exceed the half-difference of the bung and head diameters, that is, when the surface of the liquor is not beneath a straight line joining A and C. I understand that in practice this condition generally obtains, except where a cask has leaked, or where the liquor has made a long voyage, as in the case of *rums*. This formula might be useful if casks could be considered as belonging to this variety, but of course the difference of figure has a sensible effect upon the ullage.

In the other varieties, and in the first variety except in the particular case I have mentioned, the integral which has to be taken in order to obtain the ullage is extremely complicated; and I apprehend the simplest way to arrive at the ullage in all these cases is to construct Tables for various casks by quadratures, that is, to divide the liquor into parallel and thin slices, vertical and perpendicular to the axis of revolution, each of which may be considered as a small cylinder of which the base is a circular segment.

The rule which is given by Symons, p. 217, for finding the ullage of a cask *lying*, by the pen, is erroneous, and I apprehend of no utility.

In practice, the sliding-rule (or *head-rod*) is invariably used in gaging casks, and the content of the cask, considered generally of the first variety, is sought by adding to the head diameter a certain quantity, depending only on the difference of the bung and head diameter, and considering the sum so obtained as a mean diameter, and the content of the cask to be the same as that of a cylinder of the same altitude or length, with that quantity as the diameter of the base.

In the first variety,

$$\text{content} = \frac{\pi l}{12}(2b^2+h^2),$$

$$\text{if } 2b^2+h^2 = 3(h+q)^2$$

$$h+q = \sqrt{\frac{2b^2+h^2}{3}}; \quad \text{and since } b = h+b-h$$

$$= \sqrt{h^2+\frac{4h}{3}(b-h)+\frac{2(b-h)^2}{3}}$$

$$= h+\tfrac{2}{3}(b-h)+\frac{(b-h)^2}{9h}+\&c.$$

$q = \frac{2}{3}(b-h)$ nearly. The error of the content obtained by this approximate method $= -\frac{\pi l}{18}\{b-h\}^2$.

This, however, is only to be considered as a rough approximation, and the quantity q cannot be made strictly to depend only upon $b-h$. If we take a cask of which the dimensions are $l = 46{\cdot}9$, $b = 31{\cdot}2$, $h = 26{\cdot}1$ (which are those of an average brandy piece,) the content of which is accurately 116·37 gallons, we find $q = \frac{2}{3}(5{\cdot}1) = 3{\cdot}4$, $h+q = 29{\cdot}5$, and the content is found by the approximate method above equal to 115·61 gallons. It is obvious that the error will be nearly constant for *the same kind of cask*, and hence either a correction may be obtained experimentally or by calculation to be added in all cases for each kind of cask, or this correction may be included in that due to the variety. The direction given by Symons is, "Look for the difference of the bung and head diameters on the line of inches on the edge of the rule, and whatsoever number stands opposite to it on the respective lines of varieties, add to the head diameter, and the respective sums will be mean diameters, or diameters of cylinders equal to the solidities of the cask when the altitudes of those cylinders and the lengths of the casks correspond." When this was written the four varieties were introduced on the sliding-rule; now only the *spheroid* is placed there, and I find opposite 5·1, 3·5 instead of 3·4. How this difference arises is difficult to ascertain: the line has most likely been laid down for a particular value of h.

The value of q, for the third variety, is found in the same way to be approximately $\frac{1}{2}(b-h)$, and the error of the content would be $= -\frac{\pi l}{16}\{b-h\}^2$.

The sliding-rule or head-rod used in gaging affords the means of multiplying the square of the *mean diameter* by

$$\frac{\pi l}{4 \times 255{\cdot}273}.$$

For gaging *by the pen*, it would be convenient to have a table of the logarithms of the numbers in Mr. Symons's Table, entitled "Areas of the Head Diameters," instead of the numbers themselves. With these, and a table of the logarithms of numbers from 1 to 1000, to four decimal places (both of which might be contained on a single quarto page,) the content of casks might probably be calculated as readily as by means of the sliding-rule. It must be recollected that the principle upon which the line on the head-rod gives the quantity q to be added to the head-diameter is only a rough approximation, which may be out nearly half a gallon, and which stands in need of a subsequent correction.

The method of determining the ullage of a cask by the line on the head-rod marked S L y (Symons, p. 214,) is not strictly accurate *in principle*; but how far the error which is so introduced is sensible in practice, depends upon how far it is desirable that the approximation should be carried, and how far the principle itself is modified afterwards by any subsequent correction. The ullage cannot be considered strictly as dependent on (or as a function of) the *dry inches* and the content of the cask as it is by the method alluded to.

In practice an error in determining the content of a cask may arise either from the errors above noticed, which are introduced by calculating the content approximately by the sliding-rule, having given the dimensions and the *variety*, or from an error in ascertaining the dimensions, or from an erroneous judgement in deciding upon the *variety* to which the cask is to be referred.

An error of a tenth of an inch in all the linear dimensions of a cask will make a difference in a cask holding 116 gallons, of about a gallon, which is, perhaps, the maximum of this error. It must be recollected also, that the instruments used in ascertaining the external dimensions are of a nature to stand in need of occasional adjustment, and that the difficulty of ascertaining the *internal* dimensions is considerable, from the irregularity of the thickness of the staves. The rules and callipers used in gaging are liable to slight but not insensible variations from heat and moisture, and should therefore be constantly compared with standard metallic scales provided for the purpose and kept in protected situations.

The extreme limits of the error due to the determination of the variety may of course be ascertained by calculating the content upon either extreme hypothesis. I find that the content of the cask for which $l = 46{\cdot}9$, $b = 31{\cdot}2$, $h = 26{\cdot}1$,

by the formula $\frac{\pi}{12} l (2 b^2 + h^2)$ is 116·37 gallons,

———————— $\frac{\pi}{8} l (b^2 + h^2)$ is 109·91 gallons,

showing 6·46 gallons as the difference between the content obtained, according to one or the other hypothesis. This error may be greatly diminished by ascertaining *experimentally* the correction required for any particular cask, when one or the other hypothesis has been used, which proceeding is facilitated by the circumstance that casks coming from the same country generally maintain a separate and peculiar character, so that a particular correction might be easily deduced for a sherry butt, a port pipe, &c., without any reference to mathematical considerations. This would amount to the same as to deduct a certain quantity from the length of the cask, calculating the content as though it were accurately of the first variety. So if the brandy piece be, in fact, of the variety which I call the fourth, namely, that of which the content is given by the expression

$$\frac{\pi}{8} l (b^2 + h^2),$$

it is sufficient to take $l = 44{\cdot}293$, that is, to deduct 2·607 from the length, and calculate the content by the expression

$$\frac{\pi}{12} l (2 b^2 + h^2),$$

which gives the same result.

It is in determining the *variety* of a cask, or the allowance to be deducted from the content previously obtained by considering the cask as of the first variety, that the skill and experience of the gager are principally required. Difference of opinion appears to exist as to the limits of the error which obtains in practice when a single cask is gaged. Mr. Archer, a gentleman particularly qualified by long experience to form an accurate opinion upon this point, has assured me that it generally amounts to 2 per cent., but that by extreme care it may be reduced to a gallon in a single cask, or to 1 per cent. nearly; on the other hand Mr. Nairn, the principal gager of the St. Katherine Docks, in a communication with which he has favoured me, considers the usual error at no more than the eighth of a gallon.

Were the practice of ascertaining the content by weighing introduced, of course all the sources of error which I have described would be got rid of, but it would be difficult to ascertain the *tare*, which might vary, probably, between 5 and 8 per cent.

In gaging wines it is not usual to make any allowance for temperature. The following Table, which was obtained by means of experiments made under my direction by Mr. Ladd, a workman in the employ of Mr. Bate, shows the specific gravity of each liquid, that of the liquid at 62° Fahrenheit being unity.

	Brandy.	Sherry.	Port.	
100	·9813	·9900	·9913	100
95	·9838	·9913	·9920	95
90	·9863	·9928	·9933	90
85	·9888	·9943	·9966	85
80	·9912	·9957	·9958	80
75	·9939	·9970	·9972	75
70	·9965	·9982	·9984	70
65	·9986	·9993	·9994	65
62	1·0000	1·0000	1·0000	62
60	1·0009	1·0005	1·0003	60
55	1·0034	1·0017	1·0012	55
50	1·0058	1·0028	1·0022	50
45	1·0078	1·0037	1·0031	45
40	1·0102	1·0046	1·0041	40

This Table gives the means of ascertaining the correction due to temperature, and which in extreme cases ought not to be neglected.

As regards the practice of gaging, I will venture to observe, that the methods employed by the officers of the Customs in ascertaining the content of casks should be definite, and not left, as at present, entirely to the arbitrary opinion of different individuals. It is usual for the Dock-gagers to gage the casks also, and their measures serve for the benefit of the merchant to check the measures or gages obtained by the officers of the Customs; but it is impossible for this check to operate effectually, unless the officers and the Dock-gagers proceed independently, and both upon recognised principles. It is difficult now, even to arrive at the method by which the usual allowances are made; nor are they, I believe, sufficiently described in any printed work. This arises, perhaps, from an unwillingness to make public the methods employed, for fear casks should, in consequence, be made purposely to defraud the revenue, called technically *game-casks.*

It seems to me, nevertheless, that uniform instructions, as far as they can be generally applicable, and leaving as little as possible to the judgement, that is, to the guess of the gager, should be carefully drawn up and published for general use, so as to be open to the examination of any one who may question their accuracy. Moreover, if the use of the sliding-rule be permitted, whether in the determination of the content or of the ullage of casks, it should be considered merely as a readier method of *working*; and it ought not to supersede the construction of accurate tables, approved by the Customs and Excise, of which the rule itself would contain the representation, and which might therefore be verified by any one without difficulty. Perhaps the best method would be to calculate the content as in p. 331, employing a mean diameter $= h + \frac{2}{3}(b-h)$, by means of a table of the logarithms of $\frac{\pi h^2}{4 \times 255{\cdot}273}$, or by the lines C and D of the sliding-rule, (See Symons, p. 192,) having previously determined experimentally for various kinds of cask a correction to be applied, in order to improve the content so obtained. Accurate tables should also be formed of the ullages of various casks, and in cases of difficulty the ullage might be gaged, the cask *standing*, which is obviously a much simpler problem.

At present it is not even known how some of the lines on the sliding-rule were originally laid down, or, I believe, for what cask they are intended. Lately an alteration was made in the line marked S L y, by which an obvious inaccuracy was removed; but this alteration, by which the ullage (vacuity) is diminished, operates disadvantageously to the merchant who pays duty on the liquor. In consequence, the *Wine and Spirit Committee* requested me to offer them an opinion as to the propriety of the change and the accuracy of the method used in determining the ullage. It is obviously impossible to give a complete and satisfactory reply to the latter question, except by means of direct experiments, while we are ignorant what function of the dimensions of the cask the line S L y is intended to represent, and without tables of the ullages of various casks with which to compare it.

No doubt when a general system of public instruction is introduced, with a greater tendency than exists at present to the cultivation of drawing, and of those arts which are most generally useful, the elements of the art of gaging will not be altogether overlooked.

LVI. *Notice of the Arrival of Twenty-six of the Summer Birds of Passage in the Neighbourhood of Carlisle, during the Spring of* 1833, *together with Notices of some of the scarcer Species that have been obtained in the same Vicinity from the* 10*th of November* 1832, *to the* 10*th of November* 1833; *with Observations, &c. By* A CORRESPONDENT.*

No.	English Specific Names.	Latin Generic and Specific Names.	When first observed.		No.
1	Quail	Coturnix vulgaris	May	10	6
2	Swallow	Hirundo rustica	April	7	35
3	House Martin	——— urbica	——	24	36
4	Sand Martin	——— riparia	——	2	36
5	Swift	Cypselus Apus	May	4	37
6	Goatsucker	Caprimulgus europæus	——	8	38
7	Pied Flycatcher	Muscicapa Atricapilla	April	25	41
8	Spotted Flycatcher	——— Grisola	May	9	42
9	Ring Ouzel	Turdus torquatus	April	5	49
10	Wheatear	Saxicola Œnanthe	——	7	53
11	Whinchat	——— Rubetra	——	26	54
12	Redstart	Sylvia Phœnicurus	——	8	57
13	Grasshopper Warbler	Curruca Locustella	——	17	58
14	Sedge Bird	——— salicaria	May	1	59
15	Greater Pettychaps	——— hortensis	——	6	62
16	Wood Wren	——— Sibilatrix	——	3	63
17	Blackcap	——— Atricapilla	April	18	64
18	Whitethroat	——— Sylvia	May	4	66
19	Yellow Wren	Regulus Trochilus	April	5	70
20	Yellow Wagtail	Motacilla flava	——	19	75
21	Field Lark, or Titling	Anthus trivialis	——	21	78
22	Cuckoo	Cuculus canorus	——	23	121
23	Wryneck	Yunx Torquilla	——	25	125
24	Corncrake, or Land-Rail	Ortygometra Crex	——	20	129
25	Dottrel	Charadrius Morinellus	May	2	164
26	Common Tern	Sterna Hirundo	——	10	235

[*Obs.*—The figures contained in the column on the right in the above Table, as well as those affixed to the species not included in it, refer to the numbers in Fleming's History of British Animals, which we have inserted, in order that the reader who wishes to have a description or to see the various synonyms of any of the birds here alluded to may find the species at once, should he possess that very excellent work.]

9. *Turtle Dove* (*Columba Turtur*).—A young male of this species was killed on Rockcliff Moss on the 14th of September. Upon referring to our last communication it will be seen that a similar specimen was obtained in this vicinity in the same month in 1832; we are consequently inclined to think that the Turtle Dove occasionally breeds in Cumberland. It

* Communicated by the Author.

is, however, as we have before remarked, very rarely met with in this district*.

39. *Greater Butcher Bird*, or *Cinereous Shrike* (*Lanius Excubitor*).—During the month of October two or three Greater Butcher Birds were repeatedly seen in the immediate vicinity of Carlisle, one of which was procured on the 29th, within a very short distance of the suburbs. Upon dissection it proved to be a male, and had all the appearance of being an old bird, notwithstanding the breast and under parts were marked with numerous fine, dusky, curved lines, which are stated by almost all authors to be peculiar to the female.

The stomach of this specimen was completely distended with coleopterous insects: amongst others we were able to recognise with certainty specimens of *Helobia brevicollis* (85), *Agonum parumpunctatum* (125), *Pœcilus cupreus* (181), &c. &c.†

49. *Ring Ouzel*, or *Ring Thrush* (*Turdus torquatus*).—One of these birds was seen and obtained within a very short distance of Dyke's Field on the 5th of April, in all probability on its passage to the Scotch hills. This is the first instance we are aware of that the Ring Ouzel has been met with in the low grounds in this vicinity.

80. *Wood Lark* (*Alauda arborea*).—A male of this species was killed within a short distance of Carlisle on the 4th of February, which is the only specimen we have been able to procure at large in this neighbourhood, where it is of very rare occurrence. We have, however, seen several that had been taken alive by bird-catchers in the vicinity of Dumfries, in Scotland, during the winter months.

98. *Common Grosbeak*, or *Hawfinch* (*Coccothraustes vulgaris*).—From the latter end of January to the middle of March, a Grosbeak was repeatedly seen in the garden and pleasure-grounds of the Misses Losh, at Woodside, about four miles south of Carlisle. It apparently subsisted chiefly upon the berries of the Whitethorn (*Cratægus Oxyacantha*), being very frequently observed upon an aged thorn, then extremely full of fruit. We have every reason to believe that this is the first instance of the Grosbeak having been observed so far north; and notwithstanding many attempts were made to secure it, it eventually escaped.

185. *Black-tailed Godwit* (*Limosa ægocephala*).—A young female Black-tailed Godwit, a species very rarely met with in the North of England, was killed on Brugh Marsh on the 12th

* Lond. and Edinb. Phil. Mag., Third Series, vol. ii. p. 97.

† The figures attached to the names of these insects refer to the numbers in Stephens's Systematic Catalogue of British Insects.

of September, within a very short distance of the locality where a young male was obtained in August 1832, with which it agreed in almost every respect: two others were seen, in all probability part of the same brood*.

152. *Pygmy Curlew* (*Tringa subarquata*). — A very beautiful male, in nearly complete summer plumage, was met with on Rockcliff Salt Marsh, on the 27th of May, which is the first specimen of the Pygmy Curlew that has been obtained in this part of the county, to our knowledge. It was still in the moult, and excessively fat. The stomach contained the fragments of Shrimps (*Crangon vulgaris*), Sandhoppers (*Talitrus Locusta*), &c., and had a very strong marine scent.

155. *Little*, or *Double Fork-tailed Sandpiper* (*Tringa minuta*). —Two young females of the year, of this diminutive species of Sandpiper were procured on the coast, at no great distance from Brow-Houses on the 24th of August, and are the first young birds we have hitherto seen. The reader, upon referring to our last communication, will find the particulars of the capture of two adults, &c.†.

159. *Turnstone* (*Strepsilas Interpres*).—On the 24th of August, five young Turnstones were taken alive in a rather singular situation, namely, in the trap of a stake-net on the coast. We have been very credibly informed that the Cuckoo (*Cuculus canorus*), Curlew (*Numenius arquata*), and several of the Gulls have been found in similar traps.

162. *Common Sanderling* (*Calidris arenaria*).—So late as the 4th of June, several Sanderlings were killed on the coast, in the vicinity of Brow-Houses, in full summer livery. The few that came under our inspection were so extremely fat, that upon some parts of the body it was nearly one quarter of an inch in thickness. The stomachs of the two or three which we had an opportunity of examining contained the remains of Shrimps (*Crangon vulgaris*), Sandhoppers (*Talitrus Locusta*), &c., and had the same marine scent as noticed in that of the Pygmy Curlew. The irides of these birds were all of a clear dark brown.

186. *Common Shoveler* (*Spathulea clypeata*). — A very fine female Shoveler was shot on Thurston-field Lough on the 6th of November, a bird of rather rare occurrence in this district. The gullet and stomach contained an immense number of freshwater shells, recently swallowed: amongst others we detected specimens of the glutinous mud shell (*Limneus glutinosus*, Drap.), and the crested valve shell (*Valvata Spirorbis* Drap.).

* Lond. and Edinb. Phil. Mag., Third Series, vol. ii. p. 99. † *Ibid*. p. 100.

Page 128. *Canada Goose* (*Anser Canadensis*).—On the 24th of June ten or eleven Canada Geese were observed in the River Caldew, near the village of Dalston, one of which was procured, and is now in the collection of a gentleman residing within a short distance of the above-named village.

211. *Northern Diver* (*Colymbus glacialis*).—A remarkably fine old female Northern Diver was taken alive, by a boy, on the sands near the village of Bowness. The intestines of this bird were fully seventy-two inches in length, and the irides were of a very fine crimson red.

221. *Common Skua* (*Cataractes vulgaris*).—A fine old female Skua Gull was brought to us alive on the 27th of April, which had been captured on the preceding day, on the coast, in the following very extraordinary manner. A fisherman, who had been examining his nets on the above-mentioned day, observed, at some distance, two large birds struggling together, and upon arriving at the scene of combat he found this bird upon the point of killing a Herring Gull (*Larus argentatus*), and so determined was the Skua to dispatch its prey, that the fisherman secured it without the least difficulty. The stomach was quite empty, and the eggs in the ovary were still very small. We cannot find that the Skua has been captured in this vicinity before.

231. *The Lesser Brown-headed Gull* (*Larus capistratus*).—An immature female of this rare Gull was accidentally procured near Sandsfield, on the 6th of June. It was found associating with several of the Black-headed species (*Larus ridibundus*), but remained after all its congeners had taken wing, was shot at, and fortunately killed. From an inspection of this bird, we are satisfied that this species may be very easily overlooked, and that many ornithologists would consider it merely a small specimen of the Black-headed Gull. We have deemed it advisable, therefore, to give its weight and dimensions.

Weight....................	$8\frac{1}{4}$ ounces.
Length	$14\frac{1}{2}$ inches.
Extent of the wings......	36 ——
Bill to the front	$1\frac{1}{10}$ ——
Bill to the gape, or rictus	$1\frac{8}{10}$ ——
Tarsi	$1\frac{6}{10}$ ——
Middle toe and claw.....	$1\frac{5}{10}$ ——

A few Meteorological Remarks on the Spring and Summer of 1833, *at Carlisle.*

During the afternoon of the 29th of March there was a remarkably heavy shower of snow, which continued without any interruption for about an hour, and the flakes of snow which then fell were the largest we ever saw, many being nearly an inch in diameter. All the surrounding country was consequently more or less covered with it, and upon the hills the snow was of a very considerable depth in many parts.

With the exception of a few fine days from the 22nd to the 25th, the month of April was exceedingly cold and chilly; smart hail-showers occurred nearly on every alternate day, and it was not until the 4th of May that the weather became fine and seasonable. The summer that followed was upon the whole fine, and the autumn one of the driest almost ever recollected in this neighbourhood, so much so, that the navigation of the canal in this vicinity is now, and has been for some time past, considerably impeded for want of water.

Carlisle, Nov. 10, 1833.

LVII. *On the Cause of the Appearance commonly termed Heat Lightning, and on certain correlative Phænomena.* By J. Hancock, *M.D.**

I BEG leave to notice an occurrence which is every year more or less observable; and although not peculiar to this country (Demerara), it appears to merit special attention as conducive, perhaps, not merely to speculative, but to practical results. I allude to those luminous flashes or corruscations of electric light which are so remarkable on the coast of Guiana, near the end of the dry season, (or before the commencement of great rains, and not unfrequently in a sky free from clouds,) darting from below the horizon either in solitary flashes, or at times occupying an arc of it, from south-east to west, almost in an incessant tremulous blaze, and rising up the vault even to emulate the aurora borealis, as the latter appears in winter, in North America, and other higher latitudes. From the coast, these meteoric or electric flashes are seen generally in a southerly direction, corresponding with that of the Macosie mountains, or the great chain of Parime (about 200 miles distant), and are most common in the months of June and July.

Similar flashes are seen also in the Orinoko near the entrance of the rainy season, which there begins in March or April, proceeding, as it were, from the vast groups of granitic mountains to the south-west of Angostura, whilst they are rarely observed in the direction of the plains on the north, as

* Communicated by the Author: a paper by whom, containing the principal points here discussed, was read in 1826, at a Meeting of the Philosophical Society of British Guiana, by the Rev. Stephen Isaacson, M.A., Secretary.

those of Barcelona, Chaymas, and Calaboso. Soon after this the rivers begin to swell, and the rains commence in the Lower Orinoko, where, conversing upon this subject, I was told by one, it was *relampago sin trueño*; by another, *una fantasma*; a learned friar very gravely pronounced it to be *la madre de trueño*. I inquired of several, whether it was not attended with thunder? O, never; it was *relampago, no mas*, 'lightning only.' Why so? had they ever been involved in it, or seen it close to them? Never; it was *always* seen *far off*, '*muy lexos*'. This answer almost confirmed me in the converse opinion to theirs, and I have since fully satisfied myself on that head. These appearances occur more or less every year, but were especially remarkable in Essequibo, at the end of May and beginning of June 1819, and were followed soon after by great thunder-storms on the coast; and I have ever since regarded these corruscations, not merely as the precursors, but as announcing the actual commencement of great rains and thunder-storms in the interior, such in fact as I had experienced in travelling there at the same season in 1811.

The same phænomenon is, in North America, called heat lightning, as I believe it is in Europe—*feux d'horizon* of the French. Without considering the great distance at which lightning is perceived and the small distance that thunder is heard, it is believed that such flashes are not accompanied by thunder or any explosion. As it occurs chiefly in hot weather, it is called *heat lightning*, *fires of the horizon*, and commonly regarded as some anomalous meteors, or electrical emanations unattended by thunder. At the same time, it is not merely a vulgar fancy, but one which has been entertained by the learned of all ages, and I am surprised to find it subscribed to by such distinguished travellers as Humboldt and Volney.

"At Arragua," says M. Humboldt, "in the beginning of the month of March, the accumulation of the vesicular vapours, visible to the eye, and with them, signs of atmospheric electricity augmented daily. We saw flashes of *heat lightning* to the south: and the electrometer of Volta displayed constantly at sun-set vitreous electricity."—*Pers. Narr.* vol. iv. p. 402. "On the morning of April 15th (Upper Orinoco) the sky was in a great part obscured, and lightnings furrowed thick clouds at more than 40° of elevation. We were surprised at not hearing the sound of thunder. Was it on account of the prodigious height of the storm? It appears to us, that in Europe *the electric flashes without thunder*, vaguely called *heat lightning*, are seen generally nearer the horizon."—*Pers. Narr.* vol. v. p. 8.

M. Volney, speaking of the combination and disunion of water and caloric, observes, "Hence the violent showers which follow loud claps of thunder, and which happen, generally, at the end of storms, the igneous matter being then expended. Sometimes the particles of fire being combined with the air only, it melts like nitre; and this it is, doubtless, which produces those lightnings when no thunder is heard, called fires of the horizon (*feux d'horizon*). But is this igneous matter distinct from the electric? Does it observe peculiar laws and affinities in its combinations and detonations? This I shall not take upon me to examine. These researches are not suited to a narrative of travels," &c.—*Travels in Syria*, vol. i. p. 354. Such were the sentiments of this philosophical traveller, who, for sound judgement and unaffected science, had few superiors.

From what I have seen, and heard from the inhabitants respecting those luminous appearances which "haunt the mountain summits," I am confident that they are in general no other than the usual development of electric matter from thunder-clouds. A mountain up the Sibaroni is (doubtless from the same cause,) reputed to be volcanic: some peaks, indeed, seem to attract the lightning more than others; to this, both their form and composition may contribute, and perhaps even their vegetable covering. It is said that the *Mora* tree (a large species of *Mimosa*,) attracts the lightning more than any other tree in Guiana.

I have found from numerous trials (by counting seconds after the flash,) that thunder is seldom heard at a greater distance than 20 miles; indeed, I have rarely heard it when more than 15 miles distant, and the electric stream, in this case, is seen at a considerable elevation above the horizon.

Great guns are heard much further than thunder, or at nearly double the distance: the 8-o'clock gun at Demerara is often heard at Cape Batave, on the west coast of Essequibo, a distance of 40 miles. In respect to the intensity of sound, however, there is nothing we are acquainted with, that can bear a comparison with the explosions of volcanos. On the bursting forth of Mount Soufriere, in the Island of St. Vincent, on the night of the 1st of May 1812, the explosions were heard in this colony (Demerara) like reports of cannon, more than 500 miles from the island. To this fact many people here can testify as well as myself. We are told, indeed, that these explosions were heard even at Cayenne, and the Rio Negro, a distance of about 300 nautical leagues. Of this I was assured by the natives, as well as by Captain Orosco, then commandant of the Fort of Rio Negro. The island of

Barbadoes at noon of this memorable day was involved in midnight darkness, and the whole surface of the island was covered with the dust or cinders: of this substance I have a specimen which fell upon the deck and sails of a vessel, 150 miles to windward of the volcano. In this case it is probable that a continued succession of ignited materials being projected from the mountain and rarefying the air, an upward draught was established, which carried the volcanic dust into the higher regions of the atmosphere, whence it was wafted by upper currents and fell over a vast space around. With these phænomena history affords no parallel instance; although it is mentioned by Pliny, that the explosions of Ætna had been heard, and its cinders thrown upon the African coast. But to resume the consideration of 'heat lightning.'

When reflected lightning only can be seen, darting from below the horizon, we have no means of forming an estimate of the distance of thunder-storms. We shall perceive the flash or reflected light when the electric stream itself is developed far below the horizon; it is not strange, therefore, that we so often see the flash without hearing the explosion.

Were observations made for this purpose in countries well inhabited and of considerable extent, as France, Germany, &c., an estimate might be formed of the place of distant thunder-storms and great rains, or of those at least which happen at night. Such observations would, perhaps, effect much for the advancement of meteorological knowledge; and thus, I presume, those meteors which are vaguely regarded as silent electrical emanations, would be found to proceed from real thunder-storms, in France engendered about the Pyrenees especially, and in North America from the Alleghany mountains: that such is the case I feel persuaded, at least with respect to the equatorial regions. By observing the direction of those flitting lights, and by subsequent inquiry at places falling under the same rhumb, we might often be enabled to trace out the localities of those mysterious *fires of the horizon.* This remark regards the observations requisite for finding the place of *heat lightning*, so called, properly reflected lightning, as I presume.

Since this subject was laid before the Philosophical Society (of British Guiana), I have been enabled to obtain such information from intelligent persons residing far up the Demerara River (near the falls and mountainous regions) as seems to place the matter altogether beyond a doubt; especially from Mr. Forsyth, who lives near the falls, and Mr. Paterson (of Christiansburgh), a gentleman of probity and correct observation. According to their testimony, the great rains (about June and

July) are there ushered in by the most tremendous peals of thunder and vivid lightning, by which large trees are often shattered to pieces. The same is further corroborated by the testimony of the Indian natives*, who (in light canoes) pass in a few hours from the high lands enveloped in thunder-storms to a serene air on the coast where no signs of the storm appear, except at night, in those silent flashes which dart from below the horizon in the direction of the mountains.

On the flat coast of Guiana thunder-storms are comparatively rare. It is the more elevated summits, as the chain of granitic mountains, which produce these extraordinary meteoric phænomena, furnishing, as it were, electrical magazines, which, for certain periods of the year, render it the region of incessant thunder-storms and impetuous rains. Thus, too, the elevated ridge of the Isthmus of Darien, attracting the electricity from both oceans, is so remarkable for torrents of rain and unequalled tempests of thunder and lightning; as at Porto Bello and other parts of this chain.

I trust that the foregoing considerations may serve to prove that the phænomena which go under the vague names of heat lightning, *feux d'horizon*, &c., being regarded as *phosphorescences*, *ignes fatui*, or some *silent* form of electricity, are no other than *reflected lightning* from distant thunder-storms. It should be recollected, too, that they are seldom observed during the *dry season in the interior*, between September and March. It is not generally considered that the driest season on the coast is often contemporaneous with great rains amongst the interior mountains; that here the rains commence; and that when the plains also become the theatre of rains, and veiled in clouds, those distant and faint illuminations are now rendered invisible by the density of the horizon. The small distance at which thunder is audible, and the vast space over which the electric fluid, in reflected gleams, be-

* The vast tract, only known by the name of *Interior Guiana*, forms one of the most interesting portions of South America. Those countries bordering on the north, the south and west, as Mexico, Brazil, Peru, &c., have been more or less explored by European travellers, but Guiana remains a real *terra incognita*. The writer has traversed some part of it, having been deputed by the Colonial Government in 1810, on an embassy amongst the interior tribes inhabiting the Essequibo, the Parime, the fabled El Dorado, and North Brazil, in an expedition most summary and ill planned: he was able to do little more than take a draught of the route and to mark a few of the more interesting features of the country. Having resided, however, nearly 25 years in the British Colonies and Spanish America, and made numerous excursions inland, he can aver that a hundredth part of the amount so liberally bestowed by the British Government for the discovery of a *North-west passage* would, if laid out in *exploring Guiana*, be productive of far greater benefit to science and to the interests of the nation.

comes visible, is in general overlooked, the visual ray of the *stream* itself being intercepted by the earth's rotundity, or by an extended horizontal volume of dense vapours. It may be further observed, that those supposed silent gleams are never seen in the zenith nor close to any one, and that when the rains approach sufficiently near, the *electric stream* itself becomes visible, and thunder audible, at first very low, but louder as the distance diminishes.

The northern lights (which are doubtless electric) are attended with a loud crackling noise, which, in America, is often experienced as low as 44° north, as I have witnessed at Bath, New Hampshire, in the months of December and January, especially in the winter of 1798–9. This happens only in the most severe cold; but thunder-storms and *heat lightning*, so called, chiefly occur in the hottest season, or about the month of July. It would hence appear that both a high degree of heat and great cold are favourable to the production of atmospheric electricity: its explosions in the latter case (that of the aurora borealis) seem to be greatly diffused, and sound like numerous sparks elicited by the electrical battery.

It would seem, in such cases, that the electric fluid, being in a manner insulated by the frozen surface, and its evolution from the earth thus intercepted, becomes, in the absence of aqueous vapour and gaseous emanations, incapable of condensation, so as to form an explosive current: and in such condition—the earth being sealed by frost—a thunder-storm was probably never known to happen.

A brief quotation or two may serve concisely to exhibit the exceedingly vague ideas entertained on the present subject.

"In a *serene sky*, the lightning in this country at least, almost always hath a *kind* of *indistinct appearance* without any determinate form, like the *sudden illumination* of the atmosphere occasioned by firing a quantity of *loose gunpowder*; *but when accompanied with thunder, it is well defined*, and hath very often a zigzag form.........that which appears *like indistinct flashes*, whose form cannot be readily observed, is seldom or never known to do hurt."—*Encyc. Brit.*, Art. LIGHTNING. "The reason why this kind of lightning is never attended with any report, is, that there is no particular object against which the force of the flash is directed....A flash of lightning, however limited its extent may appear, diffuses its effects over a great space of atmosphere, for, after one of these *silent* flashes, it is no uncommon thing to observe the sky become obscure though it had been quite serene before."—*Encyc. Londinensis*, Art. LIGHTNING. These remarks, although but an echo of established opinions, appear altogether too puerile and absurd to require any comment.

In conclusion, it may be remarked, that these phænomena have been little noticed in this country, and for the reason, perhaps, that mountains of considerable magnitude are not abundant in England; nor are thunder-storms so common here as in tropical countries, or during the very hot months in North America*.

J. HANCOCK.

LVIII. *On the probable future Extension of the Coal-fields at present worked in England.* *By the Rev.* W. D. CONYBEARE, *M.A., F.R.S., &c.*

[Continued from p. 163.]

WE have already traced the carboniferous beds reposing on the eastern flank of the great anticlinal line of the Pennine chain through Derbyshire as far south as Nottingham; and we concluded with the observation that it appeared desirable to endeavour to ascertain its prolongation in this direction south of the Derwent.

Mr. Sedgwick has lately ascertained that the transition chain of Charnwood Forest forms an anticlinal line ranging from north-west to south-east †; and he considers the carboniferous limestone of Breedon, &c., as resting against the western slope of this anticlinal in its extension northwards, so that it

* We have deemed it right to give insertion to Dr. Hancock's paper, because the error which he refutes is still widely prevalent; but meteorologists, we believe, have for some time been aware of the true nature of the phænomenon in question, in proof of which we may cite the following passage from Mr. Luke Howard's "Climate of London," (Second Edition, vol. ii. p. 309,) with which, however, Dr. Hancock is evidently unacquainted.

"*Summer Lightning.*—It is a popular error, very commonly entertained, that on fine summer evenings there is sometimes a harmless kind of lightning without the usual accompaniments of dense clouds and electrical explosions. The mistake has originated in the great distance at which lightning may be perceived in a dark night."

"Seventh Mo. 31. [July 31, 1813.]—I perceived much faint lightning in the S.E., although it was bright starlight, and not a cloud visible at the time. On communication with my brother, who was then at Hastings, he informed me that they had on the above-mentioned evening a heavy thunder-storm in view for some hours, ranging, as he conceived, *in a line between Dunkirk and Calais on the opposite coast.* It is probable, therefore, that the greater part of the discharges, the faint light of which was perceptible at Tottenham, were actually made at the distance of a hundred and twenty miles. I saw, however, one stroke with the usual linear zigzag appearance, which I judged to proceed from the earth to the clouds, and which may have been *a returning stroke* far on this side of the storm." See also p. 45. of the same volume of Mr. Howard's work for additional evidence on the subject, well understood by the author, in conformity with the explanation here quoted.—EDIT.

† See Lond. and Edinb. Phil. Mag. for January, p. 69.—EDIT.

probably joins on to the great Derbyshire anticlinal in this direction. If so, we must necessarily look for the prolongation of the Nottingham coal-field to the east of a line drawn from Derby through Charnwood Forest and the insulated hummocks of the same sienitic chain ranging along the west bank of the Soar nearly to its source. The valley of the Soar below Leicester will sufficiently indicate the most probable line of the extension of the carboniferous deposits; and in this Journal for May 1829 (p. 347.), Mr. Forster has already communicated some account of this district, from which it appears that traces indicating coal have been observed at Birstall on the Soar, in this quarter: but still as the whole surface is covered, and as the substrata are effectually concealed by the overlying horizontal deposits of red marl, the only prudent mode of proceeding would be to attempt to trace this hidden outcrop of the coal-measures from the known Nottingham coal-field on the north, and to pursue it thence to the south by a regular series of borings. The Charnwood anticlinal line appears, as we have said, to be prolonged southwards nearly to the source of the Soar; and in the direction of Lutterworth it must be overlaid by such a mass of new red sandstone and lias that we can entertain little hope of reaching the coal within any workable depth in that quarter.

But on the west of this anticlinal line we find the Warwickshire coal-field, ranging by Atherstone and Nuneaton: this is bounded on the east by a transition chain of grauwacké and quartz rock traversed by beds and dykes of trap rocks*. This coal-field is worked from near Coventry on the south, almost as far as Tamworth to the north.

In Mr. Yates's account referred to in the note, it is said to be bounded on the west by a limestone ranging from Bedworth by Arbury to Annesley; but no description of its character or relative position is given which can enable us to judge whether it is magnesian lime resting on the coal, or an older lime supporting it. As it is said at Bedworth and Arbury to dip west, conformably to the coal, we should naturally conclude it to be the magnesian superstratum; but as at Annesley it is said to have an opposite dip, it may possibly be an older rock abutting against the coal by a fault. This point should be carefully investigated, because on its solution depends the problem, whether the coal of this field is

* In my 'Outlines' a hasty glance had induced me to mistake the quartz for a variety of millstone grit, and the grauwacké for coal shale; but I am happy to declare my assent to the correction of my errors by Mr. Yates, Geol. Trans. N.S. vol. ii. p. 261.

here thrown out, or whether we may expect to be able to pursue it further westwards beneath this limestone *.

I conceive it most probable that the Warwickshire coal-field is separated from that of Ashby Wolds on the north by a prolongation of the same anticlinal undulation which throws up the transition chain of Atherstone and Nuneaton already mentioned, although this prolongation is concealed by overlying horizontal strata of red marl.

The Ashby coal-field, which skirts the Charnwood chain on the north-west, appears to be subdivided into the two small basins of Ashby Wolds and Cole Orton by an anticlinal ranging in a direction parallel to that already assigned to the Charnwood anticlinal, viz. north-west and south-east, and passing through the town of Ashby: altogether the substrata of this whole district appear affected by so many undulations as to afford scarcely any indications of the probable lines in which we may look for their prolongations, beyond their known boundaries, beneath the horizontal investiture of red marl.

Crossing the Trent to the north, and approaching towards the great emergence of the subjacent carboniferous lime of Derbyshire, we find two localities in which coal is worked to the south and south-east of Ashborne, viz. Darley Moor, and Sprinxhall in Edlaston parish. Patches of carboniferous limestone emerge from the red marl in the vicinity of both these pits; but we have as yet no information how far the circumstances indicate any connexion of the coal-measures between them, or with the nearest coal-field on the west, that of Cheadle, which must be within five miles of Darley Moor. According to Farey, however, the undulation of the strata constitutes the Cheadle field into a detached basin; but the whole of this district requires reexamination.

Of the central coal-fields, that of Dudley remains for examination; but the probable extension of this being connected with the western coal-fields of Shropshire, it will be more properly considered in a future communication, which I hope to prepare for the ensuing month.

Your old Correspondent,

W. D. Conybeare.

* I was originally inclined to believe, from the general dip of the Warwickshire coal-field to the west, and that of the south-eastern portion of the Dudley coal-field to the east, that these two fields extended continuously beneath the intervening red marl; but Mr. Yates's observations of the eastern dip of the Annesley lias, and a westerly dip along the eastern extremity of the Dudley field, where also, near Walsall, the transition lime emerges, render it more probable that they are separated by an anticlinal line.

LIX. *A Catalogue of Comets. By the Rev.* T. J. Hussey, *A.M., Rector of Hayes, Kent.*

[Continued from p. 207.]

[The Chronology employed is that of Petau or Petavius.]

A, the comet of 1680. B, that of 1652. C (Halley's), that of 1682. D, that of 1759. E, that of 1661. F, that of 1677. G, that of 1556. H, that of 1665. I, that of 1585. K, that of 1744. L, that of 1737.

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
352	1338	...	April	Gemini	Chr.Rot.,Nangis.	
353	1340	...	Febr., March..	Scor.,Vir., Leo	Ch.Rec.,Gregor., Fab.Chro.,&c.	
354	1345	...	July	Ur. Maj., Leo..	Gregoræ Byz. Hist.	
355	1347	...	August	Caput Medusæ	Chron. Nuremb. Giov. Villani.	
356	1351	...	Nov., Dec......	Andro., Aries, Taurus,Canc.	Chin. Rec., Matt. Villani, &c.&c.	
357	1356	...	Sept.Oct.Nov.	Hydra	Chinese Records	Elements computed by Burckhardt*.
358	1360	...	March	Tow[ds] the east	Chin.Rec., Chro. Zwetl.	
359	1362	...	March, April ..	Aquar., Pegas., Taurus.....	Ch. Rec., Syn. Chr.Nangis,&c.	Elements computed by Burckhardt*.
360	——	...	June,July,Aug.	Capricornus...	Chinese Records.	
361	1363	...	March..........	......	Chinese Records.	Seen for a month.
362	1366	...	August	Ur.Maj., Scorpio,Aquarius	Chinese Records.	
363	1368	...	Feb., March, April	Taurus	Chin.Rec., Walsingh.,Nan.&c.	
364	1371	...	January.........	Towards the north	Bonincont. Ann.	
365	1376	...	June,July,Aug.	......	Chinese Records.	
366	1378	B	September.....	Antinous, Ur. Major	Chin. Rec., Ann. Adlzreitter ...	Motion very rapid and retrograde.
367	1380	C	November.....	......	Chi.Rec., Chron. Citizense.	
368	1382	...	March.........	......	Chi.Rec., Chron. Bothon.	
369	——	...	August	......	Annal. Aug. Bonfin. Ann.	
370	——	...	December	To the westward	Walsingham....	Seen 15 nights.
371	1385	...	October	Ur.Maj.,Hyd., Crater	Chinese Records.	
372	1391	...	May	Ursa Major...	Ann.Foroliv.,Ch. Records	Small & faint.

* See note, page 352.

