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"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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June 1st, 1836.

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THE

LONDON AND EDINBURGH

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

JANUARY 1836.

I. *On the Retina and Pigment of the Eye of the Common Calamary* (Sepia Loligo). *By* THOMAS WHARTON JONES, *Esq.**

IN the eye of the Cuttlefish tribe, the retina is said to be situated behind a thick layer of dark pigment. As such a structure would have the effect of intercepting more or less completely the rays of light, the strangeness of it has particularly attracted the attention of the physiologist and optician. The eye in the Cephalopodous Mollusca presents several anomalies when compared with that of the Vertebrata, but I am inclined to think that the structure just mentioned is not one of them. This is at least the case in the eye of the *Sepia Loligo*, which I have carefully examined; and what is found in one species probably also exists in the other genera and species of the order.

My dissections and microscopical examinations of the eye of the *Sepia Loligo* show, that what has hitherto been described as pigment is in reality not so, but a nervous expansion of a peculiar texture, tinged of a reddish brown colour—a circumstance which has given rise to the error of supposing it merely pigment. Before describing the structure of the retina, it will not be out of place first to notice the optic nervous apparatus, which differs remarkably from what is observed in the Vertebrata. In connexion with the cerebral ganglion, on each side, is a large nervous mass, or ganglion, from which fibrils proceed in a peculiar manner to the eyeball. On either side of the nervous mass, or optic ganglion as it may be called, the fibrils

* Communicated by the Author.

arise, and immediately after their origin intercross with each other, those from the one side going to the opposite side of the eye, and contrariwise. The nervous fibrils from the fore end of the ganglion do not intercross, but proceed directly to the eye. The fibrils which thus arise from the optic ganglion are in very great number. They cover to a considerable extent the posterior surface of the eyeball, and each penetrates singly the thin cartilaginous lamina which corresponds to a sclerotica. The optic fibrils having thus entered the eyeball expand into a layer of a light reddish-brown tinge, which I shall distinguish by the name of the first layer of the retina. What I call the second layer of the retina, is the reddish brown membrane, which, I have already mentioned, is the part usually considered as pigment. It is situated within the first layer; and betwixt the two there intervenes a pretty thick and dark layer of pigment, through apertures in which the nervous substance passes from the first layer of the retina to form the second. Examined with the microscope, the second layer of the retina, which, as I have said, is of a reddish brown colour, is observed to be composed of short fibres perpendicular to its surfaces. These fibres, towards the inner surface, end in a delicate pulpy nervous substance, also tinged of a reddish brown colour, particularly on its inner surface, which has a corrugated or papillary appearance.

I think it unnecessary to notice further the structure of the eye of the Calamary, but shall content myself with referring to the annexed figures (Plate I.) and their explanation.

Since writing the above I have examined the eye of an *Octopus*, and have found the retina and pigment to possess the same structure as in this Calamary.

Explanation of the Plate.

Figure 1. Represents the brain and two eyes of the *Sepia Loligo*. On the right side are seen the optic ganglion, and the fibrils which arise from it expanding themselves on the back of the eye previously to their penetrating the sclerotica. On the left side is a horizontal section of the eye and optic ganglion.

a. Cerebral ganglion. *b*. Subœsophageal ganglion. *c*. A black probe introduced into the nervous collar, through which the œsophagus passes. *d*. Optic ganglion of the right side: *e*, *e*, *e*. The nervous fibrils which arise from it and enter the eye to form the retina. *f*. Optic ganglion of the left side cut horizontally. *g*, *g*. Optic fibrils arising from the ganglion and penetrating the sclerotica at different points. *h*, *h*, *h*, *h*. The

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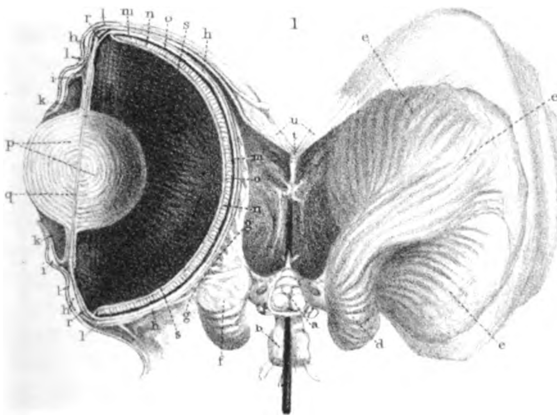

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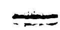


EYE OF THE SEPIA LOLIGO.

sclerotica, which is cartilaginous. *i, i.* A cartilaginous part, which may be compared in some respects to the cornea of the Vertebrata. It has a large opening in its centre, through which the lens projects. *k, k.* A thin membrane which lines the inner surface of the preceding part, and extends a little beyond the edge of the opening in it. This membrane, which is covered on its inner surface with a dark pigment, may be looked on as a kind of iris. It is reflected on the anterior surface of the structure which supports the lens, and is continuous with the outer lamina of the anterior segment of the latter. *l, l, l, l.* A silvery-like membrane, which may be called the conjunctiva. *m, m.* First layer of the retina. Opposite that part where the optic fibrils cover the back part of the eye, this layer of the retina is joined by the fibrils immediately on their entrance, but further forward the fibrils run a little way within the sclerotica before joining the retina, which produces the appearance of another layer. *n, n.* Second layer of the retina. *o, o.* Pigment situated betwixt the two layers of the retina. *p.* The lens. It is a sphere divided into two unequal segments, an anterior smaller, and a posterior larger. Betwixt the two segments is interposed a thin transparent membrane *q*, which is continued from the first layer of the retina, and is joined by a thin membrane *r*, which arises from the sclerotica. The lens, as in the Vertebrata, is composed of laminæ and fibres. The outer laminæ of the lens are continued into a plicated structure situated around its circumference, on either surface of the membrane which is interposed betwixt the segments of the lens. *s, s.* A transparent membrane, which may be called the hyaloid. It does not, however, completely inclose the aqueous fluid which represents the vitreous humour. *t.* A cartilaginous pulley, through which play the subdivisions of a tendon *u*, common to a membraneous muscle surrounding either eyeball.

Figure 2. Shows the intercrossing of the optic fibrils after their origin from the optic ganglion.

Figure 3. A section of the layers of the retina magnified. *a.* The optic fibrils joining *b.* the first layer of the retina. *c.* The pigment interposed between the first and second layers. *d.* Second layer of the retina, composed of short fibres perpendicular to its surfaces. *e.* A pulpy nervous substance in which the fibres end on the inner surface.

II. *Notice on the Fossil Beaks of four extinct Species of Fishes, referrible to the Genus* Chimæra, *that occur in the Oolitic and Cretaceous Formations of England. By the Rev.* Wm. Buckland, *D.D. F.G.S., Professor of Geology and Mineralogy in the University of Oxford.**

ABOUT six years ago, Sir Philip Grey Egerton procured from the Kimmeridge clay of Shotover Hill, near Oxford, five remarkable fossil bodies of most curious configuration, in some degree resembling beaks of Cuttlefishes and Turtles, but not reducible to any known form in either of these families.

In 1832, the Rev. C. Townsend of Great Milton, near Oxford, discovered in the Portland stone of that village another series of bones, resembling those from the Kimmeridge clay, but very much larger, and of a different species.

On my submitting these specimens to Mr. Mantell, he immediately compared them with three similar bones in his collection,—one from the Chalk marl of Hamsey, and two from the Chalk near Lewes. These were obviously the same parts of two other species of animals of the same genus. That from the Chalk marl had been shown by him to Cuvier, who could only recognise in it a distant resemblance to the articulating posterior portion of a jaw of a Saurian; but this resemblance was not maintained in the more perfect fragments of other species which had come into my possession from the Kimmeridge and Portland beds.

Mr. Mantell permitted me at this time to prepare a drawing of the fragment from the Chalk marl which he had submitted to Cuvier.

After searching in vain through the best collections in London, and consulting our best comparative anatomists, I could find no animal whose beak or jaws corresponded with either of the forms of fossil bones under consideration.

During the last five years I have lost no opportunity of submitting these fossils to skilful comparative anatomists, and with the same result. My exhibition of several of them to some of the most distinguished anatomists of Germany, at the meeting of the Naturforscher at Bonn in September last, threw no further light upon the subject. The nearest approximation that was suggested to me came from Professor Carus, who advised me to compare the two smallest of these fossils (evidently a pair) with the beak of a *Tetrodon*.

In pursuance of this advice, I examined all the Tetrodons in every museum I visited after my departure from Bonn, and arrived at no other conclusion than the assurance that

* Communicated by the Author. This paper was read before the Geological Society on the 4th of November, 1835.

not one of these supposed fossil beaks could be referred to that genus.

In examining the rich collection in the museum at Leyden, a few days ago, with my friend Professor Van Breda, I found by the side of a *Tetrodon* a skeleton of that rare fish the *Chimæra monstrosa*, of which I had never before seen the bones, and instantly recognised in the upper and lower jaws of this animal the object of my long research. The two intermaxillary bones of the upper jaw corresponded with the pair of tooth-like bones from the Kimmeridge clay, which I had in vain compared with the teeth of the *Tetrodon*; the superior maxillary bones corresponded with a second pair of the fossil bones from the same clay; and the lower maxillary of the *Chimæra* presented the form of the fossil inferior maxillary bones of my four different species from the Portland stone, Kimmeridge clay, Chalk marl, and Chalk.

This discovery of the type of each of these new forms of fossil bones in the mouth of a living species of *Chimæra*, at once clears up all the difficulties of which I have so long been seeking the solution, and enables me to announce the existence of four fossil species of a genus hitherto unheard of in the annals of Palæontology; one in each of the following four different formations, namely, the Portland stone, Kimmeridge clay, Chalk marl, and Chalk. To that discovered in the Portland stone, I propose to give the name of *Chimæra Townsendii*; to that in the Kimmeridge clay, *Chimæra Egertonii*; to that in the chalk marl, *Chimæra Agassizii*; and to that in the chalk, *Chimæra Mantellii*.

On my submitting these fossils to Professor Agassiz, he at once admitted them to belong to the genus *Chimæra*, a genus of which the living individuals are extremely rare, and of which he knows not where a single prepared skeleton exists, except in the museum at Leyden.

The only known living species of the genus *Chimæra* is widely diffused, and is usually found pursuing herrings and migratory fishes: it lives chiefly in the northern seas, and occurs also in the Mediterranean. It is most nearly allied to the family of Sharks, and is from two to three feet long. The cartilaginous nature of its skeleton explains the reason why no other bones of the fossil *Chimæra* have been found, together with those that form their very peculiar jaws. The hard horny plates which cover these jawbones in the living species, and perform the office of teeth, are in none of our fossil specimens preserved. The two intermaxillary bones of the upper jaw of the *Chimæra Egertonii* have nearly the hardness of enamel, and appear to have had no separable horny covering: the superior and inferior maxillary bones of the same species exhibit rugous surfaces of attachment, from which their horny coverings have been removed. The same marks of attachment are seen in the lower jaw-bones of the *Chimæra Agassizii* and *Chimæra Mantellii*. The horny investment of all these bones has evidently fallen off and perished, like the horny covering which separates readily from the bony beak of Turtles, and which is rarely, if ever, found with the bones of fossil *Testudinata*.

The genus *Chimæra* is one of the most remarkable among living fishes, as a link in the family of *Chondroptérygiens*. The fact of the existence of many fossil species of this curious genus (and some of these much larger than the single known existing species) in such early periods as those of the Oolitic and Cretaceous formations leads to important considerations in Physiology.

Professor Agassiz has at my request prepared the following description of the four fossil species which form the subject of this communication. Further details and figures will be published by him in the eighth number of his *Poissons Fossiles*.

Note by Professor Agassiz.

The discovery of the genus *Chimæra* among fossil fishes is one of the most interesting and unexpected.

Recent *Chimæras* are very little known, and have been arranged in the order of cartilaginous fishes, but their organization, and especially the structure of their skeleton, has not been sufficiently studied. Dr. Buckland's discovery will draw the attention of Ichthyologists in a particular manner to this singular family. The four fossil species about to be enumerated differ essentially from each other, and are considerably larger than the living. Unfortunately the fossil fragments which we now possess are far from being complete; only the jaws of these curious fishes have hitherto been discovered, and principally the lower jaws.

In the Portland species, the *Chimæra Townsendii*, which is the largest, the inferior maxillary is very large, short, and proportionally much thicker, the groove of the symphysis of its two branches shallower, and the cavity of the dental edge broader than in the other species; its exterior surface is convex and furrowed longitudinally with shallow wrinkles. The intermaxillary bone is much bent.

In the *Chimæra Egertonii* the inferior maxillary is short and flat; its snout is truncated, and in proportion very large; the cavity of the dental edge is very wide and the groove of its symphysis very deep; the intermaxillary is much bent, and the dental edge truncated and square; the superior maxillary is irregularly triangular, much elongated, and contracts insensibly towards its dental extremity, which is bifid.

In the *Chimæra Agassizii* of Dr. Buckland the inferior maxillary is the most regular in form of the four species; it is nearly square, and has the dental edge slightly open; the surface of the symphysis is flatter than in the other species.

The *Chimæra Mantellii* has the inferior jaw straighter and thinner: its exterior surface is perfectly smooth and flat; its snout is much elongated and pointed, and the cavity of the dental edge wider.

Since Dr. Buckland's discovery of the above four species, I have found a fifth in the collection of Mr. Greenough, which differs considerably from them all, in the extreme shortness of the lower jaw, the length of which is less than its height. The symphysis of the lower jaw is flat; the dental margin truncated and grooved in its hinder part. The external surface is smooth; the middle of the inner surface concave; the intermaxillary is flatter than in the *Chimæra Egertonii*, and terminates in a straight point. The superior maxillary is shorter than that of the *Chimæra Egertonii.*

I propose to give to this species of so remarkable a genus the name of *Chimæra Greenovii.* The locality of this fossil is unknown.

Oxford, Oct. 27, 1835.

III. *On the Relation between the Velocity and Length of a Wave, in the Undulatory Theory of Light. By* John Tovey, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the last volume but one of your Magazine, the Rev. Professor Powell presented us with an abstract of the essential principles of M. Cauchy's View of the Undulatory Theory of Light; by which, as Mr. Powell says, it appears "that a relation between the velocity and length of a wave is established on M. Cauchy's principles, provided the molecules are so disposed that the intervals between them always bear a sensible ratio to the length of an undulation." vol. vi. p. 266.

Since I first read this, I have arrived at the same result as M. Cauchy by a less complicated method, which I proceed to lay before you. I do this with diffidence, having read scarcely anything on the subject besides the abstract above mentioned and Professor Airy's tract. Should you deem my method worthy of a place in your Journal, I shall probably send you a continuation of the subject.

Let m, m', m'', &c. be the masses of the particles of æther; let the rectangular coordinates of m be x, y, z; those of m', $x+\Delta x, y+\Delta y, z+\Delta z$; of m'', $x+\Delta x', y+\Delta y', z+\Delta z'$, &c. Let $r = \sqrt{(\Delta x^2+\Delta y^2+z^2)}$, $r' = \sqrt{(\Delta x'^2+\Delta y'^2+\Delta z'^2)}$, &c. Suppose the masses to be all equal, and the force of one particle on another to be a function of their distance multiplied by m; and suppose each particle to be influenced only by the attractions or repulsions of the other particles; then as the cosines of the angles which r makes with the positive directions of x, y, z are $\frac{\Delta x}{r}$, $\frac{\Delta y}{r}$, $\frac{\Delta z}{r}$, we have (by the principles of statics), when the system is in equilibrium,

$$m\Sigma . \frac{f(r)}{r} \Delta x = 0, \; m\Sigma . \frac{f(r)}{r} \Delta y = 0,$$

$$m\Sigma . \frac{f(r)}{r} \Delta z = 0. \qquad (1.)$$

The sums Σ extending to all the particles within the sphere of the attractive or repulsive influence of the particle m, which may be any particle of the system.

Now, suppose the system to be disturbed, and that at the end of the time t, the displacements of m, in the directions of x, y, z, be ξ, η, ζ; and those of m', $\xi+\Delta\xi, \eta+\Delta\eta, \zeta+\Delta\zeta$; and suppose $\Delta\xi, \Delta\eta, \Delta\zeta$ to be so small that we may neglect their squares and rectangles; then the distance of these particles being $r+\Delta r = \sqrt{[(\Delta x+\Delta\xi)^2+(\Delta y+\Delta\eta)^2+(\Delta z+\Delta\zeta)^2]}$, we have

$$\Delta r = \frac{\Delta x \Delta \zeta + \Delta y \Delta \eta + \Delta z \Delta \zeta}{r}.$$

The cosines of the directions which $r+\Delta r$ makes with those of x, y, z will be $\frac{\Delta x+\Delta\xi}{r+\Delta r}$, $\frac{\Delta y+\Delta\eta}{r+\Delta r}$, $\frac{\Delta z+\Delta\zeta}{r+\Delta r}$; and if we write X, Y, Z for the sums of the components of the forces acting on m, in the directions of x, y, z, we have

$$X = m\Sigma . \frac{f(r+\Delta r)}{r+\Delta r}(\Delta x+\Delta\xi),$$

$$Y = m\Sigma . \frac{f(r+\Delta r)}{r+\Delta r}(\Delta y+\Delta\eta), \qquad (2.)$$

$$Z = mZ . \frac{f(r+\Delta r)}{r+\Delta r}(\Delta z+\Delta\zeta).$$

Now, $f(r+\Delta r) = f(r) + \frac{d f(r)}{d r}\Delta r$, $\frac{1}{r+\Delta r} = \frac{1}{r} - \frac{\Delta r}{r^2}$;

consequently,

$$\frac{f(r+\Delta r)}{r+\Delta r}(\Delta x + \Delta \xi) = \left\{\frac{f(r)}{r} + \left(\frac{d f(r)}{r\, d r} - \frac{f(r)}{r^2}\right)\Delta r\right\} . (\Delta x + \Delta \xi)$$

$$= \frac{f(r)}{r}\Delta x + \frac{f(r)}{r}\Delta \xi + \left(\frac{d f(r)}{r^2 d r} - \frac{f(r)}{r^3}\right) . (\Delta x \Delta \xi + \Delta y \Delta \eta + \Delta z \Delta \zeta)\Delta x,$$

by substituting for Δr its value previously found.

If in this expression we write $\phi(r)$ for $\frac{f(r)}{r}$, $\psi(r)$ for $\left(\frac{d f(r)}{r^2 d r} - \frac{f(r)}{r^3}\right)$, and substitute it in the first of the equations (2.), we shall have by virtue of the first of the equations (1.),

$$X = m\,\Sigma . \{\phi(r)\Delta \xi + \psi(r) . (\Delta x \Delta \xi + \Delta y \Delta \eta + \Delta z \Delta \zeta)\, x\}.$$

The second and third of the equations (2.) are similar to the first, consequently if we transform them in the same manner, and (by the principles of dynamics) put $\frac{d^2 \xi}{d t^2}$, $\frac{d^2 \eta}{d t^2}$, $\frac{d^2 \zeta}{d t^2}$ for X, Y, Z, we shall have

$$\frac{d^2 \xi}{d t^2} = m\,\Sigma . \{\phi(r)\Delta \xi + \psi(r) . (\Delta x\Delta \xi + \Delta y\Delta \eta + \Delta z\Delta \zeta)\Delta x\},$$

$$\frac{d^2 \eta}{d t^2} = m\,\Sigma . \{\phi(r)\Delta \eta + \psi(r) . (\Delta x\Delta \xi + \Delta y\Delta \eta + \Delta z\Delta \zeta)\Delta y\}, \quad (3.)$$

$$\frac{d^2 \zeta}{d t^2} = m\,\Sigma . \{\phi(r)\Delta \zeta + \psi(r) . (\Delta x\Delta \xi + \Delta y\Delta \eta) + \Delta z\Delta \zeta)\Delta z\}.$$

From these general equations, a number of integrals, adapted to particular cases, may be found. Let us suppose the vibrations of the particles to be performed in straight lines, all in one direction. This is a case of polarized light. Let x be taken in the direction of the vibrations; then η and ζ will be zero, and the first of the equations (3.) will give

$$\frac{d^2 \xi}{d t^2} = m\,\Sigma . \{\phi(r) + \psi(r)\Delta x^2\}\Delta \xi. \quad (4.)$$

Now, let ξ be a function of z and t, then for $\Delta\xi$ we may put

$$\frac{d\xi}{dz}\Delta z + \frac{d^2\xi}{dz^2}\cdot\frac{\Delta z^2}{2} + \frac{d^3\xi}{dz^3}\cdot\frac{\Delta z^3}{2.3} + \frac{d^4\xi}{dz^4}\cdot\frac{dz^4}{2.3.4}.$$

If we substitute this value of $\Delta\xi$ in equation (4.), and suppose the particles to be so arranged in their state of equilibrium that for every particle on one side of m, within the sphere of its influence, there is another at an equal distance on the opposite side, we may divide the sum Σ into two parts, one comprehending the positive values of Δz, and the other the negative; and for every term in one part we shall have a term equal to it, and involving the same power of Δz, in the other. But in the one part all the terms involving odd powers of Δz will be positive, and in the other all negative; consequently all these terms will vanish: the other terms being all positive, the sum of one part will be equal to that of the other. Consequently equation (4.) will become

$$\frac{d^2\xi}{dt^2} = m\Sigma.\{\phi(r) + \psi(r)\Delta x^2\}\Delta z^2\left(\frac{d^2\xi}{dz^2} + \frac{d^4\xi}{dz^4}\cdot\frac{\Delta z^2}{3.4}\right):$$

the sum, in this equation, extending only to the positive values of Δz.

For $m\Sigma.\{\phi(r)+\psi(r)\Delta x^2\}\Delta z^2$, write s^2, and for $m\Sigma.\{\phi(r)+\psi(r)\Delta x^2\}\Delta z^4$ write s'^2, then the last equation will be

$$\frac{d^2\xi}{dt^2} = s^2.\frac{d^2\xi}{dz^2} + s'^2.\frac{d^4\xi}{dz^4}. \qquad (5.)$$

If we omit the last term, this equation becomes exactly of the same form as that for the transmission of sound, and gives then no relation between the length and velocity of the waves. But if we integrate the equation as it is, we shall obtain a relation of this sort; and this relation will afford a theory of the dispersion of light.

As ξ is a function of z and t, it may be expressed by a series of terms such as $p\sin nt + q\cos nt$, where p and q are functions of z, and n a constant quantity*. Suppose then $\xi = p\sin nt + q\cos nt$; substitute this value of ξ in equation (5.), and it will become

$$\left.\begin{array}{l}\left(n^2p + s^2.\dfrac{d^2p}{dz^2} + \dfrac{s'^2}{3.4}.\dfrac{d^4p}{dz^4}\right)\sin nt \\ +\left(n^2q + s^2.\dfrac{d^2q}{dz^2} + \dfrac{s'^2}{34}.\dfrac{d^4q}{dz^4}\right)\cos nt\end{array}\right\} = 0.$$

* Poisson, *Traité de Mécanique*, No. 514, edit. 2.

This equation must be true for all values of t, therefore

$$n^2 p + s^2 . \frac{d^2 p}{d z^2} + \frac{s'}{3 . 4} . \frac{d^4 p}{d z^4} = 0, \qquad (6.)$$

$$n^2 q + s^2 . \frac{d^2 q}{d z^2} + \frac{s'^2}{3 . 4} . \frac{d^4 q}{d z^4} = 0. \qquad (7.)$$

Now, as p is a function of z, it may be expressed by a series of such terms as $a \sin k z + b \cos k z$, where a, b, and k are constant quantities. Substitute $a \sin k z + b \cos k z$ for p in equation (6.), and it will become

$$n^2 - s^2 k^2 + \frac{s'^2}{3 . 4} k^4 = 0; \text{ hence } \frac{n}{k}$$

$$= \sqrt{\left(s^2 - \frac{s'^2}{3 . 4} k^2\right)} = s \left(1 - \frac{k^2}{2 . 3 . 4} . \frac{s'^2}{s^2}\right), \text{ nearly.}$$

As the equations (6.) and (7.) are similar, and as we have put $a \sin k z + b \cos k z$ for p, we must put $a' \sin k z + b' \cos k z$ for q; a' and b' being two more constant quantities. Hence ξ may be expressed by a series of terms similar to

$$(a \sin k z + b \cos k z) \sin n t + (a' \sin k z + b' \cos k z) \cos n t.$$

With respect to any particular value of z, this term goes through all its values while t increases by $\frac{2 \pi}{n}$; and with respect to any particular value of t, it goes through all its values while z increases by $\frac{2 \pi}{k}$; consequently it represents a wave of light, moving in the direction of z, with the velocity $\frac{n}{k}$, the value of which has just been found equal to

$$s \left(1 - \frac{k^2}{2 . 3 . 4} . \frac{s'^2}{s^2}\right).$$

Professor Powell's expression for this quantity is

$$\text{H} \left\{ \frac{\sin\left(\frac{\pi r n}{l}\right)}{\frac{\pi r n}{l}} \right\}, \text{ which is equal to H } \left(1 - \frac{\pi^2 r^2 n^2}{2 . 3\, l^2}\right), \text{ very}$$

nearly. As the Professor considers only one term instead of the sums s^2 and s'^2, and as $r n$ and l in his notation are the same as Δz and $\frac{2 \pi}{k}$ in ours, the two expressions are virtually the same.

If we examine the composition of the quantity $\frac{s'^2}{s^2}$, we shall see that the relation between the length and velocity of a wave does not depend merely upon the ratio of the intervals of the particles to the length of an undulation, but also upon the radius of the sphere of their influence.

On the last principle, I think we can (as M. Fresnel seems to have conjectured *) account for the dispersion of light without supposing that the waves move with different velocities in the free ætherial medium; to which supposition there seems to be an insuperable objection †. But I reserve this for future consideration. In the mean time I hope Professor Powell will favour us, through the medium of your Journal, with what he has done towards the verification of M. Cauchy's formula, having told us that he is engaged on the subject ‡.

I am, yours, &c.

Evesham, Aug. 17, 1835. JOHN TOVEY.

P.S. In my next communication I believe I shall be able to show that if the vibrations of the particles of æther be each decomposed into three rectangular directions, two of which are perpendicular to the direction in which the light is propagated, and the other parallel to it, the vibrations in any one of these three directions may be calculated separately; and that a satisfactory reason may be assigned why the vibrations in the direction of propagation are insensible. (Airy's Tract, art. 101.)

September 3, 1835.

IV. *Extracts from a Prize Essay on Iodine.* By JAMES INGLIS, *M.D.*

[Continued from p. 444, and concluded.]

M. COURTOIS shortly after his discovery of iodine formed the black pulverulent iodide of nitrogen by the action of iodine upon hydrous ammonia. Gay-Lussac next described another compound of dry ammoniacal gas and iodine, which he called the ioduret of ammonia, but which might rather be called a hydriodate of an iodide of nitrogen. When this compound is thrown into water, decomposition takes place, hydriodic acid is found in the water, and the black iodide

* Airy's Tracts, p. 285, *note.* † *Ibid.*

[‡ Notices of Professor Powell's verifications of M. Cauchy's modification of the undulatory theory, so far as they have yet been made public, will be found in Lond. and Edinb. Phil. Mag., vol. vi. p. 374; and last volume, p. 293.—EDIT.]

of nitrogen is precipitated; so that if we suppose it to be a hydriodate, then we should not require to say that hydriodic acid and iodide of nitrogen were formed at the moment of solution, but that they existed in the compound ready formed, and that in consequence of the greater affinity which hydriodic acid has for water than for the iodide, they separate, and the iodide precipitates, whilst the acid is held in solution, as I found to be frequently the case when experimenting on the double iodides to be noticed hereafter.

If, instead of using the iodine alone with the aqua ammoniæ, there be added equal parts of a strong tincture of iodine and the ammonia, then I found that instead of the dark detonating ioduret, there was formed an *iodide of carbon*, similar to that formed by the tincture of iodine, and the alcoholic solution of potash. The periodide of carbon by this process is obtained in pretty large plates. 1 equivalent of ammonia + 1 of alcohol + 6 of iodine are required, and there result 1 atom of nitrogen gas (which is evolved and found in the upper portion of the vessel), 1 atom of water, 2 of iodide of carbon, and 3 of hydriodic acid, which latter unites with the excess of ammonia remaining in the solution. Its presence is indicated by the addition of bichloride of mercury, and a little muriatic acid to saturate any excess of ammonia which might redissolve the red periodide of mercury which is precipitated.

The black detonating teriodide of nitrogen is decomposed by almost every substance: oils and fatty matter do not cause its detonation as they do that of the chloride of nitrogen. The strong mineral acids all explode it when in a perfectly dry state.

I allowed aqua ammoniæ to remain over the iodide of nitrogen, in which there was no trace of free iodine, for several weeks; the iodide was decomposed, almost half of the vessel in which I had it was filled with nitrogen, and small crystalline points were seen floating in the fluid, at the same time that a yellowish deposition was seen at the bottom. Alcohol when allowed to remain in contact with the iodide, decomposes it. Nitrogen is evolved, the liquid becomes of a deep red colour, and iodide of carbon is formed, the smell of which is perceived in the fluid. Pure water is even reacted on by, or reacts with, the elements of this iodide. I found azote given off as before, the water assumed a ruby tint, and small crystals of iodine were precipitated.

The most probable cause of the explosive power of the iodide of nitrogen is this: Nitrogen requires an immense power to liquefy it. Indeed, by condensation it has never yet been done; but chlorine and iodine possess this power; the tendency, however, is still to regain its original capacity, so that any substance containing hydrogen, or any other element which combines with the chlorine or iodine, instantly liberates the nitrogen, and it expands with a force equal to that which would be required to liquefy it. * * * *

There is but one hydrocarburet of iodine noticed by authors; it appears in colourless acicular crystals, and is formed by the action of olefiant gas on iodine. Faraday, its discoverer, found it, in composition, quite analogous to chloric æther, and called it hydrocarburet of iodine †.

After having thoroughly dried a portion of iodine I introduced it into a flask, which was luted on to a gas tube with sulphate of lime; then the stop-cock was opened, and a constant supply of gas was thus allowed to enter as fast as the former was absorbed. Instantly there is action observed; the small grains of iodine on the sides of the vessel become semifluid, and of a dark colour, and the interior of the flask is gradually filled with ruddy brown fumes. In the course of four hours, the acicular colourless crystals of Faraday began to appear, and that in the *shade*, showing that the direct rays of the sun are not necessary, as he supposed‡. After the gas had been acting on the iodine for eighteen days I removed the flask, and observed a fluid at the bottom, which when examined was of a blackish green colour. It does not combine with water, but runs into globules like oil, or more exactly like a solution of iodine in creosote when it is placed in water. On the application of the heat of a spirit-lamp to a tube containing this fluid with the mixture of a drop or two of water, slight explosions take place, the black liquid is decomposed, a red fluid rises in vapour, and olefiant gas is evolved. The red fluid is probably a mixture of olefiant gas and free iodine, for it instantly casts with starch the characteristic blue tint.

When the black fluid is put into a small retort, and heat applied, olefiant gas is first driven off, and then a copious effusion of hydriodic acid; whilst at the same time the orange red fluid again appears. When the beak of the retort is placed in water the hydriodic acid is absorbed, and portions of the red and black fluids come over also; the latter falls ot the bottom. The water precipitates starch blue, and the perchloride of mercury instantly causes the precipitation of the periodide of mercury. Alcohol removes the substance that keeps this compound fluid, and the solid green hydrocarburet,

† See Phil. Trans. for 1821: or Phil. Mag., vol. lix. p. 352.—Edit.

‡ Mr. Faraday certainly formed this compound by exposing iodine and olefiant gas to the sun's rays, but he does not explicitly represent that the "direct rays" are necessary.—Edit.

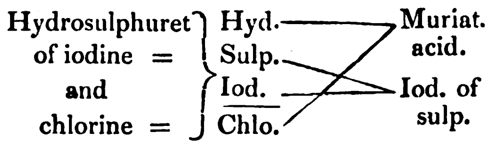
to be spoken of immediately, results. The sulphuric and muriatic acids have no action with the dark fluid, whilst they cause the decomposition of the red, precipitating its iodine. There can be little doubt that these two fluids are different compounds, but a limited period prevented any further inquiry.

The next compound to be spoken of is a *solid* hydrocarburet of iodine, sometimes of a dark blackish colour, at other times, and oftener, of a decided green. It has never been noticed by any chemical author, and differs from Faraday's in the following particulars. His is transparent, in white acicular crystals, shooting out from the sides of the flask, and formed, as I noticed before, in a very few hours after olefiant gas is brought in contact with iodine; of a sweet taste, and aromatic smell: it fuses and sublimes unchanged. Insoluble in water, acids, and alkalies; *soluble* in *æther* and *alcohol*, and may be crystallized from them.

This new hydrocarburet is opake, of a dark green colour, and not of crystalline texture; is formed after a longer action of olefiant gas on iodine; is destitute of taste and smell: it fuses and is decomposed, giving rise to another new compound, hereafter to be noticed; and lastly, is insoluble in both æther and alcohol. Mr. Kemp was the first to discover this compound, but he has never examined its properties. When it is first removed from the flask in which it has been formed, it is mixed with a large portion of Faraday's hydrocarburet, and also with the fluids already noticed: these last are allowed to drip from it, and then on boiling with alcohol the whole of Faraday's hydrocarburet is taken up, and the green compound remains behind, which, after repeated washings with alcohol, may be considered pure. The former sinks in sulphuric acid, whilst the green floats on its surface, and both are alike unacted on by it. It burns with a clear flame by heat; it emits olefiant gas and hydriodic acid, and there remains behind a carbonaceous residue.

At first, from the negative qualities of this green hydrocarburet of iodine, I thought it was merely carbon; but I soon altered my opinion, for I found that by placing this in a small tube retort, I obtained a perfectly new compound by distillation. I was led to this process by observing that when the green hydrocarburet was heated, dense brown fumes escaped, which emitted the odour of garlic. A receiver was therefore adapted to the retort, and being kept cool, a liquid of a deep reddish brown colour collected in it. Whenever the stopper is removed from the bottle in which it is contained, the room is soon filled with the smell of assafœtida. It is, like the former, highly inflammable, and consists also of carbon, hydrogen, and iodine. I have not examined its properties further, but from its peculiar odour have called it the fœtid hydrocarburet of iodine. * * * *

The compound of sulphur and iodine formed by Gay-Lussac is most likely merely a mechanical mixture, for after keeping it in alcohol in a closed vessel for several months, the alcohol became saturated with iodine, and the sulphur remained unaltered. I tried to procure a chemical compound in the same way as the chloride of sulphur is formed, but a similar one to the last resulted. I caused hydriodic acid to come in contact with the chloride of sulphur; instant reaction took place, muriatic acid was formed, and a dark compound, which was probably an iodide of sulphur, presented itself. Other means may be had recourse to, as



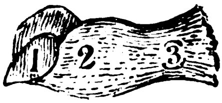
or, by the action of chloride of iodine on sulphuretted hydrogen, there would result, either muriatic acid and iodide of sulphur, or chloride of sulphur and hydriodic acid.

Having formed the sesquiodide of phosphorus, I laid it aside in a well-stoppered bottle; it, however, in a short time attracted moisture from the air, and on removing the stopper much condensed hydriodic acid burst forth. To get rid of the fumes, I added a small portion of water, and laid it aside; on examining it again I found a yellow powder at the bottom of the fluid. I added now a little more water; but whenever the red powder came in contact with it, instant decomposition took place, and much gas was evolved with brisk effervescence. After the disengagement of the gas had ceased, there still remained a red powder, which being dried and exposed to a moist atmosphere did not attract moisture; therefore it is not any of the former iodides. It bears a considerable heat without change; if, however, it be continued and agitated, it bursts into flame, and burns with the characteristic appearance of phosphorus. It is most probably an oxide of phosphorus, but differs from the following in being darker in colour and much less inflammable. The oxide of phosphorus, which the former resembles, is conveniently formed by placing phosphorus in a long glass tube, and then heating the tube until the phosphorus catches fire and liquefies. A current of air is now made to pass through the tube by blowing forcibly into one end, vivid combustion ensues, and the whole interior of the tube is filled with the yellow oxide. On raising the tube after the combustion has ceased, from the horizontal to the perpendicular position, a splendid phænomenon takes place; a bright red glow of light commences at the bottom of the tube and gradually rises to the top after traversing the whole mass. This compound Mr. Kemp considers to be an oxide of phosphorus.

* * * * * * *

I found that when carbon and dry pure boracic acid are heated to redness in a porcelain tube, and pure iodine dropped into it whilst at this high temperature, a small portion of a yellow compound sublimed, which I considered as an iodide of boron. * * * *

When solutions of the protonitrate of mercury and hydriodate of potassa are mixed together, the green protiodide of mercury is precipitated; but by this method a portion of the yellow iodide is almost invariably found mixed along with the green, on account of the presence of a portion of the pernitrate. But this is completely obviated, and a very pure protiodide formed, when the elements themselves are made to act on each other. I found that on agitating together the iodine with an excess of metallic mercury in a tube, that combination was formed. After the action has commenced, the addition of a little water facilitates its completion. This iodide, by the combined influence of air and light, is resolved into metallic mercury and the biniodide. To try which of these agents had the greater power, I placed a portion of the green iodide (being perfectly dry) in a closed box impenetrable by any species of light. On examining it in a few weeks afterwards, I found that it was only partially decomposed, and those portions that had undergone the change had assumed a very beautiful appearance. There was rising out of the mass at different places a formation exactly similar to one of vegetable origin: 1. represents a small mass of green iodide from which the cryptogamic-looking excrescence sprung; 2. represents the root, which was of a red crimson colour; and 3. is the upper expanded portion, which on the exterior was covered with a feathery crystallization of the yellow iodide. The interior was hollow, and was externally of the same rich red colour as its root. These vegetations resembled much in richness and beauty the bells of some of the finest heaths.



Another portion of the green iodide was placed in a small phial filled with distilled water, which after being exposed to the light for many weeks, still retained its original green colour, being as yet undecomposed. Air, therefore, is the principal agent in effecting its decomposition, since in both instances the temperature was the same, and must have affected each alike. As the bichloride of mercury added to a solution of the hydriodate of potassa causes the formation of the biniodide, it might be expected that the protochloride would give us the green protiodide, which on trying I found to be the case; when equal parts of calomel and hydriodate of potassa are added to each other (the one in solution and the other suspended in water) an instant interchange takes place, and the green iodide is produced. This took place in all cases, whether I used the calomel in excess, or *vice versâ*. But I found that on pouring off the supernatant liquid from the green iodide, in either of the above instances, and now adding the calomel, the precipitation wholly consisted of the beautiful bright yellow iodide; or, if to a solution of the nitrate of mercury in excess, there be added the above-mentioned liquid, there is instantly a flocculent precipitation of pure sesquiodide. From these facts I presume that in the process for the production of the green iodide there is formed a sesquichloride of mercury, *i. e.* a chloride having one half more chlorine in its composition than calomel = ($2\ Hg + 3\ Ch$, or $1\ Hg + 1\frac{1}{2}\ Ch$), and analogous to the sesquiodide. I mentioned before that the same results always followed whether I used the calomel, or the hydriodate of potassa in excess.

The yellow sesquiodide of mercury may be kept for any length of time excluded from the light without changing colour; but if exposed it soon acquires a dark hue. By heat one might suppose it had been converted into the biniodide, for it assumes first a red hue, then by continuing the heat it fuses and becomes of a deep crimson colour, and volatilizes into crystals of the same tint, but on cooling the original yellow is restored. It is singular enough that exactly an opposite effect is produced by heat on the biniodide; it is converted at 400° Fahrenheit into a deep blood-red-coloured fluid, which volatilizing condenses on the sides of the tube into yellow acicular crystals, which retain that colour for a considerable time, unless suddenly cooled or agitated, when the characteristic crimson tint of the biniodide again appears.

* * * * * * *

The biniodide falls as a rich red powder when solutions of the bichloride of mercury and hydriodate of potassa are mixed together, and in this form it is generally seen. I, however, have procured it in pretty large crystalline cubes by the following process. I found that it was dissolved in great abundance by a boiling solution of the hydriodate of zinc. I added the powdered biniodide till no more could be taken up, and then placed this saturated solution under the exhausted receiver of an air-pump; in a short time the biniodide began to be deposited, and soon they increased and assumed the form of large regular cubes. The hydriodate of zinc that remained was capable of dissolving a fresh quantity of biniodide, or of redissolving that which was crystallized from it: the crystals contain no zinc; they are acted on by chemical agents and by heat just in the same manner as the precipitated biniodide, and are quite distinct from the hydrargo-biniodide of zinc to be mentioned hereafter. The biniodide of mercury is sufficiently soluble in an excess of the hydriodate of potassa, from which there results on slow evaporation a yellow salt, which I discovered many months ago, and called it the double hydriodate of the biniodide of mercury and potash, because I found that whenever it was brought in contact with water, instant decomposition took place, the water from its strong affinity for hydriodic acid removed it, and the red biniodide was precipitated. Bonsdorff, however, calls this compound the hydrargo-biniodide of potassium. * * *

On mixing bichloride of mercury in powder to a saturated solution of hydriodate of potassa, and agitating together, the whole becomes nearly a solid red mass, and much heat is at the same time generated. * * * This red iodide is formed by many other processes, as when a solution of bicyanuret of mercury is added to an alcoholic solution of iodine, in which case it instantly precipitates.

When the yellow sesquiodide is kept for some time under water, and exposed to light, very good *small* cubic crystals of the red iodide are found covering the surface; but the method I described above is the best one for obtaining it in its crystalline form.

I have reason to think that there is another iodide of mercury, of a blue colour; it is formed by freely exposing in an open vessel the red iodide with an excess of metallic mercury. In the course of three or four weeks the surface assumes a decidedly blue tint. I have not as yet further examined this compound. * * * * * *

On examining the crystal of the iodide of lead with the aid of the microscope, it is found to be a flat six-sided crystal [prism?]. This, next to the tetrahedron, appears, from what I have observed to be the most common form of crystallization. * * *. I found that when, instead of using just a neutralizing sufficiency of hydriodate of potassa and acetate of lead, the hydriodate was added in excess, there was thrown down a white soft powder and not the yellow iodide. And by ammonia the yellow iodide is converted into

a similar white powder, which, perhaps, may be another iodide of lead. If metallic tin be boiled with the iodide of lead, no reaction takes place; but if the dry iodide be mixed with granulated tin, and exposed to heat, combination takes place, and a double iodine of tin and lead results, of a brown colour and differing from either iodide separately. By boiling this double iodide in water, very beautiful crystals of the yellow iodide of lead are obtained. [To be continued.]

V. *Observations on the Steam Engines of Cornwall; in Reply to* John Taylor, *Esq., F.R.S., Treas. G.S., &c. By* W. J. Henwood, *F.G.S. Lond. & Paris, Hon. M.Y.P.S., Curator of the Royal Geological Society of Cornwall.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Mr. Taylor's communication in your present month's Number* appears to imply that a *rotatory single* engine working expansively is something of a novelty.

Now, Mr. Watt's first *expansion* engine was erected in 1778†; the patent for his *rotatory* engine was taken out in 1781, October 25th ‡; and that for his *double* engine in 1782, March 12th §.

Thus the *rotatory single* engine *working expansively preceded* the *invention* of the *double* engine; and some of the former construction were erected by Messrs. Boulton and Watt on the Cornish mines.

At Binner Downs mine in this county Messrs. Gregor and Thomas have erected *five rotatory single* engines working expansively; the first of them in 1828, the last in 1833. Captain Gregor also set up a similar one, for driving a common grist mill, for Messrs. Harvey and Co. of Hayle Foundry.

All these have performed their work extremely well; are quite as manageable as *double* engines; and, where they have taken their place, have worked with much less coal. The duty of those at Binner Downs, which are used as winding (whim) engines, Captain Gregor *estimates* at about 15 millions of pounds raised one foot high by the use of each bushel of coal consumed ||.

Mr. Taylor speaks of "the method of working high pressure steam expansively, which we owe to Mr. Woolf ¶."

* Vol. vii. p. 369.

† Farey on the Steam-Engine, p. 341.

‡ *Ibid.*, p. 346.

§ *Ibid.*, p. 350.

|| Captain Lean reports the duty of Mr. Sims's engine at Charlestown 44, and not 60, millions, as stated by Mr. Taylor.

¶ Lond. and Edinb. Phil. Mag., vol. vii. p. 369.

Now, in former numbers of this Journal *, I have shown that *in* 1811—1812, *Captain Trevithick erected a single engine* at Huel Prosper mine, *in which steam of above* 40 *pounds pressure on the square inch was worked expansively.* Mr. Farey observes† that *Mr. Woolf came to reside in Cornwall about the year* 1813, and the "first engines for pumping water from the mines were set up by him in 1814;" but these, he adds, had two cylinders. I therefore repeat, that *we do not owe this practice to Mr. Woolf, but to Captain Trevithick.* But it has been already shown that this is *only an extension of Mr. Watt's practice* of 1778.

The advance in the duty of steam engines which has taken place in this county within the seven years last past, is principally, if not entirely, due to Captain Grose; and is obtained mainly by the application of substances which transmit heat very slowly, to the surfaces of the portions of the apparatus containing *dense* steam.

I remain, Gentlemen,

Yours, &c.

1, Morrab Place, Penzance, November 28, 1835. W. J. Henwood.

VI. *On an Error in* Dr. Apjohn's *Formula for inferring the Specific Heats of dry Gases. By* H. Hudson, *M.D., M.R.I.A.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I BEG to point out a serious error in the formula given (in p. 385 of your 7th volume) by Dr. Apjohn for ascertaining the specific heats of gases by their action on the "evaporation thermometer." I have shown (in your Journal for October last, same vol., p. 257,) that (taking the density of air at 212° under 30 pressure as unity) the density of the vapour of saturation at $t°$ is $= \frac{f}{448+t} \times 13{\cdot}75$, consequently (the weight of a cubic inch of air at 212° under 30 being $\frac{\cdot 327958}{1\cdot 375}$ ‡) the weight of a cubic inch of the vapour of saturation at $t°$ is $= \frac{f}{448+t} \times \overset{\text{Grs.}}{3{\cdot}27958}$; also the latent heat of vapour at $t°$ be-

* Phil. Mag. and Annals, vol. vii. p. 323, March 1830, and vol. x. p. 98, August 1831.

† *Ibid.*, vol. viii. p. 308.

‡ ·327958 is the weight at 32° (Prout), and 1·375 the expansion between 32° and 212° (Gay-Lussac).

ing $= 1168-t$ (capacity of water being $= 1$), it follows that $\frac{1168-t}{448+t} \times f \times \overset{\text{Grs.}}{3{\cdot}27958}$ of *water* in falling 1° will give out the quantity of heat necessary to produce this cubic inch of vapour. But in the experiments with dry gases an equal volume (*i. e.* one cubic inch) of the gas falling V° produces this same effect; consequently (S being the weight of a cubic inch of the gas at t°) $V \times S$ gives the weight of the gas which would produce this effect in falling 1°; from whence it is obvious that 1 (the capacity of water): C (the capacity of the gas) :: $S \times V : \frac{1168-t}{448+t} \times f \times 3{\cdot}27958$, and $C = \frac{1168-t}{448+t} \times \frac{f}{V \times S} \times 3{\cdot}27958$; or (since S′, the weight of a cubic inch of the gas at 60°, $= \frac{S \times 508}{448+t}$) we have by substitution

$$C = \frac{1168-t \times f}{V \times S'} \times {\cdot}006456.$$

Now, if V (the depression of wet thermometer) in hydrogen gas be $= 20°$ (t', the temperature of wet ball being 48°), and if V in atmospheric air be $= 25°$ (t being $= 43°$), and taking weight of cubic inch of hydrogen at $60° = \overset{\text{Gr.}}{0{\cdot}02153}$, and weight of cubic inch of air at $60° = \overset{\text{Grs.}}{0{\cdot}3099}$*; consequently,

$$\text{capacity of hydrogen} = \frac{1120 \times 34875}{20 \times {\cdot}02153} \times {\cdot}006456 = 5{\cdot}856,$$

$$\text{and capacity of air} = \frac{1125 \times {\cdot}29348}{25 \times {\cdot}3099} \times {\cdot}006456 = {\cdot}2751.$$

Dr. Apjohn appears to have used (*for every gas*) the weight of a cubic inch of air, instead of S, the weight of the particular gas. Accordingly, the experiments (except with hydrogen) rather favour my view that the capacities of gases are equal *in equal volumes.*

The same I believe to be true with hydrogen, and that V will be found the same in every gas (with an improved apparatus for trying the experiments) under similar circumstances of temperature and pressure, if the current of gas be sufficiently powerful.

* I have supposed P (the pressure) $= 30$. The requisite alteration in the value of S′ gives $C = \frac{1168-t \times f}{V \times S' \times P} \times {\cdot}006456 \times 30.$

Hoping for an immediate insertion of this, I shall reserve any remarks on the probable errors in the experiments, and their causes, for a future occasion.

I am, Gentlemen, yours, &c.

24, Stephen's Green, Dublin, Nov. 11, 1835. H. Hudson.

VII. *Remarks on a Paper on the Transmission of Calorific Rays, &c. by* M. Melloni, *in the Phil. Mag. and Journal of Science, No.* 42. *By the Rev.* B. Powell, *M.A., F.R.S., Savilian Professor of Geometry, Oxford.**

IN the last Number of this Journal (vol. vii. p. 475) a short communication appears from M. Melloni, in which that distinguished experimenter has honoured me with a reference to the experiment which I tried in 1825, which forms the basis of a peculiar view of the nature of the heat originating from luminous hot bodies, and which M. Melloni has since successfully verified with his extremely delicate apparatus, so as entirely to remove all doubt, which (I presume from the silence of physical writers) must previously have been felt on the subject.

But while he speaks with approbation of that experiment, M. Melloni refers to my views connected with it in terms which imply a most singular misconception of them, and on which I therefore feel it necessary to offer a very few remarks.

M. Melloni describes me as "endeavouring to explain his results by hypotheses" which are untenable. Now, I am not aware of having attempted to *explain* M. Melloni's results at all. All that I have contended for is, that if the distinction between two kinds of heat, "luminous and obscure" (as the author terms them), be admitted, (and he himself, I believe, admits it,) it will follow, that all results which have hitherto been commonly stated as referring to "radiant heat," will require now to be *more precisely worded*, and we must say *which sort* of radiant heat we mean, in all cases where there may be both present.

That particular result which M. Melloni obtained in the repetition of my experiment with the thermomultiplier, and which so strongly confirms it, I have, indeed, adverted to as, to my apprehension, ill explained by the gratuitous hypothesis, that the heat acquires a new property with regard to its relations to surfaces by merely passing through glass; which seems to me at once needless and contrary to all analogy;

* Communicated by the Author.

and, instead of it, I have maintained the simple conclusion of two kinds of heat simultaneously emanating or originating from luminous hot bodies.

As to contending that this distinction of two kinds of heat "suffices to explain all the facts relative to transmission," I should have been glad if M. Melloni had pointed out any passage in which I have contended for anything of the kind. The facts of transmission (and for all these most curious and important facts we are entirely indebted to the experimental skill of M. Melloni) are of a kind as yet appearing so little reducible to fixed laws that I should imagine *any* theory entirely premature; certainly I have offered none.

The experiment described by M. Melloni (p. 477 at the bottom) is undoubtedly a most curious and interesting one, but how it applies to the question relative to mine I fail in perceiving. It proves, the author conceives, that there are in this case "*several different kinds of dark heat.*" It is true we have hitherto known of but *one*, and I have referred only to *one* in my researches, but I have never denied that there may be *two*, *three*, or a hundred kinds. I have merely maintained that there are characteristic and well-marked distinctions in the properties of any or all non-luminous heat (to adopt for brevity the barbarously incorrect language which is becoming current) from those of the luminous kind; but there may still be many more such characteristic distinctions, and some such M. Melloni seems to have established in this experiment.

I look with great interest to the further extension of this curious inquiry on a point requiring the most careful examination; while I acknowledge that the thermomultiplier of M. Melloni has opened to us an entirely new field of investigation, and in the hands of its inventor and of Prof. Forbes has done more for the advance of our knowledge in this department within a very short time past, than the most sanguine would have ventured to anticipate.

VIII. *Further Observations on* M. Cauchy's *Theory of the Dispersion of Light. By the Rev.* BADEN POWELL, *M.A., F.R.S., Savilian Professor of Geometry, Oxford.**

IN a former paper, which is inserted in successive portions of this Journal, No. 31 *et seq.* (vol. vi.), I have given an abstract of M. Cauchy's highly important researches on the undulatory theory, so far as they bear on the great question of the dispersion of light, and in conclusion have deduced a simple for-

* Communicated by the Author: on this subject see also a preceding article by Mr. Tovey.

mula expressing the relation between the length of a wave and the velocity of its propagation. In a paper in the Philosophical Transactions for 1835, Part I., I have exhibited the results of calculation by means of this formula, by which theory is compared with observation for all the cases determined by M. Fraunhofer, and it will, I believe, be admitted that the accordance is as close as can be reasonably expected.

Since that paper was printed I have been indebted to Professor Sir W. R. Hamilton for bringing to my notice the circumstance that the formula as there deduced, owing to certain assumptions made in the course of the investigation, is not absolutely rigorous, although under conditions which may be easily admitted as likely to subsist it is reduced to the form which I have used. The state of the case will be rendered evident from the following considerations.

In order to simplify the investigation M. Cauchy adopts the method of supposing an expression, which really consists of *the sum of a series* of analogous terms, reduced to a *single term*; upon this he pursues his inferences with respect to it, and then in the conclusion recurs to the summation again. The complete resulting expression would represent the motions of an entire system, considered as produced by the combination of many, or even an infinity of, similar motions, each represented by the simplified equations obtained with the omission of the sign of summation. This will be understood on a comparison of those parts of my abstract which introduce equations (21.) and (56.). On the same supposition I have proceeded to that deduction which leads to the formula expressing the relation between the length of a wave and the refractive index. (See p. 265, Lond. and Edin. Phil. Mag., April 1835.)

The formula thus deduced, in its simplified shape, viz.

$$\frac{1}{\mu} = \mathrm{H}' \left\{ \frac{\sin\left(\frac{k\,r\cos\delta}{2}\right)}{\left(\frac{k\,r\cos\delta}{2}\right)} \right\}$$

is obtained by collecting together into one constant (H′) the sum of a number of terms of analogous forms which compose the values of the coefficients L, M, &c. Now if we recur to the expressions from which these values were originally derived, the equations (22.) and (12.), (or in the original memoir, more explicitly, equation (20.),) we shall readily perceive that the values of these coefficients in their *exact* form (that is, retaining the sign of summation,) are such as these:

$$\frac{L}{k^2} = S\{F(m, r, \alpha) \cdot \phi(k, r, \cos\delta)\},$$

$$\&c. = \&c.$$

Whence we should derive.

$$\frac{s^2}{k^2} = S\left\{\left(F(m,r,\alpha) + F(m,r,\beta) +, \&c.\right) \cdot \phi(k, r, \cos\delta)\right\}.$$

Hence by the same process as that employed before, we may obtain a corresponding abridged expression

$$\left(\frac{1}{\mu}\right)^2 = S\left\{(H^2)\left(\frac{\sin\left(\frac{k\,r\cos\delta}{2}\right)}{\left(\frac{k\,r\cos\delta}{2}\right)}\right)^2\right\}.$$

To perceive more clearly the difference between the exact and approximate expressions, we may first observe, that since we have from equations (19.) and (45.)

$$\frac{k}{2} = \frac{\pi}{\lambda} \text{ and } r\cos\delta = \Delta\varrho,$$

the arc which is involved in the formula becomes $\frac{\Delta\varrho \cdot \pi}{\lambda}$.

Now, if we take the *simplified* formula, develope the sine in terms of the arc, and divide by the arc, we shall have

$$\frac{1}{\mu} = H'\left\{1 - \frac{1}{6}\left(\frac{\Delta\varrho \cdot \pi}{\lambda}\right)^2 + \frac{1}{120}\left(\frac{\Delta\varrho \cdot \pi}{\lambda}\right)^4 - \&c.\right\};$$

whereas the *exact* formula, in the same way, would give

$$\frac{1}{\mu} = \sqrt{S(H^2)}\left\{1 - \frac{1}{6}\frac{S\left[(H^2)\left(\frac{\Delta\varrho \cdot \pi}{\lambda}\right)^2\right]}{S(H^2)} + \frac{1}{120}\frac{S\left[(H^2)\left(\frac{\Delta\varrho \cdot \pi}{\lambda}\right)^4\right]}{S(H^2)} - \&c.\right\},$$

supposing the series to converge rapidly enough.