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
373	1399	...	November....	......	Mezerai.	
374	1402	E	Febr., March, April......	Aries..........	Pogg., Ebendorf., Ann. Japon. &c.	
375	—	...	June, July, Aug., Sept.	......	Ducas, Bossii Chr. Cand. Dec. &c.	
376	1403	...	March, April	......	Camer. Fabric. Aret., &c. &c.	
377	1406	...	In the first six months....	......	Chron. Brem. Gerbrand.	
378	1408	...	October........	......	Petri Phil. de Lig., Rolewink, &c.	
379	1430	...	August.........	......	Japanese & Chinese Records.	
380	1431	...	May............	Gemini.......	Chinese Records.	
381	1432	...	February	......	Chron. Polon., Dlug. Ann. Tur.	
382	1433	...	Sept., Oct., Nov.......	Coron. Boreal. Boot. Hercul.	Chron. Dlug. &c. Chin. Rec.	Small; seen one week.
383	1436	...	Autumn........	......	Boethi. Bzovius.	
384	1439	...	......	......	Ann. Japon., Dlugoss., Fabric.	
385	1444	...	June...........	Aries..........	Leovit. Fabric., Mizaud, &c. &c.	
386	1450	...	January.........	Hercules.......	Chinese Records.	
387	1452	...	March	Taurus.........	Chinese Records.	
388	1453	...	January.........	Cancer	Chinese Records.	
389	1454	...	Summer........	......	Phranza.	
390	1456	C	June...........	Taurus, Perse.	Sim., Ann. Flan. Hirsaug., &c.	Elements computed by Pingré*.
391	1457	...	June...........	Pisces..........	Chron. Noremb. Carion, &c. &c.	
392	—	...	June...........	Gemini	Ebendorf., Antoni. Roritz. &c.	
393	1458	...	June...........	Taurus.........	Chinese Records.	
394	1460	...	August.........	......	Boethius, Sturm. Wolf., &c. &c.	
395	1463-4	...	Winter, spring	Virgo, Leo	Chinese Records.	
396	1465	...	March	......	Chin. & Japanese Records.	
397	1467	...	October........	Pisces..........	Anon. Chron., Chro. St. Ægid.	
398	1468	...	February.....	Tow^{ds} Ur. Maj.	Chinese Records.	
399	—	...	Sept., Oct., Nov.	Leo, Ur. Maj...	Theolph. Annal. Aug., &c. &c.	Very small.
400	1471	...	Autumn........	Virgo..........	Matt. de Michov. Chron. Cureus.	
401	1472	...	Jan., Feb.......	Virgo, Bootes, Ceph., Andro.	Chi. and Japan. Rec., Mich. &c.	Elements computed by Halley*.

* See note, page 352.

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
402	1476–7	...	Dec., Jan.......	......	Ripamont. Pontanus, &c. &c.	Small.
403	1477	...	December	......	Chron. Bossian.	
404	1478	...	September.....	......	Chron. Bossian., Cavitellius.	
405	1479	...	......	......	Lycosthenes, Hevelius.	
406	1491	...	Jan., Feb.......	Pisces, Aries...	Chron. Boss., Michov., Regiom.	
407	?	...	January	Cygnus.........	Chinese Records.	
408	1494 or-5	...	......	......	Scaliger.	
409	1500	...	April, May.....	Sagit., Capric.	Ch.Rec., Michov. Comiers, &c.	
410	1503	...	August.......	......	Chron. Waldsassense.	
411	1505	...	April............	Aries............	Chron. German., Stumpff. &c.	
412	1506	...	July, August..	Near the Pole	Chin. and Japan. Rec., Crom. &c.	
413	1512	...	March, April...	......	Chron. Magd., Schuler, &c.	
414	1513–4	...	Dec., Jan., Feb.	Can., Leo, Virg.	Vicom., Schuler.	
415	1516	...	January.........	......	Bizar. Gen. Hist., Chr. Mansf. &c.	Seen only a few nights.
416	1518	...	April............	......	Cavitellius.	
417	1521	...	June............	......	Keckerman, Cavitellius, &c.	
418	1522	...	......	......	Mizaud. Rockembach, &c.	
419	1523	...	October........	......	Schuler, Rockembach, &c.	
420	1528	...	January	......	Littara, Riccioli.	
421	1530	...	November.....	......	Mizald. Cardan., &c. &c.	
422	1531	C	July, August...	Gemini	Chin. and Japan. Rec., Vapov., Guicciard. &c.	Elements computed by Halley*.
423	1532	E	Sep., Oct., Nov.	Virgo, Libra...	Ch. Rec., Vapov., Fracastor., &c.	Elements computed as in note*.
424	1533	...	June, July, Aug.	Gemi., Taurus, Aries	Ch. Rec., Gassar. Ann. Vapo. &c.	Elements computed as below*.
425	1538	K ?	January	Pisces	Eber. Mizald., Fabric. &c. &c.	
426	1539	L ?	April, May.....	Leo, Virgo	Chin. Rec., Apian Ann. Aug. &c.	
427	1545	...	......	......	Aretius.	
428	1556	G	Feb., Mar., Apr.	Libra, Virgo, Bootes.......	Annal. Aug., Cardan., Gemma.	Elements computed by Halley*.

* See note, next page.

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
429	1557	...	October	Sagittarius....	Camerarius.	
430	1558	...	July, Aug. Sept.	Leo, Virgo. ...	Mezerai, Camerarius, &c. &c.	Elements computed by Olbers*.
431	1560	....	December......	......	De Thou.........	Seen 28 nights.
432	1569	...	November....	Serpen., Sagit., Capric.......	Kepler, Gemma, Gilbert, &c.	
433	1577	...	November.....	Pegasus.........	Ann. Japon., Crusius, &c. &c.	Elements computed by Halley*.
434	1580	...	Oct., Nov......	Pisces, Austr., Aquar., Oph.	Tycho, &c.	Elements computed as in note*.
435	1582	...	May...........	Gemini........	Chi. Rec., Tycho, Camden	Elements computed by Pingré*.
436	1585	...	Oct., Nov......	Pisces..........	Tycho, &c.	Elements computed by Halley*.
437	1590	...	March..........	Pisces, Triang. Perseus.....	Mylius, Tycho...	Elements computed by Halley*.
438	1593	...	July, August...	Cancer, Gemi., Tau., Ceph.	Chi. Rec., Rock, Hevelius.......	Elements computed by La Caille*.
439	1596	...	July, August...	Auriga, Cancer, Virgo, U. Maj.	Kep., Tycho, &c.	— as below*.

Year.*	Passage of the Perihelion in Mean Time at Greenwich.	Longitude of the Perihelion of the Orbit of the Comet.	Longitude of the Ascending Node.	Angle between the Perihelion and the Node.	Inclination.	Distance in the Perihelion.	Logarithm of the Mean Motion.	Motion.	Name of the Computer.
	d h m	s ° ′ ″	s ° ′ ″	s ° ′ ″	° ′ ″				
1351	Nov. 26 11 5	2 9 0 0	uncertain	uncertain	uncertain	1·000000	9·960130	D.	Burckhardt.
1362	Mar. 11 4 51	7 9 0 0	8 9 0 0	1 0 0 0	21 0 0	0·455800	0·472020	R.	Burckhardt.
	2 7 51	7 17 0 0	7 27 0 0	0 10 0 0	32 0 0	0·470000	0·470730	R.	Burckhardt.
1456	June 8 22 1	10 1 0 0	1 16 30 0	3 17 30 0	17 56 0	0·585520	0·308818	R.	Pingré.
1472	Feb. 28 22 24	1 15 33 30	9 11 46 20	7 26 12 50	5 20 0	0·542730	0·358252	R.	Halley.
1531	Aug. 24 21 19	10 1 39 0	1 19 25 0	3 17 46 0	17 56 0	0·567000	0·329754	R.	Halley.
	25 19 1	10 1 12 0	1 15 30 0	3 14 18 0	17 0 0	0·579930	0·315058	R.	Halley.†
1532	Oct. 19 22 12	3 21 7 0	2 20 27 0	1 0 40 0	32 36 0	0·509100	0·399924	D.	Halley.
	19 14 53	4 15 44 0	3 29 8 0	0 16 36 0	42 27 0	0·612550	0·279416	D.	Méchain.
	18 7 59	3 21 48 0	2 27 23 0	0 24 25 0	32 36 0	0·519220	0·387101	D.	Olbers.
1533	June 16 19 31	3 14 12 0	4 5 44 0	0 21 32 0	35 49 0	0·202800	0·999526	R.	Downes.
	14 21 11	7 7 40 0	9 29 19 0	9 8 21 0	28 14 0	0·326860	0·688585	D.	Olbers.
1556	Apr. 21 20 4	9 8 50 0	5 25 42 0	3 13 8 0	32 6 30	0·463900	0·460492	D.	Halley.
1558	Aug. 10 12 51	10 29 49 0	11 2 36 0	0 2 47 0	73 49 0	0·577300	0·318030	R.	Olbers.
1577	Oct. 26 18 46	4 9 22 0	0 25 52 0	8 16 30 0	74 32 45	1·183420	1·064958	R.	Halley.
1580	Nov. 28 15 1	3 19 5 50	0 18 57 20	3 0 8 30	64 40 0	0·596280	0·296953	D.	Halley.
	28 13 45	3 19 11 55	0 19 7 37	3 0 4 18	64 51 50	0·595530	0·297774	D.	Pingré.
1582	May 6 15 59	8 5 23 10	7 21 7 20	11 15 44 10	61 27 50	0·225695	0·929845	R.	Pingré.
	7 8 21	9 11 26 45	7 4 42 35	9 23 16 50	59 29 5	0·040066	2·055997	R.	Pingré.
	O — S								
	N — S								
1585	Oct. 7 19 21	0 8 51 0	1 7 42 30	11 1 8 30	6 4 0	1·093580	9·901853	D.	Halley.
1590	Feb. 8 3 46	7 6 54 30	5 15 30 40	10 8 36 10	29 40 40	0·576610	0·318805	R.	Halley.
1593	July 18 13 39	5 26 19 0	5 14 15 0	0 12 4 0	87 58 0	0·089110	1·535218	D.	LaCaille.
1596	Aug. 10 19 56	7 18 16 0	10 12 12 30	2 23 56 30	55 12 0	0·512930	0·395041	R.	Halley.
	8 15 33	7 28 30 50	10 15 36 50	2 17 6 0	52 9 45	0·549424	0·350266	R.	Pingré.

[To be continued.]

† Eccentricity 0·967373.

Erratum in page 206, line 8 from bottom, *for* 1264, G, *read* G?

LX. *Account of two Experiments on Accidental Colours; with Observations on their Theory.* *By Sir* David Brewster, *LL.D., F.R.S. & V.P.R.S. Ed.*

ALTHOUGH considerable progress has been made in observing the phænomena of accidental colours in all the various forms under which they present themselves, yet we know almost nothing of the manner in which light acts upon the retina when it is rendered insensible to particular colours of the spectrum. In making some experiments on this subject, I observed two very curious facts, which possess some interest independent of their theoretical relations.

1. It has been long known that when the eye is under the influence of a luminous impression which causes it to see the accidental or complementary colour of the exciting light, the accidental colour often vanishes and re-appears. If a smart blow is given to the head when the eye sees the accidental colour in its first and brightest phase, it will instantly disappear, so that the vibration thus suddenly communicated to the retina restores to that membrane the sensibility to the primitive or exciting colour which it had lost. I varied this experiment by giving the blow to the head before the accidental colour was seen, that is, when the eye was still fixed upon the exciting colour. When this was done, and the eye quickly turned upon the white ground, the accidental colour was not visible.

I was now anxious to learn whether the same effect would be produced by strong vibrations communicated to the head through the intermedium of the air, and with this view I had a large gong powerfully struck close to my head when the accidental colour was a maximum. I could not observe, however, the slightest change either in the intensity or duration of the complementary impression.

2. The influence of strong light in rendering the retina partially insensible to red rays, even when these rays fall upon a part of the membrane which has not been directly acted upon by the strong light, has been finely shown by the experiment of Dr. Smith of Fochabers*, and I have mentioned in a former paper † that a stick of red sealing-wax may be thus made to appear of a dark liver-brown colour.

If we apply the strong light to the eye when the sensibility of the retina has been locally diminished by looking at a red object, a total insensibility to red light will be produced. In order to observe this curious result in perfection, let the eye be steadily fixed for some time upon a seal of red wax

* Dr. Smith's paper will be found in Lond. and Edinb. Phil. Mag., vol. i. p. 250. † *Ibid.*, p. 172.

which reflects white light from all its elevated parts. When the eye has been so fatigued that it would see a bright accidental green, bring a candle close to the excited eye, and so near its axis that the red seal will be seen by rays which pass near the flame of the candle. When this is done, the red wax seal will be converted apparently into a seal of black wax, the lights reflected from its elevations being still distinctly seen. This experiment, when successfully made, affords one of the most remarkable optical deceptions with which I am acquainted.

The method now described of eliminating the impression of the primitive or exciting colour leads us to a very important determination in the theory of accidental colours. I endeavoured long ago to show from analogy, as well as from the evidence of experiment, that the vision of the primitive and the accidental colour is contemporaneous, in the same manner as the fundamental and the harmonic sound are heard contemporaneously by the ear. That this is the case may be shown in the following manner. When the eye is fatigued with the excitation of the red seal, a faint green phosphorescent-looking light covers for a while the surface of the red seal, occasionally overpassing its margin, showing, in the clearest manner, that the accidental green is seen at the same time with the exciting red. The effect of this vision of the green is to make the red appear much paler by its admixture with it. The red and green tend to produce whiteness; but as the direct red greatly predominates over the accidental green, the result is always a pale red. But when a brilliant light is brought near the excited eye so as to extinguish completely the red rays, the phosphorescent green appears alone; and thus we have ocular demonstration that the accidental green is not the light of a white ground deprived of the red rays to which the eye has been rendered insensible, but is a colorific impression generated in the retina itself, and superadded to the whiteness of the ground in the case when the eye is turned from the exciting colour to a white object. These results are obviously incompatible with the theory of accidental colours recently published by M. Plateau.

LXI. *Observations on the Visibility of the Retina; with Remarks upon its probable Cause. By* T. W. W.*

THE remarkable experiment in which the blood-vessels of the retina are rendered visible has excited so much attention, that the following observations upon it may, perhaps,

* Communicated by the Author. For other papers on this subject see Lond. and Edinb. Phil. Mag. for January, February, and April.

not be thought undeserving of a place in the pages of the Philosophical Magazine. It should be previously stated that the writer is so near-sighted as not to be able to see any object with perfect distinctness at a greater distance from the eye than 8 or 9 inches: but this defect has not proved in the slightest degree prejudicial to the inquiry in the present case, being associated with a power of enduring a glare of light, which has permitted the experiment to be repeated many times under the most favourable circumstances.

The ramifications of the blood-vessels are seen by the writer without difficulty, whenever a motion in any direction is given to the flame of a candle at the distance of a few inches from the eye, nor do they instantaneously vanish when the motion is discontinued. They invariably appear very dark, upon a brownish ground. The position of the flame with respect to the eye is not material, so long as a distinct image of it is received upon the retina. In proportion as the candle is removed from the axis of vision, the ground appears darker upon which the vessels are seen, while they become more sharply defined, their distinctness seeming to depend upon their distance from the flame. The effect is precisely the same whether the eye or the candle be moved. The motion of the flame, the eye remaining at rest, produces an apparent motion of the spectrum, always opposite in direction, but much inferior in degree. When two flames are alternately employed in rapid succession, one in front of the bridge of the nose, the other near the exterior canthus, the vessels successively observed appear at first not to be the same; but this is merely a deception, arising from a difference in the position of the spectrum corresponding to the change in the direction of the light. When two flames are placed on the same side of the eye, two spectra, separated from each other by a small interval, have occasionally been seen, but with difficulty. When two flames are simultaneously employed on opposite sides of the eye, and moved in contrary directions, the two spectra may be plainly observed at once, and easily distinguished from each other by their relative motion. The addition of a second candle doubles or at least materially increases the brightness of the ground upon which the vessels are seen. The interposition of a concave lens of 8 inches focus, which is requisite to produce perfect vision of distant objects, considerably diminishes the distinctness of the phænomenon. When a convex lens, of about an inch focus, is so placed with respect to a candle that its whole area appears luminous, the candle also being very near the eye in an oblique position, the blood-vessels are seen strongly marked and with peculiar precision, their most minute ramifications being rendered visible. When in this state, if the light is suddenly intercepted by moving the lens, the spectrum changes from dark to bright previous to its disappearance. The vessels near the flame have occasionally been thought to exhibit an effect of light and shade when viewed in this way, or even without the lens, if the distance of the candle from the eye is very small ; the side nearest the flame exhibiting a faint degree of reddish illumination : this appearance, however, was rather uncertain.

The foregoing observations relate solely to the ramifications of the blood-vessels ; but the most remarkable facts are those connected with the appearance of the *foramen centrale.* When the flame is situated very obliquely with respect to the axis of vision, a space is perceived in the centre of the field of view across which no ramifications extend, though a great number of minute ones diverge from it, especially in the right eye, the distribution of the vessels in the two eyes being very dissimilar. As the flame is made to approach, in any direction, to this vacant space, a dark crescent becomes visible within it, bounded on the convex side by a fine line of light. As the flame approximates to this crescent, it increases rapidly in breadth in the centre, becomes of a semilunar form, and could it be seen in apposition with the flame, would apparently be converted into a dark circle. By examining this phænomenon on every side, and especially by causing the flame to describe a circle round the centre of the field of view, in which case the crescent appears to describe a similar but opposite circle round the same point, it is rendered perfectly evident that this crescent is a shadow upon the interior declivity and bottom of the *foramen centrale*, which follows in every position the laws of the shadow in a very shallow cup or cavity illuminated by an oblique light. It is everywhere accompanied by a narrow ring at its external edge, more luminous than the surrounding space, precisely as though the depression were encompassed, like the cavities on the lunar surface, by an elevated rim ; but no traces can anywhere be perceived of a shadow cast by this apparent rim outwards, on the side which is furthest from the light, nor can the rim itself be discerned beyond the extent of the dark crescent. The shadow is always better defined on its convex than on its concave side. On the side nearest to the nose, in the apparent position of the cavity, which, however, is unquestionably inverted, both the shadow and the rim are much more conspicuous than in the opposite direction ; they may, however, be satisfactorily traced all round. The *foramen centrale*, thus distinctly rendered visible, is elliptical in a horizontal direction, the ratio of the axes being by estimation about as 4 to 5. When the flame is sufficiently removed from the axis of vision in any direction, not a trace of the rim or shadow can be perceived. That part of the *foramen* which is not actually in shadow does not in any case appear darker than the surrounding retina; when brought very near the flame it becomes brighter, and an indistinct yellow glare is perceived without it, on the side furthest from the light, such as might result from reflection from the interior of a slight depression; when very near the flame, this reflection appears to extend considerably beyond the limits of the *foramen*. In certain positions, especially immediately above the summit of the flame, some minute ramifications of vessels may be traced within the *foramen*, on its apparently upper side.

Such are the phænomena observed when the experiment is tried with a flame from 4 to 6 inches distant from the eye: when it is removed to twice the latter distance, the ramifications, though still visible, are less distinct, and upon a darker ground; they may, however, be more plainly detected within the *foramen* than before. At the distance of 18 inches the ramifications disappear; but with great attention the *foramen* may be discerned, and the diminished glare of light permits a more careful examination of it. When about twice its own longest diameter removed from the flame, it appears filled with shadow; but as the interval diminishes, the shadow is succeeded by a yellowish light, brightest in the part which is most usually in shade, namely, the declivity of the sides: this effect does not extend across the centre of the *foramen*, which is occupied, even when nearly in apparent contact with the flame, by an ill-defined dusky blot. The rim which surrounds the cavity is very visible in the same situation, on the side next the candle; but it is now converted into a dark line, forming a conspicuous object in the greatest possible proximity to the flame.

When the experiment is repeated with the solar rays, the eye being gently moved through a small space, or the rays being alternately admitted and intercepted, the phænomenon may be very distinctly seen, but not so conveniently as with an artificial flame; the direction also of the light is required to be more oblique, or the spectrum is effaced by its brilliancy. The blood-vessels occasionally exhibit an indistinct effect of light and shade, as in certain cases with the flame of a candle. The ridge of the nose being made by a slight motion of the head to admit and exclude alternately the beam of light, the ramifications become successively dark and bright without vanishing, at least in the left eye. In this position the *foramen centrale* was invisible: when it was well seen it presented an appearance as though there were another rim parallel to the one before mentioned, situated a little within it on the slope of the cavity; but its real nature could not be satisfactorily ascertained. By candlelight no trace of this kind could be discovered, excepting at the end of the ellipsis apparently furthest from the nose, where the rim is least defined, and a suspicion of some arrangement of this kind may be entertained.

The base of the optic nerve lies so far from the axis of vision that the writer finds much difficulty in commanding a view of it. By removing the candle, however, very far towards the exterior canthus, a point may be indistinctly seen from which a number of thick blood-vessels radiate in diverging lines towards the remoter parts of the eye, presenting, in conjunction with the more delicate and tortuous ramifications in the centre of the retina, a spectacle of a beautiful and imposing character. The position of the optic nerve is sometimes marked by an undefined luminous white spot, the existence of which may be considered certain, although it has been frequently sought for without success.

Among the minuter vessels in the centre of the retina, there seem to be many instances of *anastomosis*, but no distinction can be perceived between veins and arteries in their appearance.

Such are the facts which the writer has noticed in a frequent repetition of the experiment, which succeeded at the first attempt, and has never failed in his hands. Many of them may have been previously observed, or may be thought of little importance; but there is one of unquestionable interest, which, as far as his information extends, has never hitherto been noticed,—the shadow in the interior of the *foramen centrale*. That it is real shadow, is incontrovertibly evident from mere inspection when the flame is in motion; but its regular increase in proportion to the diminution of obliquity in the position of the flame, appearing totally at variance with the simplest laws of light and shade, was at first matter of much surprise. The consideration of it has led, however, to the following explanation of the phænomenon, which is here given, not as affording a satisfactory solution of every difficulty, but as deserving the attention of those who are far better qualified to decide upon its probability.

The direction of the shadow and its variations are precisely what they should be, upon the supposition that they depend upon the position and motion of a luminous object situated, not without the eye, but upon the deeply curved surface of the retina itself. That luminous object is evidently the image of the candle formed upon the retina, which becomes a radiant point illuminating and rendering visible all the surrounding parts of the eye, precisely as the image of the sun, received through a small aperture into a darkened room, becomes not only a representation of the object, but a source of illumination to everything in its vicinity. Upon this supposition the brown ground upon which the blood-vessels are seen is no other than a picture of the retina itself, coloured, perhaps, in consequence of its imperfect transparency, by a slight reflection from the choroid coat beneath it. The singular variations in the appearance of the *foramen* when the candle is 18 inches distant from the eye are not at all at variance with this solution. Illumination may be derived from the image of the flame in two different ways, either in consequence of the rapid curvature of the retina, by rays traversing a portion of the vitreous humour in straight lines, or by rays generally diffused through the substance of the retina itself in the vicinity of the image in consequence of its imperfect transparency. The latter mode of illumination will come into operation at those small distances where the former ceases to act from the excessive obliquity of the ray to the illuminated surface. The former produces the shadow observed at a moderate distance from the flame; the latter, the luminous appearance when almost in contact with it. The dusky spot in the bottom of the *foramen* in this instance proves the thinness of the retina in that place; but its perfect uniformity of colour with the surrounding parts in other cases seems to indicate that it is not, as usually stated, deficient. This may very probably be a peculiarity of the eye of the observer, especially as it is asserted in page 550 of the Report of the British Association, that to other observers it appears as a dark spot. The experiments seem to show that the *foramen* is encompassed at its extreme edge with an opake fibre. Its total disappearance at a considerable distance from the flame is a clear proof of its shallowness. The difference of distinctness of the ramifications at different distances from the candle may probably arise from the difference of the above-mentioned modes of illumination. The imperfect transparency of the living retina has been hitherto only inferred from its condition when deprived of life; it may now be considered as established in a very singular and unexpected manner both from the shadow and the light which are observed under different circumstances on the declivity of the *foramen centrale*.

An observation of another kind may be here introduced as having a remote bearing upon the same interesting subject.

The writer distinctly remembers having frequently amused himself, when a child, by applying a considerable and unremitted pressure to the fore part of the eyeball, and watching the extraordinary phænomena which gradually developed themselves, among which the most constant was a luminous ellipsis in the axis of vision. Thinking the experiment an imprudent one, he has long discontinued it, but being struck with the recollection of the peculiar form of the principal object, he has latterly ventured to repeat it two or three times. After a luminous appearance which may be compared to effervescence, he finds that a ring of light begins to be visible, of the same ellipticity as the *foramen centrale* in his eye, inclosing a dark space, equal in diameter to its own thickness, so as somewhat to resemble the remarkable perforated nebula between β and γ Lyræ. The ring, however, is not continuous, but consists of a number of small irregular spots of dull green, with narrow dark interstices. If the pressure is continued, some part of the ring suddenly exhibits a much brighter tinge of brownish pink, surrounded by a narrow border of brilliant yellow; and this hue gradually overspreads the whole ring, filling up all its interstices, and covering nearly the whole of the included space. It seems highly probable that this phænomenon has some relation to the *foramen centrale*; and it is very remarkable that upon one occasion, soon after pressure was applied to the eye, the principal ramifications of the vessels near the axis of vision were seen for an instant in the form of dark lines.*

March 14, 1834. T. W. W.

LXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 13. (*continued from p.* 295.)—THE reading of a paper, entitled, "An Inquiry into the Nature of Death; being an attempt to ascertain its more immediate causes, with a view to the better regulation of the means of obviating them." By A. P. W. Philip, M.D., F.R.S. L. & Ed.—was commenced.

Feb. 20.—The reading of Dr. Philip's paper was resumed and concluded.

The object of the present paper, which the author intends as a sequel to those he has lately presented to the Society, and which have been published in the Philosophical Transactions, is to investigate the operation of the different causes of death, and the mode in which the several powers of the living system influence each other during the period of their decline. In the more perfect animals, he observes, there are three distinct classes of functions, namely, the sen-

* See Lond. and Edinb. Phil. Mag., vol. i. p. 91, for notices of phænomena related to those above described.—Edit.

sorial, the nervous and the muscular, which have no direct dependence on each other, although they are linked together by the connexions of the organs in which they reside; the consequence of which is, that the cessation of any one class of functions is more or less immediately followed by the destruction of the rest. What is commonly called *death* consists in the extinction of the sensorial functions only; for the nervous and muscular functions may still, for a time, survive; although, in consequence of the failure of respiration, which in the more perfect animals the author considers as, in the strictest sense, a function of volition, they also speedily terminate. Thus he distinguishes this sensorial death from what constitutes actual death, that is, the cessation of all the functions, and which occurs at a later period. As far as the sensorial powers are concerned, their decline and cessation are exceedingly analogous to the approach and occurrence of sleep; the only difference being that the former is an irrevocable failure of those powers, while the latter admits of their being resumed with renovated vigour by the continued action of the vital powers.

The modes in which the sensitive functions are extinguished, or in other words *the forms of death*, are referred by the author to five different heads: the first and only natural mode is that from the simple effect of old age, when all the powers of life are completely exhausted by the continued operation of the agents which had excited them; and death is, in that case, only the last sleep. The vital functions are here impaired, chiefly from the diminished frequency of respiration, which is itself a consequence of the impaired sensibility; so that there is a diminution of the action, but not of the powers, of the vital organs. If the decay of the vital powers be gradual, and nothing occurs suddenly to accelerate it, they will necessarily cease at the time when their excitement is the smallest, that is, during the state of sleep.

In all other cases, death arises from causes which must be regarded as adventitious, and consequently as inducing a more or less violent death. The first class of these causes comprises those arising from the continued action of stimulants, more powerful than the ordinary stimulants to which the system is subjected, and making their immediate impression on the organs of the sensitive system. These may be considered as producing a diseased condition of the sensorium, which, by sympathy, communicates its influence to the vital organs. The next form of death is that which is induced by such causes as are applied, in a sufficient degree, to act as direct sedatives to the organs of the sensitive system, that is, to impair their excitability without previous excitement. The third set of causes of death comprehends those which operate by depriving some of the vital organs of those stimulants on which their functions depend; and the last consists of such as directly debilitate those organs themselves. Thus, according to the author, these adventitious causes act either directly by destroying the power of the brain and spinal cord, or by affecting the vital parts of those organs, so as, through them, to destroy the circulation or the assimilatory functions. The destruction of the circulation appears, in all cases, to be the cause of instantaneous death, and always to be effected through impressions made on the vital parts of the brain and spinal cord, except where the injurious agent operates directly on the organs of circulation themselves.

The author considers the vital functions, together with the muscular and nervous powers, which carry them on, as the results of inanimate agents acting on living parts, or living parts on them; and hence he explains the analogy which exists between all these functions and the operations of inanimate nature; while, with regard to the sensorial functions alone, as they are the results of vital parts acting on each other, so no analogy can be perceived between them and those operations.

In the course of the paper the author frequently reverts to the argument, that, to the sentient being, death being simply the loss of sensibility, the last act of dying can in no case be an act of suffering: and in the majority of instances of the long continuance of disease, our tastes, and our relish for life itself, being gradually impaired, death is met, not only with composure, but even with satisfaction.

A paper was then read, entitled, "On the Tides." By John William Lubbock, Esq., V.P. and Treasurer of the Royal Society.

Various tables relating to the tides are communicated in this paper, calculated, according to the instructions of the author, by Mr. Dessiou. In the tables given by the author in former papers, already published in the Philosophical Transactions, and having reference to the corrections due to the influence of the parallax and declination of the moon, Mr. Dessiou employed only observations of the tides made between conjunction and opposition; but in those now given, similar corrections have been obtained from observations made between opposition and conjunction.

The author enters into an inquiry into the correction due to the calendar month, which is mixed up with that due to the moon's declination, and shows that the correction for the moon's parallax, as well as declination, deduced from the theory of Bernoulli, are quite discordant with the results of Mr. Dessiou's calculations, founded on actual observation.

The author agrees with Mr. Whewell in the remark, that the theory of the tides is now in the same state as that which the theory of the motions of the moon and planets presented about a century ago; and unless considerable exertions be made, it may so continue for many years to come. The tables of the planets have acquired their present accuracy only through the liberal encouragement of learned bodies, and of some of the governments of Europe; nor can tables of the tides, adapted to the present state of science, be now constructed, unless very considerable expense be incurred, and immense labour bestowed.

The results of numerous observations on the influence of the wind on the tides in the River Thames, are stated; and the author observes, that this is a subject of considerable importance as regards the accuracy of which tide predictions are susceptible.

The reading of a paper, entitled, "An Account of some Operations executed at Cape Frio, by the Officers and Crew of His Majesty's Ship Algerine, for the purpose of raising a part of the Stores, &c. lost in

His Majesty's Ship Thetis." By the Hon. Commander F. T. de Roos, R.N., F.R.S.—was commenced.

Feb. 27.—The Hon. Commander de Roos's paper was resumed and concluded.

The author, who had the command of His Majesty's ship Algerine, was instructed to take charge of the enterprise commenced by the officers and crew of His Majesty's ship Lightning, having for its object the recovery of the treasure and stores from the wreck of the Thetis, which, in the month of December 1830, had sunk in a cove to the south-east of Cape Frio. He reached this spot on the 6th of March, 1832, having with him eleven officers and eighty-five men. A certain number of men were appointed to remain on board the ship, which was moored in a harbour two miles off; a party of artificers and others were employed at the huts which they inhabited near the Cape; and the rest, nearly thirty-five in number, were stationed at the wreck.

The author gives a description of Cape Frio, and of the island of which it forms the south-eastern extremity, which is an immense promontory of insulated granite jutting into the Atlantic Ocean, sixty miles east of Rio de Janeiro. The cove, in the middle of which the wreck of the Thetis lay, is a square indenture in the cliffs, six hundred feet deep by as many wide. It is surrounded by nearly perpendicular masses of granite, from one hundred to two hundred feet high, and is exposed to the whole swell of the South Atlantic, which sets in with remarkable force in that direction. The weather is singularly variable; and transitions frequently take place in the course of a few hours, from perfect stillness to the most tremendous swell. The author states that he has witnessed few scenes in nature more sublime than that presented by the Thetis Cove during a gale of wind from the south-west.

The author enters into a minute description of the mechanical apparatus employed for obtaining the necessary purchases for the various operations which were required, and gives a circumstantial history of his proceedings. Frequent interruptions were experienced from the state of the weather, and the almost incessant agitation of the water, which was often so powerful as to render the diving-bell unmanageable, and to expose the divers to serious danger. The diving-bell consisted of a one-ton ship's water-tank, with eight inches of iron riveted to the bottom in order to give it more depth, and having attached to it 18 pigs of ballast, the weight of which (17 cwt.) was found to be sufficient to sink it.

As soon as the necessary arrangements had been completed, the author states that he made a minute survey of the bottom, by means of the diving-bell, and ascertained the exact position and shape of all the large rocks which covered the spot where the treasures and stores of the Thetis had been scattered. The shape of the area where the precious metals in particular had been deposited, was an ellipse, of which the two principal axes measured 48 and 31 feet; and large boulders of granite had been subsequently rolled over these treasures, and required being removed before the latter could be recovered. The superincumbent pressure of the sea, aided by the huge materials of the wreck of the frigate, which, under the influence of the swell, had acted like a paviour's hammer, with enormous momentum, had jammed together the rocks, and produced a strong cohesion between the fragments of wood, and the gold, silver and iron.

The first object was to clear away every portion of the wreck; and after this had been accomplished, to loosen and remove all the large rocks in succession, beginning with the smallest, and ending with the largest and most unwieldy. Some of these, which they succeeded in rolling from their situations into deeper water, weighed about thirty or forty tons; and the largest, which required the greatest efforts to move from its place, was computed to weigh sixty-three tons. This last effort served to show, that no part, either of the wreck or the stores, which was of any value, remained behind; and after fifteen sixteenths of the property had been recovered, the enterprise, which had so perfectly succeeded, terminated on the 24th of July, and the Algerine returned to Rio de Janeiro on the 1st of August.

The author subjoins an account of the currents off Cape Frio, and a description of the climate, which seems to have been favourable, for his party suffered but little from sickness, and the expedition was unattended with the loss of a single life. On one occasion the party were visited by a whale, which approached very near the diving-bell, but fortunately changed its course, without doing any mischief.

A paper was then read, entitled, "An Account of a Concave Achromatic Lens, adapted to the Wired Micrometer, which has been named *Macro-micro,* from its power to increase the primary image of a Telescope without increasing the diameter of the wires in the Micrometer." By George Dollond, Esq., F.R.S.

The application of a concave achromatic lens to the wired micrometer of a telescope, arose out of the series of trials that were made for the purpose of correcting the aberrations of the eye-glasses applied to the telescope constructed by the author for the Royal Society, with a fluid correcting-lens, on the plan suggested by Professor Barlow. The concave lens, being interposed between the object-glass and the eye-glass, and being at the same time achromatic, combines the advantages of doubling the magnifying power, without a corresponding diminution of light, and without altering the apparent distances of the threads of the micrometer. The results of the trials made with telescopes to which this addition was made, are given in a letter to the author from the Rev. W. R. Dawes, of Ormskirk; from which it appears that Mr. Dollond's method was attended with complete success. Mr. Dawes states, that, in order to put its illuminating power to a severe test, he had examined with this instrument the satellites of Saturn and the minute companion of κ Geminorum, but could discover no decided difference in the apparent brightness of the former, allowance being made for the difference in the power employed; and the latter star was seen quite as distinctly with a much smaller power.

Extracts are subjoined from a letter of Professor Barlow's to the author, containing formulæ for the construction of the lens.

March 6.—The reading of a paper, entitled, "On the Structure and Functions of tubular and cellular Polypi, and of Ascidiæ." By Joseph Jackson Lister, Esq., F.R.S.—was commenced.

March 13.—The reading of Mr. Lister's paper was resumed and concluded.

This paper contains the account of a great number of observations made by the author during the last summer, while he was at the southern coast of England, on several species of *Sertulariæ, Plumulariæ, Tubulariæ, Campanulariæ, Flustræ,* and other polypiferous zoophytes, and also on various *Ascidiæ.* Each specimen was placed for examination in a glass trough with parallel sides, before the large achromatic microscope of the author, directed horizontally; and care was taken to change the sea-water frequently, which was done by means of two syphons, the one supplying fresh water, while the other carried off the old; a plan which succeeded in keeping the animals in perfect health and vigour. The drawings which were taken of the appearances that presented themselves were traced with a camera-lucida, slid over the eye-piece of the microscope.

In a specimen of the *Tubularia indivisa,* when magnified 100 times, a current of particles was seen within the tube, strikingly resembling, in the steadiness and continuity of its stream, the vegetable circulation in the *Chara.* Its general course was parallel to the slightly spiral lines of irregular spots on the tube; on one side flowing from, and on the other towards, the polypus, each current occupying one half of the circumference of the tube. The particles were of various sizes, some very small, others larger, but apparently aggregations of the smaller: a few were nearly globular, but in general they had no regular shape. At the knots, or contracted parts of the tube, slight vortices were observed in the current; and at the ends of the tube the particles were seen to turn round, and pass over to the other side. Singular fluctuations were also observed in the size of the stomach and of the cavity of the mouth; the one occasionally enlarging, while the other contracted, as if produced by the passage of a fluid from the one into the other and its subsequent recession, thus distending each alternately. This flux and reflux took place regularly at intervals of 80 seconds; besides which two currents were continually flowing, both in the mouth and stomach; an outer one in one direction, and an inner one in the opposite direction.

In all the species of *Sertulariæ* examined by the author, currents of particles were observed passing along the soft substance which occupies the axis of the stem and branches, and were even seen extending into the substance of the polypi themselves, and traversing the stomachs belonging to each. Contrary to what happens in the *Tubularia,* the stream does not, in these animals, flow in the same constant direction; but after moving towards one part for about a minute or two with considerable velocity, it becomes much slower, and then either stops or exhibits irregular eddies, after which it resumes its motion with the same velocity as before, but in the contrary direction; and so on alternately, like the ebb and flow of the tide. If the current be designedly obstructed in any part of the stem, those in the branches go on without interruption, and independently of the rest. It appears from a passage which the author has quoted from Cavolini, that he had noticed the circumstance of currents existing in the interior of *Sertulariæ*, but had not detected their continuation into the stomachs of the expanded polypi. Similar phænomena, which the author describes in detail, were observed in several *Campanulariæ* and *Plumulariæ*; and several particulars are noticed with regard to the ovaries, and to the movements of the fluids contained in the ova of these zoophytes, before their exclusion from the body of the parent. In some cases, the young polype, after it has attained a certain growth, but while still adhering to the parent, becomes decomposed, and, its substance being absorbed into the body of the latter, it entirely disappears. Changes of the same kind frequently take place in different parts of the whole group; one of the polypes being seen to shrink and gradually disappear, while others shoot forth in more luxuriant growth, rapidly acquiring a large size. The author regards the circulating fluids in these animals as the great agent both in the absorption and the growth of parts, and throws out the suggestion, that as it flows into the stomach, it may also act as a solvent to the food received into that cavity. The particles which exist in these fluids show their analogy to those in the blood of the higher animals on the one hand, and to those in the sap of vegetables on the other: some appear to be derived from the digested food, and others from the melting down of parts absorbed. In these polypi the author never saw the least appearance of cilia, or of currents in the surrounding water, which are so frequently met with in other tribes of zoophytes.

The latter part of the paper is occupied by the account which the author gives of his various observations, first, on *Ascidiæ*, of which he enters into an anatomical description; secondly, on the internal currents of water, permeating the branchial sacs, and determined by the vibratory movements of cilia which are seen in those animals; and, thirdly, on the alternations in the course of the circulation of the blood in the vessels, which at one time flows in one direction, and, after a certain interval, takes the contrary course; so that the same vessel which at one time performs the function of an artery, performs, at another, that of a vein. This phænomenon of alternate currents, like that in the *Sertularia*, was met with in every specimen of *Ascidiæ* which was examined by the author, and also in a *Polyclinium*.

The paper concludes with several observations on *Flustræ*, from which, as far as relates to the circulation of currents, the author was led to results in many respects analogous to the preceding.

A paper was then read, entitled, "On the Theory of the Moon." By J. W. Lubbock, Esq., V.P. and Treasurer of the Royal Society.

The author, adverting to the appearance of M. Plana's admirable work entitled *Théorie du Mouvement de la Lune*, enters into a comparison of the analytical methods employed by that author and M. Damoiseau, and points out some differences in the numerical values of the coefficients of some of the arguments in the expression for the true longitude of the moon in terms of her mean longitude. He then prosecutes the subject by a series of analytical investigations, which are not susceptible of abridgement, but from which he obtains formulæ which do not quite agree with those of M. Plana.

A paper was also read, entitled, "Some Suggestions relative to the best method of employing the new Zenith Telescope lately erected at the Royal Observatory." By John Pond, Esq., F.R.S., Astronomer Royal.

During the observations made by the author, in the course of last summer, with the new zenith telescope lately erected at the Royal Observatory, for the purpose of measuring the zenith distance of γ Draconis, it occurred to the author to avail himself of subsidiary observations on another star, of about the fifth magnitude, which has nearly the same zenith distance towards the south that γ Draconis has towards the north, and which passes the meridian between 20 and 30 minutes, in time, after it. The angular distance between the two stars being determined in the usual manner, by observing them on the same night, and in the same position of the instrument, gives the *sum* of their zenith distances: and if on the next or some following night γ Draconis be observed, and after its passage the instrument be turned half round, and the other star observed, then the difference of the measure, as read on the micrometer, will be the *difference* of the zenith distances of the two stars. These sums and differences, thus ascertained on different nights, will be independent of any change that may happen to the instrument in the interval. This method affords the means of determining, with almost unlimited precision, the value of the small equations which become the subject of investigation in the employment of the instrument. Thus all changes of the position of the stars, occasioned by aberration, nutation, &c., will produce double the effect on the small differential or subsidiary angles, as measured by this method. For the investigations of these small equations it will not be necessary to have determined either the exact zenith distance of each star, or the exact difference of their zenith distances, or the absolute magnitude of this subsidiary angle; its variation from time to time being the only important object of research. The author is led to expect that this property may, at some future period, be applied with advantage in investigations made with moveable zenith instruments.

March 20.—A paper was read, entitled, "Narrative of the Proceedings of Commander Thomas Dickinson, of His Majesty's Sloop Lightning, while employed in the Enterprise for the Recovery of the Public Stores and other property sunk in His Majesty's late Frigate Thetis, on the south-west side of the Island of Cape Frio." By Commander Thomas Dickinson, R.N. Communicated by P. M. Roget, M.D., and J. G. Children, Esq., Secretaries to the Royal Society. It was preceded by the reading of a letter from the author to the Secretary, explaining the reasons which induced him to lay this narrative before the Royal Society, and place on the records of its proceedings the information it contains relative to the commencement of an enterprise, wholly planned and undertaken by himself, and which, under his superintendence, was, by the great, persevering and meritorious exertions of his officers and crew, most successfully accomplished.