Now, this would manifestly be the same as the last if we were at liberty to suppose

$$S\left[(H)^2\left(\frac{\Delta\varrho \cdot \pi}{\lambda}\right)^2\right] = [S(H^2)] \cdot \left[\frac{\Delta\varrho \cdot \pi}{\lambda}\right]^2,$$

and similarly in the other terms: in which case we should only have a common multiplier for the whole series, which would be represented by H'. Now this supposition would be the same as that of

$$S[(H^2)(\Delta\varrho^2)] = (S(H^2)(\Delta\varrho^2)$$

for the same value of λ; or, since $\Delta\varrho$ is the difference in perpendicular distance from a given plane, of the molecule at the point $x\,y\,z$ at the end of the time t, it will be evident, on a little consideration, that to disregard the sign of summation altogether corresponds to taking into account only the action of two adjacent molecules. If again we apply it only to H^2 (as in *our* simplified formula), without regarding $\Delta\varrho$ as variable, this is equivalent to considering only the action of two adjacent parallel strata of molecules, for all of which $\Delta\varrho$ is the same. But if $\left(\frac{\Delta\varrho}{\lambda}\right)$ be small, and the series consequently converge rapidly, $\left(\frac{\Delta\varrho}{\lambda}\right)$ being still of sensible magnitude, we may suppose that this is not far from the truth.

I will not, however, say more with regard to the analysis of the theory at present, as the subject has been taken up by Sir W. R. Hamilton, with whose researches on systems of rays, in fact, the other parts of M. Cauchy's investigations are closely connected. My abstract has been restricted to so much of those investigations as refers directly to the subject of the dispersion; but the entire theory, of which it forms a part, embraces the curious and beautiful discussion of wave surfaces: and the connexion and analogy of some of the most important of these results with his own researches are specificially pointed out by the Irish Astronomer Royal in his third Supplement to the Theory of Systems of Rays in the Transactions of the Royal Irish Academy, vol. xvii. p. 125 and 141. Since this paper went to the press, that eminent mathematician has kindly given me permission to make what use I please of some further investigations on the subject of the dispersion-formula, including its numerical applications, which he had communicated to me. I hope, therefore, in a subsequent Number of this Journal to give some account of these important researches.

The development of the value of $\left(\frac{1}{\mu}\right)$ in a series of powers of λ, in a form available for the actual comparison of theory with observation, by the use of a peculiar method for determining the coefficients, appears also to have been lately investigated by M. Cauchy. His "*Exercices de Mathématique*," which, as I stated in a former paper, were broken off abruptly in 1830, have now been resumed, and are in the course of publication at Prague, under the title of "*Nouveaux Exercices*," &c. They will contain the continuation of the theory of dispersion, and the development in a form adapted to calculation. The distinguished author also has recently produced a memoir on interpolation, by a new method, which in conclusion he briefly applies (but without sufficient explanation) to the calculation of the refractive indices, in one instance of flint glass from Fraunhofer.

One thing, however, is clear, viz. that from the close accordance between all the results which I have calculated (by the approximate formula) and those of observation, viz. the ten sets of indices obtained by Fraunhofer, and since that, ten other sets determined by M. Rudberg (very recently communicated to the Royal Society*), it is sufficiently evident that at least for all these cases the approximate supposition is as near the truth as, perhaps, will be thought sufficient, when all circumstances are considered.

It is, however, still quite conceivable that the differences, minute as they are, may be accounted for by a more accurate prosecution of the analysis. Again, it remains to be seen whether in other cases, especially those of more highly dispersive media, the same method will still apply, or whether we must have recourse to a more complex investigation, which shall yet include, as a simplified case, the formula which holds good for media of low dispersive power.

IX. *A Sketch of the Geology of West Norfolk. By* C. B. Rose, *Fellow of the Royal Medical and Chirurgical Society of London.*

[Continued from vol. vii. p. 376, and concluded.]

Diluvium.—CLAY, sand, or gravel of varying thickness, and frequently alternating beds of these substances, are found immediately incumbent on the chalk, and obscure in many places its outcrop, as they also do that of the *gault*, *lower greensand*, and clays of Marshland†. These irregular beds, alternating with each other, without any order of superposition, have received the name of *diluvium*; but it is so difficult to determine what has been deposited by diluvial agency, in other

* So long ago as 1827 we received and inserted in Phil. Mag. and Annals, N.S., vol. ii. p. 401, a paper on the undulatory theory of dispersion from M. Rudberg. Has this been lost sight of in the recent investigations of the subject? Some of the calculated numerical results obtained by M. Rudberg, we observe, are identical with those obtained by Professor Powell, as given in Phil. Trans. 1835, pp. 252, 254.—Edit.

† The *diluvium* in Marshland is covered by a considerable thickness of *alluvial* deposits.

words, by *the mighty debacle*, and what was deposited by the usual currents found in large basins of water, into which huge rivers emptied themselves, that, with our present knowledge, in my opinion, we cannot apportion the effects due to each agent. That the clays, particularly, are composed of transported materials there cannot be a question, for the chalk which forms the substratum of the greater part of western and central Norfolk could not furnish the *blue* clay so frequently met with upon it: not only this, the *boulders*, so abundantly found in the clay, inclose organic remains which enable us to determine that their parent rocks are situated fifty, nay hundreds of miles apart from *them*. Without noticing the fragments of primitive rocks (which are more difficult to identify, in consequence of their not containing *organic remains*), I may particularize boulders from the *old red sandstone*, *mountain limestone*, *alum-shale* of Whitby, *blue lias*, *cornbrash limestone*, *Septaria of the Oxford and Kimmeridge clays*, &c., all inclosing animal exuviæ that indubitably determine from what strata they were disrupted. As many of these boulders weigh some hundreds of pounds, indeed, some tons*, it is fair to infer that no common current or torrent could have impelled them to their present sites, making every allowance for time; indeed, the magnitude of some of them, the distance they must have travelled, and the want of order in the arrangement of the clay, sand, and gravel, all combine to render it highly probable that the transport of these materials could not have been effected by any other agent than the Noachian Deluge.

The *light* lands covering the outcrop of the *chalk* abound in bleached fragments of flint, the debris of the abraded chalk; these fragments are in many places (as around Castleacre) so abundant, that it is found necessary to pick them from the land about once in four years, to the amount sometimes of two loads (24 bushels to the load) per acre. The clay of the *heavy* lands is either yellow or blue: the former contains a large proportion of calcareous matter, and in it large fractured flints predominate, with their angles *sharp*; the *blue* clay is in a much greater degree argillaceous, and is also remarkable for the abundance of *boulders* of the *oolitic* series of rocks, having all their angles *rounded*. The *gravel* beds are principally composed of fragments (in the form of pebbles) of almost every member of the series of rocks, from *granite* upwards, with every angle effaced, manifestly the result of long exposure to attrition.

In some situations, as is exemplified on Necton Common

* A boulder of *breccia* in a clay-pit at Fouldon, south of Swaffham, weighs several tons.

near Swaffham, the gravel beds contain so large a quantity of decomposing iron pyrites, that the water percolating the gravel is sufficiently charged with iron to cement the sand and stony fragments together, and form a coarse breccia. Under similar circumstances, the water of some springs has a considerable ferruginous impregnation: at Thetford a *chalybeate* spring occurs, containing also *carbonate of soda* and free *carbonic acid*, with a proportion of *iron* not inferior to that of Tunbridge Wells, although it flows from a very different source; the elaboratory of *our* chalybeate being situated in the diluvial beds, and the decomposition of iron pyrites from the disrupted chalk strata affording the ferruginous ingredient.

Fragments of *calcareous tufa* are occasionally met with in these beds.

Being desirous of not extending this paper beyond the limits of a periodical, I forbear noticing the œconomical and agricultural purposes to which these beds are applied; and for the same reason I shall refer your readers to Mr. Samuel Woodward's Geology of Norfolk * for a list of the *antediluvian* organic remains, which are, for the most part, inclosed in *boulders*.

The only *mammalian* remains I have seen are, part of a tusk of *Elephas primigenius* found at Hunstanton, teeth and vertebræ of *Elephas Indicus* from beneath the *brick-earth* at Narford, and part of a tooth of the *Mastodon latidens*? found in a gravel-pit at Swaffham.

It is worthy of notice, that the parent strata from which the boulders must have been originally detached are all situated to the north and the west of our county.

Alluvium.—The first deposit I shall notice under this head has received the name of "*Brick-earth of the Nar*," from my having (till very recently) found it only in the valley through which that river takes its course. In this valley I have traced it west and east from Watlington through East Winch and West Bilney to Narford, a distance of nine miles. It occupies low ground, except at its inland extremity, where it rises to about eighty feet above the level of the Nar.

Mr. Arthur Young in his "Agricultural Survey of Norfolk," speaking of this deposit, under the article "*Manure Oyster Shells*," says, "In East Winch and West Bilney, and scattered for ten miles to Wallington (Watlington ?), there is a remarkable bed of oyster-shells in sea mud: the farmers use them at the rate of ten loads an acre for turnips, which are a very good dressing; they are of particular efficacy on land

* An Outline of the Geology of Norfolk, page 39, "*Clay of Western Norfolk.*"

worn out by corn. * * * * * They are found within two feet of the surface, and as deep as they have dug, water having stopped them at sixteen or eighteen feet deep. They fall into powder on being stirred."

The clay inclosing the shells is of a slate blue colour, and upon drying falls into laminæ; it contains numerous spangles of *mica*, and in the lower part of the bed at Winch and Bilney there is a considerable admixture of sand. It has a very muddy smell when first opened, and the water which rises from it is too offensive to be used for culinary purposes. No *boulders* have been found in it.

At West Bilney it is generally covered by two or more feet of earth, consisting of vegetable soil, and yellow sandy loam, containing small pebbles and angular fragments of flint. The *yellow* loam burns into a red brick; a portion lying between the loam and the blue clay, and probably a mixture of the two, produces a mottled brick; and the blue clay, usually denominated the *brick-earth*, becomes a fine white brick. At another part of the brick-yard bleached shells, chiefly *Turritella Terebra* and *Mactra subtruncata*, are found immediately beneath the vegetable soil in white sand: the same shells are also scattered through the *brick-earth*, with *Ostrea edulis*, *Rostellaria Pes Pelecani*, &c. At this locality a well was sunk to the depth of forty feet, and *Ostreæ* and *Rostellariæ* were still brought up; but the oysters were most abundant at the depth of three or four feet from the surface. Two fragments of the grinding teeth of the *Ox*, and small portions of bone, were also found in the blue clay, at the depth of five feet.

At East Walton, *Ostreæ*, *Turbo littoreus*, and fragments of a *Pecten* are turned up by the plough; in a pit they may also be seen imbedded in a light-coloured alluvial clay, rising abruptly from the valley of the Nar to the height of eighty feet above the level of the river: the shells are much more broken than those found in the blue clay, situated at a lower level; indeed, in the latter situation but few are at all injured. At Walton Stocks the same shells were also found.

At Narford, near the Hall, in the same fetid blue clay as at Bilney, *Ostreæ*, accompanied by *Rostellariæ*, were discovered beneath a considerable bed of sand and loam; the clay was sunk through at the depth of twenty-seven feet, and in its lowest portion teeth and vertebræ of the Asiatic Elephant were found: this is the most inland extremity of this deposit at present detected. The shells of the same deposit have also been found at a brick-yard in East Winch, covered by seven feet of sand and loam: beneath these lie a light-coloured argillaceous earth, six feet in thickness, containing a few shells, which reposes upon the blue clay, in which the *Ostreæ*, *Ros-*

tellariæ, &c. are very abundant; the *blue clay* has here been opened to the depth of ten feet. Very recently have been discovered at this spot, in the *loam*, fragments of a tooth and bones of an elephant, and a broken tooth of a rhinoceros.

In the middle of the village of East Winch, by the side of the road leading to Lynn, *Ostreæ* and *Rostellariæ* were discovered on sinking a well; and on Mr. Forster's farm, in the same parish, similar shells were found.

At Tottenhill brick-yard, a short distance from the road leading from Lynn to Downham, and at Watlington, the same bed of blue clay is met with, inclosing similar shells to those at West Bilney.

The same kind of blue clay was opened last summer about half a mile to the south of Middleton Tower, in a valley running parallel to that of the Nar, and separated from it by the high ground on which the village of Middleton stands; a small stream takes its course through this valley, emptying itself, as the Nar does, into the Ouse at Lynn. *Ostrea edulis* and *Turbo littoreus* were found six feet below the surface.

In some localities, with the *Ostreæ* have been found *Cardia*, *Mactræ*, and other shells, of which the following is a list. The greater number of the oysters are large, thick, and antiquated; they and the *Rostellariæ* are very abundant; *Natica glaucina* is next in abundance; *Pecten* and *Cerithium* are scarce. The shells have not suffered by attrition, but few are broken, and none of them are mineralized.

Organic Remains.

Name.	*Reference.*	*Locality.*
Vermilia triquetra	Brown's Illust., pl. 2. f. 1, 5.	On Ostreæ, W. Bilney.
Cardium echinatum ...	———— pl. 21.	East Winch.
——— edule...........	Wood's Conch., pl. 55. f. 4.	Ditto. Ditto.
Corbula Nucleus.........	Brown's Illust., pl. 14. f. 6, 9.	W. Bilney.
Mactra subtruncata......	———— pl. 15. f. 7.	Ditto. E. Winch.
——— solida	Penn. Brit. Zool., pl. 55. f. 2.	Ditto.
Ostrea edulis	Brown's Illust., pl. 31. f. 19.	Do. Narford, &c.
Pecten varius	Penn. Brit. Zool., vol. iv. pl. 64....................	Do. Walton.
Tellina, young specimens	, species undetermined.	W. Bilney.
Cerithium reticulatum	Geol. Norf., t. 1. f. 2.	W. Bilney.
Turritella Terebra ...	Brown's Illust., pl. 51. f. 56. Min. Conch., t. 565. f. 3.	Do. E. Winch.
Buccinum reticulatum	Penn. Brit. Zool., pl. 75. f. 2.	Ditto.
Turbo littoreus	Brown's Illust., pl. 46 f. 1, 9.	Do. Do. Walton.
Rostellaria Pes Pelecani	Penn. Brit. Zool. vol. iii. pl. 78. Sow. Min. Con., t. 558. f. 1.	Do. Do., &c.
Natica glaucina	Brown's Illust., pl. 43.	Do. Do.
Bos, teeth of, ...	....	Ditto.
Elephas Indicus, teeth and vertebræ of.		Narford. E. Winch.
Rhinoceros, fragments of a molar tooth of the lower jaw*.		E. Winch.

* Dr. Buckland's *Reliquiæ Diluvianæ*, pl. 7. fig. 6.

We have here shown, that within the valley of the Nar there occurs an extensive deposit of mud, containing marine shells, the living congeners of which inhabit the adjoining sea. The accompanying map (vol. vii. Plate I.) of the ground occupied by this deposit is a portion copied from the Ordnance map, and exhibits the high grounds bounding the valleys. I have affixed the various localities where the shells have been found, to render my account more intelligible, and to show the extent and course of the deposit. The shells have at present been found on the north side of the valley only, except at Tottenhill and Watlington; they have not been met with on the south side (the present course of the Nar) beyond Wormegay, but occupy the low ground to the north and east of the elevated patch of carstone on which Bilney Lodge stands, and are again found in the valley of the Nar at Narford.

The general level of that portion of the brick-earth in which the oyster-shells are most congregated is not much above low-water mark at Lynn; at the Bilney brick-yard they are about seventeen feet above it. Their elevation to the extreme height (about 100 feet) at which they are found at East Walton was probably effected by spring tides in conjunction with storms casting them upon the shore of the creek (presuming this valley to have been once a creek of a sea): the fractured state of the shells and the high angle of their elevation at this locality will, I conceive, justify such an inference; indeed, the equinoctial gales, which here blow with great violence from the west, and consequently *towards* Walton, would impel waves with corresponding force up this very acclivity.

We are therefore led to infer that this valley was, at a remote period, occupied by the waters of the ocean: upon examining the accompanying map, and observing the relative situations of it and the estuary called the Wash, it will be seen that the *embouchure* of the former is in the direction of the latter; and when we bear in mind that there is a process of filling up constantly in progress in all estuaries, and that our estuary, therefore, must once have extended much higher into Marshland, we cannot doubt that the valley of the Nar at a former period opened *directly* into the estuary, and that the ocean's waves flowed freely into the valley, forming an extensive creek, bounded by the high grounds of North Runcton, Middleton, and East Winch on the north; those of Walton, Westacre, and Narford on the east; and of Marham, Shouldham, and Tottenhill on the south.

I think it not at all improbable that similar deposits of mud and shells to those of the Nar and Middleton Tower may hereafter be discovered in the valleys of South Wooton and Castle Rising; indeed, on the road leading from Lynn to Hunstanton, there are many visible indications of a residence of the sea upon lands now raised to an elevation beyond the reach of the highest tides. At Dersingham Heath and at Ingoldisthorpe it is not difficult to trace at various points terraces parallel to the shore of the Wash, raised by the waves of the flowing tide, and troughs and gulleys formed by the retiring waters of ebb tide. The adjoining marshes are considerably below the level of high tide, and are protected from inundation by embankments.

It is probable that the elevation of the strata at Hunstanton Cliff (rising about fifteen yards in a mile), continued along the eastern shore of the Wash, mainly contributed to the exclusion of the *salt* water from the valley of the Nar, and that it was further reclaimed by the silting up of the upper part of the estuary, and the embankments constructed by man.

As this *brick-earth* is nowhere covered by transported materials inclosing boulders of distant strata, we must consider it to be a *post-diluvian* deposit*.

Ancient Beach.—At Hunstanton, manifestations of a great change in the relative level of the sea and the present cliff exist. I paid a visit to this interesting spot last summer, and whilst examining the greensand stratum at a part considerably beyond the point where the incumbent *red chalk* crops out (the least attractive portion of the cliff), I discovered traces of an ancient beach composed of rounded fragments of red and white chalk, immediately reposing upon the greensand, and covered by $9\frac{1}{2}$ feet of sandy loam, containing small angular fragments of flint. The weather came on so stormy and wet, which continued during my stay, that I could not then carry further my examination. At the spot I examined, the *old*

* The following are references to deposits of the same epoch: "Recent shells resting on the out-goings of the floetz strata in Clackmananshire," as stated by Mr. Bald, in Mem. Werner. Soc., vol. i. p. 403;—"Marine shells found in the line of the Ardrossan canal," by Capt. Laskey, Mem. Wern. Soc., vol. iv. part ii. p. 568;—"Marine shells of existing species on the left bank of the Mersey, and above the level of high-water mark," discovered by J. Trimmer, Esq.; *vide* Proceedings of Geol. Society of London, vol. i. p. 419;—the occurrence of similarly situated shells near Preston in Lancashire, as stated by Mr. Gilbertson, and confirmed by R. I. Murchison, Esq., who likewise observed "similar phænomena over a very considerable tract of country occupying the ancient estuary of the Ribble;" *vide* Proceedings Geol. Soc. Lond., vol. i. p. 365, 366.; [also Phil. Mag. and Annals, N.S., vol. xi. p. 366.—Edit.];—and "Description of a bed of recent marine shells near Elie on the southern coast of Fifeshire, by W. J. Hamilton, Esq., Sec. Geol. Soc., read March 11, 1835. [Lond. and Edinb. Phil. Mag., vol. vii. p. 318.] In imitation of the technical language of Mr. Lyell, the period of these deposits may be termed the *pascene*, from *πας omnis*, and *καινος recens*, all the shells being of recent species.

beach is immediately incumbent on the *breccia* of the green-sand, five feet above the level of the present beach, and rises towards the east from Lynn bay; consequently it inclines in an opposite direction to the *regular* strata. I purpose taking the earliest opportunity of prosecuting my research into this interesting relic of "olden time," to trace its course and extent, and particularly to explore it for testaceous exuviæ; at present none have been seen. Mr. E. Mugridge, at my request, has endeavoured to trace the course of the old beach, and thinks it rises to the surface at a part of the cliff which is about 40 feet high.

Alluvium of Marshland.—Marshland is part of the Bedford Level, forms the western boundary of this county, and contains about 63,000 acres of low-land. It is geologically composed of alternating beds of *lacustrine* silt and peat (covering, in the immediate vicinity of Lynn, a *marine* silt), lying upon a stiff clay inclosing small nodules of chalk, the whole reposing on the Oxford clay.

The various canals and dikes cut for inland navigation and for the drainage of the Level have exposed the beds above mentioned. The following are the *sections* I have been able to procure. At Salter's Lode, near Downham, "the silt was observed to be ten feet deep; and next below that, three feet thicknesse of firm moor; then bluish gault, which the workmen judged to have been silt originally, because being dry, it not only crumbled, like it, but had the roots of reeds in it; then below it moor of three feet thicknesse, much firmer and clearer than the other; and lastly, whitish clay, which is supposed to have been the very natural and bottome soyle at the first, before those changes happened, either from the alteration of the course of the sea, or choaking up of these outfalls"*.

In making the Eau-brink Cut near Lynn the beds were found arranged in the following manner:

1.	Vegetable soil, and brown clay with sand	4 ft.
2.	Blue clay, a brick-earth	3
3.	Peat, containing bones and horns of ruminants	2 to 2½
4.	Blue clay, similar to No. 2.	8
5.	Peat, with alder and hazel bushes; the lower portion clay, containing roots of marsh plants ...	3
6.	Dark blue clay, a marine silt, containing the following shells in great abundance: *Cardium edule, Mytilus edulis, Tellina solidula, Lutraria com-*	

* Dugdale on Embanking, &c. Edit. 1662, page 178.

pressa, and *Turbo ulvæ*; this bed was not cut through*.

In the beds Nos. 2 and 4, fluviatile shells were found: the smaller ones appear to have been overlooked by the labourers, but *Anodons* were noticed, and were found interspersed throughout both beds; in No. 2. they were abundant, forming a layer immediately upon the peat, No. 3.

The shells from No. 6. are certainly not of the same æra as those of the brick-earth of the Nar; they are evidently of a more recent date, and resemble those now existing in the river at Lynn.

At Mr. Allen's well, in the town of Lynn, similar alluvial strata to those at Eau-brink were met with, and were immediately succeeded by a bed of blue clay containing nodules of chalk, between 20 and 30 feet in thickness, which we consider to be *diluvium.*

For the section of the *alluvium* at Denver Sluice see a former part of this paper, vol. vii. p. 173. By the above sections we are informed of, and are enabled to arrange, the succession of changes to which Marshland has been exposed. Commencing with the period of the irruption of the sea, and its residence in Marshland, during which the marine exuviæ discovered in making the Eau-brink Cut were deposited, we learn next that this district became a marsh, in which alders, hazels, and marsh plants vegetated. Its next change was inundation by fresh water, forming a lake inhabited by freshwater *Testacea*, a transition probably effected by obstruction to the outlet of the rivers of the *great level*, from bars thrown up by the tidal waves in the estuary now called the Wash; this state continued till the deposition of eight feet of mud had elevated its surface, and, with the aid of other natural forces, burst its barrier; again the waters escaped, leaving, perhaps, but a solitary river to drain the interior; again aquatic plants took possession of the surface, and from the occurrence of large trees in the peat (No. 3.), forests of oak and other trees indigenous to this island sprang up, encompassed by brush-wood of hazel, alder, &c. This was the state of the fens at the period of the Roman invasion; and after the invaders had established their authority in the country, they commenced embanking this district, to protect it from the inroads of the sea †: it is said, that the Emperor Se-

* Marine silt containing similar shells occurs in Lewes Levels. (Dr. Mantell's Geology, &c.)

† Tacitus, in his life of Agricola, says, "the Britons complained that their hands and bodies were worn out and consumed by the Romans, in clearing the woods, and embanking the fens."

verus was the first to intersect the fens with causeways*. From the time that the Romans finally renounced the sovereignty of Britain, in the year 427, to the reign of Charles I., 1630, (when the drainage of the *level* was projected by, and commenced under the auspices of, the Earl of Bedford, being completed by his son in 1653,) a period of 1203 years, its cultivation was neglected, and it became a second time extensively inundated, its forests laid prostrate, and in process of time buried beneath lacustrine silt (No. 2, Eau-brink section); again the rivers flowed in natural channels †, and ultimately this morass, through the enterprise and skill of man, is reclaimed, and the Bedford Level emerges a fertile tract of country. The coin of Charles II. and the pair of scissors met with in the excavation at Denver Sluice (vol. vii. page 173.) must have been found at a spot that had been previously opened, for the bed of peat No. 4. could not have been formed at so late a period.

Many small formations of peat and deposits of silt are found on the margins of the rivulets, and in the small basins occurring on the surface of the *diluvium*; these contain the shells of existing and indigenous species of fluviatile *Testacea*. Horns and bones of a species of elk, stag, and other mammalia are found imbedded in the peat.

Submarine Forest.—We possess but little information respecting the submarine forest off the coast of Norfolk.

"A forest seems to have extended from the coast of Lincolnshire a considerable way along the Norfolk coast, as there is on the shore near Thornham, at low water, the appearance of a large forest having been at some period interred and swallowed up by the waves. Stools of numerous large timber trees, and many trunks are to be seen, but so rotten, that they may be penetrated by a spade. These lie in a black mass of vegetable fibres, consisting of decayed branches, leaves, rushes, flags, &c.‡ Also, off Hunstanton and Brancaster, at ebb tide, a bank of mud inclosing trunks and branches of

* Dugdale mentions one, "supposed to have been made by him of 24 miles in length, extending from Denver to Peterborough; this was composed of gravel about three feet in depth, and sixty feet broad; it was discovered beneath a covering of moor from three to five feet in thickness." Other works of art have also been found beneath the moor at various places in the *great level.*

† "In the year 870, the Danes (then Pagans) led by Inguar and Ubba, made an incursion into this realm, and destroyed it (the religious house at Ely): for such was the depth of the waters, which compassing this isle extended to the sea, that they had an easy access unto it by shipping."—*Dugdale*, Edit. 1772, page 181.

‡ Philosophical Transactions, No. 481, and Beauties of England and Wales, vol. xi. p. 94.

trees is seen. Mr. S. Woodward notices the above forest under the head of "*Lacustrine formations*," and says, "The ligneous deposit on Brancaster beach comes under this head, and deserves our particular notice. In this locality, trunks of trees are found abundantly imbedded in the mud; and at low water, the proprietors of the land thereabouts remove them by means of a team of horses, and convert them into posts and fences, or use them for similar purposes; the wood being quite sound, and not in the least impregnated by the soil in which they have been imbedded. With these are found the horns and bones of the deer and ox, in excellent preservation*."

Mr. R. C. Taylor, speaking of the subterranean forest, says, "Doubtless this must be the southern extremity of that submarine forest which has long engaged the notice of geologists, on the north-west part of Norfolk, whence it is traced across the Wash and the fens of Cambridgeshire to Peterborough, and all along the Lincolnshire coast, as far as the Humber. There is no important variation in the general level of this woody tract. As relates to the Norfolk portion, it appears so closely in connexion with the crag formation, as almost to form a part of it: the shells of the one being occasionally mixed with the vegetable matter of the other; and are further accompanied by bones of stags, elephants, and oxen †." Mr. Taylor writes thus of its situation near Cromer: "It is not possible to say how far inland this subterranean forest extends, but that it is not a mere external belt is obvious from the constant exposure and removal of new portions, at the base of the cliffs;" and again, "near Cromer, the trees are a few feet above the crag stratum, and are about the level of high water." He also believes it to have been antediluvian, as we learn from the following reference to Dr. Alderson's "Geological Observations on the Vicinity of Hull and Beverly ‡." "Dr. Alderson in describing the geological characters of that district (Holderness), many years ago, was of opinion that the diluvial hills were heaped upon the submarine forest. Nothing has arisen to discourage that idea; but it derives confirmation from the parallel case which is presented by the cliffs of Norfolk §."

Professor Lyell (evidently in reference to Mr. Taylor's observations) expresses himself thus: "After examining in 1829, the so-called submarine forest of Happisborough in

* Outline of the Geology of Norfolk, p. 13.
† Phil. Mag. and Annals, N.S., vol. i. p. 289.
‡ Nicholson's Philosophical Journal, 4to, vol. iii.
§ Phil. Mag. and Annals, N.S., vol. i. p. 289.

Norfolk, I found that it was nothing more than a tertiary lignite of the 'crag' period; which becomes exposed in the bed of the sea as soon as the waves sweep away the superincumbent strata of bluish clay*."

Mr. Bakewell makes the following remarks: "But these subterranean forests in England deserve more attention than they have hitherto received from geologists; the period of their growth, and the causes by which they were submerged, are at present unknown. A similar subterranean forest extends into the sea on the coast of Flanders. *Have these forests been once united, and afterwards separated by a subsidence, which formed the bed of the German Ocean* †?"

Dr. Alderson and Mr. Taylor appear to have considered these forests to have been antediluvian: I am not sure that I understand Professor Lyell on this subject; his *lignite*, I am aware, is antediluvian, but in it, does he include "large stools of trees, their stems, and branches"? If the subterranean and submarine forests of the eastern *coast* be antediluvian, the subterranean forests of *Marshland* are not contemporaneous to them, but of a more recent period, for they, with the beds of peat, are invariably found *above* what is considered *diluvial debris.*

On the date of this "once sylvan tract" I ought not to venture an opinion, for I have no personal acquaintance with it, never having had an opportunity of examining the spot; indeed, it is very difficult of access; and until it is determined upon what substratum the "mud inclosing the vegetable matter" is deposited, we cannot assign to the *submarine* forests their place in the scale of formations: still, I cannot consider this *submarine* forest (from the data connected with it already collected) to be contemporaneous with the *lignite* of the *crag* exposed in the cliff at Cromer, but believe it to be of the same epoch as the *subterranean* forest of the fens, and that its submergence was the result of the subsidence which formed the trough for the German Ocean.

Recent writers evidently consider the *subterranean* forests to be *postdiluvian*: thus, Phillips writes, "All the lacustrine deposits containing peat, which I have inspected in Holderness, agree in this general fact, that the peat does not rest immediately upon the diluvial formation beneath, but is separated from it by at least one layer of sediment, which is seldom without shells ‡."

* Principles of Geology, by C. Lyell, Esq., second edition, vol. ii. p. 273.
† Bakewell's Geology, 3rd edit. p. 513.
‡ Illustrations of the Geology of Yorkshire, by J. Phillips, Esq., p. 55. [See also Phil. Mag. and Annals, N.S., vol. ix. p. 353.—Edit.]

Again: "The extensive accumulations of peat and trees, along the shores of the Humber and its tributary rivers, happened, probably, at the same period of time as those which have contributed to fill up the ancient lakes of Holderness. This is inferred, with the highest probability of truth, from the position of the peat with respect to the diluvial clay and pebbles; for wherever these occur together, the former is invariably uppermost*."

Dr. Fleming, describing a submarine forest in the Frith of Forth, tells us the peat reposes upon a lacustrine silt; and from the tenour of his remarks he evidently considers it to belong to the "modern epoch†".

My observations on the alluvial phænomena are brief, in consideration of the great length to which my communication had already extended. For much valuable information on the subject I refer those who feel an interest in the inquiry to two essays by the Rev. Dr. Fleming, published in the Transactions of the Royal Society of Edinbugh, vol. ix. p. 419, and in the Quarterly Journal of Science, 1830, vol. vii. p. 21; and to Mr. R. C. Taylor's communication to a former volume of this Magazine, entitled "On the Natural Embankments formed against the German Ocean on the Norfolk and Suffolk Coast, and the Silting up of some of its Æstuaries‡",—papers replete with instructive matter.

Waving further speculation on the causes of the mutations to which the small area that I have examined has been subjected, I have in conclusion merely to state that in thus arranging and publishing my geological notes I have but responded to an appeal made by Dr. Fitton, from the chair of the Geological Society at the Annual General Meeting of the Fellows in 1828, in the following words: "But those who are deprived of the privilege of travelling even in England, must not suppose that they can be of no service as geologists; or if they belong to our body, that they are thus released from their obligation to be active in our cause: and there are two descriptions of persons,—the resident clergy, and members of the medical profession in the country,—to whom what I am about to say may be more particularly deserving of attention. Such persons, if they have not yet acquired a taste for natural science, can hardly conceive the interest which the face of the country in their vicinity would gain, however unpromising it may appear, by their having such inquiries before them; how much the monotony of life in a remote or thinly inhabited

* Illust. Geology of Yorkshire, p. 56.
† Quarterly Journal of Science, vol. vii. 1830, p. 21.
‡ Phil. Mag. and Annals, N.S., vol. ii. p. 295.

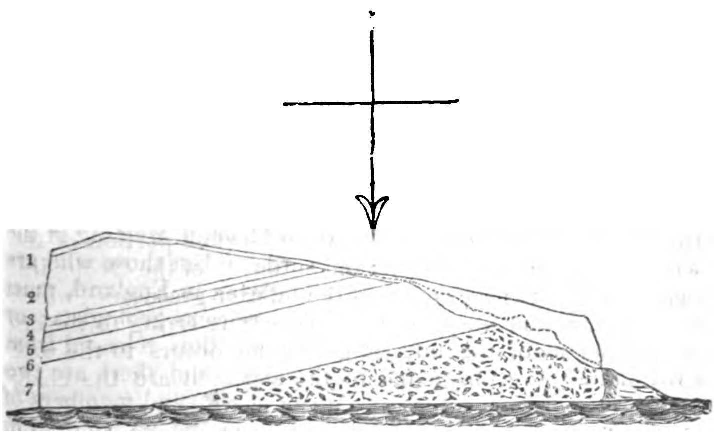
district would thus be relieved, nor how much benefit they might confer on the natural history of their country*."

Swaffham, May 5, 1835.

Explanation of the Sections and Map.

The sections do not give the relative proportions or true dip of the respective strata, but merely their order of superposition.

Section at Hunstanton Cliff.	Thickness. feet.	inches.
No. 1. Vegetable soil and diluvium.		
2. Lower chalk, Lond. and Edinb. Phil. Mag. vol. vii. page 275	28	0
3. Chalk-marl, *ib.* page 276, 2 ft. 6 in. to	3	0
4. White zoophytic bed, *ib.* page 181 ...	1	4—6
A thin seam of red argillaceous matter occurs in this place, *ib.* page 181 ...	0	2—3
5. Red zoophytic limestone in two beds, the equivalent of the gault, *ib.* page 181	3	10
6. 7. Lower greensand. Carstone, *ib.* page 175	8	9
8. Sandy breccia, *ib.* page 176	14	0



The dotted line points out the course of the *ancient* beach.

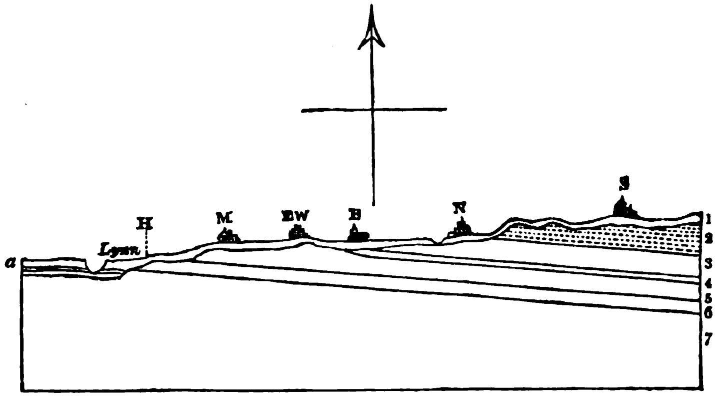
For these admeasurements I am indebted to Mr. E. Muggridge of Lynn; they were taken at the highest part of the cliff. Mr. Richard C. Taylor's statement of the greatest depth exposed of each stratum is published with his section in the Philosophical Magazine, vol. lxi. 1823.

* Proceedings of the Geological Society of London, vol. i. p. 60,—[also Phil. Mag. and Annals, N.S., vol. iii. p. 299.—Edit.]

Section from Lynn to Swaffham.

a. Alluvium.

No. 1. Vegetable soil and diluvium; 2. Chalk with flints; 3. Chalk without flints, including chalk-marl; 4. Gault;



5. Inferior greensand; 6. Kimmeridge clay; 7. Oxford clay. H. Hardwick Tollgate; M. Middleton; E. W. East Winch; B. West Bilney; N. Narborough; S. Swaffham.

Map of the Valley of the Nar, &c. (vol. vii. Plate I.)

The map exhibits the localities of the shells of the *brick-earth*; the course of the deposit, described page 33, will be readily traced upon it. The marks (◠) show the situation of the quarries of *carstone*, and the spots where the *brick-earth* is dug.

The dotted line across the map traces the course of the *gault*, and serves to correct the West Norfolk portion of the Geological Map published by the authority of the Geological Society, as regards the course of the inferior greensand, which is made to approach too near Swaffham: referring to the accompanying map, the inferior greensand occurs to the west of the dotted line only; and to the east of it are the chalk strata.

Note.—Since my paper was sent to the Editors, I have verified my anticipations (vol. vii. p. 181.) respecting the extent inland of the red chalk of Hunstanton cliff. Having expressed my opinion to Mr. Durrant of Sandringham that the red chalk extended to West Newton (the village in which the valley of *blue gault* commences), that gentleman informed me he had seen it opened in that part of his parish immediately adjoining to West Newton. I took an early opportunity of examining the spot myself, and had the satisfaction of seeing its outcrop, which lies in a direct line with the *strike* of the *blue gault*; and I collected some of its characteristic Belemnites, which were very abundant. Mr. Durrant also informed me that it occurs at Ingoldisthorpe; and Mr. E. Muggridge has recently stated to me that it has been sunk through in making a well at Dersingham Mill; therefore its course from the coast to its junction with the *blue* gault is now pretty well traced throughout.—Sept. 11, 1835.]

X. *On the Theory of Congeneric Surd Equations.* By W. G. Horner, *Esq.* (*In a Letter addressed to* T. S. Davies, *Esq., F.R.S. L. & E.*)*

[If those mathematicians who have met with a quadratic equation † whose "roots" either under a real or imaginary form could not be exhibited, will recall to memory the surprise with which they viewed the circumstance, and the attempts which they made to solve the mystery, they will read with no ordinary gratification the following discussion of the general question of which this forms a part. The general theory of such equations, very happily named by Mr. Horner "Congeneric Equations," is here laid down with great clearness, and, so far as I know, for the first time,—as it is, indeed, nearly the first time the formation of any general and philosophic views respecting them has been attempted.

The following letter was drawn up in answer to some passages in one which I had a short time previously addressed to Mr. Horner, and was a private and friendly communication; yet I have sincere pleasure in having obtained his permission to publish it in the Phil. Mag. I do so under the conviction that it will furnish the same satisfaction to others that it has done to me. I shall only add in conclusion my hope that the inquiry which, in the close of his letter, he has assigned to me, will be pursued by himself, as I know no man to whom such researches can be so safely and successfully referred.

Royal Military Academy, Nov. 15, 1835. T. S. D.]

My dear Sir,

I AGREE with you in thinking that the properties of irrational equations have not received that degree or kind of attention from writers on the elements of algebra, which was due either to the importance of the subject, or to a consideration for the comfort of young students. This appears the more extraordinary, because the methods of clearing an equation from irrational expressions, whether involving the unknown or not, have been so fully discussed, that really very little remained to be done for rendering the state of the whole case very intelligible. Waring (Med. Alg., Prob. 26.) may be cited as a case in point. But "a miss is as good as a mile." In solving equations involving radicals every one has experienced the necessity of putting his results to the proof before he could venture to decide which of them, or whether any of them, could be trusted; but as the latter alternative, or the failure of *every* result, is of rare occurrence in books of

* Communicated by Mr. Davies.

† For instance, $2x + \sqrt{x^2-7} = 5$, the "roots" of which are 4 and $\frac{8}{3}$ as determined by the common process; neither of which substituted in the equation reduces it to zero. These are the roots of its *congeneric surd equation* $2x - \sqrt{x^2-7} = 5$.

exercises, because, no doubt, the compilers had not thought the matter out, we who use their collections, being as indolent as they, have contented ourselves with the general probability of, at least, partial success. In the mean time even classical writers have spoken of clearing an equation from radicals, in order to its solution, as a process of course, and which would not in any way affect the conditions. The consequence is, that a habit prevails of talking about equations without any regard to this peculiar case, and therefore in language which when applied to it becomes quite incorrect. The term *root* of an equation passes for synonymous with any quantity which, being substituted for the unknown, satisfies the conditions; and it is affirmed, and demonstrated, that every equation has at least one root; and that, having one, it must have as many roots as there are units in the greatest index attached to the unknown. It is therefore quite startling, when we are reminded that equations may be proposed *ad libitum*, whose conditions cannot be satisfied by any quantity, positive, negative, or imaginary; that notwithstanding this, the roots obtained from such equations may be real quantities. Nor is the enigma solved by discovering that the roots obtained from one equation are sure to satisfy the conditions of another, not much unlike it: on the contrary, one is quite displeased at this kind of thimble-rig shuffling, where we were assured of finding truth, the whole truth, and nothing but the truth. A logician of the old school would settle the business by crying "*distinguo*"; but we should still reply, that it is a lame distinction which clears up only one half of the premises: we know that these are surd equations we are now speaking of, and that just before we were speaking of rational equations, or equations cleared of surds; but the difficulty remains unexplained. If he really knew a little of the subject, he would, perhaps, next try the pass-word *ambiguity*: "There is always a certain ambiguity adhering to surd expressions." When, however, the most is said that can be said to that purport, it amounts in short to this, that in the reading of formulæ, when we meet with a radical, we ought not to use the definite but the indefinite article. We have a knack of saying "*the*", where we ought to say "*a*", that is all; and if we did but read *a* square root, *a* cube root, and so on, we should be certain of finding one that would satisfy the existing conditions. This sounds plausibly, and at least ninety-nine out of every hundred of algebraists would inquire no further; but you would perhaps object, that at this rate $+\sqrt{x} = -\sqrt{x}$ might be a good equation, unless, with

Lindley Murray, we admit the "*the*" when the same quantity appears a second time under the same radical; and that without a greater latitude still, we should never be able to prove that $+\sqrt{x-a} \times -\sqrt{x+a}$ made $-\sqrt{x^2-a^2}$, and so on. So that the professed ambiguity is subject, after all, to a conventional permanence.

The source of the whole mystery, in my judgement, is to be found in the almost unavoidable imperfection of the manner in which we are taught to transform equations when we are at school. The operations consequent on *transposition* are correct as far as their principles are resolvable into Euclidian axioms. Beyond that they are liable to fallacy; and, generally speaking, we are infallible in our judgement only as long as every term is on one side. We may then determine satisfactorily in what cases zero is admissible as the aggregate value. An instance of the hazard attending the neglect of this principle is given in my paper in the Lond. and Edinb. Phil. Mag. for September 1834, (vol. v.) p. 189. In the management of surds, instances might easily be accumulated.

And whence this hazard? and why consequent upon transposition? Because, from the nature of analysis, we are continually arguing from the direct to the converse. An equation is formed *hypothetically*. We trace out certain direct consequences, in the form of equations also, and so on; until an equation is obtained, such that if the first be true, the last is therefore true. But the converse is that which we wished to ascertain. Is the *hypothetical equation true, because* the resulting equation is so? To determine this, a similar query must be instituted from link to link throughout the chain of reasoning. Is each equation in succession true, because the next in succession is so? If each of these subordinate inquiries admits of a decided affirmative, the reply to the general query is satisfactory; otherwise, it is not. Now in the management of equations, we have been taught, either virtually or in direct terms, to rely upon certain axioms, which for the present purpose will be most effectually stated in pairs, viz.

If equals be added to equals, the wholes are equal; and,
If equals be subtracted from equals, the remainders are equal. } I.

If equals be multiplied by equals, the products are equal; and
If equals be divided by equals, the quotients are equal. } II.

If equals be raised to powers denoted by equal exponents, the powers are equal; and
If of equal quantities roots be extracted, which are denoted by equal exponents, the roots are equal. } III.

It has never been my chance, either to hear the validity of any of these principles called in question, or even any caution suggested as necessary in the application of them; and yet, when tested by the combined trial of their direct and reflex action, they will presently appear to be very susceptible of misuse in incautious hands.

The first and second pair, abstractedly considered, afford such entire conviction, that in each of them, if either proposition is granted, the other can be strictly demonstrated by means of it; and the second pair are truly corollaries to the first. No hesitation, no ambiguity, is felt.

The fifth proposition, as a clear corollary to the third, is, in itself, equally satisfactory; but quite otherwise in regard to its reflex effect, as described in the sixth. For, being aware that if *unequal* quantities $(+a, -a)$ be raised to power, denoted by equal exponents, the powers *may* nevertheless be equal; we are assured that, conversely, if of equal quantities roots be extracted which are denoted by equal exponents, the roots may nevertheless be *unequal.*

This remark furnishes a sufficient reason for rejecting the third pair of principles, and consequently the ordinary method of clearing an equation from surds. For, in every instance in which this is effected by transposition and involution, in compliance with the fifth axiom, we tacitly assume that such step can be retraced with equal certainty by means of the sixth; whereas, in any such transit, the consequent equation may be quite true, and yet the antecedent be quite false.

If, however, we attribute the failure of the third pair of axioms to a special ambiguity peculiar to evolution, we shall remain under a delusion, and miss the cause and remedy of the evil. Involution is but a single instance of the erroneous application of the axioms of the third pair; but the use of any of the four unexceptionable axioms is liable to be frustrated by a similar cause, although in some cases the absurdity introduced is so palpable as to occasion a kind of instinctive unconscious avoidance. In other instances, however, even acute minds have failed to observe the fallacy. This I shall now point out, and prove that unless connected with the use of the first pair of axioms, it will be avoided, if no member of the equation is transposed to the zero side.

The origin of the fallacy in question will be rendered more evident by a course of amusing experiments upon a familiar equation, *e. g.*

$$x^4 + 2x^3 - 7x^2 - 8x + 12 = 0,$$

whose roots are 1, 2, -3, -2. Applying the four axioms in succession, we shall perceive how the incautious blending of two truths, by means of rules in themselves unexceptionable, will produce a falsehood.

$$\begin{array}{lr} \text{1st. To} & x^4 + 2x^3 - 7x^2 - 8x + 12 = 0 \\ \text{Add} & x - 1 = 0 \\ \hline & x^4 + 2x^3 - 7x^2 - 7x + 11 = 0; \end{array}$$

a false equation, with regard to all the values of x, with the single exception of 1, the value already used. Similar results would accrue from the addition or subtraction of any other divisor of the equation; the result will be false in every value, except those which are also found in the equation added or subtracted. Thus,

$$\begin{array}{lr} \text{From} & x^4 + 2x^3 - 7x^2 - 8x + 12 = 0 \\ \text{Take} & 6x^2 - 18x + 12 = 0 \\ \hline & x^4 + 2x^3 - 13x^2 + 10x = 0; \end{array}$$

whose only correct roots are those also of $x^2 - 3x + 2 = 0$.

2ndly, The given equation is resolvable into the quadratics $x^2 - 3x + 2 = 0$, and $x^2 + 5x + 6 = 0$.

$$\begin{array}{lr} \text{Therefore, multiply} & x^2 - 3x = -2 \\ \text{by} & x^2 + 5x = -6 \\ \hline & x^4 + 2x^3 - 15x^2 = 12; \end{array}$$

a statement altogether erroneous, not containing a single correct value of x.

On the other hand, divide

$$\begin{array}{lr} & x^4 + 2x^3 - 7x^2 - 8x = -12 \\ \text{by} & x^2 - 3x = -2; \end{array}$$

$$\therefore \frac{x^3 + 2x^2 - 7x - 8}{x - 3} = 6,$$

$$\therefore x^3 + 2x^2 - 13x = -10:$$

incorrect, except in respect of the roots of $x^2 - 3x + 2 = 0$.

You will clearly perceive, without dwelling upon the distinction of cases, the very simple nature and origin of the paradox. The axioms speak of quantities which are *simultaneously* equal; but no two roots of an equation, unless they be equal roots, are *coexistent*: if $x = 1$ it is not at the same time $= 2$. Consequently, as in each of the examples x in the upper of the two equations has some values, which substituted in the lower will render its sides *unequal*, the results, as far as such values of x are concerned, are no longer coincident with the conditions of the axioms on which the management of equations is founded, but are illustrations of the opposite axioms, viz. that unequals added to, or subtracted from, or multiplied or divided by, equals, produce *unequal* results, or in algebraic language, *false equations.*

The reason why this inconvenience, in the use of the second pair of axioms, cannot occur when all the terms are on one side and zero alone on the other, is very evident; although, by another of those paradoxes by which equations are beset, the complete truth appears at first sight to be the result of combining a truth with an error, and equals to result from combining equals with unequals. It is, however, easy to avoid all suspicion of error. Thus, it was said, that the given equation is resolvable into $x^2 + 3x + 2 = 0$, and $x^2 + 5x + 6 = 0$. But as these statements are not *simultaneously true*, but, on the contrary, any value of x which satisfies one of the quadratics will render the other $=$ A, some numerical quantity differing from 0, we in fact collect the

$$\begin{array}{ll} \text{product of} & x^2 - 3x + 2 = 0, \text{ or A}, \\ \text{by} & x^2 + 7x + 12 = \text{A}, \text{ or } 0, \end{array}$$

in finding $(x^2 - 3x + 2)(x^2 + 7x + 12) = 0$; where the premises being strictly correct, the result is unexceptionable. And the same result arises, although not with equally clear evidence of its truth, when A is superseded by zero.

The same test, of a hypothetical adjustment of one of the two proposed equations, would at once expose the fallacy of each of the conclusions attained in our imaginary experiment.

The general propriety of keeping the zero-side of each equation in a chain of argument clear from any transposed terms, is proved therefore by the liberty which it allows to the mind, of conceiving any zero, which happens to be *pro tempore* incorrect, to be superseded by the correct value, and of perceiving without any embarrassment or additional labour the exact conditions of the final result. But the especial propriety of adhering to this expedient, when surds are to be extricated, appears in the necessity which it imposes of attending to the *copula* of the argument, the suppression of which in the vulgar process occasions all the obscurity that is complained of. Thus, between the statements

$$\begin{array}{lll} & a = \sqrt{x} & \text{or } a - \sqrt{x} = 0 \\ \text{and} & a^2 = x & \text{or } a^2 - x = 0 \end{array}$$

the copula $a + \sqrt{x} = \mathrm{A}$ has been lost sight of. The complete chain is

$$\begin{aligned} a - \sqrt{x} &= 0 \text{ or } \mathrm{A} \\ a + \sqrt{x} &= \mathrm{A}_1 \text{ or } 0 \\ \therefore \quad a^2 - x &= 0. \end{aligned}$$

You are well aware that this copula will, in all cases of surd equations, consist of all the variations that can be made of the given formula by varying the affection of each radical it contains in all possible ways. You also can refer, more readily than myself, to various authors in whose works the method of forming the continued product of a formula and all such variations of it (for the sake of a convenient term I would venture to say, its *congeners*) has been simplified. You see, that by retaining the entire set of congeneric equations, all doubt respecting the constancy of every symbol employed, whether letter or radical sign, is entirely cleared away. Uncertainty, indeed, still remains attached to the results of the solution of the final equation; namely, uncertainty as to which of the congeneric formulæ will be reduced to zero, by the resulting values of x; but this doubt is unconnected with any perplexity respecting the general theory.

A very unnecessary ambiguity is admitted in the current acceptation of the word *root*; and great advantage would accrue from restricting it to its only legitimate signification of "such a value of the unknown in any linear divisor of the equation, as will cause that divisor to vanish."

The sum of the whole matter, respecting surd equations, I conceive to be this. We know that the continued product of a surd formula and all its congeners will produce a rational formula; and that such rational formula, being equated to zero, may be solved by as many roots as it has dimensions. We are also certain that each of these roots will cause one of the congeneric surd formulæ to vanish; otherwise the product of all would not be $= 0$ as assumed. But, is the value of x which effects this, to be called a root of the surd formula? No, it is a root of the rational combination only.—Have irrational equations, then, no roots? None at all.—What have they, then, in the place of roots? An equitable chance, in common with each formula in the congeneric society, of solution by means of the solution of the stock-equation.—But if an equation has no root, nor even a certainty of solution, in what form can it be intelligibly proposed? A note of interrogation subjoined might serve to intimate that the equation is proposed either for solution or correction.—To what order can surd equations be assigned? To the fractional order $\frac{m}{n}$, when

n congeneric formulæ produce a rational equation of the mth order; thus,

$$2x + \sqrt{x^2 - a} = b? \text{ is of the } \tfrac{2}{2} \text{ order.}$$

$$a + \sqrt{x} + \sqrt{x - b}? = 0? \quad \tfrac{1}{4} \ldots\ldots$$

$$a\sqrt[3]{a + x} - \sqrt[3]{a - x} = 0? \quad \tfrac{6}{9} \ldots\ldots$$

Are the chances of solution equal for each individual congener? I leave that question in good hands, and remain,

Yours, very affectionately,

Bath, Nov. 12, 1835. W. G. HORNER.

XI. *Letter to* Peter Barlow, *Esq., F.R.S., &c., &c., respecting some parts of his Reports addressed to the Directors and Proprietors of the London and Birmingham Railway Company. By the Rev.* DIONYSIUS LARDNER, *LL.D., F.R.S., &c.*

DEAR SIR,

IT was not until my return to London within the last few days that I had the pleasure of receiving a copy of your Second Report addressed to the Directors of the London and Birmingham Railway Company. The previous communications which had passed led me to anticipate some collision of opinion between us, but I confess I did not expect that any difference should exist on a question of a nature so elementary as that which you have noticed in your Report. In page 87 you say:

"If (as was assumed in the Parliamentary Committee on the question of the Great Western Railway) as much power was gained in the descent as was lost in the ascent, the odds would be made all even. But that assumption is altogether erroneous both in theory and practice."

And again, in page 91, you say, referring to your theory of the deflexion of bars:

"The only doubt, therefore, which can remain is, how far I ought to reject as inconsiderable any increase of power on the descending side. This point cannot be met experimentally, and I am therefore obliged to depend here only on demonstration. The case certainly involves no difficulty of conception to those acquainted with theoretical mechanics; but the question having been seen in a different light by a gentleman of considerable scientific eminence, I should have been glad to have exhibited the effect experimentally; but as the whole turns upon velocity, this is of course impossible."

To those who have taken an interest in the question respecting the effect of gradients, raised in my evidence before the Great Western Railway Committee, and subsequently more fully developed by me at the meeting of the British Association in Dublin, it will of course be evident that I am the person here alluded to. But since your Report must needs fall into the hands of many persons who have neither seen my evidence before Parliament, nor heard the discussion in Dublin, I think it right to explain briefly what the conclusions are at which I arrived, and which you declare to be erroneous both in theory and practice.

There are on railways certain inclined planes, forming so small an angle with the horizon, that a load placed upon them will not descend by its gravity, the friction being greater than the tendency down the plane by gravity. Let the angle of elevation of such a plane be ϵ, and let the greatest angle of elevation which is compatible with this conclusion be ϵ'. I shall call this angle, ϵ', the angle of repose. Now let us suppose an inclined plane at the inclination ϵ, the length of which expressed in feet is L: let a load be placed upon the plane, the amount of which we shall take as the unit of weight. Let t be the ratio of the friction to the pressure peculiar to the nature of the road, the carriages, &c. which is of course a constant quantity, so long as the carriages and the road continue the same. Now the pressure upon the plane will be expressed by $\cos \epsilon$; but as ϵ must be a very small angle, we may, without sensible error, take $\cos \epsilon = 1$, and consider the whole weight as pressing upon the inclined plane. In fact, ϵ' is an angle so small that its sine does not exceed 0·004, and ϵ being still smaller, it is clear that $\cos \epsilon$ is so nearly equal to the unit that we are justified in this assumption.

To determine the tractive force which must be applied to the load to draw it up the inclined plane, it is only necessary to add together the forces necessary to overcome the friction and the gravity: now the friction is t, and the gravity $\sin \epsilon$; therefore the force which resists the motion up the plane will be $t + \sin \epsilon$. The moving power, therefore, which will keep the load moving up the plane at a uniform speed will exert a pull upon it which shall be expressed by $t + \sin \epsilon$. The unit being the weight of the load, it is clear that the total expenditure of mechanical power in drawing the load the entire length of the plane will be expressed by $L\,(t + \sin \epsilon)$.

Now to estimate the mechanical force necessary to draw the load at a uniform speed down the plane, we have only to consider that the force which is opposed to the drawing power is the friction t, diminished by that component of the weight of the load which is directed down the plane, and which of course conspires with the drawing power. The effective resistance, therefore, to the drawing power will be $t \sin \varepsilon$, referred to the weight of the load as the unit. The total expenditure of mechanical force, therefore, necessary to cause the required descent will be L $(t - \sin \varepsilon)$. Now if we wish to determine the whole mechanical power expended in ascending and descending the plane, it is only necessary to add L $(t + \sin \varepsilon)$ to L $(t - \sin \varepsilon)$: the sum is $2 \mathrm{L} t$. Now it is obvious that this would be the amount of mechanical force expended in drawing the same load backwards and forwards on the level plane of the length L.

If the angle of inclination of the plane were the angle of repose, then the tendency of the weight down the plane by its gravity would be precisely equal to the friction or $\sin \varepsilon' = t$: there would in fact be no resistance to the motion down the plane, and consequently any velocity imparted to the load down the plane would be continued uniform without any drawing power to the bottom, supposing of course the plane to be free from the inequalities which would alter the amount of friction.

To ascend such a plane, on the other hand, would require a drawing force of twice the amount necessary for a level, since $t + \sin \varepsilon' = 2t$; and we accordingly arrive at the same conclusion,—that in ascending and descending planes whose inclinations do not exceed ε', the total expenditure of mechanical power is the same as on the level, the only difference being that on the level it is expended by one continued uniform exertion, and that on the inclinations it is greater in the ascent and less in the descent, the mean being the amount upon the level.

I explained fully, both before Parliament and at the British Association, that this reasoning would not extend to greater elevations than ε', for that in these cases the power saved in the descent would be less than the excess expended in the ascent, and that, consequently, such gradients would always occasion a loss of mechanical power.

Now really this conclusion is so plain a result of first principles that I have been utterly at a loss to discover in what can originate our difference of opinion about it. It struck me, therefore, that this discrepancy must have its origin, not in the above reasoning, but in some difference of conception respecting the very foundations of mechanical science. It occurred to me, therefore, to look over your Reports, to see whether the same difference as to first principles would lead you to conclusions upon other points different from those at which I should have arrived: in this I was not disappointed, for I found in another case, in which the force of gravity is considered, and indeed one which is the extreme case of an inclined plane, viz. that of a perpendicular fall, you have arrived at a conclusion which certainly is totally at variance with the views which I have been accustomed to take of the theory of forces. In your First Report to the Directors of the London and Birmingham Railway Company, page 90, in speaking of the effect produced by the wheels of wagons passing over the joints of rails where one has sunk below the level of the other, so as to form a sort of slip, you say:

"It has perhaps never occurred to such persons that a difference of level at a joint-chair will, when the carriage is moving from the higher to the lower level at its greatest speed, cause the wheel to pass the distance of a foot without pressing on the rail, and consequently throwing the whole weight, which ought to be borne equally by the two rails, wholly upon one. Yet this is a fact resting upon a natural law, and cannot be otherwise. To fall $\frac{1}{10}$th of an inch by the action of gravity requires $\frac{1}{44}$th part of a second, and in that time the carriage will have advanced a foot, and consequently in that space the whole weight has been borne by one rail only."

I freely confess that I am one of the persons you allude to, to whom such an idea never would have occurred. I am entirely ignorant of the natural law to which you allude, but I am not ignorant of a natural law which is altogether incompatible with your conclusion; and my conviction that the whole weight cannot press on the remaining wheel is quite as clear and strong as yours is that it will so press. It is quite true that the force of gravity will cause a body to fall freely $\frac{1}{10}$th of an inch in $\frac{1}{44}$th part of a second, but when the force of gravity is thus employed it cannot at the same time cause the whole weight of the same body to press upon a fixed point. The fact is, that when the wheel passes from the higher to the lower level, the centre of gravity being unsupported falls, and the only pressure exerted upon the remaining wheel depends on the moment of inertia of the load in receiving an incipient angular motion, and it is evident that this pressure must be extremely slight; but, whatever be its amount, it is totally different both as to effect and cause from that which you allude to. If you consider that during the moment of a perpendicular fall, in the case of one rail being below the level of the other, the weight while it falls still presses with its whole force upon the rails, your view even of the most elementary principles of this part of mechanics is so essentially different from mine, that the wonder is, not that we should differ in one instance, but that we should agree in any.

Referring again to your second Report, where you have noticed my inferences respecting gradients, I find that you say that,

"As the question wholly turns upon velocity, it is of course impossible to exhibit the effect experimentally."

Now although I do not perceive this to be at all a matter of course, but on the contrary have found it very easy to reduce questions depending on velocity to experiment, yet I beg to observe that the present question does not depend either wholly or at all upon velocity. Whatever be the speed of the load upon the inclined plane, provided only it be maintained uniform, my theory of gradients (if it deserve to be so called) will still hold good. It will take the same expenditure of mechanical force to move a load on such inclined planes as I have described, and the mean between the ascending and descending forces will be the force along a level plane. You must surely be so well acquainted with the laws of friction that it is needless for me to remind you that that resistance is altogether independent of the velocity. And I would also beg to observe that the case is one totally distinct from the consideration of accelerating forces. In page 91 you say:

"This point cannot be met experimentally, and I am therefore obliged here to depend only on demonstration. The case certainly involves no difficulty of conception to those acquainted with theoretical mechanics, &c."

I admit that it does not; but I apprehend the conception which those acquainted with theoretical mechanics form of it will be altogether different from that at which you appear to have arrived, and I therefore regret that you seem to have forgotten your expressed intention of giving a demonstration of your own peculiar view of the matter. In the next page (92) you mention the intention as one which you *had*, but seem to have immediately abandoned it.

It will be very gratifying to me, and I am sure it will be useful to all who are practically engaged in those extensive enterprises for the formation of lines of communication through the country, if you will show how these views of mine are at variance with the established principles of mechanics. Although I am not aware that any one has hitherto pointed out the property which I have explained in reference to inclined planes of less inclination than the angle of repose, yet, so far as I am informed, there is no difference of opinion whatever as to the legitimacy of the method of estimating the tractive force both in ascending and descending these planes. The same formulæ that I have used, viz. L $(t \pm \sin \epsilon)$, have been in substance universally adopted in estimating the mechanical force necessary to work railroads. You will find that many eminent engineers, although they have not thrown the principle into the language of analysis, have nevertheless used it arithmetically; and indeed I have never before heard any doubt expressed about it.

During the last autumn I have been engaged in an extensive course of experiments on rail-roads in different parts of the kingdom, with a view to determine with greater precision than has been hitherto attained, the values of the different constant quantities which enter into their theory. The results of all these experiments are in the most perfect accordance with the principle you have called in question.

I remain, dear Sir, yours very truly,

DION. LARDNER.

36, Cambridge Terrace, Edgeware Road,
December 14, 1835.

XII. *Remarks on a supposed new Law of Magnetic Action. By the Rev.* WILLIAM RITCHIE, *LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution of Great Britain and in the University of London.**

IN the last Number of the London and Edinburgh Philosophical Magazine†, Mr. Fox has endeavoured to show that the mutual attraction of two magnets does not follow the law formerly adopted by all philosophers, viz. the law of the inverse *square* of the distance; but the law of the simple inverse of the distance. This law he deduces from experiments on the attraction of the opposite ends or poles of magnets placed at very small distances from each other. Thus, for example, when the ends of the magnets are at the distance of $\frac{1}{2000}$ of an inch, he found the effect to be only *one half* of what it was when they were in contact; when removed to the distance of $\frac{1}{1000}$ of an inch the effect was *one half of one half*, or *one fourth*; when separated by a distance of $\frac{1}{500}$ of an inch, the force was only *one half of one fourth*, or *one eighth*, &c.; which numbers are to each other in the inverse ratio of the distances

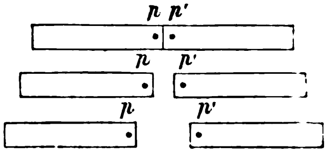
I *admit* the truth of the experiments, but differ from Mr. Fox in the conclusion he has drawn from them. To show that the deduction is unfounded, we must first describe what is meant by the *pole* of a magnet, and its *position* with regard to the extremity of the magnet. The pole of a magnet is the *centre of parallel forces* of all the attractions and repulsions of the elementary magnets of which it is composed. Now the position of this *centre* will obviously depend on the *form* of

* Communicated by the Author.   † Vol. vii. p. 439.

the magnet, and also on its *length*. Biot has shown that in a steel wire 24 inches long, and properly magnetized, the pole is *an inch and a half* from its extremity, and that this distance diminishes with every diminution in the length of the magnet *. The centre of parallel forces or the pole of a magnet is similar to the centre of gravity of a body. In the one case the effect is the same as if all the matter of which the body is composed were concentrated in the centre of gravity, in the other the effect is the same as if the *difference* between the sum of all the attractive and repulsive forces were concentrated in the pole. Now, in the case of the mutual attraction of bodies, our measurements are always taken between the *centres of gravity*; in the case of magnetic attractions the distances of the magnets are, in fact, the distances between the poles.

Let the distance of the poles of the magnets when in *apparent* contact be called 2, as in fig. (1.), and then separated by an interval of 1, as in fig. (2.), and by intervals of 2. as in fig. (3.)

(1.) (2.) (3.)



Then the distances between the centres of force in these three positions are 2, 3, 4. Hence if the law of the *inverse squares* of the distances, investigated by Coulomb, be the real law of action, the attractive forces will be inversely as $2^2, 3^2, 4^2$, that is, as $\frac{1}{4}, \frac{1}{9}, \frac{1}{16}$; but $\frac{1}{9}$ is nearly the half of one fourth, and $\frac{1}{16}$ nearly the *half* of $\frac{1}{9}$, as Mr. Fox found by actual experiment. These experiments then, instead of leading to a *new law* of action, afford a beautiful illustration of that law which universally prevails whenever we have matter acting on matter by *attractive* or *repulsive* forces.

XIII. *Description of a Thermometer for determining minute Differences of Temperature.* By Marshall Hall, *M.D., F.R.S. &c.* †

In pursuit of the theory of the *inverse ratio of the respiration and of the irritability* in the animal kingdom, announced in a late volume of the Philosophical Transactions‡, I have found it absolutely necessary to determine the minute

* Biot, *Traité de Physique*, tom. iii. p. 90.

† Communicated by the Author.

‡ An abstract of Dr. Marshall Hall's paper on this subject will be found in Phil. Mag. and Annals, N. S. vol. xi. p. 453.—Edit.

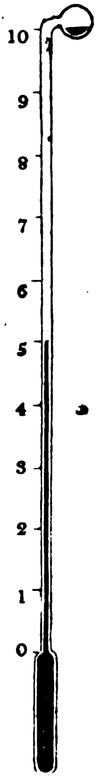
differences of temperature which exist in animals of the same class. In pursuing this inquiry, I soon discovered that it was essential to devise other instruments than those in ordinary use.

It was easy by enlarging the bulb and by selecting a tube of extremely fine calibre, to render the common thermometer capable of more minute indications. But it was impossible to carry this change beyond a certain degree, the augmented length of the instrument becoming highly inconvenient.

In order to obviate this difficulty, I devised the instrument which I am now about to describe.

The form of this instrument is represented in the accompanying outline. The relative size of the bulb and calibre of the tube is such that the tenth part of a degree occupies a considerable space upon the scale. The entire scale consists of ten degrees. At the upper part of the thermometric tube a small bulb is blown, which I shall designate the *reservoir*; it is turned forwards so as to remain at a right angle with the tube.

The bulb and the tube are filled with mercury, and a little of that fluid is included in the reservoir, when the whole is hermetically sealed.



When an experiment is to be made, the mercury in the tube is to be brought into contact with the mercury in the reservoir, by placing the instrument horizontally, with the reservoir upwards, in water of a sufficient temperature.

I will now suppose that I wish to try the comparative temperature of the swallow which shuns, and the sparrow which abides, the rigours of our winter. The thermometer is removed from the water at the temperature of 110° Fahr., and placed upright. The contiguity of the mercury in the tube with the mercury in the reservoir being broken, the highest point in the scale will represent that degree, viz. 110°. The lowest will consequently be the 100th degree. The entire scale is one of six degrees between these extremes, each degree being divided into *tenths*.

The same plan is adopted for any other part of the scale.

We have thus an instrument of the usual size, capable of measuring the tenths of a degree of temperature, at any part of the scale. It only requires the addition of a common thermometer to afford the extreme limit of the magnified scale.

I may be permitted to add, that the temperature of an animal indicated by such a thermometer compared with that of the medium in which it is placed, affords a near approximation to the degree of respiration, and, inversely, of the irritability of the muscular fibre.

LXV. *Proceedings of Learned Societies.*

OFFICIAL REPORT OF THE PROCEEDINGS OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, AT THE DUBLIN MEETING, AUGUST 1835.

Communicated by the Council and Secretaries.

[Continued from vol. vii. p. 513.]

Notices and Abstracts of Miscellaneous Communications to the Sections, continued.

MEDICAL SCIENCE,—*continued.*

Experimental Inquiry into the different Offices of Lacteals, Lymphatics, and Veins in the Function of Absorption. By P. D. HANDYSIDE, *M.D.*

THE author's general position is thus stated: "The lacteals, lymphatics, and veins are endowed each with a peculiar office in the general functions of absorption; for example, 1. The *lacteals* are those vessels which absorb the aliment which is necessary for maintaining the nutrition and increase of the body, and exercise the property of refusing entrance to all other matters; 2. The *lymphatics* absorb the elements of the body upon their becoming useless or noxious, so as by their final discharge from the system to make room for the deposition of new matter, and these vessels possess no absorbing power over any substances foreign to the system; 3. The *veins* not only return to the heart the blood after that fluid has fulfilled the object of its diffusion over the system, but enjoy the office of receiving into the animal system by absorption various *foreign* matters which may be brought into contact with their orifices.

In support of these views the author presents a short review of results obtained by various eminent anatomists and physiologists.

The following is the order of the subjects discussed:

Lacteals.—Their distention after a full meal,—their condition as observed in living animals;—effects of ligatures on the thoracic ducts of horses.

Lymphatics,—Anatomical origin of,—analogy of lymphatics and lacteals,—exact resemblance of the lymph prior to its absorption to that found in the lymphatic vessels,—absence of lymphatics in vegetables,—no proof afforded by examination of lymph that lymphatics serve as the channel through which foreign matters gain entrance into the system,—no communication between lymphatics and veins except through the great lymphatic trunks.

Veins.—Analogy between the anatomy and disposition of the veins of animals and the vessels corresponding to these in plants, favours the doctrine of *venous absorption*.

"When foreign matters capable of affecting the constitution generally, and of being diluted in its solids and fluids, are brought into contact with the *serous and mucous surfaces* of the body, with the *cutis vera*, and with the interstitial cellular tissue of different organs, the resulting phænomena exhibited by the blood in the veins give evidence that these vessels are the sole agents employed in this variety of absorption." These four points are discussed by reference to a variety of experiments, to which the author adds the following from his own researches, as bearing on the question of absorption of *foreign* matters by veins of the *cellular tissue*.

Exp. 1. Having made a fistulous opening in the abdominal parietes of a *dog*, he took advantage of the period when a complete granulating surface should be formed, to apply to it very freely the solution of pruss. potass. On killing the animal three minutes after the application, and applying the appropriate chemical test to the blood, *it* was seen to exhibit traces of the prussiate.

Exp. 2. He induced the formation of a granulating surface four inches square in extent in the fleshy substance of the back of a large *cat*, and then retained pledgets of lint moistened with 1 ℥ of the usual solution of the prussiate of potash in contact with this surface during the space of four hours. A fair indication of the presence of the poison in the blood was seen, on submitting to the usual test the blood from the carotid arteries, both in its fluid and coagulated states, while no indication whatever of its presence was observed in the lymph.

These experiments now put forth as evidence in favour of the doctrine of absorption by the veins of *foreign* matters, from the *interstitial cellular tissue* of the animal body, when taken along with the previous experiments also adduced to prove the absorption of *foreign* matters from the surface of the *cutis vera* and the different *mucous* and *serous* superficies, would appear to justify a *conclusion*—that the absorption of *foreign* matters occurring from the interstices and surfaces of the body occurs solely through the channel of the venous system.

Observations on the effects of Cold on different parts of the Human Body, and on a mode of measuring Refrigeration. By Dr. Osborne.

In this communication Dr. Osborne began by adducing some facts to show the importance of cold, viewed as a cause of disease. He stated, that of 57, the entire number of patients on the preceding day (13th August, 1835,) in Sir Patrick Dun's Clinical Hospital, 34 could distinctly refer to cold as the cause of their complaints, contracted in the following manner: in 12 from damp clothes, 5 from damp feet, 3 from bathing, and 14 from cold air when heated. This proportion, however, would probably be very different in winter. The direct effect of cold on the air-passages of the lungs is almost restricted to inflammation at the rima of the glottis, and this is usually caused by suddenly rushing from heated to cold air. It may be proved that the respired air, being of nearly the same temperature as the blood, and not deriving its heat from the action of respiration in the lung (see Brodie's Experiments), must, in its passage downwards, be heated to considerably more than half the difference between the temperature of the blood and that of the air; that, consequently, at its arrival in the air-vesicles of the lungs, it must have acquired such a temperature as amounts to a protection against the effects of cold. Dr. Osborne considers this as a provision of nature in a matter in which we are not able to guard ourselves.

When, owing to an oppression of nervous energy, the healthy temperature of the surface is not maintained, then the air arrives at the air-vesicles without being heated; hence, he conceives, may be explained the numerous instances of sudden death which occur in chronic bronchitis and low fevers when sudden depressions of the temperature of the atmosphere have taken place during the night. In those cases the cold thus admitted to the lungs causes a torpor in their capillary circulation; and after death it is found that the blood has stagnated in the lungs, and in the veins and right cavities of the heart.

The common opinion that various inflammatory diseases are contracted by sleeping in newly-built houses appears to be ill founded, except in as far as the clothes worn by the individual may contract moisture. The air under the bedclothes being kept up by the heat of the body to the temperature 80°, the only way in which the damp air can prove injurious is by the lungs, which, as before stated, are, in health, enabled to resist its effects. It appears that in a regiment which was quartered in newly-built barracks no injury resulted from the damp.

On the stomach the effect of cold is perceived, not by a sensation of cold in that organ, but by thirst, in consequence of reaction, as is experienced after taking ices. When the cold is long-continued or overpowering, in consequence of feeble reaction, then gastritis is produced from torpor of the capillaries. This last mode of explanation is derived from the phænomena observed in the exterior of the body on the application of cold. When the application is transient and the circulation vigorous, the contraction of the vessels and paleness of the surface are only momentary, and are succeeded by reaction evinced in increased heat and diffused blush of redness. When it is long continued, then the pale and shrunk state of the surface is gradually succeeded by a purple or livid colour, attended with increase of size, as may be proved by a ring on the finger, from the swollen state of the vessels. Comparing these facts with the experiments detailed by Dr. Alison,—which showed that in inflamed parts not only the small vessels but the large arterial trunks leading to the part are dilated, and rendered incapable of contracting like other arteries,—Dr. Osborne proposes the question, whether there is not sufficient evidence to prove that cold produces inflammation by producing torpor and dilatation of the vessels, either of the part itself or of some connected or adjacent part, which, if not removed by transient reaction, is followed by the more permanent reaction of inflammation, causing a number of new phænomena.

With regard to the effect of cold on the skin, which is the most important of all, it is evident that meteorology has contributed very little to our knowledge of the influences of the atmosphere on health or disease. It has appeared to the Author, that in order to connect this science with utility, as far as mankind is concerned, one consideration has been omitted, which is, *the cooling power of the atmosphere estimated with reference to ourselves.* The human body has a heat of nearly 98°, and is placed in a medium always cooler than itself. The degree of cooling influence exerted on it has never been made the subject of measurement, and to the present time is estimated solely by the feelings. In order to measure the cooling influences of the air or other media, Dr. Osborne used a spirit thermometer, without a frame, carefully graduated from the degree 90 to 80 inclusive, that being nearly the temperature of the exterior of the body. Having heated the bulb to 90°, he exposed it in different situations, observing the time during which the spirit descended from 90° to 80°; and adopting, as a measure of the refrigerating power, the rate of cooling deduced. And by this contrivance is exhibited the result of radiation, and of the conducting power of the atmosphere as modified by its temperature, its density, its moisture, and its currents; and that result, the most interesting of all to the invalid, who, in respect to temperature, may be conceived as represented by the instrument. As the variety in the shape of the bulb, the bore of the tube, the thickness of the glass, or the density and quantity of the fluid employed will cause variety in the time of the descent, the result obtained with two thermometers must not be expected exactly to correspond. In order to procure uniformity for this purpose, it will be necessary to place a number of them, previously graduated between 90° and 80° and heated to 90°, in air at 60° or 50°, and to select those which contract according to the time fixed on as a standard. The thermometer so applied, Dr. Osborne proposes to call a psychometer, or measurer of refrigeration.

Amongst the observations brought forward by him to illustrate its use are the following:

To show the refrigerating effect of agitation or of a breeze, the temperature of the air remaining the same.

In air, temp. 70° at rest, it cooled from 90° to 80° in 5^{m} 20^{s}.
——————— in a slight breeze.................... in 2^{m} 50^{s}.
——————— blown on with a bellows .... in 58^{s}.

These observations show the fallacy of determining climate by the thermometer. There are situations in which, owing to constant currents of air, a cold is produced of the utmost consequence to health, but not appreciable by the thermometer. Dr. Osborne expects that by means of this mode of observation much light may be thrown on the climates of the western coast of Africa, and of other unhealthy localities. The meteorological tables at present kept in those places fail in showing the effect of the sea and land breezes.

The following shows the refrigerating power of water above air of the same temperature, at rest, to be above 14 to 1.

In air at rest, temperature 70°, it cooled from 90° to 80° in 5^{m} 40^{s}.
In water at rest, same temperature......................... in 24^{s}.

It is well known that in swimming it is not the fatigue so much as the refrigeration which fixes the limit. This appears from the following observation compared with the preceding.

The instrument agitated in water, cooled from 90° to 80° in 15^{s}.

In order to ascertain the refrigeration produced by damp clothes, Dr. Osborne covered the bulb of the instrument with cotton wool, and having placed it at rest in an apartment at 68½°, found it to cool from 90° to 80° in 10^{m} 14^{s}. Placing it in the same circumstances, but with the cotton wool slightly damped, it cooled down in 2^{m} 57^{s}. This proportion must be much increased when under the influence of the open air. The application of cotton wool to the skin, moistened with water or an evaporating lotion, he has found the most eligible means of cooling the surface in disease, not only on account of the constancy with which the refrigeration is maintained, but from its being peculiarly agreeable to the feelings of the patient.

On the Influence of the Artificial Rarefaction or Diminution of Atmospheric Pressure in some Diseases, and the Effects of its Condensation or increased Elasticity in others. By Sir JAMES MURRAY.

The paper was divided into two parts. The first detailed the general principles of the *rarefaction* of air, and its powers as a remedial agent on the human body. The second part related to the local agency of condensation of air in topical diseases.

The propositions were submitted, not as remedial means of themselves alone, but as auxiliary to those already in use. It was shown, That the ordinary atmospheric pressure sustained by the whole body averages 15 tons;—that by placing a person in an air-tight bath, with provision for breathing the ordinary atmosphere, half a ton or a ton can be removed without danger:

That the abstraction of this elastic compression permits the easier expansion of the chest, elicits the blood and animal heat to the surface of the body, opens the pores of the skin, and restores to the surface rashes or eruptions which had been suppressed.

It was therefore submitted, that an agent capable of producing such effects is entitled to consideration in treating certain conditions of pectoral diseases; in eliciting internal congestions or inflammations from central organs to the surface; in preventing certain fevers, and other complaints arising from obstructions of the cutaneous functions; in translating gout and rheumatism from vital organs to the limbs; in restoring a due balance of the circulation, and attracting the blood into the superficial veins from the deep-seated arteries.

A case of a patient was detailed, in which congestion of the brain was diverted from the head by inclosing one of the lower extremities in a rarefying bath, and abstracting about two pounds and a half of pressure from each inch of the surface: the influx of the fluids was so great, that in two hours the circumference of the limb was increased nearly three inches, the vessels of the skin rendered red, warm, and turgid, and the head relieved.

The case of a painter was also adduced, whose right arm had long been paralysed and cold from the effects of lead paint. The arm was put for two hours into the rarefying case, and afterwards continued hot and vigorous, so that the man was able to resume his work.

Part second.—As diseases of an opposite nature require opposite remedies, the principle of *rarefaction* is *reversed* in certain cases, and *condensation*, or additional pressure, employed.

This part of the paper detailed several cases illustrative of the powers of this agent. Where there was too much vascularity of parts, then local pressure, pumped under an air-tight covering, emptied the vessels, propelling onwards the overflow of blood contained in the veins, and preventing its undue influx by the arteries.

The consequences were, to diminish inflammations, dissipate tumours and white swellings, facilitate the reduction of hernia and other protrusions, and to diminish the influx of fluids into indurated breasts or enlarged glands.

The author adduced a very interesting case, the reduction of a *prolapsus ani* by atmospheric pressure, without touching or bruising the sensitive intestine.

The powers of condensation of air were then alluded to, for the treatment of fungous sores or ulcers, and for the suppression of uterine hæmorrhages, as well as bleeding from wounds or lacerations *.

On the Differential Pulse. By Dr. M'Donnell.

Dr. M'Donnell's paper began with a description of what he terms "the Differential Pulse," and with proofs of his claim to priority in ascertaining it in 1784. The observations which succeed related to the following subjects.

The influence of disease and of particular remedies upon the pulse, with a reference to the effect of posture on the number of beats; the absence of this phænomenon in quadrupeds, owing to their natural vessels being horizontal in both the lying and standing posture; certain cases of health and disease, in which the maximum and minimum of this variation are found; the methods to be pursued for investigating the number of the pulse in wild and ferocious animals as deducible from their respirations; the proportion between the stops, pulses, and respirations in man and quadrupeds in active exercise; observations made at a depth of 26 feet in a diving-bell, which corroborate the views of Sir David Barry and Dr. Carson on the moving powers in the circulation; proofs that barometrical variations have no influence upon the pulse or breathings.

Part 2.—On the limitations of the doctrine of the "Differential

* In vol. xiv. of the Philosophical Magazine, First Series, p. 293, was published a paper on Smith's Air-Pump Vapour-Bath, an instrument which was designed to effect, by the same means, many of the objects contemplated by Sir J. Murray.—Edit.

Pulse"; of stationary or permanent pulses; observations made on the pulses of children before and after their having respired; of the acceleration of the pulse after birth; observations on quadrupeds with respect to this; supposition that the fœtus remains before birth in the state of the cold-blooded animals; of the final cause of this peculiarity; of the cause of the stethoscopic sounds of the fœtal heart being very rapid, although the pulse in the funis be slow; an account of an experiment made by a watch ticking under water; of the remarkable strength of the fœtal pulse as felt in the chord; of the absorption of the blood in the chord into the system of the fœtus after delivery; and the inference from this in favour of the views of Sir David Barry and Dr. Carson respecting the suction power of the thorax as influencing the circulation.

On some hitherto unobserved Differences in the Effects of Accumulations of Liquids or of Air within the Cavity of the Thorax. By Dr. WILLIAM STOKES.

On Aneurism by Anastomosis. By R. ADAMS, *A.M., Member of the Royal College of Surgeons.*

Abstract of a Case of deficient Development of the right Hemisphere of the Brain, with Congenital Malformation of the Hip and Wrist Joints, and Atrophy of the Members of the same Side. By Dr. HUTTON.

Description of a Case of Deformity of the Pelvis, in which the Cæsarean Operation was successfully performed. By G. B. KNOWLES, *M.R.C.S., F.L.S., Lecturer on Botany at the Birmingham School of Medicine.*

Propositions concerning Typhus Fever, deduced from numerous Observations. By Dr. PERRY.

On the Use of Chloride of Soda in Fever. By ROB. J. GRAVES, *M.D.*

Dr. Graves commenced a series of clinical experiments in 1832 upon the efficacy of chloride of soda in petechial and maculated fever. He has exhibited this medium at Sir Patrick Dun's Hospital and at the Meath Hospital, where its effects have been witnessed by a great number of physicians as well as pupils. The form recommended is Labarraque's solution, which is a saturated solution of chloride of soda. This was given in doses of from fifteen to twenty drops in an ounce of camphor mixture every fourth hour. In the commencement of fever, where there is great heat of skin and signs of vascular excitement, its employment is contraindicated. It is also inadmissible in cases where there is decided evidence of visceral inflammation. When the early stage of fever is past, when all general and local indications have been fulfilled, when there is no complication with local disease, when the patient lies sunk and prostrated, when restlessness, low delirium, and more or less derangement of sensibility is present, when the pulse is quick, when the body is covered with maculæ or petechiæ, and the secretions from the skin and mucous membranes give evident proof of what has been termed a putrescent state of the fluids, it is then that the chloride of soda may be prescribed with advantage. It operates, although not rapidly, yet energetically, in arresting many of those symptoms which create most alarm. It seems to counteract the tendency to tympanitis, to correct the fœtor of the excretions, to prevent collapse, to promote a return to a healthy state of the secretions of the skin, bowels, and kidneys; in fact, it appears admirably calculated to meet the bad effects of low putrid fever. Its employment does not preclude the use of wine or other approved remedies. Dr. Graves has used it in several hundred cases of typhus, and strongly recommends its employment in that disease.

Original Views of the Functions and Diseases of the Intestinal Canal, &c. By Dr. O'BEIRNE.

On Purulent Ophthalmia. By Dr. EVORY KENNEDY.

Dr. Evory Kennedy gave a report of numerous cases of purulent ophthalmia of infants, in which leeching, constant removal of the purulent secretion, and caustic applications, modified according to the violence of the attack, and, in aggravated cases, the solid nitrate of silver, applied to the interior of the lids, had proved most successful.

A notice of the curved Drill Catheter, invented by Mr. FRANCIS L'ESTRANGE, was presented to the meeting.

Mr. HAWKINS exhibited to the Section specimens of Harrington's patent Electrizer.

Abstract of Registry kept in the Lying-in Hospital of Dublin. By ROBERT COLLINS, *M.D., late Master of that Institution.**

MECHANICAL SCIENCES APPLIED TO THE ARTS.

On Impact and Collision. By EATON HODGKINSON.

Mr. Hodgkinson reported to the Section the results of certain experiments made by him on impact and collision, in continuation of those communicated to the Association in the year 1834 on the collision of imperfectly elastic bodies. The results were,

First, That cast-iron beams being impinged upon by certain heavy masses or balls of metal of different kinds, were deflected through the same distance, whatever were the metals used, provided that the weights of the masses were equal.

Secondly, That the impinging masses rebounded after the stroke through the same distances, whatever was the metal of which they were composed, provided that the weights were the same.

Thirdly, That the effect of the masses of different metals impinging upon an iron beam were entirely independent of their elasticities, and were the same as they would give if the impinging masses were inelastic.

Mr. Hodgkinson also gave the result of some interesting experiments on the fracture of wires under different states of tension, from

* Of this and some other communications made to the Medical Section, the titles only of which are given in our report, abstracts will be found in the copies of the Proceedings of the Association, separately printed for the use of the members.—EDIT.

which it appeared that the wire best resisted fracture and impact when it was under the tension of a weight which, being added to that impinging upon it equalled one third of the force that was necessary to break it.

On the Solid of least Resistance. By J. S. Russell.

Mr. Russell was called upon to give an account of a new form for the construction of ships, by which they should experience least resistance from the water in their passage through it. A vessel of 75 feet keel and 6 feet beam had been built on this new formation, and made the subject of very accurate experiments, from which it appeared that this vessel, named the "Wave", experienced much less resistance in passing through the water than vessels of the very finest formation and from the best builders on the old construction.

Mr. Russell then detailed very minutely the mode of forming any vessel on his plan when the length and breadth were given. The peculiarity, in general terms, appears to be the formation of the entrance lines from parabolic arcs, so as to have a point of inflection at about one sixth part from the bow of the vessel, before which the bow is concave externally, giving the finest possible entrance at the stern, at an angle of contact infinitely small, and behind which the convexity is external and the formation elliptical to the midship section, after which the formation becomes wholly ellipsoidal. Mr. Russell had been induced to consider this solid as the solid of *least* resistance from a phænomenon that appeared to distinguish this form from all others, namely, that it entered the water at the highest velocities without breaking in the slightest degree the evenness of its surface; that, while at high velocities all other formations dashed the water into spray or raised it in waves above the surface, this vessel, at velocities of 16 or 18 miles an hour, appeared to give no motion to any particles of water, excepting such as happened to lie in its path. He considered the entrance into smooth water without ruffling the surface as the criterion of minimum resistance.

Mr. Russell observed, that the form had been constructed on a hypothetical view of the subject, viz. that the minimum force requisite to alter the position of any fluid particle would be that which gave to the particle a uniformly accelerated velocity through the former half of its path, and a uniformly retarded velocity during the remainder; that the well-known relation of the coordinates of the parabola accomplished this in the manner formerly explained, but that he rested for the proof of the correctness of the theory upon the experiments he had already adduced.

Mr. Russell then described a very simple mode of construction, by which the ordinates of a circle or a table of sines might be used so as, in the most elementary mechanical manner, to form a very close approximation to the solid of least resistance; and he concluded by drawing the lines of a vessel of given dimensions according to the new formation of least resistance.

On certain points in the Theory of the Construction of Railroads. By the Rev. Dr. Lardner *and* C. Vignolles.

On the Monthly Reports of the Duty of Steam-engines employed in draining the Mines of Cornwall. By JOHN TAYLOR, *F.R.S., Treasurer of the British Association.*

Mr. Taylor observed that he had found at this and other Meetings of the Association considerable interest to be expressed with regard to this method of recording the actual effect produced by the consumption of a given quantity of fuel, and recommended it to the notice of engineers in general. The monthly reports alluded to gave the means of comparing one engine with another in this district; they also afforded an historical view of the progress of improvement in this important machine; and they had, Mr. Taylor believed, contributed largely to that improvement, by the emulation and attention excited by them in the persons who had the charge of constructing and managing the engines.

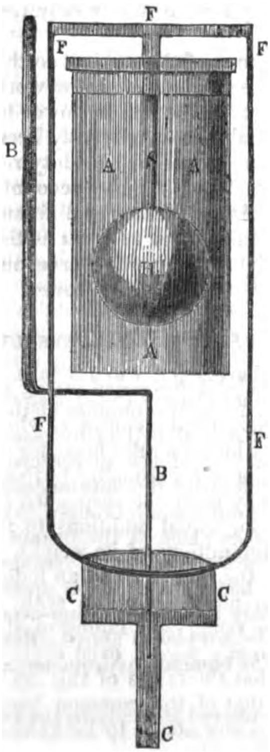
Mr. Taylor stated that the work done in the best engines now employed in Cornwall by the consumption of one bushel of coal, required ten or twelve years ago the consumption of two bushels; that during the period of Boulton and Watt's patent four bushels were consumed to do the same work, and that in the earlier stages of the employment of steam power the quantity of coal used was 16 bushels. So that by the progressive advance of improvement one bushel had become sufficient for the duty that formerly required sixteen.

Mr. Taylor, in remarking on the importance of this subject to the deep mines of Cornwall, stated, that the steam-engines now at work for the purpose of draining the mines there were equal in power to at least 44,000 horses, and that as some doubts had frequently been expressed as to the accuracy of the results shown by the duty reports, he had compared them some time since with the accounts of the coal actually used in some of the principal mines at different periods, by which he found the saving of money was as great as the reports indicated, and that their general accuracy was borne out fully by the account books, where this was incontestably proved.

Description of a Self-registering Barometer &c. By Prof. STEVELLY.

During the oscillations of the common barometer, when it falls, a certain quantity of mercury is added to that already in the cistern, which of course adds so much to its weight; on the contrary, when it rises, mercury retires from the cistern, which thereby becomes so much lighter than before. If, then, the tube of a barometer be fixed firmly in its place, but the cistern be by any means so suspended as to move downwards by equal distances for equal additions to its weight, and to rise similarly for similar diminutions of its weight, it is clear that a scale may be placed beside the cistern; and an index carried by the cistern may be made to mark upon the scale a variety of positions corresponding to the rising and falling of the common barometer. It may be shown to any person even slightly conversant with mathematical subjects, that the range of this scale may be made to bear any proportion to that of the common barometer. Supposing for an instant what is now stated to be accomplished, it is obvious that a pencil may be so attached to the cistern as to rise and fall with it, and thus to mark on a properly ruled sheet of paper, carried by clockwork across the instrument, the indications of the barometer at the successive hours of the day; and thus a curve representing the actual diurnal oscillations of the barometer can be placed before the eye, and a registry kept from day to day on separate sheets of paper. The mean curve can also be had by making the pencil traverse, day after day, for a long period, the same sheet of paper; for the pencil-marks will at length become blackest and heaviest upon the parts corresponding to the mean curve; and thus all the labour of actual observation, registry, &c. will be avoided, and thus, too, much of the trouble of reduction, if not all, will be saved.

Many mechanical methods of suspending the cistern will readily suggest themselves to persons conversant with practical matters; but the method that is preferred by the author is by a mercurial hydrometer, the cistern, for the sake of stability, being suspended underneath the hydrometer, as in Ronchetti's modification of Nicholson's hydrometer. The accompanying drawing will give an idea of the form of the instrument; the following is the description of it. The guide-wheels and supports are omitted.



B the barometer tube (it may be of iron) firmly fixed in its place, and dipping below into

C, the cistern, which is suspended by

F, a frame, supported by

S, the pillar or stem of

H, the hydrometer ball, which floats in

A, a vessel firmly fixed, and containing the mercury (or other fluid) in which the hydrometer floats.

In the description of this instrument given to the Subsection, it was supposed that the surfaces of the mercury in the cistern and in the vessel A were so large that the rising or falling of the fluid in these vessels might be neglected; also, since the instrument is very sensible, it was supposed that the lower part of the barometer tube which dips into the cistern should be rendered very small, in order to diminish unsteady oscillation. Also the internal part of the barometer tube B at the upper part, the external part where it dips into the mercury in the cistern, as well as the cistern and the vessel A, at the surfaces of the mercury in them, were all supposed to be cylindrical. And it was then shown in a popular manner, that if the internal cross section of the barometer tube at its upper part were made equal to the cross section of the pillar or stem of the hydrometer, the sensibility of the instrument would be too great for practice; the scale in that case would be lengthened out indefinitely, since the hydrometer could never sink sufficiently to attain a position of equilibrium upon a fall of the barometer, and *vice versâ*. But if the cross section of the stem or pillar of the hydrometer be made twice as great as the internal cross section of the upper part of the barometer, the rising and falling of the cistern would be exactly equal to the rising and falling of the common barometer; and therefore the scale of this instrument would then be equal to the scale of the common barometer; and between these limits any desired scale, however long, may be obtained. A scale shorter than that of the common barometer may also be had by increasing the cross section of the stem of the hydrometer beyond the above limit; but this is not likely to be ever desired. When it is desirable to save expense, the hydrometer may be made to float in water; but of course its dimensions will require to be much greater in that case: or the cistern may be counterpoised, and a cylinder like the stem of the hydrometer, dipping into the mercury, may, by its varying buoyancy, be made to restore the equilibrium.

The exact mathematical formula which gives the relation of the scale to that of the common barometer, whatever be the dimensions of the parts of the instrument, is of the form $\delta h = \delta h' \times C$, where δh is the variation of the height of the common barometer, $\delta h'$ is the corresponding part of the scale of this instrument, and C a constant depending for its value upon the dimensions of the several parts of the instrument.

Professor Stevelly also described a very simple and cheap instrument for weighing hydrometrically, the sensibility of which is very remarkable,—a hydrometer-ball with a stem of steel wire, having upon it one or two dots of gold, and a scale-pan attached to it, either above as in Nicholson's, or below as in Ronchetti's modification of the hydrometer. An index, or a microscope with a horizontal wire, is attached to the side or cover of the vessel in which the hydrometer floats in such a way that it may be steadily and slowly raised or lowered to mark the position of the gold-dot, instead of taking the indications from the surface of the fluid, as in the common method. The weight of the substance to be weighed is then had by placing it in the scale-pan, bringing the index or wire of the microscope to mark the position of the gold-dot, then removing the substance and substituting for it known weights until the dot is again brought to the same position. Since the adjustment takes place at the instant of using the instrument, it becomes almost incapable of being deranged, and thus a very correct balance may be had by a common apothecary's phial, with a little mercury to steady it, and a knitting-needle pushed down through its cork, and a scale-pan placed above. Every person knows the difficulty of adjusting the common hydrometer, and its liability to derangement.

The same principle may be readily conceived to apply to the construction of a self-registering rain-gauge.

On Vibration of Railways. By Capt. DENHAM, *R.N.*

Capt. Denham ascertained that the vibrating effects of a passing laden railroad train in the open air extended laterally on the same level 1110 feet, (the substratum of the positions being the same,) whilst the vibration was quite exhausted at 100 feet when tested vertically from a tunnel.

The tunnel was through a stratum of sandstone rock: the rails laid in the open air on a substratum of 12 feet of marsh over sandstone rock. The method of testing was by mercury reflecting objects to a sextant. The experiments were made in the neighbourhood of Liverpool.

Mr. ANDREW PRITCHARD exhibited examples of various kinds of apparatus constructed by him for illustrating the Polarization of Light; and gave a brief account of his improved achromatic microscope, one of which was placed upon the table.

The construction of a simple polariscope invented by Mr. Pritchard was explained. The crystals to be examined were mounted in slides and introduced between tourmalines, by which means sections of any crystals that present themselves may be examined, and the cell of the upper tourmaline being removeable can be employed for other experiments. A lens was attached for condensing artificial light.

The mechanical part of the achromatic microscope produced was constructed on the principles recently published by Dr. Goring and Mr. Pritchard in their works on the microscope: the chief feature in the optical part was the execution of a set of object-glasses which admitted a pencil of light of *sixty-eight* degrees, free from spherical and chromatic aberration, having the oblique pencils nearly correct and the field of view moderately flat. Mr. Pritchard stated expressly of this instrument, that it was the simplest that had yet been constructed that would accomplish all the work that might be required of a microscope, either for general examination, dissection, or minute investigation.

Preparations of various classes of microscopic objects in Canada balsam were exhibited.

Mr. HAWKINS explained the principle of Saxton's locomotive Differential Pulley; and a mode of producing rapid and uninterrupted travelling by means of a succession of such pulleys set in motion by steam-engines or by horses.

Mr. CHEVERTON read a paper on Mechanical Sculpture, or the production of busts and other works of art by machinery, and illustrated the subject by specimens of busts and a statue in ivory, which were laid on the table. This machine, in common with many others, produces its results only through the medium of a model to govern its movements; but it has this peculiarity, that the copy which it makes of the original is of a size reduced in any proportion, and that it is enabled to effect this result not merely on surfaces, such as bas reliefs, but in the round figures, such as busts and statues.

Mr. Ettrick gave an account of certain improvements proposed by him in the Astronomical Clock for giving the pendulum a free motion at right angles to the line of its motion, and thereby preventing the tendency to acquire a circular motion by any improper adjustment of the pendulum-spring.

He described a mariner's steering-compass provided with two adjustments, whereby the card was made to point *true* bearings on the horizon; the variation and local attraction being allowed for by regulating the position of the needle on the card.

He also read an account of certain improvements on steam-engines, for making available the power of the steam of high-pressure boilers, which is below the pressure of the atmosphere, by allowing the high-pressure steam to pass off into the atmosphere, and allowing the steam of low-pressure to pass into a condenser through a secondary slide. He gave a description of a method of securing the seams of boilers by longitudinal instead of the present circular clenches; and of a machine for drilling boiler plates as rapidly as they can be punched by the punching machine.

Mr. Roberts exhibited a machine which renders objects visible while revolving 200,000 times a minute.

If a firebrand be whirled, in the dark, round a centre in a plane perpendicular to the eye of the spectator, it will present the appearance of a luminous circle. From this fact it has been inferred, that the impression on the retina made by the luminous body in its passage through every point of the circle, remains until the body has completed a revolution. How rapidly soever the firebrand may be made to revolve, the circle, and, therefore, every part of it, will be distinctly visible: hence a probability arises, that at the greatest attainable velocity, a perfect impression of the object in motion will still be produced on the optic nerve, provided that the time of viewing such object be limited to that which is required for passing through a small space—small, at least, with reference to the size of the revolving body—and also that no other object be presented on the field of vision before the former spectrum shall have vanished from the eye; unless in the case of the same object under similar circumstances. The former of these conditions is provided for in machine, No. 1, in which the eye-hole is made to travel through 180 feet between every two inspections of the moving object, and which object is made to assume a different position at each successive inspection. The latter condition is included in machine No. 2; the object is there presented to the eye in one position only.

GEOLOGICAL SOCIETY.

Nov. 7, 1835.—The Society assembled this evening for the Session.

A paper was first read, entitled "A notice on the Fossil Beaks of four extinct species of Fishes, referrible to the genus Chimæra, which occur in the oolitic and cretaceous formations of England," by the Rev. William Buckland, D.D., F.G.S., &c. This paper has been given at length in the present Number, at p. 4.

A paper was next read, "On the recent discovery of Fossil Fishes (*Palæoniscus catopterus*, Agassiz,) in the new red sandstone of Tyrone, Ireland;" by Roderick Impey Murchison, Esq., V.P.G.S.

A small specimen of new red sandstone, presenting the first impressions of fishes found in this formation in the British Isles, having been exhibited before the Geological Section of the British Association at the late meeting in Dublin, Mr. Murchison, in company with Prof. Sedgwick, Lord Cole, and Mr. Griffith, visited the spot where it had been obtained.

The quarry is at Rhone Hill, in the parish of Killyman, about three miles east of Dungannon. The new red sandstone in which it is excavated is a prolongation of the deposit which occupies large tracts in the county of Antrim, and extends into this part of Tyrone, where it surrounds a small, slightly productive coal-field, but reposes for the greater part upon mountain limestone. The eastern flank of the district is covered by a vast thickness of clay, containing lignite, the exact age of which is not known; and the surface generally is very much overlaid by loose detritus, consisting of sand and gravel, derived from the adjacent formations. Large blocks of syenite and greenstone, referrible to a northern origin (Antrim), are scattered here and there.

The beds of new red sandstone exposed in the quarry dip about 15° to the N.N.E., and consist, in the upper part, of red and green marls, passing down into a dark red, thickly bedded, siliceous sandstone, with a few irregular, highly micaceous way-boards of a deep purple colour. The surface of some of the beds exhibits ripple-marks. The quarry, which is the property of Mr. Greer, is from 25 to 30 feet deep, and the fishes are found only in the bottom beds, but are in great abundance *.

Dr. Agassiz afterwards gave a systematic enumeration of the fossil fishes which he has found in English collections. He commenced by detailing the general results of his researches, from which it appears, that the discovery in England of three hundred new species has corroborated the laws of development which he had previously determined in the succession of these animals during the different changes which our globe has undergone, with the exception of the discovery in the chalk of two species belonging to two genera which he had before observed only in the oolitic series, and of a species of one of those genera in the lower tertiary strata.

The secondary systems (*terrains*) of England are the richest in fossil fishes; and Dr. Agassiz stated that the number of specimens which he has seen in English collections is astonishing. The species which he has determined are about 400; but the specimens too imperfect to be described, at present, announce the existence of a still greater number.

Their geological distribution presents the following details:

In the *Silurian system* of Mr. Murchison there are five or six species which exhibit the first appearance and organization of this long

* A slab, presented to the Geological Society by Mr. Greer, exhibits, on a surface not exceeding two feet square, above 250 fishes.

series of vertebral animals, the species of which become more and more numerous, and more and more diversified, as well in their forms as in the details of their organization.

The *old red sandstone*, including the Caithness schist and the Gamrie deposit, contains twenty species.

In the *coal measures* there are fifty-four species; in the *magnesian limestone* sixteen.

The *oolitic series* is particularly rich in ichthyolites, the number of species from the lias to the Wealden inclusive being one hundred and fifty.

The *greensand and chalk* are also very rich in fossil fishes, and even much richer than their equivalents on the Continent. The number of English species is fifty.

In the *London clay* the species perfectly determined are about fifty, but it is certain, from the fragments preserved in different collections, that this formation incloses the remains of a much greater number. M. Agassiz stated that the London clay, particularly in Sheppey, will be, for a long time, an inexhaustible mine.

The *crag* contains five or six species peculiar to it, and belonging to genera which do not inhabit our northern seas.

As an example of what remains to be done in the study of fossil fishes, and of the importance of these researches to zoology and geology, M. Agassiz afterwards described two singular genera found in the lias. One is the animal which has been described under the name of Squalo-raia, discovered at Lyme Regis; the other a new genus, called by M. Agassiz *Gyrostris mirabilis*, and is probably the largest known fish. This fossil was discovered at Whitby; but there have hitherto been found only some detached bones of the head, of the branchial arcs, and some portions of vertebræ and fins: traces of the same fish have been recently observed at Lyme Regis.

Nov. 18.—A letter was first read from Dr. Pingel of Copenhagen to the President, containing a notice of some facts showing the gradual sinking of part of the west coast of Greenland.

The first observations which led to the supposition that the west coast of Greenland had subsided, were made by Arctander between 1777 and 1779. He noticed, in the firth called Igalliko (lat. 60° 43′ N.), that a small, low, rocky island, about a gun-shot from the shore, was almost entirely submerged at spring tides, yet there were on it the walls of a house 52 feet in length, 30 feet in breadth, 5 feet thick, and 6 feet high. Half a century later, when Dr. Pingel visited the island, the whole of it was so far submerged that the ruins alone rose above the water.

The colony of Julianahaab was founded at the mouth of the same firth in 1776; and near a rock, called the Castle by the Danish colonists, are the foundations of their storehouse, which are now dry only at very low water.

The neighbourhood of the colony of Frederickehaab (lat. 62° N.), was once inhabited by Greenlanders; but the only vestige of their dwelling is a heap of stones, over which the firth flows at high water.

Near the well-known glacier which separates the district of Frederickehaab from that of Fiskenäss, is a group of islands called Fulluartalik, now deserted; but on the shore are the ruins of winter dwellings, which are often overflowed.

Half a mile to the west of the village of Fiskenäss (lat. 63° 4′ N.), the Moravians founded, in 1758, the establishment called Lichtenfeld. In thirty or forty years they were obliged once, perhaps twice, or move the poles upon which they set their large boats, called Umiak, or Women's boats. The old poles still remain as silent witnesses, but beneath the water.

To the north-east of the mother colony, Godthaab (lat. 64° 10′ N.), is a point called Vildmansnäss by St. Egede, the venerable apostle of the Greenlanders. In his time, 1721—1736, it was inhabited by several Greenland families, whose winter dwelling remains desolate and in ruins, the firth flowing into the house at high tide. Dr. Pingel says that no aboriginal Greenlander builds his house so near the water's edge.

The points mentioned above the writer of the letter had visited; but he adds, on the authority of a countryman of his own highly deserving of credit, that at Napparsok, 10 Danish miles (45 miles English) to the north of Nÿ-Sukkertop (lat. 65° 20′ N.), the ruins of ancient Greenland winter houses are to be seen at low water.

Dr. Pingel is not aware of any instance of subsidence in the more northern districts; but he suspects that the phenomenon reaches at least as far as Disco Bay, or nearly to 69° north lat.

Some notes by Capt. Fitzroy, R.N., read at a Court Martial at Portsmouth, Oct. 19th, 1835, on Capt. Seymour and his Officers for the loss of His Majesty's Frigate Challenger, wrecked on the coast of Chili, near the port of Conception, and communicated to the President by Capt. Beaufort, R.N., Hon Mem. G.S., were then read.

These notes refer to the effects produced by the earthquake of Feb. 1835, in the currents on the coasts of Chili, from the Island of Mocha to the parallel of Conception. Capt. Fitzroy also mentions that the island of Santa Maria was elevated ten feet.

A letter dated Valparaiso, 22nd of March 1835, from R. E. Alison, Esq., addressed to the President, on the earthquake of Chili of the 20th of February 1835, was then read.

The earthquake began at quarter past 11 A.M. by a gentle heaving or undulation of the earth; but the motion increased in a few seconds to so great a degree that no person could stand. It destroyed the cities of Conception and Chillan, with the ports of Talcahuano and Maule, as well as above twenty smaller towns, and an immense number of country houses. It was felt to the southward as far as the Indian territory opposite the island of Chiloe; to the northward beyond Copiapo; at Mendoza on the east of the Andes; by the crew of a ship 100 miles to the westward of the coast, and at Juan Fernandez 300 miles from it.

At the port of Talcahuano the same phenomena occurred which accompanied the destruction of Penco in 1730 and 1751. Forty minutes after the first shock the sea suddenly retired so far that part of the bottom of the Boca chica, the smaller or southern entrance of the bay, could be seen; but the sea afterwards returning through the same channel with a great bore, flowed 20 feet over the town, carrying everything before it. This phenomenon was repeated three times. Mr. Alison says that the sea was reported to have receded, or rather the land to have risen, 2 or 3 feet, a difference having also taken place in the soundings in the bay; and that a rock, which was invisible previously to the earthquake, was afterwards near the surface.

Large fissures are stated to have been made in the earth, and water to have burst from some of them: the earth is also said to have opened and closed; and near Los Angeles several hills to have disappeared, and others to have opened and vomited steam and black smoke. The harbour of the island of Santa Maria was destroyed, and the sea retired between 300 and 400 yards, while the reefs which surrounded the greater part of the island are said to have entirely disappeared.

At the island of Juan Fernandez phenomena occurred similar to those which accompanied the destruction of Talcahuano. About a league from the shore the sea appeared to boil, a high column of water was thrown into the air, when the sea retired so far that a number of old anchors and brass guns became visible; but it soon returned with great violence, carrying off all the houses of the convicts. A volcano also burst forth at the point where the sea was first agitated. The brig Glanmalin was in the latitude of Talcahuano, and about 100 miles to the westward of it, at the time of the earthquake, when the crew felt a shock as if the vessel had struck upon a rock.

Mr. Alison also mentions the existence near Valparaiso of the recent marine shells 1400 feet above the level of the sea, and of recent marine shells being dug near Conuco for the purpose of making lime. In the bay of Valparaiso, he says, a rock which in 1817 could be passed over in a boat, is now dry, except at spring tides.

LINNÆAN SOCIETY.

Nov. 17.—A notice, by Mr. White was read, of an individual of the Great Black Woodpecker (*Picus martius* of Linnæus,) having been shot in 1834 at Billingford, near Scole Inn, Norfolk. The stuffed specimen is in the possession of Mr. Drake, a farmer of that place. The bird was shot in a moist natural wood, where the *Rhamnus Frangula* and *Viburnum Opulus* abound. Another of the same species was seen at the same time.

The conclusion of Mr. Don's "Descriptions of Indian *Gentianeæ*" were then read.