The narrative commences with the statement of the consternation produced at Rio de Janeiro on the receipt of the intelligence of the loss of the Thetis, with a freight of about 810,000 dollars, on the south-west side of the island of Cape Frio, and of the determination of the author, on finding that no one seemed disposed to take any step towards the recovery of the property thus lost, to make the attempt himself, if he could obtain from the Commander-in-chief at that station, Rear-Admiral Thomas Baker, C.B., orders to that effect. He accordingly exerted himself to obtain every possible information relative to the nature of the coast, depth of water, and other circumstances, which might enable him to judge of the practicability of the undertaking, and of the means necessary for its successful accomplishment; and became convinced that the difficulties and obstacles to be encountered, although numerous and formidable, might be overcome by the employment of the means which suggested themselves to him as practicable on this occasion, if sufficient assistance were afforded him in putting them into execution. He accordingly had models of the proposed machinery made, and submitted them, together with his plans, to the Commander-in-chief, by whom they were approved. He experienced great difficulties in procuring a suitable diving-bell, for it was impossible to obtain any instrument of the kind at Rio de Janeiro, or even any facilities for the construction of one by casting. It at length occurred to him that a ship's iron water-tank might be converted to this use; and being supplied with one from the Warspite, he was enabled to render it available for that purpose. The next difficulty was to procure an air-pump, which, after much delay, owing to the tardiness of the native workmen in that country, was at length constructed. The want of air-hoses, however, was a still more formidable obstacle to the success of the plan; but the ingenious contrivances of the author for rendering the common pump hoses air-tight, supplied this deficiency; and on a trial which he made with the whole apparatus on the 22nd of January, 1831, it was found to answer completely. The next day he received his orders from the Commander-in-chief, and, sailing on the following day, arrived at the harbour of Cape Frio on the 30th, and immediately proceeded to inspect the coast, and ascertain the situation of the wreck, not a vestige of which was visible. An account is then given of the local circumstances of the Thetis Cove, or inlet, surrounded by almost perpendicular cliffs from 108 to 194 feet in height, with a depth of water varying from $3\frac{1}{2}$ to 24 fathoms, and the bottom being strewed with huge perpendicular rocks, occasioning these inequalities. These surveys showed that the execution of the plan originally conceived by the author was opposed by so many unforeseen difficulties, that he was obliged to relinquish some parts of it, and resort to fresh expedients for surmounting them. The idea of constructing a derrick then occurred to him; but the materials were wanting, for no trees existed in the island except those in the forests in the interior, which were inaccessible from their distance and the heights on which they grew, and of which the wood was, from its quality, unsuitable to the purpose. His only resource, therefore, was to make it of the fragments of spars saved from the wreck. With great exertions, a circumstantial account of which is given in the paper, the work was at length accomplished; and the result fully equalled the anticipations which had been formed of its utility in affording a stable point of support for the operations with the diving-bell. Previously to the erection of a derrick, however, much had been done by working the diving-bell from a boat, and a considerable quantity of stores and treasure raised. At one time the anxiety of the author to forward the undertaking, and avail himself of favourable weather, induced him to try the experiment of working by torch-light, which succeeded to a certain extent; but after a few trials the danger was found to be excessive, and the fatigue to the divers so great as to oblige him to desist.

After the derrick had been for some time in operation, a tremendous sea arose, the shock of which, for want of sufficient materials to support it, effected its destruction; and a substitute was then resorted to by the setting up of a suspension cable diagonally from the cliffs, which, after great difficulties, was at length effected.

A great portion of the narrative is occupied with the details of the various proceedings, and of the serious impediments which were successively overcome by the zeal, perseverance and extraordinary exertions of the officers and crew, under the orders of Captain Dickinson, subjected as they were, for so long a period, to the greatest privations and hardships, arising from the laborious nature of the work, the unhealthiness of the climate, the attacks of the chigger, producing distressing ulcers in the feet, the annoyance from drifting sand, which penetrated into every place, the exposure to constant wet in huts which could not be made to exclude either wind or rain, and the perils arising from the boisterous gales and tremendous swell of the sea, which the whole ship's company, but more particularly the men in the diving-bell, had to encounter; forming a combination of difficulties which the author is convinced could have been surmounted by none but British seamen.

After having succeeded so far in the undertaking, and made, at various times, shipments for England of treasure amounting to about three fourths of the whole which had been on board the Thetis when she sunk, orders were received by the author, on the 6th of March, to resign the charge of the enterprise to the Hon. Capt. De Roos, of His Majesty's brig Algerine; on the receipt of which he immediately ordered a survey to be taken of the stores, and on the 9th descended in the bell, surveyed the bottom of the Cove, ascertained the position of the remaining stores and a considerable quantity of treasure; and after having communicated the whole of the results to Captain de Roos, instructed him, his officers and ship's company in the way of working the bell, as well as in the different modes of removing rocks, recovering stores and treasure, and the use of the whole of the machinery, and furnished him with every necessary information for his guidance, he lent twenty of his men to the Algerine for their assistance, resigned the charge to his direction on the 10th, and sailed for Rio de Janeiro on the 13th.

Annexed to the paper is a journal of the amount of treasure of various descriptions recovered between the 31st of March, 1831, and the 10th of March, 1832, by His Majesty's sloop Lightning.

There was then read an extract of the letter of instructions, bearing date the 10th of March, 1832, from Commander Thomas Dickinson, then of His Majesty's ship Lightning, to Commander the Hon. S. F. de Roos, then of His Majesty's brig Algerine, on the former resigning to the latter the charge and direction of the enterprise for the recovery of the public stores and treasures sunk in His Majesty's late frigate Thetis, off Cape Frio.

The Society then adjourned over the Easter Recess, to meet again on the 10th of April.

GEOLOGICAL SOCIETY.

Jan. 22.—A paper was read "On the Structure and Classification of the Transition Rocks of Shropshire, Herefordshire and part of Wales, and on the Lines of Disturbance which have affected that Series of Deposits, including the Valley of Elevation of Woolhope," by Roderick Impey Murchison, Esq., F.R.S., F.G.S., &c.

I. Another summer's work, during which the author revisited Shropshire and the Welsh counties, formerly described, and also examined the eastern side of Herefordshire, with portions of Monmouth, Gloucester, Worcester and Staffordshires, has enabled him to lay before the Geological Society much more copious information respecting the "Transition rocks," or "Fossiliferous Grauwacké" of this quarter of Great Britain; and to subdivide the same into formations.

The following classification is, therefore, substituted for that proposed last year, being founded upon more extensive observation and an increased knowledge of the organic remains and of the order of superposition.

1. *Ludlow Rocks.*—Commencing beneath the old red sandstone, described in the memoir read before the Society, January 8th*, he names the superior formation of the grauwacké series, the "Ludlow rocks." This sandy, argillaceous deposit, has in its centre, the zone of limestone well known at Aymestry, Downton on the Rock and other places, by its containing in abundance the *Pentamerus Knightii*. It is stated that the black limestone of Sedgeley, near Dudley in Staffordshire, is identical in character and in organic remains with that of Aymestry in Herefordshire, and the Yeo edge in Shropshire, and that this calcareous zone is everywhere separated from the Wenlock and Dudley limestone by a thick deposit of shale and flag, to which the author has assigned the name of "Lower Ludlow rock". For the chief characters and order of superposition of the groups of this formation and all those of the descending series, the reader is referred to the annexed tabular view, in anticipation of details and illustrations which will, at some future time, be laid before the public. The number of unpublished organic remains

* See this Journal for March, p. 228.

Ch Some Localities.

differing in specin, south end of Clee Hills, and Llanymynech, Shrop-
idstone. Productth Wales Coal-basin.
c. (Defence and t

o organic remains cans, S.E. part of Black Forest, Brecknockshire: flanks
hropshire.
Crustacea of undescril of Herefordshire: eastern part of Brecknockshire:
nd base of the Clee Hills, Shropshire: Tenbury and
inster, Worcestershire.
Avicula, n. s. Pileo ermarthenshire: Clyro Hills, Brecknockshire: Tin-
rulites? Castle, Herefordshire: Clun Forest, Shropshire.

UPPER GRAUWACKE SERIES.

Avicula, n. s. A. retunslow, Diddlebury, Larden, Berriew-ditches, Shrop-
cardia, n. s. Homonc, Kington, Fownhope, Stoke Edith, Herefordshire:
V. Buch. Orthis, sevd Abberley Hills, Worcestershire: west flank of May
cera, several new sperrshire: Presteigne, Stow Hill, Radnor Forest, Pain's
Gigantic serpuline berne Hills, Corn-y-fan, Brecon, Llanbadock near Usk.
Pentamerus Knightii, Gatley, Brindgwood Chase, Downton on the Rock,
Lingula, n.s. Atrype, Shelderton, Norton Camp, Dinchope, Caynham
fibrosa, Goldf., and ley, Staffordshire.
Phragmoceras, new gand Brindgwood Chase, Gatley, and valley of Wool-
Ichthydorulites? smaary Knoll Dingle, Westhope, Hopedale, and Long
tilus, n. s. Spirulitesst side of Abberley and Malvern Hills; escarpments
Pleurotomaria, n. s. Brecknock and Caermarthenshires.

Corals and Crinoidea iWenlock Edge, Shropshire: Burrington, Nether Lye,
Euomphalus rugosus Presteigne, Old Radnor: Pwll-Calch, Caermarthen-
Pentamerus, n. s. N, Ledbury, and west side of Malvern Hills: east side
Dalman. Spirifer lin, Worcestershire: Long Hope, near May Hill, Glou-
Producta depressa, Cil-na-Caya, near Usk.
caudatus. Calymene
As. caudatus variety, nstow and Clungunford, Salop: escarpments in Mont-
and others. Cyrtia tock and Caermarthenshires: west flank of Malvern
n. s. O. annulata, Mhire: centre of Wren's Nest, Dudley, &c. &c.

Pentamerus lævis, M.Iorderley, Acton Burnell, Chatwall: the Hollies near
n. s. Orthis Callactis,ongville, Acton Scott: east flank of Wrekin and Caer
rebratula, n. s. Park, Obelisk, and centre of Woolhope Valley, Here-
ntaculites and Crinoidlucestershire.
Nucula, n. s. Pentaong Lane, and Corston, Shropshire: Ankerdine Hill,
14 species of the geleath, S.W. of Malvern Hills, Worcestershire: May
ratus, Dalm., all diffd the same localities as i in Shropshire: Guilsfield
omeryshire: Castell Craig Gwyddon, Caermarthen-

Asaphus Buchii. Agthropshire: Llandrindod and Wellfield, near Builth,
species; differing fro to Llandeilo, Caermarthenshire.

No organic remains hlaughmond, Lyth, Pulverbatch Hills, Salop: Gwas-
Brecon, &c. &c.: hills west of Llandovery, Caermar-

N.B. oids? have been foundaly matter, beyond small nests of Anthracite. In the
list of affinis, M. C. (Atrypa s short Table, but will be given hereafter in a full and
descrip niferous limestone.

(Wrekin, Caer Caradc

Table of the stratified Deposits beneath the Coal-measures in the Counties of Hereford, Salop, Montgomery, Radnor, Brecknock, Caermarthen, Monmouth, Worcester, Stafford and Gloucester.

Phil. Mag. and Journal, N.S. May 1834.

DESCENDING ORDER.

[To face page 370.

	Formations.	Maximum approximate thickness.	Subdivisions.	Lithological Characters.	Characteristic Organic Remains.	Some Localities.
	Carboniferous limestone.	Feet. 500?	Shale. Limestone. Shale.		Corals differing in species from those of the formations below the old red sandstone. Producta hemispherica. P. Martini. Spirifer triangularis, &c. (Defence and teeth of fishes. Clee Hill, Salop.)	Lilleshall, Steeraways, Orleton, south end of Clee Hills, and Llanymynech, Shropshire. The edge of the South Wales Coal-basin.
	Old red sandstone.	10,000	a Red conglomerate and sandstone.	a Quartzose conglomerate overlying thick-bedded sandstones ...	a No organic remains observed ...	a. Caermarthen and Brecon Fans, S.E. part of Black Forest, Brecknockshire: flanks of the Brown Clee Hill, Shropshire.
			b Cornstone and argillaceous marls.	b Red and green, concretionary limestones, with spotted argillaceous marls and beds of sandstone.	b Crustacea of undescribed genera ...	b. Central and northern parts of Herefordshire: eastern part of Brecknockshire. Whitbach near Ludlow, and base of the Clee Hills, Shropshire: Tenbury and Shatterford, near Kidderminster, Worcestershire.
			c Tile stone	c. Flaggy, highly micaceous, hard, red and green sandstone ...	c. Avicula, n. s. Pileopsis, n. s. Small Orthocera. Small Ichthyodorulites?	c. Pontarlleche, Cwmdwr, Caermarthenshire: Clyro Hills, Brecknockshire: Tinmill Copse, near Downton Castle, Herefordshire: Clun Forest, Shropshire.
UPPER GRAUWACKE SERIES	I. Ludlow rocks.	2000	d. Upper Ludlow rock.	d. Slightly micaceous, grey-coloured, thin-bedded sandstone...	d. Avicula, n. s. A. retroflexa, Hisinger. Atrypa (Dalman), n. s. Cypricardia, n. s. Homonolotus Knightii, new genus (Konig.) Leptæna lata, V. Buch. Orthis, several new species. Orbicula, 2 new species. Orthocera, several new species. Pleurotomaria? 2 new species. Turbo, n. s. Gigantic serpuline bodies, &c. &c.	d. Ludlow Castle, Whitcliffe, Munslow, Diddlebury, Larden, Berriew-ditches, Shropshire: Croft Castle, Titley, Kington, Fownhope, Stoke Edith, Herefordshire: West flanks of Malvern and Abberley Hills, Worcestershire: west flank of May Hill, Tortworth, Gloucestershire: Presteigne, Stow Hill, Radnor Forest, Pain's Castle, Radnorshire: Treweme Hills, Corn-y-fan, Brecon, Llanbadock near Usk.
			e Aymestry and Sedgeley Limestone.	e Subcrystalline or gray and blue argillaceous limestone ...	e Pentamerus Knightii, M. C. Pileopsis vetusta, M. C. Bellerophon, n. s. Lingula, n. s. Atrypa, n. s. Terebratula Wilsoni, M. C. Calamopora fibrosa, Goldf., and a few other corals.	e Aymestry, Croft Ambry, Gatley, Brindgwood Chase, Downton on the Rock, Herefordshire: Yeo Edge, Shelderton, Norton Camp, Dinchope, Caynham Camp, Shropshire: Sedgeley, Staffordshire.
			f Lower Ludlow rock.	f Sandy, liver and dark-coloured shale and flag, with concretions of earthy limestone.	f Phragmoceras, new genus, (Broderip,) 3 species. Asaphus caudatus. Ichthydorulites? small. "Cardiola," Brod., a new genus, 2 sp. Nautilus, n. s. Spirulites, 2 n. s. Pentamerus. (Atrypa galeata, Dalm.) Pleurotomaria, n. s. Orthocera pyriformis, n. s. and several others.	f Escarpments of Mocktree and Brindgwood Chase, Gatley, and valley of Woolhope, Herefordshire: Mary Knoll Dingle, Westhope, Hopedale, and Long Mountain, Shropshire: west side of Abberley and Malvern Hills; escarpments in Montgomery, Radnor, Brecknock and Caermarthenshires.
	II. Wenlock and Dudley rocks.	1800.	g Wenlock and Dudley limestone.	g Highly concretionary gray and blue subcrystalline limestone ...	g. Corals and Crinoidea in vast abundance. Bellerophon tenuifascia, M. C. Euomphalus rugosus. Eu. discors. Conularia quadrisulcata, M. C. Pentamerus, n. s. Natica, n. s. N. spirata, M. C. Leptæna euglypha, Dalman. Spirifer lineatus, M. C. S. n. s. Terebratula cuneata, Dalm. Producta depressa, M. C. Orthocera, several species. Asaphus caudatus. Calymene Blumenbachii. The Bar Trilobite and others.	g. Lincoln Hill, Benthall and Wenlock Edge, Shropshire: Barrington, Nether Lye, near Amestry, Nash, near Presteigne, Old Radnor: Pwll-Calch, Caermarthenshire: valley of Woolhope, Ledbury, and west side of Malvern Hills: east side of Abberley Hills, Dudley, Worcestershire: Long Hope, near May Hill, Gloucestershire: Prescoed and Cil-na-Caya, near Usk.
			h. Wenlock and Dudley shale.	h Argillaceous shale, liver and dark gray coloured, rarely micaceous, with nodules of earthy limestone.	h. As caudatus variety, C. Blumenbachii. Lingula, n. s. Orthis, n. s., and others. Cyrtia trapezoidalis, Dalm. Delthyris, n. s. Orthocera, n. s. O. annulata, M. C. Crinoidea, &c.	h. Buildwas, Hughley, Wistanstow and Clungunford, Salop: escarpments in Montgomery, Radnor, Brecknock and Caermarthenshires: west flank of Malvern Hills, Alfrick, Worcestershire: centre of Wren's Nest, Dudley, &c. &c.
	III. Horderley and May Hill rocks.	2500.	i. Flags.	i. Thin-bedded, impure, shelly limestone, and finely laminated, slightly micaceous, greenish sandstone.	i. Pentamerus lævis, M. C. P. oblongus, n. s. Leptæna, n. s. Pileopsis, n. s. Orthis Callactis, Dalm., and several new species. Terebratula, n. s. Tentaculites and Crinoidea, abundant, } Corals rare.	i. Banks of the Onny, near Horderley, Acton Burnell, Chatwall: the Hollies near Hope Bowdler, Cheney Longville, Acton Scott: east flank of Wrekin and Caer Caradoc, Salop: Eastnor Park, Obelisk, and centre of Woolhope Valley, Herefordshire: May Hill, Gloucestershire.
			k. Sandstones, grits, and limestones.	k*. Thick-bedded, red, purple, green, and white freestones. Conglomeritic quartzose grits. Sandy and gritty limestones.	k. Nucula, n. s. Pentamerus, n. s. Trilobites of undescribed species, and 11 species of the genus Orthis have been found, including O. aperturatus, Dalm., all differing from those of the overlying formations.	k. Horderley, Hoar Edge, Long Lane, and Corston, Shropshire: Ankerdine Hill, Old Storridge, Howler Heath, S.W. of Malvern Hills, Worcestershire: May Hill, Gloucestershire: and the same localities as i in Shropshire: Guilsfield and Alt-y-maen, Montgomeryshire: Castell Craig Gwyddon, Caermarthenshire.
	IV. Builth and Llandeilo flags.	1200.		l. Dark-coloured flags, mostly calcareous, with some sandstone and schist.	l. Asaphus Buchii. Agnostus, Brongn., undescribed Trilobites of three species, differing from those of the overlying formations.	l. Rorrington, near Shelve, Shropshire: Llandrindod and Wellfield, near Builth, Radnorshire: Tan-yr-Allt to Llandeilo, Caermarthenshire.
	V. Longmynd and Gwastaden rocks.	Many thousand feet.		m. Hard, close-grained, grey, greenish and purple sandstone. Red and grey quartzose conglomerate. Slate-coloured and purple schists. Coarse slates: little or no calcareous matter.	l. No organic remains have yet been observed in this great system.	m. The Longmynd, Linley, Haughmond, Lyth, Pulverbatch Hills, Salop: Gwastaden, east of Rhayader, Brecon, &c. &c.: hills west of Llandovery, Caermarthenshire.

N. B. No vegetable remains, except the *Fucoides serra?* (Brongn.), Syn. *Graphilitis sagittarius*, (Korch.) and some very imperfect fragments of Fucoids? have been found in any portion of the deposits below the carboniferous limestone, nor has any coaly matter, beyond small nests of Anthracite. In the list of organic remains, only such individuals have been mentioned as are characteristic of each subdivision. Others, as for example the *Terebratula affinis*, M. C. (*Atrypa reticularis*, Wahl.), which occurs in several formations, have been omitted in this short Table, but will be given hereafter in a full and descriptive account of all the organic remains. None of the species of corals or shells of the grauwacke series, are identical with those found in the carboniferous limestone.

* The sandstones (*i* and *k*) pass into quartz rock in the vicinity of certain trap rocks (Wrekin, Caer Caradoc, Bluen Dyffrin garn, &c.), as will be explained in a subsequent Memoir.

in the formation is considerable, including a new genus of Crustaceans, another of Cephalopods, and a third of Conchifers.

2. *Wenlock and Dudley Rocks.*—The coralline limestones of Wenlock and Dudley having been shown to be equivalent, the term of "Wenlock and Dudley rocks" has been adopted, that the name of the last-mentioned place, so well-known for its organic remains, may serve to mark a zone in the Geological Series.

The lower part of this formation is termed the "Wenlock shale," the local term of "Die earth," used last year, having been abandoned by the author, because he finds that it has been applied by the miners to any stratum of the transition series upon which their coal bearing measures have been deposited; so that in one case their "Die earth" is the lower shale of the Ludlow formation, in another the shale beneath the Wenlock limestone.

3. *Horderley and May Hill Rocks.*—This formation, consisting of shelly sandstones, impure limestones, grits, &c., having been more thoroughly examined, is found to contain many more organic remains, and to be of much greater thickness than was formerly supposed.

The strata on the banks of the river Onny at Horderley, Salop, are described in detail, as they present a full type of the formation. The name of May Hill in Gloucestershire, is added to that of Horderley, because in that well known hill, several members of the formation (particularly the red and shelly sandstone) are well exhibited.

4. *Builth and Llandeilo Flags.*—These flags, so peculiarly marked by the presence of the large *Asaphus Buchii*, and so fully developed in Caermarthen and Brecknockshires, have this year been discovered at Rorington in the west of Shropshire.

5. *Longmynd and Gwastaden Rocks.*—These constitute a great mineral axis in Shropshire, occupying the hilly region of the Longmynd, Linley, the Stiper Stones, &c.; extending to the north-east in Lyth and Haughmond Hills, and throwing off upon their vertical flanks, the overlying formations.

These rocks are again found in the same geological horizon at Gwastaden in Brecknockshire, and also west of Llandovery in Caermarthenshire, where the siliceous and gritty members are frequently in the form of concretions. The whole of this vast system (No. 5) is void of organic remains.

Each of the four fossiliferous formations above enumerated, is distinguishable by an individuality of character of the organic remains, by lithological structure, and by geographical boundaries; whilst the underlying slaty and conglomeritic system is perfectly dissimilar in aspect and composition from any of the overlying groups. These distinctions are explained in the annexed Table. The author however states, that in Brecknock and Caermarthenshires it is very difficult to draw those neat lines of separation between these formations, which nature has established in Salop, Herefordshire, &c.; and the calcareous members being almost entirely absent in South Wales, it is only in certain places that these subdivisions can be defined. In addition to this, the several formations thin out so rapidly in their course to the south-west, that beyond Llandeilo, the Ludlow and Wenlock formations appear in the same escarpment, and in like manner on the left bank of the Towey, between Llandeilo and Llangadock, it is difficult to separate the shelly sandstones of the third formation from the black flags of Llandeilo, with which they are associated; the great fossiliferous system which in Shropshire, Hereford, Radnor, &c. is expanded over so wide an area, being there compressed into a narrow zone between the old red sandstone on the one side and the rocks of roofing-slate on the other.

II. The next chapter describes such of the above-mentioned formations as occur in the Abberley Hills, upon the flanks of the Malvern Hills, Herefordshire, in the vicinity of Usk, Monmouthshire, or in May and Huntley Hills, Gloucestershire, &c. It is stated that Nos. 1 and 2, or the Ludlow and Wenlock formations, are much the most persistent, usually occupying two distinct ridges, as in Shropshire, though on a smaller scale. The impure limestone at the base of the Wenlock shale and constituting the top of the shelly sandstones is also strongly marked by its peculiar characters and organic remains. The arenaceous or great mass of this third formation is brought out from beneath the others at certain points only, as at Ankerdine and Old Storridge Hills, and at Howlers Heath, Worcestershire. May Hill, in Gloucestershire, is cited as a good type of the formation, where it is also overlaid by the superior deposits.

III. *Woolhope Valley of Elevation.*—A valley of elevation which the author conceives to be the most symmetrical in Great Britain, is then pointed out as occurring south-east of Hereford, where the two superior formations of the grauwacké series are incurvated round a central dome-shaped mass, composed of the shelly sandstones of the third formation, from which the strata dip away on all sides at angles varying from 15° to 70°. The harder strata of each formation having resisted destruction, whilst the shales have been worn away, the former constitute the higher encircling ridges, the latter deep trenches of intervallation. The outer zone contains all the fossils characteristic of the Ludlow rocks, and passes beneath the old red sandstone; the inner zone, those of the coralline formation of Wenlock and Dudley, and both these are wrapped round a nucleus of the third formation. The outer zone is unbroken by any transverse gorge, throughout two thirds of its circumference; but at Mordiford it is violently dislocated, and the result has been a chasm, by which and by two minor fissures, the valley is entirely drained. The whole of the valley is stated to be one of clean denudation, being entirely free from any fragments, even of the old red sandstone, though the inferior and denuded strata must have been raised up through that formation. The author calls attention to the south-eastern apex of this valley, where the encircling formations successively become confluent, and the line of the axis of elevation in its prolongation to the south-east being alone marked by a thin ridge of the Ludlow formation, bears the same geographical relations to the ovoidal mass, as the tail of a school-boy's kite does to its body. This phænomenon he will hereafter point out as occurring in other valleys of elevation of similar epoch.

This line of elevation trending from north-west to south-east, is stated to be prolonged through May and Huntley Hills to Flaxley in Gloucestershire, where the three superior grauwacké formations being reproduced, a separate description of them is annexed.

Mr. Maclauchlan had previously laid down upon the map of the Ordnance Survey a correct outline of this mass of transition rocks, in its general relations to the old red sandstone.

IV. *Lines of Direction and Dislocations.*—The author here referred to the sheets of the Ordnance Survey on which he had laid down numerous details illustrative of the outlines of each formation, and of the direction and dislocation of the strata.

The prevailing strike of all the deposits described, is from north-east to south-west, as indicated by the line of junction of the Ludlow rocks with the old red sandstone in the counties of Salop, Hereford, Brecknock and Caermarthen. The western limits of these counties, together with those of Montgomery and Radnorshires, exhibit the same direction of the strata. This tract is about 100 miles in length from Lilleshall Hill and Wellington on the north-east to the mouth of the Towey on the south-west, and has a breadth of from 30 to 40 miles. Within this space there are numerous minor axes of elevation, which are only traceable for short distances on the strike, but they are all parallel to each other, and subordinate to the same great line of elevatory movement. These are for the most part marked by eruptive ridges of trap rock, which tilt the strata upon their flanks both to the north-west and to the south-east; and wherever such parallel outbursts are numerous, as between the Wenlock edge and the river Vierniew in Montgomeryshire, they occasion the folding over and repetition of strata of the same age, far to the north-west of their regular line of bearing, upon the confines of Shropshire and Herefordshire. Wherever on the contrary, the longitudinal influence of these short outbursts has ceased, the younger formations, or those of Ludlow and Wenlock, overlie in slightly disturbed positions, the vertical and dislocated beds of the older groups. Hence it is, that the old red sandstone and the Ludlow rocks occupy so large and detached an area on the west, as in Clun, Knighton and Radnor forests.

The line of elevation of which the trap rocks of Old Radnor mark the center, terminates on the north-east in the valley of elevation of Wigmore, having the promontory of Ludlow as its apex; and on the south-west in the narrow anticlinal ridge of Corn-y-fan near Brecknock. The trap rocks extending from Llandrindod to Builth, and other ridges of porphyritic rocks discovered by the author in Brecknock and Caermarthenshires (hereafter to be described), occasion similar, short anticlinal lines all trending from north-east to south-west. This general line of direction is broken through transversely by many cracks and fissures, some of which have been the scene of great dislocation, and it is shown, that through such rents in the strata, the principal rivers escape from the mountains of Wales into the lower counties of England, viz., the Severn, the Onny, the Teme, the Lug, the Wye, &c.

On the eastern side of the trough of old red sandstone of Herefordshire, the mean direction of the strata, as determined by the outline of the trap and sienitic ridges of Abberley and Malvern, is from north to south. But there are many aberrations from that direction, and innumerable local disturbances, curvatures and faults. Thus, for example, the superior grauwacké formations strike south-south-east from Knightwick bridge upon the Teme for a distance of six miles, until they are met by the sienitic ridge of the Malvern running due north and south. The result of this contact is, that the sedimentary deposits are cut out, deflected from their course, and their direction accommodated to the western sides and promontories of the intrusive rock. In the neighbourhood of Eastnor Park three of the grauwacké formations have the north-east and south-westerly strike so persistent in Salop and Wales; but this direction is merely local, being only maintained in a length of about $2\frac{1}{2}$ miles, for the Ledbury ridge, which terminates this group, is seen to strike due south at its apex near Clencher's Mill. The discrepancy is still greater between the strike of the major axis of the Woolhope Valley extending to Flaxley in Gloucestershire (a distance of about 18 miles), and that of Shucknell Hill, which although only two miles distant from the northern end of the Woolhope Valley, and composed of the same rocks, has a direction from south-west to north-east, and at right angles to the former, which runs from north-west to south-east. The strike of the strata of Shucknell Hill is parallel to the line of bearing of the adjoining trap rocks of Bartestree.

Notwithstanding the numerous data explanatory of these divergencies in the direction of the strata of the same age on the east side of Herefordshire, and in other parts an occasional coincidence of parallelism between the strike of formations of very different age, the author declines for the present to enter upon the general theoretical question put forth by M. Elie de Beaumont, conceiving that the scale upon which he has observed may by some geologists not be considered sufficiently expansive to enable him fairly to discuss the merits of that theory. It might also be contended that the phænomena apparent on the eastern side of Herefordshire have been simply the offsets of those stupendous forces to which the mountain chains of Wales owe their origin, and that such small exceptions could not vitiate a train of reasoning deduced from the phænomena observed throughout a whole mountainous region.

Intending to pursue this inquiry, he restricts himself, on this occasion, to the statement of the fact, that the carboniferous limestone and coal-measures near Wellington, and in the Titterstone Clee Hills where these deposits are penetrated by basalt, have been thrown up into the same north-east and south-west direction as the grauwacké series of Salop and Wales; whilst in the Abberley

Hills, and at Dudley in Staffordshire, a southerly movement has affected both the transition rocks and the coal-measures.

This part of the memoir was concluded by the notice of a very remarkable case of dislocation, amounting to an *entire reversal of the two younger formations of the grauwacké series* along a distance of several miles upon the flanks of the Abberley Hills. In this tract the Lower Ludlow overlies the Upper Ludlow rock, at angles varying from 70° to 45°; and the adjacent parallel ridge of the Wenlock limestone is conformably tilted over, giving the appearance of the Ludlow rocks passing beneath the older formation. This phænomenon is supposed to have been brought about by the outburst of the contiguous trappean hills of Abberley and Woodbury, the elevating forces accompanying which, it is conceived, have bent them back upon their axes, and produced their present inverted position. This mode of explanation, it is stated, is rendered conclusive on tracing the same sedimentary groups to those points where, from their south-south-easterly direction they impinge upon the sienite of the Malvern chain, and where the same phænomenon of reversal, on a small scale, is again apparent. Here the strata near the sienite are bent backwards; but those which are removed from it not having been disturbed in so great a degree, incline towards the west, the Wenlock and Dudley limestone dipping beneath the exterior and upper zone of Ludlow rocks.

In further illustration of the country of which he has undertaken the review, the author announced as future communications—

1st, A special notice of the Ludlow and Wenlock formations as they appear at Sedgeley and Dudley in Staffordshire, and of their relations to the coal-measures of that district.

2ndly, A sketch of the gravel and alluvial deposits of Herefordshire and the surrounding counties.

3rdly, On certain new lines of demarcation which he has established between the old and new red sandstones in Shropshire, Worcestershire, Hereford and Gloucestershire; with some observations on the coal-fields of Pensax, Billingsley, Deuxhill and Sherlot in Salop.

4thly, An account of the trap rocks in the country described, including basalts, green-stones, porphyries and sienites, and of the effects produced at their points of eruption through the sedimentary deposits.

Feb. 5.—A paper was first read "On some of the Faults which affect the Coal-field of Coalbrookdale," by Joseph Prestwich, jun., Esq., F.G.S.

In this communication the author confines his observations almost entirely to the direction of the principal faults, and to the changes which they have produced in the relative position of the beds of coal; and he refers to the memoirs of Mr. Murchison for an account of the formations on which the coal-measures repose.

The author concludes his memoir with some observations on the fossils he procured principally from the ironstone of these coal-measures. Of 18 genera of shells which he enumerates, 12 are marine. The lower layers of the ironstone nodules contain, in general, the greatest number of shells, and the upper the greatest number of plants; but the bed called the "Chance-penny ironstone," the highest wrought, contains the greatest abundance of a species of Productus. The most remarkable fossils obtained by the author, are the remains of Trilobites hitherto undescribed. He procured them from a bed of ironstone in the centre of the coal-measures. He notices also a Coleopterous insect, and another apparently belonging to the genus Aranea, in the possession of Mr. Antice of Madeley, and which were obtained from ironstone nodules.

A paper was then read, entitled, "Notes on the Forest of Wyre Coal-field," by the Rev. Thomas England, F.G.S.

The district, described in this memoir, is bounded on the east by the Severn, on the north by the coal-field of Coalbrookdale, on the west by the Rea and the Hopton to their junction with the Teme, and on the south by the latter river and the Abberley Hills.

A paper was lastly read "On a Freshwater Formation containing Lignite in Cerdagne in the Pyrenees," by Charles Lyell, Esq., Foreign Sec. G.S.

The upper part of the basin of the river Segre in Cerdagne presents an example, rare in the Pyrenees, of a great longitudinal valley running east and west, or nearly parallel to the axis of the chain. This basin is formed by a depression in the central region of granitic rocks, which, in the eastern division of the Pyrenees, is of considerable breadth. The lacustrine strata occupy the lower parts of the depression, reposing horizontally on the granite, hornblende schist and argillaceous schist. The breadth of the freshwater formation is about five miles, and its elevation above the sea probably between three and four thousand feet. Its eastern limits are seen to the eastward of Livia, where the boundary is formed by the ridge of granite from which the head waters of the Segre descend. At this outcrop the freshwater clays are seen to be covered with beds of such gravel as might now be supplied from the waste of the surrounding mountains of granite and schist. On crossing the ridge of granite from the basin of Segre to that of the Têt, the author found no recurrence of the freshwater strata in the valley of the last-mentioned river. The northern outcrop of the lacustrine deposit is well seen at Ur, between Porté and Puycerda, where the strata consist chiefly of coarse gravel resting on highly inclined hornblende schist. The deposit is for the most part composed of variously coloured clays, often laminated, in which shells of the genera Limneus and Planorbis abound, as at Estavan, near Livia, in French Cerdagne, where lignite has been worked, and where there are bituminous clays containing impressions of plants. Lignite is still procured from pits at Prats, near Senabastre, in Spanish Cerdagne.

The author offers no opinion as to the tertiary epoch to which the formation may belong, for the shells obtained at Estavan, although entire, were too much flattened to allow M. Deshayes to determine the species. As some portions of the freshwater beds, especially those to the eastward, are highly elevated above others, it appears that the country has undergone great geographical changes since this part of Cerdagne was a lake.

The position of the tertiary basin of Cerdagne was illustrated by two transverse sections across the Pyrenees. The memoir concluded with some remarks on the signs of obliterated lakes which abound in the valleys of the Pyrenees at a great variety of levels.

The first of the sections alluded to, extended from Pamiers south of Toulouse through the highest part of the Pyrenees to Puycerda in Spain. Beds of a conglomerate (*a*), are seen on the river Arriege near Verhilles, inclined to the south; and next in a succession are limestone strata much contorted, containing nummulites and other fossils (*b*); then a formation of grit, sandstone and coal, (*c*). In passing from Verhilles to Foix, the groups *b* and *c* appear to be more than once repeated in consequence of the derangement of the strata. In the remaining part of the section between Ussat and Puycerda, the rocks consist chiefly of granitic and argillaceous schists, covered here and there by limestone in which no fossils were observed.

The second section extended from La Estéla in the low country of Catalonia to Céret in France. Immediately north of La Estéla, which stands upon horizontal tertiary strata, the geologist, on ascending the Pyrenees, finds strata of conglomerate having a southerly dip and probably identical with the group *a* of the first section. This conglomerate resembles the pudding stone which caps the lofty hill of Montserrat in Catalonia. Next to this comes a nummulitic limestone (*b*), which is seen at Tarrades, and is also inclined to the south; then a series of shales and sandstones (*c*); and next to this, proceeding towards the north, a formation of red sandstone and red marl (*d*). This last is well exhibited in the valley of the river Muga not far from San Lorenzo. It recurs on the north of that valley with a southerly dip, and rests on strata of mica schist and gneiss, which dip to the north. A little to the north of this point the secondary and primary rocks come in contact with opposite dips; the Pyrenees here consist of gneiss, mica schist and clay slate, which continue to near Céret, where tertiary formations are seen at the base of the Pyrenees.

The above section passes through Masanet, to which place Maclure supposed that the volcanic rocks of the Olot district extended. The author searched in vain for any of these modern igneous rocks near that place, and convinced himself that the extent of the modern volcanic region of Catalonia has been greatly exaggerated, and that it does not stretch in a direction from Olot towards Masanet much farther than Castel Follit, or some point between the last-mentioned town and Besalu.

ZOOLOGICAL SOCIETY.

Dec. 10, 1833.—Specimens were exhibited of *Nyctinomus acetabulosus*, Geoff.; an *Ibis*, apparently *Ibis religiosa*, Cuv.; and a *Chamæleon*, *Chamælco verrucosus*, Cuv. They were presented to the Society by Charles Telfair, Esq., Corr. Memb. Z. S., by whom they were obtained from Madagascar.

Colonel Sykes placed on the table his specimen of the *wild Dog* of Dukhun, *Canis Dukhunensis*, Sykes, for the purpose of comparing it with a skin of the *wild Dog* of Nepâl, *Canis primævus*, Hodgs., recently presented to the Society by B. H. Hodgson, Esq., Corr. Memb. Z. S. (See p. 62.) He showed that the two *Dogs* are perfectly similar in their general form, and in the form of the *cranium*; and that in his specimen, equally with that of Mr. Hodgson, the hinder tubercular tooth of the lower jaw is wanting. The only differences remarkable between the two specimens are in the quality and colour of the fur, that of the *Dukhun Dog* being paler and less dense than that of the individual from Nepâl. These differences, depending probably on climate and individual peculiarity, cannot be regarded as sufficient to indicate a distinction between the two races. Identical as they are in form and habits, Col. Sykes considers them as belonging to one species. A short notice of the *Dukhun Dog*, communicated by him to the Committee of Science and Correspondence, was published in the Phil. Mag. and Annals, N.S. vol. x. p. 305; and a detailed account of it, read by him before the Royal Asiatic Society, has just appeared in the Transactions of that body.

At the request of the Chairman, Mr. Gould exhibited a series of *Eurylaimi*, Horsf., in illustration of a paper "On an undescribed Species" of that genus from Rangoon, which he characterized as *Eurylaimus lunatus*. The beak of this bird is dark olive inclining to black, and is lighter at its edges and along the *culmen*. The *tarsi* are brownish black. The beautiful semilunar mark which extends across the whole of each side of the neck, consists of silvery white feathers, elevated above the rest, and abruptly terminated as if clipped by scissors.

The exhibition was resumed of the new species of *Shells* contained in the collection made by Mr. Cuming on the Western Coast of South America and among the Islands of the South Pacific Ocean. Those exhibited on the present evening were accompanied by characters by Mr. G. B. Sowerby, and consisted of the following species, thirty-six in number, of the genus PLEUROTOMA, viz. PLEUROT. *maura*, *unimaculata*, *rosea*, *Clavulus*, *rudis*, *Oxytropis*, *maculosa*, *albicostata*, *clavata*, *bicolor*, *splendidula*, *olivacea*, *cincta*, *bicanalifera*, *cornuta*, *rugifera*, *modesta*, *discors*, *pallida*, *aterrima*, *nigerrima*, *adusta*, *Turricula*, *corrugata*, *interrupta*, *excentrica*, *incrassata*, *duplicata*, *unicolor*, *rustica*, *granulosa*, *variculosa*, *nitida*, *collaris*, *hexagona*, and *formicaria*.

The skins were exhibited of a *Lion* and *Lioness* killed in Guzerat by Captain Walter Smee, who, at the request of the Chairman, stated that they were selected from among eleven obtained by him in the same country, eight of which he had brought with him to England. The *Lion* is distinguished from those previously known by the absence of a mane from the sides of the neck and shoulders, the middle line of the back of the neck being alone furnished with longer hairs, which are erect like those in the same situation in the *Cheetah*, *Felis jubata*, Schreb. The under surface of the neck has long, loose, silky hairs, and there is a tuft at the angle of the anterior legs.

Captain Smee remarked that the existence in Guzerat of a *maneless Lion* had been known thirty years since by Colonel Sykes, and that Olivier had seen at Bagdad a similar animal, which was understood to have been brought thither from Arabia; but that hitherto, he believed, no skin of such a race had fallen under the observation of naturalists in Europe. Besides the absence of the extensive mane, it has to distinguish it from the ordinary *Lion*, a somewhat shorter tail, furnished at its tip with a much larger brush.

Regarding it as a strongly marked variety of the *Lion* hitherto known, Captain Smee proposed for it the following characters:

FELIS LEO, Linn., Var. GOOJRATENSIS. *Jubâ maris cervicali brevi, erectâ; caudæ flocco apicali maximo nigro.*

Hab. in Guzerat (et in Arabiâ?) [et in Persiâ? *].

A male measured, including the tail, 8 feet $9\frac{1}{2}$ inches in length His total weight, exclusive of the entrails, was $4\frac{1}{2}$ cwt.

The *maneless Lion* extends in Guzerat through a range of country about forty miles in length, where it is known as the *Ontiah Baug* or *Camel Tiger*, a name derived from its colour. In the hot months it is found in the low bushy wooded plains that skirt the Sombermutty and Bhardar rivers, from Ahmedabad to the borders of Cutch. It is destructive to cattle, but does not appear to attack man. When struck by a ball it exhibits great boldness, standing as if preparing to resist its pursuer, and then going off slowly, and in a very sullen manner; unlike the *Tiger*, which, on such occasions, retreats springing and snarling.

Captain Smee entered into various details respecting the animals exhibited by him, comprehending the heads of a paper "On the *maneless Lion* of Guzerat," which he had prepared for the Society*.

* We are enabled to add, upon the authority of an English gentleman holding a high rank in the Persian military service, that a *maneless* variety of the *Lion* exists also in Persia, inhabiting the forests of Mazanderan and Ghilan; and that it resembles (some of?) the sculptured representations of the Lion on the ruins at Istakhr, in the same country, hitherto usually regarded as those of the ancient Persepolis. It will be seen, on reference to Heeren, (Historical Researches, 'Asiatic Nations,' vol. i. pp. 169, 184, Oxford 1833,) that the animal designated by some of these sculptures has been a subject of doubt in the minds of those who have described or endeavoured to interpret them. Such doubt would naturally arise, if they actually represent a variety of the Lion nearly destitute of a mane, the existence of which was unknown. The deficiency in this character has probably led to the supposition, (of which we have an instance in Rhode, who takes one of the figures to be a hound or dog,) that these sculptures are not intended for the Lion but for some other animal. The geographical connexion of Persia with Guzerat on the one hand and Arabia on the other, renders it most probable that the *maneless Lion* which thus appears to occur in all the three countries belongs to the same variety. It remains to be seen, whether, as conjectured by Major Hamilton Smith (see Griffith's Cuvier, 'Mammalia,' vol. ii. p. 428), "the skin and jaws of a new species of Cat, larger than the Lion, and without mane," expected to be received from Nubia by Professor Kretschmen, will prove to be those of the maneless Lion represented in the Egyptian sculptures, and point out a fourth locality for this remarkable variety.