Among the numerous families which compose the class of Dicotyledonous plants, there is perhaps none so equally and generally distributed over the surface of the globe as the *Gentianeæ*, extending almost to the extremities of both hemispheres, and occurring in every intermediate region wherever the elevation of the land and other local circumstances favour their development. By a comparison of the Floras of different countries they appear to constitute the proportion of about 1 to 83 of the phænogamous vegetation. By the indefatigable researches of Dr. Wallich and Mr. Royle, the number of species of this order belonging to the Indian Flora has been more than doubled, and they now amount to about 50. Of the 13 genera into which they have been distributed, *Carscora*, *Exacum*, *Slevogtia*, *Crawfurdia*, *Agathotes*, and *Ophelia* are exclusively Indian, and the remaining 7 are common to the European and Northern Asiatic Floras. Of these 50 species, 33 belong to the Alpine Flora, which, in 3500, the number at which the phænogamous plants of the Flora of Northern India may be estimated, will give a proportion equal to that above stated. The author has confined himself in this paper to the description of the species found by Mr. Royle. We subjoin the new genera and species:

Gen. 1. GENTIANA. *Borck. Brown.*

1. *G. contorta*, annua; floribus solitariis, corollâ infundibuliformi 5-lobâ: lobis lineari-oblongis obtusis æstivatione convolutis, dentibus calycinis lanceolatis acuminatis, foliis ellipticis obtusis 5-nerviis subsessilibus.

Gen. 2. PNEUMONANTHE. *Schmidt.*

1. *P. Kurroo*, caulescens, subuniflora; dentibus calycinis elongatis subulatis, corollâ campanulatâ: lobis acutis, foliis obtusis; radicalibus elongato-lanceolatis; caulinis linearibus.
2. *P. depressa*, subacaulis, cæspitosa, uniflora; dentibus calycinis ovato-lanceolatis mucronatis, corollâ campanulatâ: lobis integerrimis aristatis, foliis lanceolatis mucronatis margine scabris; surculinis obovatis.

Gen. 3. ERICALA. *Renealm.*

1. *E. capitata*, caulescens, simplex; foliis ovatis, floribus aggregatis, dentibus calycinis ovatis mucronatis recurvis, corollæ lobis ovatis obtusis: sinubus crenatis.
2. *E. argentea*, acaulis; foliis calycibusque lanceolatis mucronatis conduplicatis recurvis margine scariosis, floribus fasciculatis, corollæ lobis ovatis acuminatis.
3. *E. marginata*, caulescens, ramosa; foliis lanceolatis mucronulatis planis margine cartilagineis, floribus fasciculatis, dentibus calycinis ovato-lanceolatis mucronatis erectis, corollæ lobis obtusis: sinubus acutis.
4. *E. decemfida*, caulescens, ramosa; dentibus calycinis subulatis mucronatis rectis, corollæ lobis lanceolatis acuminatis: sinubus bidentatis, foliis radicalibus ovatis mucronatis maximis; summis subulatis.
5. *E. pedicellata*, caulescens, ramosissima; dentibus calycinis lanceolatis mucronatis revolutis, corollæ lobis ovatis acuminatis: sinubus integris, foliis lanceolatis acuminatis, capsulâ longè stipitatâ.
6. *E. canaliculata*, caulescens, erecta, ramosa; segmentis calycinis cuneatis mucronatis, corollæ lobis ovatis acutiusculis, foliis ovato-lanceolatis obtusis margine scabris.

Gen. 4. EURYTHALIA. *Renealm.*

1. *E. coronata*, breviter caulescens; floribus aggregatis, corollâ 10-lobâ: sinubus lobis subæqualibus ovatis uniformibus, foliis lanceolatis acutis margine cartilagineis.
2. *E. pedunculata*, caulescens, ramosa, diffusa; pedunculis elongatis filiformibus unifloris, corollâ 5-fidâ calyce ter longiore, laciniis calycinis ovatis obtusiusculis.
3. *E. carinata*, caulescens, simplex; foliis lanceolatis mucronatis carinatis, floribus fasciculatis, corollâ 10-lobâ: lobis lanceolatis acuminatis; sinuum duplò brevioribus argutè denticulatis.

Gen. 5. CRAWFURDIA. *Wall.*

1. *C. fasciculata.*
2. *C. speciosa.*

Gen. 6. Swertia. *L.*

1. *S. speciosa*, foliis oppositis connato-vaginantibus elliptico-oblongis acuminatis 7-nerviis, floribus racemoso-paniculatis, corollæ segmentis acuminatis: glandulis connatis.
2. *S. petiolata*, foliis oppositis petiolatis oblongis obtusis 5-nerviis, floribus racemoso-paniculatis, corollæ segmentis obtusis: glandulis distinctis filamentoso-ciliatis.
3. *S. alternifolia*, foliis alternis! elliptico-oblongis acuminatis 7-nerviis basi vaginantibus, floribus racemoso-paniculatis, corollæ segmentis ellipticis obtusis: glandulis orbiculatis contiguis.
4. *S. cuneata*, foliis oppositis petiolatis spathulato-oblongis obtusis 5-nerviis, floribus racemosis, corollæ segmentis obtusis: glandulis lineari-oblongis subremotis filamentoso-ciliatis.
5. *S. cærulea*, floribus subsolitariis, corollæ segmentis ovatis mucronulatis: glandulis linearibus distantibus, foliis inferioribus spathulatis petiolatis; superioribus calycibusque lanceolatis obtusiusculis.

Gen. 7. Agathotes.

1. *A. Chirayta*, caule tereti, foliis ovato-lanceolatis, foveis nectariferis oblongis distinctis: squamulis margine capillaceo-fimbriatis.
2. *A. alata*, caule tetragono alato, foliis ovatis, foveâ nectariferâ orbiculatâ: squamulâ rotundatâ fimbriatâ.

Gen. 8. Ophelia.

1. *O. angustifolia*, floribus 4-fidis, foliis petiolatis lineari-lanceolatis acutis, segmentis calycinis linearibus mucronulatis, corollæ laciniis ovatis acuminatis calyce brevioribus.
2. *O. pulchella*, floribus 4-fidis, foliis lanceolato-linearibus acutis, segmentis calycinis lanceolatis acuminatis, corollæ laciniis ovatis mucronulatis calyce longioribus, caule tetragono.
3. *O. paniculata*, floribus 5-fidis, foliis linearibus scabris margine revolutis, petiolis ciliatis, segmentis calycinis lanceolatis acuminatis, corollæ laciniis ovato-lanceolatis acuminatis calyce vix longioribus, caule tereti.
4. *O. purpurascens*, floribus 5-fidis, foliis lanceolatis acuminatis 3-nerviis scabris, petiolis ciliatis, segmentis calycinis lanceolatis mucronatis, corollæ laciniis ovato-lanceolatis acuminatis basi bituberculatis calyce longioribus, filamentis basi connatis, caule teretiusculo.
5. *O. cordata*, floribus 5-fidis, foliis sessilibus cordatis acutis 5-nerviis, segmentis calycinis ovato-lanceolatis acuminatis, corollæ laciniis oblongis obtusiusculis calyce brevioribus.
6. *O. lurida*, floribus 4-fidis, foliis superioribus cordatis acutis amplexicaulibus, segmentis calycinis lineari-lanceolatis mucronulatis, corollæ laciniis ovatis acuminatis calyce longioribus.

Gen. 9. Halenia. *Borck.*

1. *H. elliptica*, corollis campanulatis 4-fidis calcaribus filiformibus brevioribus, laciniis calycinis obtusis abbreviatis, foliis ellipticis obtusis 5-nerviis; inferioribus petiolatis.

Gen. 10. Erythræa. *Renealm.*

1. *E. Roxburghii*, floribus pedunculatis corymbosis, corollæ laciniis lanceolatis: tubo calycis longitudine, foliis superioribus linearibus 3-nerviis, caule quadrangulo.

Gen. 11. Canscora. *Lam. Brown.*

1. *C. diffusa.*
2. *C. decussata.*
3. *C. pusilla.*

Gen. 12. Exacum. *L. Brown.*

1. *E. pedunculatum.*
2. *E. tetragonum.*

Gen. 13. Slevogtia. *Reichenb.*

1. *S. verticillata.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

At the anniversary meeting on Friday, November 6th, 1835, the following officers were elected for the ensuing year:

Dr. Clark, Trinity College, President;—Professor Cumming, Trinity College; Professor Sedgwick, Trinity College; Dr. F. Thackeray, Emanuel College, Vice-Presidents;—Rev. G. Peacock, Trinity College, Treasurer:—Rev. Professor Henslow, St. John's College; Rev. W. Whewell, Trinity College; Rev. J. Lodge, Magdalen College, Secretaries;—W. Hopkins, Esq., St. Peter's College; Rev. J. Hymers, St. John's College; Dr. Haviland, St. John's College; Rev. J. J. Smith, Caius College; Rev. S. Earnshaw, St. John's College, Old Council;—Rev. L. Jenyns, St. John's College; Rev. R. Murphy, Caius College; Rev. A. Thurtell, Caius College; C. C. Babington, Esq., St. John's College; Rev. H. Philpott, Catherine Hall, New Council.

November 16.—After various presents of books and objects of natural history had been announced, a Memoir was read by the Rev. R. Murphy, "On the Resolution of Equations of Finite Differences."

Extracts were then read of letters from Sir J. Herschel to the Rev. W. Whewell, containing various meteorological observations, and especially some tending to show that the height of the barometer at the equator is less by about a quarter of an inch than it is at twenty or thirty degrees from it.

The following are a portion of the extracts here referred to:

"The barometer certainly has a permanently and very decidedly lower mean level at and near the line. The strong upward current due to the circulation of the trades can alone account for this. Of the general fact I have no doubt, and however difficult it is to observe the barometer on shipboard, from the unusual quietness of our passage, I think I can come pretty near to its true difference from that in our latitudes. The depression at the equator below that in lat. 20° may I think be stated at $0^{in}\cdot2$ nearly.

"These are the results of a series of barometrical observations, made at my request by Sir E. Ryan, in his voyage to Calcutta from this place. The barometer is reduced to 32° F., and *to the Royal Society's Standard,* by careful comparison with my Troughton's barometer.

Limits of the Zone of Latitude N. and S.	No. of Obs.	Mean observed Pressure.	Mean observed Latitude corresponding.
		in.	
Lat. 5° N. to 5° S., the equatorial zone ..	7	29·821	0° 41′
Lat. 5° to 15°, mean of N. and S. zones ..	10	29·849	9 50
Zones 15° to 25° lat...	8	30·030	19 12
Zones 25 to 35	10	30·125	31 0
Zones 35 to 40	24	29·934	38 25

"The following set of observations (made with no care, and with a bad barometer,) by Mr. MacHardy, surgeon of the Mountstewart Elphinstone, in its last voyage homewards, at my request, also confirm the equatorial depression:

Zone.	No of Obs.	Mean observed Pressure.	Mean observed Latitude.
		in.	
Lat. 0° S. to 5° S. ..	8	29·821	1° 42′
5 S. to 15 S. ..	5	29·802	9 20
15 to 25	6	29·960	19 41
25 to 35	16	30·085	31 20

"Not having Mr. MacHardy's zero, I have made his equatorial result correspond to Sir E. Ryan's, by addition of a constant (+ 0·188). Sir E. Ryan's depression is greater; Mr. MacHardy's about the same which I first assigned ($\frac{1}{4}$ inch) from my own observations in my voyage out."

Extracts were also read of letters from C. Darwin, Esq., of Christ College, to Professor Henslow, containing an account of the geological phænomena of some parts of the Andes.

Nov. 30.—Various presents were announced, and a paper by Professor Wallace of Edinburgh was read, containing geometrical theorems and formulæ, particularly applicable to some geodetical problems.

Afterwards Professor Airy stated his views and the results of his observations with reference to the supposed analysis of the solar spectrum into red, yellow, and blue, by Sir David Brewster, which supposition he conceived to be unfounded. Professor Airy, Mr. Rothman, Mr. Peacock, and Mr. Power then gave an account of their observations of the Aurora Borealis of Nov. 18th.

Dec. 14.—A communication by Mr. Potter, of Queen's College, was read, containing an Explanation of the Rainbow on the doctrine of Interference, and referring especially to the Supernumerary Bows often seen within vivid displays of the rainbow, near its summit. It was shown that such additional bands would accompany the primary bow if the drops were of approximately equal size, and that the circumstances usually observed would be accounted for by supposing the rain-drops of the diameter of 1-72 of an inch. The considerations account for the supernumerary bows being seen near the summit of the bow only, since the drops, as they fall lower, coalesce, and become larger; and the white fog-bows, which are often seen, would result from very minute drops, of the diameter perhaps of one thousandth of an inch.

Afterwards a communication was also read from C. Darwin, Esq., containing a notice that animals (lizards, &c.) which are oviparous in certain districts of South America, as they are in this country, are viviparous in the province of Mendoza, which he visited. Mr. Darwin also gave an account of red snow observed by him in the road from St. Jago de Chili to Mendoza, by the Portello pass, and of a microscopical examination of the substance.

XV. *Intelligence and Miscellaneous Articles.*

ON THE ANALYSIS OF GERMAN SILVER, AND THE SEPARATION OF ZINC FROM NICKEL, &c. BY MR. JOSEPH D. SMITH.

WISHING to ascertain in what proportions the copper, zinc, and nickel were combined in a specimen of German silver, or pakfong, an alloy which has within a few years been extensively employed as a substitute for silver plate, I referred to Rose's Analytical Chemistry, in which I find it stated, that when a solution of potash or soda is added to one containing the oxides of nickel and zinc, their separation is by no means completely effected, a great excess of alkali not redissolving the whole of the oxide of zinc, even when boiled; he therefore directs the oxides to be converted into chlorides, and then to volatize the chloride of zinc; but the manipulation in this process is of so complicated a nature and requires such numerous precautions to ensure accuracy, that it is quite unsuited to any but the most experienced and skilful analysts.

Among many of the experiments performed I tried to discover a simpler method than that recommended by Rose: there was one which I made to ascertain to what extent the oxide of zinc was soluble in soda or potash after precipitation in mixture with oxide of nickel; for if the method would give results correct within 3 or 4 per cent., it would have answered my purpose sufficiently well: therefore I took 20 grains of zinc and 30 grains of oxide of nickel, dissolved them in muriatic acid, and having largely diluted the solution, boiled it for half an hour with four times as much soda as it took to precipitate the oxides. The precipitate when dried and ignited weighed 46·4 grains, showing that 16·4 grains of oxide of zinc were precipitated with the 30 grains of oxide of nickel. Thus the results obtained by the use of caustic alkali are as incorrect in this instance, or even more so, than when this process is employed to separate copper from zinc, the unfitness of which method was pointed out by Mr. Keates in a paper on the Analysis of Brass (Ann. of Philosophy, vol. xix.), and he at the same time gave directions for the performance of its analysis. Although the results obtained by the employment of his process are sufficiently correct, yet it may be advantageously superseded by passing a current of sulphuretted hydrogen gas through the solution of the mixed metals made strongly acid, separating the bisulphuret of copper, and after heating the solution to expel the excess of sulphuretted hydrogen, the zinc may be precipitated by carbonate of soda. Another experiment, in which the process recommended by Mr. Phillips for separating oxide of cobalt from oxide of nickel by adding a solution of potash to an ammoniacal one of the mixed metals, was tried with equally unsatisfactory results, for from 20 grains of zinc and 30 grains of oxide of nickel a precipitate was thrown down from an ammoniacal solution by potash, weighing after ignition 48 grains, giving 18 grains of oxide of zinc mixed with 30 grains of oxide of nickel.

After many other methods, of which it will suffice to say that they were all unsuccessful, I was led to try the effect of sulphuretted hydrogen gas upon the salts of nickel and zinc, when solutions of them, previously neutral, are acidified by oneof the weaker acids, and which acid should likewise form soluble salts with both metals. Pursuing this idea, I mixed a little acetic acid with a neutral solution of zinc; and also with a neutral solution of nickel, and passed a current of sulphuretted hydrogen through both the solutions: in the solution of nickel no precipitation took place, but in that of zinc an abundant white precipitate fell; and by passing excess of the gas through the solution, the whole of the zinc was precipitated as a sulphuret. I then made an experiment to determine whether it was possible to analyse German silver by passing sulphuretted hydrogen through its solution, differing in its acidity, both as regards the acid and degree of acidity at different times, according to the nature of the precipitate desired, whether copper or zinc; the results obtained were very satisfactory.

$24\frac{1}{2}$ grains of copper, 12 grains of zinc, and 20 grains of oxide of nickel were dissolved in nitromuriatic acid; the solution, strongly acidified with muriatic acid, was diluted with about a pint of water; a current of sulphuretted hydrogen gas was then passed through the solution until all the copper was precipitated; the bisulphuret of copper formed having been well washed, was acted on by nitric acid, which dissolved the copper and left some sulphur; after the separation of the latter, the solution of copper was boiled with caustic soda to precipitate the peroxide, which after ignition weighed $30{\cdot}4 = 24{\cdot}3$ grains of copper. The solution containing the zinc and nickel was carefully evaporated to dryness, to expel the excess of acid, and the residue dissolved in water acidulated with one fluid ounce of strong acetic acid sp. gr. 1·0691, and warmed to assist the action: when this was effected, the solution was diluted to about a pint, and a stream of sulphuretted hydrogen gas was passed through it until the gas was in excess; a dingy white precipitate of sulphuret of zinc fell, which weighed 18 grains = 12 grains of metallic zinc.

The remaining solution containing the nickel, after being heated to expel the sulphuretted hydrogen, was decomposed by caustic soda; this gave hydrate of nickel, which when reduced to protoxide by strong ignition, weighed 20·1 grains. In this experiment there was a loss of 0·2 grain of copper and a surplus of 0·1 grain of oxide of nickel; errors so small that they are evidently those of manipulation and not owing to any defect in the process followed.

70 grains of German silver gave by the above process:

Copper	42·1
Zinc	12·5
Nickel	13·2
Cobalt	2·4
	70·2

There appears to be a larger proportion of copper in this than is generally met with in German silver, for only one out of four alloys, the composition of which, on the authority of Gersdorff, is given by Bourchardat (*Cours de Chimie*, 1er partie, page 289), contains as large a proportion of copper as the specimen I analysed, and to it may be attributed the very yellow shade which this alloy possessed; its chief advantage over many others being what is technically called 'dipping well.'

The same method may doubtless also be employed to separate manganese and cobalt from zinc, for solutions of the former metals, when acidified by acetic acid, are not precipitated by sulphuretted hydrogen gas.

St. Thomas's Hospital,
December 1835.

ACTION OF MUSHROOMS ON ATMOSPHERIC AIR.

M. Marcet subjected known quantities of mushrooms to the action of oxygen, azote, and atmospheric air under graduated receivers; and having left them for a certain time, and ascertained the alteration both in the volume and nature of the gases; the conclusions at which he arrived, after performing numerous experiments, are the following:

1st. That mushrooms produce very different effects upon atmospheric air from those which result from the action of green plants under similar circumstances. They vitiate the air very readily, either by absorbing its oxygen to form carbonic acid gas at the expense of the carbon of the vegetable, or by disengaging carbonic acid ready formed.

2nd. That the modifications which atmospheric air suffers by the contact of mushrooms in a vegetating state are the same day and night.

3rd. That if fresh mushrooms are suffered to remain in pure oxygen gas, a large portion of the gas disappears in a few hours. One part of it combines with the carbon of the vegetable to form carbonic acid gas, while another portion is fixed in the vegetable, and is replaced by azotic gas liberated from the mushrooms.

4th. That fresh mushrooms, by remaining for some hours in azotic gas, produce but little change in it; a small quantity of carbonic acid is disengaged, and a very minute portion of azote is absorbed.—*Journal de Chimie Médicale, Octobre* 1835.

ALDEHYD—A NEW COMPOUND.

This substance, composed of hydrogen, carbon, and oxygen, was discovered by M. Liebig; it was procured by distilling with a gentle heat a mixture of,

Alcohol (80 per cent. real)	4 parts
Water	4 do.
Sulphuric acid..................	6 do.
Peroxide of manganese	6 do.

Aldehyd, mixed with alcohol and some other products, comes over, and some carbonic acid is disengaged. As the new product is extremely volatile, the receiver must be kept very cool to prevent the great loss of it which would otherwise occur. When about six parts have been distilled, they are to be withdrawn and redistilled, mixed with an equal weight of well-dried chloride of calcium, from a retort in a salt-water bath; when three parts have come over, these are to be again distilled with an equal weight of chloride of calcium, and then one part and a half, which is aldehyd, completely free from water, and partly so from alcohol and æther, is to be distilled. To this aldehyd its volume of æther is to be added, and ammoniacal gas is to be passed through it to saturation, taking care to surround the vessel with cold water, on account of the great heat disengaged. It is requisite to place a safety bottle between the vessel from which the ammonia is evolved and that which contains the aldehyd and æther, in order to prevent the passage of vapour of the aldehyd into the apparatus used for disengaging the ammonia, the absorption of this latter gas being so extremely rapid, that it would be impossible to prevent its rise. As the absorption of the ammoniacal gas proceeds, the liquor becomes turbid, and numerous transparent colourless crystals are precipitated, which are a compound of ammonia and aldehyd; these are to be purified by two or three washings with æther; they are then to be dissolved in an equal weight of water, and to the solution is to be added a mixture of three parts of sulphuric acid and four of water. At a gentle heat the aldehyd distils with brisk effervescence, and the operation ought to be stopped as soon as the water in the bath begins to boil; the aldehyd comes over mixed with water, and is to be re-distilled with an equal volume of chloride of calcium in large pieces; surrounding the apparatus carefully with cold water, on account of the heat generated by the union of the aldehyd and chloride: the product is to be again distilled from a salt-water bath with chloride of calcium in powder, and the aldehyd has then the following properties: it is liquid, colourless, transparent; its odour is æthereal and suffocaing, and its specific gravity is 0·790; it boils at 71·24 Fahr.; it combines with water in all proportions, much heat being evolved; if chloride of calcium be added, the aldehyd separates and floats on the surface; it combines in the same way with alcohol and with æther; its solutions produce no change on vegetable blue colours; it is very inflammable, burning with a blue flame; if it be kept in a bottle containing air, it decomposes it; absorbs the oxygen and becomes very concentrated acetic acid. Phosphorus, iodine and sulphur are dissolved by this fluid, and much heat is given out during its combination with bromine and chlorine, and hydrochloric and hydrobromic acids are formed, and it appears then to be converted into *bromal* and *chloral*. Nitric acid, when heated with aldehyd, decomposes it; nitric acid [oxide ?] and nitrous acid being disengaged. By the action of heat it is converted into a yellow turbid fluid, on the surface of which a resinous substance of a red brown colour appears, so elastic that it may be drawn out into long filaments; this substance is called by M. Liebig, *resin of aldehyd*; long prismatic crystals are formed in aldehyd kept in bottles.

These crystals are but slightly volatile; they do not freeze at 212°; they sublime in very brilliant white transparent needles, and they are hard and easily pulverized; they are inodorous, combustible, nearly insoluble in water, but dissolved by alcohol and æther. M. Liebig inquires whether these crystals are produced by the absorption of oxygen, and he has not decided the question. After the formation of these crystals, the aldehyd contains another soft (*mou*) volatile liquid, which bears a great resemblance to *acetal*.

Aldehyd is composed of

4 atoms carbon......	305·748........	55·024
8 atoms hydrogen....	49·918.......	8·983
2 atoms oxygen......	200·000........	35·993
	555·666	100·000

Journal de Chimie Médicale, Novembre 1835.

ON THE PROPERTIES OF TELLURIUM. BY BERZELIUS.

When tellurium is fused in a glass vessel containing hydrogen, and it is allowed to cool slowly, a very shining regulus is obtained, resembling burnished silver. The surface in contact with the glass is a perfect mirror. The other surface is covered with a crystalline vegetation, perfectly analogous to that which is formed when a solution of muriate of ammonia is dried upon a glass plate: the crystallization appears to appertain to a regular system; but when the regulus is broken and the angle of the crystallized surface is measured, it is found to be incompatible with a regular system. If the metal be fused in a cupel in a sand-bath it presents crystals, the form of which is determinable.

Tellurium is not in the slightest degree malleable; it may be reduced to the finest powder, so as to entirely lose its metallic lustre. If this powder be sprinkled with water, it is covered with a grey, brilliant, metallic pellicle, which it is difficult to sink in water. In this respect it resembles the powder of sulphur, selenium, and silicon.

The fusing point of tellurium is such as to soften glass too much to retain it. If it be heated in glass to full redness, the body of the retort is filled with a yellow-coloured gas resembling chlorine; some small drops of tellurium are deposited in the neck of the retort, which, however, do not appear to increase, although the red heat be continued for several hours. In the distillation of tellurium in hydrogen, already mentioned, there are formed at the place where the cold gas is in contact with the vapour of tellurium, long, flat, crystalline needles; they are not sufficiently wide and thick to allow of the determination of any angle. These crystals are also found, but in smaller quantity, in the places at which the gaseous mixture goes out from the hottest parts of the tube. If tellurium be strongly heated in a covered crucible, it gives out when uncovered a peculiar disagreeable odour, different from and much weaker than that of the oxide of selenium. M. Magnus has already noticed this fact. On cooling, tellurium contracts very much; and if the surface solidifies faster than the interior, so that it can support atmospheric pressure, cavities containing air are formed in the interior, which are discovered when the regulus is broken. These cavities often communicate with the surface. This property of tellurium also belongs to selenium; it influences the density of the metal; Müller of Reichenstein found it to be 6·343, Klaproth 6·115, and Magnus 6·1379. Berzelius found great difficulty in determining the density; the metal, which sublimed in drops in distillation with hydrogen gas, had a density of 6·1305, which is less than above stated, and proves that these drops also had cavities. Several fragments taken from a regulus which had a cavity, and from near it, gave the following numbers: 6·2324, 6·2516, 6·2445, 6·2578. The mean is 6·2455, but it is proper to take the greatest as the true density, since the cause of the discordant results tends to render them too light.

ACTION OF OXACIDS ON PYROXYLIC SPIRIT—NITRATE OF CARBOHYDROGEN.

[Continued from vol. vii. p 539.]

Justice to Dr. Thomson induces me to copy the following from the Records of Science:

" Dumas has unnecessarily coined a new name to distinguish this base, viz. Méthylène (from μεθυ, *wine*, and ὑλη, *wood*). What advantage is gained by this innovation it is difficult even to guess at. The disadvantages of designating simple compounds by arbitrary names (since this compound turns out to be one of the simplest organic compounds with which we are acquainted) are sufficiently obvious, and we trust that this name will not be adopted by British chemists.

" The existence of this simple compound of hydrogen and carbon in pyroxylic spirit was demonstrated in 1826 by Dr. Thomson. (*Edin. Trans.*, xi. 15. *Inorganic Chemistry*, i. 194; ii. 294.) It is difficult to allow ourselves to suspect that Dumas should have been ignorant of this fact, which has been published for nine years; but, in consequence of the absence of any allusion to it, it is impossible in charity to avoid drawing such a conclusion."

To the above I will only add that MM. Dumas and Peligot have committed a most useless innovation in calling the nitric acid *azotic* *acid.* This is the more remarkable, because M. Dumas, in the fifth volume of his *Chimie appliquée aux Arts,* printed in 1835, employs the term nitric acid and nitrate of méthylène.—R. P.

Nitrate of carbohydrogen is obtained with difficulty by the direct action of nitric acid upon pyroxylic spirit. At first nothing remarkable is obtained; but towards the end of the operation red vapours are procured, which are a mixture of nitrate of carbohydrogen and formic acid.

It is, however, easily obtained by the following process. Put into a retort 50 grammes of powdered nitre, and add to it as soon as made a mixture of 100 grammes of sulphuric acid and 50 of pyroxylic spirit: the retort should be large, the receiver tubulated, and it is to communicate with a bottle containing salt water, placed in a freezing mixture, and with a tube to conduct the gases into a chimney.

The heat given out by the mixture and action of these substances is sufficient to produce their mutual decomposition and the production of the nitrate of carbohydrogen. A small quantity of red vapour appears in the apparatus; while, on the contrary, much æthereal matter is produced, which condenses partly in the tubulated receiver and partly in the cooled bottle.

When the operation is over there is obtained at the bottom of the bottle a thick and colourless stratum of the new æther. In order to purify it, it must be decanted, several times distilled from a mixture of massicot and chloride of calcium. The distillation should be performed in a bath of boiling water; the quantity of the new compound obtained is equal to that of the pyroxylic spirit employed.

This product is not, however, pure, and evidently contains several different substances. When distilled, the boiling point is at first 140°; it then gradually rises to 152°, and goes on without further alteration. The portion distilled between 140° and 145°, exhales a very distinct odour of hydrocyanic acid; it is probably formed of carbohydrogen. The portion which boils at 152° is the most abundant, and evidently the purest. The authors consider it, provisionally, as nitrate of carbohydrogen; it is colourless, has a weak æthereal odour; its density 1·182 at 72°. It is perfectly neutral; it burns rapidly with a yellow flame. When a few drops are put into a tube, and it is heated, it is soon vaporized, and detonates with force if the heat be continued. It detonates with violence on the approach of flame; and a receiver containing 15 cubic inches might occasion serious accidents if exploded.

While it remains liquid it is not dangerous, but its vapour at a temperature not exceeding about 300°, detonates with singular violence; in fact it contains azote, hydrogen, and carbon, elements analogous to those of gunpowder: the products of the detonation are nitric oxide, carbonic acid, and water.

By analysis it appears to be composed of

Carbon	17·7
Hydrogen	4·2
Azote	18·2
Oxygen	59·9

While nitrate of carbohydrogen would give by calculation:

C^4	153·0........	15·8
H^6	37·5........	3·8
Az^2	177·0........	18·3
O^6	600·0........	62·1
	967·5	100·

When it is heated with a solution of potash in alcohol, rapid decomposition takes place, and crystals of nitrate of potash are formed in considerable quantity, the formation of which cannot be explained on the supposition of its being a nitrite instead of a nitrate of carbohydrogen; and a nitrite would yield results further removed from those above given than those of a nitrate.

[To be continued.]

SCIENTIFIC BOOKS.

New Scientific Journal.—On April 1st, 1836, will appear The London Geological Journal, No. I., with coloured engravings, by J. de Carle Sowerby, F.L.S., of new Fossil *Echinidæ* from the English strata.

Herpetologia Mexicana; seu Descriptio Amphibiorum Novæ Hispaniæ. Pars Prima, Saurorum Species amplectens. Adjecto Systematis Saurorum Prodromo, additisque multis in hunc Amphibiorum ordinem observationibus. Edidit Dr. Arend Fr. Aug. Wiegmann. Accedunt Tabulæ lithographicæ decem.—Lately published at Berlin.

METEOROLOGICAL OBSERVATIONS FOR NOVEMBER 1835.

REMARKS.

Chiswick.—November 1. Hazy: clear. 2. Dense fog: rain. 3. Rain. 4. Overcast. 5. Cold: haze. 6. Sharp frost: fine: slight fog. 7. Frosty haze: rain at night. 8. Clear and fine. 9—12. Cloudy and cold. 13. Rain: fine. 14. Fine. 15. Cloudy. 16. Fine. 17. Hazy. 18. Overcast: fine: clear at night, with extensive aurora borealis which illuminated the whole of the visible northern hemisphere. At times the streamers extended from the horizon even beyond the zenith. 19. Very fine. 20. Slight haze. 21. Clear and fine. 22. Overcast: cloudy and windy. 23. Rain: fine. 24, 25. Very fine. 26. Hazy: fine. 27, 28. Overcast: rain: fine. 29. Rain. 30. Rain: fine.

Boston. — November 1. Fine. 2. Foggy. 3. Rain. 4—6. Cloudy. 7. Cloudy: rain P.M. 8, 9. Cloudy. 10. Cloudy: rain A.M. 11. Fine. 12. Rain. 13. Cloudy. 14. Cloudy: rain P.M. 15—17. Cloudy. 18—21. Fine. 22. Cloudy. 23. Rain. 24. Cloudy. 25. Cloudy: rain P.M. 26—28. Fine. 29. Cloudy: rain all night. 30. Cloudy: rain A.M.

[We have incorporated with our Meteorological Table on the next page, a selection from the Observations made at the Apartments of the Royal Society, Somerset House, London, by the Assistant Secretary, by order of the President and Council, as published in the *Athenæum;* and we shall continue to do so in future, giving in our next the altitude and other particulars of the station.]

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1835.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London. Roy. Soc. 9 A.M.	Chiswick. Max.	Chiswick. Min.	Boston 8½ A.M.	London: Roy. Soc. Fahr. 9 A.M.	London: Roy. Soc. Self-registering. Min.	London: Roy. Soc. Self-registering. Max.	Chiswick. Max.	Chiswick. Min.	Bost. 8½ A.M.	London: Roy. Soc. 9 A.M.	Chisw.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Bost.	Lond.: R. S. 9 A.M. in degrees of Fahr.
Nov. ⊙ 1	30·016	30·276	30·218	29·70	42·2	40·0	44·6	51	32	38	NNE.	S.	calm	0·038	...	0·13	42
M. 2	30·094	30·298	30·145	29·39	37·7	34·9	46·7	51	44	33·5	S. var.	SE.	calm	...	0·28	...	36
T. 3	29·909	30·034	30·071	29·64	46·4	36·7	47·4	48	39	45	SE.	S.	calm	·283	·04	·10	42
W. 4	29·913	30·099	30·059	29·65	43·0	41·0	42·7	44	38	43	E.	S.	E.	·041	...	·05	38
○ Th. 5	29·800	30·008	29·938	29·64	40·2	38·2	41·3	42	28	41	E.	SE.	N.	...	...	...	35
F. 6	29·726	30·039	29·932	29·50	38·5	35·2	41·4	48	28	37	E.	SW.	calm	...	...	...	35
S. 7	29·880	30·053	29·930	29·65	43·2	36·2	48·9	50	36	38·5	E.	SE.	calm	...	·20	...	38
⊙ 8	29·804	30·036	30·024	29·60	43·3	40·6	46·7	51	36	39	NE.	W.	calm	·191	·05	·26	40
M. 9	29·942	30·471	30·149	29·75	40·6	39·4	40·5	41	30	42	NNE.	E.	E.	...	·02	...	36
T. 10	30·156	30·438	30·343	29·98	37·7	35·6	38·2	40	34	40	E.	NE.	calm	...	·04	...	30
W. 11	30·218	30·441	30·343	30·02	38·9	35·2	42·7	45	37	38	N.	N.	calm	...	...	·08	32
Th. 12	30·039	30·249	30·317	29·98	39·8	36·9	44·2	46	36	35	N.	N.	calm	...	...	...	35
☾ F. 13	30·200	30·409	30·370	30·03	38·8	36·7	41·6	46	35	40	N. var.	NE.	calm	...	...	·08	35
S. 14	30·010	30·235	30·148	29·80	39·2	37·6	42·7	45	38	37·5	SW.	N.	calm	·038	...	...	33
⊙ 15	29·936	30·151	30·107	29·80	42·7	38·0	44·7	46	37	41	NE.	N.	calm	...	...	·11	39
M. 16	29·837	30·052	30·018	29·60	40·4	38·3	44·2	47	35	38	WSW.	W.	calm	·033	...	...	35
T. 17	29·756	29·979	29·890	29·40	43·9	39·3	46·6	51	42	45	WSW.	S.	calm	...	...	...	40
W. 18	29·659	29·946	29·763	29·20	47·7	42·8	52·4	55	36	48	SW.	SW.	calm	...	...	...	42
Th. 19	29·841	30·062	30·034	29·45	41·6	39·0	46·4	50	33	40	SW.	SW.	calm	...	...	...	39
● F. 20	29·853	30·057	29·963	29·47	47·5	40·0	50·2	54	47	44	SW.	S.	W.	...	...	...	42
S. 21	29·752	29·935	29·906	29·30	50·5	46·2	52·3	54	49	50·5	S. var.	SW.	W.	...	...	...	45
⊙ 22	29·689	29·875	29·768	29·30	51·8	48·6	53·2	54	50	54	S. var.	S.	W.	...	·20	...	47
M. 23	29·685	29·866	29·859	29·30	51·5	50·0	53·7	56	45	50	S. var.	S.	calm	·213	...	·11	47
T. 24	29·705	29·881	29·836	29·35	50·7	46·3	52·7	57	49	47	NE.	S.	calm	...	...	·06	46
W. 25	29·675	29·838	29·733	29·20	51·2	49·3	53·4	54	51	52	ESE.	S.	calm	...	·09	...	46
Th. 26	29·536	29·717	29·419	28·86	52·5	50·3	55·5	57	52	52	ENE.	S.	W.	·111	·15	·04	49
☽ F. 27	29·251	29·415	29·330	28·90	53·8	52·0	53·8	53	45	48	NE. var.	S.	calm	·105	·49	...	51
S. 28	29·182	29·628	29·351	28·89	49·2	47·6	51·3	51	37	48	SW.	SW.	calm	·408	·02	...	45
⊙ 29	29·382	29·568	29·323	29·14	48·7	44·8	50·6	52	48	43·5	S. var.	SE.	calm	...	·34	...	45
M. 30	28·976	29·265	29·152	28·70	51·7	47·8	55·3	58	46	48	SE.	S.	SE.	·255	·02	·72	46
		Max.	Min.					Max	Min.					Sum.	Sum.	Sum.	
	29·807	30·471	29·152	29·47	44·8	41·5	47·5	58	28	43·2				1·716	1·94	1·74	40·4

THE

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AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

FEBRUARY 1836.

XVI. *On Capillary Attraction and the Molecular Forces of Fluids.* By the Rev. JAMES CHALLIS.*

IN Laplace's capillary theory the fluid is supposed to be perfectly incompressible; and the only forces acting on it besides gravity, are assumed to proceed both from its own molecules and those of the solid with which it is in contact, to be wholly attractive, and to become insensible at all sensible distances from the attracting centres. Calculations made on these hypotheses serve to explain phænomena in a remarkable manner. But the principles of this theory are liable to objection, as no account is taken of a repulsive molecular action, whilst, on the supposition of a molecular constitution of the fluid, the particles could not be held in places of equilibrium but by the action of repulsive as well as attractive forces. The theory of M. Poisson not only meets this objection by regarding the fluid as composed of insulated molecules subject to the opposite tendencies of attractive and repulsive forces, and as liable to compression, but takes into consideration also the effect of the variation of density, which, according to this constitution of the fluid, must exist within a small depth both from its free surface and from that in contact with the solid. This, no doubt, is the only complete method of treating the problem. The next inquiry is, to what degree is that superficial variation of density effective? M. Poisson's theory does not bear on this point, and experi-

* Communicated by the Author.

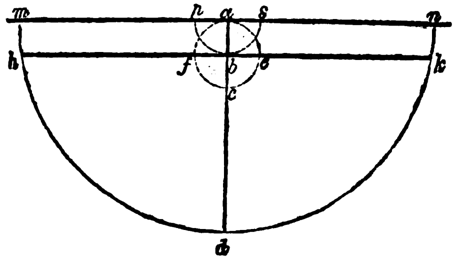
ments have not succeeded in establishing even the existence of such a variation.

The object of the following remarks is to show, in the first place, that capillary phænomena will be little affected by the superficial variation of density, when the molecular attraction is feeble compared to the repulsion, and the sphere of attractive activity great compared to that of the repulsive activity; and then to give a reason for supposing the molecular forces of fluids to be of this nature, by showing that such an hypothesis will account for their *fluidity*.

The constitution of fluids is here assumed to be molecular, and the molecules are conceived to be held in places of equilibrium by attractive and repulsive forces, which near the boundaries of the fluids necessarily produce a variation of compression. The condition of equilibrium of the molecules at the free surface requires the extent of action of the attractive force to be greater than that of the repulsive, while experience shows that the sphere of molecular activity is of insensible magnitude. With respect to any point at a sensible distance from the surface, if we conceive a plane to pass through it, the resultant of the repulsions it is subject to from the action of the particles on one side of the plane will be perpendicular to the plane, and equal and opposite to the resultant from the action of the particles on the other side. The same may be said of the attractions. But the attractive resultant may be very different from the repulsive resultant. The ratio of the two may be taken as the measure of the proportion of the attractive to the repulsive action of the fluid. Let us now assume the attractive action to be very feeble compared to the repulsive, but to have a much larger sphere of activity, and consider what will result from this hypothesis.

Conceive $m\ a\ n$ to represent the projection of the plane surface of the fluid on the plane of the paper; $a\ b\ c\ d$, a normal to the surface; $a\ b$, the radius of the sphere of repulsive activity, and $b\ d$ the same for the attractive force.

Let the spherical surface $a\ f\ c\ e$ be described with centre b and radius $b\ a$; and $m\ d\ n$, a portion of a spherical surface, with centre b and radius $b\ d$. The former surface includes all the atoms which exert a sensible repulsive force on an atom at b, and the space between this surface and the latter includes all the atoms whose attractions are sensible at the same point. The resultants both of the attractive and repulsive forces on b will be in the normal $a\ b\ c\ d$. The attractive resultant is the excess of the action of $h\ d\ k\ e\ c\ f$ above that of $m\ a\ f\ h$ and $n\ a\ e\ k$, and takes effect in the direction $b\ c$. The repulsive resultant is the excess of the action of the hemisphere *f c e* above that of the hemisphere *f a e*, and is



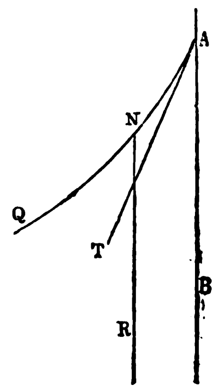
effective in the direction *b a*. These two resultants must be equal. But as *a b* is small compared to *b d*, the attraction on *b* will differ little from the attraction on *a*. And as the latter must be just equal to the repulsion of the hemisphere *p b s*, whose radius = *a b*, it follows that this repulsion is very little greater than the *difference* of the repulsions of *f c e* and *f a e*. That this may be the case, there must be a rapid variation of density at *a*, and at the same time, on account of the feebleness of the attractive resultant, a small variation at *d*. The repulsion on any particle will thus be chiefly owing to the action of the particles in its immediate neighbourhood, and be increased but little by the action of the more remote particles within the sphere of activity. The law of repulsion will be one of rapid diminution with the increase of distance from the repelling molecule; and the depth to which the superficial variation of density is considerable, will be less than the radius of the sphere of activity of the repulsive force, and therefore much less than that for the attractive force. And if the variation of density is inconsiderable at *b*, *à fortiori* it will be so at all points intermediate to *b* and *d*.

When, therefore, it is required to investigate any effect due to the molecular attractions of fluids, the superficial variation of density may be neglected, if the suppositions on which the foregoing reasoning is founded be correct; and by treating the fluid as incompressible, the repulsive force will be taken account of with the same kind of approximation as when, in problems on the common theory of fluids, a change of pressure is supposed to be unaccompanied by a change of density. According to these views Laplace's hypotheses suffice for a theory of capillary attraction, and any theory which, like that of M. Poisson, leaves the laws of the attractive and repulsive forces perfectly arbitrary, would seem to be not inconsistent with Laplace's, but inclusive of it.

The way in which the above hypothesis may be conceived to account for the fluidity of fluids may be stated as follows: The distinguishing characteristic of fluids is the facility with which the relative positions of different parts may be made to change, excepting when the forces applied tend to compress the whole mass into a smaller space. Now, since it is a consequence of our hypothesis, that the repulsive force of each atom varies very rapidly with the distance, and the attractive force slowly, the above-mentioned property will be accounted for by saying, that any cause which compresses in a very slight degree any portion of the atoms, by diminishing their mutual distances, calls into play their repulsive action, without sensibly affecting their attractions on each other, or on particles more remote, so that they will be ready to obey any inequality of repulsion without *drawing* surrounding particles with them. Suppose, for instance, a plate of small but finite thickness were dipped in a fluid with its planes vertical. When it just begins to enter, it compresses the particles immediately under, and by diminishing their mutual distances calls forth a repulsion, which again compresses and puts in motion the contiguous particles: these in like manner act on the next in succession, and so on till the whole mass is made to yield to the immersion. If at the same time a sensible attractive force were excited by the compression, the surrounding particles would be drawn towards the compressed parts, and the effect on those at the surface would be a depression near the immerging plate, such as we know to take place when a solid is dipped in a *semi-fluid* substance. An obstacle would thus be presented to the immersion of the plate, the same in kind as that presented by a solid, the rigidity of which, according to this theory, will be owing to a great degree of energy in the molecular attractions. But when the attractive forces are very feeble, there will be little resistance to the immersion besides that arising from the inertia of the particles, and the fluidity will consequently be nearly perfect. This distinctive property which fluids possess of being readily divided, I have elsewhere observed, may be conveniently employed as a principle from which the fundamental equations relating to their equilibrium or motion may be derived.

Some mathematical consequences, which, as I am about to show, follow from the views expounded above, will also serve to confirm the initial hypothesis. Admitting that the variation of density at the surfaces of fluids has small influence on any effects resulting from their molecular attractions, and that the molecular repulsions are taken account of by supposing the fluid incompressible, we may employ the common equation of the equilibrium of fluids, viz. $d\,p = \varrho\,(X\,d\,x + Y\,d\,y + Z\,d\,z)$, in questions relating to capillarity. It is well known that this equation determines the resultant of the forces acting on any particle at the surface to be perpendicular to the surface.

Let A N Q be the projection on the plane of the paper of a fluid surface near its contact with a plane solid surface, supposed to be projected into the straight and vertical line A B. Draw a tangent A T at A. The resultant of the forces acting on a particle at A will be perpendicular to A T. Conceive a plane perpendicular to the plane of the paper to pass through A, and to make an angle θ with A B; and another plane, similarly situated, to make an angle $\theta + \delta\theta$ with the same line. Then $\delta\theta$ being indefinitely small, the attraction of the fluid between the planes on A will vary as $\delta\theta$. Let it $= q\,\delta\theta$, and let the angle T A B $= \varphi$. The total attraction of the fluid between the planes A T and A B on A, will in the vertical direction be $\int q\,d\,\theta \cos\theta$ [from $\theta = 0$ to $\theta = \varphi$], or $q \sin\varphi$; and in the horizontal direction, $\int q\,d\,\theta \sin\theta$ [from $\theta = 0$ to $\theta = \varphi$], or $q\,(1 - \cos\varphi)$.



The resultant of the solid's attraction, which will be in the horizontal direction, will be found by putting q' for q, and 180° for φ in the last expression, and will consequently be $2\,q'$. The resultant of the attraction of the portion Q A T between the surface and the tangent plane will be nearly in the direction A T. Calling it k, and the force of gravity g, the whole attraction in A B $= g + q \sin\varphi + k \cos\varphi$, and that in the horizontal direction $= 2\,q' - q\,(1 - \cos\varphi) - k \sin\varphi$. Since the resultant of these is perpendicular to A T, their ratio $= \tan\varphi$, from which equality we readily obtain

$$(2\,q' - q) \sin\varphi = g \cos\varphi + k.$$

Now, for all fluids capable of hanging in drops of sensible thickness from the horizontal surface of the solid, $2\,q'$ is greater than q; and considering the small extent of activity of the molecular forces, both q' and q must be exceedingly greater than gravity. Also if φ be an angle of considerable magnitude, k must be much smaller than the left side of the equation. Hence this equation cannot in general be satisfied except for a very small value of φ. If the surface were horizontal, we should have $k = 0$, $\cos \varphi = 0$, and therefore $2\,q' = q$. This relation appears to be nearly satisfied between mercury and glass, since the experiment of Casbois shows, that by merely boiling the mercury, its upper surface in a capillary glass tube may become horizontal, or even concave. The reasoning by which φ was shown to be in general a very small angle, does not apply to mercury, which is incapable, like water and oils, of adhering to a solid. The smallness of the angle of *actual* contact, seems to be a condition always satisfied, whenever a fluid moistens a solid*.

It is not possible to determine theoretically the form of the curve A N Q near the solid, because the laws of the molecular forces are unknown. Considering, however, the small extent of their sphere of activity, we may say that for a small distance the curve will be the same, whether the fluid rise against a plane surface or in a capillary tube. Let N be a point of the fluid surface situated just beyond the sphere of sensible action of the solid, let N R be drawn vertically, and the angle Q N R $= \omega$. It is proved, on the supposition of incompressibility, (see Poisson's New Theory of Capillary Action, art. 18,) that the quantity of fluid raised in a given tube varies as $\cos \omega$. Now, as it is shown above that the angle φ of actual contact is constant, and very small, it follows that the angle ω, and consequently the heights of ascent, may be different for different relative positions of A and N. If the fluid ascend in a tube not previously wetted, the points A and N will be nearest each other, ω will have its greatest value, and the height of ascent be least. As A and N are more removed from each other by wetting more of the solid surface, the rise of the fluid may be expected to be greater, and to attain its maximum value when the moistening is carried to such a degree, that the solid can retain no more fluid attached to it. In this case the influence of the solid on the form of

* The views which this communication is intended to explain are stated, in part, at the close of my Report on Capillary Attraction (contained in the Fourth Report of the British Association), to which I take this opportunity of referring for the sake of pointing out an error in the remarks (p. 273.) on an equation of Laplace's theory, equivalent to that obtained above, excepting that gravity is not taken into account. It is not true, as there stated, that Laplace neglects the superficial variation of pressure. The strictures of Dr. Young upon it, adduced at p. 266 of the Report, will appear to be inapplicable from the reasoning above, which it is hoped will serve to place the inferences to be drawn from this equation, and its importance in the capillary theory, in a true point of view.

the fluid surface will be the least possible, and the column may be considered to be supported by the action of the fluid on itself. The fluid will thus hold the place of the solid in the equation previously obtained; we shall have $q' = q$, and the equation will become

$$q \sin \varphi = g \cos \varphi + k.$$

Hence, on account of the smallness of g and k compared to q, the angle of contact with the aqueous tube will be small as well as of that with the solid. Consequently the intermediate angle ω will be small, and the heights of ascent be the same whatever be the solid, if the fluid thoroughly wets it. In the experiments of M. Link, (see Poggendorff's *Annalen* 1833, p. 404,) the condition here supposed was fulfilled by means of an apparatus for dipping the solid repeatedly in the fluid. The height of ascent is found to be independent of the nature of the solid, or nearly so. In subsequent experiments, (Poggendorff's *Annalen*, 1834, p. 593,) the plates between which the fluids ascended were previously dipped in strong caustic alkali and concentrated sulphuric acid, to get rid of a film of grease attaching to them, which they contract in the act of polishing. Water, in these new experiments, stood very nearly at the same height between glass, copper, and zinc plates. Other fluids did not follow this law, and in the instance of sulphuric acid, the deviation appeared to depend on a chemical action between the solid and fluid. Such a circumstance would be likely to affect the form of the curve A N Q and angle ω, and consequently, according to the theory, the height of ascent.

The general inference from the foregoing reasoning is, that the heights of ascent do not merely depend on the molecular attractions, but, while these remain the same, may be affected by any circumstance that alters the form of the fluid surface near the solid, and particularly by the manner and degree of moistening the solid by the fluid. In this way the differences of the heights, as determined by different experimenters, may be accounted for.

The *maximum* height of ascent in a given tube varies, according to theory, for different fluids, as a certain quantity $\frac{H}{\varrho}$, in which ϱ is the specific gravity of the fluid, and H* de-

* This is the quantity called H by Poisson, and by Laplace in his Supplementary Treatise. The quantity so denominated in Laplace's first Treatise is equal to $\frac{H}{\varrho}$. This letter is inadvertently used sometimes in one of these senses, and sometimes in the other, in the Report on Capillary Attraction.

pends on the law, intensity, and sphere of activity of its molecular forces. Admitting the superficial variation of density to be negligible, and the fluids to be incompressible, H will be a measure of their *cohesiveness*, or the inverse of it a measure of their fluidity, as it is proportional to the effect of their molecular attractions acting under the same circumstances. (Laplace's *Treatise*, art. 12. and *Supplement*, p. 18.) On this account it formerly appeared to me simplest to suppose this quantity to vary as ϱ; and the first experiments of M. Link favoured this idea by assigning to different fluids the same height of ascent. Those subsequently made, which are the more accurate, do not give the same result, but sufficiently prove that the heights are not as the specific gravities, and consequently that H does not vary as ϱ^2, as is usually supposed. Probably nothing can be determined respecting it *à priori*. The last-mentioned experiments gave the following results when the fluids ascended between glass plates thoroughly moistened:—height $\times$ specific gravity $= 5{\cdot}3$, for sulphuric æther; 6·7, for alcohol; 10·7, for liquid caustic alkali; 10·9, for liquid ascetic [carbonated?] alkali; 12·5, for water; 15·6, for muriatic acid; 16·8, for nitric acid; 20·3, for sulphuric acid. According to what is said above, these numerical quantities are in the order of cohesiveness, sulphuric æther being the least cohesive body, or possessing the greatest degree of fluidity.

The phænomenon of *endosmose* may be appealed to as indicating a great attractive energy in partially fluid substances. When water is on one side of the porous membrane, and an imperfect fluid, as treacle, or solution of gum, on the other, the latter is found to draw the water powerfully through the pores. The force exerted at any time appears by experiment to be in this case proportional to the difference of the densities of the fluids on the opposite sides of the membrane.

The subjects treated of in this paper are of such a nature as scarcely to admit of any very definite discussion. Since, however, the degree in which capillary phænomena may be affected by a variation of density at the surfaces of fluids is at present quite unknown, it seemed desirable at least to ascertain whether an approximation to the truth is obtained when that variation is neglected; and, perhaps, the preceding reasons, connected with the nature of fluidity, may make it probable that such is the case.

Papworth St. Everard, Dec. 10, 1835.

XVII. *Letter from* Peter Barlow, *Esq., F.R.S., to the Rev.* D. Lardner, *LL.D., F.R.S., on the Theory of Gradients in Railways.*

Dear Sir,

As you have addressed a letter to me in the last London and Edinburgh Philosophical Magazine, I feel myself bound by courtesy to reply to it through the same Journal, not, however, with a view of entering into any controversy on the question. You have in your letter very clearly stated your mode of solution, I will endeavour to explain also the grounds of my objection; the readers of the Lond. and Edinb. Philosophical Magazine will then be able to form their own judgement.

First, t being the fraction expressing the ratio of the friction to the load, and ϵ the angle of the plane's inclination, you take t to denote the resistance on the horizontal plane, $t + \sin \epsilon$ to denote the same on the ascending plane, and $t - \sin \epsilon$ for the same on the descending plane. Then, assuming what may not, perhaps, be quite true, (but to which I do not here make any objection,) *i. e.* that in locomotive engines the power generated and expended is the same in the same time, you arrive at the conclusion, that in cases of uniform velocity the resistance into the velocity is constant: taking, therefore, V to denote the velocity on the horizontal plane, and v the velocity on the descending plane (which you also assume to be uniform), you arrive at the equation

$$(t - \sin \epsilon)\, v = t\mathrm{V}.$$

Whence
$$v = \frac{t\mathrm{V}}{t - \sin \epsilon}.$$

And by this formula your tables of velocities are computed for the Great Western and Basing lines; but where the formula gives more than 40 miles an hour the results of the computation are not stated.

Thus, for example, assuming, as you do, 25 miles to be the velocity on a horizontal plane, and $t = \frac{1}{250}$, the formula becomes

$$v = \frac{\cdot 1}{(\cdot 004 - \sin \epsilon)}.$$

I have gone over all the numbers in your four tables, and, except very trifling numerical errors, I find your results consistent with this formula as far as you have given the computed velocities; but those which you have not stated (by supposing the break to be employed) are so extraordinary that I think you can scarcely consider them as correct.

For example, taking a very common gradient of 16 feet to a mile, or $\sin \epsilon = \frac{1}{330}$, we find

$$v = \frac{\cdot 1}{(\cdot 004 - \frac{1}{330})} = 103{\cdot}09 \text{ miles per hour.}$$

Such a gradient is very common in practice, and in practice in such a case the break is not applied. The computed and practical result ought, therefore, to agree, supposing the solution correct; but such a velocity as one of 103 miles per hour has never yet, I believe, been obtained.

Again, with a slope of 1 in 250, by no means an uncommon gradient, your formula gives the velocity of descent *infinite*: now such gradients are descended without the break, but, of course, not with an infinite velocity. For slopes greater than the last, of which also there are many, your velocity passes through infinity, and becomes negative, and the time of descent negative also, or less than no time.

I have certainly stated in my Second Report to the Directors of the London and Birmingham Railway Company, that a solution which leads to such extraordinary results must be "erroneous both in theory and practice." And my opinion is not altered. The error, I conceive, arises from combining the two dissimilar forces t and $\sin \epsilon$, and then treating the question of descent as one belonging to the case of uniform motions; whereas (according to my view of the subject) it properly belongs to the class of accelerated motions.

Your solution, however, and my objection to it are thus placed before the readers of the London and Edinburgh Philosophical Magazine, and I leave the question in their hands.

I remain, dear Sir, yours truly,

Woolwich, Jan. 2, 1836. PETER BARLOW.

After closing my letter, I have thought it might be satisfactory to some of the readers of the Phil. Mag. to see the view I take of this question, which is as follows.

Suppose a body free from friction to arrive at, or to be propelled from, the upper end of an inclined plane with a velocity v, and let the angle of the plane $= \epsilon$, then the velocity acquired by that body in the time t will be $v + 2g. \sin \epsilon . t$; and the space descended will be $t\,v + g \sin \epsilon . t^2$; ($g$ denoting the space fallen through by gravity in one second, or $g = 16\frac{1}{12}$ feet.)

Suppose now a body subject to friction to reach the same plane with the same velocity, but that this body contains within itself an acting power which exactly balances the friction at all velocities; then I consider that the descent of this body will take place under precisely the same circumstances as the former, and therefore that the space described in the time t will be as before $l = t\,v + g \sin \epsilon \,.\, t^2$, and that in both these cases the velocity will be uniformly accelerated.

But if the internal acting power within the body will only balance the friction when the velocity of the body is v, there will be three circumstances on which the velocity of descent will depend, viz.

First, the original velocity $= v$.

Secondly, the accelerating force $= g \sin \epsilon$.

Thirdly, the retarding force arising out of the excess of friction beyond that of the internal force employed to overcome it.

In this form the solution of the problem falls under the case of variable forces, and is so involved as not to admit of solution without employing differentials of the second order.

Now, I consider a locomotive engine to be a body constituted as above supposed, that is, liable to friction, but containing within itself an acting power capable of overcoming the friction, so that where gravity does not act, the motion of the body continues uniform; and if on reaching a descending plane, the internal force was still such as to balance the friction due to the increased velocity, then its descent would be governed by the same laws as supposed in the second case, that is, the velocity after any time t would be $v' = v + 2\,t\,g \sin \epsilon$, and the space described would be $l = t\,v + g \sin \epsilon \,.\, t^2$.

The actual law of locomotive machines is not yet well understood; some engineers are of opinion that an increased velocity, by throwing the steam faster into the funnel, causes an increased draught, which produces a proportionally greater quantity of steam, in which case the above would be an exact expression for the space described; and if it is not so, and we still reject the consideration of retardation, then at all events the above formula will mark a limit in the problem, and the velocity and space thus obtained will be the greatest that can be acquired in a given time. This, therefore, is the most favourable view of the question for Dr. Lardner in comparing his velocities with mine. Let us see, then, what the results are on this supposition. We have seen that, l being any length of plane, $g \sin \epsilon\, t^2 + v\,t = l$,

or $$t^2 + \frac{1}{g \sin \epsilon}\, v\,t = \frac{l}{g \sin \epsilon} = \frac{h}{g \sin^2 \epsilon},$$

h denoting the height of the plane.

By the solution of this equation, we obtain

$$t = \frac{1}{2g \sin \varepsilon} \left\{ -v + \sqrt{(v^2 + 4gh)} \right\},$$

(h and v being taken in the same measure,) and the last acquired velocity v' will be

$$v' = \sqrt{(v^2 + 4gh)}.$$

Taking now the first example given in my letter, that is to say, a plane sloping 16 feet in a mile, and let the length of the plane be half a mile, (which is one of the cases in the Basing line given by Dr. Lardner in his table): Here, since $v = 25$ miles, and $h = 8$ feet: we find $v' = 29{\cdot}29$ miles per hour for the greatest velocity, and 27·11 miles per hour for the mean velocity; whereas, Dr. Lardner's formula gives the uniform velocity of descent 103·09 miles per hour.

The time of descent will, in like manner, by my formula be	$1^m\ 6^s$
By Dr. Lardner's formula........................	0 17

Taking the second example, of a slope of 1 in 250, and the length of plane half a mile, we have

$$h = 10\tfrac{1}{2} \text{ feet nearly.}$$

Whence v', the greatest velocity	$= 30\frac{1}{2}$ miles.
Mean velocity	$= 27\frac{1}{2}$
Time of descent t	$= 1^m\ 5^s$.
By Dr. Lardner's formula the velocity	= infinity.
Time of descent	= zero.

With these very wide differences in our results, it must be that one of the two solutions is wrong, and without saying which, I am, on my part, quite content to leave the decision to those whose minds have not already received a bias from preconceived notions of the forces. A locomotive power is rather a novel consideration in mechanics; and either Dr. Lardner or I have certainly taken a wrong view of the subject, that is, of the "fundamental law of gravitations." That the results as computed by Dr. Lardner's formula are inconsistent with practice there can be no doubt, and how that can be theoretically right which is practically wrong is rather difficult to conceive.

XVIII. *On Symbolic Notation, as applied to Mineralogy.* By H. J. Brooke, *Esq., F.R.S. F.L.S. F.G.S.*

To Richard Phillips, Esq.

Dear Sir,

I HAVE, at the repeated solicitation of several of my friends, undertaken and made some progress in a new work on Mineralogy, in the course of which some difficulties have occurred relative to the chemical constitution of minerals and their distribution into species, which perhaps some of your chemical readers may assist in removing; and on this account I shall feel obliged by an early insertion of this notice in your Journal.

Before I commenced the work in which I am engaged, I had thought of proposing a new edition of the late W. Phillips's useful volume; but upon a close examination of its contents I found it would require so much correction, and so great a remodelling, to adapt it to the present state of mineralogical knowledge, that it would be less troublesome to prepare a new treatise.

Notwithstanding the decided objection entertained by you to the use of symbols, I am disposed to regard them as very serviceable in œconomizing time and labour; and it will probably turn out that your objection applies rather to the unnecessary and capricious changes to which these short-hand characters have been subjected, and their employment in the expression of conflicting theories, than to the characters themselves.

Symbols were first introduced by Berzelius, which fully answered the purpose for which they were intended; and it would have been much better, and much confusion would have been spared, if they had been rigidly adhered to, notwithstanding any slight improvement of which they might have been susceptible. But, unfortunately, in the symbolical representations of the composition of minerals published by different authors, not only are the symbols of Berzelius changed, but the formulæ are made up according to the peculiar views of each author concerning atomic weights, and their several notions of the most fit manner of parcelling out into definite compounds such of those constituents of a mineral, as given by analysis, as they choose to consider essential to its constitution, after having rejected whatever they imagine to be foreign matter.

These differences will be apparent on a comparison of the analyses of different minerals, as given by Leonhard in his *Handbuch der Oryctognosie*, and by Thomson in his recently published work on Mineralogy, with the formulæ by which the composition of such minerals is expressed.

Hence arises the difficulty in selecting a chemical formula which shall accurately represent the chemical constitution of a mineral, and particularly as other symbolical expressions might, with a little contrivance, be framed to represent equally well the result of the same analysis. A question, therefore, occurs whether there is any rule to guide us in determining which formula is the most correct.

The doctrine of definite proportions is, as I understand it, applicable to all cases of chemical union. An electro-positive and an electro-negative element are, in all cases, to be regarded as the combining atoms, whether such elements are simple, or consist of binary, quaternary, or any other compound of simple elements. Thus, one proportional of oxalic acid and one of potash are the combining atoms in oxalate of potash; in binoxalate, two proportionals of the acid form the atom; and in quadroxalate four proportionals. But in what manner do these two and four proportionals constitute the combining atoms? Again, in sulphate of nickel and potash, are the two elements sulphates; or is sulphuric acid one, and the combined oxides the other? And what function does the water perform in hydrous salts, as it does not appear in the formulæ representing such? in what manner, or even whether it is combined with any, or which, of the elements of the compound in which it occurs?

On referring, however, to the last edition of Berzelius's Theory of Chemical Proportions, I find that the inquiry I have thus ventured to suggest will probably not produce a satisfactory answer; but as some chemists retain in their formulæ, and others reject, the same ingredient of a mineral, as shown by analysis, they perhaps have some rule by which they are guided, and which if worked out might furnish a clue to the object I am seeking. The passage from Berzelius is as follows: "Sulphuric acid, potash, alumina, and water are compound atoms of the *first* order; sulphate of potash and sulphate of alumina are of the *second* order; dry [anhydrous] alum, of the *third* order; and lastly, crystallized alum, containing water *combined with the double sulphate*, may be regarded as an atom of the *fourth* order. We are as yet ignorant of the extent of the number of these orders. Affinity among compound atoms decreases rapidly as the *numbers* of the orders increase; and the degree which exists even among atoms of the *third* order is too feeble to be observed in the operations of the laboratory. This affinity is seldom manifested, except in minerals; and to understand thoroughly the nature of these, it is necessary to know how far the combination of compound atoms can take place, and whether there is any limit to the *numbers* of the orders of the combining atoms."

Under these circumstances, it would have been better not to construct any mineralogical formulæ, except the simple ones; and in future the composition of such minerals as involve the *uncertain orders* of atoms should be expressed simply by the proportion of each ingredient in 1000th parts, or by the nearest equivalent numbers of each, taking the element which occurs in the smallest quantity as an atomic unit. We should then express by our formulæ what we know, instead of contriving to represent an imaginary atomic constitution, which, if the atomic theory be true, is probably in all cases false.

I am, dear Sir, yours truly,

January 7th, 1836. H. J. Brooke.

XIX. *On the Laws of Reflexion from crystallized Surfaces. By* J. MacCullagh, *Fellow of Trinity College, Dublin.*

To Sir David Brewster.

Dear Sir,

I HAVE great pleasure in sending you an account of the laws by which I conceive that the vibrations of light are regulated when a ray is reflected and refracted at the separating surface of two media; especially as the only guide which I had, in my inquiry after these laws, was your paper on the action of crystallized surfaces upon light, published in the Philosophical Transactions for the year 1819. The observation which I found there, that the polarizing angle was the same for a given plane of incidence, "whether the obtuse angle of the rhomb [of Iceland spar] was nearest or furthest from the eye, or whether it was to the right or left hand of the observer," disappointed me at first, being contrary to what I had anticipated from principles analogous to those which had been employed by Fresnel in the problem of reflexion from ordinary media. I then sought other principles, and the observation is now a result of theory.

Assuming, as a basis for calculation, that Fresnel's law of double refraction is rigorously true, I have been obliged to make an essential change in his physical ideas. Conceive an ellipsoid whose semiaxes are parallel to the three principal directions of the crystal, and equal respectively to its three principal indices of refraction, and let a central section of the ellipsoid be made by a plane parallel to the plane of a wave passing through the crystal. The section will be an ellipse, and the wave will be polarized by the crystal in a plane parallel to either semiaxis of this ellipse, the index of refraction for the wave being equal to the other semiaxis. This is Fresnel's law of double refraction; and the theory which led him to it makes it necessary to admit that the vibrations of the wave are perpendicular to its plane of polarization; whereas, according to the views which I have adopted, the vibrations of the wave are parallel to its plane of polarization, and to one semiaxis of the elliptic section, while its index of refraction is equal to the other semiaxis. These views nearly agree with the theory of M. Cauchy, according to whom the vibrations of polarized light are parallel to its plane of polarization, but inclined at small angles to the plane of the wave in crystallized media, instead of being exactly parallel to the latter plane, as I have supposed them to be. Besides, the theory of M. Cauchy, founded on the six equations of pressure in a crystallized medium, implies the existence of a third ray of feeble intensity, and for the other two rays gives a law somewhat different from that of Fresnel. Being obliged, in order to account for your experiments, to abandon the physical ideas of Fresnel, and to approximate towards those of M. Cauchy, I was embarrassed by this third ray; and wishing to get rid of it, as well as of the slight deviations from the symmetrical law of Fresnel, I adopted the expedient of altering the equations of pressure, in such a way as to make them afford only two rays, and give a law of refraction exactly the same as Fresnel's. The equations which I found to answer this purpose are the following:

$$A = -2\left(c^2 \frac{d\eta}{dy} + b^2 \frac{d\zeta}{dz}\right) V^2 \varrho,$$

$$B = -2\left(a^2 \frac{d\zeta}{dz} + c^2 \frac{d\xi}{dx}\right) V^2 \varrho,$$

$$C = -2\left(b^2 \frac{d\xi}{dx} + a^2 \frac{d\eta}{dy}\right) V^2 \varrho,$$

$$D = a^2\left(\frac{d\eta}{dz} + \frac{d\zeta}{dy}\right) V^2 \varrho,$$

$$E = b^2\left(\frac{d\xi}{dz} + \frac{d\zeta}{dx}\right) V^2 \varrho,$$

$$F = c^2\left(\frac{d\xi}{dy} + \frac{d\eta}{dx}\right) V^2 \varrho.$$

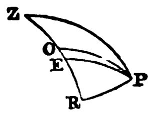
In these equations, the axes of coordinates are perpendicular to each other, and parallel to the three principal directions of the crystal; x, y, z are the coordinates of a vibrating molecule at the time t; ξ, η, ζ are the components of the displacement of the same molecule at the same time; a, b, c are the three principal indices of refraction out of the crystal into an ordinary medium in which the velocity of light is equal to V; and ϱ is the density of the æther, which density I suppose to be the same in all media. The quantities A, F, E are the components, parallel to the axes of x, y, z respectively, of the pressure upon a plane perpendicular to the axis of x; F, B, D are the components of the pressure upon a plane perpendicular to the axis of y; and E, D, C, the components of the pressure upon a plane perpendicular to the axis of z. The values of D, E, F are the same as those given by M. Cauchy; but the values of A, B, C are different from his, and much simpler. By introducing into the equations of M. Cauchy the condition that the vibrations shall be performed without any change of density, the resulting values of A, B, C might be shown to agree nearly with those given above. The six pressures, A, B, C, D, E, F, being known, it is easy to find the pressure upon a plane making any given angles with the axes of coordinates.

These things being premised, it is time to mention the laws, or rather hypotheses, which I have imagined for discovering the relations that exist, as to direction and magnitude, among the vibrations in each ray, when reflexion and refraction take place at the separating surface of two media, whether crystallized or not. In stating the two very simple laws that have occurred to me for this purpose, it will be convenient, when the first medium is an ordinary one, to suppose that the incident light is polarized. Then by the first law, *the vibrations in one medium are equivalent to those in the other*; that is to say, if the incident and reflected vibrations be compounded, like forces acting at a point, their resultant will be the same, both in length and direction, as the resultant of the refracted vibrations similarly compounded. By the second law, *the lateral pressure upon the separating surface is the same in both media*; the lateral pressure being understood to mean the pressure in a direction perpendicular to the plane of incidence.

As it would engage us too long to follow these laws into detail, I shall merely state some of the results which I have obtained from them, for the case of a uniaxal crystal into which the light passes out of an ordinary medium.

Imagine the surface of the crystal to be horizontal, and call the point of incidence I. With the centre I and any radius, conceive a sphere to be described, cutting in the point Z a vertical line I Z drawn through the centre, and let a radius

I P, parallel to the axis of the crystal, meet the surface of the sphere in P. Let the great circle Z O E be the plane of incidence, containing both the direction I O of the ordinary refracted ray produced backwards, and the direction I E of a normal to the extraordinary wave; and draw the great circles P Z, P O, P E. The angle Z will be the azimuth of the plane of incidence. Let Z O $= \phi$, Z E $= \phi'$, P O $= \psi$, P E $= \psi'$, the angle Z O P $= \theta$, and the angle Z E P $= \theta'$. Call the angle of incidence i, and suppose b to be the reciprocal of the ordinary refractive index, and a the reciprocal of the extraordinary.



Each of the refracted rays, in turn, may be made to disappear, by polarizing the incident ray in a certain direction assigned by theory. When the extraordinary ray disappears, the reflected ray is polarized in a plane inclined to the plane of incidence at an angle β determined by the formula

$$\tan\beta = \cos(i+\phi)\tan\theta + 2(a^2-b^2)\sin\theta\sin\psi\cos\psi\,\frac{\sin^2 i}{\sin(i-\phi)} \quad (2).$$

When the ordinary ray disappears, the plane of polarization of the reflected ray is inclined to the plane of incidence at an angle β' determined by the formula

$$-\tan\beta' = \cos(i+\phi')\cot\theta' + (a^2-b^2)\frac{\cos 2\theta'}{\sin\theta'}\sin\psi'\cos\psi'\,\frac{\sin^2 i}{\sin(i-\phi')}. \quad (3.)$$

And when the angles β, β', become equal, the plane of polarization of the reflected ray becomes independent of the plane of polarization of the incident ray; and the angle of incidence i, at which this equality takes place, is the polarizing angle of the crystal. Hence we have the equation of condition

$$\left.\begin{aligned}&\cos(i+\phi)\tan\theta + 2(a^2-b^2)\sin\theta\sin\psi\cos\psi\,\frac{\sin^2 i}{\sin(i-\phi)}\\ &+\cos(i+\phi')\cot\theta' + (a^2-b^2)\frac{\cos 2\theta'}{\sin\theta'}\sin\psi'\cos\psi'\,\frac{\sin^2 i}{\sin(i-\phi')}\end{aligned}\right\} = 0. \quad (4.)$$

to be fulfilled at the polarizing angle.

Since $i+\phi$, in this equation, is nearly equal to a right angle, put $i+\phi = \frac{\pi}{2} + \delta$, and δ will be a small quantity. Draw P R an arc of a great circle perpendicular to Z O E, and let

$ZR = p$, $PR = q$. Then we shall find from equation (4.), after various substitutions and reductions,

$$\delta = K \cos^2 q\,(\cos^2 \phi - \cos^2 p); \text{ where } K = \frac{(a^2-b^2)\,(1+b^2)}{2\,b\,(1-b^2)}. \quad (5.)$$

In deducing this value of δ, the approximations were made with a tacit reference to the case of reflexion in air from a common rhomb of Iceland spar. The coefficient K, in this case, is equal to about nine degrees, and the resulting numerical values of the polarizing angles in various azimuths agree very well with your experiments. You will perceive that the value of δ is the same in supplementary azimuths, which explains the observation, cited in the beginning of my letter, relative to the equality of the polarizing angles at opposite sides of the perpendicular I Z in a given plane of incidence.

When the point R falls upon O, we have $\delta = 0$, and $i+\phi$ equal to a right angle. Hence, when the cotangent of Z R is equal to the ordinary index, the tangent of the polarizing angle is equal to the same index. This theorem, though deduced from an approximate equation, might be shown to be exact.

When the axis of the crystal lies in the plane of incidence, we may obtain an exact expression for the polarizing angle. The condition of polarization then becomes

$$\cos(i+\phi') - (a^2-b^2)\sin\psi'\cos\psi'\,\frac{\sin^2 i}{\sin(i-\phi')} = 0; \quad (6.)$$

from which, by the proper substitutions, we obtain the following expression:

$$\sin^2 i = \frac{1-a^2\cos^2\lambda - b^2\sin^2\lambda}{1-a^2 b^2}; \quad (7.)$$

where λ denotes the complement of Z P, or the inclination of the axis to the face of the crystal, and i is the polarizing angle. This formula, in a shape somewhat different, was communicated, above a year ago, to Professor Lloyd, who has noticed it, in connexion with your paper, in his Report on Physical Optics. When a and b become equal, the formula gives your law of the tangent for ordinary media.

The foregoing results show that, when a ray is polarized by reflexion from a crystal, the plane of polarization deviates from the plane of incidence, except when the axis lies in the latter plane; and that the deviation may be made very great by placing the crystal in contact with a medium whose refractive power is nearly equal to that of the crystal itself; for when i is nearly equal to ϕ or to ϕ', the divisor sin $(i-\phi)$ or sin $(i-\phi')$ is very small, and therefore tan β or tan β' is very great. But this remark is of no value whatever in explaining the very singular phænomena which you have observed in the extreme case just mentioned; nor can I imagine any reason why there should be a deviation, as there was in some of your experiments, when the axis lies in the plane of incidence, since everything is then alike on both sides of this plane. Indeed the whole of this subject, which occupies the latter part of your paper of 1819, is very extraordinary and interesting; and I was glad to hear that you had resumed the investigation of it, and made many experiments which have not been published.

I wish you would publish them. They seem to be of great importance in the present state of optical science.

I am, dear Sir, ever truly yours,

Trin. Coll. Dublin, Dec. 22, 1835. J. MacCullagh.

XX. *Some further Remarks on the Magnetic Forces. By* R. W. Fox.*

I AM glad that Dr. Ritchie has noticed my remarks on the laws of the magnetic forces, because I hope that it will be the means of exciting more attention to the subject. I cannot, however, admit the justness of his conclusion, unless it can be shown that the results of my experiments are conformable to the law of the inverse of the squares of the distances throughout the *whole series* of nine or ten removals of the magnet, calculating from any assumed points whatever in them. Dr. Ritchie has confined his calculations to only two or three distances.

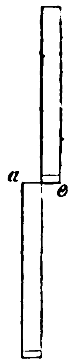
The magnets which I employed were cylinders of three inches long and one tenth of an inch in diameter, and attracted each other with half the force of contact when separated about $\frac{1}{2000}$ of an inch. From this minute distance to that of $\frac{1}{8}$ and even $\frac{1}{4}$ of an inch, the results were nearly† in accordance with the law of the *simple inverse ratio of the distance*, calculating from the contiguous surfaces of the magnets; and when the same bars were made more strongly magnetic, their force, at half an inch, much more nearly approximated to the *simple*, than to the *duplicate*, inverse ratio of the distance.

* Communicated by the Author.

† I have used this qualifying word, because at very minute distances the diminution of the force did not seem to be quite equal to the inverse ratio of the distance; whereas it rather exceeded it towards the end of the series. At the distance of $\frac{1}{4}$ of an inch, the force, in the case referred to, was about $\frac{1}{1750}$ of that of contact.

Now, the question is, will it be possible to reconcile these facts with the latter law?

I found that when the magnets referred to, adhered to each other at their terminal *edges* only, as shown in the annexed figure, it required a much greater force to separate them than when the two surfaces, or ends *a*, *e*, were together. This may, perhaps, be attributed to a better contact in the former case than in the latter; and I conceive that if the contact had been still more perfect, the force would have been reduced one half at less than $\frac{1}{2000}$ of an inch. The fact, moreover, proves, I think, that the greatest force of magnets, when in contact at their dissimilar poles, is not fixed in their axes, or at any appreciable distance from their points of junction. Under these circumstances the distribution of their united forces is, I consider, much the same as it is in the case of a single magnet.



XXI. *Remarks on* M. Melloni's *and* Professor Powell's *Papers on the Transmission of Calorific Rays, inserted in Lond. and Edinb. Phil. Mag. for December* 1835 *and January* 1836. *By* H. Hudson, *M.D., M.R.I.A.**

IN referring to the remarks which M. Melloni has done me the honour to make relative to a communication which I read to the Physical Section of the British Association†, I beg to say, that I stated in the paper that I considered my experiments with the thermo-multiplier as *imperfect*, and *merely* mentioned them as *indicating* a method of determining a point on which some doubts were still entertained.

I have since adopted the method alluded to, which has fully confirmed the correctness of M. Melloni's observations; and as it appears capable of being advantageously employed in some branches of the investigation, I shall briefly describe it. A B C D is a square, mahogany board, on which two rectangular brass plates (each 4 inches wide), E F G H and F I L K, are fixed (as in fig. 1, p. 110,) at right angles to each other and divided into half inches; at C M I O a plate of brass is fixed perpendicular to the board, forming a complete screen to T (the thermo-electric pile), except through a square opening (at N) equal to the section of the pile. A second brass screen E I is fixed in like manner perpendicular to the board, and

* Communicated by the Author.

† An abstract of the communication here referred to will be found in Lond. and Edinb. Phil. Mag., vol. vii. p. 297.—Edit.

forming at its base the hypothenuse of the right-angled isosceles triangle I F E; in this second brass screen there is a circular opening (P), about $2\frac{1}{2}$ inches in diameter, whose centre is immediately opposite to the centre of the square hole (N); both centres being in the *axis* of the pile, which is parallel to P Q, the central line of the brass plate E G; to this circular opening a brass circle is attached (moveable round its centre and) having four screws (as in figure 2.) by means of which any substance intended to be used as a screen may be firmly fixed in the circle. Brass plates are made which fit tightly into the circle, having rectangular pieces (of different sizes) cut out of their centres.

Fig. 1.

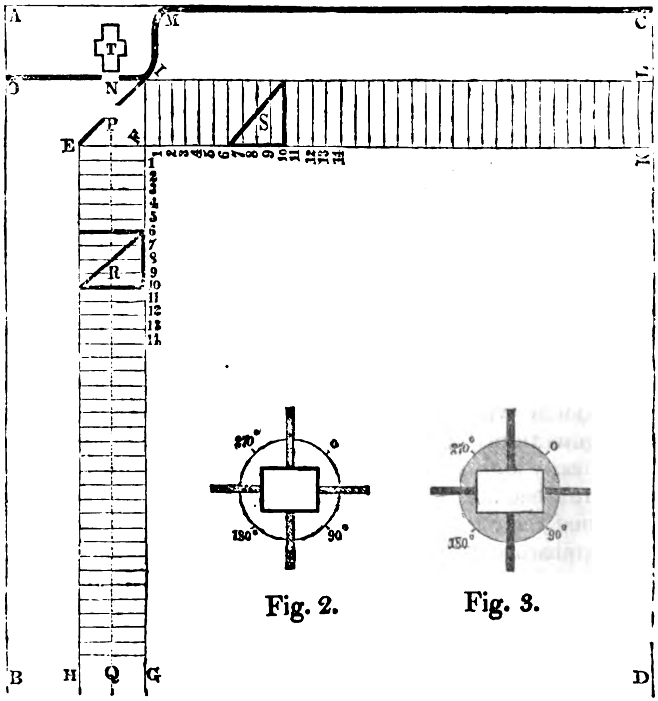


Fig. 2. Fig. 3.

Having selected one of these plates (in which the opening is somewhat smaller than the crystal, or other substance intended to be used as a screen) it is fixed into the circle behind the screws (as in figure 3.) and the crystal is then fixed in front of it so as to completely cover the opening; the bottom of the canister (for containing hot water, &c.) is a right-angled isosceles triangle, whose equal sides measure four inches, so that the canister being placed alternately in the different positions at R and S, it is evident that the lines drawn from any point of the radiating surface to a given point of the crystal are of equal length, and form the same angle with the crystal and canister in each case; while the other sides of the canister cannot interfere with the effect. Under these circumstances it would appear that any effect produced on the thermo-multiplier by the mere *heating* of the crystal should be precisely the same in both cases; and any *excess* of effect in the position R may be taken as the measure of the crystal's diathermancy for the kind of heat which the canister radiates. The circle (in which the crystal is fixed) being moveable, the experiment can be repeated after turning the crystal round through 180° so as to verify the result.

Having made use of the apparatus above described, in addition to its furnishing abundant confirmation of M. Melloni's statements, I think I have obtained a proof that rock crystal (about $\frac{1}{8}$th of an inch thick) and other bodies, which are usually considered wholly impervious to the heat radiated from bodies at low temperatures, *do transmit* heat from a canister containing hot water, although the effect is obscured by the rapidity with which the crystal absorbs heat. I first put the canister as at S, and the needle almost (if not altogether) *instantaneously* begins to move with a slow but rather steady motion, and at length stops (say) at 5°. The canister being removed the needle soon settles at zero again. The canister is then placed at R, and immediately the needle begins to move (with more *energy*, however, than in the former case), and goes to $5\frac{1}{2}$°, from which it again *returns* to about 3°, and ultimately settles at 5°, as when the canister was at S.

Thus, though there is no *perceptible* difference in the *statical* effect, there appears to be a force acting on the needle at R (producing a larger arc of vibration and in a shorter time) which, I believe, can only be attributed to rays of heat transmitted through the crystal.

With regard to Professor Powell's remarks on M. Melloni's paper, in the Lond. and Edinb. Phil. Mag. for December, it appaers to me that the learned Professor has in some degree misapprehended M. Melloni's observations. Professor Powell's ingenious experiment went to prove that rays of heat issuing from a *luminous* heated body were transmitted freely, while those from a *non*-luminous source were apparently not so transmitted.

The *general* fact on which Professor Powell founded the distinction M. Melloni admits, but he maintains (and on grounds which I believe to be incontestably true) that the *distinction* is not between *luminous* and *obscure* sources of heat, but between the kind of rays of heat emitted from bodies at different *temperatures*; and that the *accident* (as I may term it) of the bodies being at such a temperature that rays of light accompanied the rays of heat, has nothing whatever to do with the fact of the different transmissibilities of the calorific rays: 1st, Because the same difference of transmission exists between sources altogether *obscure*; 2nd, Because this *difference* (between luminous and obscure sources) does not exist with reference to some bodies, *e.g. rock salt*; and, thirdly, in bodies emitting light, the quantity of heat transmitted is in no way *proportional*, *either* to the degree of light which accompanies it in the first instance, or to the quantity of light which passes through along with it.

In reference to this subject, it is to be observed, that it is altogether erroneous to consider "diathermancy" in the science of heat as *analogous* to "transparency" in optics; for that property of bodies by which they *stop* (absorb) or *transmit* rays of a *particular refrangibility or colour* is the true analogue in the latter science.

I suspect that this necessary distinction escaped Professor Powell's attention when he alluded to M. Melloni's *hypothesis* as "needless and contrary to all analogy;" for in this view of the subject, the explanation which M. Melloni has given of the heat being more abundantly transmitted through successive plates (of similar natures) is *perfectly analogous* to the effect of a succession of screens (of the *same colour*) on common light. The "diathermancy" of *rock salt* alone appears entitled to be compared with "transparency" as used in optics.

Stephen's Green, Dublin, Jan. 9, 1836.

XXII. *On the Theory of Dispersion. By the* Rev. B. Powell, *MA., F.R.S., Sav. Prof. of Geometry, Oxford.*

LEARNING that there is not room in this Number for the continuation I had proposed of the researches commenced in the last, I am anxious meanwhile to make a brief remark on two points referred to in the last Number.

I. Mr. Tovey in an excellent paper on the formula for dispersion, introduces a most material simplification on M. Cauchy's process. I allude to this more particularly now, because the writer refers to the importance of not omitting the summation. He will, I trust, find that the introduction of it as discussed in my paper (and still more when the continuation appears,) will produce an entire accordance in our results.

II. In the Editors' note appended to my paper (p. 28.) there is a reference to some investigations of M. Rudberg, published in a former volume of this Journal. I ought, perhaps, to have referred to those curious researches at an earlier period: but it will readily appear that they are quite distinct from mine. The author states at the commencement of his paper: "In investigating the principle, according to which, for the explanation of the dispersion of light on the system of undulations, we must suppose that when the light passes from the air into a more refractive medium the length of the undulations are much contracted, in fact, much shorter,—I have found that the following relation appears to exist between the length of the undulation of a certain colour in the air and the corresponding one in any other substance:" $L = a \cdot l^m$; l being the length in air, L in the medium, a and m constant depending on the medium.

He then takes Fraunhofer's value of l for each ray, and assuming that they are diminished in proportion to the refractive power, proceeds to calculate L for the different media examined by Fraunhofer: and thence the refractive indices by the formula, which on this assumption follows from the former (N being the index)

$$N = \frac{1}{a\,l^{m-1}},$$

and the resulting numbers certainly exhibit a very good agreement with those of observation.

Now, with regard to the nature of the formula, it is to be observed that the author neither gives any theory from which it is deduced, nor a reference to any other paper, or investigation of such theory; and the form of it is such as would appear extremely unlikely to have any connexion with the analysis of undulations. Again, had any such investigation either of the author or of any other philosopher been in existence, it is hardly conceivable that it could have remained since 1827, without becoming known to some, at least, of the numerous mathematicians in all parts of Europe who have since that period been directing their attention to the subject.

But further, (unless I greatly mistake the author's meaning,) it appears to me from the very form of expression used in the passage above quoted, that *the formula is not derived from any theory of undulations*: for when, he says, "In investigating, &c...... I have found, ..." the meaning really seems to me simply this; that in *attempting* such an investigation on the undulatory theory he had *not* been able to *succeed* in obtaining any *theoretical relation* between the velocity of a wave, and its length: but that EMPIRICALLY he found upon a comparison of numerical results, "that this relation appears to subsist," which is expressed by the above formula. Such is the impression which the passage conveys to my mind; and, indeed, the tenor of the whole confirms it. I can only add, that I should be truly glad to have pointed out any deduction *from theory* which would give so simple a formula.

Considered as an *empirical law*, it certainly merits great attention. But thus much will be at once evident,—it is totally *independent* of M. Cauchy's *principles*, and of my *results* deduced and calculated on those principles.

Oxford, Jan. 6, 1836.

XXIII. *Experimental Researches in Electricity.—Tenth Series. By* MICHAEL FARADAY, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.**

§ 16. *On an improved form of the Voltaic Battery.* § 17. *Some practical results respecting the construction and use of the Voltaic Battery.*

1119. I HAVE lately had occasion to examine the voltaic trough practically, with a view to improvements in its construction and use; and though I do not pretend that the results have anything like the importance which attaches to the discovery of a new law or principle, I still think they are valuable, and may therefore, if briefly told, and in connexion with former papers, be worthy the approbation of the Royal Society.

§ 16. *On an improved form of the Voltaic Battery.*

1120. In a simple voltaic circuit (and the same is true of the battery) the chemical forces which, during their activity, give power to the instrument, are generally divided into two portions; the one of these is exerted locally, whilst the other is transferred round the circle (947. 996.†); the latter constitutes the electric current of the instrument, whilst the former is altogether lost or wasted. The ratio of these two portions of power may be varied to a great extent by the influence of circumstances: thus, in a battery not closed, *all* the action is local; in one of the ordinary construction, *much* is in circula-

* From the Philosophical Transactions for 1835, Part II. This paper was received by the Royal Society June 16th, and read June 18th, 1835.

[† The paragraphs of the author's former series of Researches here referred to, from 875 to 1047 both inclusive, belong to his Eighth Series, reprinted in Lond. and Edinb. Phil. Mag., vol. vi. p. 34 *et seq.*—EDIT.]

tion when the extremities are in communication; and in the perfect one, which I have described (1001.), *all* the chemical power circulates and becomes electricity. By referring to the quantity of zinc dissolved from the plates (865.* 1126.), and the quantity of decomposition effected in the volta-electrometer (711. 1126.) or elsewhere, the proportions of the local and transferred actions under any particular circumstances can be ascertained, and the efficacy of the voltaic arrangement, or the waste of chemical power at its zinc plates, be accurately determined.

1121. If a voltaic battery were constructed of zinc and platina, the latter metal surrounding the former, as in the double copper arrangement, and the whole being excited by dilute sulphuric acid, then no insulating divisions of glass, porcelain, or air would be required between the contiguous platina surfaces; and, provided these did not touch metallically, the same acid which, being between the zinc and platina, would excite the battery into powerful action, would, between the two surfaces of platina, produce no discharge of the electricity, nor cause any diminution of the power of the trough. This is a necessary consequence of the resistance to the passage of the current which I have shown occurs at the place of decomposition (1007. 1011.); for that resistance is fully able to stop the current, and therefore act as insulation to the electricity of the contiguous plates, in as much as the current which tends to pass between them never has a higher intensity than that due to the action of a single pair.

1122. If the metal surrounding the zinc be copper (1045.), and if the acid be nitrosulphuric acid (1020.), then a slight discharge between the two contiguous coppers does take place, provided there be no other channel open by which the forces may circulate: but when such a channel is permitted, the return discharge of which I speak is exceedingly diminished, in accordance with the principles laid down in the eighth series of these Researches.

1123. Guided by these principles I was led to the construction of a voltaic trough, in which the coppers, passing round both surfaces of the zincs, as in Wollaston's construction, should not be separated from each other except by an intervening thickness of paper, or in some other way, so as to prevent metallic contact, and should thus constitute an instrument compact, powerful, economical, and easy of use. On examining, however, what had been done before, I found that the

[* The paragraphs referred to from 661 to 874 will be found in Mr. Faraday's Seventh Series, reprinted in Lond. and Edinb. Phil. Mag., vol. v. p. 161, *et seq.*—Edit.]

new trough was in all essential respects the same as that invented and described by Dr. Hare, Professor in the University of Pennsylvania, to whom I have great pleasure in referring it.

1124. Dr. Hare has fully described his trough*. In it the contiguous copper plates are separated by thin veneers of wood, and the acid is poured on to, or off, the plates by a quarter revolution of an axis, to which both the trough containing the plates, and another trough to collect and hold the liquid, are fixed. This arrangement I have found the most convenient of any, and have therefore adopted it. My zinc plates were cut from rolled metal, and when soldered to the copper plates had the form delineated, fig. 1. These were then bent over a gauge into the form fig. 2., and when packed in the wooden box constructed to receive them, were arranged as in fig. 3†, little plugs of cork being used to keep the zinc

Fig. 1.

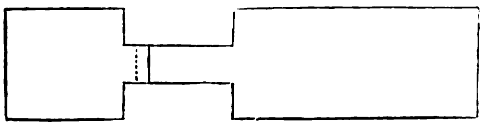


Fig. 2.

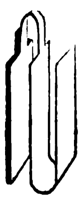
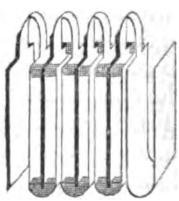


Fig. 3.



plates from touching the copper plates, and a single or double thickness of cartridge paper being interposed between the contiguous surfaces of copper to prevent them from coming in contact. Such was the facility afforded by this arrangement, that a trough of forty pairs of plates could be unpacked in five minutes, and repacked again in half an hour; and the whole series was not more than fifteen inches in length.

1125. This trough, of forty pairs of plates three inches

* Philosophical Magazine, 1824, vol. lxiii. p. 241; or Silliman's Journal, vol. vii. See also a previous paper by Dr. Hare, Annals of Philosophy [Second Series], 1821, vol. i. p. 329, [also Phil. Mag., First Series, vol. lvii. p. 284.] in which he speaks of the non-necessity of insulation between the coppers.

† The papers between the coppers are, for the sake of distinctness, omitted in the figure.

square, was compared, as to the ignition of a platina wire, the discharge between points of charcoal, the shock on the human frame, &c., with forty pairs of four-inch plates having double coppers, and used in porcelain troughs divided into insulating cells, the strength of the acid employed to excite both being the same. In all these effects the former appeared quite equal to the latter. On comparing a second trough of the new construction, containing twenty pairs of four-inch plates, with twenty pairs of four-inch plates in porcelain troughs, excited by acid of the same strength, the new trough appeared to surpass the old one in producing these effects, especially in the ignition of wire.

1126. In these experiments the new trough diminished in its energy much more rapidly than the one on the old construction; and this was a necessary consequence of the smaller quantity of acid used to excite it, which in the case of the forty pairs new construction was only one seventh part of that used for the forty pairs in the porcelain troughs. To compare, therefore, both forms of the voltaic trough in their decomposing powers, and to obtain accurate data as to their relative values, experiments of the following kind were made. The troughs were charged with a known quantity of acid of a known strength; the electric current was passed through a volta-electrometer (711.) having electrodes 4 inches long and 2·3 inches in width, so as to oppose as little obstruction as possible to the current; the gases evolved were collected and measured, and gave the quantity of water decomposed. Then the whole of the charge used was mixed together, and a known part of it analysed, by being precipitated and boiled with excess of carbonate of soda, and the precipitate well washed, dried, ignited, and weighed. In this way the quantity of metal oxidized and dissolved by the acid was ascertained; and the part removed from each zinc plate, or from all the plates, could be estimated and compared with the water decomposed in the volta-electrometer. To bring these to one standard of comparison, I have reduced the results so as to express the loss at the plates in equivalents of zinc for the equivalent of water decomposed at the volta-electrometer: I have taken the equivalent number of water as 9, and of zinc as 32·5, and have considered 100 cubic inches of the mixed oxygen and hydrogen, as they were collected over a pneumatic trough, to result from the decomposition of 12·68 grains of water.

1127. The acids used in these experiments were three,—sulphuric, nitric, and muriatic. The sulphuric acid was strong oil of vitriol; one cubical inch of it was equivalent to 486 grains of marble. The nitric acid was very nearly pure; one cubical inch dissolved 150 grains of marble. The muriatic acid was also nearly pure, and one cubical inch dissolved 108 grains of marble. These were always mixed with water by volumes, the standard of volume being a cubical inch.

1128. An acid was prepared consisting of 200 parts water, $4\frac{1}{2}$ parts sulphuric acid, and 4 parts nitric acid; and with this both my trough, containing forty pairs of three-inch plates, and four porcelain troughs, arranged in succession, each containing ten pairs of plates with double coppers four inches square, were charged. These two batteries were then used in succession, and the action of each was allowed to continue for twenty or thirty minutes, until the charge was nearly exhausted, the connexion with the volta-electrometer being carefully preserved during the whole time, and the acid in the troughs occasionally mixed together. In this way the former trough acted so well, that for each equivalent of water decomposed in the volta-electrometer only from 2 to 2·5 equivalents of zinc were dissolved from each plate. In four experiments the average was 2·21 equivalents for each plate, or 88·4 for the whole battery. In the experiments with the porcelain troughs, the equivalents of consumption at each plate were 3·54, or 141·6 for the whole battery. In a perfect voltaic battery of forty pairs of plates (991. 1001.) the consumption would have been one equivalent for each zinc plate, or forty for the whole.

1129. Similar experiments were made with two voltaic batteries, one containing twenty pairs of four-inch plates, arranged as I have described (1124.), and the other twenty pairs of four-inch plates in porcelain troughs. The average of five experiments with the former was a consumption of 3·7 equivalents of zinc from each plate, or 74 from the whole: the average of three experiments with the latter was 5·5 equivalents from each plate, or 110 from the whole: to obtain this conclusion, two experiments were struck out, which were much against the porcelain troughs, and in which some unknown deteriorating influence was supposed to be accidentally active. In all the experiments, care was taken not to compare *new* and *old* plates together, as that would have introduced serious errors into the conclusions (1146.).

1130. When ten pairs of the new arrangement were used, the consumption of zinc at each plate was 6·76 equivalents, or 67·6 for the whole. With ten pairs of the common construction, in a porcelain trough, the zinc oxidized was, upon an average, 15·5 equivalents each plate, or 155 for the entire trough.

1131. No doubt, therefore, can remain of the equality or even the great superiority of this form of voltaic battery over the best previously in use, namely, that with double coppers, in which the cells are insulated. The insulation of the coppers may therefore be dispensed with; and it is that circumstance which principally permits of such other alterations in the construction of the trough as gives it its practical advantages.

1132. The advantages of this form of trough are very numerous and great. i. It is exceedingly compact, for 100 pairs of plates need not occupy a trough of more than three feet in length. ii. By Dr. Hare's plan of making the trough turn upon copper pivots which rest upon copper bearings, the latter afford *fixed* terminations; and these I have found it very convenient to connect with two cups of mercury, fastened in the front of the stand of the instrument. These fixed terminations give the great advantage of arranging an apparatus to be used in connexion with the battery *before* the latter is put into action. iii. The trough is put into readiness for use in an instant, a single jug of dilute acid being sufficient for the charge of 100 pairs of four-inch plates. iv. On making the trough pass through a quarter of a revolution, it becomes active, and the great advantage is obtained of procuring for the experiment the effect of the *first contact* of the zinc and acid, which is twice or sometimes even thrice that which the battery can produce a minute or two after (1036. 1150.). v. When the experiment is completed, the acid can be at once poured from between the plates, so that the battery is never left to waste during an unconnected state of its extremities; the acid is not unnecessarily exhausted; the zinc is not uselessly consumed; and, besides avoiding these evils, the charge is mixed and rendered uniform, which produces a great and good result (1039.); and, upon proceeding to a second experiment, the important effect of *first contact* is again obtained. vi. The saving of zinc is very great. It is not merely that, whilst in action, the zinc performs more voltaic duty (1128. 1129.), but *all* the destruction which takes place with the ordinary forms of battery between the experiments is prevented. This saving is of such extent that I estimate the zinc in the new form of battery to be thrice as effective as that in the ordinary form. vii. The importance of this saving of metal is not merely that the value of the zinc is saved, but that the battery is much lighter and more manageable; and also that the surfaces of the zinc and copper plates may be brought much nearer to each other when the battery is constructed, and remain so until it is worn out: the latter is a very important advantage (1148.). viii. Again, as, in consequence of the saving, thinner plates will perform the duty of thick ones, rolled zinc may be used; and I have found rolled zinc superior to cast zinc in action; a superiority which I incline to attribute to its greater purity (1144.). ix. Another advantage is obtained in the economy of the acid used, which is proportionate to the diminution of the zinc dissolved. x. The acid also is more easily exhausted, and is in such small quantity that there is never any occasion to return an old charge into use. Such old acid, whilst out of use, often dissolves portions of copper from the black flocculi usually mingled with it, which are derived from the zinc; now any portion of copper in solution in the charge does great harm, because, by the *local* action of the acid and zinc, it tends to precipitate upon the latter, and diminish its voltaic efficacy (1145.). xi. By using a due mixture of nitric and sulphuric acid for the charge (1139.), no gas is evolved from the troughs; so that a battery of several hundred pairs of plates may, without inconvenience, be close to the experimenter. xii. If, during a series of experiments, the acid becomes exhausted, it can be withdrawn, and replaced by other acid with the utmost facility; and after the experiments are concluded, the great advantage of easily washing the plates is at command. And it appears to me, that in place of making, under different circumstances, mutual sacrifices of comfort, power, and economy, to obtain a desired end, all are at once obtained by Dr. Hare's form of trough.

1133. But there are some disadvantages which I have not yet had time to overcome, though I trust they will finally be conquered. One is the extreme difficulty of making a wooden trough constantly water-tight under the alternations of wet and dry to which the voltaic instrument is subject. To remedy this evil, Mr. Newman is now engaged in obtaining porcelain troughs. The other disadvantage is a precipitation of copper on the zinc plates. It appears to me to depend mainly on the circumstance that the papers between the coppers retain acid when the trough is emptied; and that this acid slowly acting on the copper, forms a salt, which gradually mingles with the next charge, and is reduced on the zinc plate by the local action (1120.): the power of the whole battery is then reduced. I expect that by using slips of glass to separate the coppers at their edges, their contact can be sufficiently prevented, and the space between them be left so open that the acid of a charge can be poured and washed out, and so be removed from *every part* of the trough when the experiments in which it is used are completed.

1134. The actual superiority of the troughs which I have constructed on this plan, I believe to depend, first and principally, on the closer approximation of the zinc and copper surfaces;—in my troughs they are only one tenth of an inch apart (1148.);—and, next, on the superior quality of the rolled zinc above the cast zinc used in the construction of the ordinary pile. It cannot be that insulation between the contiguous coppers is a disadvantage, but I do not find that it is any advantage; for when, with both the forty pairs of three-inch plates and the twenty pairs of four-inch plates, I used papers well imbibed with wax*, these being so large that when folded at the edges they wrapped over each other, so as to make cells as insulating as those of the porcelain troughs, still no sensible advantage in the chemical action was obtained.

1135. As, upon principle, there must be a discharge of part of the electricity from the edges of the zinc and copper plates at the sides of the trough, I should prefer, and intend having, troughs constructed with a plate or plates of crown glass at the sides of the trough: the bottom will need none, though to glaze that and the ends would be no disadvantage. The plates need not be fastened in, but only set in their places; nor need they be in large single pieces.

§ 17. *Some practical results respecting the construction and use of the Voltaic Battery.*

1136. The electro-chemical philosopher is well acquainted with some practical results obtained from the voltaic battery by MM. Gay-Lussac and Thenard, and given in the first forty-five pages of their *Recherches Physico-chimiques.* Although the following results are generally of the same nature, yet the advancement made in this branch of science of late years, the knowledge of the definite action of electricity, and the more accurate and philosophical mode of estimating the results by the equivalents of zinc consumed, will be their sufficient justification.

1137. *Nature and strength of the acid.*—My battery of forty pairs of three-inch plates was charged with acid consisting of 200 parts water and 9 oil of vitriol. Each plate lost, in the average of the experiments, 4·66 equivalents, or the whole battery 186·4 equivalents, of zinc, for the equivalent of water decomposed in the volta-electrometer. Being charged with a mixture of 200 water and 16 of the muriatic acid, each plate lost 3·8, or the whole battery 152, equivalents of zinc for the water decomposed. Being charged with a mixture of 200 water and 8 nitric acid, each plate lost 1·85, or the whole battery 74·16, equivalents of zinc for one equivalent of water decomposed. The sulphuric and muriatic acids evolved much hydrogen at the plates in the trough; the nitric acid no gas whatever. The relative strengths of the original acids have already been given (1127.); but a difference in that respect

* A single paper thus prepared could insulate the electricity of a trough of forty pairs of plates.

makes no important difference in the results when thus expressed by equivalents (1140.).

1138. Thus nitric acid proves to be the best for this purpose: its superiority appears to depend upon its favouring the electrolization of the liquid in the cells of the trough upon the principles already explained (905. 973. 1022.), and consequently favouring the transmission of the electricity, and therefore the production of transferable power (1120.).

1139. The addition of nitric acid might, consequently, be expected to improve sulphuric and muriatic acids. Accordingly, when the same trough was charged with a mixture of 200 water, 9 oil of vitriol, and 4 nitric acid, the consumption of zinc was at each plate 2·786, and for the whole battery 111·5 equivalents. When the charge was 200 water, 9 oil of vitriol, and 8 nitric acid, the loss per plate was 2·26, or for the whole battery 90·4, equivalents. When the trough was charged with a mixture of 200 water, 16 muriatic acid, and 6 nitric acid, the loss per plate was 2·11, or for the whole battery 84·4, equivalents. Similar results were obtained with my battery of twenty pairs of four-inch plates (1129.). Hence it is evident that the nitric acid was of great service when mingled with the sulphuric acid; and the charge generally used after this time for ordinary experiments consisted of 200 water, 4½ oil of vitriol, and 4 nitric acid.

1140. It is not to be supposed that the different strengths of the acids produced the differences above; for within certain limits I found the electrolytic effects to be nearly as the strengths of the acids, so as to leave the expression of force, when given in equivalents, nearly constant. Thus, when the trough was charged with a mixture of 200 water and 8 nitric acid, each plate lost 1·854 equivalent of zinc. When the charge was 200 water and 16 nitric acid, the loss per plate was 1·82 equivalent. When it was 200 water and 32 nitric acid, the loss was 2·1 equivalents. The differences here are not greater than happen from unavoidable irregularities, depending on other causes than the strength of acid.

1141. Again, when a charge consisting of 200 water, 4½ oil of vitriol, and 4 nitric acid was used, each zinc plate lost 2·16 equivalents; when the charge with the same battery was 200 water, 9 oil of vitriol, and 8 nitric acid, each zinc plate lost 2·26 equivalents.

1142. I need hardly say that no copper is dissolved during the regular action of the voltaic trough. I have found that much ammonia is formed in the cells when nitric acid, either pure or mixed with sulphuric acid, is used. It is produced in part as a secondary result at the cathodes (663.) of the different portions of fluid constituting the necessary electrolyte, in the cells.

1143. *Uniformity of the charge.*—This is a most important point, as I have already shown experimentally (1042. &c.). Hence one great advantage of Dr. Hare's mechanical arrangement of his trough.

1144. *Purity of the zinc.*—If pure zinc could be obtained, it would be very advantageous in the construction of the voltaic apparatus (998.). Most zincs, when put into dilute sulphuric acid, leave more or less of an insoluble matter upon the surface in the form of a crust, which contains various metals, as copper, lead, zinc, iron, cadmium, &c., in the metallic state. Such particles, by discharging part of the transferable power, render it, as to the whole battery, local; and so diminish the effect. As an indication connected with the more or less perfect action of the battery, I may mention that no gas ought to rise from the zinc plates. The more gas which is generated upon these surfaces, the greater is the local action and the less the transferable force. The investing crust is also inconvenient, by preventing the displacement and renewal of the charge upon the surface of the zinc. Such zinc as, dissolving in the cleanest manner in a dilute acid, dissolves also the slowest, is the best; zinc which contains much copper should especially be avoided. I have generally found rolled Liege or Mosselman's zinc the purest; and to that circumstance attribute in part the advantage of the new battery (1134.).

1145. *Foulness of the zinc plates.*—After use, the plates of a battery should be cleaned from the metallic powder upon their surfaces, especially if they are employed to obtain the laws of action of the battery itself. This precaution was always attended to with the porcelain trough batteries in the experiments described (1125. &c.). If a few foul plates are mingled with many clean ones, they make the action in the different cells irregular, and the transferable power is accordingly diminished, whilst the local and wasted power is increased. No old charge containing copper should be used to excite a battery.

1146. *New and old plates.*—I have found voltaic batteries far more powerful when the plates were new than when they have been used two or three times; so that a new and a used battery cannot be compared together, or even a battery with itself on the first and after times of use. My trough of twenty pairs of four-inch plates, charged with acid consisting of 200 water, $4\frac{1}{2}$ oil of vitriol, and 4 nitric acid, lost, upon the first time of being used, 2·32 equivalents per plate. When used after the fourth time with the same charge, the loss was from 3·26 to 4·47 equivalents per plate; the average being 3·7 equivalents. The first time the forty pair of plates (1124.) were used, the loss at each plate was only 1·65 equivalent; but afterwards it became 2·16, 2·17, 2·52. The first time twenty pair of four-inch plates in porcelain troughs were used, they lost, per plate, only 3·7 equivalents; but after that, the loss was 5·25, 5·36, 5·9 equivalents. Yet in all these cases the zincs had been well cleaned from adhering copper, &c., before each trial of power.

1147. With the rolled zinc the fall in force soon appeared to become constant, i. e. to proceed no further. But with the cast zinc plates belonging to the porcelain troughs, it appeared to continue, until at last, with the same charge, each plate lost above twice as much zinc for a given amount of action as at first. These troughs were, however, so irregular that I could not always determine the circumstances affecting the amount of electrolytic action.

1148. *Vicinity of the copper and zinc.*—The importance of this point in the construction of voltaic arrangements, and the greater power, as to immediate action, which is obtained when the zinc and copper surfaces are near to each other than when removed further apart, are well known. I find that the power is not only greater on the instant, but also that the sum of transferable power, in relation to the whole sum of chemical action at the plates, is much increased. The cause of this gain is very evident. Whatever tends to retard the circulation of the transferable force, (i. e. the electricity,) diminishes the proportion of such force, and increases the proportion of that which is local (996. 1120.). Now the liquid in the cells possesses this retarding power, and therefore acts injuriously, in greater or less proportion, according to the quantity of it between the zinc and copper plates, i. e. according to the distances between their surfaces. A trough, therefore, in which the plates are only half the distance asunder at which they are placed in another, will produce more transferable, and less local, force than the latter; and thus, because the electrolyte in the cells can transmit the current more readily, both the intensity and quantity of electricity is increased for a given consumption of zinc. To this circumstance mainly I attribute the superiority of the trough I have described (1134.).

1149. The superiority of *double coppers* over single plates also depends in part upon diminishing the resistance offered by the electrolyte between the metals. For, in fact, with double coppers the sectional area of the interposed acid becomes nearly double that with single coppers, and therefore it more freely transfers the electricity. Double coppers are, however, effective, mainly because they virtually double the acting surface of the zinc, or nearly so; for in a trough with single copper plates and the usual construction of cells, that surface of zinc which is not opposed to a copper surface is thrown almost entirely out of voltaic action, yet the acid continues to act upon it and the metal is dissolved, producing very little more than local effect (947. 996.). But when by doubling the copper, that metal is opposed to the second surface of the zinc plate, then a great part of the action upon the latter is converted into transferable force, and thus the power of the trough as to quantity of electricity is highly exalted.

1150. *First immersion of the plates.*—The great effect produced at the first immersion of the plates, (apart from their being new or used (1146.),) I have attributed elsewhere to the unchanged condition of the acid in contact with the zinc plate (1003. 1037.): as the acid becomes neutralized, its exciting power is proportionably diminished. Hare's form of trough secures much advantage of this kind, by mingling the liquid, and bringing what may be considered as a fresh surface of acid against the plates every time it is used immediately after a rest.