E. W. B

Some notes by Mr. Martin on the anatomy of the *Grison*, *Galictis vittata*, Bell, (*Gulo vittatus*, Desm.,) were read. They were derived from the examination of an individual which recently died at the Society's Gardens, and are given in the 'Proceedings.'

Dec. 24.—Extracts were read from a letter, addressed to the Secretary by the Rev. R. T. Lowe, Corr. Memb. Z.S., and dated Madeira, November 15, 1833. They related to a collection of *Fishes* made in that island by the writer, and accompanied about thirty species presented by him to the Society, in addition to those formerly transmitted by him, and exhibited at the Meeting of the Committee of Science and Correspondence on August 14, 1832. Those now sent were severally exhibited. They include the following species regarded by Mr. Lowe as hitherto undescribed, and for which he proposes the subjoined names and characters, further particulars being given in the 'Proceedings'.

Serranus marginatus. *Beryx splendens*. (This new species of *Beryx*, —a genus remarkable for the excess in number of the soft rays of the ventral fins beyond that which is normal in *Acanthopterygian Fishes*, viz. five,—has their number (12) greater than any other except *Ber. Delphini*, recently described by M. Valenciennes from an individual obtained from the Indian Ocean.)

Fam. CHÆTODONTIDÆ. Genus LEIRUS. *Corpus* ellipticum, compressum; squamis deciduis parvis. *Caput* parvum, nudum, declive. *Os* parvum: maxillâ superiore obtusissimâ; inferiore breviore, truncatâ. *Dentes* minuti, simplices, in utrâque maxillâ 1-seriati: palatini nulli. *Opercula* marginibus serratis. *Pinnæ* dorsalis analisque posticè latiores, squamosæ. *Membrana branchiostega* 7-radiata.

OBS. Genus *Bramæ*, Bloch, maximè affine. Differt præcipuè dentibus palatinis nullis: etiam pinnâ caudali haud profundè bilobâ. *Leirus Bennettii*.

Tetragonurus? simplex (*Tet. caudâ utrinque simplici.*) (If this be a true *Tetragonurus*, Risso, (and there is no reason to doubt it except the absence of the *carinæ* on each side of the tail which give to that part in the type of the genus a square form,) it furnishes strong evidence of the affinity of that group to the *Scombridæ*. The spurious finlets behind its second dorsal and its anal fins denote a closer approach to the *Mackerels* than could be inferred from *Tet. Cuvieri*, Risso.) *Crenilabrus Trutta*. *Rhombus Maderensis*. *Centrina nigra*. (It is said that this fish does not grow larger than the individual sent, (10 inches in length). It is intermediate in characters between *Centrina*, Cuv., and *Acanthias*, Ej.; having the teeth of the former genus, and the form of body of the latter, as well as the backward position of the second dorsal fin. It is entirely black, even on its under surface.)

We observe from the List of Contributors and from the Index to the first part of the Society's 'Proceedings,' now complete, that the number of the former during the past year (1833) amounted to 47; among whom, for the interest and variety as well as the number of their communications or exhibitions, the names of the Secretary (Mr. E. T. Bennett), Mr. Bell, Mr. Broderip, Mr. Cuming, Mr. Gould, Dr.

Grant, Mr. Gray, Mr. Owen, the late Mr. Telfair and Mr. Yarrell, stand conspicuous; and that the number of new species, and (what are almost equally valuable) species newly characterized was 247, and that of species on which detailed anatomical observations were communicated 26.

LINNÆAN SOCIETY.

April 1, 1834.—A paper was read, entitled, "Observations on the Metamorphosis of Insects;" by Edward Newman.

Mr. Newman refers the whole of the wonderful transformations in insects to that tendency to continual decay and reparation observable in all organized beings: he considers that in this portion of the creation it has become a constant change of skin. Instead of admitting the existence of the four distinct states of egg, larva, pupa and imago, he contends that there are only three, that of pupa being frequently wanting; and when it does exist, being merely the matured larva state waiting its final ecdysis. The three states he considers to be these: the egg, or fœtal state; the larva, or adolescent state; and the imago, or adult state. Mr. Newman terms the true or winged insects Tetraptera, and divides them into these four groups: Tetraptera Amorpha, containing the classes Lepidoptera and Diptera, in which the penultimate bears no resemblance to the final state; Tetraptera Necromorpha (Hymenoptera and Coleoptera), in which the penultimate resembles the final state, but appears as though dead; Tetraptera Isomorpha (Orthoptera and Hemiptera), in which all the states have the same shape and appearance; and Tetraptera Anisomorpha (Neuroptera), in which the metamorphosis is various.

ROYAL ASTRONOMICAL SOCIETY.

March 14, 1834.—The following communications were read:—

I. On the Satellites of *Uranus*. By Sir J. Herschel.

This paper, dated from Portsmouth, on the eve of the author's departure for the Cape of Good Hope in November last, contains an investigation of the motions of two of the satellites of *Uranus*.

Notwithstanding the remarkable peculiarities presented by the satellites of this planet, in the great inclinations of their orbits to the orbit of the primary planet, and their retrograde motions, they have never been observed, or even seen (so far as the author is aware), except in the telescope with which they were originally discovered. In a paper of the late Sir William Herschel, published in the *Philosophical Transactions* for 1815, and containing the whole series of his observations on these satellites, the existence of at least two of them appears to be placed beyond a doubt. But since that time the unfavourable situation of the planet, to the south of the equator, has opposed a serious obstacle to their re-observation, even with telescopes of the highest optical capacity. Since the year 1828, the author has made repeated observations upon two of the satellites with the 20-feet reflector at Slough, from which he has deduced an approximate determination of their orbits.

There being no eclipses of these satellites, and the measurement of their distances from the planet with any approach to exactness being hopeless in its present situation, the only *data* by which a knowledge of the elements of their orbits can be obtained are their angles of position with the meridian, which are susceptible of tolerably correct determination. For the investigation of the elements, the author considers the best method to be that which he has used for determining the orbits of revolving double stars from *data* of the same nature. The application of this process to the case of the satellites, however, is greatly facilitated by the approximate knowledge already possessed of their periods, and the situations of the planes in which they revolve. In that of the double stars, these elements are wholly unknown. But in the case of the satellites, by using the approximate node and inclination, the observed angles of position, as seen projected on the heavens, may be reduced to the plane of the orbit, thereby simplifying the computations considerably. And a knowledge of the periodic time enables positions, observed in different revolutions, to be used as if they had been observed consecutively in a single revolution. In order, however, to justify this mode of proceeding, it is necessary, in the first instance, to show that a sufficient approximation to the values of the elements is already possessed. The author has at present limited himself to this preliminary verification, as he had not sufficient time to investigate the subject more completely.

There are given 49 observed angles of position of the first of the two satellites, and 59 of the second; 31 of the first, and 32 of the second, having been observed by Sir William Herschel between 1787 and 1798; and the remainder by the author, between 1828 and 1832. Assuming the position of the nodes of the satellites' orbits, and their inclinations to the orbit of the planet, as given by Sir W. Herschel (viz. longitude of ascending node of each 165° 30′, and inclination 101° 2′), the observed angles of position are reduced to the plane of the satellites' orbits: and supposing that the orbits are circular, and assuming the times of revolution as delivered by Sir W. Herschel, two epochs are determined at which the satellites pass through the ascending node; the first being in the year 1787, and the second in the year 1828. And, the number of revolutions performed in the interval being known from the approximate periods, the following correct values of the times of revolution are obtained, viz.:

	d	h	m	s
First Satellite	8	16	56	31·3
Second Satellite . . .	13	11	7	12·6

The first being 26s·1 greater, and the second being 1m 46s·4 less, than the times given by Sir William Herschel in his paper above mentioned.

On comparing the angles of position, computed from the corrected elements, with those actually observed, the errors for the first satellite seldom exceed 10°, and for the second 7°; and they are, for the most part, inferior in amount. This the author considers a reasonable degree of precision under the circumstances. There is reason to suspect an ellipticity in the orbit of the first satellite, corresponding to an excentricity of 0·035.

The author gives formulæ for determining the positions of the two satellites at any time, and tables for facilitating their application to the first. And he concludes with remarking, that of other satellites than these two he has no evidence; but if any exist, he hopes soon to procure a sight of them. Neither has he ever seen any appearance about the planet which gives ground for the least suspicion of a ring.

II. Continuation of Researches into the Mass of *Jupiter*, by Observations of the Elongations of the Fourth Satellite. By Professor Airy*.

The results of this paper are founded on six sets of measures, made in the same manner as the last, and with the same instrument, before the opposition of *Jupiter*. The unfavourable state of the weather after the opposition prevented any more being made. By observation of δ *Ursa Minoris* in different positions of the instrument, the errors of the line of collimation, and the declination and polar axes, were found to be very small, as was also the rate of the clock. In every instance, twelve transits over three wires were obtained, both of the planet and satellite.

The mode of calculation in this paper differs from that in the preceding, in a correction applied to the observed difference of right ascension for refraction; in the addition of $0^{s}\cdot0073$ to the mean longitude of the satellite in finding the Jovicentric right ascension, for reasons mentioned in the *Memoirs* of the Society, vol. vi. p. 98; and in subtracting $0^{s}\cdot0050$, or $0^{s}\cdot0054$, for the equation of the equinoxes.

The following are the individual results:—

1833.	Log. mass of ♃.	Pos. 4th satellite.
Sept. 15	6·9792116	Preceding.
24	6·9794552	Following.
Oct. 1	6·9801776	Preceding.
9	6·9801971	Following.
10	6·9790139	Following.
19	6·9802385	Preceding.

The constant difference between *preceding* and *following* observations of the satellite does not exist in the above series; and Professor Airy states that he is unable to determine whether this arises from the smallness of the number in the series, or from some alteration in his method of observing.

Considering all of the preceding as entitled to equal credit, the log. mass of the sum of the planet and satellite is 6·9797157, or (as in the former paper) the log. mass of the Jovial system is 6·9797717; the corresponding number is $\frac{1}{1047\cdot68}$, that found in the preceding paper being $\frac{1}{1048\cdot70}$. On this difference, Professor Airy observes: "If this proceed from an error in the inclination of

* For accounts of Professor Airy's former researches on this subject, see Lond. and Edinb. Phil. Mag., vol. ii. p. 314, and vol. iii. p. 233.

the orbit of the fourth satellite, the difference will become more conspicuous in observations when *Jupiter* is near the solstitial colure, and the true mass of *Jupiter* will be greater than that obtained even by the present determination. I believe, however, that the difference is due entirely to the small errors which cannot be eliminated, even from the mean of many observations."

III. Observations of the Transit of *Mercury* over the Sun's Disc, in May 1832; and of the Comet of Encke in June 1832: at Buenos Ayres. By M. Mossotti.

IV. Transits of the Moon with Moon-culminating Stars, observed at Cambridge Observatory in the months of January and February 1834. Long. $23^{s}\cdot54$ east of Greenwich.

ENTOMOLOGICAL SOCIETY OF LONDON.

This Society, established during the last year, which already includes in its list of Members nearly all the principal cultivators of the science of Entomology in Great Britain, holds its meetings on the first Monday in every month, at its Rooms, No. 17, Old Bond Street. The subjects which during the present year have occupied the attention of this Society have possessed not only much entomological but also considerable practical utility and interest.

At the meeting in January, among others a paper was read entitled "Explanation of the sudden Appearance of the web-spinning Blight of the Apple, Hawthorn, &c.," by Mr. Lewis, who stated that the grubs composing this blight, which makes its appearance in the spring, had been hatched at the commencement of the preceding winter, but had remained under a glutinous cover, formed by the female parent as a case for her eggs, until the young brood had attained some size.

At the February meeting the subject of mummy insects formed part of the proceedings, in which Mr. Pettigrew took part.

At the meeting in March, Captains Ross and James C. Ross being present, the arctic insects brought home by those gentlemen were exhibited, as well as those collected by the late Captain Lyon, some of which formed the subject of a paper by Mr. Westwood.

April 7.—At this meeting the following papers were read:—"A Memoir upon the Habits of various Species of Indian Insects," by W. W. Saunders, Esq., F.L.S., &c. "Observations on a mode practised in Italy of excluding the common House-fly from Apartments," by Wm. Spence, Esq., F.L.S., &c. In this paper, which had reference to the instinctive powers of insects, it was stated that the mode of exclusion in question consisted of fixing netting across the open window-frames; but that even when the meshes were more than an inch in diameter, the flies instinctively were prevented from entering the rooms. A portion of a "Memoir upon Insects found in Gums and Amber," by the Rev. F. W. Hope, F.L.S., &c., in which the author entered fully into the subject of the general properties of various gums, noticing the common error of mistaking the gum anime for copal, which latter he stated was never found to contain insects. This statement was confirmed by Dr. Ure, who was present, and who stated, that at

Mr. Hope's request he had made a minute analysis of these gums, an account of which he detailed, adding that he had obtained very valuable practical results, in operating on these substances, by the application of the new æthereal essence of caoutchouc.

A lengthened discussion terminated the proceedings.

LXIII. *Intelligence and Miscellaneous Articles.*

CHLOROCARBONIC ACID.

M. DUMAS finds that this acid is best formed in a diffused rather than a vivid light: when mixed with anhydrous alcohol it yields an æther, in which an atom of chlorine is replaced by an atom of oxygen.

The æther put in contact with ammonia, yields a compound which crystallizes very readily, and offers a case of isomerism with lactate of ammonia, and also presents the composition of ammonia combined with sugar.

Oxalic æther gave with ammonia a compound which may be represented by alcohol and oxamide, and from which this last substance may be extracted.—*Journal de Chimie Médicale*, Feb. 1834.

PYROGENOUS ACIDS.

M. Pelouze announces that he has discovered a general law, applicable to all pyrogenous acids (*acides pyrogénés*) which do not contain azote.

This law is thus expressed: A pyrogenous acid being given, this acid + a certain quantity of water and of carbonic acid, or only one of these bodies, always represents the organic matter from which it is derived.

Thus, gallic acid exposed in a retort to 410° Fahr. is entirely converted into pyrogallic acid and carbonic acid, totally absorbable by solution of potash, and these two products are exactly equivalent to the acid employed, at the temperature of 482° Fahr., the results of the decomposition being then equivalent to metagallic acid, carbonic acid and water.

M. Robiquet had discovered that by distilling meconic acid at 410° Fahr. metameconic and carbonic acids, together representing meconic acid, were obtained; and that at 482° Fahr. he procured pyromeconic acid and carbonic acid.

M. Dumas obtained, by the distillation of citric acid, pyrocitric acid and water, with scarcely a trace of charcoal.

When the volatility of an acid prevents the above-described decomposition, it may be effected by combining it with a base. For example, by saturating acetic acid with barytes, carbonic acid remains combined with the barytes in the residue, and pyroacetic spirit is distilled.

M. Bussy, by distilling margaric and stearic acids in contact with lime, obtained new substances, the composition of which accords with the preceding law.

M. G. de Claubry has observed that the decomposition of several organic bodies yields oxide of carbon: this happened especially during the decomposition of oxalic acid by M. Gay-Lussac. M. Pelouze states, that with proper precautions, and especially by keeping a proper temperature, this apparent anomaly may be avoided, and the law above stated will hold good.

M. Despretz remarks, that the two compounds obtained with the pyrogenous acids are the most stable yielded by the elements present; and this circumstance would lead *à priori* to the law indicated.—*Journal de Chimie Médicale, Feb.* 1834.

ON HYPERIODIC ACID AND HYPERIODATES.

MM. Ammermüller and Magnus remark, that considering the strong resemblance which exists between chlorine and iodine, it appears singular that analogous compounds of these with oxygen have not yet been discovered, especially as the affinity of iodine for oxygen appears to be stronger than that of chlorine. The method of Serullas for procuring hyperchloric acid does not succeed for procuring hyperiodic acid, for on heating iodate of potash, hyperiodate cannot be procured. M. Liebig's method of procuring iodate of soda consists in mixing iodine with a large quantity of water, passing excess of chlorine into it, and adding carbonate of soda to the solution. When saturation is effected, a considerable quantity of iodine is precipitated: fresh chlorine is passed into the solution until all the iodine precipitated is redissolved. The saturation with soda is repeated, iodine again separates, and then chlorine must be added till no more precipitation takes place. The iodate of soda is separated by evaporating the clear solution, and treating it with alcohol.

During this process a white pulverulent matter, insoluble in water, was obtained, and this, considering the circumstances under which it was formed, appeared to be subhyperiodate of soda; and it was soon found that it might be procured by adding a solution of soda to one of iodate of soda, and passing chlorine into the mixture. The separation of this salt is accelerated by a gentle heat, and if carbonate be used instead of caustic soda, the mixture must be heated nearly to ebullition, because at common temperatures hyperiodic acid cannot expel the carbonic acid of the carbonate.

Instead of passing chlorine gas into the solution, a mixture of iodate and chlorate of soda may be heated with chloride of soda, obtained by decomposing chloride of lime with carbonate of soda.

Having found that the subhyperiodate of soda gave out oxygen when heated, an attempt was made to analyse it; but the difficulties were such as rendered it necessary to employ another salt for this purpose, and that of silver seemed preferable. It was prepared by dissolving the salt of soda in dilute nitric acid, and precipitating with a solution of nitrate of silver. When the acid was slightly in excess, a bright yellow precipitate with a tint of green was obtained, and this was washed with water acidulated with nitric acid. It was then dissolved, with heat, in dilute nitric acid, and then, by crystallization, there were obtained small brilliant crystals of a pale straw colour. When these were treated with hot water they became of a deep brown colour, but did not dissolve; afterwards almost black; and they became of a fine red by powdering. On the contrary, by evaporating the solution of the precipitate in nitric acid, until the salt crystallized in the hot solution, crystals of a yellow orange colour were procured.

By these processes these three different combinations of hyperiodic acid and oxide of silver were obtained. They were analysed by heating in a glass retort to obtain the oxygen, and the silver was obtained by solution in nitric acid, and precipitated by the muriatic.

The *yellow salt* appeared to be composed of

Iodine............	28·598
Oxygen	16·307
Silver............	48·981
Water............	6·114
	100·000

Supposing hyperiodic acid to contain 7 atoms of oxygen, it will appear by calculation that this salt is a dihyperiodate of silver consisting of

One atom of hyperiodic acid....	182
Two atoms of oxide of silver	236
Three atoms of water..........	27
	445

The *red salt* appeared by analysis to differ from the above only in containing but one atom of water, or

One atom of hyperiodic acid....	182
Two atoms of oxide of silver....	236
One atom of water............	9
	427

The *orange salt* consisted of

One atom of hyperiodic acid	182
One atom of oxide of silver	118
	300

This salt is therefore neutral and anhydrous. It is decomposed by water into a subsalt and hyperiodic acid: this occurs at common temperatures. The form of the crystals is destroyed, a straw-coloured subsalt is deposited, and the liquor, separated by the filter, acts as an acid, and does not contain any silver, but merely hyperiodic acid dissolved in water. This is the best method of obtaining this acid in a pure state. When the orange salt is treated with hot water, the same effects are produced, except that the remaining subsalt is not the yellow, but the red one.

Hyperiodic acid, thus obtained, may be heated to ebullition without decomposition: by evaporation, crystals are obtained which do not deliquesce. At a high temperature they lose part of their oxygen and are converted into iodic acid; at a still greater heat they are decomposed into oxygen and iodine. Muriatic acid converts them into iodic acid, chlorine being evolved.

Hyperiodate of Potash.—This salt may be obtained by adding potash or its carbonate to iodate of potash and passing chlorine into the solution: it is then precipitated in small white crystals, which are difficultly soluble in water, and in appearance greatly resemble hyperchlorate of potash.

This salt appeared to be composed of

One atom of hyperiodic acid....	182
One atom of potash	48
	230

A subhyperiodate of potash was obtained by adding potash to the above, and evaporating the solution: white crystals were obtained, which were not more difficult of solution than the neutral salt. It appeared to consist of

One atom of hyperiodic acid....	182
Two atoms of potash..........	96
	278

It is then an anhydrous dihyperiodate.

Hyperiodate of Soda.—The neutral salt was obtained by dissolving the subsalt in hyperiodic acid to saturation. The solution yields a white salt by evaporation, which dissolves readily in water, and is unalterable in the air. It consisted of

One atom of hyperiodic acid....	182
One atom of soda	32
	214

Subhyperiodate of Soda.—This consisted of

One atom of hyperiodic acid....	182
Two atoms of soda.............	64
Three atoms of water.........	27
	273

This salt is therefore a dihyperiodate.—*Ann. de Chim. et de Phys.*, tom. liii. p. 92.

CRYSTALLIZATION OF SULPHURET OF LEAD.

M. Becquerel has succeeded in forming and crystallizing sulphuret of lead, by means of a liquid and two substances, so placed that an electric current results from their reciprocal action. He put into a tube, about four inches long, one quarter of an inch in diameter, and closed at one end, sulphuret of mercury to the height of about one inch; upon this a solution of chloride of magnesium was poured, and a plate of lead was immersed to the bottom of the tube: the apparatus was then hermetically sealed. In about a month or six weeks, a very small stratum, of a metallic grey brilliant precipitate, began to appear on the sides of the tube, under the sulphuret: this was easily detached, and gradually other small crystals appeared. These crystals, examined with a glass, were found to be tetrahedrons, having the appearance of galena. When the tube was opened, gas was evolved, which had the smell of the compounds of sulphur with chlorine and with hydrogen. On examining, a short time afterwards, the liquor with an acid, sulphurous acid was disengaged. The lower part of the plate of lead had become brittle, on account of its combination with mercury. These results are easily explained. When the lead is in contact with an alkaline or earthy chloride, such as that of magnesium, a double chloride is formed; magnesium is momentarily developed by this reaction; the lead becomes electro-negative, and the solution electro-positive. The first attracts the mercury of the sulphuret, while the sulphur, which is the electro-negative element, acts upon the double chloride by the intermedium of the infinitely small stratum of liquid which adheres to the glass. This possesses peculiar properties, which M. B. has lately described. One portion of the sulphur combines with the lead of the double chloride, and gives rise to a sulphuret which crystallizes, whilst the other combines with the chloride of magnesium, and the chlorine which was combined with the lead: whence results a sulpho-chloride of magnesium.

When the operation is continued for many months, the fluid in the part adjacent to the sulphuret of mercury becomes of a reddish tint, which is that of the chloride of sulphur. No trace of lead remains in solution, which is a proof that it is entirely precipitated by the sulphur. The crystallization of the sulphuret can be attributed only to the slowness with which it is formed.

The small stratum of liquid alluded to as adhering to the glass favours the circulation of the electric fluid. The natural crystals of sulphuret of lead are either cubes or octahedrons, or combinations of these two forms: the tetrahedron, which is the form of the artificial compound, is comprehended in the same crystalline system.—*An. de Chim. et de Phys. May,* 1833.

PRESENCE OF AZOTE IN SEEDS.

M. Gay-Lussac remarks, that some seeds contain azote is a well-known fact, since a substance of an animal nature may be extracted from them, as gluten from wheat flour; but he observes that he has no where seen it stated that all seeds contain an animalized matter.

To be satisfied that is the case, however, it is requisite only to submit any seed to distillation, either in its natural state, or, what is better, deprived of its ligneous envelope.

Nevertheless, ammoniacal products are not always immediately obtained by distillation. Rice, for example, gives a very acid product; but the presence of ammonia is easily proved by the addition of lime. Kidney beans, and many other similar seeds, give a very ammoniacal product. In general, any grain, deprived of its envelope, may be considered as composed of two parts, one vegetable, which gives an acid product by distillation, and the other animal, yielding ammonia; so that the acid or alkaline character depends upon the predominance of one or other of these matters.

M. Gay-Lussac submitted all grains which came in his way to distillation, and all of them gave ammonia, the greater number immediately, and the others after the addition of lime; and he considers that this circumstance will explain the nutritive quality of grain, and the astonishing fertilizing property of seeds as manure, after the oil has been extracted from them.—*Ibid.*

ON KREOSOTE. BY M. REICHENBACH DE BLANSKO.

M. Reichenbach of Blansko, to whose labours we owe the discovery of Paraffine, of Eupione*, and Picamore, has recently found in the products of the destructive distillation of wood a new substance, which he terms Kreosote, from the Greek words κρέας flesh, genitive by contraction κρεῶς, and σώζω I save.

This substance is highly interesting, not only on account of its chemical properties, but from its useful application to therapeutics, domestic œconomy, and the preservation of provisions for long voyages. Two processes are given for its preparation. By the one the kreosote is obtained from pyroligneous acid, by the other from the tarry matter which distils over along with that acid. These processes do not differ much; both are tedious, but the latter method seems to be the easier. The tarry matter yields an oil by distillation, to which, after being rectified and heated, carbonate of potash is added, to neutralize the acetic acid associated with it. The acetate of potash separates, and the oil is again distilled, care being taken to reject the first products, and not to carry the distillation to dryness. The oil that comes over is then treated with a solution of caustic potash of sp. gr. 1·12, great heat is produced, and a portion of eupione, &c., formed, which floats on the surface. These are rejected, and the alkaline solution is slowly made to boil in an open vessel. A chemical action takes place,—it absorbs oxygen from the air, and assumes a brown colour. After it is cooled in the open air, diluted sulphuric acid is added until the oil is set free. It is again distilled with water, to which a little caustic potash should be added. The oil is then separated from the water in the receiver, and again treated with a solution of potash sp. gr. 1·12, boiled as before—cooled—treated with rather an excess of sulphuric acid—poured off from the sulphate of potash—well washed with water to carry off the excess of acid—again distilled with water, to which a little phosphoric acid is added, to saturate the ammonia associated with the oil. Lastly, it is dissolved in caustic potash; and if the preceding operations have been carefully attended to, the kreosote and the potash unite, and the mixture, when heated, leaves no residuum of eupione, nor becomes brown by exposure to the air. The kreosote may then be separated from the potash by distillation, and, although not quite pure, is sufficiently so for medical purposes. The foregoing is a very imperfect outline of the process, which will be seen to be sufficiently tedious. The processes will be found minutely described in the Annals of Schweigger-Seidel, vols. vi. and vii.

Kreosote is an oily, colourless, transparent liquid, possessing great refrangibility. Its odour is penetrating, disagreeable, and similar to that of smoked beef. It is of the consistence of oil of almonds, and has a sp. gr. of about 1·037 at 20° Cels. (68° Fahr.) It boils at 203° Cels. (397·4° Fahr.) and is not congealed at a temperature of —27° Cels. (—16·6° Fahr.) It burns with a smoky flame. It is a non-conductor of electricity. At 20° Cels. (68° Fahr.) it forms with water two different combinations, the one containing one fourth part of kreosote in 100 parts of water, the other, ten parts of water in 100 kreosote.

This substance forms numerous interesting compounds, with acids

* See Lond. and Edinb. Phil. Mag. vol. i. p. 402.

and alkalies. Concentrated, it dissolves the deutoxide of copper, and assumes a chocolate brown colour. At a boiling heat it reduces the deutoxide of mercury, and is then transformed into a resin, which has no longer the properties of kreosote. Nitric acid acts on it strongly, and acid vapours are disengaged. It combines with chlorine, bromine, iodine, phosphorus, and sulphur. Potassium thrown into it disappears, gas is disengaged, and potash remains combined with thickened kreosote. From this combination the kreosote separates by distillation. Concentrated sulphuric acid added in small quantities gives to kreosote a reddish colour; but when the quantity of acid is increased, the kreosote becomes black. Of all the organic acids, the acetic seems to have the greatest affinity for kreosote, uniting with it in every proportion.

This substance, when cold, forms two combinations with potash. The one is an anhydrous liquid, of an oily consistence; the other is a hydrate, and crystallizes in white scales. All the acids, not excepting carbonic acid, separate the kreosote from these combinations. With soda it forms combinations similar to those with potash. It has a great affinity for lime and the hydrate of barytes; with these bodies it forms compounds of a dirty-white colour, soluble in water, but which, when dried, assume the appearance of a rose-coloured powder.

Kreosote, in a warm and cold state, dissolves a great number of salts. Some are reduced, but the greater part are separated in the form of crystals by cooling, such as the acetates of potash, soda, ammonia, lead, and zinc, and the hydrochlorates of lime and tin. It reduces the acetate and nitrate of silver.

Alcohol, æther, acetic æther, carburet of sulphur, eupione, and oil of petroleum, combine with kreosote in every proportion. Paraffine, though issuing from the same source with kreosote, has little tendency to combine with it. Indeed, the combination cannot be effected unless eupione be present, and is in a direct ratio to the quantity of eupione. Kreosote with difficulty dissolves caoutchouc, and only by the assistance of boiling, differing very much in this respect from eupione, which readily dissolves caoutchouc.

If to a solution of albumen in a large quantity of water a single drop of kreosote be added, the albumen is immediately coagulated. When fresh meat is put into a solution of kreosote, allowed to remain for half an hour or an hour, then withdrawn, and afterwards dried, it may be exposed to the heat of the sun without putrefying, and in the space of eight days it becomes hard, the colour changes to a reddish brown, and the flavour is that of good smoked beef. Fish may likewise be preserved by it. It is pretty evident that kreosote is the antiputrescent principle of pyroligneous acid and of wood smoke.

M. Reichenbach has ascertained that kreosote does not act upon pure fibrin, which by itself is said not to be susceptible of putrefaction. Its action upon the animal œconomy is deleterious. Placed upon the tongue it occasions violent pain, and when poured in a concentrated state upon the skin it destroys the epidermis. Insects and fish thrown into it immediately die. Plants also perish when watered with it. M. Reichenbach has made experiments with this substance concentrated and diluted, and his success has surpassed his expectations. It has, he alleges, effected a speedy cure in cases of caries, of cancer, and of carcinomatous ulcers.

M. Schweigger-Seidel has made a comparative examination of kreosote, and the aqua Binelli, from which he has come to the conclusion, that the fundamental base of this hemostatic liquor is kreosote, of which it is only an excessively weak solution.—*Edin. Med. and Surg. Journ.*

ON IODOUS ACID.—BY SIG. L. SEMENTINI.

In a Letter addressed to E. DANIELL, Esq., Secretary of the Royal Institution.

Naples, 20th April, 1833.

Sir,—After a long silence, occasioned by ill health, I beg to trouble you with a few scientific observations, which I will thank you not only to make public, but to favour me also with the opinion of the Royal Institution upon their value.

The many difficulties which I experienced whenever I endeavoured to obtain directly the iodous acid, have induced me to change my method, and I have undertaken another series of experiments and reasonings, of which the following is the result.

Considering that nitrous acid is nothing else than nitric acid *plus* nitrous gas, it struck me, by analogy, that an acid, which might properly be called *iodous* acid, would be the result of a combination of *iodic* acid with the oxide of iodine, which I had already discovered. Having effected this combination, I obtained from it a liquid of an amber yellow colour, which, when preserved in close vessels, continues to retain the same colour; but which, when in contact with atmospherical air, gradually loses its colour, the oxide of iodine being dissipated in the same way as when nitrous acid, in contact with air, is discoloured by the volatilizing of the oxide of nitrogen. This combination does not take effect in all proportions, but in definite quantities, because, if an excess of oxide of iodine be added, this is decomposed, and the iodine is precipitated.

This phænomenon can only, I think, be explained by admitting, that when the dose of oxide of iodine is of sufficient strength, the iodous acid, which is immediately formed, decomposes such oxide, depriving it of its oxygen, and is itself again converted into iodic acid; and, in truth, during the precipitation of the iodine, the liquid is discoloured, again losing its yellow colour. This fact appears to me to be a strong proof of the intimate union which takes place between the two compound substances,—each of them evidently acting energetically upon the other.

This acid, which I call iodous acid, has been made by me by combining about one hundred parts of solid iodic acid dissolved in water, with three parts of oxide of iodine, of the greatest density.

Although the analogy between nitrous acid and iodous acid would imply that this last would not form iodites, as the nitrous acid only forms nitrates, and not nitrites, I combined it with highly saturated ammonia: the union took place without the disengagement and precipitation of iodine, though the yellow colour totally disappeared immediately on the union of the acid with the alkali. This solution, when evaporated, formed a salt, differing from the iodate of ammonia by the following properties.

The iodite of ammonia is less efflorescent than the iodate; its taste is less decidedly salt; it detonates, if heated in an open vessel, but with a longer escape of vapours of iodine; its colour is a light greenish tint. The solubility of the two salts is very different, the iodite being soluble in little more than half the quantity of water required to dissolve the iodate.

Although I have not yet effected an exact analysis and comparison of the two salts, yet, when I caused equal quantities of them to detonate with a proper apparatus, and particularly in small long-necked retorts, I observed the following effects, which appeared to me of some weight: these are, the production of aqueous vapours much more considerable in the detonation of the iodite, and the greater quantity of iodine obtained from its detonation. This is easily recognised by observing, before and after the detonation, the weight of the retorts which are used.

I am now employed in combining this acid with other bases, and in repeating with still greater accuracy the above experiments; but I think I may fairly consider as an acid in *ous* the combination of the iodic acid with the oxide of iodine, if, without being decomposed or altered, it forms with the bases particular compounds, just as we call *nitrous* acid the union of the nitric acid with the oxide of nitrogen, which does not form similar compounds, but, at the moment of combining with the bases, the oxide gas of nitrogen separates from it, and forms, as I have said, nitrates.

I have discovered in zinc a new and important quality, which I am surprised has not been before known. If, when the zinc is fused to a red heat, the vessel is removed from the fire, its combustion will continue as long as the least particle of metal remains, provided it be kept continually agitated, and the oxide which is formed be gradually removed. When a large quantity of the metal is employed, it will be curious to see a combustion continue for a very long time, without the addition of any other heat than that which is developed by the metal itself.

A grey oxide is thus formed, with qualities very different from the common oxide: its specific gravity is much greater, and it absorbs no carbonic acid from the atmosphere.

I am much occupied at present with this phænomenon, which is remarkably beautiful in itself, and which may, in time, lead to the discovery of other important facts.

Luigi Sementini.

E. Daniell, Esq., Secretary of the
Royal Institution, London.

LEDERERITE *NOT* A NEW MINERAL.

A Correspondent has sent us a small specimen of the supposed new mineral Ledererite, imperfectly described in our last Number, p. 317;

but, on examination, it proves, by its form and angles, to be the Hydrolite of De Drée, or Gmelinite of Sir D. Brewster. The analysis of Vauquelin gives silica 50, alumina 20, which agree nearly with the proportions of the Nova Scotia mineral. Vauquelin, it is true, found 21 of water instead of 8·58; but as, perhaps, water may turn out to be *isomorphous* with both lime and phosphoric acid, the two analyses may, when expressed *symbolically*, appear to agree with each other and with the theory; or there *may* be some error in one of the analyses.—Edit.

THE LATE THOMAS TREDGOLD, CIVIL ENGINEER.

The application of the practical sciences to the manufacturing and commercial interests of the empire—forming so remarkable a feature in its appearance—has given to the present age a command over the material world unparalleled in the history of mankind. For the advantages attending this gigantic power, in the use of which this country is so preeminently distinguished above all other nations, the British public is deeply indebted to a "self-created set of men" called into action by the progressive changes in its political condition—the Civil Engineers of Great Britain—the constructors of those "grand and varied works" which, for boldness of design and skill in execution, will be had in remembrance even when the traces of their existence may have been done away.

The exercise of a profession in a country sensible of its importance has generally been attended with emolument proportionate, at least, to the magnitude of the works which its followers have been called upon to construct; but when the engineer has been employed in the investigation of principles forming the basis or tending to the improvement of his practice, the pecuniary remuneration thence resulting has too frequently been very inferior, not only to the value of his labours, but even to the time which they have occupied.

From this cause, the personal friends of the late Mr. Tredgold, a once valuable member of this body, are desirous of making more extensively known the merits of a man whose writings are of such interest in his profession, but whose early death has left his family in a state bordering upon destitution. The testimony of approbation which his works have received from the most able engineers and scientific professors here, and in America, where they are well known and much valued, and also in France, where many of them have been translated and published, conduces powerfully to support an appeal, founded on the justice due to talent on the one hand, and to the discernment and disposition of those to whom it is addressed on the other, and made with the conviction that they will be as ready to relieve, as they are able to appreciate, the difficulties and distresses of "unassuming worth."

The life of an individual endowed with the spirit of scientific inquiry, but prevented by circumstances from indulging it, except in the hours when nature calls for repose, is usually too deficient in incident to be generally interesting. The following brief account is therefore presented to the reader, in the hope that it may excite his sympathy. If the subject of it be known to him, his indulgence is asked for its brevity; if otherwise, it is requested for its length.

An ordinary education at his native village, Brandon, near Durham, and an apprenticeship of six years in that city, occupied his time until his twentieth year. Subsequently he was employed five years as a working carpenter in Scotland, and ten years in an architect's office in London. During both periods the regular hours of labour or of business were sufficiently fatiguing; and the leisure and pecuniary means which they afforded him would appear very limited for other studies than those which the nature of his employment did not render imperative. Yet, under these disadvantages, he acquired an extensive knowledge of Geometry, Chemistry, Geology, &c., and became a very excellent mathematician. But his great merit is in the skill with which he has applied his scientific attainments to the practical advancement of his profession, for to him we are indebted for the best Treatises upon Carpentry, and the Strength of Cast Iron; the extensive use of which latter material forms so decided a characteristic of the safety and durability of our various structures. He also supplied the article JOINERY for the Supplement to the *Encyclopædia Britannica*, and several valuable contributions to the *Philosophical Magazine* and *Thomson's Annals of Philosophy*. During the five following years, while he was practising as an engineer, he investigated and exemplified "the Principles of Stone-Masonry;" "of the Construction of Rail-Roads and Steam-Carriages;" "of Warming and Ventilating Buildings;" added Notes to the Hydraulic Tracts of Venturi, Smeaton, and Dr. Young; and in 1827 completed the last of his works,—an "Essay on the Steam-Engine."

The number and importance of these subjects, all of which his labours have enriched, and the practical utility of the numerous tables and rules which he has deduced from careful experiments, are sufficient proofs of his extraordinary talents, and of the benefits he has conferred upon his country; while the original manner in which he has treated those subjects, and his frank acknowledgement of authors from whom he derived information, show a liberality of feeling, combined with extensive variety of general knowledge.

With the qualifications which distinguished him as a writer, he was no less admirable as a man:—exemplary in the performance of all the duties of Son, Brother, Husband, Father, and Friend; possessing strong benevolent feelings for his fellow-creatures; an anxiety for the improvement of all around him, for their happiness and their good; with a readiness to communicate information, equalled only by his eagerness to acquire it.

During the prosecution of his researches in the limited time which his occupation allowed, he laboured under the effects of painful and increasing illness, the consequence of those sacrifices which an unquenchable thirst for knowledge had required of him. Aware of its tendency, he had made many, but unavailing, efforts at relaxation—for the dominion of Genius over its votaries is absolute and uncontroulable, and to its irresistible influence he fell a victim, at a time when his loss was of the greatest consequence to his family. The declining health and spirits of his widow did not long survive his loss; and the recent death of his eldest daughter has diminished the number of those whose future fate pressed heavily on his last hours, and for whose relief a subscription, originating in the best feelings of some distinguished members of the Institution of which Mr. Telford is the President, has been set on foot*, which has been seconded by several scientific men and military officers, and contains one of the numerous instances of benevolent liberality, characteristic of a Northern nobleman, which is, however, made public by solicited permission. Besides the several architects, engineers, and others engaged in the construction of buildings, who have more particularly profited by Mr. Tredgold's works, and who are, consequently, the more indebted to him, the appeal has been directed to the promoters of our manufactures and commerce,—to those who are most interested in the improvement of our engines, our machinery, our means of internal communication,—and to the eminent societies in which the talent of the country is concentrated.

The patrons of merit among the noble and the affluent, and all who feel an interest in our intellectual progress as a nation, while they look with admiration upon the wrecks and fragments of perfection in art descending down to us with the fame of the master-minds which produced them, from the great nations of antiquity, may feel some gratification in the reflection, that, however we may suffer in the comparison of the Fine Arts, we stand unrivalled by any age, or by any country. in the creation, the improvement and the application of an astonishing power, resulting from the efforts of those followers of Science of whom Great Britain is justly proud.

Not among the least of these stood poor Tredgold, (if the departed possessor of genius and worth may be called *poor,*) whose exertions have so eminently contributed, in connexion with other honoured names, to render science a living letter, by applying it to " the business and affairs of men ;" and the reflecting portion of the British nation is directed to this example of native talent, as possessing powerful claims upon the exercise of its characteristic liberality, destitute though it be in the associations so successfully eloquent for the sufferers in literature, in art, or in the pursuits administeriug to our refined amusements.

In offering itself to the notice of the members of the Legislature, the present appeal would suggest for fut.re consideration, whether those who have devoted their attention, and in many cases sacrificed their lives for the public welfare, in works for its safety, its health, and its convenience, are not deserving of public reward.

M. CAUCHY ON THE UNDULATORY THEORY OF LIGHT.

The slowness with which continental researches find their way into England has been in more than one instance singularly exemplified, and especially with regard to optical researches. The profound investigations of M. Cauchy were, it is believed, for the first time announced to the English scientific world in the Report on Optics, by Sir David Brewster, read to the British Association at Oxford in 1832, two years after the latest part of them had been completed. At the present time

* In order to promote this benevolence, we shall place the List of Subcribers on the cover of a future Number.—Edit.

it is believed that the particulars of those investigations remain nearly unknown, even to many of the cultivators of science who are most given to such subjects in this country. The investigations themselves are somewhat abstruse, and are unfortunately scattered through various memoirs; but they contain the development of a principle which divests the undulatory hypothesis of the only great and hitherto insurmountable objection, viz. its deficiency in the explanation of the unequal refrangibility of light. It is presumed, therefore, that an abstract of these important researches, compiled from the several memoirs of M. Cauchy, may not be without its use to the English student. Professor Powell, of Oxford, has, accordingly, been for some time engaged upon the subject, and intends shortly printing a concise and connected view of the theory in question, in which expressions are deduced of very little more complexity than in the ordinary theory, yet including the considerations which give rise to the relation between the lengths of waves and the refrangibility.

DESICCATION OF CHLORIDE OF SILVER.

Chloride of silver, as Dr. Prout informs me, invariably gives out muriatic acid at a certain stage of drying; and he suggests whether the loss may not be sufficient to influence the atomic weight of chlorine. Into this point I have carefully examined. Nitrate of silver was precipitated by muriatic acid in excess, was well washed with warm water, and set to drain in a dark closet. Exposed to light it acquired an acid reaction; and heated in contact with litmus-paper, the colour was reddened, even though not at the same time under the influence of light. A portion was introduced into a clean retort, and heat applied so as to dry it in that situation, day-light being excluded: the water which thus came over was quite neutral, and the chloride was at length left quite white and dry, without a trace of acid being lost. The same experiment was repeated with the same result. I hence consider it as certain that pure chloride of silver may be completely dried at 300° Fahr. without loss of any acid, if light and organic matter be excluded. On heating this dry and white chloride in a test-tube, a portion of acid sufficient to redden litmus was given out, just as the colour of the chloride darkened in the act of fusing. This phænomenon is constant. To try the amount of the loss, a quantity of pure chloride of silver was well dried at 300° Fahr.; and in one experiment forty-one grains, and in another ninety grains, were fused in a platinum crucible. In neither case did the loss amount to an appreciable quantity: I could not satisfy myself that it reached 0·01 of a grain. In these trials, however, the dry chloride was corked up in the weighing flask while still warm; for if allowed to cool in the open air, it absorbs a little air and moisture, and then on being fused a slight loss is perceptible. These experiments have been preferably made with chloride of silver thrown down from the nitrate by muriatic acid; the results are similar with any pure chloride; but when precipitated by sea-salt, it is apt, unless very carefully washed, to retain a little chloride of sodium, and then I believe the developement of acid is more easy then when the chloride of silver is quite pure.—*Phil. Trans.* 1833, Part II. pp. 534—5.