1151. *Number of plates**.—The most advantageous number of plates in a battery used for chemical decomposition, depends almost entirely upon the resistance to be overcome at the place of action; but whatever that resistance may be, there is a certain number which is more œconomical than either a greater or a less. Ten pairs of four-inch plates in a porcelain trough of the ordinary construction, acting in the volta-electrometer (1126.) upon dilute sulphuric acid of spec. grav. 1·314, gave an average consumption of 15·4 equivalents per plate, or 154 equivalents on the whole. Twenty pairs of the same plates, with the same acid, gave only a consumption of 5·5 per plate, or 110 equivalents upon the whole. When forty pairs of the same plates were used, the consumption was 3·54 equivalents per plate, or 141·6 upon the whole battery. Thus the consumption of zinc arranged as twenty plates was more advantageous than if arranged either as ten or as forty.

1152. Again, ten pairs of my four-inch plates (1129.) lost 6·76 each, or the whole ten 67·6 equivalents of zinc, in effecting decomposition; whilst twenty pairs of the same plates, excited by the same acid, lost 3·7 equivalents each, or on the whole 74 equivalents. In other comparative experiments of numbers, ten pairs of the three-inch plates (1125.) lost 3·725,

* Gay-Lussac and Thenard, *Recherches Physico-chimiques*, tom. i. p. 29.

or 37·25 equivalents upon the whole; whilst twenty pairs lost 2·53 each, or 50·6 in all; and forty pairs lost on an average 2·21, or 88·4 altogether. In both these cases, therefore, increase of numbers had not been advantageous as to the effective production of *transferable chemical power* from the *whole quantity of chemical force* active at the surfaces of excitation (1120.).

1153. But if I had used a weaker acid or a worse conductor in the volta-electrometer, then the number of plates which would produce the most advantageous effect would have risen; or if I had used a better conductor than that really employed in the volta-electrometer, I might have reduced the number even to one; as, for instance, when a thick wire is used to complete the circuit (865. &c.). And the cause of these variations is very evident, when it is considered that each successive plate in the voltaic apparatus does not add anything to the *quantity* of transferable power or electricity which the first plate can put into motion, provided a good conductor be present, but tends only to exalt the *intensity* of that quantity, so as to make it more able to overcome the obstruction of bad conductors (994. 1158.).

1154. *Large or small plates* *.—The advantageous use of large or small plates for electrolyzations will evidently depend upon the facility with which the transferable power or electricity can pass. If in a particular case the most effectual number of plates is known (1151.), then the addition of more zinc would be most advantageously made in increasing the *size* of the plates, and not their *number*. At the same time, large increase in the size of the plates would raise in a small degree the most favourable number.

1155. Large and small plates should not be used together in the same battery: the small ones occasion a loss of the power of the large ones, unless they be excited by an acid proportionably more powerful; for with a certain acid they cannot transmit the same portion of electricity in a given time which the same acid can evolve by action on the larger plates.

1156. *Simultaneous decompositions.*—When the number of plates in a battery much surpasses the most favourable proportion (1151—1153.), two or more decompositions may be effected simultaneously with advantage. Thus my forty pairs of plates (1124.) produced in one volta-electrometer 22·8 cubic inches of gas. Being recharged exactly in the same manner, they produced in each of two volta-electrometers 21

* Gay-Lussac and Thenard, *Recherches Physico-chimiques*, tom. i. p. 29.

cubical inches. In the first experiment the whole consumption of zinc was 88·4 equivalents, and in the second only 48·28 equivalents, for the whole of the water decomposed in both volta-electrometers.

1157. But when the twenty pairs of four-inch plates (1129.) were tried in a similar manner, the results were in the opposite direction. With one volta-electrometer 52 cubic inches of gas were obtained; with two, only 14·6 cubic inches from each. The quantity of charge was not the same in both cases, though it was of the same strength; but on rendering the results comparative by reducing them to equivalents (1126.), it was found that the consumption of metal in the first case was 74, and in the second case 97, equivalents for the *whole* of the water decomposed. These results of course depend upon the same circumstances of retardation, &c., which have been referred to in speaking of the proper number of plates (1151.).

1158. That the *transferring*, or, as it is usually called, *conducting*, *power* of an electrolyte which is to be decomposed, or other interposed body, should be rendered as good as possible*, is very evident (1020. 1120.). With a perfectly good conductor and a good battery, nearly all the electricity is passed, i. e. *nearly all* the chemical power becomes transferable, even with a single pair of plates (867.). With an interposed non-conductor none of the chemical power becomes transferable. With an imperfect conductor more or less of the chemical power becomes transferable as the circumstances favouring the transfer of forces across the imperfect conductor are exalted or diminished: these circumstances are, actual increase or improvement of the conducting power, enlargement of the electrodes, approximation of the electrodes, and increased intensity of the passing current.

1159. The introduction of common spring water in place of one of the volta-electrometers used with twenty pairs of four-inch plates (1156.) caused such obstruction as not to allow one fifteenth of the transferable force to pass which would have circulated without it. Thus fourteen fifteenths of the available force of the battery were destroyed, being converted into local force, (which was rendered evident by the evolution of gas from the zincs,) and yet the platina electrodes in the water were three inches long, nearly an inch wide, and not a quarter of an inch apart.

1160. These points, i. e. the increase of conducting power,

* Gay-Lussac and Thenard, *Recherches Physico-chimiques*, tom. i. pp. 13, 15, 22.

the enlargement of the electrodes, and their approximation, should be especially attended to in *volta-electrometers*. The principles upon which their utility depend are so evident that there can be no occasion for further development of them here.

Royal Institution, Oct. 11, 1834.

XXIV. *Experiments on the Protection of Iron from the Action of Salt-Water.* *By* T. Tassell Grant, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

WITH reference to the interesting papers which appear in your Journal of the present month (November 1835, vol. vii. pp. 389, 391.) giving an account of som eexperiments recently made with the view of protecting iron from the action of salt water, &c., I beg leave to communicate, that for some months past I have myself been engaged in a series of very similar experiments. My attention was drawn to the subject with the view of obtaining a remedy for the great wear and tear arising from the oxidation of the iron tanks at present generally used in the Navy for the stowage of fresh water. I first fitted a small plate of zinc, 3 inches square, $\frac{1}{16}$th of an inch thick, with iron rivets, to a piece of sheet iron 6 inches square, the two metals being completely in contact, and immersed the same into six gallons of spring water; at the same time I also immersed a piece of sheet iron of the same dimensions, without the zinc, into the same quantity and quality of water: at the expiration of thirty days the two pieces of iron presented nearly the same appearance, viz. oxidation was perceivable, and to the same extent in both. I repeated the experiment with protectors of larger dimensions, still without any satisfactory result; and I have other experiments still in progress, in which the two metals bear a more equal proportion, but sufficient time has not yet elapsed to form a correct opinion as to the result. Experiments with the two metals in contact in salt water, for the purpose of substituting iron sheathing for ships' bottoms instead of copper, have also engaged my attention, and have been attended with various results. As far as these experiments have proceeded, I fear they are not likely to be productive of the great benefit I at first anticipated. Although no doubt can exist as to the zinc protecting the iron from oxidation, as the simple electrical action arising by the contact of the two metals in presence of the fluid will produce that effect, yet I have found in all instances that the corrosion of the zinc is very considerable: the following experiment will show to what extent: Two pieces of sheet iron fastened to a piece of wood, the one with nine zinc nails, the other with the same number of iron nails having pieces of zinc ⅝ths of an inch in diameter under the head of each nail; also a third piece of sheet iron fastened to the wood simply with iron nails: the board was then floated in the sea, and at the expiration of thirty days, I found that the heads of six of the nine zinc nails had completely disappeared, and the pieces of zinc corroded to such an extent that only a very small portion of zinc remained. The protected iron down to this period was free from oxidation, whereas the iron unprotected was perfectly oxidized. This experiment has been repeated several times with the same result, which clearly shows that although the zinc completely protected the iron, the zinc itself became corroded in exact proportion to the protection that it afforded to the iron. Experiment has also proved that the same evil which rendered Sir Humphry Davy's system of no practical use for the protection of copper on ships' bottoms from oxidation, is also apparent to a certain extent as regards the protected iron; viz. that by rendering it slightly negative, a calcareous substance is found deposited on its surface; and that sea vegetable matter appeared also in a short period to attach itself to the iron, although in a much less degree than in the experiments tried on the bottoms of boats which were subject to the constant friction of the water passing by them.

In the experiments tried in still water, vegetable matter was found to make its appearance on the iron in six weeks after immersion, although a strong electrical current was kept up during that period. The results of the experiments, as far as they have proceeded, lead me, therefore, to the following conclusions: in the first place, that iron and zinc in connexion will not protect the former from oxidation in fresh water; secondly, that when iron and zinc are in connexion in salt water, the iron will be protected, but a calcareous and vegetable matter is generated upon it; and, thirdly, that in the same proportion as the zinc protects the iron, the zinc itself becomes subject to corrosion.

I wish it, however, to be clearly understood, that although these experiments are not so favourable as might be wished, I by no means consider them so conclusive as to preclude the necessity of further investigation.

I am, Gentlemen, yours, &c.

THOMAS TASSELL GRANT.

Royal Clarence Yard, Gosport,
November 22, 1835.

XXV. *On the Conducting Power of Iodine, Bromine, and Chlorine for Electricity.* By Edward Solly, *Jun., Esq.**

IN the Philosophical Magazine and Journal of Science, No. 42, p. 441, Dr. Inglis, in his prize essay on iodine, states that he has found solid iodine to be a conductor of electricity. In my own observations I had always found it a non-conductor; I was therefore led to repeat my experiments with greater care, and the following are the results.

1. I first sought for conducting power by the beautiful method proposed by Dr. Wollaston, namely, the effect produced upon the tongue when two metals of different degrees of oxidibility, placed on either side of it, are made to communicate with each other, through any portion of conducting matter. Iodine was melted in a thin glass tube, which, when cold, was broken, and the iodine obtained in a solid state; a portion was then placed between the extremities of the two metals; but on no occasion was the least taste produced; though if the metals were connected together only by being immersed in spring water, a taste was immediately perceived. When the two terminations of the metal plates are made to dip into a solution of iodine in water, a strong taste is perceived.

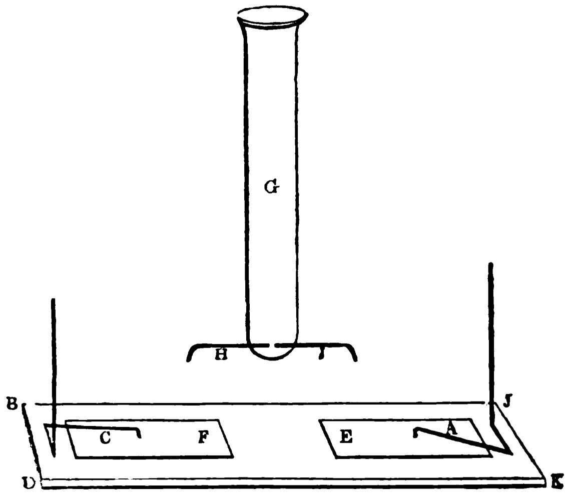
2. In order to examine the conducting power with the voltaic battery, and where the application of the tongue would have been uncertain and inconvenient, the following apparatus was used. B D J K is a slip of glass, on which two pieces of bibulous paper, E and F, soaked in a solution of iodide of potassium, are placed. The wire A, resting upon E, was always made the anode C; or that resting on F, the cathode: thus arranged, of course no action took place. But if a wire was made to touch with one end the paper F, and with the other end the paper E, the usual series of phænomena took place; iodine was evolved at A, and also at that end of the temporary wire which rested upon F. The fluid to be examined was placed in a glass tube, G, having two platinum wires, H and I, fused into it; they were separated from each other by an interval of about the $\frac{1}{20}$th of an inch; thus, when the two wires were made to rest upon the two pieces of paper F and

* Communicated by the Author.—It appears that we were correct in thinking that Dr. Inglis's experiments on this subject would attract attention. He has favoured us with the following reply to our note respecting it appended to the first part of his paper, as referred to above. "In answer to the note regarding the conducting power of iodine, I may just quote a sentence from my original Essay: 'The preparation sent in, shows the state in which iodine requires to be, for the transmission of Electricity. It has merely been fused in a glass tube, and the tube afterwards broken from around it. But although it still continues to conduct, it did so with far more energy when in the fluid state.' Dec. 18, 1835."—Edit.

E, any current that passed would be rendered evident by the decomposition of the iodide of potassium.

3. Iodine was fused in the tube G, and the end of its two wires, I and H, were placed on the papers E and F, as soon as the iodine was solid; not the least spot of iodine was perceived at E or H, though the battery employed consisted of sixty pairs of plates, four inches square, in very strong action: a small piece of wire was then made to connect I and H, just where they are fused into the glass tube; and though they were but momentarily connected, yet a dark spot of iodine was produced; thus proving that the only interruption to the current was that in the tube G, between the wires H and I.

Fig. 1.



4. The iodine was then replaced by a solution of iodine in water; the current passed immediately, and produced its full effect at A and H: the water only, however, was decomposed, and no peculiar action was occasioned. But this is certainly no proof that iodine is at all a conductor; we very well know that sulphuric acid added to water improves its conducting power, and so do phosphoric and sulphurous* acids, and many other acknowledged nonconductors; indeed, were it not for the addition of certain nonconducting substances, such as sulphuric acid, the decomposition of water would hardly be effected by the voltaic battery. Again, M. De la Rive† has remarked that bromine and chlorine are nonconductors, and

* See Phil. Trans. 1834; Faraday's Seventh Series, No. 755. [or Lond. and Edinb. Phil. Mag., vol. v. p. 257.—Edit.]

† *Annales de Chimie et de Physique*, 1827, vol. xxxv.

that pure water is also one; but that a solution of bromine or chlorine in water is a good conductor. A solution of iodine in æther also allowed the transmission of electricity, but in a less degree.

5. Iodine is soluble in carburet of sulphur, forming a fine pinkish red solution; when boiled in it, a considerable quantity is dissolved, which, upon cooling, is again deposited in crystals: neither the hot nor the cold solution conducted the electricity.

6. Iodine is also soluble in chloride of sulphur, forming a deep red liquid; much more is taken up by boiling, and upon cooling, crystals, probably of unaltered iodine, are precipitated. Dr. Inglis says that "iodine and chloride of sulphur form a compound, having many of the properties of bromine; but that it is decomposed by galvanism, which the real bromine is not." The result of my experiments was different, for I found that when the red liquid was submitted to the electric current in G, it formed a perfect barrier to the passage of the electricity, and it is very certain that decomposition cannot be effected without conduction. Perhaps Dr. Inglis will state how the experiment was performed, and at which electrode the iodine was evolved, or what were the substances evolved.

7. Bromine I found to be a nonconductor when placed in the tube G; a solution of bromine in water was a much better conductor than pure water, as M. De la Rive has mentioned (see the above-quoted memoir). In these and all the following experiments here described, the test of the wire (3.) was applied.

8. A solution of bromine in æther conducts. Æther seems to have a remarkable action on the colours of solutions containing bromine, for whenever it is added to any of the deep red solutions containing bromine, or the iodide of bromine, the colour is rendered considerably lighter, so that an almost opake solution becomes pale yellow, and quite transparent.

9. Bromine is soluble in chloride of sulphur, in the same way as iodine, forming a beautiful red solution: this proved a nonconductor; but upon adding a few drops of æther it became a conductor. Bromine is also soluble in carburet of sulphur, forming a splendid red solution, similar to the forementioned one: this was likewise a nonconductor; but a few drops of æther rendered it a conductor.

10. Periodide of bromine was a conductor; the current transmitted by it was fully able to decompose the iodide of potassium at E and F; but the decomposition of water, also placed in the circuit, was effected with some difficulty. A little water was now added to the periodide of bromine; the water floated at the top, and dissolved a small portion of it: the water and the

iodide of potassium indicated that the current was passing; but the liquids in the tube G were not visibly affected.

11. An aqueous solution of the periodide of bromine being put into the tube G, conducted, and was briskly decomposed; but both the platinum wires remained bright and clean, and neither iodine, bromine, nor any compound of them, was evolved or deposited on either electrode, though the action was continued for some time.

12. A solution of the periodide of bromine was a good conductor, and the current transmitted had sufficient intensity for the electrolyzation of water. Solutions of the periodide, in chloride of sulphur and carburet of sulphur, were nonconductors; upon the addition, however, of a few drops of æther, they became good conductors.

13. The conducting power of chlorine was next tried, and for this purpose the following apparatus was employed: A B C,

Fig. 2.

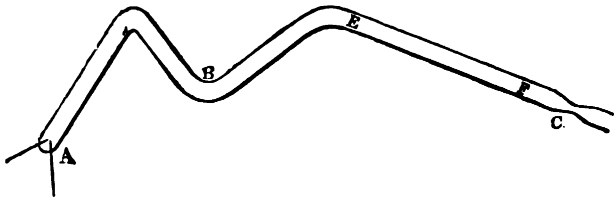


fig. 2, is a glass tube $\frac{4}{10}$ths of an inch in diameter, having two platinum wires fused into it at A, so as to be separated the $\frac{1}{10}$th of an inch from each other; the tube being then inverted, the space from E to F was filled with peroxide of manganese and muriatic acid; the end C was then carefully closed by a spirit lamp, and the whole being cooled, it was placed in the position represented in the figure, the space from E to C being filled with a mixture for generating the chlorine, the other parts of the tube having been carefully kept dry. Heat was then applied to C, and B was immersed in ice-cold water; as soon as a sufficient quantity of liquid had collected in B, A was immersed in a mixture of ice and salt, and B was gently warmed; by this means the liquid chlorine was rectified, and obtained quite free from water or other extraneous fluids at A. Matters being thus arranged, and sufficient chlorine having been condensed, the tube was placed in the same position as the tube G in the former figure, one of the two platinum wires resting on the moistened paper F, the other upon E, fig. 1. I was at first surprised by finding it a conductor; but when the tube was carefully wiped, so as to be quite free from all adhering salt from the freezing mixture, it proved a perfect nonconductor.

14. The crystallized hydrate of chlorine was then put in the tube G, fig. 1: it proved a nonconductor. A strong solution of chlorine, placed in the same situation, was a good conductor.

From these experiments the following conclusions may be drawn: 1st, that iodine, bromine, and chlorine are nonconductors; 2ndly, that they improve the conducting power of badly-conducting electrolytes; and 3rdly, that two nonconductors combining can form a body which can conduct electricity, and which resists the decomposing power of the voltaic battery.

7, Curzon Street, 15th January, 1836.

XXVI. *Description of the Aurora Borealis of November 16, 1835. By* W. Sturgeon, *Lecturer on Experimental Philosophy at the Honourable East India Company's Military Academy, &c. &c.**

AN aurora borealis of a very unusual character was seen in this neighbourhood, and I imagine over a large tract of
18 country, on Wednesday evening the 1[illegible]th instant. I was walking from Greenwich to Woolwich between nine and ten o'clock; and when I had arrived at the top of Maize Hill, by the side of Greenwich Park, then about ten minutes past nine, my attention was first attracted by the fine light of the aurora in the north. I walked on a little further till a good opening to the northern horizon presented itself from the road leading from Maize Hill to Mr. Angerstein's estate. At this opening I made a determined stand, for the purpose of observing any novel phænomenon which the aurora might happen to present.

At this time it consisted principally of a very extensive lateral range, on both sides of the pole star, of vertical streamers, which were pencilling the northern heavens from about 15° above the horizon to Cassiopeia's Chair, then about the meridian; and so uniform was their arrangement and splendour that they presented one sheet of yellowish white light, the most intense at the base, and becoming more and more faint as they proceeded upwards, until quite lost at their terminal altitudes.

This appearance of the aurora had but just stamped its impression on my mind, when in one moment the whole of the northern heavens appeared in one complete state of undulating commotion, heaving upwards in rapid succession immense waves of light†, which, like the streamers which preceded

* Communicated by the Author.

† These waves were seen at Milton next Gravesend by my scientific friend Mr. Swinny; and I beg to acknowledge the obligation I am placed

them, gradually diminished in brilliancy from their source near the horizon till their arrival at the zenith, which was their general vanishing point.

The horizontal range of the aurora during this unusual display was eastward as far as Jupiter, whose azimuth from the north was then about 75°; and perhaps about the same extent westward on the other side of the pole star. I observed it stretch to beyond α Lyræ (Vega), whose azimuth from the north was about 60°, but could not very well ascertain the position of the western extremity at the place where I was standing, on account of the reflexion of the gas light in London mixing with that of the aurora, and the intervention of trees, &c.

This extensive ocean of light, which illuminated nearly half of the visible heavens, and whose waves rolled with the rapidity of thought, lasted about eight or ten minutes, perhaps longer, when they gradually began to disappear, and the aurora to contract in all its dimensions. Until this time (nearly half past nine) no dense nucleus had marked the centre of the aurora: the stars were seen between the horizon and the luminous base as decidedly, though not so clear, as if no aurora were present. The star Benetnasch, in the tip of the tail of the Great Bear (η Ursæ Majoris), was one of those which were observed below the aurora; but Mizar (ζ Ursæ Majoris), then on the meridian below the pole, was seen in the bright arched base of the streamers and waves.

The last-mentioned change in the appearance of the aurora brought it gradually to that state which is usually exhibited in some period or another of this boreal phænomenon. The dense black nucleus began to form, and soon curtained the stars which had previously twinkled in that segment of the northern sky. The luminous margin also, its usual attendant, became well defined, and its highest point was well marked to the westward of the meridian, perhaps nearly in the magnetic north. I now walked on, keeping the aurora in view, which shot occasional streamers from various parts of the luminous arch. Just before I entered Woolwich, about ten o'clock, another fine display of vertical streamers spread over the northern sky, and continued for nearly a quarter of an hour. By this time I reached home; but too late to ascertain their effect, if any, upon the magnetic needle, for they faded away very rapidly after my arrival, and before eleven the aurora had entirely disappeared.

under to Mrs. Swinny, who also saw these waves, for a more happy description of them than any I had before thought of. They appeared to this lady as "waves of thin smoke or steam, behind which was placed a strong light." A more expressive description could not possibly be given.

During my walk home, I observed several fine meteoric stars, most of which appeared to be shot from the same point of the heavens, which point was somewhere in a right line between me and the Twins. One of these meteors shot with a moderate velocity across the north part of the meridian, at an altitude of about 80°, and appeared to traverse an arch of the heavens of 90° or 100°. It burst into several luminous fragments at the western termination of its range, and became extinct in a moment. I listened for some time, but heard no noise; neither did my servant who was with me, and who listened attentively at my request. I had previously pointed out to him the direction he was to look in, and he saw the meteor from the first to its last appearance. He also directly afterwards saw another from the same quarter, which traversed the heavens in nearly the same direction as the former. He called out to me, but it was lost without my seeing it. These meteors were seen about five minutes before the last display of streamers mentioned above.

I saw no appearance of the aurora to the south of the zenith, though frequently looked for. The sky was quite clear of clouds, and the black southern expanse, studded with its brilliant stars, afforded a fine contrast to the display of the aurora in the north.

Artillery Place, Woolwich, Nov. 19, 1835.

N.B. Whilst writing the above, a friend has called on me, who saw fine streamers about half-past eight o'clock.

XXVII. *On the History of Rotatory Single Steam Engines working expansively, in reply to* Mr. Henwood. *By* John Taylor, *Esq., F.R.S., Treas. G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

MR. HENWOOD by his letter in your last Number (p. 20.) seems to exhibit a great desire for controversy, in as much as he attacks me because my communication appears to him to imply that a rotatory single engine working expansively is something of novelty. Now, not to insist upon the thing being little known, it must be evident that it was no part of my object to discuss whether the engine which I described was new or otherwise, and that, in fact, I stated that it was not a new invention, and mentioned another on the same construction formerly erected at Wheal Vor.

I have since found that Captain Francis of the Mold mines has successfully applied the same principle to Whim engines; and I am glad to hear that those of Messrs. Gregor and

Thomas seem further to prove that which I merely wished to draw the attention of engineers to, and by making it public in your pages, to give information which otherwise might not for a long time to come have reached to other districts.

Having observed a certain instance of great improvement in the œconomy of fuel, applicable to that kind of engine which is mostly employed in all the varied operations of our numerous manufactures, I merely desired to communicate the knowledge of the fact; and, as I expressed in my letter, I pointed it out as deserving attention and inquiry. I think it much more important to the public to consider the steps by which improvements are worked out to practical advantage than to indulge in disputes about such originators of an invention as did little more than to broach an idea, good enough, perhaps, in itself, but which may only have been rendered valuable by the superior skill or industry of others exerted in bringing it into useful and general application.

This observation may apply to what Mr. Henwood chooses to say of Mr. Woolf, respecting whom he seems to lose no opportunity of endeavouring to detract from the merit to which I and many others think he is entitled; my expression was that we owe to him the method of working high-pressure steam expansively *, and this is still my opinion. I have in another place recorded Captain Trevithick's engine at Wheal Prosper, and so far have done him justice, but this engine did only about 26 millions duty, and did not equal other engines then working in the common way; nor does it appear that Captain Trevithick followed up his invention or produced any improvement upon the duty of the engines in Cornwall, the average not having increased until two years afterwards, when some of Mr. Woolf's engines had attained to a duty of 50 millions, and Messrs. Jeffrey and Gribble had successfully adapted the same principle to an engine with one cylinder.

Mr. Henwood in a note states correctly that Captain Lean reports the duty of the Charles Town engine at 40 millions, and not at 60, as stated by me. What, however, I did state was, that when I saw it, soon after it was put to work, it was calculated to be performing a duty of about 60 millions. This calculation was made by the principal agent of the mine, and the engineer on the spot, and I saw no reason to doubt their accuracy, and gave their account as I received it, adding, however, that I had desired that its performance should be regularly reported in the monthly duty papers, by which of course any error in this respect would certainly be set right;

[* Our much respected predecessor, Dr. Tilloch, expended a considerable part of his property in his zeal for assisting Mr. Woolf.—Edit.]

and observing that, if the rate of duty which I had mentioned could be maintained, a very great improvement might take place in the engines most generally employed.

I still think that this is a very important object for the consideration of persons who employ or construct steam engines; and if Mr. Henwood's notice of my short letter should assist in interesting them in the subject, it will not be without its use, though I may not be inclined to trouble you again upon the disputed points. I am, Gentlemen, yours very truly,

Bedford Row, Jan. 26, 1836. JOHN TAYLOR.

XXVIII. *On the Crag Formation; in answer to* Mr. Charlesworth's "*Reply.*" *By* SAMUEL WOODWARD, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE severe animadversions by Mr. Charlesworth, in your Number for December 1835, on my notice of his former paper, necessitates my requesting the favour of your inserting a few lines in reply.

Mr. Charlesworth's remarks about "breach of decorum" and "impugning his veracity," &c. &c. I leave to the good sense of your readers; neither do I intend to quarrel about words. The difference between my opponent and myself appears to be this: He makes his "red crag" a distinct formation, newer than the one upon which it reposes. I, on the contrary, have asserted it to be diluvial or disrupted crag, and cited as examples the cliffs north and south of Yarmouth and at Cromer. Mr. R. C. Taylor, at p. 21 of his Geology of Eastern Norfolk, states, that "the crag itself has, at the last of the geological epochs, been subject to abrasion by the diluvial currents to which allusion has been made. Their fragments, mingled with those of the chalk and preceding formations, piled in enormous heaps, form the cliffs of Cromer and Trimmingham, 250 or 300 feet in thickness, upon the original crag, which rests, *in situ*, at their base." And, strange to say, at p. 86, Mr. Charlesworth, forgetting his discovery that "the red crag was a gradual deposit formed by successive accumulations of marine exuviæ," quotes my friend Mr. Searles Wood as follows: "I am inclined to think the whole of the upper stratum has been produced from the ruin of the lower." After such a contradiction of himself I might surely with greater propriety retort upon him the passage occurring at p. 466, l. 27.

My impression on the first glance at the upper bed at Ramsholt was, (comparing it with the Norfolk deposit,) that, from its discoloration by the oxide of iron and there being no superincumbent bed except vegetable mould, it must belong to the diluvium; and on examination I could find no perfect shells; all appeared to me to be waterworn and broken into fragments, and to have been transported from some other deposit.

I have shown Mr. Charlesworth the note I made at the time on the lower bed, which is as follows: "From the organic remains resembling those of Malta, figured by Scilla, in his *Corporibus Marinis*, &c., I am inclined to think the shells of this bed much newer than those reposing on them." This opinion has unexpectedly been confirmed (contrary to Mr. Charlesworth's conjecture at p. 92,) by the latest information from M. Deshayes, communicated to Mr. Lyell; by which it appears that a larger percentage of *recent species* has been detected in the so-called "coralline crag" than from perhaps any other portion of the great deposit. If such is the fact, and I have no reason to doubt my authority, there is an end of the question between my opponent and myself.

The propriety or impropriety of calling the lower bed "coralline crag" is, I conceive, of little moment, and a mere question of words, and not of facts. What I contended for was, that it was not composed of corallines [corals] as that at Aldborough; and my opinion still is, that the use of the term, as distinguishing the epoch of any portion of the crag, tends only to mislead the inquirer.

I am, Gentlemen, yours, &c.

Lakenham Grove Cottage, Norwich, Dec. 4, 1835. SAMUEL WOODWARD.

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

Newton and Flamsteed. Remarks on an Article in Number CIX. of the Quarterly Review: by the Rev. WILLIAM WHEWELL, M.A., Fellow and Tutor of Trinity College, Cambridge. Deighton, Cambridge; and Parker, Strand.

THE attention of the public has been lately directed with anxious interest to the character of Sir Isaac Newton, by an article in the Quarterly Review, founded upon Mr. Baily's Account of Flamsteed, prefixed to an edition of his Observations, printed by the Board of Admiralty; and copious extracts from this 'Account' have been given by some of our contemporaries.

The work itself not being published, but privately distributed, we have had no opportunity of judging for ourselves as to the value of the conclusions drawn from the letters of Flamsteed, and the grounds which they afford the Reviewer for announcing to the world, though with professions of regret, that the name of Newton is no longer to be had in reverence. From the appearance, however, of Mr. Whewell's short pamphlet entitled 'Newton and Flamsteed,' we have now the satisfaction of finding that the subject has been fully investigated by one who will be universally considered as most competent to form a correct judgement:—and to very many who have been dwelling with grief and wonder upon the painful impressions created by this Review of an unpublished book, the following decisive expression of Mr. Whewell's opinion will be very grateful:

"I shall conclude; leaving it to the reader to decide—whether the blame of intemperate virulence of feeling and irrational violence of conduct does not rest solely with Flamsteed;—whether Newton's philosophical and moral character do not come out from this examination BLAMELESS and ADMIRABLE, as they have always been esteemed by thinking men;—and whether the Reviewer has not shown extraordinary ignorance of that part of scientific history which he has pretended to elucidate, and unaccountable blindness and perverseness in his use even of the *ex parte* evidence which he had before him."

Mr. Whewell's pamphlet evinces the clear-sightedness and candour for which he is distinguished. Though a very short, it is a very admirable and interesting production, and entitles its author to rank as high for moral discrimination, as he does for scientific attainments. We shall take the liberty to quote rather largely, knowing the interest which must be taken in the subject. With regard to the Reviewer's unaccountable blindness, perverseness, and partiality, he justly remarks, that "he has taken for his sole guide the statements of one of the parties, written in the warmth of the moment, —has identified himself with Flamsteed's most petulant feelings, and has not corrected them by any attention to the case of the opposite party. When the great body of Review Readers are called upon, in this temper, to cast away all their reverence* for the most revered name of our nation, it must be right that some one should interpose a warning, and deprecate judgments of such levity and partiality."

"It is to be observed," adds Mr. Whewell, "that if we adopt the Reviewer's opinion, that Flamsteed was throughout a man bitterly wronged, and that there was an extreme of baseness and tyranny on the side of the persons with whom he quarrelled, we involve in our condemnation almost all the eminent literary and scientific men of the day †: for we have, acting with Newton, and sharing in his views, not only Halley, the object of Flamsteed's intense dislike, but Gregory, Arbuthnot, Mead, Sloane, Wren.

"The purpose for which Newton desired that the world should possess the best observations, was the confirmation of the great Theory of Universal Gravitation;—incomparably the greatest discovery ever made by man; and at that period, we may say, in the agony of that latent struggle by which the confirmation and general reception of great discoveries is always accompanied. We of the present day are accustomed to consider this immense step as effected at once, on the publication of the first edition of the Principia in 1687; but we may easily convince ourselves that this was not so. Even under the most favourable circumstances, a vast theory like

* The Edinburgh Review intimates that this reverence has been all a mistake, attributable to one Mr. Conduit!

† Designated by the Edinburgh Reviewer as "Newton's *party*": *vide infra*, p. 144.

this could not make its way at once. No man of Newton's standing (I believe) thoroughly accepted his views: Halley was sixteen, David Gregory nineteen years his junior. In England this acceptance of the theory required half a generation, in France and Germany more than a whole generation. And during this interval, the result of the struggle depended upon the accordance of the theory with the best observations, which the Greenwich ones undoubtedly were. Upon these observations, then, depended a greater stake in the fortune of science than was ever before at hazard, and this Newton knew well. How then can one be surprised at the earnestness and importunity with which he begs for Flamsteed's observations; and tries to soothe a jealousy and reserve which appear to have shown themselves at an early period?

"As for your observations, you know I cannot communicate them to any body, and much less publish them, without your consent. But if I should perfect the moon's theory, and you should think fit to give me leave to publish your observations with it, you may rest assured that I should make a faithful and honourable acknowledgment of their author, with a just character of their exactness above any others yet extant. In the former edition of my book, you may remember that you communicated some things to me, and I hope the acknowledgments I made of your communications were to your satisfaction: and you may be assured I shall not be less just to you for the future. For all the world knows that I make no observations myself, and therefore I must of necessity acknowledge their author: and if I do not make a handsome acknowledgment, they will reckon me an ungrateful clown.—*Account of Flamsteed*, p. 151.

"This the Reviewer has quoted; but he has not quoted what immediately follows, striking as it is.

"And, for my part, I am of opinion that for your observations to come abroad thus with a theory which you ushered into the world, and which by their means has been made exact, would be much more * for their advantage and your reputation, than to keep them private till you die or publish them, without such a theory to recommend them. For such theory will be a demonstration of their exactness, and make you readily acknowledged the exactest observer that has hitherto appeared in the world. But if you publish them without such a theory to recommend them, they will only be thrown into the heap of the observations of former astronomers, till *somebody shall arise* that, by perfecting the theory of the moon, shall discover your observations to be exacter than the rest. But when that shall be, God knows: I fear, not in your life-time, if I should die before it is done. *For I find this theory so very intricate, and the theory of gravity so necessary to it, that I am satisfied it will* NEVER *be perfected but by* SOMEBODY WHO UNDERSTANDS THE THEORY OF GRAVITY AS WELL OR BETTER THAN I DO.—p. 151–152.

"I have several times," observes Mr. Whewell, "in reading this passage, felt a kind of terror at the peril to which the success, or at least the speedy success, of the greatest of physical truths is here represented as exposed." He justly adds,

"With this consciousness of being in possession of such a truth, while Flamsteed's records of his observations contained the only language in which it could be made generally convincing, we may easily imagine that Newton could not help urging the publication and employment of the observations, in a manner which excited no

* Erroneously printed "worse" in the work.

sympathy in Flamsteed, unconscious of the nature of the then existing crisis in the history of astronomy.

"Flamsteed was only four years younger than Newton; he never fully accepted Newton's theory, *nor comprehended its nature.* Like all astronomers of his time, he understood by 'theory' only a mode of expressing *laws of phænomena*, not a new generalization by which such laws are referred to a physical *cause.*" Thus, having been told that Newton had deduced all the inequalities of the moon's motion from the laws of gravity alone, he says, in a letter to Lowthorp,

"With some indignation I answered that he had been as many years upon this thing, as I had been on the constellations and planets altogether:..... that I had imparted above 200 of the moon's observed places to him, which one would think should be sufficient to limit any theory by; and since he has altered and suited his theory till it fitted these observations, 't is no wonder that it represents them: but still he is more beholden to them for it than he is to his speculations about gravity, which had misled him.

"He appears," adds Mr. Whewell, "to have thought *too directly* of the value of his observations, *as the means of purchasing reputation.* How otherwise are we to account for the jealousy with which he objected to Newton's combining Cassini's observations of the comet of 1680 with his? when it must have been clear, even with his own notion of a theory, that the truth of the theory would be the better established, the more observations it agreed with. This is Flamsteed's own account of an interview with Newton in the letter just quoted:

"Some occasion of discourse about comets happening, I acquainted him that Dr. Gregory gave out that since he had altered his paths of comets, and instead of parabolas made them ellipses, his theories would represent all Mons. Cassini's observations within a minute, whereas I thought he had only *my* observed places to represent, and that it was not only an injury *to me*, but the nation, to *rob* our Observatory of what was due to it, and further to bestow it *on the French.*—p. 174.

As the accusations against Newton are, as Mr. Whewell justly observes, wholly *ex parte*, so it is evident how narrow were the views and how wretched the temper of his detractor. Mr. Whewell characterizes Flamsteed as having been "of weak temper, suspicious, irritable, and self-tormenting. We can hardly think otherwise of a man who was in the habit of brooding over the movements of spleen excited by casual expressions in the letters of his correspondents, and recording them in ink on the paper. Thus, as early as 1694, Newton happens to write to him (p. 139), 'I believe you have a wrong notion of my method in determining the moon's motions'; on which Flamsteed makes this note, 'I had: *and he of me: and still has.*' And after this period almost every letter of Newton's has a similar comment appended to it, and these become more and more bitter. Yet Newton appears to have been his friend as long as Flamsteed's temper allowed him to be so. A little after the above letter he writes:

"What you say about my having a mean opinion of you is a great mistake. I have defended you where there has been occasion, but never gave way to any insinuations against you. And what I wrote to you, proceeded only from hence, that *you seemed to suspect me of an ungrateful reservedness, which made me begin to be uneasy.*"—p. 146.

"Surely there is no want of kindly feeling in these expressions. * * *

"The wrathful temper of Flamsteed's dealings with Newton and his friends is indeed so manifest, that it is quite marvellous the necessity of making allowance for it should not have occurred to the Reviewer. Who, for example, can overlook it in the account which Flamsteed has given in his own Diary of his appearance before the Committee of the Royal Society on Oct. 19, 1711, and which the Reviewer has quoted at length? This Committee, it is to be observed, were the guardians of the national interest in the Greenwich observations, and were bound to see that Flamsteed made them accessible and useful to the public. According to his own account he began by calling them '*the robbers of his property*.' In describing the altercation which ensued, he says, 'I *only* desired him (Newton) to keep his temper, restrain his passion, and thanked him as often as he gave me ill names.' And again, in another part of the conversation: 'I *only* desired him (as I had often done) to restrain his passion, keep his temper, &c.' We hardly require the recollection of Sir Anthony Absolute to see here the demeanour of a very angry man; far too angry, certainly, to allow us to accept literally what he asserts, much less what he implies merely. I confess therefore I have great doubts whether, from the expression in the same account, 'he called me many hard names, *puppy* was the most innocent of them;' (p. 228.) we can confidently infer that the obnoxious term was used."

Perhaps we should express more exactly the impression conveyed by Flamsteed's passionate and wrong-headed statements, if we were to say, that the term 'puppy' may very possibly have been in some way or other used by Newton (certainly Flamsteed's self-complacency and self-will made it far from inapplicable), but that, if so, it is certain that it was the *most* and not the *least* angry word which was thus employed. If anything worse had been said, it is very clear that Flamsteed was not in a temper to omit recording it in his letters and journal.

With regard to the accusation, of which it is attempted to make so much, respecting the packet containing that part of Flamsteed's Catalogue which had, after much prevarication and many excuses, been obtained from him, and placed in Newton's hands *sealed* up, Mr. Whewell observes, p. 16, "that the case is very different from that which the Reviewer has collected, by confining himself to, and, what is more extraordinary, by adopting without modification, the indignant and querulous account given by Flamsteed. * *

"It must be recollected that any assumption on the part of Flamsteed, that he might deal with the observations made in his official capacity of Astronomer Royal, as if they were his private property, could not be allowed by the guardians of the institution;—that Newton and the persons who acted with him, acted not as private persons, nor at the suggestion of their own caprice, but took measures for the publication, as the Visitors of the Observatory, bound by their duty to see the office made effective;—that *the sealed packet* being thus national property, the seal was declared to have been broken by the Queen's command."—p. 16.

We may remark, that it seems impossible to assign any meaning to the deposit of the sealed packet but that, *if necessary*, it was to be opened. The necessity did occur, for Flamsteed refused to go on with the printing. If the work had been allowed to be finally stopt, of what use was the packet?

"Newton was acting as the authoritative head of a national body, and had, in that capacity, to repress, what must have appeared to him, extravagant claims and offensive behaviour."—p. 13.

"Almost thirty years had elapsed during which Flamsteed possessed the title of Astronomer Royal, and nothing had proceeded from the magnificent Observatory with which he was entrusted."—*Halley's Preface.*

In further illustration of the intellectual and moral character of Flamsteed, we add the following from Number CXXVI. of the Edinburgh Review, just published, in which we do not find any mention of Mr. Whewell's Remarks:

"If we consider the vast progress that was made in astronomy from the time of Tycho, or rather Hevelius, to Bradly, and the numerous inventions and discoveries by which every branch of it was enriched, we can scarcely find one, either in respect of theory, or of instruments, or of methods of observation or computation, to which he [Flamsteed] can justly lay claim. Delambre, indeed, allows him the merit of having been the first who practised the method of determining the place of the equinoxes from observed equal declinations of the sun; but if we except this, which, moreover, was an easy corollary from Picard's then well-known method of determining the time from observations of equal altitudes, *it would be difficult to show that he made a single step in advance of his age.* The character of his mind is more remarkable for activity, and that sort of sagacity which leads to practical skill, than for any of the higher endowments. In point of genius, his name is not to be mentioned with that of Newton; he was immeasurably inferior even to his rival, Halley. His *mathematical knowledge*, even for the time, appears to have been *extremely limited.* He set no value on the physical speculations of Newton, and evidently never understood them. He sneers at his 'conceptions' about gravity, calls him 'our great pretender,' carps at the lunar theory, does not 'relish the small equations,' and determines 'to lay these crotchets of Newton aside.' On no occasion does he attempt to establish a principle, or refer a phænomenon to gravitation; and he left to one of his successors the two most brilliant discoveries of modern astronomy, the aberration and nutation, though both were within his reach. In fact, he himself clearly pointed out the effect of the former, and even defined its amount; but he mistook (in common, however, with others) the phænomenon for the effect of parallax, although the simplest mathematical considerations might have shown him that parallax would be manifested in quite a different manner. Newton suggested to him the importance of noticing the state of the barometer and thermometer at the time of the observations, but he neglected the good advice. Even in respect of *instrumental accuracy* he perhaps came *rather behind* than preceded *the attainments of his day.*"—p. 369.

"On the recommendation of Newton and Arbuthnot, a royal warrant was obtained from the Queen, in December, 1710, constituting the President (Newton) of the Royal Society, and whoever else the Council should think fit, *Visitors of the Observatory*; authorizing them to demand of the Astronomer, 'within six months after the year shall have expired,' a copy of the annual observations, to direct such observations to be made as they should think fit, and to inspect the instruments, and report on their state to the Board of Ordnance. Flamsteed, who saw clearly that he would now be placed more *at the mercy of Newton's party** than ever, complains loudly of this measure—insinuates that nobody knew his business but himself—and tells us he was worse used than the 'noble Tycho, who had no visitors of his observatory appointed over him.'

"He also remonstrated against the appointment to the Secretary of State (St. John), but was 'answered haughtily, the Queen would be obeyed.' He even drew up a petition to the Queen, in one of the clauses of which he prays,

"'That I may not have the President of the Royal Society, nor any of their Council, set over me as visitors, nor suffered to prescribe to me what observations to make, since they know little of my business, and will but incommode me in my progress, and obstruct me; as some of them have done formerly; but that such of the nobility or gentry that are skilful in mathematics, together with the principal officers of your Majesty's Ordnance, that have been founders of my studies, may have the inspection and care of the Observatory.'

"The idea of seeking among the nobility and gentry for visitors who should know more of his business than Newton and Halley, with the Council of the Royal Society, may be allowed to be original; but we suspect the qualification on which Flamsteed would have set most value, would have been a disposition to allow him to manage his business in his own way. At all events he seems to have had no very high opinion of the Royal Society, of which he thus writes in a letter to Sharp: 'Our Society decays, and produces nothing remarkable, nor is like to do it, I fear, while 't is governed by persons that either value nothing but their own interests, *or understand little but vegetables*, and how, by making a bouncing noise, to cover their own ignorance.'"—p. 383.

Halley, however, the Edinb. Reviewer adds, "was better acquainted with Flamsteed's business, that is, with practical astronomy, than any other individual; and hence Flamsteed's anxiety, of which some amusing instances occur, to keep him in ignorance of what he was doing.

"Flamsteed, as our extracts have made evident, entertained a most exalted opinion of his own importance, and the superiority of his knowledge of astronomy. Halley commenced his scientific career as an astronomer, and by making a catalogue of stars; and therefore from the first was placed in a situation of direct rivalry with him. He had likewise acquired, by the versatility of his pursuits and employments, a large share of popular fame, of which Flamsteed appears to have been not a little envious."—p. 393.

The Quarterly asserts that Halley was "low and loose in his conduct, and a shameless infidel." On this subject the Edinburgh says:

[* These are unfair expressions of the Reviewer, calculated to create a prejudice.—EDIT.]

"It has been surmised that Flamsteed's aversion to Halley arose from the libertine conduct and infidel opinions* which the latter entertained, and took no pains to conceal. *We have no evidence of this.*"

"Flamsteed's charges are exaggerated to a degree that at the present time makes them appear almost ludicrous."—*Edinb. Rev.* cxxvi. p. 363.

To this more might be added from the records of the time in proof of Flamsteed's bad disposition and disingenuousness†, of which what is called his piety cannot be received as an extenuation, as it appears to have consisted in claiming the Deity as his partisan in all his quarrels. Nor can his belief in judicial astrology be lost sight of, nor his journey to Ireland to be touched for the benefit of his diseased body by a gentleman, "whose gift," he tells us, "was of God."

How great is our astonishment, then, to find the Edinburgh Reviewer, after giving such a low estimate of Flamsteed's character, so inconsistent, or so far mystified by the 'leperous distilment' of the Quarterly, as to call on us to credit the uncorroborated assertions of such a person, in opposition to the uniform testimony of Newton's distinguished contemporaries! During a very long life, no man ever was more the object of general interest and of close observation. If the letters of ill-conditioned persons are brought to light, what can we expect to find in them but complaints and calumnies? The Quarterly tells us for our consolation, that if we have lost a Newton, we have, forsooth, gained a *Flamsteed!* The Edinburgh goes so far as to favour us with an hypothesis to account for the origin of the mistaken opinion which has hitherto prevailed that Newton was a man of great moral worth! namely, that all the traditions of his moral character are derived from no other source than the Eloge of Fontenelle, who had his information solely from Mr. Conduit, a relation of Newton, and the inheritor of his property! Can anything be more ludicrous?

We shall conclude with citing the character which is given of Mr. Whewell by another writer in the same Number of the Edinburgh Review.

"Mr. Whewell has already, by his writings, approved to the world, not only his extensive acquirements in mathematical and physical science, but his talent as a vigorous and independent thinker. To a narrower circle, he is known as the principal public tutor of the principal college of his University; and in this relation, his zeal, and knowledge, and ability have concurred in raising him to an enviable eminence. Though more peculiarly distinguished by his publications in that department of science so exclusively patronized by the University, he has yet shown at once his intelligence and liberality, by amplifying the former circle of studies pursued in the college under his direction; and, in particular, we are informed that he has exerted his influence in awakening a new spirit for the cultivation of

* It appears that Newton sometimes gravely and kindly expostulated with Halley upon the latter subject: Flamsteed reviled him, and calumniated his moral character.

† See his conduct with regard to Hooke, in the Royal Society, Nov. 2, 1681, and Feb. 15, 1682, when he attempted to escape the disgrace due to his confident ignorance by a low fraud.

mental philosophy; in which department he has already introduced, or is in the course of introducing, a series of more appropriate authors than those previously in use."—Review of *Whewell's Thoughts on the Study of Mathematics as a part of a Liberal Education*, p. 410.

We rejoice that such a man has stept forward to dissipate our anxieties, and to disabuse the public; and do not hesitate to rely on the assurance of so competent a judge, and one who has had opportunities of carefully examining the subject, that "Newton's philosophical and moral character come out from this examination blameless and admirable, as they have always been esteemed by thinking men."

XXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. vii. p 412.]

Nov 19, 1835.—"ON the Empirical Laws of the Tides in the Port of Liverpool." By the Rev. William Whewell, M.A., F.R.S.

The author employs the results of the discussion of sixteen years of tide observations made at Liverpool, published by Mr. Lubbock in the Philosophical Transactions for the present year, in testing and improving the formulæ, expressing the mathematical laws of the inequalities of the phenomena of the tides, which had already been deduced by the author from the London tide observations. He finds that the Liverpool observations have not only confirmed, in the most satisfactory manner, these formulæ, but have furnished the means of greatly improving them. The corrections for lunar parallax and declination, which, as far as they depended on the former investigation, might be, considered as in some measure doubtful, and only locally applicable, have now been fully verified as to their general form; the nature of the local differences in the constants of the formulæ has also, in part, come into view; and the investigation has, moreover, shown that, notwithstanding the great irregularities to which the tides are subject, the results of the means of large masses of good observations agree with the formulæ with a precision not far below that of other astronomical phenomena. The formulæ obtained point directly to a very simple theory of the circumstances of tides, namely, that the tide at any place occurs in the same way as if the ocean assumed the form of equilibrium, corresponding to a certain antecedent time, and different place. The ocean, in its position of equilibrium, would have the form of a spheroid, of which the pole would revolve round the earth, following the moon at a certain distance of terrestrial longitude. This distance is termed by the author the *retroposition of the theoretical tide in longitude*, its mean value being what he has termed in other communications, the *corrected establishment* of the place. If from an original equilibrium tide, a derivative tide were sent off, along any channel, in which it is no longer influenced by the forces of the moon and sun, it would take a certain time in reaching any place in that channel, and the circumstances of the tide at that place would not depend on the positions and distances of the moon and sun at the time when the tide happens, but on the positions and distances of those luminaries at a certain time, anterior to the time of the tide, by the interval occupied in the transmission of the tide along the channel. This interval of time, which, in his former papers, the author had called *the age of the tide,* he here terms the *retroposition of the theoretical tide in time.*

Adopting this phraseology, the author finds that the phenomena of the Liverpool tides may be expressed as follows.

1. The effects which the changes of the moon's force produce on the tides are the same as the effects which those changes would produce upon a retroposited equilibrium tide.

2. The retroposition of the tide in longitude is affected by small changes, which changes are proportional to the variations in the moon's force.

3. The retroposition of the tide in time is also affected by small changes, which changes depend on the variations in the moon's force.

On the hypothesis that an equilibrium tide give rise to the Liverpool tides, we must suppose that the channel by which they are transmitted occupies in length, from west to east, $11^h\ 6^m$ of longitude; or we may suppose the tide spheroid to lie behind the position of equilibrium by a certain space; and the longitude occupied by the channel from end to end, may be supposed to make up the rest of the $11^h\ 6^m$, the retroposition of the tide in longitude. The author proceeds to show how the circumstances of the tide may be hypothetically represented on these suppositions; although it is not to be imagined that these hypotheses are strictly accordant with the true state of the case. As the general laws of the tides at other places must resemble those at Liverpool, they will of course be capable of being represented in a similar manner.

The remainder of the paper is occupied by a comparison of the data of observations at London and Liverpool, and by an investigation of the corrections in the formulæ thence resulting.

November 26, 1835.—"Observations on Halley's Comet, made at Mackree, Sligo, in the Months of August, September, October and November 1835." By Edward J. Cooper, Esq. Communicated by Capt. Beaufort, R.N., F.R.S.

These observations are communicated in the state in which they were taken, and without the corrections for refraction and parallax, with a view to assist computers in the calculation of a new approximate orbit. They were made principally with the author's equatorial telescope, having a focal length of 25 feet 3 inches, and a clear aperture of 13·3 inches. Some few, however, were taken with the finder, which is 6 feet 6 inches in focal length, and 4·9 inches clear aperture. The eye-pieces used were, one by Frauenhofer (an illuminated wire-micrometer), one by Messrs. Troughton and Simms (an illuminated field-micrometer), a comet eye-piece, and the ordinary eye-piece of the finder. The first of these had a magnifying power of about 400, the second of 226, the third of about 95, and the fourth about 40.

"An Account of the great Earthquake experienced in Chili, on the 20th of February 1835," with a Map. By Alex. Caldcleugh, Esq., F.R.S.

An idea formerly prevailed among the inhabitants of Chili, that the earthquakes of those regions take place at certain regular periods; but it is now sufficiently proved, from the numerous catastrophes of this kind which have occurred during the present century, that they may happen indiscriminately at all times, and in all states of the atmosphere. The author is disposed to place but little reliance on most of the supposed prognostics of these convulsions: but he mentions that, previously to the earthquake described in the present paper, there were seen immense flocks of sea birds, proceeding from the coast towards the Cordillera, and that a similar migration had been noticed prior to the great shock of 1822. From his own observations, he concludes that the barometer usually falls shortly before any considerable shock, and that it afterwards rises to its ordinary mean height. Both before, and also at the time of the convulsion, the volcanos of the whole range of the Cordillera were observed to be in a state of extraordinary activity.

The earthquake began at half-past eleven o'clock in the morning of the 20th of February. The first oscillations of the earth were gentle, and attended with little noise: they were succeeded by two extremely violent tremors, continuing for two minutes and a half, the principal direction of the motion being from south-west to north-east; and they were attended by a loud report, apparently proceeding from the explosions of a volcano to the southward. All the buildings of the town of Conception were thrown down during these undulations. At the expiration of half an hour, when the inhabitants, who, on the first alarm, had fled to the neighbouring heights, were preparing to return to their houses, it was observed that the sea had retreated to such a distance that the ships in the harbour were left dry, and all the rocks and shoals in the bay were exposed to view. At this period an immense wave was seen slowly advancing towards the shore, and, rolling majestically onwards, in ten minutes reached the city of Conception, which was soon overwhelmed in a flood of an altitude of 28 feet above high-water mark. The few persons who had remained in the town had but just time to make their escape, and to behold from the rising grounds, the complete submersion of the city. All objects that were movable were swept away into the ocean by the reflux of this great wave, which was succeeded by several similar, but smaller waves, completing the work of destruction, and leaving behind them, on their final retreat, a scene of universal havoc and desolation.

The island of Santa Maria, which is situate to the southward of the bay of Conception, and is about seven miles broad, and two long, remained, after the earthquake, permanently elevated at least ten feet above its former position; and a similar change was found to have taken place with regard to the bottom of the sea immediately surrounding the island. The amount of this elevation was very accurately ascertained by the observations of Capt. Fitzroy, who had, previously to the earthquake, made a careful survey of the shores of that island; thus supplying the most satisfactory and authentic testimony to this important fact.

The author gives, in the course of the paper, several particulars relating to the effects of the earthquake in different parts of the Chilian coast; the oscillations appearing to have extended to the north as far as Coquimbo, and to the east as far as Mendoza, at the ridge of the great chain of the Andes. Vessels navigating the Pacific Ocean, within a hundred miles of the coast, experienced the shock with considerable force. Its influence was very perceptible in the island of

Juan Fernandez, a basaltic mass 360 miles distant from the coast; as was shown by the sudden elevation and subsidence of the sea, which at one time rose 15 feet above the usual level, carrying all before it.

Anniversary Meeting, Nov. 30th, 1835.—John William Lubbock, Esq. V.P. and Treasurer, in the Chair.

After the transaction of certain formal business the Secretary read the Report, from which the following are extracts:

"The Council have to report the following statement of their proceedings during the past year, as far as they relate to matters of general interest to the Society.

"By an arrangement made with the Trustees of the British Museum, a sum of 165*l*. has been placed at the disposal of the Library Committee for the purchase of books, in consideration of a grant by the Society to the British Museum of fifty-five volumes of Oriental Manuscripts.

"The printing of the classed Catalogue, under the direction of Mr. Panizzi, is in great forwardness, and will soon be completed.

"The Council have the satisfaction of reporting, that the Committee appointed, in compliance with the wishes of the Lords Commissioners of His Majesty's Treasury, and of the Honourable Board of Excise, for the purpose of giving their opinion on the construction of instruments and tables for ascertaining the strength of spirits, in reference to the charge of duty thereon, have nearly completed their labours, and will very shortly be ready with their Report.

"The Copley Medal for the present year has been awarded to William Snow Harris, Esq., for his 'Experimental Investigations of the Forces of Electricity of high Intensity,' contained in his paper published in the Philosophical Transactions for the year 1834 (p. 213; Proceedings, p. 277. No. 16.)

"One of the Royal Medals for the present year has been awarded to Michael Faraday, Esq., for his investigations and discoveries contained in the series of 'Experimental Researches in Electricity,' published in the Philosophical Transactions, and more particularly for the Seventh Series, relating to the definite nature of Electro-chemical Action. (Phil. Trans. for 1834, p. 77; and Proceedings, p. 261, No. 15.)

"The other Royal Medal for the present year has been awarded to Sir William Rowan Hamilton, Andrews Professor of Astronomy in the University of Dublin, for the papers published by him in the 16th and 17th volumes of the Transactions of the Royal Irish Academy, entitled 'Supplement to an Essay on the Theory of Systems of Rays,' and more particularly for those investigations at the conclusion of the third and last Supplement, which relate to the discovery of Conical Refraction.

"The Council propose, in the year 1838, to give one of the Royal Medals to the most important unpublished paper on Chemistry, and the other Medal to the most important unpublished paper on Mathematics, which shall have been communicated to the Royal Society for publication in its Transactions, after the present date and prior to the month of June 1838.

The Secretary also read the following List of Fellows deceased since the last Anniversary: viz.

On the Home List.—His Royal Highness the Duke of Gloucester;

Sir William Blizard, Knt.; Sir David Barry, Knt.; The Marquis of Breadalbane; The Earl of Charleville; The Bishop of Cloyne; The Earl of Darnley; Lord De Dunstanville; Colonel Sir Augustus Simon Frazer, K.C.B.; Major-General Hardwicke; Captain Kater; Rev. Thomas Robert Malthus, Thomas James Mathias, Esq.; William George Maton, M.D.; Rev. Robert Morrison, D.D.; Michael Thomas Sadler, Esq.; Richard Sharp, Esq.; William Smith, Esq.; Edward Troughton, Esq.; Sir George Lemon Tuthill, Knt. M.D.; Ralph Watson, Esq.

On the Foreign List.—Frederich Stromeyer.

The Secretary stated that of these only three, namely, Captain Kater; John Brinkley, Lord Bishop of Cloyne, and Edward Troughton, Esq. have contributed papers to the Royal Society.

Capt. Kater contributed the following papers, fifteen in number, to the Philosophical Transactions.

1. On the light of the Cassegrainian Telescope, compared with that of the Gregorian. (Phil. Trans. 1813, p. 206.)

Having remarked the superiority in the performance of a Cassegrainian telescope over those of similar dimensions in the Gregorian construction, Capt. Kater made a series of experiments to determine the comparative excellence of these two methods of constructing that instrument. From a mean of these experiments and from a consideration of all the circumstances in which they were made, he concludes that the comparative superiority of the Cassegrainian over the Gregorian telescope of equal apertures and magnifying powers, is as 20 to 11, or very nearly twice as great. He conjectures that the superiority of illumination in telescopes of the former construction may possibly depend on their being exempt from the mutual interference of rays meeting in the same point, as happens in the Gregorian telescope, when the small speculum receives the rays after they have arived at the focus, and after they have become sufficiently concentrated to interfere with each other's motion.

2. In a subsequent paper, the experimental research relating to the same subject is further prosecuted, and the conclusion arrived at is, that the illuminating power of the Cassegrainian telescope, as compared to the Gregorian, is in the proportion of $2\frac{1}{2}$ to 1.

3. His next communication to the Society relates to "An improved method of dividing Astronomical circles and other instruments." The general principle of the method there proposed is the same as that of the beam compass; but the apparatus, instead of having points, is furnished with two micrometer microscopes, adjustable to different distances, as aliquot parts of the arc or line to be divided. As a specimen of the method by which this apparatus is to be used, Capt. Kater describes the series of divisions and subdivisions which he thinks most convenient in a circle of two feet diameter.

4. The series of investigations in which Capt. Kater was engaged for many years, relative to the pendulum, commences with a paper entitled, "An account of experiments for determining the length of the Pendulum vibrating seconds in the Latitude of London." To ascertain with exactness the length of the seconds pendulum, an object of considerable importance in Physical Science, was scarcely

possible by the methods which had been before resorted to: for the determination of the precise centre of oscillation of a body vibrating as a pendulum, depending as it does on the regular figure and uniform density of that body, involves difficulties which might be regarded as insurmountable. Capt. Kater fortunately discovered the means of solving this problem, by the application of a mathematical property already known to belong to the centre of oscillation, but which had never hitherto been practically employed with this view; namely, that this centre and the centre of suspension are reciprocal to one another: that is to say, that if a body, vibrating as a pendulum, be inverted, and suspended by its former centre of oscillation, its former point of suspension will become its centre of oscillation in its new position; and the vibrations in both positions will be performed in equal times. This property, therefore, furnishes an easy method of determining the exact distance between these two points, in a body of any form, or however irregular may be the densities of its different parts; for it will be only necessary, for that purpose, to provide a second axis of suspension, placed by estimation very near to the centre of oscillation, while the body is vibrating on its first axis, and also capable of adjustment as to distance, and as to its being kept in the line passing through the first axis, and the centre of gravity: thus by repeated trials of the number of vibrations performed, in a given time, by that body, when suspended on either of these two axes, and by altering the place of the moveable axis until this number becomes the same in both positions, we obtain a final adjustment which gives the exact distance between the centres of suspension and oscillation in that body; a distance equivalent to the length of a simple pendulum performing the observed number of vibrations in a certain time.

The mode of suspension adopted by Capt. Kater was the knife-edge, of which he points out the various advantages and disadvantages, and the methods he took for overcoming the difficulties of the inquiry. By employing the method of coincidences he found that the number of vibrations made by the pendulum in twenty-four hours might be obtained to within half a second of the truth in the space of eight minutes: and he then applied the usual correction for the extent of the arc of vibration, and also for the height of the place of observation above the level of the sea.

5. This paper was followed by another, "On the length of the French Metre estimated in parts of the English Standard:" in determining which he employed the same micrometer microscopes as were used in the pendulum experiments, bringing them alternately over the metre and over the standard scale, placed in the same plane parallel to and in contact with one another; care being taken that their temperatures were the same.

6. In the following year (1819) Capt. Kater gives an "Account of experiments for determining the Variation in the Length of the Pendulum vibrating Seconds, at the principal stations of the Trigonometrical Survey of Great Britain:" a paper which is full of laborious calculations, founded on the observations therein detailed. The investigation of the diminution of terrestrial gravity from the equator

to the pole is pursued by the comparison of determinations of the length of the seconds pendulum at various stations: and is founded on the theorem demonstrated by Clairaut, that the sum of the two fractions expressing the ellipticity and the diminution of gravity from the pole to the equator is always a constant quantity, and is equal to $2\frac{1}{2}$ times the fraction expressing the ratio of centrifugal force, and that of gravity at the equator. The extreme degree of accuracy with which the force of gravitation may be determined by the apparatus employed by Capt. Kater, suggested to him the possibility of ascertaining by its means minute variations in this force observable in passing through a country composed of materials of various degrees of density: instances of the occurrence of which are given in this paper.

7. In the year 1823, Capt. Kater communicated to the Royal Society an account of experiments made with an invariable pendulum belonging to the Board of Longitude, by Sir Thomas Brisbane and Mr. Dunlop, at Paramatta in New South Wales, and thence deduces the fraction expressing the terrestrial compression.

8. In a paper which appeared in the Phil. Trans. for 1821 (p. 75.) Capt. Kater gives an account of the comparison which he instituted of various British Standards of Linear Measures for the purpose of accurately examining the standard yard employed by General Roy, in the measurement of a base on Hounslow Heath, as a foundation for the trigonometrical operations carried on by the Ordnance throughout the country. He found material differences to exist between the standards of Sir George Shuckburgh, of Bird, of the Royal Society, of General Roy's, and of the one constructed by Ramsden, which was used in the trigonometrical survey. Capt. Kater then proceeds to investigate the effect of these differences on the figure of the earth.

9. Sir George Shuckburgh Evelyn had, in the course of his inquiries respecting a standard of weights and measures, examined with great care the weights of a standard cube, cylinder, and sphere, and the methods employed for this purpose had been minutely described; but the mode of ascertaining the dimensions of these bodies had not been so fully detailed. Capt. Kater was accordingly desirous of re-investigating this latter branch of the subject before the Commissioners of Weights and Measures should make their final report. The apparatus he employed for this purpose, and the results of his experiments, are stated in a paper also published in the Philosophical Transactions for 1821.

10. These researches were continued by Capt. Kater in the year 1825; and the details are given in a paper published in the Phil. Trans. for 1826, and entitled "An Account of the construction and adjustment of the new standards of weights and measures of the United Kingdom of Great Britain and Ireland."

11. The series was completed in 1830 by the account he gives of the detection of a source of error in estimating the standard of linear measure, arising from the thickness of the bar, on the surface of which the lines are traced, and of the means he took to obviate it.

12. The attention of Capt. Kater was at one time directed to the

ascertaining the best kind of steel for the construction of a compass needle, the most advantageous form to be given to the needle, and the most effective mode of communicating to it magnetism. Many curious and unexpected results were obtained in the course of this investigation.

13. A remarkable volcanic appearance in the moon being observed by Capt. Kater in February 1821, he communicated to the Society shortly afterwards an account of the phænomenon, which was published in the Phil. Trans. for the same year.

14. One of the greatest benefits conferred on science by Capt. Kater was his invention of the floating collimator, an instrument of which the object is to determine the situation of the line of collimation of a telescope attached to an astronomical circle, with respect to the zenith or the horizon in any one position of the instrument; or in other words, to determine the zero-point of the divisions on the limb: an operation which was before usually performed by the use of the level or the plumb-line, or by the reflexion of an object from the surface of a fluid. Each of these methods was liable to many inconveniences and defects: all of which are avoided in the floating collimator. The principles on which this instrument is constructed are two; the first is the property of a telescope employed by Gauss, and subsequently by Bessel, in virtue of which the cross wires of a telescope adjusted to distinct vision on the wire, may be distinctly seen by another telescope also similarly adjusted, at whatever distance the telescope may be placed, provided their axes coincide; in which case the rays diverging from the cross wires of either telescope, will emerge parallel from its object-glass, and will therefore be refracted by that of the other telescope to its sidereal focus, as if they came from an infinite distance. The other principle, which is employed as a substitute for the common level, is the invariability with respect to the plane of the horizon, of the position of a body of determinate figure and weight, when floating on the surface of a fluid. Thus the telescope being attached to a box floating on mercury, and serving as a stand to the telescope, may be fixed either in a horizontal or a vertical position; in which latter case the reverse observations may be made by merely turning the float half round in azimuth.

15. The later improvements made by Capt. Kater in the vertical floating collimator are described by him in a subsequent paper published in the Philosophical Transactions for 1828. Besides obviating the sources of error arising from the necessity of transferring the instrument to different sides of the observatory, and of taking the float out of the mercury and replacing it at each observation, the vertical floating collimator has the further advantage of being adapted for use, not only with a circle, but also with a telescope, either of the refracting or reflecting kind. Such a telescope, furnished with a wire micrometer, and directed to the zenith, becomes a zenith telescope, free from all the objections to which the zenith sector, and the ordinary zenith telescopes with a plumb-line, are liable. From the greater degree of precision attainable by the employment of this instrument, from the facility of its construction, the readiness

of its application, and the economy of time resulting from its use, the employment of the level and plumb-line may be wholly superseded.

John Brinkley, Lord Bishop of Cloyne, commenced his scientific career, while Andrews Professor of Astronomy in the University of Dublin, by a mathematical paper published in the Phil. Trans. for 1807, containing an investigation of the general term of an important series in the inverse method of finite differences. In 1810 Dr. Maskelyne, then Astronomer Royal, announced to the Society by the communication of a letter from Dr. Brinkley, the supposed discovery by the latter of the annual parallax of α Lyræ, which he was confident exceeds 2″. In 1818 he reported having met with apparent motions in several of the fixed stars which he could explain only by referring them to parallax. Among these α Aquilæ exhibited the greatest change of place. The observations made at the Greenwich observatory not being in accordance with those made at Dublin, Dr. Brinkley, in a subsequent paper published in the Phil. Trans. for 1821, institutes a new series of observations with a view to discover the source of this discordance. In conclusion he states his inability to discover any explanation of this difference, or to obtain any result opposed to his former conclusions. He remarks, however, that the discrepancies between his observations and those made at Greenwich may by some be considered as showing the great precision of modern observations, since the whole extent of the absolute difference is only one second. In the last paper on this important subject, which was published in the Phil. Trans. for 1824, Dr. Brinkley endeavours to form a correct estimate of the absolute and relative degrees of accuracy of the instruments at Dublin and at Greenwich. He first considers the difference of parallax between γ Draconis and α Lyræ, and secondly the absolute parallax of α Lyræ.

Four other papers by the same author are also contained in the Philosophical Transactions: the first in 1819, giving the results of observations made at the observatory of Trinity College, Dublin, for determining the obliquity of the ecliptic, and the maximum of the aberration of light; the second, published in 1822, containing the investigation of the elements of a comet observed by Captain Basil Hall; the third published in 1824, on the North Polar distances of the principal fixed stars; and the last, which appeared in 1826, communicating the results of the application of Capt. Kater's floating collimator to the astronomical circle at the observatory of Trinity College, Dublin. He regards the results of these observations as highly favourable to the principle of the collimator, which he considers as a new astronomical power, and as even belonging to a more advanced era of practical astronomy than the present.

Mr. Edward Troughton is the author of a paper in the Phil. Trans. for 1809, entitled "An Account of a method of dividing Astronomical and other instruments by ocular inspection; in which the usual tools for graduating are not employed; the whole operation being so contrived, that no error can occur but what is chargeable to vision when assisted by the best optical means of viewing and measuring minute quantities." The intrinsic excellence of Mr. Troughton's method, as detailed in this paper, consists in the process of examination employed to correct the imperfections in laying down the divisions by methods which give only approximate degrees of accuracy.

The Copley Medal, and the two Royal Medals for the present year were delivered, pursuant to the awards made by the Council.

The ballot for the election of Council and Officers being taken, the Scrutators reported the following as the result.

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

Other Members of the Council: William Allen, Esq.; Rev. William Buckland, D.D.; Samuel Hunter Christie, Esq.; Rev. James Cumming; Davies Gilbert, Esq.; Joseph Henry Green, Esq.; Henry Holland, M.D.; William Lawrence, Esq.; John William Lubbock, Esq.; Herbert Mayo, Esq.; Roderick Impey Murchison, Esq.; Rev. Robert Murphy, M.A.; Sir John Rennie; William Henry Smyth, Capt. R.N.; Edward Turner, M.D.; Rev. William Whewell.

GEOLOGICAL SOCIETY.

(*Nov.* 18, 1835, *continued.*)—" Geological notes made during a survey of the East and West Coasts of South America, in the years 1832, 1833, 1834, and 1835, with an account of a transverse section of the Cordilleras of the Andes between Valparaiso and Mendoza;" by F. Darwin, Esq., of St. John's College, Cambridge; communicated by Prof. Sedgwick, were afterwards read.

Prof. Sedgwick began by observing that the notes were extracted from a series of letters (addresssd to Professor Henslow), containing a very great mass of information connected with almost every branch of natural history; and that he had selected for the occasion those remarks only which he thought more especially interesting to the Geological Society.

Mr. Darwin's first letter contained some account of St. Jago (one of the Cape Verde Islands), which he visited early in 1832; and he considered that he had good evidence of its recent elevation, as he found on its surface beds of *recent shells and corals* considerably above the actual level of the sea.

In various portions of the notes he shortly described the vast extent of primary rocks along the shores of Patagonia, the existence of highly crystalline schists in the Falkland Islands, alternating with micaceous slaty sandstone, exhibiting the casts of bivalves (Terebratulæ), and encrinital stems, and a rock near Cape Famine containing "some sort of Ammonites." On the line of the western coast of South America, from Chiloe to Tres Montes, he found a widely extended formation of mica-slate, traversed and burst through by a grand transverse chain of granite, and penetrated by innumerable dykes of great complexity of mineral structure.

From the position of the tertiary deposits, which exist on both sides of the Southern Andes, he concludes that the primary chain must have had a great elevation anterior to the tertiary period: and he thinks that a rough approximation may be made to the date of the commencement of the volcanic period, by observing the first association of streams of lava, with certain tertiary groups on the Patagonian side.

A considerable portion of the extracts was devoted to a description of the great tertiary groups on both sides of the chain of the Andes. Some of the details respecting the eastern side were derived from observations made on the Rio Negro, and on the line of a transverse section from Rio Santa Cruz to the base of the Cordilleras. These exhibit the structure of what Mr. Darwin calls the great southern tertiary formations of Patagonia.

The lowest of these formations appears to be of great extent and thickness, and in one instance was found to contain a bed of ancient lava, which seemed to mark the commencement of the eruption from the craters of the great chain of the Andes. It is characterized by a great Oyster, and by other shells and corals, *some* of which belong to species now living on the neighbouring coasts. Over it is a deposit which Mr. Darwin describes as chiefly composed of rolled porphyry pebbles, which he had himself traced for more than 700 miles. Overlying all the rest, and at a greater elevation above the level of the sea, were beds of recent shells, identical in species with the littoral shells of the neighbouring shores. Among these, he more especially notices a widely extended bed of Muscles, which still retain their blue colour, and emit an animal smell when thrown in the fire. From these facts, he thinks the tertiary deposits of Patagonia may be separated into distinct periods, somewhat similar to those derived by Mr. Lyell from a comparison of the newer deposits of Europe: and in making the transverse section, he thought that he saw traces in the valley of Santa Cruz of an ancient channel, which must have traversed a great portion of the south part of the continent before the elevation of the tertiary groups.

In noticing the groups on the western side of the Andes, he describes an old tertiary deposit (eocine or miocene?) south of Rio Maypo, and abundance of recent shells 1300 feet above the same level. He also describes the association of lava with beds containing recent shells in the island of Chiloe. Among other facts, he notices the appearance of pitchstone among the beds of lava, and the occurrence of a forest growing over a bed of recent oysters 350 feet above the actual level of the sea. All these *recent shells* are the *littoral shells* of the neighbouring shores; from which he concludes that the elevation must have been gradual, or by successive hitches, similar to those by which the coast of Chili and, more recently, the coast of Chiloe have been unquestionably elevated.

In addition to these very remarkable notices, Mr. Darwin mentions other *tertiary deposits* at Chiloe and Conception, composed of beds of sandstone and carbonaceous shale without shells, but containing many silicified trunks of dicotyledonous trees, and alternating with beds of lava.

During the progress of the four years' survey (in addition to the traverse above mentioned), Mr. Darwin crossed from Rio Negro to Buenos Ayres by Sierra de la Ventana, a chain almost unknown to geographers. He found two immense collections of large bones (of Mastodons) near Santa Fé, but in a condition not to admit of their being removed. He also found bones of a species of Mastodon at Fort St. Julian, S. lat. 50°, and more than 600 miles from the former localities. In one instance the bones appear to have been associated with marine shells. In the gravel of Patagonia he also found many bones of the Megatherium and of five or six other species of quadrupeds, among which he has detected the bones of a species of Agouti. He also met with several examples of the polygonal plates of the Megatherium, which at first induced him to regard the animal as a gigantic Armadillo. A very large collection of these fossils has been sent to England, and are in the custody of Professor Henslow till Mr. Darwin's return.

Professor Sedgwick concluded by reading extracts from two letters describing a section transverse to the Andes, extending from Valparaiso to Mendoza. The Cordillera is here composed of two separate and parallel chains. The western chain is composed of sedimentary rocks, distinctly stratified, and resting on granite. The sedimentary rocks (composed of red sandstone, conglomerate, gypsum, &c.) are violently contorted, and dislocated along parallel north and south lines, and as they approach the granite, become so crystalline that they cannot be distinguished from the porphyritic dykes by which they are traversed.

Following the line of section, Mr. Darwin found, at the Pass of Puquenas, elevated 12,000 feet above the sea, that the red sandstone was replaced by a black rock, like clay slate and pale limestone, containing numerous impressions of shells; a Gryphæa? is the most abundant; but he also found Ostrea, Turritella, Ammonites, and a small bivalve (Terebratula?).

At the Portillo Pass is a conglomerate resting on micaceous sandstone, and traversed by great veins of granite. But at the Uspellata Pass (in the eastern chain), he found highly crystalline and felspathic rocks, regularly bedded, and resting on granite, the peaks of which reach the elevation of 14,000 feet. A wider examination of the overlying groups convinced him, not only that they were more recent than the western chain (being partly made up of its *debris*), but that they were of the same age with certain tertiary formations above noticed. For example, he discovered along the line of section, in the eastern chain, beds of sandstone, with silicified trunks of dicotyledonous trees, and beds of carbonaceous shale, resting on an ancient stream of lava, and surmounted by black augitic lava, 2000 feet thick; over all these were five grand alternations of black volcanic rocks and sedimentary deposits, amounting to several thousand feet in thickness. This series, in its structure and fossils, is considered as identical with certain tertiary deposits of Patagonia, Chiloe and Conception; for it loses its mineral character only where it approaches the granite; in which case it is shattered, contorted, and traversed by great veins rising out of the central mass; and its several beds, as well as the fossils they contain, become entirely crystalline. Mr. Darwin further states, that this singular overlying group contains very numerous veins of copper, silver, arsenic, and gold, which may be traced down to the granite; and as a general conclusion, he expresses his conviction that the granite (now rising into central peaks 14,000 feet in elevation), must have been in a fluid state since the tertiary group, above described, was deposited.

Dec. 2nd.—A letter was first read from Capt. Belcher, R.N., F.G.S., addressed to Woodbine Parish, Esq., Sec. G.S., dated 10th of March, 1835, inclosing two others from Lieut. Bowers, R.N., and H. Cuming, Esq.

These letters referred to the effects produced at Valparaiso by the earthquake of November 1822.

Capt. Belcher says that he had carefully searched the Remark-Books of His Majesty's vessels stationed on the Chilian coast, between September 1822 and March 1823, but had not found a notice in any way connected with the Port of Valparaiso. He therefore infers that no British ship of war was present; but he thinks that if the disturbance produced by the earthquake of November 1822 had been of a nature to alter the soundings, or even induce the residents to attach importance to any known changes, they would have formed a subject of special communication by the commanders of ships of war.

Lieut. Bower states in his letter, dated 7th of March 1835, that he was not at Valparaiso at the time of the earthquake, but arrived from England in February 1823, and found everything in the same situation as when he quitted it twelve months previously. He adds, that since the earthquake, the water has gradually receded from the part situated between the landing-place and the market-place, and that a row of stores and substantial dwellings had been erected where the sea formerly flowed.

Mr. Cuming's letter is dated 5th of March 1835.

The writer arrived at Valparaiso in January 1822, and resided there constantly until 1827, and from the latter period, with occasional absences, till May 1831. At the time of the earthquake, he lived in the Plaza Mayor, near the landing-place at the Arsenal, and his house was destroyed by the first shocks. He did not go to the beach during the night, but was informed that the sea had retired a considerable distance, and had returned with great force. On the morning of the 20th he noticed the effects, but found nothing more than a high tide. He never heard of the rocks having been heaved up, or of the permanent retirement of the sea, until the publication of Mrs. Graham's work, to the statements contained in which neither he nor his friends could subscribe.

Mr. Cuming's pursuit of conchology and natural history generally, caused him to visit frequently the rocks and inlets with which the northern and southern parts of the Bay abound; but though the rocks were covered with Fuci, Patellæ, Chitons, Balani, &c., yet he never perceived the least difference in their appearance from the date of his arrival to his finally quitting Valparaiso. He mentions particularly, as points which he often examined, the Caleta, the Quebràda de Dios, and the Cruz de Reyes. He also never found the least trace of the above productions, except in situations covered by the tide.

After the earthquake, Mr. Cuming resided in a house in the Arsenal, where the spring tides came up to the same mark as they did previously to that event. He refers especially to the tides of 1822 and 1823.

Another circumstance which convinced Mr. Cuming that no change of level had taken place was the existence of a small detached rock opposite the Estanco, half-way between the Custom-house and the Market-place, and about fifty yards from the walls at half tide. From this rock he had often taken *Concholepas Peruviana* previously to the earthquake, and subsequently it retained the same position.

The vessels occupied the same anchorage as they did before November 1822; and nautical men affirmed that there was not the least difference in the depth of the water in any part of the Bay.

The opinion that a change had taken place in the relative level of land and sea, Mr. Cuming conceives originated in the accumulation of detritus at points where the tide flowed anterior to the earthquake, and on which houses, and even small streets have been since erected. Though these accumulations appear to have been going on between 50 and 80 years, yet they were small previously to the violent rains in June 1827, which brought down into the bay the loose granitic soil of the hills and the ravines. This detritus has since been thrown up by the tides, and formed into a firm open space exceeding 250 feet in breadth, on which the buildings have been erected.

The quantity of matter thus carried into the Bay has not affected the anchorage, and Mr. Cuming, when dredging within two hundred yards of low-water mark, never found a grain of decomposed granite, or any kind of recent soil, but fine sandy mud, well stocked with several species of shells of mature growth.

Both to the northward and southward of Valparaiso, where the coast is open, namely, at Lagunilla, Vina del Mar, Con-Con, and Quintero, the sea has thrown up high banks of sand, many feet above the level of the land behind them, and reaching inland from 1000 to 2000 feet, and at Quintero to a much greater extent. At this place the sand contains beds of shells "in a semi-fossil state." Mr. Cuming visited these localities previously to the earthquake, and often subsequently, but never saw a shell beyond the range of high water, except those in the state above mentioned, and the owners declared that no change had taken place.

Mr. Cuming also states that about 70 years since, he believes at the same time that Conception was visited by an earthquake, Valparaiso was also visited. The sea retired to a very great distance, and the reflux was so violent that it destroyed all the houses, carrying the boats and canoes to the church of San Francisco, which is about a quarter of a mile on a gradual ascent from where the tide usually flows.

A paper on the effects of the Earthquake-waves on the Coasts of the Pacific, by Woodbine Parish, Esq., Sec. G.S., was afterwards read, and will be inserted at length in our next number.

ZOOLOGICAL SOCIETY.

(Continued from vol. vii. p. 534.)

Oct. 27, 1835.—The following "Observations on the Habits, &c. of a male *Chimpanzee, Troglodytes niger*, Geoff., now living in the Menagerie of the Zoological Society of London, by W. J. Broderip, Esq., V.P.Z.S., F.R.S., &c.," were read:—

"The interesting animal whose habits in captivity I attempt to describe, was brought to Bristol in the autumn of this year by Capt. Wood, from the Gambia coast. The natives from whom he received it, stated that they had brought it about one hundred and twenty miles from the interior of the country, and that its age was about twelve months. The mother was with it, and, according to their report, stood four feet six inches in height. Her they shot,—and so became possessed of her young one; and those who have seen our animal will well understand what Dr. Abel means, when, in his painful description of the slaughter of an Asiatic *Orang* (*Pithecus Satyrus*, Geoff.), he observes that the gestures of the wounded creature during his mortal sufferings, the human-like expression of his countenance, and the piteous manner of his placing his hands over his wounds, distressed the feelings of those who aided in his death, and almost made them question the nature of the act they were committing. During the period of his being on ship-board, our *Chimpanzee* was very lively. He had a free range, frequently ran up the rigging, and showed great affection for those sailors who treated him kindly.

"I saw him for the first time on the 14th instant, in the kitchen belonging to the Keeper's apartments. Dressed in a little Guernsey shirt, or banyan jacket, he was sitting child-like in the lap of a good old woman, to whom he clung whenever she made a show of putting him down. His aspect was mild and pensive, but that of a little withered old man; and his large eyes, hairless and wrinkled visage, and man-like ears, surmounted by the black hair of his head, rendered the resemblance very striking, notwithstanding the depressed nose and the projecting mouth. He had already become very fond of his good old nurse, and she had evidently become attached to her nursling, though they had been acquainted only three or four days; and it was with difficulty that he permitted her to go away to do her work in another part of the building. In her lap he was perfectly at his ease; and it seemed to me that he considered her as occupying the place of his mother. He was constantly reaching up with his hand to the fold of her neck-kerchief, though when he did so she checked him, saying "No, Tommy, you must not pull the pin out." When not otherwise occupied, he would sit quietly in her lap, pulling his toes about with his fingers, with the same pensive air as a human child exhibits when amusing itself in the same manner. I wished to examine his teeth; and when his nurse, in order to make him open his mouth, threw him back in her arms and tickled him just as she would have acted towards a child, the caricature was complete.

"I offered him my ungloved hand. He took it mildly in his, with a manner equally exempt from forwardness and fear;—examined it with his eyes, and perceiving a ring on one of my fingers, submitted that and that only to a very cautious and gentle examination with his teeth, so as not to leave any mark on the ring. I then offered him my other hand with the glove on. This he felt, looked at it, turned it about, and then tried it with his teeth. His sight and his ordinary touch seemed to satisfy him in the case of a natural surface, but, as it appeared to me, he required something more to assure his senses when an artificial surface was presented to him; and then he applied the test of his teeth.

"At length it became necessary for his kind nurse to leave him; and after much remonstrance on his part, she put him on the floor. He would not leave her, however, and walked nearly erect by her side, holding by her gown, just like a child. At last she got him away by offering him a peeled raw potato, which he ate with great relish, holding it in his right hand. His keeper, who is very attentive to him, and whom he likes very much, then made his appearance, and spoke to him. Tommy (for by that name they call him) evidently made an attempt to speak too, gesticulating as he stood nearly erect, protruding his lips, and making a hoarse noise "hoo-hoo" somewhat like a deaf and dumb person endeavouring to articulate. He soon showed a disposition to play with me, jumping on his lower extremities opposite to me like a child, and looking at me with an expression indicating a wish for a game of romps. I confess I complied with his wish, and a capital game of play we had.

"On another occasion, and when he had become familiar with me, I caused, in the midst of his play, a looking-glass to be brought, and held it before him. His attention was instantly and strongly arrested: from the utmost activity he became immoveably fixed, steadfastly gazing at the mirror with eagerness and something like wonder depicted on his face. He at length looked up at me—then again gazed at the glass. The tips of my fingers appeared on one side as I held it—he put his hands and then his lips to them—then looked behind the glass—then gazed again at its surface—touched my hand again, and then applied his lips and teeth to the surface of the glass—looked behind again, and then, returning to gaze, passed his hands behind it, evidently to feel if there was anything substantial there. A savage would have acted much in the same way, judging from the accounts given of such experiments with the untutored natives of a wild and newly discovered land.

"I broke a sugared almond in two, and, as he was eating one half, placed the other, while he was watching me, in a little card-box which I shut in his presence—as soon as he had finished the piece of almond which he had, I gave him the box. With his teeth and hands he pulled off the cover, took out the other half, and then laid the box down. He ate the kernel of this almond, rejecting the greatest part of the sugary paste in which it was incased, as if it had been a shell: but he soon found out his error; for, another almond being presented to him, he carefully sucked off the sugar and left the kernel.

I then produced a wine-glass, into which I poured some racy sherry, and further sweetened it with sugar. He watched me with some impatience, and when I gave him the glass he raised it with his hands to his lips, and drank a very little. It was not to his taste, however, for he set down the glass, almost as full as he had taken it up; and yet he was thirsty, for I caused a tea-cup with some sugared warm milk and water to be handed to him, and he took up the cup and drained it to the last drop.

"I presented him with a cocoa-nut, to the shell of which some of the husk was still adhering: the tender bud was just beginning to push forth—this he immediately bit off and ate. He then stripped off some of the husk with his teeth, swung it by the knot of adhering husk-fibres round his head, dashed it down, and repeatedly jumped upon it with all his weight. He afterwards swung it about and dashed it down with such violence that, fearing his person might suffer, I had it taken away. A hole was afterwards bored through one of the eyes, and the cocoa-nut was again given to him. He immediately held it up with the aperture downwards, applied his mouth to it, and sucked away at what milk there was with great glee.

"As I was making notes with a pencil, he came up, inquisitively looked at the paper and pencil, and then took hold of the latter. Before I gave it up, I drew the pencil into the case, foreseeing that he would submit the pencil-case to examination by the teeth. Immediately that he got it into his possession, he put the tip of his little finger to the aperture at the bottom, and having looked at it, tried the case with his teeth.

"While his attention was otherwise directed I had caused a hamper containing one of the *Pythons* to be brought into the room and placed on a chair not far from the kitchen dresser. The lid was raised, the blanket in which the snake was enveloped was opened, and soon after Tommy came gamboling that way. As he jumped and danced along the dresser towards the basket, he was all gaiety and life. Suddenly he seemed to be taken aback, stopped—then cautiously advanced towards the basket, peered or rather craned over it—and instantly with a gesture of horror and aversion, and the cry of Hoo! hoo! recoiled from the detested object, jumped back as far as he could, and then sprang to his keeper for protection. He was again put down, his attention diverted from the basket, and, after a while, tempted to its neighbourhood by the display of a fine rosy-cheeked apple, which was at last held on the opposite rim of the hamper. But no—he would evidently have done a good deal to get at the apple; but the gulf wherein the serpent lay was to be passed, and after some slight contention between hunger and horror, off he went and hid himself. I then covered up the snake, and after luring him out with the apple, placed it on the blanket—No. I then shut down the lid—still the same desire and the same aversion. I then had the hamper, with the lid down, removed from the chair on which it had been placed to another part of the room. The apple was again shown to Tommy and placed on the lid. He advanced cautiously, looking back at the empty chair and then at the hamper: he advanced further with evident reluctance, but when he approached near he peered forward toward the basket, and, as if overcome by fright, again ran back and hid himself under his cage.

"I now caused the hamper with the serpent to be taken out of the room. Our friend soon came forward. I showed him the apple and placed it on the chair. He advanced a little, and I patted his head and encouraged him. He then came forth and went about the room, looking carefully as if to satisfy himself that the snake was gone—advanced to the chair more boldly,—looked under it—and then took the apple and ate it with great appetite, dancing about and resuming all his former gaiety.

"We know that there are large constricting serpents in Africa; and as the animal must have been very young when separated from its parent, I made this experiment in particular to try his instinct: it succeeded to the entire satisfaction of the witnesses who were present.

"He manifested aversion to a small living tortoise, but nothing like the horror which he betrayed at sight of the snake. I was induced to show him the former by the account of the effect produced by *Testudinata* on the Asiatic *Orang*, whose habits are so admirably described by Dr. Abel and Captain Methuen, who brought the animal to England.

"Tommy, among other exercises, is very fond of swinging. He places himself on the swing, generally in a sitting posture, holding on each side with his hands. He not unfrequently puts up his feet and grasps the cord on either side with them too, appearing more at home on his slack rope than Il Diavolo Antonio himself.

"James Hunt, one of the keepers, has observed him frequently sitting and leaning his head on his hand, attentively looking at the keepers when at their supper, and watching, to use Hunt's expression, "every bit they put into their mouths." Fuller, the head keeper, informs me that our *Chimpanzee* generally takes his rest in a sitting posture, leaning rather forward with folded arms and sometimes with his face in his hands. Sometimes he sleeps prone, with his legs rather drawn up, and his head resting on his arms.

"Of the *black Orangs* which I have seen, Tommy is by far the most lively. He is in the best health and spirits, and is a very different animal from the drooping, sickly *Chimpanzees* that I have hitherto seen. A good deal of observation made on the Asiatic *Orangs* which have been exhibited in this country, satisfies me that the intelligence of the African *Orang* is superior to that of the Asiatic. This intelligence is entirely different from that of a well-educated dog or a mere mimic, and gives me the idea of an intellect more resembling that of a human being than of any other animal, though still infinitely below it.

"The *Pygmy* of Tyson and the *black Orang* dissected by Dr. Traill, and so well described by him in the 'Wernerian Transactions,' are both stated to have progressed generally by placing their bent fists on the ground and so advancing: indeed Dr. Traill says that the individual which he saw never placed the palms of the hands on the ground. The progression of Dr. Abel's *red* or *Asiatic Orang* is described to have been after the same fashion. Whether it is that our *Chimpanzee* is in better health and more lively, I know not, but he certainly passes a great deal of his time in a position nearly approaching to erect, nor does he, generally, place the bent knuckles to the ground. He will often stand on the top of his cage and apply the palms of his hands to the smooth surface of the wall against which it stands. It is said that a spectator who saw him thus employed, with his back to the company, dressed in his little banyan jacket and woollen cap, was told by a companion to look at the monkey, as he profanely called him. "Where is he?" was the reply. "Why there on the top of the cage," was the answer. "What!" said the first, "that little man who is plastering the wall?"

"Tommy does not like confinement, and when he is shut into his cage, the violence with which he pulls at and shakes the door is very great, and shows considerable strength; but I have never seen him use this exertion against any other part of the cage, though his keeper has endeavoured to induce him to do so in order to see whether he would make the distinction. When at liberty he is extremely playful, and, in his high jinks, I saw him toddle into a corner where an unlucky bitch was lying with a litter of very young pups, and lay hold of one of them, till the snarling of the mother and the voice of his keeper, to which he pays instant respect, made him put the pup down. He then climbed up to the top of the cage where the *Marmozets* were, and jumped furiously upon it, evidently to astonish the inmates, who were astonished accordingly, and huddled together, looking up in consternation at this dreadful pother o'er their heads. Then he went to the window, opened it and looked out. I was afraid that he might make his escape: but the words "Tommy, no!" pronounced by his keeper in a mild but firm tone, caused him to shut the window and come away. He is in truth a most docile and affectionate animal, and it is impossible not to be taken by the expressive gestures and looks with which he courts your good opinion, and throws himself upon you for protection against annoyance.

"It must be remembered that though I have not observed our *Chimpanzee* to progress with his bent knuckles touching the ground, as I have seen the *Asiatic Orangs* move, there is no reason for doubting the accurate descriptions of Tyson and Dr. Traill. I consider it as my province to relate faithfully what I saw, and I have only seen our *Chimpanzee*, as yet, in a small room, where a very few paces will bring him to a chair, a leg of a dresser, or some other piece of furniture which enables him to call into action his prehensile hands and feet, so admirably adapted to his arboreal habits. The narrowness of the *pelvis*, the comparatively inferior development of the *glutæi* * and *gastrocnemii* muscles, and other peculiarities of conformation so ably

* This must be understood as limited to a comparison with the same muscles in man; for there is in the *Chimpanzee* as Mr. Owen observes, "a provision for a more extended attachment for the *glutæi* muscles, in a greater breadth of the *ilia* between the superior spinous processes, than is observed in the inferior *Simiæ*."

pointed out by Tyson, Dr. Traill, and others, but more particularly by Mr. Owen, show that the erect, or, more properly speaking, the semi-erect position, is not the natural one; though my observations upon living *Asiatic Orangs* and *Chimpanzees* accord with the inference drawn by Mr. Owen from the comparative organization of the latter, viz. that the semi-erect position is more easily maintained by the *Chimpanzee* than by any of the other known *Simiæ*.

"The great intelligence and strength of the individual now in the menagerie of the Society, added to the state of its dentition, raised a doubt in my mind as to the accuracy of the report of its age; and I wrote to my friend Mr. Owen my suspicion that he might be older than he was said to be. I received the following reply, in which so much valuable information is concentrated that I feel it to be due to those who may think this memoir worthy of attention to give it as I received it.

'21st October, 1835.

"'My dear Broderip,—I feel that we have no data towards determining with certainty the exact age of the young *Chimpanzee* at the Gardens: its present state of dentition corresponds to that which our own species presents during the period of from 2 to 7 years, viz. incisors $\frac{4}{4}$, canines $\frac{2}{2}$, molars $\frac{4}{4}$, all of which belong to the deciduous series. The deciduous canines appear in the human jaws before the completion of the second year; and those of the *Chimpanzee* are certainly the temporary ones, but are protruded by the enlarged germs of the permanent teeth behind them, so as to appear larger than natural. From this circumstance and from the space already existing beyond the deciduous molars, I infer that the appearance of some of the permanent teeth is near at hand; and we may still see an additional molar protruding in each jaw before the winter is over, if the poor animal should survive that period.

"'The human child acquires the corresponding permanent molars at the seventh year; and from the appearances on the jaws of our *Chimpanzee* I conclude that its age tallies with that of 5 or 6 years in us. But analogy will be dangerous ground for an inference as to precise age, since it is by no means improbable that, where the brain is so much less developed, the full use of it may be much earlier acquired, such as it is; and that the shedding of the teeth may take place at a proportionally early period.

'Believe me, &c. 'RICHARD OWEN.'

"I now proceed to the measurements of our male specimen, premising that the operation was a work of no small difficulty in consequence of the restlessness of the animal. Indeed I am not sure now about the height, though I am confirmed in the measurements by Mr. Miller and Fuller. The *Chimpanzee* would keep drawing up his legs and putting the *musculus scansorius* detected by Dr. Traill into action; and it was not practicable to make him stand or lie quite straight with his legs entirely extended.

	Ft.	In.
Height from the heel to the top of the head	2	0
Circumference of the bottom of the breast	1	5

	Ft.	In.
Circumference round the hips	1	3¼
——— of the head round the eyes and ears	1	3
Opening of the mouth	0	3½
Height from the middle of the upper lip to the eyebrows	0	3⅛
Length from the eyebrows to the *occiput*	0	7½
Diameter of the ear upwards	0	2⅜
Transverse diameter of the same	0	1¾
Circumference of the external edge of the same	0	6½
——— of that part which adheres to the head	0	4⅝
Height from the upper point of the *pubis* to the clavicle	0	10½
Distance between the navel and *sternum*	0	4¼
Distance between the navel and *pubis*	0	3⅛
——— nipples	0	4
Length of the arm from the shoulder to the end of the fingers	1	4½
Circumference of the arm	0	6
——— of the forearm four inches above the wrist	0	6¼
Length of the hand from the wrist to the end of the middle finger	0	5¼
Circumference of the hand	0	4½
Length of the thumb	0	1¼
——— second finger	0	2½
——— middle finger	0	3¼
——— fourth finger	0	3
——— fifth finger	0	2½
Circumference of the thumb and little finger	0	1¼
——— other fingers	0	1⅜
Length of the palm	0	2⅞
Breadth of ditto	0	2
Height from the heel to the extremity of the thigh-bone	0	11½
Length from the heel to the extremity of the middle (longest) toe	0	5½
Circumference of the thigh	0	8¾
——— leg, at its thickest part	0	6
——— foot, taken from the origin of the thumb	0	5¼
Length of the thumb or great toe	0	1⅓
——— second toe	0	2
——— third toe	0	2¼
——— fourth toe	0	2¼
——— fifth toe	0	1⅜
Greatest breadth of the sole at the origin of the thumb or great toe	0	2½
——— near the heel	0	1¾
Circumference of the great toe at the largest point	0	1¾
——— other toes	0	1¼

"On referring to the dimensions given by Daubenton we shall be struck with the stoutness of our specimen as compared with that of the individual which was the subject of his observations.

"It was my intention to have added a particular description of the individual which has been the subject of this memoir; but on carefully inspecting the animal I find Dr. Traill's elaborate description so accurate—(there really is no difference but sex at present)—that I should be needlessly occupying space if I inserted my own; and I beg, therefore, to refer the reader to that gentleman's highly valuable papers in the 'Wernerian Transactions'.

"Since writing the above the cage in which our animal was confined has been enlarged and several barked branches have been nailed to a stem so as to form an artificial tree. These branches he ascends with great activity, and frequently swings with his head downwards, holding on by his lower extremities, and recovering himself with greater agility than any rope-dancer."—W. J. B.

XXXI.—*Intelligence and Miscellaneous Articles.*

OPTICAL EXPERIMENT.

THE following description of an optical experiment will no doubt interest many of the readers of the London and Edinburgh Philosophical Magazine: this experiment is easily performed, and may be seen by several persons at the same time.

Mr. Lipkens, of Voorburg, it is believed, was the first to notice the phænomenon, and on a recent visit to this country was so kind as to call the attention of the writer to the fact, and to allow of its being published in England.

The apparatus which may be most advantageously employed for the development of this phænomenon, consists of a stout hollow spherical glass vessel about six or eight inches in diameter, with an open neck, to which should be attached, by cement or otherwise, a brass cap, furnished with a stop-cock.

The experiment is performed thus: Pour into the globe a *small* portion of water, and then, by means of the lungs or of a condensing syringe, force air into it, the quantity being adjusted according to the strength of the globe. If a lighted candle or lamp be so placed that the flame may be viewed through the horizontal diameter of the spherical vessel while it is charged with condensed air, and supported at the same time in such a position that the neck with the brass cap and stop-cock are uppermost, then every time the finger of the operator placed upon the opening of the stop-cock so as to close it, is raised, a sudden and rapid escape of a portion of the confined air or vapour taking place, it will be observed that the image of the flame will appear with a halo of coloured light. Generally the first escape gives a light yellow halo surrounded with a red circular fringe; the second liberation a blue halo; the third shows a green halo, and in most cases the haloes are encompassed by a series of coloured rings.

Before all the condensed air is allowed to escape, the water in the globe may be shaken about in it without any detriment to the experiment.

The phænomena exhibited by this experiment (if dexterously performed) give results something similar to those noticed on the egress of a polarized ray of light after it has traversed at right angles a polished plate of quartz of a given thickness, and cut perpendicularly to the axis of double refraction. For it is well known, that when a ray has thus passed through, if examined by an analysing eye-piece, it exhibits a variable coloured central aperture with rings, the tints of the aperture changing according to the position of the polished crystalline plate with regard to the eye-piece.

So far as the writer was enabled to experiment with his globe he found the results capricious, for he never could predict what would be the precise colour of the first halo, but the mean of a number of experiments gave the order of changes as above related.

Mr. Lipkens favoured the writer in French with his ideas on the subject, which may be translated thus: The cause of the phænomenon presented by the experiment is evidently due to vapours formed in the glass globe by a change of temperature in the air which it incloses, and which takes place, as we know, every time that there is a rapid change in the tension of that air.

The variety of the colours of the haloes which are obtained by allowing the compressed air to escape successively in the way described, might depend on the dimensions of the vesicular molecules of the water, or on the thickness of the envelopes of these molecules. Mr. Lipkens then states, that as far as he knew, the necessary means required to obtain one central colour rather than another, are not yet discovered, and consequently it appears that were philosophers to discover and give a general explanation of the phænomena in question, their time would not be misemployed.

London, Jan. 27, 1836. F. W.

THULITE AND STRÖMITE.

In a list of minerals appended some years since to a volume on crystallography, I gave 92° 30′ and 87° 30′ as the angles of this mineral; and I did so upon the authority of some fragments of a red mineral received by Mr. Heuland from Sweden, having "Thulite" written by Ekeberg on the paper containing them. But I have since found that the fragments I measured were bisilicate of manganese, or, as I have named it, Strömite, of which, or of thulite, I had not at that time seen any other specimen. I shall feel obliged to the Editors of the Phil. Mag. if they will insert this correction. H. J. Brooke.

ON THE MIRAGE, AS SEEN IN CORNWALL.

To the Editors of the Phil. Mag. and Journal of Science.

Gentlemen,

The *mirage* is so frequently seen in this hilly county, that I did not think of sending you the following notice of one which I lately saw, until I recollected that some eminent travellers who had witnessed similar phænomena abroad have deemed them worthy of a full description in their narratives.

Early in the morning of the 25th of September last, while on the coach upon the high ground about a mile and a half west of Truro, I observed towards the south-east, in a valley, the appearance of a beautiful lake, or inlet of the sea, winding round a distant point, and then branching off into several little creeks or sheets of water, which soon lost themselves among the windings of the contiguous vales. The nearest part of this imaginary water was about half a mile from the coach, and so much did it resemble an inlet of the sea, that a fellow passenger, who was a stranger to the locality, could not be persuaded for some time but that it was really such.

The sun had been up about an hour, and the morning was calm and unusually cold. The thick fog which had risen from the valleys, had not quite reached the tops of the hills, and its surface seemed as level as the ocean; so that while reflecting the unclouded beams of the sun, it could not possibly be distinguished from an inlet of the sea. I am, Gentlemen,

Your very humble Servant,

Redruth, 9th December, 1835. R. EDMONDS, Jun.

SUBSIDIARY HYPOTHESIS TO THE ELECTRO-CHEMICAL THEORY OF SIR H. DAVY.

The beautiful theory advanced by Sir H. Davy to account for chemical affinity, by ascribing it to electrical agency, has many supporters, because it is a theory founded on extensive observation and numerous facts. It supplies chemists with a principle capable of accounting for the phænomena ascribed to affinity, and affords a consistent explanation of the chemical agencies of the Voltaic apparatus. Dr. Turner briefly embodies the substance of the theory as follows: "What chemists term chemical attraction or affinity is under this point of view an electrical force arising from particles of a different kind attracting each other in consequence of being in opposite states of electrical excitement. Substances which have a disposition to unite, assume opposite electrical conditions when brought into contact, and the very existence of the compound depends on its elements retaining their state of excitement."

Now granting the validity of this opinion, still there is a *residual phænomenon* to account for, viz. the reason why substances of different kinds assume opposite electrical states when brought into contact. Judging from analogy, we think a reasonable explanation can be found by supposing particles of different substances to possess different capacities for electricity, or a different specific electricity. We know that bodies have different capacities for heat,—thus, we know that mercury at the temperature of 212° contains a very different quantity of heat to that of an equal weight of water at a similar temperature,—and it is a legitimate inference, in perfect conformity with the laws of induction, to assume that bodies also have different specific electricities; that two substances of different kinds, placed under such circumstances that each has an equal opportunity of parting with or retaining its electricity, will contain, when an equilibrium is established between each of them and the atmosphere or surrounding objects, and consequently between each other also, different quantities of electricity. Now we know that if two bodies of a similar nature (say two pith balls) contain dissimilar quantities of electricity, they will attract each other, the one containing the greatest charge being positive with regard to the other; and we may infer from this, if two bodies of a different kind, such as potassium and oxygen, assume, the one a positive and the other a negative electricity, when brought into contact (an admission necessary for the support of Sir H. Davy's theory), that the potassium, which evinces a positive energy, contains more electricity than the oxygen, which becomes negative. If so, if this obtains when both are placed under similar circumstances,—both at an equilibrium of electricity, it follows inevitably that they must have different capacities for electricity; so that the inference of different capacities for electricity is, we think, to the full as clear as the inference of the attractive force or affinity being dependent on electricity. This theory will account for some bodies having a disposition to unite, others evincing no such disposition at all, &c.: thus the two pith balls oppositely electrified, by the one containing an excess and the other a deficiency or at least a smaller quantity than the other, stand in the relation of two dissimilar substances having different specific electricities, and consequently containing different quantities; and in like manner they possess an attraction for each other, but no sooner do they touch than an equilibrium of electricity is established, and they are no longer in the same relative condition, in as much as each now contains the same quantity of electricity; by reason of their specific electricities being equal, they therefore no longer have any attraction for each other. This is very different with regard to oxygen and potassium; these bodies when brought into contact powerfully attract each other, and then still maintain each its peculiar specific electricity, and consequently their mutual attraction continues: hence the reason why the elements of a compound retain their respective states of excitement, upon which the very existence of the compound depends. According to this view, substances which in their natural condition not only have no attraction or affinity for each other, but repel each other, (if any such actually exist,) should be considered as possessing like specific electricities; and the disparity in the force of attraction should be considered to depend on the disparity in the capacity for electricity. To illustrate this by an example, we should consider that the disparity between the specific electricity of oxygen and potassium is so great that they unite the moment they are brought within reach of each other;—then the disparity in the specific electricities of oxygen and hydrogen we should consider as less than that between the two former, and therefore they do not unite when simply brought into contact; but they do not repel each other, therefore they do not possess equal specific electricities; so that by making the disparity more apparent, (which is done by giving each a higher charge, as when an electric spark is passed through them,) they do attract each other, and unite—once united, their natural difference in electrical capacity suffices to hold them in union. Just so the two pith balls do not repel each other, do not evince the similarity of their specific electricities, until made more apparent by their possessing a higher charge than surrounding objects; and when they have parted with their superabundance and attained an equilibrium with surrounding objects the repulsion ceases: so in the other case would the attraction cease, but the elements of the compound cannot lose their relative superabundance and deficiency:—so that we should consider that the difficulty of union of substances and the quantity of additional electrical excitement required to make them combine, is a measure of the difference of their specific electricities. This theory may be applied also to account for a substance evincing one kind of electric energy when about to combine with one substance, and the opposite state when about to combine with another; thus, according to this view, sulphur possesses a greater specific electricity than oxygen and a less specific electricity than mercury, and is consequently positive with regard to oxygen and negative in relation to mercury.

26, Woburn Place, London, August 17, 1835. — Edward B. Walford.

NOTE ON MR. CHALLIS'S PAPER ON CAPILLARY ATTRACTION.

The following editorial note was written with the intention of annexing it to that passage of Mr. Challis's paper inserted in the present number, p. 94, in which mercury is excluded from part of the investigation, as being incapable of adhering, like other fluids, to solid bodies.

We apprehend that it will eventually be found necessary to extend this investigation to the case of mercury, which is as capable of adhering to *metals*, (to platinum, for example,) as water and oil are of adhering to other solids, metals being in fact *moistened* by mercury. See Philosophical Magazine, First Series, vol. lxviii. p. 110, note; and Quarterly Journal of Science, vol. xx. p. 82 *et seq.*; vol. xxi, p. 231 *et seq.* In the note in Phil. Mag. here referred to, the perfect contact of fluids with the solids which they are capable of wetting is attributed to their mutual chemical action; and Mr. Daniell, in the Quarterly Journal, *loc. cit.*, also ascribes it to the "affinity" of the fluid for the solid; but Mr. Faraday appears to refer it to an intermediate species of attraction, "in part elective, partaking in its characters both of the attraction of aggregation and chemical affinity:" see his Exp. Res. in Electricity, Sixth Series, par. 620, 624, in Phil. Trans. for 1834, pp. 66, 67. It is even probable that the results obtained by Link, as described in the last page of Mr. Challis's paper, may be explained in a similar manner, for the order of the heights to which the fluids mentioned ascended is almost precisely that of what we should conceive, *a priori*, would be the relative degrees of an approximate chemical attraction of the fluids for glass or its elements. The mathematical investigation, we may suggest to Mr. Challis, of the entire series of facts related by Mr. Faraday in the memoir alluded to, and which are ascribed by that philosopher to the mediate attraction in question, could not fail materially to contribute to the elucidation of this obscure though very important and interesting subject.—E. W. B.

M. BREITHAUPT'S MINERALOGY.

In No. 27 of this work we noticed two varieties of wavellite from Frankenberg, and stated, upon what we now find to have been inaccurate information, that they had been named by M. Breithaupt Peganit and *Pegmatit.* This latter name should have been *Strigisan.* We have since had an opportunity of examining another of this gentleman's *new* minerals, his Arsenic Glance, which appears to consist of native arsenic and galena in distinct and unmixed layers. A specimen, said to be his *Batrachite*, has also been brought to us; but it seems to be only an amorphous mass of a grayish rock, without any definite mineralogical character; and possibly many more equally good species might be afforded from the same locality.

We cannot but regret to see an already overloaded nomenclature thus encumbered with new names representing nothing that can ever rank as distinct minerals. The *alumocalcite* of this author is another instance of considering accidental varieties of known minerals as new species; and the specimen *in situ* of his *erlanite*, from which the fragment that has been analysed was taken, is said to be some miles in extent, and at least 100 fathoms in thickness. The Andes might afford a copious harvest of such *simple* minerals as this.—Edit.

HALLEY'S COMET.