Summary of the State of the Barometer, &c., in Kendal, 1833.

Months.	Barometer.			Thermometer.			Quantity of Rain in Inches.	Wet Days.	Prevalent Winds.
	Max.	Min.	Mean.	Max.	Min.	Mean.			
Jan.....	30·47	29·25	30·02	41°	19°	31·46°	1·628	5	W.
Feb.....	29·92	28·58	29·21	48	28·5	39·78	4·582	22	W.
Mar.....	30·16	28·52	29·69	51·5	28	38·66	2·070	12	N.E.
April..	30·07	28·84	29·52	60·5	33	44·91	3·754	22	S.W.
May ...	30·26	28·99	29·89	69·5	38	57·37	2·534	11	W.
June...	30·03	29·26	29·56	73	36	55·72	7·715	20	W.
July ...	30·28	29·33	29·79	72·5	45	59·72	2·333	14	W.
Aug.....	30·27	28·96	29·76	69	40·5	55·35	1·966	11	N.
Sept....	30·18	29·03	29·68	67	35	52·65	3·527	13	W.
Oct. ...	30·04	28·88	29·59	61	28	47·76	3·752	15	W.
Nov....	30·05	28·50	29·61	56·5	26	42·21	7·438	21	W.
Dec. ...	29·84	28·77	29·33	51·5	27·5	41·75	14·219	27	W.
Annual Means, &c.	30·13	28·91	29·64	60·08	32·04	47·03	55·418	193	W.

The annual mean of the barometer is very near the average of the last 11 years, and the mean temperature under that of the same period. The quantity of rain is within an inch of the average for the preceding 11 years, this average being 56·309 inches. In four years only out of the preceding 11 was there less than in 1833, viz. 1826, 1828, 1829, and 1832. The year 1826 was by far the driest year, both as regards the number of rainy days and the quantity of rain measured, the quantity being but 43·060 inches. In 1822, 1823 and 1824, we had 62·726, 62·749 and 62·762 respectively. In 1823 there were 198 rainy days, and in 1826, 147. But in the period alluded to there has been no year, except 1823 and 1830 (in which 194 wet days occurred), equal to 1833 in the number of rainy days. Till the middle of October, or even the beginning of November, we had much less than our usual quantity of rain, since which time there has been almost continued rain, for out of the 61 days in November and December, we had 48 rainy days, 27 of which occurred in December. In these two months, it will be observed, we had 21·657 inches, a quantity equalling, if not exceeding, the annual amount which falls in the neighbourhood of the metropolis.

During the year 1833, the quantity of rain which fell at Birklands, the residence of E. W. Wakefield, has been measured, with an account of which he has obligingly furnished me. The situation of Birklands is about a mile from the site of my rain-gauge, and the elevation above it, as measured by the barometer, is 30 yards, and yet, from this variation in the level of the two places, there have been 3·999 inches less rain at Birklands than at Kendal. This circumstance proves, what is already well known, that a less quantity falls in high situations than in the adjacent valleys. Taking, however, the monthly quantities at each place, it does not always happen that a less quantity of rain falls at Birklands than at Kendal; *i.e.* in January, April, May, July, August, and September, (6 months out of the 12,) more rain was measured in each of these months at Birklands than at Kendal. In comparing the quantity taken at each place, it appears that when large quantities of rain fall, the difference is greater than when small ones are measured. In several instances, especially when the quantity is small, E. W. Wakefield takes more than I do in Kendal; but in comparing one month after another, it appears that the *daily* quantity is less at Birklands than at Kendal. The apparent discrepancy between the two accounts which sometimes occurs in the daily quantity of rain taken, under the same circumstances, arises, I am inclined to think, from our measuring the quantity at different hours, probably when heavy rain is falling, by which means, he may appear to take a larger than I have done on any particular day, from including one or even two hours more rain. In the six months in which the rain taken at Birklands exceeds that at Kendal, the weather was less windy; but I am not sure that this circumstance will satisfactorily account for the increased quantity. In the wettest month, December, the quantity measured at Birklands was 12·390 inches, or nearly two inches less than at Kendal, and in November 6·308, or above one inch less. These were by far the most windy and boisterous months in the year, as the dreadful loss of shipping on our coasts lamentably proves.

As I am aware that considerable interest exists in different parts of the kingdom among scientific men respecting the meteorology of Kendal, I annex an account of the quantity of rain, which I have carefully measured during the last 12 years, the average of which period may be fairly considered as the mean quantity which falls in Kendal. In 1822 there were measured 62·726 inches.

1823	62·749
1824	62·762
1825	59·973
1826	43·060
1827	58·006
1828	54·816
1829	46·173
1830	58·030
1831	61·416
1832	49·688
1833	55·418

the average of which is 56·235 inches. On reviewing the past year, I may remark, that January was an unusually dry month, five days of which only were wet ones. February was a very wet one. The next three months were particularly dry, and the weather genial. The greater part of June was wet, but July, August, September, and the early part of October, were dry, and we had no frost from the 30th of March to the 9th of October. The remaining part of October, and nearly the whole of November and December, were extremely wet. We have not had many appearances of the aurora borealis during the year. The first appearance of it was on the 12th of September. In that and the following month it was repeatedly seen, and once in December. SAMUEL MARSHALL.

Kendal, 19th 2nd Month, 1834.

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. R. HOCKING at Penzance, and Mr. VEALL at Boston.

Days of Month. 1834.	Barometer.					Thermometer.					Wind.			Rain.		
	London.		Penzance.		Boston 8½ A.M.	London.		Penzance.		Bost. 8½ A.M.	Lond.	Penz.	Bost.	Lond.	Penz.	Bost.
	Max.	Min.	Max.	Min.		Max.	Min.	Max.	Min.							
Mar. 1	30·473	30·403			29·92	56	45			47·5	W.		W.	...		...
☾ 2	30·410	30·301			29·75	56	47			50	SW.		calm	...		...
3	30·417	30·293			29·95	57	43			36	S.		calm	...		...
4	30·162	29·962			29·56	56	44			47	S.		W.	...		...
5	29·793	29·569			29·15	57	50			54	SW.		W.	0·10		...
6	30·039	29·985			29·36	57	39			45	SW.		W.	·01		0·03
7	30·314	30·230			29·52	60	43			46	SW.		W.	·04		·04
8	30·386	30·321			29·64	59	44			51	S.		W.	...		...
9	30·496	30·463			29·84	59	41			50	W.		calm	...		...
● 10	30·403	30·338			29·77	59	39			50	SW.		NW.	...		...
11	30·504	30·496			29·88	56	32			49	E.		calm	...		...
12	30·524	30·451			29·98	57	43			48·5	W.		calm	·03		...
13	30·432	30·382			29·97	50	30			47	E.		calm	...		·04
14	30·463	30·375			29·92	53	40			45	NE.		calm	...		...
15	30·569	30·547			30·11	47	28			43	NE.		calm	...		·02
16	30·575	30·491			30·08	53	33			40	E.		calm	...		...
17	30·523	30·419			30·04	45	34			45·5	NE.		calm	...		...
☽ 18	30·587	30·552			30·15	46	24			42	NE.		calm	...		...
19	30·596	30·537			30·10	47	26			41	E.		calm	...		...
20	30·491	30·468			30·09	47	25			41·5	E.		calm	...		...
21	30·459	30·343			30·03	48	35			38·5	SE.		calm	...		...
22	30·260	30·125			29·70	54	36			43	SW.		W.	·01		...
23	30·058	29·770			29·46	52	44			44	NW.		W.	..		·02
24	29·877	29·814			29·32	52	31			45	NW.		NW.	...		...
○ 25	30·117	29·899			29·40	52	25			36·5	N.		N.	...		...
26	30·200	30·185			29·77	52	35			38	NW.		calm	...		...
27	30·129	30·008			29·60	57	44			50	W.		calm	·04		...
28	29·814	29·559			29·27	51	40			48	SW.		calm	·10		...
29	29·814	29·687			29·16	53	30			45·5	SW.		calm	·03		·12
30	29·946	29·896			29·47	54	36			45	W.		calm	·50		...
31	30·026	29·892			29·42	53	35			41	W.		NW.	...		·09
	30·596	29·559			29·72	60	24			44·9				0·86		0·36

Remarks.

London.—March 1. Cloudy. 2. Fine. 3. Hazy. 4. Very fine. 5. Windy, with rain at night. 6. Fine. 7—11. Very fine. 12. Foggy: fine. 13. Drizzly. 14. Frosty: fine. 15. Clear, cold, and dry. 16. Fine: very clear at night. 17. Cloudy and cold. 18. Cold and dry. 19. Sharp frost: cold and dry: frosty at night. 20. Cloudy: clear and frosty at night. 21. Hazy and cold. 22, 23. Fine. 24, 25. Cold and dry. 26. Sharp frost: very dry. 27. Cloudy and fine. 28. Overcast: rain. 29. Fine. 30. Fine: rain at night. 31. Fine.

Boston.—March 1, 2. Cloudy. 3. Foggy. 4. Cloudy. 5. Rain. 6. Fine. 7. Fine: rain early A.M. 8. Stormy. 9. Cloudy. 10, 11. Fine. 12. Cloudy: rain A.M. 13. Cloudy. 14. Cloudy: rain A.M. 15, 16. Fine. 17, 18. Cloudy. 19—21 Fine. 22. Cloudy: rain P.M. 23. Cloudy. 24, 25. Stormy. 26, 27. Fine. 28. Cloudy: rain P.M. 29. Fine. 30. Fine: rain P.M. 31. Fine.

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[THIRD SERIES.]

JUNE 1834.

LXIV. *Reply to* Mr. Phillips's *Additional Observations on Chemical Symbols. By* THOMAS GRAHAM, *Esq., M.A., F.R.S.E., Lecturer on Chemistry in the Andersonian Institution, Glasgow.*

To Richard Phillips, Esq., F.R.S., &c.

Dear Sir,

I SHALL endeavour to supply the information which you consider still required to render my paper on the phosphates intelligible, in a brief summary of its principal theoretical results. For the evidence upon which the conclusions rest, I must refer entirely to the paper itself, which is now before the public.

Two phosphoric acids had been previously recognised, which possess very different properties, judging from the characters of the salts which they form with the same bases. These two acids were said to be *isomeric* bodies, or to be of the same composition and atomic weight, and were distinguished as the *phosphoric* or *common phosphoric* acid, and the *pyrophosphoric* acid. The observation was made by me that there is a *third* phosphoric acid, which may be produced by heating the biphosphate of soda to redness, and by other processes; and to which I applied provisionally the name *metaphosphoric acid*. But on pursuing the investigation, a key to the whole mystery of these compounds was discovered, by means of which the perplexing mutations to which the acids and their salts are subject might, it appeared to me, be explained in a satisfactory manner.

There is, I suppose, only one phosphoric acid, an integrant particle of which may be represented as consisting of

One atom of phosphorus..................	392·3
Five atoms of oxygen	500
	892·3

and of which the formula is $\overset{\cdot\because\cdot}{P}$. But this acid is capable of combining intimately with water in three different proportions, viz. one atom of acid, with one, two and three atoms of water respectively; and each of these hydrates is capable of dissolving in a large quantity of water without change. Now solutions of these hydrates possess respectively the properties of metaphosphoric, pyrophosphoric and common phosphoric acids. Hence, if we retain these names (and any immediate change in the nomenclature of the phosphates, however faulty it may be, is of doubtful propriety,) the acids are composed as follows:

	Phosph. Acid.		Water.	In Atoms. Acid.		In Atoms. Water.	Formula.
Metaphosphoric acid.........	892·3	...	112·5	1	...	1	$\dot{H}\overset{\cdot\because\cdot}{P}$
Pyrophosphoric acid.........	892·3	...	225·	1	...	2	$\dot{H}^2\overset{\cdot\because\cdot}{P}$
Common phosphoric acid...	892·3	...	337·5	1	...	3	$\dot{H}^3\overset{\cdot\because\cdot}{P}$

When these acid hydrates are treated with alkali, the whole combined water does not follow the acid into the salt formed, and cause the differences between metaphosphates, pyrophosphates and phosphates, as might, perhaps, be expected. The water in these hydrates discharges the function of a base to the acid, and, on the addition of an alkali, is *displaced* by an equivalent quantity of the stronger base. Thus, when we treat a solution of common phosphoric acid ($\dot{H}^3\overset{\cdot\because\cdot}{P}$) with caustic soda, the three atoms of water are displaced by three atoms of soda, and a salt results, of which the formula is $\dot{N}^3\overset{\cdot\because\cdot}{P}$. When pyrophosphoric acid ($\dot{H}^2\overset{\cdot\because\cdot}{P}$) is treated in the same way, the two atoms of water are replaced by two atoms of soda, and no more, and the salt $\dot{N}^2\overset{\cdot\because\cdot}{P}$ results. Sometimes, however, the water in the acid hydrate is only partially displaced, as when we treat $\dot{H}^3\overset{\cdot\because\cdot}{P}$ with carbonate of soda; then only two of the three atoms of water are supplanted by soda, and a salt is formed consisting of two atoms of soda, one water and one acid, $\dot{N}^2\dot{H}\overset{\cdot\cdot\cdot\cdot\cdot}{P}$. This is common phosphate of soda; and the one atom of basic water which it contains may yet be supplanted by treating the salt with caustic soda; whence results subphosphate of soda, $\dot{N}^3\overset{\cdot\cdot\cdot\cdot\cdot}{P}$. In fine, there appear to be three classes of phosphates derivable from the three hydrates of phosphoric acid; and every member of the same class contains the same number of atoms of base. In the following Table this view of the modifications of phosphoric acid is exhibited, and also the salts of soda which the acid forms. The names, when not new, are applied to the salts which they have hitherto designated.

First class, or phosphates containing three atoms of base.

	Acid. Water. Soda.	In Atoms. *Acid. Water. Soda.*	*Formula.*
Common phosphoric acid	892·3...337·5... 0·	1... 3 ...0	$\dot{H}^3\overset{\cdot\cdot\cdot\cdot\cdot}{P}$
Biphosphate of soda......	892·3...225· ...390·9	1... 2 ...1	$\dot{N}\dot{H}^2\overset{\cdot\cdot\cdot\cdot\cdot}{P}$
Phosphate of soda.........	892·3...112·5...781·8	1... 1 ...2	$\dot{N}^2\dot{H}\overset{\cdot\cdot\cdot\cdot\cdot}{P}$
Subphosphate of soda....	892·3... 0·...1172·7	1... 0 ...3	$\dot{N}^3\overset{\cdot\cdot\cdot\cdot\cdot}{P}$

Second class, or phosphates containing two atoms of base.

	Acid. Water. Soda.	In Atoms. *Acid. Water. Soda·*	*Formula.*
Pyrophosphoric acid	892·3...225· ... 0·	1... 2 ...0	$\dot{H}^2\overset{\cdot\cdot\cdot\cdot\cdot}{P}$
Bipyrophosphate of soda	892·3...112·5...390·9	1... 1 ...1	$\dot{N}\dot{H}\overset{\cdot\cdot\cdot\cdot\cdot}{P}$
Pyrophosphate of soda...	892·3... 0· ...781·8	1... 0 ...2	$\dot{N}^2\overset{\cdot\cdot\cdot\cdot\cdot}{P}$

Third class, or phosphates containing two atoms of base.

	Acid. Water. Soda.	In Atoms. *Acid. Water. Soda.*	*Formula.*
Metaphosphoric acid...	892·3...112·5... 0·	1... 1 ...0	$\dot{H}\overset{\cdot\cdot\cdot\cdot\cdot}{P}$
Metaphosphate of soda	892·3 0·...390·9	1... 0 ...1	$\dot{N}\overset{\cdot\cdot\cdot\cdot\cdot}{P}$

I was restrained from proposing any alteration in the nomenclature of these salts, from the difficulty of deciding at present how far the change should be carried. If we confine our attention to the soda in the first class of salts, it is evident that the biphosphate should be called the phosphate, the phosphate be called diphosphate, and the subphosphate triphosphate. But such a change in the names of common salts would be attended with great inconvenience, and still leave defects unamended. It is not worth while to alter the names till we can construct them so as to indicate the constitution of the salt in reference to *water* as well as to soda. Now here we find our present system of nomenclature at fault. Such combinations as we have to name were never contemplated, and are not provided for. The function of water in the constitution of the salts has been hitherto almost entirely overlooked. I am now prepared to show that its interference is not confined to the phosphates; that it discharges an equally important function in other classes of salts. It modifies the constitution, likewise, of many metallic peroxides,—of the peroxides of tin, for instance,—and supplies the explanation of many cases of apparent isomerism.

It is certainly one great advantage of chemical formulæ that they may be made to compensate to a certain extent for the increasing deficiencies of the nomenclature. From new light thrown upon the constitution of some familiar salt, its name ceases to indicate its received constitution, or even belies it. In such cases, where it may be inexpedient to change the name, it may be qualified when used by subjoining in parentheses the correct formula of the salt. I confess that I underrated the advantages of chemical notation till its use was forced upon me in the study of the phosphates. But now I am satisfied that to neglect it would be voluntarily to forgo a most valuable aid in the conception and exhibition of chemical relations. I remain, Dear Sir,

With much respect, yours truly,

Glasgow, April 9, 1834. THOMAS GRAHAM.

LXV. *Inquiry how far the Theory of* M. Elie de Beaumont *concerning the Parallelism of Lines of Elevation of the same Geological Æra, is agreeable to the Phænomena as exhibited in Great Britain. By the Rev.* W. D. CONYBEARE, *M.A., F.R.S. V.P.G.S. Instit. Reg. Soc. Paris.*

[Concluded from vol. i. p. 126.]

Supplement to III.—ELEVATIONS during the interval between the close of the æra of the carboniferous formations and the commencement of that of the new red sandstone and dolomite, which may be shortly called the Antedolomitic æra.

If we examine our principal coal-fields, *e.g.* (proceeding from S. to N.) the Bristol coal-basin, that of the Forest of Dean, that of South Wales, the coal-fields of Shropshire and Flintshire on the eastern edge of the transition chains of

North Wales, those of Central England at Dudley, in the N.E. of Warwickshire and at Ashby, those on both sides of the great Penine chain on the east in Derbyshire, Yorkshire, Durham, and Northumberland, and on the west in Lancashire, and again mantling round the Cumbrian group to Whitehaven, we shall find the subjacent old red sandstone and carboniferous limestone, and all the coal-measures, more or less elevated, generally very considerably so, and the stratification of this whole series generally conformable; whereas the lower strata of the new red sandstone and the intervening dolomitic limestone will be found comparatively almost horizontal, and little disturbed, and altogether unconformable to the carboniferous series; so that it should appear that the principal æra in which the disturbing forces which have produced these phænomena acted, was at the very close of the carboniferous period, and anterior to that of the dolomitic formation, although it may, indeed, be said that this may with equal propriety be assigned as the concluding æra of a succession of elevations acting with a general uniformity of direction during the whole period of the deposition of the carboniferous rocks rather than one single violent elevation; and certainly the phænomena of the exact conformity of the whole coal series has scarcely hitherto been so minutely examined as to authorize our pronouncing very positively which of these hypotheses seems best to explain them, although, from the general conformity which I have noticed, I should certainly incline to adopt that of a single period of elevation. The continental coal-basins of the Meuse, and many of those of Northern Germany, appear to have been elevated at exactly the same period. The lines of these elevations are, however, assuredly not parallel, for those in the south portion of our island generally range east and west, and those in the north range north and south, forming tangents to the curve which, as I have already noticed, expresses the general line of the elevation of our strata of whatever age. But to proceed regularly with the detail.

The carboniferous limestone first shows itself on the southern coast along the skirts of Torbay, Devonshire: it is here greatly elevated and contorted, and we may refer to the intrusive masses of greenstone as the probable cause of the disturbance. The lowest beds of the Exeter conglomerate, usually classed with the new red sandstone, also partake in this disturbance, which might seem to refer it to a more recent period than that of which we are now treating; but yet it does not appear altogether improbable that these may be the very lowest deposits of this great series, and possess a comparative antiquity greater than that of the Pontefract sandstone and dolomitic limestone, which are usually undisturbed; a supposition which would enable us to refer these disturbances to the æra we are now considering. It seems probable, but by no means certain, that the elevation of the transition chains in North Devon and on the adjoining borders of Somerset, such as the Brendon and Quantock Hills, may be of this æra. The carboniferous limestone of Cannington park, on the north of the Quantocks, appears to conform to the elevation of that chain, which may incline us to adopt this age.

Entering on the Bristol coal-field we find its southern bordering chain, the Mendips, composed of carboniferous lime with an axis of old red sandstone highly elevated, the disturbances equally affecting the incumbent coal-measures, but leaving the dolomitic conglomerate undisturbed and nearly horizontal, so that the age is here distinctly marked. This line of elevation ranges nearly E. and W. a little inclining to W.S.W. and E.N.E.: it is prolonged by the Isle of Steepholm in the Bristol Channel. The elevation bounding the coal-basin on the west is more complicated, being associated with parallel undulations and disturbed by faults. It is continuously prolonged in a direction somewhat tortuous, but generally bearing from S. to N. into the centre of Herefordshire; and as it is obviously of the same age with the Mendip elevation, which ranges E. and W. affecting equally the coal-measures and all the older rocks, but leaving the younger dolomitic conglomerate and new red sandstone undisturbed, it seems to afford a convincing example that disturbances of the same age do not uniformly pursue parallel lines: thus, it reaches Shackwell hill, 7 miles east of Hereford, where it inosculates with the anticlinal line which forms the western border, as the above does the eastern, of the Forest of Dean basin. This ranges southwards from Westhide by Kenchurch, where it crosses the Manno River, as it does the Uske, a little west of Ryland, throwing up the transition limestone thence to Uske. A few miles south-east of this place this line, which has hitherto ranged north and south, trends towards the west, hence forming the southern boundary of the South Welsh coal-basin. Thus it affords us an example, not simply of lines of elevation of the same age, but of the same individual line, which, after pursuing a course in one direction for 50 miles, curves round, and adopts for 100 miles a course exactly at right angles to the former. It ranges north of Caerleon by Llandaff to Cowbridge: thence ranging by Kenfig, it crosses Swansea Bay to Gower, where it throws up the old red sandstone central chain of the peninsula of Gower, thence crossing to Tenby, where we have two parallel undulations of carboniferous limestone and old red sandstone. From Pembroke this line crosses Milford Haven, still proceeding westwards. Between the haven and the Irish Channel intrusive masses of trap rocks penetrate the strata, which are here very remarkably contorted: these may, perhaps, be considered as indicating the probable volcanic origin of these disturbances. The whole of these phænomena are fully described in Mr. De la Beche's memoir in the Geological Transactions, and most accurately represented in the accompanying plates. The age of all this line is clearly defined by the contact of the disturbed coal and undisturbed dolomitic conglomerate in Glamorganshire. To trace it more particularly: its southern extremity forms a double line of elevation, including the Nailsea coal-field; between them the southern of these anticlinal lines ranges from the Island of Flatholm in the Bristol Channel through Broadfield down to Leigh Down, where it is continued by the great fault of the carboniferous limestone crossing the Avon at St. Vincent's Rocks, nearly in the same line with which we trace an anticlinal line crossing the centre of the coal-field and throwing up its lowest grits on the north of Kingswood. The range of this southern portion of the double line is S.W. and N.E. The northern portion at first seems to have its axis beneath the Severn, being connected with a tremendous fault, which on the north of Clevedon has thrown down the coal-measures more than 1000 feet, so as to bring them into contact with the old red sandstone range at the foot of Leigh Down, causing the carboniferous limestone and old red sandstone again to crop out in one place at a superficial distance of 3 miles from this natural line. Beyond this subsided tract the anticlinal line is continued from Portishead Fort across the Avon, near Sneyd Park; thence northerly to Thornbury and Berkeley, between which places the transition limestone is elevated, apparently by the intrusive trap dykes of Tortworth: here it crosses the Severn, from hence forming the eastern boundary of the Forest of Dean coal-basin, as it has before formed the western boundary of that of Bristol. It ranges by Nuneham, throws up the transition quartz rocks of May Hill, and the transition limestone chain of hills which ranges between the river Wye.

The interior of the coal-basin in Glamorganshire is affected by another anticlinal line, nearly parallel to the former: this commences near Bedwas in Monmouthshire, ranges by Cefn Eglwysilon, crosses the Taafe near Newbridge, and thence ranges west to the iron-works at Duffryn Llanry, and is cut off by the æstuary of the Neath at Britton Ferry.

The strata on the north edge of the South Welsh coal-basin rise towards the general elevation of the subjacent transition chains, which, however, appear often unconformable: indeed Mr. Murchison has informed me that even the old red sandstone near Castle Carregkennon is unconformable, being nearly vertical, while the incumbent carboniferous lime is not inclined above 35°.

The north edge of the coal-basin is affected by considerable faults ranging E. and W.: one throws up the carboniferous limestone 4 or 5 miles within its general outcrop from Penderyn to the arched strata of Bwa Maln near Pont Nedd Vechon at the head of Cwm Neath. Similar to this, but on a smaller scale, is the fault at Cribborth lime rock in the upper part of Cwn Tawe, where an interval of three quarters of a mile takes place between the two outcrops of the limestone.

The line of elevation of the Malvern Hills, also N. and S., may, I apprehend, be certainly referred to this same æra, being evidently anterior to the deposition of the new red sandstone which skirts its base horizontally. Its main mass is a protrusion of sienitic rocks, which apparently have been the agents in effecting this disturbance, which throws up at a high inclination the transition limestone covering its western slope. The transition limestone of the Abberley Hills in the prolongation of this line has apparently been elevated by the same convulsion; but I am at present unable to state in what manner the coal-fields of Pensax and Billingsley, at the north end of this line, are affected.

The coal-field of Coalbrook Dale and the subjacent transition limestone of Wenlock Edge appear in like manner to have been elevated by the protrusion of the trap rocks of the Wrekin chain, and at the same period, that is, anterior to the æra of the new red sandstone: the line of direction is N.E. and S.W.

The Flintshire coal-field and its subjacent carboniferous lime repose conformably on the exterior chains of the North Welsh transition slates, so that the same forces of elevation must have affected all the members of these exterior chains; but, as before, anteriorly to the æra of the new red sandstone, the line of direction again differs from the Wrekin chain, being N.W. and S.E.

If we pass to the central coal-fields, we find that of Dudley reposing on the transition limestone elevated by a high anticlinal line ranging N. and S., with a slight tendency to N.E. We naturally incline to refer this convulsion to the eruptions which have left the overlying mass of trap on the south. Dykes of the same rock have been found intersecting the coal in Tividale. The elevated quartz rock at the foot of the Bromsgrove Lickey, which is in the continuation of this line, also ranges N. and S., and presents shattered masses of transition lime and trap on its sides. Trap rocks occur here also; the surrounding new red sandstone is horizontal and undisturbed. The Warwickshire coal-field, between Coventry and Tamworth, together with a subjacent narrow tract of quartz and grauwacké on its north-eastern border, is elevated in a line of direction N.N.W. and S.S.E. Greenstone dykes intersect the subjacent grauwacké at Griff near Bedworth.

The elevation of the sienitic and schistose ranges of Charnwood Forest may next be noticed; for as the contiguous coal-measures at Grace Dien, and the magnesian beds of the carboniferous limestone at Breedon, appear to have been affected by the same convulsion, while the surrounding new red sandstone is undisturbed, this elevation must be referred to the æra we are now considering. In this view it may be compared to the eruption of the Malvern sienite and the various trap rocks of the Wrekin chain, which we have already referred to the same age*.

We now proceed to the great central ridge of the northern counties: this, as is well known, presents a central line of carboniferous limestone, ranging N. and S., having lateral zones of coal-measures on the east and west, which are overlaid on the east by the regular zone of magnesian limestone, as also on the west, if we consider this system as extending round the northern edge of the transition chains of Cumberland to Whitehaven. The magnesian limestone is separated from the coal-measures by a bed of sandstone representing the *rothe todte* of Northern Germany. Now as the magnesian limestone and its subjacent *rothe todte* repose unconformably on the elevated coal-measures, it is obvious that this elevation in a great measure took place anteriorly to the deposition of these superstrata. But again, as the *rothe todte* is often inclined when the superincumbent magnesian conglomerate and limestone is horizontal, we must suppose a second æra of convulsion posterior

* The first appearance of these rocks at Mount Sorrel exhibits a low ridge of well characterized sienite bursting through the red marl. A flat tract, concealed by this red marl, separates the Mount Sorrel ridge from the Swithland slate district on the west. This slate has rather the characters of greenstone slate than grauwacké: it alternates with and passes into greenstone and greenstone porphyry. The sienite presents several insulated portions emerging through the red marl south of Leicester along the western bank of the Soar, viz. Enderby Croft hill, beneath which the Soar flows through a defile of sienitic rocks, forming a sort of spur proceeding from the hill; Stony Stanton, where undisturbed beds of the red marl may be seen close to the sienite north of the village; and Shopcot, about a mile further south. The best account of Charnwood Forest will be found in the Annals of Philosophy for January 1824.—[See also a notice of Prof. Sedgwick's late examination of Charnwood Forest, in our Number for January last; present vol. p. 68.—Edit.]

to the age of the *rothe todte*, and yet anterior to that of the magnesian limestone; and still further, as we find the 90-fathom dyke of Northumberland throwing down likewise the magnesian limestone, we must here have recourse to a third period of convulsion. The main direction of elevation, and that which, as we have seen, must be referred to the æra of which we are now especially treating, ranges N. and S. Of the direction of the second, we have no sufficient information: the great fault exemplifying the third ranges east and west. But before we can fully pronounce as to the æras of the several disturbances of this great chain, this very interesting portion of our country requires a much more minute investigation. It may perhaps not be altogether useless, at a meeting like the present, to suggest some of the objects of inquiry: 1st, according to Farey, the limestone district of Derbyshire forms a vast elevated tract, bounded on three sides, the north, west and south, by a vast fault, which depresses the superincumbent shale of the millstone grit formation into contact with the whole series of the subjacent limestones successively, until at length along the western border the lowest limestones abut against these superior beds. But the existence of this fault still requires more accurate examination: we do not know how far the general inclination of the strata accords with this supposition. Should it be found, as is not improbable, to have been assumed in order to explain the position of the masses of toadstone, taking these for regular and conformable beds, will it not be much more probable that these intrusive rocks are absolutely unconformable to the disposition of the strata on the great scale, and must, therefore, afford the most fallacious evidence as to the general structure of the district?*

The fault traversing the coal-measures N. and S., nearly in the line of the basset edge of the magnesian limestone which Mr. Farey describes as the great zigzag fault, also stands in need of corroborative evidence.

Yet is there nothing in the existence of these faults at all incompatible with what we certainly know concerning other portions of this central ridge; for proceeding into Yorkshire, we certainly find where the western escarpment of the carboniferous limestone reposes on the slate beneath Ingleborough a vast fault ranging E.N.E. and W.S.W., throwing down the limestone at Giggleswick Scar, &c., on the south, to the same level as the slate; and a little further south a second parallel fault, also a downcast to the south, causing a subsidence of the coal-measures near Settle to a still lower

[* A portion of this subject has recently been investigated by Mr. Hopkins, a notice of whose results has been given in our Number for January, present vol. p. 66.—EDIT.]

level. We may state generally, that this range of faults extends almost 20 miles from Malhan Tarn to Kirby Lonsdale, where the limestone skirting the Westmoreland slate mountains on the south appears connected with the subsided portion proceeding from Giggleswick Scar*.

North of this, on the borders of Cumberland, we find at the foot of the western escarpment of the great calcareous chain of Cross Fell, a protruding ridge of greenstone rocks at Dufton Pike, &c., attended by shattered portions of carboniferous limestone and coal; and in the great calcareous chain itself we trace several faults, some very considerable, associated with the presence of the great whin sill in Upper Teesdale; and a little further north we have the great Burtreeford dyke (head of Weardale) ranging N. and S., and occasioning a subsidence of 80 fathoms to the west in the different members of the limestone series. At the head of Tynedale we have similar faults, apparently connected with the western prolongation of the great Northumberland 90-fathom dyke; but this, as we have seen, is certainly posterior to the age of the magnesian lime, and we are yet unable to pronounce how far many of those just noticed may not be equally recent, only that it certainly appears that the disturbances connected with the eruption of the trap rocks at Dufton are to be referred to our present æra, being posterior to the coal-measures, which are elevated and shattered, and anterior to the new red, which reposes against these fractured masses horizontally.

Our information is yet imperfect as to the termination of this great calcareous chain against the Cheviot group; still less are we acquainted with the structure and phænomena of the great Scotch coal-field.

IV. Elevations which appear to have affected the transition rocks anteriorly to the deposition of the carboniferous series.

An attentive examination of the phænomena of the great Scotch coal-field, and the relative position of the older rocks near their junction, would throw great light on this part of our inquiry. As to these points our present information is altogether deficient; but the general line of elevation of the southern transition chain clearly ranges E.N.E. and W.S.W. In the Cumbrian Lake district, as the lines of direction of the included transition series of formations are entirely unconformable to the position of the zone of carboniferous limestone which ranges round them, reposing indifferently on the younger member of that series on the S.E. and against the oldest portions of the system on the N.W., we must at once

* Mr. Phillips does not exactly explain whether the calcareous conglomerate of the new red occurring at Westhouses below Kingsdale is or is not affected by these convulsions.

refer the primary elevation of the transition rocks to a period antecedent to the formation of the carboniferous lime. The general direction of these transition lines of elevation is E.N.E. and W.S.W., and they appear to have been attended with great transverse fractures, which have often indicated the course of the actual valleys.

In the Island of Anglesea the whole of the transition chains are highly inclined and much disturbed, while the old red sandstone, mountain lime and coal repose on their edges in nearly horizontal strata. The lines of direction are here N.E. and S.W.: this is also the general line of the transition chains of North Wales. To this direction it is evident that the line of the border of carboniferous limestone on the north, proceeding from Ormes Head, is entirely unconformable. The same N.E. and S.W. direction seems to prevail also in the transition chains of South Wales, while the lines of elevation of the incumbent coal-measures range E. and W.; and in the Vale of Towy, while we find the transition rocks on the northern bank nearly vertical, the carboniferous limestone chains on the south do not deviate above 10° from being horizontal. The general range of the transition chain occupying the peninsula of Devon and Cornwall ranges also from N.E. to S.W.; but here we have scarcely any opportunity of ascertaining the relative position of those rocks as regards the carboniferous series, for the overlying mantle of new red sandstone is the only rock we observe in contact with their exterior border; unless, indeed, the limestone of Torbay, as Mr. De la Beche has stated, be carboniferous. But this district apparently has been subject to local convulsion at a much later period, from the intrusion of greenstone dykes, so as to afford no precise indications as to the general structure.

The general parallelism of these transition chains, all nearly ranging E.N.E. and W.S.W., is, as Elie de Beaumont has well urged, very favourable to his theory: he classes this as his first system, including the continental chains of the Hundsruck.

In conclusion, I would inquire whether we are to refer this general direction of the Cornubian transition chain to the eruption of the granitic masses which are constantly protruding through its axis. In this case we must refer that eruption to a period so late as to include *almost** the whole series of transition rocks which appear to be equally effected by it.

I have much preferred in this essay the endeavour to

* De Beaumont has, however, considered its precise æra to have been anterior to the formation of the more recent members of the transition series, such as the transition limestones of Dudley, Malvern and Wenlock Edge.

collect the scattered evidence we possess on the subject to which it refers, and to indicate the points in which that evidence is defective, than the attempt to deduce certain conclusions from materials so avowedly imperfect; and I will therefore content myself at present with observing, that while the evidence of the parallelism of the principal transition chains appears undoubtedly favourable to M. de Beaumont's hypothesis, I am altogether unable to reconcile to that hypothesis the facts I have stated, with regard to the anticlinal lines bounding the coal-fields of South Wales, the Forest of Dean, and Bristol, where we have seen the very same lines of elevation inflected from an E. and W. to a N. and S. bearing: and I would further observe, that to me the subject appears to embrace a much wider field of consideration; we have not only to consider the results of the more violent paroxysmal convulsions which have affected the strata at different epochs, but the more general and gradual elevation which the phænomena of the post-carboniferous rocks indicate to have proceeded very gently, and to have produced very little relative disturbance in the strata so elevated.

Finally, when the theory of parallel chains with reference to the points of the compass is so extended as to embrace half the circumference of the globe, (as is done in the conclusion of M. de Beaumont's memoir*,) I would inquire, Do we not require a more exact definition of the sense in which the term parallel is here used? The parallels of latitude are, indeed, strictly parallel lines; but the meridians are great circles passing through the centre of the planet, and no two of them can possibly be parallel to one another: thus, an east and west chain in central Africa and Iceland are parallel, in every acceptation of the word; but north and south chains in the old and new continents, the Ural mountains, for instance, and the Cordilleras of the Andes, are not parallel in the same sense; the two systems of lines in no manner bear the same relations to the spheroidal form of the planet, the meridional system having constant reference to its centre, which the parallels of latitude always neglect. If, therefore, we are to receive the idea of parallel chains, not as strictly parallel to each other, but as having always similar bearings with regard to the points of the compass, must we not further demand some explanation why north and south chains are thus determined in their elevation by their relations to the centre of the planet, while the east and west chains do not follow that relation?

[* See Phil. Mag. and Annals, N.S. vol. x. p. 259, in an extract from M. de Beaumont's Essay.—EDIT.]

I would also observe, that I have never in my life seen an anticlinal line of any extent which followed a direction mathematically straight. Those which have fallen under my own observation, on the contrary, have been always more or less sinuous, although having on the whole a prevailing tendency in one direction for considerable distances; and when we speak of the general bearing of such lines, we intend, as I conceive, to express only this general and average tendency, on either side of which the partial flexures, however, often deviate 20°, or more.

LXVI. *On the alleged Greek Traditions of the Deluge. By the Rev.* John Kenrick, *M.A.**

IT is a very generally received opinion that the same event, the destruction of the human race by a flood, which is recorded with so much minuteness of detail in the Book of Genesis, has been preserved in more vague traditions among all the principal nations of the earth, and especially among the Greeks. Not to mention a crowd of writers by whom this question has been treated in connexion with the evidences of Revelation, the sanction given to the opinion by Cuvier is sufficient to show that it belongs to science as well as to religion. Some of his readers may have thought his arguments less conclusive upon this subject than upon those which lay more immediately within his province, but the majority no doubt have considered his authority decisive.

I propose to arrange chronologically the testimonies of Greek authors to the existence of such traditions of a Deluge: without this we can arrive at no certain conclusion. The coincidences which we may find between Pagan fables and the narratives of Scripture in the second or third century after the Christian æra, can never warrant our inferring their existence a thousand years before it. I confine the inquiry to the Greeks, because the chronology of literature among other ancient nations is very uncertain, and modern accounts of the traditions of barbarous tribes come to us generally through channels not free from suspicion. Cuvier has given such a chronological view (*Discours sur les Révolutions du Globe*, Edit. 1826, pp. 83—87); but as his enumeration is not complete, and the inference which he draws seems the opposite to that to which his authorities point, it is the more necessary to reexamine them.

It is important to fix accurately what it is that we are to

* Communicated by the Author.

inquire about under the name of a *tradition*. From the etymology of the word, we can scarcely divest ourselves, in using it, of some idea of a fact the knowledge of which has been preserved from a preceding age, and that too without the intervention of a written record. Yet even those who speak of traditions have sometimes themselves no belief in the historical reality of the events to which they relate. "The oldest historians," says Mr. Lyell *, "mention a celebrated tradition in Cornwall of the submersion of the Lionnesse, a country which formerly stretched from the Land's End to the Scilly Islands. Although there is no evidence for this romantic tale, it probably originated in some catastrophe occasioned by former inroads of the Atlantic upon this exposed coast." Here *tradition* is evidently used for a statement fictitious in its circumstances, although, perhaps, having some ground in analogy. Cuvier uses the word with the same want of precision. "L'île de Samothrace, l'une de celles où il s'était le plus anciennement formé une succession de prêtresun, culte régulier et *des traditions suivies*, avait aussi un déluge qui passait pour le plus ancien de tous, et que l'on y attribuait à la rupture du Bosphore et de l'Hellespont †." Yet in a preceding page he has given convincing physical reasons from Olivier and Andreossy, why such a discharge of the Euxine, had it ever taken place, could not have caused a deluge in the Archipelago. Notwithstanding the "traditions suivies," therefore, the rupture of the Euxine, though contemporaneous with the supposed deluge, was a fiction. We want some word which, like the German *sage*, should express simply the fact that certain things are *said*, without implying either, like *tradition*, that it is reported on the authority of a preceding age, or, like *legend*, that it is without any authority. In the following inquiry whenever tradition is spoken of, without any epithet, all that is meant is a popular belief, existing at a certain time and place. The existence of this belief is itself a fact; but whether it has been derived from a fact or not, is a distinct question, to answer which we must have recourse to other considerations.

The only two floods respecting which it is worth while to collect the traditions of the Greeks are those of Deucalion and of Ogyges. The others are mentioned so slightly, and by authors so recent, that no stress can be laid upon them.

It is admitted that in the works of Homer, the Hymns as well as the Iliad and the Odyssey, there occurs no mention of Deucalion, nor any allusion to a deluge. Considering the subjects of these poems, however, it would be unfair to argue,

* Principles of Geology, vol. i. p. 282. † *Discours*, p. 87.

from the absence of such allusions, that the story of Deucalion and his flood was unknown to Homer. He might have introduced them without incongruity, for example, in the account of Thessaly in the Catalogue; but there was no necessity that he should do so. The silence of Hesiod is more important. In his Works and Days, the only one of the poems ascribed to him of which the genuineness is unquestioned*, he gives a history of the different races of mankind which had preceded the race of iron, among whom it was his own misfortune to live†. In such a deduction it appears impossible that he could have passed over such an event as the destruction of the human race by a flood, if it had been in his age an article of popular belief. The only thing which even appears like the destruction of a wicked generation is what is said, l. 136, of the silver age:

τοὺς μὲν ἔπειτα
Ζεὺς Κρονίδης ἔκρυψε, χολούμενος, οὕνεκα τιμὰς
Οὐκ ἐδίδουν μακάρεσσι θεοῖς, οἳ Ὄλυμπον ἔχουσιν.

Yet the poet goes on to describe their death in the same words as that of all the other races,

ἐπεὶ καὶ τοῦτο γένος κατὰ γαῖα κάλυψε:

and they become a race of *ἐπιχθόνιοι μάκαρες*, though inferior to their predecessors, who enjoyed the rank of *δαίμονες*. There is here no trace of the fate of the contemporaries of Noah.

There is a passage quoted by Strabo, lib. vii. p. 466., from some lost work of Hesiod, in which the name of Deucalion occurs. Speaking of the Leleges, he says,

Ἤτοι γὰρ Λοκρὸς Λελέγων ἡγήσατο λαῶν
Τοὺς ῥά ποτε Κρονίδης Ζεὺς ἄφθιτα μήδεα εἰδὼς
Λεκτοὺς ἐκ γαίης ἀλέους πόρε Δευκαλίωνος.

The last line is evidently corrupt, and Mr. Bryant, *Myth.* iii. p. 389, pressed it into the service of his argument, by reading *ἁλίῳ πόρε Δευκαλίωνι*, "to Deucalion the man of the sea." Villebrun conjectures *ἀλέας πόρε Δευκαλίωνι*, which suits well enough with the connexion; for the poet appears to represent them as a scattered tribe before, who were *collected* together to serve under Deucalion. Dionysius of Halicarnassus speaks of Deucalion as leading an army of Curetes and Leleges and others who dwelt around Parnassus into Thessaly, and expelling the Pelasgi ‡; and Hesiod probably refers to the same event. From the Scholiast on Apollonius Rhodius, iv. 266, it appears that both Hesiod and Hecatæus represented the descendants of Deucalion as reigning over Thessaly. Accord-

* Pausanias, ix. 31. † Ἔργα καὶ Ἡμ. 107—172.
‡ Ant. Rom. i. 17.

ing to other accounts he belonged properly to Thessaly; but this variation is not surprising, as Herodotus* describes the Dorians as dwelling under Deucalion in Phthiotis in Thessaly, in later times in Dryopis, which was near Parnassus. Aristotle, again, regarding Epirus as the original seat of the Hellenes, refers Deucalion to that country†. In another passage of Hesiod, preserved by Constantine Porphyrogenitus, (*Gaisf. Poet. Græc.* i. p. 201.) Macedonia is spoken of as deriving its name from Macedo, the son of Thya the daughter of Deucalion‡. There is therefore sufficient evidence that the name of Deucalion was in very ancient times connected with Thessaly and with the *incunabula* of the Hellenic nation, Hellen being made his son; but there is no evidence whatever that at this time the story of a deluge was connected with his name. And it is in this capacity only, as a patriarch of the Hellenes, that Herodotus and Thucydides in later times make mention of him.

When we examine the account of Deucalion given by Apollodorus (i. 7. 2.) we can easily detect portions of two distinct fables, which have been incongruously combined, but which can never originally have been one.

At the beginning of the 7th chapter of the first book, he relates, "that Prometheus having fashioned men out of earth and water, and stolen fire from Jupiter to give them, was condemned by him to be exposed to an eagle on Mount Caucasus. Of Prometheus was born Deucalion, who reigned in the country about Phthia, and married Pyrrha, the daughter of Epimetheus and Pandora, the first woman created by the gods. But when Jupiter determined to destroy the brazen race, Deucalion, at the suggestion of Prometheus, built an ark (λάρναξ), and having put provisions on board of it, embarked in it with Pyrrha; and Jupiter, sending down a great quantity of rain from heaven, deluged Greece, so that all men except a few were destroyed, who took refuge on the adjacent high mountains." He then goes on to relate the resting of the ark on Parnassus, and the regeneration of mankind by Deucalion and Pyrrha throwing stones behind them; but immediately after this, § 7., he resumes the genealogy of Deucalion's family by natural descent, Hellen, Amphictyon, &c. That two stories of different origin have been united here, is evident from the circumstance that the human race having originated with their creation by Prometheus, there was no time between him and his son Deucalion for their passing through those successive stages of degeneracy by which they reached the depravity

* i. 56. † *Meteorol.* i. 14.

‡ .... ὅτι Προμηθέως καὶ Πανδώρας υἱὸς Δευκαλίων Ἡσίοδος ἐν πρώτῳ Καταλόγων φησί. *Schol. Apoll. Rhod.* iii. 1086.

of the *brazen age*, which Jupiter determined to destroy. Fiction preserves a certain consistency with itself, however it may make free with the laws of nature; and we may safely pronounce that the account in Apollodorus as it now stands was put together from fables which had no original unity. Leaving out the part relating to the flood, the parts which precede and follow it correspond with what appears to have been the primary purpose of the mythic history of Deucalion, to assign the origin of the Hellenes.

The few fragments of Greek poetry which have escaped the ravages of time, between the age of Hesiod and the 5th century before Christ, appear to contain nothing relating to Deucalion. The first definite mention of the circumstances of the flood is in a passage of Hellanicus, (if correctly quoted by the Scholiast on Pindar, *Ol.* 9. 60. *seq.* ed. Böckh,) who speaks of the ark in which he saved himself as resting on Mount Othrys in Thessaly*. Pindar himself in this Ode, in honour of Epharmostus of Opûs, calls his native place the town of Protogenia, where Pyrrha and Deucalion, descending from Parnassus, made their first abode.

ἄτερ δ'
εὐνᾶς ὁμόδαμον
κτησάσθαν λίθινον γόνον.
.... λέγοντι μάν
χθόνα μὲν κατακλύσαι μέλαιναν
ὕδατος σθένος ἀλλά
Ζηνὸς τέχναις ἀνάπωτιν ἐξαίφνας
ἄντλον ἑλεῖν. l. 60—68. ed. Böckh.

The remains of the dramatic writers contain nothing to our purpose. Plato more than once alludes to the story of Deucalion. In the *Timæus*† he relates that when Solon was in Egypt, and in conversation with the priests of Saïs, mentioning Phoroneus and Niobe, and the flood in the time of Deucalion and Pyrrha, one of them ridiculed the novelty and imperfection of the Greek traditions, alleging that there had been and would be many destructions of the human race, the most extensive by fire and water, others of less magnitude by other causes. In consequence of the destruction of all historical records among the Greeks, they were always children, and had to begin their history again after each catastrophe; and thus the Athenians had lost the knowledge of the great exploits which their ancestors had performed when the Atlantians invaded Europe. Notwithstanding the solemnity with

* It must be observed, however, that there is much uncertainty in quotations by the Scholiasts from authors who are lost, when they do not give their *ipsissima verba*, as they are very apt to mix their own words with what they quote.

† iii. 21. *seq.*

which this account is introduced by Critias, as a λόγος μάλα μὲν ἄτοπος παντάπασί γε μὴν ἀληθής, told by his great grandfather, a friend of Solon, there is much reason to believe that if Plato be not altogether feigning, he has at least referred to Solon what may have happened to himself, and that the alleged destruction of historical records was designed to prepare the way for the story of the Atlantians and their submerged islands. There is, however, independent ground for believing that the Egyptians really held the doctrine of the periodical destruction of the world by fire and water, in which they agree with the Hindoos. In the beginning of the third book of the Laws, Plato speaks of "many destructions of the human race by floods and diseases, and many other causes, in which only a small remnant of them was left," as the consequence of which civilization suffered violent interruptions, and mankind, in the case of a flood, only gradually descended from the mountains into which they had fled for safety from the waters. The remark of Cuvier (p. 86. *note*), therefore, upon the mention of the flood of Deucalion in the *Timæus*, "Il (Platon) place le nom de Deucalion immédiatement après celui de Phoronée, le premier des hommes, sans faire mention d'Ogygès: ainsi, pour lui, c'est encore un événement général, un vrai déluge universel, et le seul qui soit arrivé," is without foundation. Greek tradition spoke only of one flood in Greece, which Plato calls ὁ κατακλυσμός, but he regarded it as only a part of a system which had operated at intervals through myriads of years. He took the popular belief, and adapted it to his own theory of the progress of society*.

Aristotle (*Meteorol.* i. 14.), having mentioned it as part of the order of nature that deluges of rain and inundations should from time to time occur, now in one country now in another, goes on to observe, that of such a kind was the flood, as it is called, in the time of Deucalion: καὶ γὰρ οὗτος περὶ τὸν Ἑλληνικὸν ἐγένετο μάλιστα τόπον καὶ τοῦτον περὶ τὴν Ἑλλάδα τὴν ἀρχαιάν· αὕτη δὲ ἐστὶν ἡ περὶ τὴν Δωδώνην καὶ τὸν Ἀχελῷον· οὗτος γὰρ πολλαχοῦ τὸ ῥεῦμα μεταβέβληκεν· ᾤκουν γὰρ οἱ Σελλοὶ ἐνταῦθα καὶ οἱ καλούμενοι τότε μὲν Γραικοὶ, νῦν δὲ Ἕλληνες. "Aristote," says Cuvier, *ubi supra*, "semble le premier n'avoir considéré ce déluge que comme une inondation locale, qu'il place près de Dodone et du fleuve Achéloüs, mais près de l'Achéloüs et de la Dodone de Thessalie." It is very true that Aristotle is the first author whom we have met with expressly declaring that it was a local flood; but of the authors who preceded him, which

* In the *Critias*, iii. 111. he speaks of Attica as having suffered πολλῶν γεγονότων καὶ μεγάλων κατακλυσμῶν in the 9000 years which had elapsed since the war of the Atlantians.

has said that it was universal? That Dodona of Epirus was meant (the only real Dodona, indeed,) is evident from the mention of the Graici, who were an Epirotic tribe.

[To be continued.]

LXVII. *Professor* Moseley *in Answer to Mr.* Earnshaw's *Remarks on the Principle of Least Pressure published in the Philosophical Magazine for April.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE objections originally alleged by Mr. Earnshaw against the principle of least pressure were three in number.

The *first* had reference to a *general principle* on which the demonstration of that principle was founded.

The *second* referred, not to the demonstration of the principle, but to an assumption made in the *application* of it, that the value of each of a system of resistances was necessarily a function of the coordinates of its point of application; and

The *third* applied itself to the *particular case* of three resistances in the same straight line.

With regard to his objections on these last two points, I am disposed to doubt whether Mr. Earnshaw can be *serious* in persisting in them; and that I have good grounds for scepticism on this point will, I am sure, be admitted by any person who has taken the trouble to *follow out* the controversy between us. That he should adhere to his first, and, as he calls it, his *principal* objection, I can now fully understand.

He has all along been labouring under a misconception of one of the very first principles of my theory. It is only from his last paper of objections, &c., that I make this discovery.

He imagines me, it seems, to have assumed the *directions* of the resistances of the system B to be *given.*

Whence he can have collected this erroneous view of the matter I know not. In the Number of the Phil. Mag. and Journal for October 1833, in which I first brought the subject forward, and to which Mr. Earnshaw has frequently referred, he will find a summary of the analytical operations, by which the amount and DIRECTIONS of each resistance may be determined in terms of the coordinates of its point of application. It is therefore very evident, and might have appeared to Mr. Earnshaw, that I had considered the direction of each resistance to be a *function* of those coordinates, and not otherwise given than in terms of them. But he concludes that I have put the consideration of the directions of the forces out of the question in taking their *sum*. On what, again, is this conclusion grounded? It is true that I have taken the sum of the resistances each with a positive sign, and asserted it to be a minimum; but I have also asserted each to be an independent function of the coordinates of its point of application, so that a minimum value of the sum of the functions supposes a minimum value of each function in particular; and *conversely*.

In fact, the conclusion that the *sum* of the resistances is a minimum is *consequent* upon the assumed minimum value of each resistance in particular, (as is plain enough from the order of my demonstration,) and was merely used by me as affording a more direct and comprehensive method of expressing the conditions of the question in the language of analysis*.

Mr. Earnshaw's first and great argument, that on which he states himself to place his principal reliance, is this: "It appears to me," says he, "that, speaking generally, it is impossible that each force of the system C should sustain only such pressures as are propagated to it from the system A; unless either that the system C consists of but one force, or that all the forces of which it consists are parallel. For if there be more forces than one in the system C, and if they are not parallel, their actions must of necessity mutually propagate pressures."

Now here I at once join issue with Mr. Earnshaw, and assert, that *it is possible*, &c. &c.; and I give him this case in proof of the *possibility*, which, be it observed, is the point in question. "Suppose that to the different points of application of the system A (having first removed that system) we apply forces having for their resultant *one* of the forces C; and then to the same points forces having for their resultant *another* of the forces of the system C, and so on, until we have obtained all the forces of the system C. Now, under these

* There is one case in which this assumption of a minimum value of the *sum* of the resistances consequent upon the assumed minimum values of the several component resistances fails: this assumption, as stated above, I have made, however, only as a convenient enunciation of my fundamental proposition. The case to which I have alluded is that of a system of parallel resistances. The *sum* of these resistances is *not* a minimum subject to the conditions. But still the *original* proposition holds, *each force in particular is a minimum*; subject (of course) to the condition that every other is a minimum, and their sum a constant. Under these circumstances the analytical investigation of this particular case is included in that which I have given of the general proposition. This is precisely the case which Mr. Earnshaw has brought forward in the shape of a new objection to my theory. I think it better to confine myself to his three original objections, until they are completely disposed of; I shall then be happy to meet him on this.

circumstances, the systems of forces concerned will have become precisely analogous to the systems A and C; but by the principle of the *superposition* of forces it is manifest that no pressures will be mutually propagated among the forces of C. This is one of those arguments which Mr. Earnshaw states to have had the effect of strengthening rather than of removing his previous objections to my theory. Will he do me the favour to *answer it*? Here is a case of two systems of forces in equilibrium, A and C, of which the latter are oblique to one another, and yet mutually propagate no pressures. The pressures propagated from the different forces of A to each point of application of C are exactly sustained by the force applied to that point; and the case is precisely analogous to that in which the forces C are supplied by the resistances of as many fixed points.

Now it might easily be shown that by varying the magnitudes and directions of the forces C these might each of them be made to satisfy the conditions supposed above, whatever were the forces A. The *general* case, therefore, that the forces C may be so taken in magnitude and direction as, although oblique to one another, yet mutually to propagate no pressures, is proved. Now this *general* case is what Mr. Earnshaw in his first paper particularly objected against, making at the same time an exception in favour of the *particular* case of parallel resistance; yet, strangely enough, he in his last paper seems to yield the *general* proposition, and flies at the particular case. Here, however, I am happy to meet him. He supposes the system A to be supplied by gravity, and the forces C to be resolved, each horizontally and vertically. The horizontal parts of the forces C must, he says, mutually destroy, since the forces A are vertical; and it is thus proved that the forces C mutually propagate horizontal pressures. True; but what hinders that the forces C should in this case be taken *vertically*? My argument is that they *may* be so taken as mutually to propagate no pressures; and if taken vertically, they will propagate none. I have all along assumed that in the case of gravity, in which the points of resistance are capable of supplying it in *any* direction, that direction will be *vertical**. My proposition is that the forces C may be so taken in magnitude and direction as mutually to propagate no pressures, and yet sustain the forces A. Mr. Earnshaw's case of parallel forces does not in the least shake this conclusion, in as much as it turns entirely upon the hypothesis that the direction of the forces C are *given*, and are *not* parallel to those of the

* Mr. Earnshaw will see this by reference to my paper published in the Lond. and Edinb. Phil. Mag. for December last: vol. iii. p. 431.

system A; an hypothesis never made by me. He goes on to state, that he considers me to have *erred in my argument* on this point in two particulars.

1. In assuming that each of the forces C can be divided* (not *resolved*) into two parts, one of which is solely employed in sustaining the pressures propagated by the system A, and the other in sustaining the pressure mutually propagated by the system C.

2. In supposing that these latter forces *can* be removed by the principle of the superposition of equilibrium.

Mr. Earnshaw has not informed me *wherein* I have erred in these particulars.

His next objection is couched in the following terms: "I am unable to comprehend why we are to consider the forces as functions of the coordinates of the points at which they are applied." For his inability to perceive the ground on which this assumption rests, he assigns no reason in his first paper; but perhaps, feeling from my answer to that paper (of which answer, however, he has taken only this notice,) that some *reason* was necessary, he thus *satisfactorily* supplies it. "I will now give my REASON for saying that, &c. It is this: I *believe* that only the *directions*, and not the *forces themselves*, when there are more than three, are functions of the coordinates." Now, let us try this general principle, his belief in which Mr. Earnshaw assigns as his *reason* for disbelieving the resistances to be functions of their coordinates, by applying it to a particular case, a method of verification the use of which has the sanction of Mr. Earnshaw's own authority. Let there be a perfectly smooth surface, on which let a heavy mass be placed supported upon any number of points; a table, for instance, on four legs.

Now according to Mr. Earnshaw's hypothesis, if we alter the relative positions of these legs in any way, leaving the weight of the table the same, we shall not in the least alter the amount of pressure upon each, but only its *direction*; so that we shall get four equal pressures in different oblique directions. Now let these be resolved *horizontally* and *vertically*.

The sum of the vertical pressures must equal the *weight* of the table, and result, indeed, from it; *but where do the horizontal forces come from?* And this, which is at best but an *hypothesis*, and an hypothesis involving, it seems, an *absurdity*, Mr. Earnshaw calls *a reason*!

My assumption that the several resistances are functions of the coordinates of the points of application is grounded on

* I have nowhere used the term *divided*.

this, that each such point being supposed capable of resisting in every possible direction, the circumstances under which one point resists do not differ from those under which another resists otherwise than as it respects the positions of the points. Or thus: it being *supposed* that the resistances are functions of their points of application *and* their directions,—also their directions being subject in each case to the condition of a minimum resistance,—we may from these data obtain equations from which the resistances themselves will be eliminated and the directions obtained in terms of the coordinates: the directions being then functions of the coordinates, the resistances are also functions of these.

I now come to the case of a body supported upon three props. In reference to this case I have reasoned from a finite state of the triangle formed by joining the points of support, to its evanescent state; and so has Mr. Earnshaw, and we have arrived at different conclusions, the reason of which appears to me evident enough. Neither Mr. Earnshaw nor myself can know anything of the relative pressures upon C and G when they have *actually passed* into the line A B, by reason of the evanescence of the triangles by which a comparison is established between them. All that we can know of their ratio when in that position, must be obtained by observing the limit to which it approximates as they pass into it. Now Mr. Earnshaw, by bringing, first, one of the two points into the line AB, and then the other, has rendered this observation of the limit towards which the ratio of the triangles, and therefore of the forces*, continually approaches altogether impossible. By bringing C into the line AB, he has at once plunged one of the triangles into its state of evanescence, and seeing no more of it he proceeds to thrust the other also out of his sight. How he should imagine that in this way he can have made any comparison between them applicable to their state of actual evanescence, is to me quite unintelligible.

On the contrary, I have traced the ratio of the triangles up to the limits of evanescence; and knowing the forces to have the ratio of the triangles, I know that I have thus traced the ratio of the *forces* up to the very point of their coincidence with the line AB.

My *only* hypothesis has been that when the triangles pass from this exceedingly small but finite state into the evanescent state, and the forces, from the nearest possible approximation to the line AB, *actually into that line*, these retain that identity of their ratios which they have up to that point

* The forces G and C are demonstratively proportional to the triangles AGB and ACB, so long as the latter are *finite*.

maintained. If as these triangles approach evanescence they continually approximate to a certain ratio or continually retain the same ratio, and if this approximation or this permanence continue, however *near* we bring them to a state of evanescence, it is a legitimate conclusion, that in their actual state of evanescence they attain the ratio to which they have continually approximated, or retain that which, up to the point of evanescence, they have uniformly preserved.

Now the forces have the ratio of the triangles in their finite state,—this is a matter of demonstration,—and they retain this ratio however nearly they are brought to the line AB; when, for instance, they are distant from that line by the smallest conceivable quantity: it is therefore a legitimate conclusion that, carrying on the approximation, their ultimate ratio is the evanescent ratio of the triangles.

My argument in respect to this matter of the three points of support stands thus: I have shown (and Mr. Earnshaw will not pretend to dispute my demonstration of this fact) that when the points G and C are made to approach within the *smallest conceivable distance* of the positions which I have ultimately assigned them in the line AB, the pressures upon A, B and C are each one third that upon C.

If under these circumstances Mr. Earnshaw conceives that these points G and C do not approach near enough to their ultimate positions to be supposed to sustain pressures not very different from those which they sustain when *in* those positions, then let the smallest conceivable spaces of which I have spoken be halved, and let the points take up their positions in the bisections of these spaces: the proposition will then be true, and Mr. Earnshaw's objections will be limited within the remaining half of the smallest space conceivable, where I leave them.

I now come to Mr. Earnshaw's attack upon my application to his case of a formula derived from the principle of least pressure, which formula, when thus applied, gives a result identical with that just obtained by the method of evanescent triangles, and thus furnishes a remarkable verification of the principle of least pressure. I must confess that the objection which Mr. Earnshaw has taken to my application of this formula has perfectly *astonished me*. I have arrived at the equation

$$x^2(3a-b-c)-2xa(b+ac-bc) = -abc,$$

in which equation a b and c are dependent upon the relative positions of three points of resistance situated anywhere in the same straight line, and x is the distance from one of them of a certain point, about which the moments of the resistance are shown by the principle of least pressure to be equal.

Now, if the positions of the points of resistance be supposed to vary so that $3a-b-c$ may continually *diminish*, it is evident that to satisfy this equation the value of x must continually *increase*, and that when $3a-b-c$ becomes infinitely small, x must be, and is, infinitely great. No! says Mr. Earnshaw; "when $3a-b-c$ is infinitely small, the first term vanishes as compared with the rest, and the value of x is

$\frac{\frac{1}{2}abc}{ab+ac-bc}$, which is not infinite."

Now, I will venture to suggest to Mr. Earnshaw that somewhat more of attention than he seems to have bestowed upon this objection was due to the controversy between us. Had he not very hastily and cursorily considered the subject, I am sure he would have perceived that the first term $x^2(3a-b-c)$ being made up of two factors, it does not by any means follow that we diminish the whole term as we diminish one of its factors, or that the whole term enters upon the infinitesimal state when one of its factors becomes an infinitesimal. Provided the other factor increase as this factor diminishes and become exceedingly great when it becomes exceedingly small, the term itself may retain throughout a *finite* value. Now this is precisely the case in the equation in question. It is not, therefore, true that when $3a-b-c$ is exceedingly small, the first term vanishes as compared with the rest.

If Mr. Earnshaw objects that I have here reasoned of the exceedingly small value of the factor $3a-b-c$, and not of its state of absolute evanescence, I answer that neither he nor myself can know anything of what actually obtains in that state of evanescence. We can only reason of the circumstances which attend its continual approach to that state.

I have thus shown demonstratively that when the points take up positions differing the least possible from those assigned to them in my former paper, the pressures upon them are equal; and I have shown this to result both from a formula dependent upon the principle of least pressure, and from a separate and independent investigation.

I leave Mr. Earnshaw to speculate upon the changes which can take place in the relation of the resistances during the motion of their points of application from the positions in which I have left them through the infinitesimal spaces which intervene between these positions and their *ultimate* positions.

This task is one which will call for a powerful effort of the imagination of Mr. Earnshaw, fruitful as it is in objections.

In conclusion, I beg leave to refer him again to my last paper, as containing answers to his first objections, none of which he has answered.

King's College, April 19, 1834.

LXVIII. *Reviews, and Notices respecting New Books.*

Report on the Progress, Actual State, and Ulterior Prospects of Geological Science. By the Rev. W. D. Conybeare, *F.R.S., V.P.G.S., Corr. Memb. Institute of France, &c.* (Second Report of the British Association for the Advancement of Science, 1832, p.365—414.

HAVING reason to believe that certain points discussed and elucidated in this Report have neither acquired the publicity nor received the attention which they merit, and also that the geological section through Europe which accompanies it is not so widely known as it deserves to be, we deem it proper, although the volume of which it forms a valuable portion has now been for some months before the public, to present our readers with a brief account of Mr. Conybeare's labours in this highly important contribution to the prosperity of the science of which he has so long been one of the most eminent cultivators.

Mr. Conybeare having years ago (in the first part of the "Outlines of the Geology of England and Wales," by himself and the late Mr. William Phillips,) submitted to the public a concise statement of the notices of geological observation and geological theory which are contained in the records of classical antiquity, and of the anticipations of subsequent discoveries, which are to be found in the works of numerous writers in Italy, after the revival of literature and science; and the outline he then gave having been ably filled up by subsequent writers, especially by Mr. Lyell, (in the first volume of his 'Principles of Geology,') he confines his remarks on early geology to a notice of the claims of Leibnitz. "The universal Leibnitz," he observes, "honoured this branch of physical speculation by devoting to it a portion of his attention, and anticipated, with the prophetic sagacity of a powerful mind, its future progress, and the very methods of investigation which would most effectually contribute to its successful development." In the fourth section of his *Protogæa,* Leibnitz presents us "with a masterly sketch of his general views; and perhaps, even in the present day, it would be difficult to lay down more clearly the fundamental positions which must be necessarily common to every theory attributing geological phænomena in great measure to central igneous agency." He attributes the primary and fundamental rocks to the refrigeration of the crust of this volcanic nucleus; the dislocations and deranged positions of the strata he ascribes to the breaking in of the vast vaults which the vesicular and cavernous structure assumed by masses during their refrigeration from a state of fusion, must necessarily have occasioned in the crust thus cooling down and consolidated. He assigns the weight of the materials, and the eruption of elastic vapours, as the concurrent causes of these disruptions. He adds that these disruptions of the crust, must, from the disturbances communicated to the incumbent waters, have been necessarily attended with diluvial action on the largest scale. When these waters had subsequently, in the intervals of quiescence between these convulsions, deposited the materials first acquired by their force of attrition, these sediments formed, by their consolidation, various stony and earthy strata. Thus, he observes, we may recognise a double origin of the rocky masses, the one by refrigeration from igneous fusion, the other by concretion from aqueous solution. "We have here distinctly stated," Mr. Conybeare remarks, "the great basis of every scientific classification of rock formations." By the repetition of similar causes frequent alternations of new strata were produced, until at length, these causes having been reduced to a condition of quiescent equilibrium, a more permanent state of things emerged. In concluding his sketch of this portion of the geological anticipations of Leibnitz, Mr. Conybeare particularly invites to the following clause the attention of those writers of the present day, who appear to assume it as an essential condition of their theories, that the same physical causes can never, under any former circumstances, have acted with more intense energy than they actually exert: "Donec quiescentibus causis, atque æquilibratis, consistentior emergeret rerum status."

The contributions to geology of Hooke, Lehman, Fuchsel, Saussure and Werner, are next briefly reviewed; the first general announcement that the various species of organic remains grouped together in the rock formations bear a constant relation to the age of those formations, being shown to have been made by the illustrious professor ot Freiberg. The progress of geology, from the period at which it had in his hands begun to assume the systematic character of a regularly digested science, may be considered, Mr. Conybeare remarks, as having presented three marked stages, distinguished by three successive schools. Each of these schools has selected for the more especial object of its attention a single member of the three great geological divisions in the series of formations, *i. e.* the primitive, secondary, and tertiary; and the succession of these schools has, by a singular coincidence, followed the same order with that of the formations to which they were devoted: it may also be observed that the leaders of each school have been distinguished geologists of three different nations,—Germany, England, and France. The first, or German school, is that of Werner himself. The second, or English school, generally recognises the masterly observations of Smith, first made public in 1799, as those which have principally contributed to its establishment. The third school, or that of Tertiary Geology, owes its foundation to the admirable memoir on the Basin of Paris, published by Cuvier and Brongniart in 1811. The labours of these schools of geology, and the effect upon the progress of the science produced by the establishment of the Geological Society of London and the publication of its Transactions, are succinctly related, together with the subsequent labours both of English and foreign geologists; this preliminary sketch of the progress of the science being brought down to the year 1821, at which period the first series of the *Geological Transactions* closed.

"I have been principally induced, in the present summary of the progress of geological science, to draw a line at the close of the first series of our *Geological Transactions* in 1821, because an author already alluded to [Dr. Macculloch] has asserted in a recent publication, that 'since that year geology has received scarcely any valuable additions, and not a single fundamental one.' Drawing a line at this point, therefore, I shall endeavour to give a slight sketch of the contributions which have really marked the progress of the science during this supposed period of inanition, leaving it to your judgement how far they really deserve the above depreciating character.

"Now although previously to this period the main features of English geology had been very amply illustrated, yet even in this province, where least remained to be accomplished, our additions have neither been few nor unimportant; and if we turn to Continental Europe, we shall find that what was then comparatively a blank, has been now filled up to such a degree that we are actually in possession of nearly as good materials for a general geological map of Europe at the present day as we were for one of England only at the former date; and to this, observers from our own country have contributed no less than their ablest continental brethren. Nor let it be imagined that this only supposes an extension of our knowledge in insulated details: it is in truth far otherwise; since extensive *comparative* geology affords the only materials for obtaining the *fundamental facts* of our science. It is by this inductive process alone that we can hope to collect and combine the data which exist for what may be termed a general geological chronology. It is thus only that we can ascertain to what extent and under what modifications the same geological causes have acted at the same epochs. It is thus only that we can learn, what have been the violence, extent, and epochs of the disturbing and elevating forces which have affected the strata,—whether similar groups of organic remains universally, and in the most distant countries, characterize contemporaneous geological deposits,—or whether those zoological species are not rather restricted (like most of the species of the actual period,) to different geographical districts. All these are evidently questions at the very root of any sound geological theory, whenever the time shall be fully ripe for constructing such a theory; and although it were assuredly premature to assert that this time is even yet completely arrived, we may nevertheless boldly assert that no eye at all capable of appreciating these problems, or the appropriate evidence tending towards their solution, can glance over the discoveries of any single year since 1821, without observing a very rapid accumulation of the most valuable materials for their elucidation. During the same period, moreover, our knowledge of the principal volcanic districts, both those which are still in activity and those now extinct, has been advanced to the greatest degree of precision; and the whole of that which is perhaps the most important geological series*,—that of the tertiary formations, with the lower members of which alone the previous researches of Cuvier, &c., had made us acquainted,—has within the few last years received an additional development, no less important than that which, in an earlier stage of geological progress, the secondary system of the Wernerians received from the discoveries of Smith."

In order to confirm this general statement, Mr. Conybeare enters more minutely into the detail of the recent progress of geological discovery, reviewing in succession the researches of Fitton and Martin, of Murchison and Sedgwick, of Buckland and De la Beche, of Mr. Phillips of York, (now appointed, we are happy to see, Professor of Geology in the King's College,) and of the various expositors of the structure of the northern coal-fields in the *Transactions of the Newcastle Philosophical Society*.

Concluding here the subject of British geology, Mr. Conybeare proceeds to give an outline of the rapid progress which the science has recently made in developing the structure of foreign countries, adopting the following geographical order. He begins with the countries constituting or bordering the great European basin, which he takes in the following order: France, the Alps, Germany, the Baltic Coasts and Scandinavia, ending with Russia. He next proceeds to the countries connected with the Mediterranean basin,—the Spanish Peninsula, Italy, Turkey and the African coasts. The other quarters of the globe now follow,—Asia as divided into the northern and central provinces explored by the Russian Government; and those of India by our own nation. He concludes with North and South America; but reserving in all these instances what relates to the two great points of tertiary and volcanic geology, as demanding a distinct notice rather in their relations to the general questions of the science, than to the geographical distribution of formations.

Neither this portion of Mr. Conybeare's Report, nor the succeeding notices on tertiary and volcanic geology and palæontology, being susceptible of condensation, we shall terminate our analysis by noticing the views with which it concludes respecting the present prospects of the science, and the objects which most claim the attention of geologists at this time.

"The first points of the science are undoubtedly those which connect it with its elder and far superior sister, Physical Astronomy. I mean such questions as those relating to the spheroidal figure, and to the density, of the earth;—the inquiry entered into by Sir John Herschel, how far the secular diminution of the eccentricity of its orbit may have tended to the decrease of its temperature; and the like. Now

* "The tertiary period is especially important in systematic geology inasmuch as since, on every hypothesis, the geological causes must have acted during this period under conditions most nearly approximating to those which belong to the actual order of things: the formations of this age therefore afford the most essential link in connecting our actual experience with our speculations on the former state of our planet."

one of these points, and that very important to geological theory, appears to me to require further investigation; I mean the conclusions fairly deducible from the known density of the earth, as to the solid structure and composition of its interior. As its density is known to be considerably greater than that of a solid sphere composed of any such rocks as we are acquainted with, (granite, for instance,) our *primâ facie* inference would naturally be, that the interior is solid; and that heavier materials than our ordinary rocks (such as metalliferous masses,) enter into its constitution. But to this it may be objected that the rocks alluded to have in themselves a principle of elasticity and compressibility, and therefore may, under the vast pressure existing in the interior of the globe, be condensed to such a degree as is far more than sufficient to account for the excess of the earth's density, as compared with their specific gravity, and thus still to allow for considerable vacuities. To this, however, a counter argument may be fairly adduced, that, as the resistance to further compression increases with every additional pressure, that resistance may soon, in the case of these rocks, become practically infinite. A more accurate examination of the whole circumstances of this problem appears highly desirable.

"It has been well observed in a very able article in a late periodical, that 'the most conclusive argument against the fact of any disturbance having, in remote antiquity, taken place in the axis of the earth's rotation, is to be found in the amount of the lunar irregularities which depend on the earth's spheroidal figure. However insufficient the mere transfer of the mass of the ocean, from the old to the new equator, might be to ensure the permanence of the new axis, the enormous abrasion of the solid matter of such immensely protuberant continents as would, on that supposition, be left, by the violent and constant fluctuation of an unequilibrated ocean, would (according to an ingenious remark of Professor Playfair,) no doubt, in lapse of some ages, remodel the surface to the spheroidal form; but the lunar theory teaches us that the *internal strata*, as well as the *external outline* of our globe, are elliptical, their centres being coincident, and their axes identical with that of the surface,—a state of things incompatible with a subsequent accommodation of the surface to a new and different state of rotation from that which determined the original distribution of the component matter."

The next branch to which the attention of geologists is called may be termed, according to the author, the true dynamics of geology, with far more justice than that appellation has been applied to other branches of our science. "I would so denominate," he observes, "the general consideration of the forces which appear to have been the agents in dislocating and elevating our strata; whether in the earlier geological disturbances, or in the actual phænomena of volcanoes and earthquakes." After noticing the contributions to this department by Von Buch and Elie de Beaumont, and anticipating the conclusions which may be arrived at from the future extension of physical geography and the geology of mountain chains, Mr. C. proceeds: "There is one source of analogy which has always appeared to me as likely to throw illustration on this subject, and which I yet almost hesitate to allude to, lest I should incur the charge of indulging speculations altogether rash and visionary. However, I would premise the observation, that we must surely in no respect consider our planet as an isolated body in nature; it is one only of the general planetary system, and every fair presumption of analogy favours the supposition, that similar general causes have acted in all the members of that system. Now one of these members, our own satellite, is placed sufficiently near us to enable our telescopic observations to distinguish accurately the general outlines of its mountain chains, and other similar features of its physical geography. We have been able to discern even the eruption of volcanoes; and any one who has viewed its surface through a telescope, must be struck with the exact identity of the forms which he there contemplates with the maps and descriptions of the volcanic districts of our own globe. If we recall Von Buch's account, already referred to, of crateriform amphitheatres of many leagues in diameter, encircling central conical craters; of lines of these generally disposed in a linear direction; of such linear trains often radiating from a central focus of principal disturbance; we may almost fancy that this description was intended as an exact portrait of what we observe on the lunar surface. Is it, then, altogether unfounded, to believe that by more carefully observing these phænomena, where we have a whole hemisphere of a planet at once open to our inspection,—by comparing the best of the early delineations of its telescopic appearance, with its exact actual forms, and watching diligently those forms, so that we may be able to detect any changes in them,—is it too much to hope that we may thus effectually extend our knowledge of the general laws of the volcanic forces, which should appear to be among the general planetary phænomena?"

The discussion of the remaining subject cannot be abbreviated without injury, and we therefore give it entire.

"The great branches of the comparative geology, and comparative palæontology (or study of fossil remains) of distant countries, much as they have recently advanced, have as yet even a still wider interval to pass over than that which they may have already accomplished, before they shall have obtained that degree of completeness which alone can qualify them to serve as sound bases in any geological theory. First, as to comparative geology. The very introductory question is yet inadequately answered, Is there or is there not anything like such a general uniformity of type in the series of rock formations in distant countries, that we must conceive them to have resulted from general causes, of almost universal prevalence at the same geological æras? Now it is clear that this question, if intelligently proposed, does not require, for its affirmative solution, anything like an exact *identity* of formations in remote localities. It does not require any one to be able to take to Australia a detailed list of English strata, and to be able at once to lay his hands on the exact equivalents of our lias, oolites and chalk. Such an idea would be almost to caricature the Wernerian dogma of universal formations.

We are indeed unable to trace many of these formations, even through our own island, without observing such considerable modifications in their comparative types, in our northern and southern counties, as may sufficiently remind us that we are to look only for such *analogous* rather than *identical* results, as would naturally proceed from the contemporaneous action of similar causes in distant localities; in each of which many varying local circumstances must have affected those results, for two conditions obviously enter into this problem:—first, the contemporaneous prevalence and extent of similar geological causes; and secondly, how far these causes, even where active, may have been modified by varying local circumstances. Now, at present, our materials for answering these questions accurately are confined to Europe: of America indeed we have some information; and although this may as yet be considered too vague to be fully satisfactory, yet as far as it goes it is undoubtedly favourable to the presumption of even a greater degree of geological uniformity than we should have been justified in anticipating *à priori*.

"Humboldt indeed has remarked, that while on entering a new hemisphere we change all other familiar and accustomed objects,—while in the plains around we survey entirely new forms of vegetable and animal being, and in the heavens over our heads we gaze on new constellations,—in the rocks under our feet, alone, we recognise our old acquaintances. And with regard to the primordial rocks, there is undoubtedly much truth in this pointed remark. Granite, mica slate, and their contained minerals, present the most identical resemblance, whether we collect from Dauphiné, Norway, the Alleghanies, Egypt, India or Australia. But concerning the secondary series, our information is far less precise.

"With regard to the comparative geology of secondary districts, the fossil zoology of the various districts, or *comparative palæontology*, requires to be called to our aid as our surest guide. And these investigations are the more interesting, because an important primary question here suggests itself. In the actual state of things, the limited geographical distribution of identical species, both animal and vegetable, is one of the most striking phænomena that presents itself to our view. In distant continents, the specific differences of the animal races are wide and strongly marked; and sometimes, as in Australia, the difference extends to the character of genera, and even families; and this even in countries of similar conditions, as to latitude, climate and temperature. Now we may naturally inquire, whether it does not seem most probable, that in the ancient geological æras the species then inhabiting our globe were grouped together under similar restrictions as to geographical habitation; and if so, how far we are entitled to expect to find in other countries the same series of successive organic remains (each group characterizing a distinct geological æra,) which we meet with in Europe. To illustrate this inquiry:—If in the present age the recurrence of violent convulsions were again to submerge Europe and Australia, and cover their surface with fresh sedimentary depositions, these new formations, though absolutely contemporaneous, would, in either continent, (should they again be laid dry and exposed to observation,) be found to exhibit in the organic bodies thus imbedded, differences to the full as marked as those which, in the same continent, characterize formations of the most distant ages. Now with regard to the secondary rocks of distant countries, our information is as yet far too limited to enable us to return, to the above questions, answers on which we can rely with any degree of confidence: but the evidence, as far as it goes, does undoubtedly again seem to indicate a greater degree of resemblance than we could have reasonably anticipated: this may particularly be instanced with regard to the older rocks containing organic remains, *e. g.* the transition limestone. We are extensively acquainted with this rock in Russia, in the islands of the Baltic, in Scandinavia, and in North America; and its fossils everywhere exhibit so near a *generic* resemblance, that it would often puzzle even a practised eye, if two groups of specimens, one from Dudley and another from Melville Island, were placed before him, to say which specimen came from which locality, the same Cateniporæ, Caryophylliæ, and Encrinites, being present in both. The fossil vegetables of the coal-fields of Europe and America also appear very similar; but we shall probably often, on a more accurate investigation, observe specific differences combined with generic resemblances: still, undoubtedly, the impression on my own mind, from a tolerably careful examination of all the specimens I have hitherto seen, is, that a much nearer approximation may be observed between the fossil animals and vegetables of the old and new continents than between those occupying them at the actual period. And, what is peculiarly important, we find, as I have already observed, even in the highest latitudes of the arctic ocean, types which appear characteristic of a tropical temperature; and the general diffusion of those types in the rocks of the transition and carboniferous periods, in whatever latitude they are found, appears to imply a much greater equality of temperature to have then prevailed than in the present state of things. It seems also inconsistent with the existence of these beings that such wide variations of temperature between the different seasons should then have occurred, as must necessarily accompany, in high latitudes, any temperature derived entirely from the sun,—a consideration which renders inapplicable to this case the hypothesis, that the higher temperature required by geological inferences may be accounted for by the diminution of the eccentricity of the earth's orbit, since this would still leave the inequality of the seasons in the higher latitudes as great as at present. For this more equable temperature, then, it seems difficult to account, without having recourse to the hypothesis, which so many other geological arguments render probable, of an internal source of heat proper to the globe itself.

"Of more recent secondary strata than the carboniferous series, there appear few traces in the parts of America yet explored, excepting, as I have already observed, the marls of New Jersey, of which the fossils exhibit a close generic agreement with those of our subcretaceous formations, such as the gault, although they are certainly often specifically distinct. In Virginia we have also extensive tracts of shells, approximating to recent species, as in our own tertiary deposits; and in the diluvium we may observe that the Mastodons approximate to those of Ava and Tuscany, although nevertheless mostly specifically distinct, while the Megalonyx is peculiar to America.

"We have yet collected from India and from Australia little information on which we can rely, only that it appears that the usual remains in the bone caves of the latter are generally those of kangaroos, wombats, and other animals still inhabiting that continent, mingled however with other bones belonging to some animal resembling an hippopotamus, now unknown in those parts. To India and to Australia, however, it is that we must look, no less than to America, with full confidence that we shall speedily thence obtain sufficient evidence on all these fundamental questions to afford us a basis of induction sufficiently extensive and firm to enable us, at no distant period, steadily to lay the foundation and securely to raise the superstructure of an enduring and general geological theory."

Mr. Conybeare's Report, of the value of which the foregoing analysis and extracts will enable every reader to form a correct estimate, concludes with the following notice of the geological section with which it is accompanied; a section, we believe, of greater extent than any that has yet been produced.

"As an appropriate illustration of the recent progress and actual state of geology, I have presented to the Society an engraved section traversing Europe from the northern extremity of Great Britain to Venice, being, as I believe, the first attempt at so extensive a design as yet submitted to the public. In its execution of course the merit of a careful compiler is all that I can pretend to claim: the English portion is all which I can fairly appropriate to myself on the title of original observation; for both the extremities, *viz.* the northern section of Scotland, including the Brora coal-field, and the whole Alpine section (by far the most important and instructive part of the whole), I am indebted to my friend Mr. Murchison, the present [late] President of the Geological Society, whose recent contributions to our science have so abundantly vindicated his claim to our highest office; his laborious, exact, and scientific surveys of the Alpine chains, of which a specimen is thus presented, are especially a credit to the English school of geologists. Oeynhausen and Dechen have been my authorities for the central portion of the section.

"A scale of one degree of a great circle of the globe is given with the section, which however is to be understood only as an approximation, and not strictly to be applied throughout, as some portions, especially in the Alpine districts, have been given on a somewhat larger scale to display the phænomena. No regular scale of elevations has been attempted: to have adopted such a scale would have reduced the English chains to imperceptible undulations, or exaggerated the Alps into proportions most inconvenient for the purposes of the section."

LXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

1834. April 10.—A PAPER was read, entitled, "On a General Method in Dynamics, by which the Study of the Motions of all free Systems of attracting or repelling Points is reduced to the Search and Differentiation of one central Relation, or characteristic Function." By William Rowan Hamilton, Esq., Andrews Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland. Communicated by Captain Beaufort, R.N., F.R.S.

After some introductory remarks illustrative of the scope and design of this paper, the object of which is sufficiently pointed out in its title, the author considers, 1st, the integration of the equations of motion of a system, the characteristic function of such motion, and the law of varying action; 2nd, the verification of the foregoing integrals; 3rd, the introduction of relative or polar co-ordinates, or other marks of position of a system; 4th, the separation of the relative motion of a system from the motion of its centre of gravity, the characteristic function for such relative motion, and the law of its variation; 5th, the systems of two points in general, and the characteristic function of the motion of any binary system; 6th, the undisturbed motion of a planet or comet about the sun, and the dependence of the characteristic function of elliptic or parabolic motion on the chord and the sum of the radii; 7th, the systems of three points in general, and their characteristic functions; 8th, a general method of improving an approximate expression for the characteristic function of motion of a system, in any dynamical problem; 9th, the application of the foregoing method to the case of a ternary or multiple system, with any laws of attraction or repulsion, and with one predominant mass; 10th, the rigorous transition from the theory of binary to that of multiple systems, by means of the disturbing part of the whole characteristic function, and approximate expressions for the perturbations.

A paper was also read, entitled, "Observations on the Motions of Shingle Beaches." By Henry R. Palmer, Esq., F.R.S.

The reading of a paper, entitled, "On some Elementary Laws of Electricity." By W. Snow Harris, Esq., F.R.S.—was commenced.

April 17.—The reading of Mr. Harris's paper was resumed in continuation.

April 24.—The reading of Mr. Harris's paper was concluded.

For the purpose of determining several questions relative to the forces exerted by bodies in different states of electricity, the author contrived an electroscope of peculiar construction, and also an electrometer, both of which he minutely describes; and in order to obtain a unit of measure, in estimating the quantity of electrical accumulation, instead of transmitting the electricity evolved by the machine immediately from its conductor to the battery to be charged, he interposes between them a coated jar, furnished with a discharging electrometer, so that the quantity of charges that have passed through it may be estimated by the number of explosions occurring in the process of accumulation. By increasing or diminishing the distance between the discharging balls, the value of the unit may at pleasure be rendered great or small.

A series of experiments is described, showing that when a given quantity of electricity is divided among any number of perfectly similar conductors, the attractive force, as measured by the electrometer, is inversely as the square of that number; and if different quantities of electricity be communicated to the same conductor, their attractive forces are directly as the squares of those quantities.

The author observes that the electrical force exerted by one body on another is always diminished by the vicinity of a neutral body; an effect which is analogous to the operation of screens in diminishing the force of a revolving magnet on metallic disks, as noticed by him in a former paper, published in the Philosophical Transactions *. It appears, thus, that there is, in all these cases, a portion of electricity, which is masked, and not appreciable by the electrometer. The author proposes to distinguish the terms *tension* and *intensity*, as applied to electricity; expressing by the first, the actual elastic force of a given quantity, accumulated in a given space; and by the second, the action of that part which is in a state of freedom, and which is indicated by its effects on the electrometer.

Experiments are next related, which were made for the purpose of showing the incorrectness of the explanation of the above fact proposed by Mr. Singer, namely, that it depends on the electrical action of the atmosphere. In the transmission of electricity between conductors placed at a distance, the quantity required to produce a discharge is directly as the distance; and conversely, the distance is directly as the quantity. This distance will, therefore, be a measure of the tension; whereas the attractive force, as indicated by the electrometer, is a measure of intensity only. Another conclusion deduced from this train of reasoning is, that the resistance of the atmosphere to the passage of electricity is not really greater through any one discharging distance than through another, and is in no case greater than the existing atmospheric pressure; and it was found by direct experiment, that the distance through which a given accumulation of electricity could be discharged, is inversely as the density of the interposed air. When this air preserved its density unaltered, the elevation of its temperature produced no difference in its power of controlling the escape of electricity; hence it is concluded that heated air is no otherwise a conductor of electricity, than in as much as it has thereby become rarefied; but heat applied to solid conductors was found to diminish their conducting powers.

The electrical capacities of conducting bodies of different shapes was the subject of inquiry. In plates having the form of parallelograms, the relative capacities, when the areas are constant, are inversely as the sum of the length and breadth; and when this latter sum is constant, the capacity is inversely as the area. The capacity

* An abstract of this paper will be found in Phil. Mag. and Annals, N.S., vol. x. p. 298.—Edit.

of a plane circle differs but little from that of a square having the same area; nor does it make any difference if the plates be turned into cylinders, or prisms with any number of sides; and the capacity of a sphere or cylinder is the same as that of a plane equal to it in superficial extent.

The author proceeds to investigate some laws relating to the action of electricity, when resulting from induction; and particularly that of the relation between electrical attraction and distance; adducing experiments in confirmation of the former being in the inverse duplicate ratio of the latter. The attraction actually exhibited between two equal spheres, he considers as composed of a system of parallel forces, operating in right lines between the homologous points of the opposed hemisphere. The author concludes by various observations on the transmission of electricity to bodies in vacuo, from which he infers the fallacy of all explanations of the phenomena of electrical repulsion, founded on the supposed action of the atmosphere.

The reading of a paper, entitled, "On the Generation of the Marsupial Animals; with a Description of the impregnated Uterus of the Kangaroo." By Richard Owen, Esq., Member and Assistant Conservator of the Museum of the Royal College of Surgeons, London. Communicated by Sir Anthony Carlisle, F.R.S.—was commenced.

May 1.—Mr. Owen's paper was resumed, and concluded.

The author gives a history of the opinions which have been advanced relative to the generative organs and functions of the *Marsupiata*, an extensive order of quadrupeds, including animals nourished by every variety of food, and exercising very different powers of progression, yet exhibiting a remarkable uniformity in their mode of reproduction. In all the genera included in this family, the uterus is double; in most of them the vagina is also double; and there is always a single cloacal outlet for the excrementitious substances, and the products of generation. There is a corresponding uniformity in the male organs, which are bifurcated at the extremity, and have a double groove for the transmission of the semen; and the male has not only marsupial bones, similar to those of the female, but also a muscle, similar to that which surrounds and compresses the mammary gland in the female, winding round these bones like pulleys, and acting as cremasters for the retraction and compression of the testes.

A minute description is then given of the results of the dissection of the impregnated uterus of a kangaroo, which was obtained by Mr. George Bennett, during a short residence in New South Wales, and which, together with the impregnated uteri of the Ornithorhynchus and other valuable specimens, were sent to the Museum of the Royal College of Surgeons. The membrane corresponding to the chorion, or external envelope of the fœtus, was found not to have a vascular structure, and not to adhere in any part to the surface of the uterus; neither was there any appearance of a placental or of a villous structure. It adhered internally to a vascular membrane, into which the umbilical stem of the fœtus suddenly expanded, and which terminated in a well-defined ridge, formed by the trunk of a terminal blood-vessel.

The three omphalo-mesenteric, or vitulline vessels, were traced, from the umbilical cord into the abdomen, where they terminated in the usual manner; namely, the veins in the vena portæ, and the artery in the aorta. Hence it was apparent that the membrane on which they ramified, corresponded to the vascular layer of the germinal membrane, which, in oviparous animals, spreads over the yelk, or to the umbilical vessel of the embryos of ordinary mammalia. The ventricles of the heart were completely joined together, and bore the same proportions to each other as in the adult; a perfection of structure which is not observed in the embryos of ordinary mammalia at a corresponding period of developement. The lungs were equal in size to the heart, and were of a spongy texture, and full of red blood; their precocious developement, compared with that of the abdominal or digestive organs, being evidently a provision for their early or premature exercise.

From the close resemblance in the structures of the ovary and Fallopian tubes of the kangaroo with those of ordinary mammalia, and from the circumstance of the young being nourished, after birth, by a secretion from mammary glands, the author concludes that the ovulum in the former animal quits the ovisac in a condition corresponding to that in the latter class, and increases in a similar manner as it descends to the uterus. But as there is no formation of a placenta, it remains to be determined how the aeration of the fœtal blood is effected: this, however, probably takes place through the chorion, although this membrane is not vascular, to an extent sufficient for the purposes of the vital functions of a fœtus so imperfect, and whose uterine existence is of such short duration, as they are in this animal. Reasons are given, which render it probable that in the Marsupiata an allantois and umbilical arteries are developed at a later period of gestation, corresponding in this respect to the fœtal condition of the Batrachian reptiles, and corroborating the views entertained by the author, that the former family are essentially ovo-viviparous.

The author next proceeds to investigate the structure and condition of the mammary fœtus in the Marsupiata, or that stage of its existence when it is retained in the marsupial pouch, and derives its sustenance from the imbibition of milk from the mammary glands. He relates the observations which he has lately made on the fœtus of a kangaroo in the Menagerie of the Zoological Society*. He ascertained that the period of uterine gestation in the animal is thirty-nine days, and examined the fœtus a few hours after it had fixed itself to the nipple in the abdominal pouch, and when it was not much above an inch in length, and resembled an earth-worm, both in the colour and the semi-transparency of its integument. Four days afterwards, he detached it from the nipple, and observed that although it moved its limbs freely, it was unable to regain its former situation. The parent endeavoured to replace it by introducing its head into the pouch, which it held open with its fore paws; but these efforts were in-

* For an account of these observations, see our present volume, p. 304, in the number for April.—Edit.

effectual, and the next day the fœtus had disappeared, having, probably, been destroyed by the mother.

The last section of the paper is occupied by an inquiry into the structure and analogies of the female generative organs of the Marsupiata. These are traced throughout the successive orders of mammalia, to their connexions with various tribes of birds and reptiles, and is concluded by a disquisition on the final purposes of marsupial generation, and its relations to the other modes by which the continuance of the race is accomplished, in the more elevated orders of animals, and which appear to have reference to the greater expansion and perfection of the brain, and the greater developement of the intellectual faculties.

A paper was then read, entitled, "On a new Law of Combustion." By Charles J. B. Williams, M.D. Communicated by W. G. Maton, M.D., F.R.S.

The principal object of this paper is to prove that most combustible bodies undergo a kind of combustion, attended with light and heat, at a temperature considerably below that usually assigned as their point of ignition. This fact has been already noticed with regard to phosphorus and sulphur; and the pale blue flame produced in the vapour of ether by a hot palladium or platina wire, before the wire itself becomes vividly ignited, is another instance of the same general law, which the author finds applicable to all compound, and a few of the simple inflammable bodies. Of these he gives a variety of examples among oleaginous, resinous, and carbonaceous products, both animal and vegetable, which, when thrown on a hot iron, exhibit a pale and faintly luminous flame. Those on the other hand which are very volatile, such as camphor, the essential oils, ether and alcohol, rise in vapour before they reach the temperature necessary for their combustion; but they may be made to exhibit the same phenomena, by directing their vapour against a body heated below redness. The contact of pure oxygen gas immediately heightens the intensity of the light and heat evolved on these occasions, and excites them into a more decided and vivid combustion.

The author next adverts to the nature of the products of this low form of combustion, which, in organic substances, appear to him to form an intermediate link between those of open combustion, and those of fermentation and putrefaction. He considers the phenomena he has described as confirming the truth of a law he formerly announced, namely, that "the evolution of heat during chemical change is, *cæteris paribus*, proportionate to the degree of change from isolation, or weak combination, towards firm and simple union." He thinks they will afford an explanation of many cases of spontaneous combustion, which have hitherto been involved in mystery; such as that of porous combustible matters, as oily cotton, tow, or wool, when accumulated in considerable quantities, in places protected from cooling, or where air has limited access; and also of heaps of coal or charcoal, of pyrophori and pyrites; and the same principles may perhaps also account for the phenomena of the spontaneous combustion of the human body which are on record.

May 8.—A paper was read, entitled, "On the Connexion between Refracted and Diffracted Light." By Paul Cooper, Esq. Communicated by J. G. Children, Esq., Sec. R.S.

The purport of the present paper, as stated by the author, is to connect the phenomena of the dispersion of light arising from refraction, with that consequent upon diffraction, by showing, 1st, "that white refracted light is formed by the superposition of fringes of colours, or rays of light uniformly refracted, which compensate each other in succession; 2nd, that diffracted white light is formed by the superposition of fringes which are not uniformly refracted, but which recede from it so gradually, as not to prevent the same mutual compensation, although it is distinguished by other appearances; and 3rd, that the purity of the colour of the light, in both cases, depends upon its continuity, any interruption of which, although the different portions into which it may be separated are white at the moment the division takes place, produces colours in its further progress, because each portion carries with it the difference of direction required for their developement."

A paper was also read, entitled, "Observations on the Reciprocal Influence which Magnetic Needles exercise over each other, when placed at a given distance within their respective Spheres of Action, at different positions on the Earth; with Tables of numerical Results obtained at separate Stations. Also a method of discovering where certain local influences are acting on the Needle, from which may be obtained a proportional correction to be applied to Magnetic Observations in general." By Edward J. Johnson, Esq., Commander R.N. Communicated by Francis Beaufort, Esq., Capt. R.N., F.R.S.

GEOLOGICAL SOCIETY.

At the Annual General Meeting of this Society holden on the 21st of February, the President stated that the Council had awarded to M. Agassiz the proceeds of the Wollaston Fund for the past year, in order to promote his important work on Fossil Fishes.

The Meeting proceeded to ballot for the Officers and Council for the ensuing year; and the following gentlemen were elected:—

Officers.—*President*, George Bellas Greenough, Esq. F.R.S. & L.S.: *Vice-Presidents*, William John Broderip, Esq. B.A. F.R.S. & L.S.; Henry Thomas De la Beche, Esq. F.R.S. & L.S.; Roderick Impey Murchison, Esq. F.R.S. & L.S.; Henry Warburton, Esq. M.P. F.R S.; *Secretaries*, Edward Turner, M.D. F.R.S. L. & E. Professor of Chemistry in the University of London; William John Hamilton, Esq.: *Foreign Secretary*, Charles Lyell, Esq. F.R.S. & L.S.: *Treasurer*, John Taylor, Esq. F.R.S.

Council.—George William Aylmer, Esq.; Rev. Prof. Buckland, D.D. F.R.S. L.S. Professor of Geology and Mineralogy in the University of Oxford; Major S. Clerke, K.H.; Rev. W. D. Conybeare, M.A. F.R.S.; C. G. B. Daubeny, M.D. F.R.S. Professor of Botany and Chemistry in the University of Oxford; Sir Philip de Malpas Grey Egerton, Bart. F.R.S.; William Henry Fitton, M D. F.R.S. & L.S.; D. Gilbert, Esq. D.C.L. F.R.S. S.A. L.S. & H.S Hon. Mem. R.S. Ed.

M.R.I.A.; Woodbine Parish, jun. Esq. F.R.S.; Captain A. Robe, R.E.; Rev. Adam Sedgwick, M.A. F.R.S. Woodwardian Professor in the University of Cambridge; Lieut.-Col. Sykes; J. H. Vivian, Esq. M.P. F.R.S.; Rev. J. Yates, M.A. F.L.S.

The following Address was afterwards delivered by George Bellas Greenough, Esq. President.

GENTLEMEN,

YOU have learned from the Report of the Council that the Society has considerably gained in number since the last Annual Meeting. So large an accession of members shows the growing popularity of our science, and is at once a gratifying reward of your past exertions and a sure presage of your further success.

You have also been informed that during the same period the losses of the Society have been unusually numerous. Several of the deceased, whose main objects in life, if not alien, were connected but remotely with those of our institution, conferred upon it, notwithstanding, by their enlightened encouragement, important advantage: but the merits of the poet, the historian, the statesman, the warrior, though recorded in the annals of a grateful country, must not here be dwelt upon. To the memory of those only who have been closely allied to us, as fellow-labourers, will you desire that I should pay, individually, the well-earned tribute of our common regret.

The late Dr. Babington, whom we have been accustomed to look to with a respect almost filial, attached himself in early life to the study of chemistry and mineralogy. In the year 1795, he published a Systematic Arrangement of his collection of minerals purchased of the Earl of Bute, the finest, perhaps, which at that period existed in England; and in 1799, his New System of Mineralogy, which may be considered a continuation of the former work. These works, now superseded by others, which the introduction of improved modes of inquiry and the application of new instruments have rendered more perfect, evince much patient research and an exact knowledge of the state of mineralogy at that time. Active in the cultivation of science himself, Dr. Babington was quick to discern and eager to encourage merit in others. With a view to enable Count Bournon, of whom he had been a pupil, to publish his elaborate monograph on carbonate of lime, Dr. Babington, in 1807, invited to his house a number of gentlemen the most distinguished for their zeal in the prosecution of mineralogical knowledge. A subscription was opened and the necessary sum readily collected. This object having been accomplished, other meetings of the same gentlemen took place for the joint purpose of friendly intercourse and mutual instruction. From such small beginnings sprang the Geological Society; and among the names of those by whose care and watchfulness it was supported during the early and most perilous crisis of its history, that of Dr. Babington must always stand conspicuous.

But while Dr. Babington employed his leisure in the study of chemistry and mineralogy, he gradually rose into eminence as a physician, and at last became occupied with the care of a numerous family, and subjected to all the labour and responsibility of extensive medical practice. During many years, he was disabled from pursuing his favourite sciences with that unremitted attention which alone leads to original discovery; and accordingly our Transactions do not contain any communication from his pen: no man, however, more steadily cheered us in our progress or more heartily rejoiced in our success. In the year 1822, he was elected to the presidency of this Society, an office which he accepted in deference to the earnest wish of the Members, and held for two years at great personal sacrifice. His conduct in this chair afforded to us ample opportunity of observing the native goodness and kindliness of his heart, the urbanity of his manners, the evenness and cheerfulness of his temper, and the aptitude with which he exercised every liberal feeling.

During the presidency of Dr. Babington, and at his suggestion, was established the practice of submitting to immediate discussion the papers read at the table of the Society. Apprehensions were entertained by some persons at that time, that the collision of argument and the desire of personal distinction might interfere with the love of science or break the bonds of social intercourse,— that we might learn to contend less for truth than for victory. I appeal to you, Gentlemen, whether the brighter anticipations of Dr. Babington have not been amply justified by experience; whether our discussions, continued now during twelve years, have not been strongly characterized by a love of truth; whether the bonds of friendship have not been more closely cemented by them. Our conversations have been animated, but never intemperate; they have encouraged the timid, assisted the investigator in discovering the object of his research, and given additional value to every paper in our Transactions.

Dr. Babington was a Vice-President during the years 1810, 1811, 1812, 1813 and 1814, and a Trustee from 1811 to 1821. His donations to our library and museum were extensive, and from subscriptions set on foot to promote the objects of the Society his name was never withheld.

Dr. Babington retained to the latest period of his life a keen relish for the attainment of knowledge, and made considerable sacrifices to enable himself to keep up with its rapid progress. After descending from this chair he took private lessons in geology of Mr. Webster. So late as the winter of 1832–3 he enrolled his name at the University of London as a student of chemistry, and there attended with the utmost punctuality a course on that science of seven months' duration; he afterwards in the same spirit, and in his 77th year, once more applied himself seriously to geology, and went over the collection of fossils in our museum. I can scarcely imagine a more gratifying spectacle than that of a veteran in the labours of professional duty, thus returning to the pursuits which he had loved when young, and seeking relaxation, not in ease and repose, the allowable luxuries of old age, but in the indulgence of an enlightened passion for knowledge.

I need not apologize for these extended comments; they are more than justified by the occasion. The duties which your benefactor owed to the Society he cheerfully and fully performed. May his memory kindle in us a feeling not merely of gratitude but of emulation!

Dr. Berger, who died in the early part of last year, was a native of Switzerland, and had been employed in geological study for some years previous to 1813, when he sought in England an asylum from the foreign oppression which in those days of revolution had visited his country. In 1816, at the request of some of his friends in this Society, he agreed to devote himself for three years to geological investigations in the British Islands; and an annual sum was insured to him during that period by a subscription of some of our members. The north-west coast of Ireland was suggested for his first examination, and there, as might perhaps have been foreseen, the movements of a foreigner, who spoke our language imperfectly, and whose occupation must have appeared to the inhabitants mysterious, if not dangerous, at first excited doubt and obstruction, which, though not unamusing, were attended with some embarrassment, and called for the interference of his friends. He laboured with great zeal and assiduity, in that interesting field of inquiry, till his health unfortunately gave way His papers and collections were therefore incomplete ; and his attention appears to have been given perhaps too much to the investigation of details not immediately connected with the proper and immediate business of the geologist. His merit, however, must be judged of, not by reference to the present state of knowledge and the methods of inquiry now pursued, but to the condition of the science at that time. The facts he accumulated were valuable. "A Memoir on the Dykes of the north-east coast of Ireland," by himself, appears in the third volume of our Transactions; his remaining papers were put into the hands of the Rev. William Conybeare, who subsequently went over the same country with Dr. Buckland; and we are indebted to the labours of Dr. Berger, extended and illustrated by these geologists, for one of the most valuable memoirs in the earlier volumes of our Transactions. The late years of Dr. Berger's life were passed in his native country, in bad health: he died at Geneva in 1833.

In perusing at the distance of so many years the record of the arrangement by which Dr. Berger's services were obtained for this Society, and the names subjoined*, I have been much struck by the delicacy with which his personal feelings were consulted, and have looked back with pride and exultation to the early history of our institution. I cannot be surprised at the success which has attended your exertions, when I call to mind the noble and disinterested spirit by which the first steps in your progress were directed. On no occasion since I have known the Geological Society, (and I have known it from its birth until the present hour,) have the Members hesitated to contribute, with the most liberal devotion, both personal labour and pecuniary support, whenever the *probable* advancement

* The paper bears, with the names of other Members who still remain, the signatures of the late Dr. Babington, Dr. Marcet, Mr. Francis Horner, Mr. Morgan, Dr. Wollaston, Sir Joseph Banks and Mr. Ricardo.

of science appeared to call for them. I mention this with double satisfaction, because I am convinced that this good spirit still subsists amongst us with undiminished vigour.

Dr. Alexander Turnbull Christie imbibed in the class-room of Professor Jameson a taste for geology, which he afterwards improved in India, as far as opportunity allowed, under many discouraging circumstances. On his return to Europe he applied himself to the science with great earnestness; he studied the best works, courted the society of their authors, familiarized himself with the contents of collections, and practised in the open air the most approved methods of investigation. He became the pupil of M. Brongniart at Paris, and the companion of M. de Beaumont and M. von Buch in the Alps. His studies were by no means confined to geology; they embraced every department of natural history. The climatological and geographical distribution of plants was a subject to which he paid much attention. Having provided at his own expense the best instruments for the purpose, he returned to India with the design of instituting there a continued series of barometric, hygrometric, and other experiments, as well as of exploring the physical structure of that vast region, and of determining the relations of its rocks to those of Europe. On his way he visited Sicily, and transmitted to the Society an account of some of the younger deposits of that island, and the phænomena that accompanied their elevation. He wrote also a description of some bone caves near Palermo, and of tidal and other zones observed on limestone along the shores of Greece. These notices will be found in Jameson's Journal. Dr. Christie died prematurely of a jungle fever, while crossing the Nilgherry hill in November 1832.

Mr. Lansdown Guilding, though not himself engaged in the pursuit of geology, added several valuable specimens to our collection, and materially assisted the progress of some other branches of natural history, especially in connexion with the West Indies.

Sir Charles Giesecke was born at Augsburg in 1761. He was originally intended for the church; after various changes of occupation and a life of some adventure, he devoted himself in about his fortieth year to mineralogy, and studied under Werner at Freyberg in 1801. He subsequently travelled with mineralogical views in several parts of the North of Europe; in 1806 he entered into the service of Denmark and repaired to Greenland, leaving at Copenhagen a valuable collection of books and minerals, which were destroyed during the bombardment of that city. In Greenland he formed acquisitions of great interest in various departments of natural history, but foreseeing the probability of their capture on the passage to Europe, he with great resolution and perseverance went a second time over the ground he had examined, and remained in that desolate region till his object was accomplished. In the mean time the vessel which contained his first treasures was taken, and the cargo sold by auction at Leith. The minerals attracted but little general notice, in part, I have been informed, from their being packed in moss and sea-weed, and perhaps also from the very circumstance of many of the species being unknown. Mr. Allan purchased nearly the whole collection, which upon examination proved to contain a great number of new and rare substances of the highest mineralogical interest, cryolite, sodalite, allanite, with mixed groups of striking variety and novelty; and all in such abundance that most of the cabinets of England (when collectors, if not more numerous, were at least more active than I fear they are at present,) were supplied from this source. Mr. Giesecké himself accidentally arrived at Leith in 1813, not long after Mr. Allan had published an account of his purchases, and with great generosity contributed to the improved catalogues and descriptions of specimens which subsequently appeared. He was soon after appointed Professor of Mineralogy to the Royal Dublin Society, and went to reside in Ireland, where he spent the remainder of his life. About this period also he was honoured with an Order of Knighthood by the King of Denmark; but having now passed his fiftieth year, his health was broken, and much of the energy lost which distinguished his early life. He lived to the age of 72, and died at Dublin in March 1833. Sir Charles Giesecké meditated, after his return from Greenland, an extensive work upon that country; he published a brief account of it in Dr. Brewster's Encyclopædia, but the larger work was deferred till the voyages of Ross and Parry had deprived the subject of the interest of novelty. His meteorological observations appeared in the Edinburgh Philosophical Journal for 1818; and he gave to Mr. Scoresby, for his work on the Greenland Coast, the use of his maps and other materials. The Edinburgh Philosophical Journal for 1822, contains an account of his discovery of the geological situation of Cryolite. His only publications on the mineralogy of Ireland are, I believe, a brief notice of the geological situation of Beryl in the county of Down*, and an account of an excursion to the counties of Galway and Mayo†.

Mr. Alexander Nimmo was a civil engineer of high reputation. He was born in Fifeshire in 1783, and at a very early age showed a strong propensity to physical and mathematical inquiry. One of his first public employments was a survey of some of the bogs in Ireland, on which he delivered a report to the Commissioners in 1811, containing some general observations on the geological character of part of Roscommon, Kerry, Cork and Galway. He was afterwards engaged in various works of great importance, principally in Ireland. He was the author of several articles in Brewster's Edinburgh Encyclopædia, on subjects connected with his profession. One of his latest and most valuable literary productions, on the publication of part of which he was engaged at the period of his death in January 1832, was a Chart of the Irish Channel, with sailing directions for the coast of Ireland, a performance probably

* Annals of Philosophy, 1825. New Series, vol. x. pp. 74 & 75; republished from the Dublin Philosophical Journal.

† Annals of Philosophy, 1826; republished from the Dublin Philosophical Journal.

connected with a paper which he laid before the Royal Irish Academy "On Geology as applicable to the Purposes of Navigation."

Mr. David Scott was one of the numerous class of officers in the service of the East India Company who have found means to combine with the most exemplary discharge of their official duties, a constant attention to the interests of literature and science. He was the second son of Archibald Scott, Esq., of Montrose, and died prematurely in India in 1831, at the age of 45, having passed through many offices of high trust with distinguished credit, and holding at the time of his death the situations of Civil Commissioner in Bungpoor and other districts, and agent to the Governor General in the North-east of Bengal. His exertions and success in discharging his official functions, and in promoting the welfare of the country in which he was placed, by diffusing education, were highly appreciated, and a monument has been erected to his memory by the Supreme Government of India. Mr. Scott possessed great knowledge in several branches of science not immediately connected with this institution, and lost no opportunity of attending to geological research. Our Transactions are indebted to him for the substance of a valuable paper communicated by Mr. Colebrooke*, "On the Geology of the North-eastern Border of Bengal," in which is described a remarkable deposit on the left bank of the Burrampooter river, containing an assemblage of fossils that bear an extraordinary likeness to those of the London clay. "Among the remains of fishes," Mr. Colebrooke states, "bony palates and the fins of the Balistes are common to the Indian clay and to that of Sheppey; and the shells of Cooch-behar bear a strong generic, if not specific, resemblance to the marine formations above the chalk in France and England." This communication contains also some valuable facts respecting a succession of strata, like those of our coal-fields, in the Tista and Subuk rivers; and in the same volume, is an extract from a letter written by Mr. Scott, describing the situation of a limestone and clay containing Nummulites at Robagiri, a village in the North-east of Bengal. Such resemblances, though they are far from establishing the contemporaneous formation—much less the continuity—of the groups in which they occur, are interesting, from the proof they furnish, of the operation of similar causes in very distant parts of the former surface of the globe.

On the accounts of the past year put into your hands today, I will make but one observation. From the report of the Auditors it appears that the balance of disposable property in favour of the Society, taken at a very moderate estimate, is £2010, while the total amount of the compositions of all the compounders in the List of Fellows since the foundation of the Society does not exceed £2394. The difference is less than £400. If, then, the value of the collections, library, and furniture belonging to the Society be taken into account, our actual property considerably exceeds the claims of all

* Series II. vol. i. pp. 132—140.

our compounders, our current income being wholly disposable and free.

Wollaston Medal.—The product of the Wollaston Fund during the past year has been awarded to Mr. Agassiz of Neufchatel, in promotion of his work on the "General History of Fossil Fishes."

The first part of Mr. Agassiz's publication has but recently reached England, and the Council have availed themselves of the earliest opportunity of giving support to an undertaking of great geological importance. The author's qualifications for this work were so highly appreciated by the late Baron Cuvier, who had himself been engaged in a similar project, that on seeing Mr. Agassiz's collection of drawings, and hearing a statement of his views, and the results at which he had arrived, that profound naturalist at once transferred to Mr. Agassiz the whole of his materials. The approval of Cuvier is fully sanctioned by the portion of the work which is now before the Society. In deciding on the present award, the Council have acted strictly in compliance with the bequest of Dr. Wollaston. The work of Mr. Agassiz is intimately connected with the objects of this Society; it demands for its completion great labour and expense. It is still in progress, and its publication has been ably commenced with a full assurance of the author's competency to the fulfilment of the task he has begun.

In his prospectus, Mr. Agassiz solicits the contribution of specimens from all quarters; and I cannot better close the announcement of a testimony of approbation which I trust will be gratifying to his feelings, than by requesting the Fellows of the Geological Society to aid the progress of this important work, by giving or lending to its author any drawings and specimens of fossil fishes which they may either possess or obtain. The transmission and return of these loans can be easily effected through the medium of the officers of this house.

The History of Geology has been recently treated by several authors, especially by Mr. Conybeare and Mr. De la Beche, in a manner which would render any observation from me on that subject at once superfluous and imprudent. The communications read at our general meetings have been fixed in your memory by the discussions to which they have given rise, and the published abstract of their contents. Still, however, it may be well to enumerate these communications, that you may measure the exertions made here since the last Anniversary, and the effect they have had on the state of geological knowledge.

Miscellaneous.

The experiments of Sir James Hall mark an important epoch in science. It was with great delight, therefore, that we received from Captain Basil Hall, R.N., a collection of the products of these experiments, and some of the instruments with which they were conducted. Among the latter is a machine for regulating high temperatures, accompanied by an account of its properties and mode of acting.

Mr. Gardner, the well known geographer, has drawn our attention to the curious fact, that of the land on the surface of the globe only $\frac{1}{27}$th part has land at its antipodes.

Sir David Brewster has communicated to us his interesting observations on the properties of the diamond, from which it would appear to be of vegetable origin,—the cavities whence these properties are derived being found in amber, but not in any product either of igneous fusion or of aqueous solution.

Home Geology.

Dr. Mitchell has laid before the Society a detailed account of the geology of Harwich in Essex, of the Reculvers in Kent, of Quainton and Brill in Buckinghamshire. Mr. Dadd has described the Vale of Medway and its neighbourhood. Dr. Fitton, who published in the early part of the year, a geological sketch of the vicinity of Hastings, has supplied us with an account of some instructive sections recently exposed to view at St. Leonard's. Mr. Woodbine Parish has sent to us portions of the Iguanodon and Lepidosteus from the well known "White Rock," situate in the same district, and now almost destroyed. Our knowledge of the inland Extent of the Wealden Formation has been enlarged by a paper of Mr. Strickland, accompanied by specimens of Paludina from the ferruginous sand of Shotover Hill.

Mr. Strickland has also rectified the boundaries of some of the strata near Bewdley, and traced a line of fault from the north of Bredon Hill to Little Inkbarrow.

Sir Philip Egerton has supplied us with further information in respect to the lower portion of the Connaught Coal-district. Beneath the coal at Kulkeagh in the county of Fermanagh is a shale 600 feet thick, with subordinate layers of black marlstone and clay-iron ore towards the top, and a thin stratum of micaceous grit near the bottom. All the beds are replete with ammonites, orthocera, producta, encrini, corals and calamites. This deposit lies on sandstone separated by the mountain limestone from another bed of shale marked by characteristic fossils, and the entire system therefore appears to bear a strong resemblance to the lower portion of the carboniferous beds in the South-west of England.

In the carboniferous strata of Coalbrookdale, Mr. Prestwich has described a heterogeneous assemblage of plants and shells both of fresh- and salt-water species. A band of ironstone, nearly in the centre of this series, contains four genera of Trilobites: in the same coal-field Mr. Anstice has recognised two genera of insects. On the opposite side of the Severn, Mr. Murchison has found at Pontesbury, Uffington, Le Botwood and other places, a band of compact limestone, between two beds of coal, resembling the lacustrine limestone of central France, and containing freshwater shells. These discoveries may throw light on those which have been since made at Burdie-house and elsewhere in the neighbourhood of Edinburgh.

The structure of other coal-fields has been illustrated by Mr. Murchison, Mr. R. J. Wright, and Mr. England.

After careful examination of the Old red Sandstone, Mr. Murchison has proposed to divide it into three parts: the uppermost, characterized by quartzose Conglomerate; the middle, by Cornstone; the lowermost, by Flagstone. The cornstone and marlstone of the middle group contain undescribed genera of crustacea; and in the tilestone beneath are found some defences of fish, together with a few remains of testacea.

Mr. Murchison has employed three summers in examining a range of country situate between Shrewsbury and Caermarthen; and the geological positions as well as the mineral and zoological characters of the several rocks which border England and Wales are now determined with as much exactness as those of any portion of the secondary system. Taking the old red sandstone as a line of departure, the rocks beneath are disposed in descending order as follows:

1. The Ludlow series, divisible into three parts, the upper, middle and lower. To the middle belong the well-known limestones of Amestry and Sedgley: the upper and lower consist of sand, marl, or flagstone, having some fossils peculiar to each, and others in common. The thickness of the whole is estimated at 1000 feet.

2. The Dudley or Wenlock series, consisting of limestone: its thickness may be taken at 2000 feet.

3. The Hordesley or May Hill series, composed of party-coloured sandstone, conglomerate and impure calcareous flagstone: it is said to attain a thickness of 2500 feet.

4. The Built or Llandilo series, a black flagstone, characterized by the *Asaphus Buchii.*

5. The Longmynd or Linley series, consisting of coarse roof slate, sandstone and conglomerate; no fossils have been discovered in it.

It is well known that Professor Sedgwick has studied with equal assiduity the rocks which lie beneath those I have mentioned. When his observations are published, the Society will have a type of the whole of the transition rocks of Wales. The rocks described by Mr. Murchison are, for the most part, exceedingly well characterized by their fossil contents. Some of the shells which he has discovered, appear to have escaped the notice of antecedent observers; but the genera, if not the species, of others, may occasionally be found in the works of Hisinger and other continental writers. If, then, the transition as well as the secondary and tertiary beds can be identified over great tracts of country by their fossil remains, let us hope that a clue is now at hand, by which we may find our way through that vast assemblage of beds, which, not in England only, but in Scotland, Ireland, Germany, Russia, Sweden, and North America, has hitherto presented to the observer a mere scene of confusion.

In Mr. Murchison's paper we find also, traced with exactness, several hitherto unexplored lines of disturbance, producing sometimes, as in the Abberley Hills, a complete inversion of dip. The rocks which border the old red sandstone, acquire in some places an anticlinal dip, and reappear in parallel ridges far westward of their natural site, insomuch that the Ludlow series is met with even in Montgomeryshire. Mr. Murchison has examined in detail the trappean and porphyritic rocks to which these disturbances are for the most part assignable, but the description of them has been reserved for communications not yet before us.

Professor Sedgwick has transmitted to us a notice on the granite of Shap in Westmoreland. From recent excavations it appears that veins of this granite penetrate the adjoining strata, from which he infers that it is of posterior date.

Mr. De la Beche, one of our Vice-Presidents, acting under the direction of the Board of Ordnance, has produced a geological map of the county of Devon, which, for extent and minuteness of information and beauty of execution, has a very high claim to regard. Let us rejoice in the complete success which has attended this first attempt of that honourable Board to exalt the character of English topography by rendering it at once more scientific and very much more useful to the country at large.

Organic Remains.—Every succeeding year brings to light new fossil animals which cannot be assigned to existing genera. Dr. Riley, deeply skilled in physiology and comparative anatomy, has given us an account of an animal so extraordinary, that naturalists differ even respecting its class. After careful examination, he considers it a cartilaginous fish, partaking of the character both of the Rays and the Squales. Here then is another instance of a link, now wanting to connect existing genera, having formerly existed.

Towards the close of the last session Mr. Channing Pearce exhibited to the Society a matchless collection of Apiocrinites found at Bradford in Wiltshire. To the description of this fossil as given by the late Mr. Miller, Mr. Pearce adds that the column was occasionally ten inches long. He has found in the great oolite, three species of Apiocrinites, differing in the form of their body, and the thickness of its component plates.

Foreign Geology.

Europe.—The structure of the South of Spain has been illustrated by Colonel Silvertop and Captain Cook. From the joint labours of these gentlemen we learn, that the country between the Sierra Morena and the Mediterranean consists of lofty ranges of granite, slate, serpentine and limestone, succeeded either by red sandstone or by vast beds of secondary, compact, dolomitic limestone. We also learn from them that the valleys and plains which border the shore of the Mediterranean, are composed of tertiary strata; but we are indebted solely to Col. Silvertop for pointing out to us, on the authority of M. Deshayes, that the tertiary deposit of Malaga and the districts adjacent belongs to the Pliocene, while that of the basins of Baza and Alhama belongs to the Miocene epoch.

Mr. Lyell has laid before us an account of the lignite formation of Cerdagne in the Eastern Pyrenees. This lacustrine deposit reposes in horizontal beds on granite and hornblende and argillaceous schist at the height of 3000 and 4000 feet above the level of the sea. The shells procured are too imperfect to determine its age.

A memoir on the neighbourhood of Bonn was presented last year by Mr. Horner. After describing the characters of the grauwacke, trachyte, basalt, brown coal, gravel and löss, the author compares the age of these with that of analogous formations in other parts of Europe, and of one another. The beds of grauwacke as they contain Terebratulæ and other shells he refers to the upper part of that system; he considers the brown coal more recent than the plastic clay, some of its plants and shells having been identified with specimens found at Aix en Provence. The löss, which reposes on a thick bed of gravel, and contains existing land shells, together with bones of extinct quadrupeds, is considered the latest deposit, and attributed to the bursting of a lake in the upper part of the Rhine. From the beds of trachytic tuff being interstratified with brown coal, and from the occurrence of a bed of basalt above it, Mr. Horner infers that volcanic operations took place during, and even subsequently to, the deposition of the lignite. Having thus established the comparative age of the brown coal, he also determines that of the volcanic rocks.

The tertiary coal or lignite near Gratz, in Styria, is interesting on account of its organic remains. In the memoir of Professor Sedgwick and Mr. Murchison on the Eastern Alps, the strata of this deposit, which are nearly horizontal, are shown to rest on "an inclined system of secondary green-sand." Imbedded in the coal are various vegetable remains, shells of a Cypris, scales of fishes, and fragments of bones of Mammalia and Tortoises. Professor Anker of the Joanneum, has sent to the Society an account of these, together with the drawing of a jaw, which Mr. Clift conceives to have belonged to a Hyæna.

Mr. Pratt, ignorant of the prior researches of Dr. Christie, carefully examined, in the year 1832, the caves of Monte Grifoni near Palermo; and having ascertained the height to which the perforations of lithodomi extend in each, infers that the change of level was not effected by one movement, but by several.

Asia.—Much information has been received from the East during the past year. Mr. Burnes, distinguished for his travels in India, Persia and Toorkistan, has presented to the Society his geological memoranda of the countries lying between the mouth of the Indus and the Caspian Sea. Mr. Burnes, though he did not travel for the express purpose of studying geology, carefully and faithfully noted whatever attracted his attention. In reading his account of these hitherto almost unknown regions, we cannot but be struck with the resemblance of their geological structure to that of Europe. The central axis of the Hindoo Koosh is composed of granitic rocks, succeeded by various schists, conglomerates, variegated marls, limestones and sandstones. Besides this mighty system, some portion of which cannot be identified with European strata for want of fossils, there is a vast range of salt (previously noticed by Mr. Elphinstone), of coal, and, near the mouth of the Indus, nummulitic limestone.

In a late number of Jameson's Journal is part of a memoir on the structure of the Valley of Ovelipore* by Mr. Hardie, one of our recently elected Fellows.

This valley had previously been noticed by Captain Dangerfield†; but Mr. Hardie has been the first to describe a singular Indian formation which occurs there, called Kunkur. It is rarely, if ever, stratified; it forms a bed, seldom exceeding a few feet thick, which mantles over the irregularities of the country. It is sometimes imperfectly oolitic; at others globular, botryoidal or nodular; in some places a compact limestone; in others it resembles chalk: not unfrequently it contains round and angular fragments of rocks. No animal or vegetable remains have been noticed in it. The author carefully distinguishes Kunkur from modern tufaceous deposits, but assigns to it a similar origin.

America.—Captain Colquhoun and Mr. Burkart have presented to us a specimen of native iron from Zacatecas, and memoranda on this and similar masses found in Mexico.

Captain Bayfield has communicated to us a paper on the shores of the River and Gulf of St. Lawrence from the Saguenay to Cape Whittle. The information contained in this memoir completes our knowledge of the north coast of the St. Lawrence‡; and from the previous labours of Mr. Green in the district of Montmorency§; of Lieutenant Ingall in the country bordering the rivers St. Maurice and aux Lievres‖; of Captain Bonnycastle in Upper Canada¶; of Dr. Bigsby**, Captain Bayfield†† and Dr. Richardson‡‡, on the shores of Lakes Ontario, Erie, Huron and Superior; and of Dr. Richardson in the overland expeditions to the Arctic Seas, we have a general account of the geological structure of the whole country between the mouths of the Mackenzie and Copper Mine rivers and the Gulf of St. Lawrence. The researches made during the expeditions of Captain Ross, Sir Edward Parry and Sir John Franklin, have also given us a general insight into the nature of the formations which constitute a large portion of the shores of the Western Polar Seas. Why should I repress the feeling of patriotic pride which rises within me on contemplating how vast a range of the western continent has thus, in the brief period of a few years, been brought within the pale of our science almost entirely by the exertions of English officers? Great is the gratitude we owe them; yet have their services not been wholly without reward. The

* The city of Ovelipore is in lat. 24° 25′ N. long. 73° 44′ E.

† See Sir John Malcolm's Central India.

‡ See on the country between the St. Maurice and the Saguenay, Trans. Quebec Society, vol. ii. p. 216. On the Saguenay country and St. Paul's Bay, *ibid.* vol. i. p. 79; vol. ii. p. 76. On Quebec, Proceedings Geol. Soc. No. 5, p. 37.

§ Quebec Trans. vol. i. p. 181.

‖ *Ibid.* vol. ii. p. 7.

¶ *Ibid.* vol. i. p. 62.

** Proceedings Geol. Soc. No. 3, p. 23. Trans. Geol. Soc. Series II. vol. i. p. 175. Journal Royal Institution, vol. xviii. pp. 1, 228.

†† Quebec Trans. vol. i. p. 1.

‡‡ Appendix, Expedition to Polar Seas.

taste for scientific research which sprung up in the minds of these gallant men, spontaneously, as it were, and without the aid of regular systematic culture, has been to many of them a welcome relief from the toil and monotony of professional duty; while to others it afforded pleasurable occupation in the solitude of trackless deserts, under exposure to all the rigour of an arctic climate, in the absence of European indulgences, and even under the terrible apprehension of impending starvation.

The district surveyed by Captain Bayfield is bounded by hills, composed of granite, sienite and trap rocks, which enter so largely into the structure of the two Canadas. Clay, sand and gravel, apparently recent, occupy the coast. The Mingan, the Esquimaux and Anticosti Islands are of limestone, containing fossils like those of Lake Huron. But the most interesting feature in this communication is the evidence it affords of a change in the relative position of land and water. In the Mingan Islands is a series of shingle terraces, agreeing in character with the recent beach, the most distant being 60 feet above the level of the highest tide. The author describes, with great care, the different vegetation of each terrace, the one furthest from the shore being covered with trees, the nearest almost barren; parallel to the shore, in this island, natural columns of limestone have been scooped out by the action of water at different periods; the levels of the water-worn portions agree with those of the terraces, and the depth of the scooped parts, with the rise of the present tidal wave of the St. Lawrence. Captain Bayfield has noticed similar terraces on the adjacent mainland and in the neighbourhood of Quebec, and thinks the phænomena indicate successive elevations of the land rather than successive depressions of the water.

[To be continued.]

LINNÆAN SOCIETY.

May 24, 1834.—At the Anniversary Meeting held this day, His Grace the Duke of Somerset was elected President of the Society, in the room of Lord Stanley, who had resigned; Edward Forster, Esq. was re-elected Treasurer; Francis Boott, M.D., Secretary; and Richard Taylor, Esq., Under Secretary; and the following five gentlemen, in addition to the Duke of Somerset, were elected into the Council, in the room of others going out, agreeably to the By-laws: viz. Thomas Bell, Esq.; Rev. J. S. Henslow, M.A.; Capt. J. Clark Ross, R.N., and William Spence, Esq.

ZOOLOGICAL SOCIETY.

January 14, 1834.—Several *crania* were exhibited of the *Lion* and of the *Tiger*, forming part of the Society's Museum, on which Mr. Owen explained the distinguishing characteristics of that part of the osseous system of these two large species of *Felis*. He adverted in the first instance to those pointed out by Cuvier in the 'Ossemens Fossiles', and remarked on the first of them,—the straightness of the outline in the *Lion* from the mid-space of the postorbital processes to the end of the nasal bones, in one direction, and to the *occiput* in the other,—as not being in all cases available: the second distinction,—the flattening of the interorbital space in the *Lion* and its convexity in the *Tiger*,—he regarded as being more constant and appreciable than the one just mentioned. There is, however, a distinction which he believes has never been published, which is well marked, and which appears to be constant; for it is found to prevail throughout the whole of the skulls of these animals which he has had opportunities of examining, including ten of the *Lion*, and upwards of twenty of the *Tiger*. It consists in the prolongation backwards, in the *cranium* of the *Lion*, of the nasal processes of the maxillary bones to the same transverse line which is attained by the coronal or superior ends of the nasal bones: in the *Tiger* the nasal processes of the maxillary bones never extend nearer to the transverse plane attained by the nasal bones than ⅓rd of an inch, and sometimes fall short of it by ⅔rds, terminating also broadly in a straight or angular outline, just as though the rounded and somewhat pointed ends which these processes have in the *Lion* had been cut off. Minor differences, Mr. Owen, remarked, exist in the form of the nasal aperture, which in the *Tiger* is disposed to narrow downwards, and become somewhat triangular, while in the *Lion* its tendency is towards a square shape; in the deeper sinking, in a longitudinal depression, of the coronal extremities of the nasal bones in the *Tiger* than in the *Lion*; in the bounding of this depression above in most of the *Tigers' crania* by a small but distinct semilunar ridge, which is not found in those of the *Lion*; and in the larger comparative size, chiefly in their transverse diameter, of the infraorbital *foramina* in the *Lion*. These *foramina*, it is curious to observe, are double either on one or both sides in the only four *crania* examined of *Lions* which were known to be Asiatic, while in all the others the *foramen* was single on each side.

Specimens were exhibited of *Placunanomiæ* from the collection of Mr. Cuming, and some Notes by Mr. Broderip respecting them were read, from which the following are extracts:—

Genus *Placunanomia*. Since my publication of this genus in the 'Proceedings of the Committee of Science and Correspondence*,' Mr. Cuming has found among his stores the following three species in addition to *Plac. Cumingii*, which I have already recorded: viz. PLAC. *rudis*, *foliata*, (The outer surface of the lower valve, which has been attached throughout its whole extent, bears a somewhat crystalline appearance; and this observation may be applied to the adhering surface of *Plac. rudis*,) and *echinata*. The last-named species varies much in shape, according to circumstances. Mr. G. B. Sowerby possesses one of an irregular ovate form. Indeed *Placunanomia*, in common with other adherent genera, varies much in shape, accommodating its external form to the surface to which its lower valve is attached. It is remarkable also for putting on the appearance of other genera or species; and this, with the extreme closeness of the adhesion of the lower valve, has been perhaps one of the causes why it has escaped the notice of zoologists.

* See Lond. and Edinb. Phil. Mag., vol. i. p. 394.

Thus, *Plac. Cumingii*, to a casual observer, looks like one of the plicated *Oysters*; *Plac. rudis* greatly resembles the *common Oyster, Ostrea edulis*; and *Plac. echinata* wears something of the appearance of some of the short-spined *Spondyli*.—W. J. B.

Besides the species above recorded Mr. G. B. Sowerby has kindly furnished me with an odd valve of a large species from Luçonia, beautifully iridescent internally: but as it is believed that this is identical with the fine shell sold by him to the British Museum, I leave the description of it to the officers of that institution, in whose province it is, and who are so fully capable of doing it justice.

This genus, then, appears to be widely diffused. Mr. G. B. Sowerby has some other odd valves which may prove new. I possess two or three specimens adhering to *Spondyli* from an unknown locality; but they appear to be young, and, though I am inclined to think that there is among them a new species, I wait for further information before I venture to characterize it.—W. J. B.

Mr. Owen read the following Notes on the Anatomy of the *purple-crested Touraco, Corythaix porphyreolopha*, Vig.

"In commencing the anatomical examination of this *Bird*, my attention was first directed to the form of the tongue. This was large, and not confined to the posterior region of the mouth, but extended to the end of the lower mandible: its *apex* was beset with a few small horny bristles directed forwards, as in the *Toucans, Rhamphastos*, Linn., but much less produced than in those birds. It is probable that the ripeness of fruit on which these birds feed is tested by these yielding processes. The base of the tongue was, as usual, beset with retroverted *papillæ*, and elevated into a distinct ridge, serving, as in many of the cold-blooded *ovipara*, as an *epiglottis*. The interspace between this ridge and the laryngeal aperture was very glandular. That aperture was simple and terminated posteriorly by two retroverted spines; so that it is defended in some degree against regurgitated food as well as from that which is swallowed.

"The *œsophagus* is continued down to the stomach of uniform ample width (its diameter being $\frac{2}{3}$rds of an inch) without any dilatation or *ingluvies*, as in the true *Rasorial* birds. Its termination for about $\frac{3}{4}$ths of an inch is occupied by the zone of gastric glands, forming the *proventriculus*, which does not deviate in capacity or course from the rest of the gullet. The gastric follicles are simple, elongated and rather flattened. The gizzard is small and weak in its *parietes*, resembling that of the *Toucan*. Its length is 1 inch 4 lines; its greatest diameter 10 lines. The lateral tendons are distinct, and the narrower portion beyond the *pylorus* has the strongest muscular coat, which, however, does not exceed at this part $\frac{1}{3}$rd of a line in thickness.

"The capacity of a gizzard of this structure is obviously one reason why a crop or reservoir is not required: where the muscular *parietes* encroach upon the digestive cavity, so as only to allow small portions of food to enter at a time for the purpose of undergoing trituration, then a crop is as necessary to the gizzard as the hopper to a mill. It is also required in some of the most carnivorous birds to enable them to glut themselves with portions of their prey when too bulky to be borne away entire, and thus to carry off more than the true digestive cavity can contain. But in birds which, like the *Toucans*, the *Hornbills*, the *Parrots*, and the *Touracos*, live amidst abundance of nutriment, and that of easy digestion, a superadded cavity to act as a reservoir, or to submit the food to maceration previous to its entering upon the digestive process, appears unnecessary.

"The intestinal canal in the *Touraco* has a similar affinity to that of the tribes of *Birds* above mentioned, being short, ample and without *cæca*. It measured twice the length of the bird from the end of the bill to the vent. A small pyloric canal intervenes between the gizzard and *duodenum*, and opens into the latter upon a valvular prominence. The *duodenum* suddenly dilates, and has a diameter of half an inch; but I am doubtful whether this is natural, as it was, in the present instance, distended with *Tæniæ*, which had perforated it in some places, and probably caused the death of the bird. The fold of the *duodenum* is 3 inches long, including a narrow bilobed *pancreas*. The intestine gradually diminishes in diameter to within 5 inches of the *cloaca*, when it suddenly dilates, and this portion has the usual disposition and course of the *rectum* in birds.

"The liver was composed, as usual, of two lobes. There was a gall-bladder, of an elongated form, with the cystic duct continued from the end furthest from the intestine. The mode of termination of the biliary and pancreatic ducts I was unable to determine, owing to the morbid adhesions caused by the irritation of the *Tæniæ*.

"The *testes* were small. The kidneys and supra-renal glands were of the usual structure.

"From the affinity pointed out by Cuvier between the *Touraco* and the *Curassows*, I examined carefully the structure of the *trachea*, so remarkable for its convolutions in the latter family of birds. It was, however, continued straight to the inferior *larynx*, and was connected to the *furculum* only by a slight *aponeurosis*: the sterno-tracheal muscles, a single pair, were strong in proportion to the size of the bird. The rings of the *trachea* were of a flattened form, gradually diminishing in size towards the lower extremity of the tube. The lungs were of the usual form and structure, and the air-cells apparently not extending along the neck, or beyond the abdominal cavity, except to penetrate the osseous system; but of this I cannot speak with safety, as the bird was skinned before I dissected it.

"The eye of the *Touraco* is large, measuring 7 lines in lateral diameter. The *lens* is very convex posteriorly, and its capsule is attached to a narrow *marsupium*.

"The clavicles were united, forming an *os furcatorium*; but they were extremely weak, and yielded with facility at the point of union. The keel of the *sternum* was of moderate size, its greatest depth being to the length of the *sternum* as 1 to 4. The posterior margin of the *sternum* has two notches on either side of the keel, as in the *Toucan*; the lateral ones extending along two thirds the length of the *sternum*, the mesial ones about one third.

"After this detail it is scarcely necessary to observe that in all the important points of the internal structure the *Touraco* manifests close relationship to the *Scansorial* order, and a marked deviation from the typical structure of the *Rasores*, in which the superadded lateral dilatations of the alimentary tube, the crop and *cæca*, are so largely developed.

"The same affinity is also shown in the nature of its parasitic worms,—the *Tæniæ* belonging to the species *filiformis* of Rudolphi, so remarkable for the length and tenuity of the body, and which has hitherto been met with only in the *Psittacidæ*.

"I had an opportunity in this instance of witnessing very satisfactorily the mode of generation of the *Tænia*. Many separate joints were found in the track of the intestines, which, when viewed under the lens, were seen full of *ova*. Each of these joints contained from thirty to thirty-three *ova*, of a subglobular form, and a surface rendered irregular by minute asperities. The posterior joints of the unbroken worms were similarly distended, and readily separated.

"This division of the body approximates to the fissiparous mode of generation; but as the joints are merely the capsules of the *ova*, it is more strictly analogous to the mode of generation in the *Lernææ* and *Entomostraca*."

January 28, 1834.—A preparation was exhibited of the stomach of *Semnopithecus Maurus*, F. Cuv., presented to the Society by G. H. Garnett, Esq. It was brought under the notice of the Meeting for the purpose of showing that there exists in that *Monkey* the extremely elongated and sacculated form of the *viscus*, which was first described by M. Otto, as occurring in *Semn. leucoprymnus*, and which was subsequently exhibited by Mr. Owen, at the Meeting of June 11, 1833*, as obtaining also in the only two species of the genus which he had then examined, the *Semn. Entellus*, F. Cuv., and the *Semn. fascicularis*, Raffl.,—a structure which he afterwards described and figured in the 'Transactions' (vol. i. p. 65, pll. 9 and 10). Mr. Owen's impression that this remarkable modification of the stomach is a generic peculiarity, receives confirmation from its occurrence in the first previously unexamined species which has been dissected within the Society's reach since the publication of his remarks.

An extensive series of *Eulimæ*, chiefly from the collection of Mr. Cuming, was exhibited, and an account by Mr. G. B. Sowerby of the genus and of the characters of the several species was read, part of which we now extract from the 'Proceedings.'

Genus Eulima, *Risso*.

Testa turrita, acuminata, polita, anfractibus plurimis; aperturâ ovatâ, posticè acuminatâ; labio externo subincrassato, varices obsoletos frequentes, subsecundos, plerumque efformante: operculo corneo, tenui, nucleo antico.

* See Lond. and Edinb. Phil. Mag., May, vol. iii. p. 295.—Edit.

This genus of marine *Shells* appears to be most nearly related to *Pyramidella* and *Rissoa*. A species which has been long known has had the appellation of *Turbo politus* among British Linnean writers; and a fossil species has been placed by Lamarck among the *Bulini*, under the specific name of *Bul. terebellatus*. There are two distinctly marked divisions of the genus, which are characterized by the two species above mentioned; one has a solid *columella*, and the other is deeply umbilicated. All the species are remarkable for a brilliant polish externally, and the shells are frequently slightly and somewhat irregularly twisted, apparently in consequence of the very obsolete *varices* following each other in an irregular line, principally on one side, from the *apex* toward the aperture. Several recent species are British, and the fossil species are found in the *calcaire grossier* near Paris.

* Perforatæ: Eul. *splendidula*, *marmorata*, *interrupta*, *imbricata*, *brunnea*: * * Imperforatæ: Eul. *brevis*, *hastata*, *major*, *labiosa*, *Anglica* (Turbo politus, *Mont.*, *Test. Brit.* Conch. Illustr., f. 5.), *subangulata*, *pusilla*, *articulata*, *varians*, *lineata*, (several specimens of this were in G. Humphrey's collection, marked "Spira lineata, Weymouth, M.P.": these two last letters stand for *Musæi Portlandici*. I make no further remark, save that it appears to have been published by Da Costa under the name of *Turbo glaber*.—G. B. S.,) and *acuta*.

February 11.—Extracts were read from a letter addressed to the Secretary by B. H. Hodgson, Esq., Corr. Memb. Z.S., and dated Nepâl, July 13, 1833. It conveyed the thanks of the writer for the present to him on the part of the Society of an illustrative series of skins of *Birds*; and, referring to the mortality among the living *Birds* and *Quadrupeds* forwarded by him for the Society's Menagerie, it expressed a hope that a subsequent attempt would be more successful.

Portions were exhibited of the *viscera* of a *Capybara*, *Hydrochærus Capybara*, Erxl., taken from an individual which recently died in the Society's Menagerie. They consisted of the stomach, the enormous *cæcum*, and the *fauces*. In calling the attention of the Meeting to the latter parts, Mr. Owen availed himself of the opportunity to demonstrate the structure first observed in them by Mr. Morgan, by whom it has been described and figured in the lately published Part of the 'Linnean Transactions'. The constriction of the hinder part of the soft palate, which prevents any but minutely divided substances from passing into the *pharynx*, and which was first observed in the *Capybara*, is found in many other *Rodents*, but does not obtain in the whole of the animals of that order.

Various preparations were exhibited of the *Rhea*, *Rhea Americana*, Vieill., and of the *Cassowary*, *Casuarius Emeu*, Lath. They were brought under the notice of the Society by Mr. Martin, who, at the request of the Chairman, read his notes of the dissections of these birds. They agreed generally with the descriptions published by Sir Everard Home in the 'Philosophical Transactions.'

Mr. Martin also exhibited a preparation of aneurism of the *aorta*, obtained from a *brown Coati*, *Nasua fusca*, F. Cuv., sent to the Society for *post mortem* examination by J. H. Lance, Esq. He stated that this disease appeared to be rare among *Quadrupeds*, no previous instance of it having occurred to him among more than a hundred individuals of various orders which he had dissected within the last few years.

A preparation was exhibited of a young *common Macaque Monkey*, *Macacus cynomolgus*, La Cép., which was born at the Gardens on the morning of the 25th January, but was dead when first noticed by the keeper. It is the first instance that has occurred in the Society's Menagerie of the birth of any *Monkey* of the Old Continent.

The reading was concluded of a Paper entitled "A few Remarks tending to illustrate the Natural History of two Annulose Genera, namely *Urania* of Fabricius and *Mygale* of Walckenäer: by W. S. MacLeay, Esq."

Adverting in the first place to the doubts which prevail among entomologists as to the true situation in nature of the genus *Urania*, Mr. MacLeay proceeds to contribute towards the elucidation of the problem, the history of one species which appears to him to be possibly new. He characterizes it as

URANIA FERNANDINÆ. *Ur. alis nigris, anticis utrinque lineis transversis auro-viridibus suprà undecim, septimâ bifidâ, subtùs sex humeralibus latis, septimâ bifidâ, octavâ longissimâ trifidâ, reliquis apicalibus filiformibus; posticis suprà fasciâ haud serratâ et lineis octo brevibus lateralibus transversis auro-viridibus.*

Exp. alarum 4—4½ unc.

Hab. in Cubâ.

Mr. MacLeay describes in great detail the perfect insect, and points out, as far as printed descriptions and figures exhibit them, (he having at present no access to cabinets,) the marks which distinguish *Ur. Sloanus*, Godart, and *Ur. Boisduvalii*, Guér., from the Cuban species. He conceives, however, from the many variations that he discovers in it, that this insect may be merely a variety of *Ur. Sloanus*, to which species *Ur. Boisduvalii* may also possibly be referred as a small variety.

The coast of Cuba, in every open sandy part of it, is girt immediately above the coral reefs by a copse belt, close and nearly impenetrable, composed of almost one species of tree, the *sea-side Grape*, *Coccoloba uvifera*, Linn. At the base of this belt grow various *Euphorbiaceæ* and *Convolvuli*; and behind it the parched sand supports many sea-side shrubs, including *Palms*, *Cæsalpiniæ*, *Cacti*, &c., festooned with the flowers of *Convolvuli*, *Echites*, and other climbing plants: the leaves are studded with small terrestrial shells; and large sea-shells, brought from their original element by the singular *Paguri* which have usurped them, cluster round the short stunted trunks.

Among the shrubs of these sands the most interesting is *Omphalea triandra*, the *cob* or *hog-nut* of Jamaica, a Euphorbiaceous plant, but affording a most delicious and wholesome kernel: its upper leaves are large, heart-shaped, and thick, having a leathery texture and scabrous pale green surface; the young leaves and those of young plants have the same texture and colour, but differ remarkably in form, being deeply incised, with their divisions long and narrow, particularly the middle one, and all more or less dentated on the sides. On the upper side of the entire leaves of this shrub torpidly reposes during the day, under a transparent web which protects it from the powerful rays of the sun, a caterpillar, which at night becomes active and greedily strips the *Omphalea* of its foliage: this is the *larva* of *Ur. Fernandinæ*.

The egg of this insect may be found, throughout the whole of the spring, glued to the tender incised leaves of the *Omphalea*, scarcely ever more than two being attached to a single leaf: it has a pearly lustre and a pale green colour, sometimes turning to yellow; and varies in shape from an ovate to an oblate sphæroid. A circular space on its summit is smooth, and from hence proceed about twenty-four longitudinal ribs, the intervals between which are crossed by obsolete *striæ*.

The young *larva* is of the same colour with the egg, is marked by seven longitudinal black lines of hairs, and has a dirty yellowish head. When fully grown it is cylindrical, is without hinder protuberance on the penultimate segment, and has the more usual sixteen feet: it rarely rolls itself into a ring. Its head is sessile and red, with usually nearly twenty black spots, several of which seem to be tolerably constant; the mandibles are black. The *prothorax* is velvety black, with a white dorsal line and two or three white irregular spots at the sides; but the proportion of white varies, and there is sometimes a slight red spot on the back of the segment. The body varies from pale yellowish green to a flesh colour, with five paler longitudinal lines, of which the middle one is dorsal: the false feet are somewhat paler than the body; the true feet are red. The mesothoracic segment is rarely spotted, but all the others are often marked more or less with black spots. The spiracles are usually black. Each segment is furnished with about six hairs, which are white, and nearly one fifth as long as the whole body.

The *pupa* is not at all angular, but is rather gaily coloured; it is of a yellowish brown, with the *thorax* paler and the wings darker. The head is rounded and is marked, as well as the *mesothorax*, with several black spots; on the latter these are interspersed with points: the abdominal segments are each marked transversely with numerous black linear dots. The position of the *pupa* is horizontal, in an oval cocoon composed of a loose dirty-yellow silk, (with meshes so few and so lax as to allow the inmate to be readily seen,) and spun about withered or dead leaves.

The perfect insect is truly diurnal, swift in its flight, mounting high in the air, and travelling inland for two or three leagues, where it haunts gardens in great numbers. By far the greater number, however, remain on the sea-shore, sporting about the leaves of the *Coccoloba uvifera*, unless when depositing their eggs on the *Omphalea*. Its habit of frequenting the *Coccoloba* induced Mr. MacLeay to search long in vain for its *larva* on that tree. When it alights, all the four wings are expanded horizontally, and rarely, if ever, take a vertical position.

Mr. MacLeay concludes this portion of his paper by referring to Madame Merian's description of the metamorphosis of *Ur. Leilus*, and to her figure of its *larva*; both of which he regards as unworthy of credit. He then passes to her account of a *bird-catching Spider.*

The story of a *Spider* which catches and devours birds had, Mr. MacLeay believes, its origin with Madame Merian. Oviedo, Labat, and Rochefort make no mention of any *Spider* as possessing such habits, the two latter writers going no further than the statement that in the Bermudas there exists one which makes nets of so strong a construction as to entangle small birds. Madame Merian, however, went the length of asserting that one *Spider* not only caught, but devoured small birds; and figured the *Mygale avicularia*, Walcken., in the act of preying on a *Humming-bird.* Now the *Mygale* does not spin a net, but resides in tubes under ground, and in all its movements keeps close to the earth; while *Humming-birds* never perch except on branches. The food of *Mygale* consists of *Juli, Porcelliones*, subterranean *Achetæ*, and *Blattæ*: a living *Humming-bird* and a small *Anolis*, placed in one of its tubes, were not only not eaten by the *Spider*, but the latter actually quitted its hole, which it left in possession of the intruders. The largest *Spider* of the West Indies that spins a geometrical web is the *Nephila clavipes*, Leach; and its net may perhaps, occasionally, be strong enough to arrest the smaller among the *Humming-birds*: but it is not likely that the *Spider* would eat the birds. A small species of *Sphæriodactylus*, Cuv., introduced into one of these nets, was enveloped in the usual manner by the *Spider*; but as soon as the operation was completed, the *Spider* lost no time in cutting the line and allowing her prisoner to fall to the ground. Mr. MacLeay consequently disbelieves the existence of any *bird-catching Spider.*

The Paper was accompanied throughout by numerous notes, including observations on many subjects adverted to by the author; such as the habits of the *land-Crabs* of Cuba; a description of the *grey Lizard* of the coast, apparently a species of *Agama*; &c. They also included an account of two species of *Sphæriodactylus*, Cuv., which are characterized as follows:

SPHÆRIODACTYLUS CINEREUS. *Sphær. caudâ corporis longitudine; totus cinereus, translucidus, capite flaviori, apice roseo; squamis dorsalibus punctis minutissimis nigris aspersis.*

Long. tot. $2\frac{3}{4}$ unc.

This may possibly be the *small house Lizard* of Browne's Jamaica.

SPHÆRIODACTYLUS ELEGANS. *Sphær. fasciis dorsalibus transversis nigris* 14; *capite cæruleo-cinereo, subtùs nigro-fasciato; dorso subviridi; caudâ rubrâ, corpore breviore; ventre cinereo.*

Long. tot. $1\frac{1}{2}$ unc.

Both these *Lizards* are very common in houses in Cuba, occurring among books or wherever they can find shelter. They have bright eyes, are pretty and very harmless, and come out of their corners in rainy weather, declaring war against everything in the shape of a fly or musquitoe.

The Paper was also accompanied by drawings of the egg, *larva*, and *pupa* of *Urania Fernandinæ*, which were exhibited.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

A meeting of the Cambridge Philosophical Society was held on Monday evening, April the 14th, Professor Airy, V.P., in the chair.

Various presents were received, among which were the memoirs and other publications of the Royal Academy of Sciences at Brussels. Professor Airy read a communication containing an account of his determination of the latitude of the Observatory of Cambridge, by means of observations with the mural circle. The latitude thus determined appears to be $52^\circ\ 12'\ 51''{\cdot}72$, which Professor Airy considers to be accurate within a small fraction of a second.

Mr Whewell made some remarks on the subject of Sir John Herschel's hypothesis respecting the absorption of coloured media, proposed in the Philosophical Magazine for December 1833. The object of these remarks was to show that the theory might be simplified; and it was further added, as suggested by Mr. D. Heath, that the same hypothesis would lead to an explanation of dispersion by refraction on the undulatory theory. These statements led to communications and remarks from several other members.

A meeting was held on Monday evening, April 28th, Dr. Clark, V.P., being in the chair.

A paper by Professor Miller was read on the subject of a supposed relation between the axes of optical elasticity of oblique prismatic crystals, and the axes of their crystalline forms. Professor Neumann of Köningsberg, had asserted, on the strength of observations made on certain species of crystals, that the crystalline forms might be referred to the optical axes in a manner consistent with the simplicity of crystalline relations; but in the instance of several other species examined by Professor Miller, it appeared that this law could not be maintained.

A paper by Mr. Earnshaw, of St. John's College, was also read, on the laws of motion. Mr. E. is of opinion that the three laws of motion are not proved by experience, but by means of the axiom, that similar effects are due to similar causes. Having established, by help of this principle, the laws which connect motion and force, we learn from experience in what cases force exists.

Mr. Willis exhibited and explained the construction and working of a machine which he had invented for the purpose of jointing together the bones of skeletons, the object being to connect the bones so that they may exhibit, in some degree, their natural motions. Mr. Willis's machine holds the bones firmly, however irregular their form; saws notches in their extremities, so that they may be jointed by means of a metal plate; and drills the holes by which the plate is fastened.

A meeting was held on Monday evening, May the 12th, Dr. F. Thackeray, the Treasurer, being in the chair.

A paper by A. De Morgan, Esq., of Trinity College, was read, containing observations upon the principles which have usually been referred to in treating of series and of the fundamental doctrines of the differential calculus, several of which principles the author conceives have been assumed without due proof; and examples were given in which such principles are false.

Professor Miller exhibited and explained the instrument invented by M. Say, for the purpose of taking specific gravities, with some improvements of his own.

Mr. Willis exhibited and explained an instrument constructed by him, which produces correct representations of the orthographic projections of irregular objects, as, for instance, of bones: this he proposes to call an *Orthograph.*

Mr. W. W. Fisher gave a statement of his views concerning the origin of tubercular diseases; such diseases, he conceives, arise from a deficiency of nutritive energy in the osseous system, and from the modifications introduced by this deficiency into the character of other vital processes in the animal œconomy.

LXX. *Intelligence and Miscellaneous Articles.*

FURTHER REMARKS ON CHEMICAL SYMBOLS IN REPLY TO MR. PHILLIPS. BY MR. JOHN PRIDEAUX.

In your Number for this month (April, p. 246,) is a letter from Mr. Phillips to Mr. Graham, a part of which is employed to show that my remarks on his former censure of chemical symbols "are inaccurate both with regard to the facts and fancies of symbolizing." (p. 248.) A few words may be requisite to make it appear how far this has been done with respect to the facts; the fancies may shift for themselves.

On the symbols for phosphoric acid and water he does not offer any thing new, except that (p. 250, at foot,) he cannot discover the advantage of Aq over $\underline{\dot{H}}$: the ambiguity of the latter symbol having been, hitherto, his principal objection.

My objection to "the specimen" was not one of expression, with which, having said so much on it before (Lond. and Edinb. Phil. Mag. vol. x. p. 104), I should not have meddled, but of atomic constitution, the proportions of oxygen in the different formulæ being quite at variance. *Such* mistakes were to be ascribed to misprint or other inadvertence, and not to the symbols; certainly not to Mr. Phillips, particularly after his informing us (Lond. and Edinb. Phil. Mag. vol. iii. p. 445), "the statements are taken from all the systems I have been able to collect;" and further, that he had examined "ten systems of notation, in order to discover the meaning of $\dot{N}a^2\,\dot{H}\,\dddot{P}$.

It seems I have offended in taking up statements which he neither made nor intended (p. 249); that his intention was to describe an "imaginary compound, as to the constitution of which all should agree." Yet the symbols were certainly exhibited as "specimens of confusion :" although it *now* appears they also were imaginary ; and we are plainly told (p. 250) that some of the authors quoted would have written them differently.

His reply to my statement that 'Rose and Johnstone would doubtless place 2 before the symbol for soda,' is, " Very likely ; but then they would have written $\underline{\overset{:\cdot:}{P}}$, while I have given $\overset{:\cdot:}{P}$ to express an acid 'in which many chemists, chiefly in this country, apprehend the phosphorus to enter as a single atom ;' and therefore only an atom of soda to form phosphate of soda."

But if the phosphorus enters into $\overset{:\cdot:}{P}$ only as a single atom, the oxygen enters as 5 atoms (.·.·.) ; whilst the equivalent of phosphoric acid for one atom of soda contains but $2\frac{1}{2}$ atoms of oxygen, as Mr. Phillips well knows. A quantity, therefore, of phosphoric acid, containing 5 atoms of oxygen, will require 2 atoms of soda whether the symbol be written $\overset{:\cdot:}{P}$, or $\underline{\overset{:\cdot:}{P}}$.

The "2 before soda" having, however, been omitted * below, in the symbol given as Johnstone's, we are told (p. 250), " It is curious that Johnstone's symbol is the same when mutilated by me, as when corrected by Mr. Prideaux ; and still more curious, that with my view it was correctly employed, while with the *no*-mending of Mr. Prideaux it is wrong." And most curious of all, that it changed from right to wrong, yet remaining the same as before.

It is next stated that I, "a professed admirer," &c., "in attempting to correct three symbols, have committed four errors." Two of these relate to the $\underline{\overset{:\cdot:}{P}}$ in Rose and Johnstone's symbols, which Mr. Phillips now says they would write,

$$\text{Rose} \ldots\ldots\ldots \; 2\,\mathrm{Na\,O} + \mathrm{P\,O}^5 + 24\,\underline{\mathrm{H}}\,\mathrm{O}\,;$$

$$\text{Johnstone} \ldots\ldots \; \underline{\overset{:\cdot:}{P}} + 2\,\dot{\mathrm{S}}\mathrm{o} + 24\,\dot{\mathrm{H}}.$$

Not possessing any table by either of these chemists, it was from "the specimen" I was led to conclude that they held phosphoric acid to consist of 1 atom phosphorus and 5 oxygen ; and my authority seems now to have been imaginary. But it is on their opinions of the atomic constitution of the acid, not of the quantity of soda with which it combines, that will depend their mode of expressing that ingredient. Graham writes $\dot{\mathrm{Na}}^2\,\overset{:\cdot:}{P}$ (not $\underline{\overset{:\cdot:}{P}}$), and for aught that yet appears, Johnstone and Rose may do the same.

The other two errors charged to my account appear to be in Dr. Turner's formula which Mr. Phillips will *maintain* thus: $\dot{\mathrm{S}} + \overset{:\cdot:}{P} + 2\frac{1}{2}\,\mathrm{O} + 12\frac{1}{2}\,\mathrm{Aq}$; a promise for which every professed admirer of chemical symbols must thank him, as the London Professor will certainly disown it, and it will hardly find a maintenance elsewhere. It is true, there can be no question of authorship in the case ; and it will furnish a standing proof, that "the privilege of

* I supposed, at the time, by the printer ; but whether by him or myself, I thought the correction too obvious to require notice.

changing symbols is not confined to me and Berzelius" (*ego et rex meus!*). It gives us ($\therefore$ and $2\frac{1}{2}$ O) $= 7\frac{1}{2}$ atoms of oxygen, in the equivalent of phosphoric acid, for ($\dot{\mathrm{S}}$) an atom of soda. As to the water, it is most probable, that since Clarke's experiments, Turner would add $\frac{1}{2}$ an atom to the 12; and Johnstone and Rose an atom to the 24. Mr. Phillips's specimens, and my comments upon them, related to the compostion as understood in Berzelius's symbol $\mathrm{Na}^2 \overset{\therefore\cdot}{\mathrm{P}} + 24 \dot{\mathrm{H}}$; but he now maintains that Rose and Johnstone are still to write 24 water, while Turner has $12\frac{1}{2}$.

Of my scale it needs only be observed, that the variations were intended for it *alone*, for reasons thereon given, with their explanation. Had the symbols been generally understood in this country, at the time of its publication, no such variations would have been made, or needed.

The symbol given as Warrington's was (vol. iii. 445) $\overset{\circ\circ}{\mathrm{Po}} + \overset{\circ}{\mathrm{So}} + 24\ \mathrm{H}^{\circ}$; in which Mr. Phillips has now been enabled to discover, "by a microscopic examination of Mr. W.'s new symbols," "that the O following the P is rather larger than the OO *over** which it is placed, whereas they ought to have been the same size;" and seems to think it hard that an O may not be misplaced and magnified *ad libitum*, without changing phosphate of soda into quadroxide of potassium and soda. When he resumes his microscopical investigations, I suspect he will find Warrington's new symbol for crystallized phosphate of soda† something like this, $\overset{\circ\circ}{\mathrm{P}} + 2\ \overset{\circ}{\mathrm{So}} + 25\ \overset{\circ}{\mathrm{H}}$; differing in one or two other particulars from the specimen, besides the magnitude and position of the O; which, however, placed in the same relation to the P, Po; as in the following symbol to the S, So, sodium; was quite enough to change its meaning from one substance to another having the same initial, *i. e.* from phosphorus to potassium. Warrington has most probably seen, before now, the additions made by Berzelius (*Traité de Chimie*, iv. 609.) to express the sulpho-salts, &c.; and will perhaps agree that they have rendered his new system unnecessary.

I have never represented the symbols of Berzelius as "absolutely perfect," but the contrary (see Phil. Mag., &c.); yet holding and finding them to be concise, expressive and readily acquirable, I am desirous of their general adoption, as a means of clearness and precision in our chemical notions, and of facility in their communication.

* The microscope seems to have had an inverting power.

† [I ought, perhaps, to notice some incorrect statements contained in the above communication; but though not at all "offended" as Mr. Prideaux supposes, I am somewhat weary of the subject, and shall content myself with confessing my own blunders, and leaving those of Mr. Prideaux. I blundered in putting $\therefore$ over P in Turner's formula, and give the science of symbols the full benefit of the admission. I blundered in saying *over* instead of *under*, but "the correction is too obvious to require notice."—R. P.]

ON THE ACTION OF CHLORINE ON METALLIC IODIDES. BY A. T. THOMSON, M.D., PROFESSOR OF MATERIA MEDICA IN THE UNIVERSITY OF LONDON.

To Mr. Richard Phillips.

Dear Sir, 5th May, 1834.

It has been long known that liquid chlorine, that is, a solution of chlorine in water, added to solutions of metallic iodides, or, as they then become, hydriodates, sets free the iodine, and thus enables it to be detected in minute quantity on the addition of starch. But it is also well known, that a very small excess of the solution of chlorine destroys the colour of the iodide of amidine, and renders the test fallacious. To remedy this disadvantage I have substituted chlorine gas for the liquid chlorine, and find that it is capable of discovering the minutest portion of any hydriodate in solution, even in mixed fluids. The method of testing is to mix a small quantity of solution of starch in the fluid to be tested, and pouring on the surface of the liquid some chlorine gas; as soon as the gas reaches the surface a thin film of blue appears, and gradually pervades the whole of the liquid, if any hydriodate be present. The advantage of the test is the impossibility of adding too much, as the action commences on the surface, and the superabundant chlorine, which is mixed with the common air in the upper part of the test tube or the glass, is soon dissipated.

As a proof of the delicacy of the test, I may add, that four minims of a solution of hydriodate of potassa, containing one drachm in the fluid ounce, were added to a fluid ounce of water, and tested by the method above described, when the presence of free iodine was immediately rendered evident. The proportion of the hydriodate in this case being only $\frac{1}{960}$th, I conceive that the test is adequate for any experiment in which it may be required to ascertain the presence of a hydriodate in solution.

The theory of the process is too obvious to require any comment. Your making this test generally known through the medium of the Philosophical Magazine will greatly oblige,

Yours faithfully,

A. T. Thomson.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

We have great pleasure in announcing the speedy publication of the Report of the British Association for the Advancement of Science for 1833, containing the proceedings of the meeting at Cambridge. Its contents, we believe, will be not less interesting than those of the Second Report for 1832, and will evince the steady progress of the Association in the fulfillment of the objects for which it was established.

Meteorological Observations made by Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London; by Mr.* R. HOCKING *at Penzance, and Mr.* VEALL *at Boston.*

Days of Month. 1834.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Penzance. Max.	Barometer. Penzance. Min.	Barometer. Boston $8\frac{1}{2}$ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Penzance. Max.	Thermometer. Penzance. Min.	Thermometer. Bost. $8\frac{1}{2}$ A.M.	Wind. Lond.	Wind. Penz.	Wind. Bost.	Rain. Lond.	Rain. Penz.	Rain. Bost.
☾ April 1	30·288	30·140			29·66	54	40			42·5	N.		NW.	0·04		...
2	30·305	30·274			29·73	55	49			47	SW.		calm	·07		...
3	30·515	30·302			29·69	57	30			50	N.		calm	...		0·03
4	30·471	30·394			29·98	58	45			46	N.		calm	...		...
5	30·377	30·307			29·87	60	41			48·5	NW.		calm	...		...
6	30·392	30·382			29·86	58	30			53	E.		calm	...		...
7	30·364	30·315			29·73	63	39			50	N.		calm	...		...
8	30·393	30·376			29·85	50	38			47	E.		calm	...		...
● 9	30·383	30·280			29·90	42	28			43	NE.		calm	...		...
10	30·331	30·272			29·90	50	30			45·5	NE.		calm	...		...
11	30·232	30·128			29·82	49	33			42	N.		NW.	...		...
12	30·181	30·031			29·60	50	32			41·5	NE.		E.	·11		·11
13	30·333	30·259			29·81	51	26			46	NE.		calm	...		·04
14	30·421	30·393			30·03	56	27			48	SE.		calm	...		...
15	30·436	30·405			29·94	61	35			49	SE.		calm	...		...
☽ 16	30·385	30·322			29·95	60	34			47	SE.		calm	...		...
17	30·288	30·208			29·87	60	39			50	SE.		E.	...		...
18	30·226	30·192			29·82	62	33			51	SE.		E.	...		...
19	30·336	30·213			29·90	66	38			53·5	NE.		E.	...		...
20	30·312	30·284			29·90	66	34			49	NE.		calm	...		...
21	30·341	30·311			29·84	58	32			49	NE.		calm	..		...
22	30·322	30·233			29·75	59	34			52	NE.		calm	...		...
○ 23	30·338	30·237			29·66	56	40			49	NE.		calm	...		...
24	30·343	30·262			29·79	53	27			47·5	NE.		calm	...		...
25	30·185	30·129			29·64	57	35			50	SE.		calm	...		...
26	30·093	29·912			29·60	57	32			52	SE.		calm	...		...
27	29·733	29·318			29·24	69	53			53	SE.		SE.	·07		...
28	29·374	29·305			28·75	68	46			58	SW.		W.	·10		·06
29	29·439	29·400			28·97	62	51			49	S.		calm	·15		·29
☾ 30	29·642	29·400			28·93	59	48			53	SW.		calm	·11		·11
	30·515	29·305			29·69	69	26			48·7				0·65		0·64

Remarks.

London.—April 1. Fine. 2. Rain: frosty at night. 3. Fine: frosty at night. 4—7. Fine. 8. Cold dry haze. 9. Bleak and cold: frosty at night. 10. Clear and cold, with the atmosphere exceedingly dry: frosty at night. 11. Cold and dry. 12. Clear: heavy shower of hail in the afternoon. 13. Cloudy and cold: frosty at night. 14. Frosty haze: fine: frosty at night. 15. Frosty: fine. 16, 17. Hazy: fine. 18, 19. Very fine. 20—22. Overcast: fine. 23. Cloudy. 24. Fine: sharp frost at night. 25. Overcast: dry haze. 26. Fine. 27. Very fine: cloudy: rain. 28. Cloudy: fine: rain at night. 29, 30. Rain.—The general character of this month has been cold and dry, with easterly winds, and frequently sharp frosts, which proved very injurious to vegetation, and more especially so in consequence of the latter having been previously in a very forward state.

Boston. — April 1. Fine. 2. Rain. 3, 4. Cloudy. 5—7. Fine. 8, 9. Cloudy. 10. Fine: hail-storm P.M. 11. Cloudy. 12. Fine: rain early A.M.: rain and hail P.M. 13, 14. Cloudy. 15—19. Fine. 20, 21. Cloudy. 22. Fine. 23—26. Cloudy. 27. Fine. 28. Cloudy: rain early A.M.: rain P.M. 29. Cloudy: rain P.M. 30. Cloudy: rain early A.M.

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