This interesting body has been twice seen since its perihelion passage by the Rev. R. Dawes, of Ormskirk, on the 16th and 19th instant. On the 16th its place was noted, and found to be, January 15,785, 1836, Greenwich, mean time, R.A. comet 15 h. 59 min. 46· sec. south declination 27 deg. 22 min. 30 sec. These values will require some further correction, and the declination is considered to be an approximation merely. On the 19th the haze only gave time for a casual glimpse. As the comet is said by Mr. Dawes to be "*exceedingly faint*", it is hopeless to look for it with any telescope which does not considerably exceed his in power—that is, a very excellent five-feet telescope, made and mounted by Dollond. Mr. Dawes is acknowledged to be one of the most skilful and delicate measurers of minute celestial phænomena in this country, and has contributed several valued memoirs to the Royal Astronomical Society. By interpolating from Lieutenant Stratford's admirable ephemeris of Halley's comet, for the moment of Mr. Dawes's observation, the place is found to be R.A. 15 h. 59 min. 43·5 sec. south declination, 27 deg. 22 min. 42 sec., a very near coincidence, especially when it is remembered that the ephemeris only *pretends* to be approximate, is founded on *unreduced* observations, and is not as yet corrected for *planetary perturbations*, &c.

ON SESQUISULPHATE OF MANGANESE. BY DR. THOMSON.

When neutral solutions of sulphate of zinc and chloride of manganese are mixed together, no sensible change takes place. But if the mixture be concentrated, it gradually deposits yellowish white coloured crusts, which constitute a hitherto undescribed salt of manganese.

This salt dissolves readily in water, but I could not succeed in obtaining it in crystals. Its taste is sweetish and astringent, and slightly acid.

16·26 grs. of it, rendered as dry as possible by pressure between the folds of blotting-paper, and subsequent exposure to a gentle heat, were dissolved in water, and mixed with a great excess of carbonate of ammonia. The mixture was left for twenty-four hours, and during that time was frequently agitated. It was then thrown on a filter, to collect the white precipitate which had fallen. This precipitate became brown by exposure to the air, and by ignition acquired a reddish tint. In this state it was red oxide of manganese. It weighed 5·78 grs. = 5·38 grs. of protoxide of manganese.

The ammoniacal liquid which passed through the filter being evaporated to dryness, and the residue redissolved in water, left a small quantity of matter, which became red by ignition, and was also red oxide of manganese. It weighed 0·07 gr. = 0·065 gr. of protoxide. So that the whole protoxide of manganese contained in 16·26 grs. of the salt amounts to 5·445 grs.

The liquid thus freed from base was treated with nitrate of silver. The chloride of silver obtained, weighed, after ignition, 0·5 gr. = 0·12 gr. of chlorine.

The excess of silver being removed by the addition of a little common salt, the liquid was precipitated by muriate of barytes. The sulphate of barytes obtained being collected, washed, and ignited, weighed 24·06 grs. = 8·5 grs. sulphuric acid.

What is wanting to complete the 16·26 grs. must be water. For on other constituent could be obtained.

Thus it appears that the salt is composed of,

Sulphuric acid	8·5
Chlorine	0·12
Protoxide of manganese	5·445
Water	2·195—16·26

The chlorine was doubtless combined with manganese, probably in the state of tris-chloride. We must, therefore, subtract 0·36 from the protoxide of manganese. The remainder, 5·085, is the quantity of manganese in combination with the sulphuric acid. Now, 5·1 is 8·5 as 4·5 to 7·5. So that the salt is composed very nearly of

$1\frac{1}{2}$ atom sulphuric acid	7·5
1 atom protoxide of manganese	4·5
2 atoms water	2·25—14·25

The water was rather less than two atoms. Probably a little had been driven off in the attempt to dry the salt by heat.

To what the yellow colour is owing which this salt possesses I do not know. The solution of it in water is colourless, so that none of the manganese can be in a state of red oxide. I could detect no oxide of zinc in the oxide of manganese, and none could be extracted by digesting the newly precipitated oxide in caustic potash.

Records of Science, vol. ii. p. 369.

ON CRYSTALLIZED OXIDE OF CHROMIUM. BY M. F. WÖHLER.

When perchoride of chromium is passed in the state of vapour, through a glass tube heated to redness, it is decomposed into oxide of chromium, which remains in the tube in a crystalline form, and a mixture of chlorine and oxygen gases. The crystals of oxide of chromium thus obtained are black, of a metallic lustre, hard, well defined, and brilliant, possessing exactly the same form as the native peroxide of iron (*fer oligiste*), which confirms the isomorphism before recognised in these two oxides. The exterior characters of these oxides, when crystallized, are precisely similar; the specific gravity of this oxide of chromium is 5·21, nearly approaching to that of oxide of iron; but whilst the latter gives a red powder, that of the former is green, like the common oxide of chromium. These crytals are as hard as corundum, which, next to the diamond, ranks as the hardest known body.—*Journal de Pharmacie, Juin* 1835.

NEW SCIENTIFIC BOOKS.

A Manual of British Vertebrate Animals. By the Rev. Leonard Jenyns, M.A., F.L.S., &c.

An Elementary Treatise on the Computation of Eclipses and Occultations. By J. W. Lubbock, Esq., F.R.S., &c.

Notices of Communications to the British Association for the Advancement of Science; at Dublin, in August 1835.

Geology of Yorkshire, Vol. II. By Prof. Phillips.

Philosophical Transactions, Part II. 1835.

Remarks occasioned by Lord Brougham's Paley's Natural Theology illustrated. By Thomas Martin.

METEOROLOGICAL OBSERVATIONS FOR DECEMBER 1835.

REMARKS.

Chiswick.— December 1. Clear and fine: cloudy and windy at night. 2. Very fine. 3. Cloudy. 4, 5. Fine. 6. Frosty and foggy. 7. Foggy. 8. Hazy: rain. 9. Cloudy and cold. 10. Slight snow. 11—13. Sharp frost. 14, 15. Hazy. 16, 17. Dense fog. 18. Clear: hail shower at noon. 19. Cloudy and cold. 20. Slight snow. 21. Overcast: clear and cold. 22. Sharp frost: foggy. 23—26. Frosty and foggy. 27. Cloudy. 28. Fine. 29. Overcast. 30. Fine. 31. Frosty with dense fog. The quantity of rain in this month amounted only to a quarter of an inch.

P.S. Observing the discrepancy apparent in your last Journal between the results of observations at the Apartments of the Royal Society and at this Garden, I intended to have sent some account of the instruments used here, and their situation. Such will be necessary; but as some investigations are being made on the subject, I thought it better to defer it till next Number. I shall therefore only remark that the thermometers here, indicating the *max.* and *min.* of temperature, are in an open space, *unaffected by radiation from buildings*—a circumstance which must have a very great influence on those used for the same purposes at Somerset House.—R. Thompson.

Boston.—December 1, 2. Fine. 3. Cloudy. 4, 5. Fine. 6. Cloudy. 7, 8. Foggy. 9. Cloudy: rain early A.M. 10 11. Fine. 12. Cloudy. 13. Fine. 14, 15. Cloudy. 16. Fine. 17. Foggy. 18. Fine. 19. Snow: stormy night with snow. 20, 21. Cloudy. 22—26. Fine. 27—30. Cloudy. 31. Fine.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1835. December.	Barometer. London: Roy. Soc. 9 A.M.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston 8¾ A.M.	Thermometer. London: Roy. Soc. Fahr. 9 A.M.	Thermometer. London: Roy. Soc. Self-registering. Min.	Thermometer. London: Roy. Soc. Self-registering. Max.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8¾ A.M.	Wind. London: Roy. Soc. 9 A.M.	Wind. Chis. 1 P.M.	Wind. Bost.	Rain. London: Roy. Soc. 9 A.M.	Rain. Chisw.	Rain. Boston.	Dew-point. Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
T. 1.	29·227	29·406	29·336	28·88	49·2	47·0	51·6	55	44	47	SSE.	S.	SW.	...	...	·06	47
W. 2.	29·232	29·658	29·485	28·95	46·6	44·8	48·8	52	33	46·5	S.	SW.	NW.	...	·02	...	43
Th. 3.	29·540	29·716	29·681	29·24	49·4	43·0	50·6	51	41	44	SSE.	S.	calm	...	·02	...	46
F. 4.	29·598	30·041	29·766	29·22	43·8	42·5	46·8	48	36	42	SW.	SW.	calm	...	·02	...	45
○ S. 5.	30·041	30·258	30·148	29·69	41·8	37·0	45·3	49	39	40	SW.	SW.	calm	·063	...	...	41
⊙ 6.	30·023	30·200	30·164	29·66	42·7	39·8	44·8	48	32	41	SSW.	S.	calm	...	·01	...	40
M. 7.	29·986	30·185	30·182	29·80	40·4	36·0	41·8	41	36	37	ESE.	W.	calm	...	...	...	36
T. 8.	29·936	30·134	29·797	29·66	39·2	37·7	42·3	42	37	39	SSW.	SW.	calm	...	·11	...	33
W. 9.	29·617	30·176	29·805	29·43	42·7	38·4	43·4	44	26	43	SE. var.	N.	E.	·202	...	·21	40
Th. 10.	30·172	30·427	30·360	29·98	35·8	31·7	35·4	34	20	35·5	E.	S.	calm	...	...	...	32
F. 11.	30·085	30·335	30·284	29·90	29·3	25·6	34·5	35	19	26	E.	S.	NW.	...	...	...	22
S. 12.	30·130	30·351	30·331	29·93	31·0	25·2	36·6	38	21	35	SSW.	SW.	calm	...	...	...	25
☾ ⊙ 13.	30·158	30·377	30·348	29·94	35·2	29·6	38·6	42	27	38·5	SW.	SW.	calm	...	...	...	29
M. 14.	30·188	30·392	30·271	29·92	38·6	33·5	40·8	40	34	42	SW.	S.	calm	...	...	...	32
T. 15.	30·186	30·389	30·262	29·89	40·2	38·3	41·5	41	36	41	SW.	S.	calm	...	...	...	35
W. 16.	30·200	30·477	30·399	29·97	41·6	39·2	42·4	41	33	38	SW.	NW.	calm	...	·01	...	38
Th. 17.	30·196	30·409	30·324	29·95	37·8	36·6	41·6	44	34	36	SW.	SW.	calm	...	...	...	35
F. 18.	29·928	30·134	30·022	29·66	42·3	37·2	45·3	47	34	42	SW.	N.	W.	·038	·06	...	35
● S. 19.	29·821	30·025	29·988	29·66	35·7	34·3	35·8	37	31	34	NE. var.	N.	E.	·044	...	...	32
⊙ 20.	29·794	30·101	30·011	29·77	33·2	31·0	33·3	33	31	34	NE. var.	NE.	E.	...	...	...	29
M. 21.	29·978	30·394	30·202	29·87	32·5	30·2	32·2	33	26	36	NE.	NE.	calm	...	...	...	27
T. 22.	30·198	30·622	30·414	30·06	28·8	26·3	34·6	36	17	28·5	NW.	NE.	E.	...	...	...	25
W. 23.	30·400	30·624	30·597	30·21	25·7	24·0	30·2	31	25	27	N.	SW.	calm	...	...	...	19
Th. 24.	30·309	30·543	30·484	30·13	30·9	24·4	30·6	30	19	25	WSW.	NW.	calm	...	...	...	23
F. 25.	30·220	30·457	30·424	30·05	22·2	21·0	24·6	27	17	24	WSW.	S.	calm	...	...	...	18
☽ S. 26.	30·208	30·443	30·390	30·05	24·2	21·5	28·2	31	16	22	SW.	S.	calm	...	...	...	22
⊙ 27.	30·101	30·343	30·300	29·95	30·2	22·2	38·8	42	32	34	SSW.	SW.	calm	...	...	...	22
M. 28.	29·956	30·205	30·131	29·65	40·9	38·7	45·4	45	35	38	S. var.	SW.	W.	...	...	...	32
T. 29.	30·083	30·324	30·298	29·80	38·2	36·3	41·4	42	36	40	SW.	W.	calm	...	...	...	32
W. 30.	30·031	30·343	30·232	29·74	41·6	37·7	45·3	46	26	42·5	S.	NW.	calm	...	...	...	36
Th. 31.	30·162	30·373	30·348	29·95	33·2	31·3	33·6	31	26	29	SW.	S.	calm	·033	...	...	32
	29·990	30·624	29·336	29·76	36·9	33·6	39·6	55	16	36·3				Sum ·380	·25	·27	30·2

THE

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[THIRD SERIES.]

MARCH 1836.

XXXII. *On the general Magnetic Relations and Characters of the Metals.* *By* MICHAEL FARADAY, *D.C.L. F.R.S., &c.**

GENERAL views have long since led me to an opinion, which is probably also entertained by others, though I do not remember to have met with it, that *all* the metals are magnetic in the same manner as iron, though not at common temperatures or under ordinary circumstances†. I do not refer to a feeble magnetism‡, uncertain in its existence and source, but to a distinct and decided power, such as that possessed by iron and nickel; and my impression has been that there was a certain temperature for each body, (well known in the case of iron,) beneath which it was magnetic, but above which it lost all power; and that, further, there was some relation between this *point* of temperature, and the *intensity* of magnetic force which the body when reduced beneath it could acquire. In this view iron and nickel were not considered as exceptions from the metals generally with regard to magnetism, any more than mercury could be considered as an exception from this class of bodies as to liquefaction.

I took occasion during the very cold weather of December last, to make some experiments on this point. Pieces of various metals in their pure state were supported at the ends of

* Communicated by the Author.

† It may be proper to remark that the observations made in par. 255 of my "Experimental Researches," have reference only to the three classes of bodies there defined as existing at ordinary temperatures.

‡ *Encyclop. Metrop.*, 'Mixed Sciences,' vol. i. p. 761.

fine platinum wires, and then cooled to a very low degree by the evaporation of sulphurous acid. They were then brought close to one end of one of the needles of a delicate astatic arrangement, and the magnetic state judged of by the absence or presence of attractive forces. The whole apparatus was in an atmosphere of about 25° Fahr.: the pieces of metal when tried were always far below the freezing-point of mercury, and as judged, generally at from 60° to 70° Fahr. below zero.

The metals tried were,

Arsenic,	Lead,
Antimony,	Mercury,
Bismuth,	Palladium,
Cadmium,	Platinum,
Cobalt,	Silver,
Chromium,	Tin,
Copper,	Zinc,
Gold,	

and also Plumbago; but in none of these cases could I obtain the least indication of magnetism.

Cobalt and chromium are said to be both magnetic metals. I cannot find that either of them is so, in its pure state, at any temperatures. When the property was present in specimens supposed to be pure, I have traced it to iron or nickel.

The step which we can make downwards in temperature is, however, so small as compared to the changes we can produce in the opposite direction, that negative results of the kind here stated could scarcely be allowed to have much weight in deciding the question under examination, although, unfortunately, they cut off all but two metals from actual comparison. Still, as the only experimental course left open, I proceeded to compare, roughly, iron and nickel with respect to the points of temperature at which they ceased to be magnetic. In this respect iron is well known*. It loses all magnetic properties at an orange heat, and is then, to a magnet, just like a piece of copper, silver, or any other unmagnetic metal. It does not intercept the magnetic influence between a magnet and a piece of cold iron or a needle. If moved across magnetic curves, a magneto-electric current is produced within it exactly as in other cases. The point at which iron loses and gains its magnetic force appears to be very definite, for the power comes on suddenly and fully in small masses by a small diminution of temperature; and as suddenly disappears upon a small elevation, at that degree.

With nickel I found, as I expected, that the point at which it lost its magnetic relations was very much lower than with

* See Barlow on the Magnetic Condition of Hot Iron. Phil. Trans. 1822, p. 117, &c.

iron, but equally defined and distinct. If heated and then cooled, it remained unmagnetic long after it had fallen below a heat visible in the dark: and, in fact, almond oil can bear and communicate that temperature which can render nickel indifferent to a magnet. By a few experiments with the thermometer it appeared that the demagnetizing temperature for nickel is about 630° or 640°. A slight change about this point would either give or take away the full magnetic power of the metal.

Thus the experiments, as far as they go, justify the opinion advanced at the commencement of this paper, that all metals have similar magnetic relations, but that there is a certain temperature for each beneath which it is magnetic in the manner of iron or nickel, and above which it cannot exhibit this property. This magnetic capability, like volatility or fusibility, must depend upon some peculiar relation or condition of the particles of the body; and the striking difference between the necessary temperatures for iron and nickel appears to me to render it far more philosophical to allow that magnetic capability is a general property of all metals, a certain temperature being the essential condition for the development of this state, than to suppose that iron and nickel possess a physical property which is denied to all the other substances of the class.

An opinion has been entertained with regard to iron, that the heat which takes away its magnetic property acts somehow within it and amongst its electrical currents (upon which the magnetism is considered as depending) as flame and heat of a similar intensity act upon conductors charged with ordinary electricity. The difference of temperature necessary for iron and nickel is against this opinion, and the view I take of the whole is still more strongly opposed to it.

The close relation of electric and magnetic phænomena led me to think it probable, that the sudden change of condition with respect to the magnetism of iron and nickel at certain temperatures, might also affect, in some degree, their conducting power for electricity in its ordinary form; but I could not, in such trials as I made, discover this to be the case with iron. At the same time, although sufficiently exact to indicate a great change in conduction, they were not delicate enough to render evident any small change; which yet, if it occurred, might be of great importance in illustrating the peculiarity of magnetic action under these circumstances, and might even elucidate its general nature.

Before concluding this short paper, I may describe a few results of magnetic action, which, though not directly concerned in the argument above, are connected generally with the subject*. Wishing to know what relation that temperature which could take from a magnet its power over soft iron, had to that which could take from soft iron or steel its power relative to a magnet, I gradually raised the temperature of a magnet, and found that when scarcely at the boiling-point of almond oil it lost its polarity rather suddenly, and then acted with a magnet as cold soft iron: it required to be raised to a full orange heat before it lost its power as soft iron. Hence the force of the steel to *retain* that condition of its particles which renders it a permanent magnet, gives way to heat at a far lower temperature than that which is necessary to prevent its particles assuming the *same state* by the inductive action of a neighbouring magnet. Hence at one temperature its particles can of themselves retain a permanent state; whilst at a higher temperature that state, though it can be induced from without, will continue only as long as the inductive action lasts; and at a still higher temperature all capability of assuming this condition is lost.

The temperature at which polarity was destroyed appeared to vary with the hardness and condition of the steel.

Fragments of loadstone of very high power were then experimented with. These preserved their polarity at higher temperatures than the steel magnet; the heat of boiling oil was not sufficient to injure it. Just below visible ignition in the dark they lost their polarity, but from that to a temperature a little higher, being very dull ignition, they acted as soft ron would do, and then suddenly lost that power also. Thus the loadstone retained its polarity longer than the steel magnet, but lost its capability of becoming a magnet by induction much sooner. When magnetic polarity was given to it by contact with a magnet, it retained this power up to the same degree of temperature as that at which it held its first and natural magnetism.

A very ingenious magnetizing process, in which electro-magnets and a high temperature are used, has been proposed lately by M. Aimé†. I am not acquainted with the actual results of this process, but it would appear probable that the temperature which decides the existence of the polarity, and above which all seems at liberty in the bar, is that required. Hence probably it will be found that a white heat is not more advantageous in the process than a temperature just above or about that of boiling oil; whilst the latter would be much

* See on this subject, Christie on Effects of Temperature, &c. Phil. Trans. 1825, p. 62. &c.

† *Annales de Chimie et de Physique*, tome lvii. p. 442.

more convenient in practice. The only theoretical reason for commencing at high temperatures would be to include both the hardening and the polarizing degrees in the same process; but it appears doubtful whether these are so connected as to give any advantage in practice, however advantageous it may be to commence the process above the depolarizing temperature.

Royal Institution, Jan. 27, 1836.

XXXIII. *On the Effects of the Earthquake Waves on the Coasts of the Pacific. By* WOODBINE PARISH, *Esq., F.R.S., Secretary to the Geological Society.**

AT one of our meetings last season, in a discussion which arose out of the discovery of recent shells and other marine deposits on several parts of the coasts of Chile and Peru above the present level of the sea, I ventured to throw out the opinion that a great part of those appearances might, perhaps, be referred to violent upheavings of the *sea* under the influence of earthquakes. I had then only in my recollection the earthquake waves which burst over Scylla and Lisbon in Europe, and Callao in America; but upon looking into the early writers upon the countries bordering on the Pacific, I have found so many recorded instances of these disruptions of the great ocean, and in some cases attended with such remarkable effects, that I have thought it might interest the Society to have them collectively before it, particularly upon any new discussion of such phænomena as first gave rise to my observations upon the subject.

Under that impression I have put them together in this paper.

Historical Notices of the Effects of the Earthquake Waves on the Coasts of Chile and Peru.

Acosta in his *Historia Natural y Moral de las Indias*, written in 1590, gives us a chapter upon the earthquakes of his time, from which the following is a translation: he says, "On the coast of Chile, I do not remember precisely the year, there took place a very terrible earthquake, which overthrew whole mountains, stopping up with them the courses of rivers and turning them into lakes, destroying towns, and a vast number of people." *It caused the sea to rise out of its bed some leagues, leaving ships dry far inland.*

* Read before the Geological Society Dec. 2, 1835; and now communicated by the Author.

A few years afterwards (in 1582) Arequipa was destroyed by an earthquake.

In 1586, on the 9th of July, happened an earthquake at Lima which, according to the Viceroy's account of it, was felt for 170 leagues along the coast. Amongst other calamitous consequences of it, Acosta mentions that "*the sea then was upheaved as on the former occasion on the coast of Chile, rising after the shock of the earthquake mightily out of its bed, and bursting over the shore nearly two leagues inland, overwhelming all that shore, and leaving the shrubs and trees as it were swimming in the waters.*"

Frezier in the account of his voyage to the South Seas (in 1712, 1713, 1714,) speaks of an earthquake wave which destroyed the town of Arica in 1605. He says, "On the 26th of November 1605, the sea, being agitated by an earthquake, suddenly flooded and bore down the greatest part of it; the ruins of the streets are to be seen to this day," &c.

The next account we have of a similar event is from Ulloa's Voyage to South America. In enumerating the earthquakes experienced at Lima, he writes: "One of the most dreadful of which we have any account was that of the 20th of October 1687. It began at four in the morning, with the destruction of several public edifices and houses, whereby great numbers of persons perished; but this was little more than a presage of what was to follow, &c. &c.

"*During the second concussion, the sea retired considerably from its bounds, and returning in mountainous waves, totally overwhelmed Callao and the neighbouring parts, together with the miserable inhabitants.*"

Our own countryman Lionel Wafer, who was in those seas at the time, says, "When we were in the latitude of 12 degrees 30′ south, and about 150 leagues from the coast, our ship and bark felt a terrible shock, which put our men into much consternation, so that they could hardly tell where they were, or what to think, but every one began to prepare for death; and, indeed, the shock was so sudden and violent that we took it for granted the ship had struck upon a rock: but when the amazement was a little over, we cast the lead and sounded, but found no ground, so that after consultation we concluded it must certainly be some earthquake. The suddenness of the shock made the guns of the ship leap in their carriages, and several of the men were shaken out of their hammocks: Capt. Davis, who lay with his head over a gun, was thrown out of his cabin. The sea, which ordinarily looks green, seemed then of a whitish colour, and the water which we took up in our buckets for the ship's use we found to be mixed with sand: this at first made us think there was some spit of land; but when we had sounded, it confirmed our opinion of the earthquake...... Afterwards we heard the news that at that very time there was an earthquake at Callao, *and the sea ebbed so far from the shore that on a sudden there was no water to be seen; and that after it had been away some time it returned in rolling mountains of water, which carried the ships in the road of Callao a league up into the country, overflowing the city of Callao, together with the port, and drowned man and beast for* 50 *leagues along the shore,*" *&c. &c.*

Wafer describes the effects of a similar catastrophe at Santa, about three degrees to the north of Callao, which he witnessed. He says, "On landing I went up to the town, which was three miles or thereabouts from the sea. In our way to the town we crossed a small hill, and in a valley between the hill and the town we saw three small ships, of about 60 or 100 tons each, lodged there and very ruinous: it caused in us great admiration, and we were puzzled to think how those ships could come there; but proceeding towards the town we saw an Indian, whom we called, and he at the first motion came to us. We asked him several questions, and among the rest how those ships came there. He told us that about nine years before (1678) those three ships were riding at anchor in the bay, which is an open place about five or six leagues from point to point; and *that an earthquake came and carried the water out of sight, which staid away twenty-four hours, and then came in again tumbling and rolling with such violence that it carried these ships over the town which then stood on the hill which we came over, and lodged them there*, and that it destroyed the country for a considerable way along the coast. This account when we came to the town was confirmed to us by the priest and many other inhabitants of the town.

Ulloa speaks of great earthquakes at Lima in the years 1697, 1699, 1716, 1725, 1732, 1734, and 1745, but does not give us the details of them. He gives a more particular account of that one, the most dreadful of all up to the time he wrote, which took place in 1746. But of this I shall rather give an extract from the narrative published at the time by authority of the Viceroy.

After stating the direful effects of this earthquake at Lima, that account proceeds thus:

"Yet at least the remains of what Lima was are still existing; not so fares it with the garrison and port of Callao, where the very objects of the misfortune are quite vanished out of sight: this doubles the concern and anguish of the mind, which shudders at the contemplation of the dreadful calamity. Not the least sign of its former figure does now appear: on the contrary, vast heaps of sand and gravel occupying the spot of its former position, it is at present become a spacious strand, extending along that coast. Some few towers, indeed, and the strength of its walls for a time endured the whole force of the earthquake, and resisted the violence of its shocks; but scarcely had its poor inhabitants begun to recover from the horror of the first fright which the dreadful ruin and devastation had occasioned there, (and how great that was is not to be known,) when suddenly the sea began to swell, either through the impulsive force which the earth by its violent agitation impressed upon it,—and thereby keeping up for a time, in one vast body, mountains of water,—or by what other means natural philosophers may please to assign, which on these occasions are the causes of its elevation—and swelling rose to such a prodigious degree, and with so mighty a compression, that on falling from the height it had attained, (although Callao stood above it on an eminence which, however imperceivable, yet continues still increasing all the way to Lima,) it rushed furiously forward, and overflowed with so vast a deluge of water its ancient bounds, that foundering the greater part of the ships which were at anchor in the port, and elevating the rest of them above the height of the walls and towers, drove them on and left them on dry ground, far beyond the town; at the same time it tore up from the foundation everything that was in it of houses and buildings, excepting only the two great gates, and here and there some small fragment of the walls themselves, which as registers of the calamity are still to be seen among the ruins and the waters, a dreadful monument of what they were.

"In this raging flood were drowned all the inhabitants of the place, who at that time might amount to near 5000 persons, of all ages, sexes, and conditions, according to the most exact calculation that can be made," &c. &c.

"There were twenty-three ships, great and small, at anchor in the port at the time of the earthquake; and of these, as has been mentioned before, some were stranded, being four in number, viz. the 'San Firmin', man of war, which was found in the low ground of the upper Chacara, the part opposite to the place where she rode at anchor, and near her the 'St. Antonio,' belonging to Don Thomas Costa, a new ship just arrived from Guayaquil: the vessel of Don Adirar Corsi rested on the spot where before stood the hospital of St. John, and the ship 'Succour' of Don Juan Baquixano, which had just arrived that very evening with a cargo from Chile, was thrown up towards the mountains, both one and the other of them at great distances from the sea; and all the rest were foundered."

Ulloa adds, that "this terrible inundation extended to other ports of the coast, as Cavallos and Guanape, and the towns of Chancay, Guara, and the valleys Della Barranca, Sape, and Patevilca underwent the same fate as the city of Lima."

Five years afterwards, in 1751, on the 26th of May, the city of Conception, called by the Indians Penco, on the coast of Chile, was totally destroyed by an inundation of the sea; in consequence of which the inhabitants removed to some distance from it, and rebuilt their city on the spot where it now stands.

In Molina's account of Chile, speaking of this event, he mentions that the city was *again* inundated, alluding to a similar catastrophe which had befallen it some years before, in 1730: to which Ulloa has more particularly alluded. He says, on that occasion (in 1730) "*the sea at first retired a considerable way; but soon it rose so greatly that, passing its ordinary bounds, it inundated the city (Penco) and the country about it, obliging all the inhabitants to seek safety on the neighbouring hills.*"

The earthquake wave which overwhelmed Conception in 1751, was equally felt at the island of Juan Fernandez. I have a manuscript report of the Viceroy of Peru (lately published in the Journal of the Royal Geographical Society), wherein it is stated that "the first colony of the Spaniards had not long been settled there when it was almost totally destroyed by the same dreadful earthquake which, in the year 1751, overthrew the city of Conception in Chile: with the earthquake the sea rose and overwhelmed the houses, most of which had unfortunately been built along the shore. Thirty-five persons perished from this calamitous event, and amongst them the governor with his wife and children."

It is unnecessary for me to repeat, that in the great earthquake of 1822, on which there has been so much discussion, the sea is said to have been agitated in an extraordinary degree. Mrs. Graham, in her Journal, tells us that "on the night of the 19th of November, during the first great shock, the sea in Valparaiso Bay rose suddenly, and as suddenly retired, in an extraordinary manner, and in about a quarter of an hour seemed to have recovered its equilibrium."

She further mentions having heard from the officers on board Lord Cochrane's ship, that when His Lordship "and others threw themselves immediately into a boat to go to the assistance, if help were still possible, of the sufferers, *the rushing wave landed them higher than any boat had been before, and they then saw it retire frightfully, and leave many of the launches and other small vessels dry.*"

I had written so far when the accounts reached this country of the dreadful earthquake which, on the 20th of February last, again utterly destroyed the city of Conception, with its sea-port at Talcahuano, and all the towns of Chile between the parallels of 35° and 38° south latitude.

The details which have as yet reached us afford another remarkable evidence of the direful and irresistible effects of the earthquake wave. In the Bay of Talcahuano the sea is said to have risen three times, overwhelming the town, and sweeping away its ruins with such a rush and whirl of waters as, to quote one account, it is more easy to imagine than describe, and carrying some of the ships far up upon the shore.

The circumstance of a vessel, "the Glemalier," having experienced a violent shock when at a distance of ninety-five miles from the coast, which stopped her course and induced the master to believe she had struck the ground, coincides remarkably with old Wafer's account of what happened to himself off the coast of Peru during the earthquake of 1687, and is of value, in as much as it corroborates his testimony to a fact which before seemed hardly credible*.

Such is the list with which history furnishes us of these terrific inundations. Fearful as it is, if we bear in mind how recently we have become aware even of the existence of those coasts, and how extremely imperfect is our knowledge of them at all, we shall have no difficulty in believing that it comprises but a very small portion, indeed, of a series of events which, in a very short period of time, geologically speaking, must have left indelible marks of their tremendous agency, attesting but too well the calamitous visitations to which the inhabitants of those shores are subject.

XXXIV. *Note on the Transmission of Radiant Heat. By the Rev.* Baden Powell, *M.A., F.R.S., Savilian Professor of Geometry, Oxford.*†

FEW scientific discussions are more unsatisfactory than those in which writers endeavour to point out, or explain, misconceptions of each other's meaning. In reference to one or two remarks somewhat of this nature made in the course of papers in recent Numbers of this Journal, on the subject of radiant heat, I will merely state, in the fewest possible words,

* We believe that several if not many other instances of the same fact have been recorded, chiefly in the older accounts and collections respecting earthquakes.—Edit.

† Communicated by the Author.

distinctly what my meaning was, and there leave the question.

The result which I obtained in 1825, and which M. Melloni has so fully verified, was this: The effect from a luminous hot body, without any screen, upon a *black* and a *white surface* respectively, was observed to be in a certain ratio; the same effects when a transparent screen was interposed were in a *different* and *greater ratio*. M. Melloni's theory is, that this *new* and *different relation to surfaces* is communicated to the rays by, and in, the act of passing through the screen; so at least I understand it. Now *this* is what I objected to as a very singular theory, discordant (as I conceive) with all analogy, and *needless*, in as much as the effect is explained by the much simpler supposition, that there are two distinct sorts of heat emanating at the same time from the luminous body, distinct in their relations to *surfaces* as well as to *screens*.

It was to this single point *alone* that my remarks applied. With the other valuable results of M. Melloni I am not now concerned. I will merely add, in the present unformed state of this entire branch of our knowledge it seems hardly safe to adopt any theory, except as a mere conjectural guide. But I have the greatest hopes that before long we shall be in a condition to advance to some satisfactory general principles, when I find such an instrument as M. Melloni's in active employment in the hands of such able and zealous experimenters as are now engaged with it in Edinburgh and Dublin.

XXXV. *On* Sir G. S. Mackenzie's *Remarks on certain Points in Meteorology, &c., inserted in Lond. and Edinb. Phil. Mag. for November* 1835. *By* Joseph Atkinson, *Esq., Secretary to the Carlisle Literary and Philosophical Society.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I HAVE been induced by Sir G. S. Mackenzie's remarks on the equinoctial gales, contained in your Magazine for November last, to look for a corroboration of his supposition that the equinoctial gales have of late come from the eastward, but I cannot find that such has been the case here. The situation of Coul is most probably the cause of the easterly winds being so prevalent there. On the 22nd of September, which is the day mentioned as the one on which he wrote his letter, while an easterly wind was blowing, I find a southerly wind noted in my journal. This of itself is enough to show that locality has a great deal to do with the kind of winds prevalent at each place. On the 30th of September, in the present year, the easterly winds began to below here, and continued without intermission till the 8th of October; and from the 26th of August to the 2nd of September, easterly winds also prevailed: but while these winds were altogether light, scarcely approaching to the character of a breeze, the westerly winds, which prevailed from the 2nd to the 29th of September, were very strong, and on several days, especially on the 28th of September, might be called gales.

Sir G. Mackenzie's remarks as to the easterly winds having lately come much charged with moisture will, I find on reference to my journal, apply to Carlisle as well as Coul; for on looking back at those days on which easterly winds have prevailed during the past year, I find a great proportion noted as drizzly or showery.

I have also remarked that easterly winds have become much more prevalent here of late years than was wont to be the case, while the south-westerly have of late been decreasing in number. Whether this excess of easterly winds is likely to continue, or will only be for a year or two, it is of course almost impossible to tell, but I am inclined to think it only temporary. It is rather remarkable that in the month of February last (1835) the wind never blew from an easterly point for even a quarter of a day. The month of May seems to be the one in which easterly winds have most prevailed here for the last two years.

The observations which are made at the Apartments of the Royal Society in London must surely be very loosely taken, for I often find the height of the thermometer as noted at 3 P.M., to *exceed* the *maximum*! This most probably arises from the use of two thermometers, having either different scales, or else hanging in different positions. This circumstance, added to the evidently too small quantity of rain which used to be given as the results of the observation of the rain-gauge (but which have lately been seemingly better attended to), shake the faith of the meteorologist in any of the observations published under the sanction of the Royal Society. Surely there ought to be one maker appointed to construct every instrument used by Societies, whether Royal or not, and by those who, living at a distance from towns, would still wish to compare their observations with those of others. The great attention which is now paid to meteorology as a branch of science demands that something should be done to secure the agreement of all instruments used by observers of the weather. Several pages of some scientific Journal should also be devoted to the publication of the monthly results (according to a fixed tabular form) of observations taken in various parts, not only of this kingdom, but of the whole world, if possible. I am, Gentlemen, yours obediently,

JOSEPH ATKINSON,

Carlisle, Dec. 1, 1835. Sec. of the Carlisle Lit. and Phil. Soc.

XXXVI. *On the repulsive Power of Heat. By* H. F. TALBOT, *Esq., F.R.S.**

Experiment 1.—*On the Vaporization of Sulphur.*

WHEN a minute portion of sulphur is warmed between two plates of glass, it sublimes, and forms gray nebulous patches, which are very curious microscopic objects. Each cluster consists of thousands of transparent globules, exactly imitating in miniature the nebulæ which we see figured in treatises on astronomy†. By observing those particles which are larger than the others, we find their figure not to be spherical, but plano-convex, with the flat side to the glass. Being very transparent, each of them acts the part of a little lens, and forms in its focus the image of a distant light, which can be perceived even in the smaller globules until it vanishes from minuteness. If they are examined again after a certain number of hours, the smaller globules are generally found to retain their transparency, while the larger ones are become opake, in consequence of some internal change in the arrangement of their molecules. I find that Mitscherlich and others have noticed this property in sulphur of undergoing a spontaneous change‡. There is a circumstance attending this experiment which deserves particular attention. Although the sulphur has been sublimed by heating it over a lamp between two plates of glass almost in contact with one another, yet the globules are found adhering to the upper glass only; and as their number amounts generally to many thousands, it is evident that the preference which they thus exhibit to the upper glass must have some strong determining cause.

The reason of it is, no doubt, that the upper glass is a little cooler than the lower one; and by this means we see that the vapour of sulphur is very powerfully repelled§ by heated

* Communicated by the Author.

† And owing, perhaps, their mutual disposition to the same general laws of attraction as the Nebulæ?—EDIT.

‡ See Phil. Mag. and Annals, N.S., vol. iii. p. 144, 152, for correlative facts.—EDIT.

§ This is a very beautiful instance in corroboration and extension of Prof. Powell's experiment described in the Phil. Trans. 1834, part ii. p. 485.—EDIT.

glass. The plano-convex form of the particles is owing to the force with which they endeavour to recede from the lower glass, and their consequent pressure against the surface of the upper one. I think this experiment is a satisfactory argument in favour of the repulsive power of heat, and I believe it has not been hitherto described.

Experiment 2.—On the Vaporization of Arsenic.

When a particle of arsenic is sublimed between two plates of glass, it forms nebulous patches, considerably resembling those formed by sulphur in the preceding experiment. But the microscope detects a great difference. Instead of a globular or semiglobular form, the particles of arsenic are crystallized. The minuteness of some of the crystals almost exceeds calculation. I would suggest the employment of this method to detect the presence of arsenic in minute quantities of matter. The difficulty of demonstrating its presence with sufficient certainty is shown by the number of chemical essays that have been written on the subject, while a particle of the size of a pin's head is amply sufficient to display this microscropic crystallization; and the form of the crystals being distinct and definite, the observer can soon make himself acquainted with their figure, so as to run little risk of mistaking any other substance for them.

Note on Radiant Heat.

M. Melloni says (in the Number of this Journal for December last, vol. vii. p. 475,) that

"For a long time the immediate transmission of terrestrial radiant heat by transparent substances, both solid and liquid, has been denied; and the opinion has become prevalent that we see in experiments of this kind only an effect of the heat absorbed by the body submitted to the calorific radiation."

This "prevalent opinion" he has shown to be erroneous, but by experiments which are too delicate to be repeated with facility.

As a popular illustration of the fact, therefore, seems to be wanted, I subjoin the following rude but convincing experiment.

Let a poker be heated bright red hot, and having thrown open a window, approach the poker quickly to the *outside* of a pane, and the hand to the *inside*. A strong heat is felt at the instant, which ceases as soon as the poker is withdrawn, and may be again renewed, and made to cease as quickly as before. Now, everybody knows that if a piece of glass is so much warmed as to convey this impression of heat to the hand, it will retain some part of that heat for a minute or more; but in this experiment the heat vanishes in a moment. It is not, therefore, heated glass which we feel, but heat which has come through the glass, in a free or radiant state.

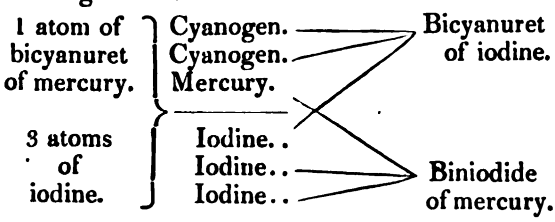
XXXVII. *Extracts from a Prize Essay on Iodine. By* James Inglis, *M.D.*

[Continued from p. 20, and concluded.]

I MENTIONED before that Sérullas had proposed iodic acid as a test for the vegetable alkalies. I find that hydriodic acid may be used as such also. To the sulphate of quinine I added in solution a few drops of sulphuric acid, so that the sulphate might be soluble in water. I next added a solution of hydriodate of potassa; instantly there was a yellow precipitation, which became gradually of a greenish colour. I added more hydriodate, and the precipitation of yellow iodide of quinine still took place, which finally became of a reddish brown colour. I have not examined this compound, but call it iodide of quinine, the *hydriodates* being all *soluble*; and I do not see how it could be an iodate.

I find also that the hydriodate of potassa throws down a white precipitate with tincture of capsicum. I cannot decidedly say that this acid is as good a general test as the iodic. * * * *

I wished to obtain a compound of cyanogen with iodine, and for this purpose made a solution of bicyanuret of mercury in water, which I added to an alcoholic solution of iodine; immediately the red biniodide of mercury fell, and the action I thought to be this:



If too much cyanuret of mercury be added, then all the precipitation is red. But if there be only sufficient, then a lightish brown powder in crystals falls. I boiled and strained off the supernatant liquor from the biniodide of mercury, and laid it aside to crystallize; but this could not be effected. The liquid is exceedingly pungent, and exhales a vapour painful to the eyes. When added to water a copious yellow precipitation takes place. It contains about two thirds of alcohol; is, when pure, of a clear amber colour; when placed on the skin it causes a painful sensation, and excites an inflammatory blush. It, after being boiled, deposits acicular red crystals, interspersed with yellow ones of a similar shape. Even when much diluted with either water or alcohol, the odour and taste of this new cyanide are very pungent. * * *

I next wished to obtain an iodide of chromium, and this I endeavoured to do in the same manner as the chloride is got. I mixed a drachm of the hydriodate of potassa with half a drachm of the bichromate of potassa, and adding fuming sulphuric acid, I distilled; but could not succeed in procuring it. In the Lond. and Edinb. Phil. Mag. and Journal of Science, (No. 15. September, 1833, vol. iii. p. 235,) I observed that M. Peligot describes a compound of chromic acid with metallic chlorides. I thought that some analogous compound might be formed with the metallic iodides.

To the bichromate of potassa in solution, and when boiling, I added concentrated hydriodic acid in excess. A considerable portion of iodine is evolved, and a thick black compound formed, having iodine in excess. I put a portion of this into water, and having boiled it, laid it aside to cool. No crystallization took place, but the solution was of a decided green colour.

The black compound when dried and broken into pieces resembles kino; it is of a dark green colour, with considerable lustre, and is friable. The liquid that drained from it gave crystals of hydriodate of potash. The action I suppose to be as follows:

Chromic acid.	Chromate of potassa.
Chromic acid.	
Potassium. ...	
Oxygen.	Water.
Hydrogen.	
Iodine......	Chromate of the iodide of potassium.
Potassa.....	

I thought that hydrocyanic acid, being of easy decomposition, would, when combined with tincture of iodine, give hydriodic acid and a cyanuret of iodine. I allowed them to react on each other for some weeks; no deposition was observed, but on testing the fluid, hydriodic acid is readily detected. I consider this solution as a hydriodate of the iodide of cyanogen." * * * * * *

Fig. 1.

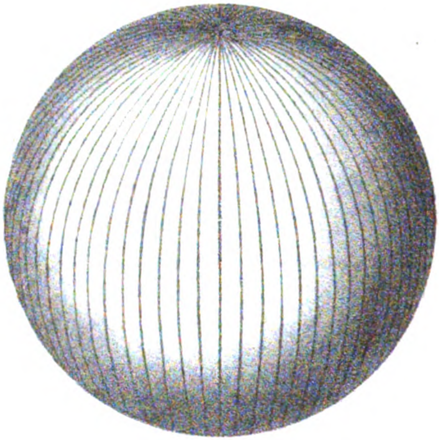
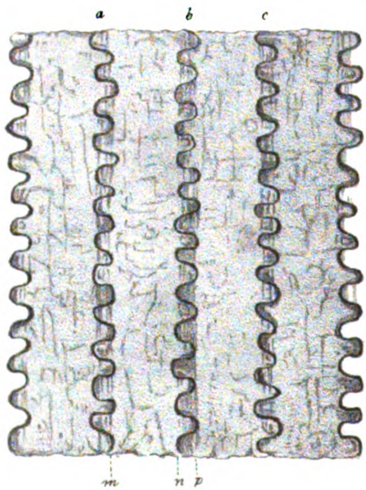


Fig. 2.



. 3

Fig. 4.

Fig. 5.

XXXVIII. *On the Anatomical and Optical Structure of the crystalline Lenses of Animals, particularly that of the Cod.* *By* Sir DAVID BREWSTER, *LL.D., F.R.S., V.P.R.S. Ed.*

[With a Plate.]

[From the Philosophical Transactions for 1833, p. 323–332; with additions.]

HAVING observed very singular phænomena in the crystalline lenses of fishes and quadrupeds when exposed to polarized light, I was led to examine their anatomical structure, with the view of ascertaining if it had any relation to these optical appearances. Leeuwenhoek and Sattig had previously made some progress in this research, but their methods of observation were ill fitted for so delicate an inquiry, and experience soon convinced me that the structure of the lens could not be thoroughly investigated either by the microscope or the scalpel.

Anatomists had long regarded the crystalline lens as composed of concentric laminæ, and these laminæ of minute fibres; but M. Soemmerring, in his work on the Human Eye, published in 1804, regards this structure as the effect merely of maceration in alcohol, and maintains that it does not exist in the recent or the living eye*. This decision, which its author has supported by many plausible but unphilosophical and inefficient arguments, appeared to set aside all the results which had been obtained by preceding inquirers, and rendered it necessary for me to adopt a new mode of investigation, which should not be liable to the same criticism.

The crystalline lens of the cod, like almost all globular lenses, has the form of a prolate spheroid, the axis of revolution being a little longer than the equatorial diameter. This axis is the axis of the eye, or of vision.

The body or substance of the lens is inclosed in an exceedingly thin and transparent membrane, called its capsule; and this membrane is so elastic that when it was stretched upon a plate of glass, the extended portion polarized a blueish white tint of the first order. If we puncture the capsule, a thickish fluid flows from the opening; but upon removing the capsule altogether, this fluid is found to constitute only the outer coat of the lens, the substance of the lens growing denser and harder as we approach to the centre of it.

The body of the lens is not connected with the capsule, as

* "Lens, post mortem ita tractata, etiamsi in segmenta sphærica, laminas et fibras dehiscat, tamen minime inde sequitur lentem recentem seu vivam ex ejusmodi fibris, lamellis et segmentis sphæricis conflari, aut per vitam lentis sanæ fabricam zeolitidi ullo modo similem esse."—pp. 67, 68.

Dr. Young supposed, by any nerves or filaments whatever: on the contrary, it floats as it were within the capsule; and in holding the lens in my hand, I observed its axis of revolution take a horizontal position whenever it was placed in an inclined direction. This observation I repeated many times with the same lens, though I have not been able to do it with others.

When the lens is taken out of its capsule, and the softer parts removed by rubbing it between the finger and the thumb, we obtain a hard nucleus, the structure of which we shall now proceed to examine. In order to get rid of Soemmerring's objections, I have often removed the recent lens from a newly caught fish, and taken out the nucleus without exposing it to any process of maceration or induration. I then detach a film or two from the nucleus, and find it to consist of regular transparent laminæ of uniform thickness, and capable of being separated like those of sulphate of lime or mica. The surface of these laminæ is perfectly smooth, and reflects light as copiously as any other polished substance of the same refractive power*.

When we look with a microscope at the surface of any lamina, before it has been detached, it has the appearance of a grooved surface, like mother-of-pearl, or one of Mr. Barton's iris surfaces; and in large lenses it is often easy to trace these apparent grooves or lines to the two poles of the axis of revolution, the lines being widest at the equator, and growing narrower and narrower as they approach the poles. In small lenses, however, it is extremely difficult, and often impossible, to follow these lines to their points of convergency by the use of the microscope; and hence both Leeuwenhoek and Sattig maintained, that in fishes the points of convergency formed a line at each pole, the line at the one pole being perpendicular to the line at the other. These lines are bisected by the polar point, and the parts of the line on each side of the pole are called septa. Hence the lines or fibres which compose the laminæ are related to two septa in fishes, according to Leeuwenhoek and Sattig.

In examining these phænomena, I observed the surface of the laminæ sparkling with the colours of mother-of-pearl †, and upon applying a microscope, adjusted to such a focus as to show distinctly the reflected image of the candle, I observed

* The objections urged by Soemmerring may be still more completely removed by examining the lenses when newly taken out of the eye and immersed in a glass trough of distilled water. In many cases the septa and the fibres will be distinctly seen before it is possible that the lens could have undergone any change whatever.

† These colours may be transferred to wax, &c., like those of mother-of-pearl.

on each side of this image one or more highly prismatic images, produced by interference, like those of grooved surfaces. As the direction of the fibres is necessarily perpendicular to the line joining the coloured images, I was enabled to trace them to their termination, or points of convergency, when the fibres themselves could not be rendered visible by the best microscope. But while this new method of observation enabled me to detect all or most of the varieties of fibrous organization which exist in the crystalline lenses of animals, it furnished at the same time a simple and accurate method of determining the diameter of the fibres at any point of the spheroid. The distance of any of the coloured images from the colourless image was a direct measure of the breadth of the fibres; and with the aid of a series of Mr. Barton's beautiful divisions upon steel, from 312·5 in an inch to 10,000, which he was so kind as to make for me, it was easy to obtain these measures without even the trouble of calculation.

By these means I succeeded in tracing the fibres, or thin slender laminæ, to two poles diametrically opposite to each other, and coinciding with the poles of the spheroidal lens, as shown in Plate II. fig. 1. In small lenses, and particularly in the lenses of birds, the accurate convergency of the fibres to one focus is less distinctly seen; but it is easy to distinguish this diffused polarity, as it may be called, from the convergency of the fibres to points arranged in a straight line, and constituting the two septa already mentioned.

The distribution of the fibres now described is the simplest that occurs in the lenses of animals. Every fibre has the same length and the same form; and its curvature is, like the meridian of a spheroid, without any contrary flexure.

The perfect flatness of the surfaces of the concentric laminæ, as indicated by their power of forming a distinct image by reflexion, shows that the fibres which compose them are flat and not cylindrical; and when we look through them with a powerful microscope, this conclusion is amply confirmed by the uniform distribution of the refracted light.

In order to measure the diameter, or rather the breadth of the fibres, I detach from the equator of the lens an extremely thin lamina, and having placed it above a small aperture in a plate of brass, I look through it at a candle, and measure the angular distance of the first coloured image from the white or central image, taking the centre of the red ray as the point from which the measurement is to be taken. When the first coloured images on each side of the central image are extremely distinct, it will be better to measure the angular distance of the red parts of their spectra, and to take half that distance as the angular distance of the first image from the central image. If we call this distance A, and put B for the breadth of each fibre, or the breadth of the transparent part, together with the breadth of the opake interval or line which separates the fibres, we shall then have $B = \frac{C}{A}$, C being a constant quantity for the red ray to be determined by experiment. According to Fraunhofer's experiments on Interference, C is equal to 0·0000256 of an English inch for the middle red ray; hence the formula becomes $B = \frac{0 \cdot 0000256}{A}$ *.

In order, however, to save the trouble of calculation, and in cases where a general estimate only is required, I use Mr. Barton's system of grooves already mentioned; and upon looking through the lamina at the white image, or rather the central image of the iris surface, I can at once compare the distance of the first prismatic images of the one with the same distance in the other. If I am using, for example, the divisions of 5000 in an inch, and if I find the distances of the coloured images the same with the laminæ and with the steel, I infer that the breadth B is the 5000dth of an inch. This conclusion, however, is not quite correct, for we may have been comparing the effect of a perpendicular incidence on the lamina with the effect of an oblique incidence on the steel. Now, Fraunhofer has shown that the coloured images separate as the incidence in a plane intersecting the grooves at right angles is increased, and that the value of B, to which this increased distance corresponds, is smaller in the proportion of radius to the cosine of the angle of incidence, I. Hence the formula becomes $B = \frac{0 \cdot 0000256}{A \,.\, \cos \,.\, I}$. This property gives us the advantage of a variable scale; and in making the experiment, I found that at an incidence of 25°, when the divisions of 5000 in an inch were employed, the coloured images of the laminæ corresponded with those of the grooved steel. Hence the value of the quantity B, or the breadth of the fibres, is about the 5500dth part of an inch.

I next detached laminæ from parts nearer the pole, and I found that the coloured images gradually separated as the fibres approached to the poles; thus proving, what might have been inferred from other facts, that the fibres gradually taper in breadth from the equator to the poles of the lens. In order

* Fraunhofer found that in a system of lines where B was 0·0001223 of a Paris inch, A was 11° 25′ 20″: and in another system, where B was 0·0005919, A was 2° 20′ 57″.

to allow the fibres to be packed without condensation into spherical laminæ, their breadths must diminish as the cosine of the distance from the equator.

Although the prolate form of the spheroid indicates that the thickness of the laminæ, or of their component fibres, must increase slightly towards the poles, yet I have not been able to prove this experimentally, or even to determine the thickness of the fibres. I have more than once detached, by accident, a single fibre from the mass, and by an examination of the black line which forms its edges, I am satisfied that its thickness is at least five times less than its maximum breadth.

Having thus determined the form and size of the fibres, we come now to a very delicate and interesting part of the inquiry, namely, to ascertain the mode in which the fibres are laterally united to each other, so as to resist separation, and form a continuous spherical surface. The remarkable mechanism by which this is effected was first pointed out to me by an optical phænomenon. In looking at a bright light through a thin lamina of the lens of a cod, I observed two faint and broad prismatic images, situated in a line exactly perpendicular to that which joined the common coloured images. Their angular distance from the central image was nearly five times greater than that of the first ordinary prismatic images, and no doubt whatever could be entertained that they were owing to a number of minute lines perpendicular to the direction of the fibres, and whose distance did not exceed the $\frac{1}{2500} \times 5$, or the 12,500dth of an inch.

Upon applying a good microscope to a well-prepared lamina, I was delighted to observe the structure shown in fig. 2, where the two fibres, *a b*, *b c*, are united by a series of teeth, exactly like those of rackwork, the projecting teeth of one fibre entering into the hollows between the teeth of the adjacent one. The length of the teeth, or $n\,p$, is equal to about one half of $m\,n$, and this length of course diminishes towards the pole in the same ratio with the fibre. The breadth of the teeth is such, that five of them, namely, three of one fibre together with the two teeth of the adjacent fibre, which they inclose, are equal nearly to $m\,n + n\,p$, which is the measure of the quantity B in the preceding formulæ. With an ordinary microscope, the series of teeth between any two fibres seem to form a dark line, whose breadth is $n\,p$; and it is in consequence of the interruption of the light thus occasioned, when the lamina is dry, and the surfaces not in optical contact, that the fibrous lamina acts upon light like grooved steel, the space $n\,p$ in the lamina corresponding to the groove formed by the cutting diamond point in the metallic surface.

Although the parallel sides of the teeth do not form continuous lines, yet they produce colours by interference in the same manner as if they were continuous; and it is by their influence that the secondary prismatic images are produced in a line perpendicular to the sides of the teeth. In the living as well as in the recent lens the faces of the teeth and of the fibres are all in optical contact, and light passes through them in every direction in the same manner as if the whole lens were a continuous solid.

The toothed structure which has now been described, I have found in the salmon, haddock, herring, shark, and, indeed, in the lens of every fish where I have looked for it; and in this class of animals it is generally very distinct, and almost always capable of producing the secondary prismatic images. In the salmon, however, the teeth are much narrower than in the cod, and the colours which they produce are less distinctly seen.

Having thus determined the form and magnitude of the fibres and their teeth in a given lamina, it becomes interesting to ascertain if they suffer any change in shape or size at different distances from the centre of the lens. With this view I continued to remove one coat of the lens after another, till I reduced it to such a minute nucleus that I could no longer use it. I then dried it at the fire, and having crushed it, I obtained small portions of laminæ extremely near the centre: in this way I found that the fibres gradually diminished towards the centre of the lens, and the teeth in the same proportion, so that the number of fibres in any spherical coat or lamina was the same from whatever part of the lens it was detached.

In the lens of a cod, I found that there were 2000 fibres in an inch at the equator of a spherical coat or lamina, whose radius was $\frac{6}{30}$ths of an inch; consequently there must have been 2500 in the spherical surface*. If we now suppose that the breadth of each fibre is five times its thickness, and that each tooth is equal to the thickness of the fibre, or that five teeth are equal in breadth to a fibre, we shall obtain the following results for the lens of a cod four tenths of an inch in diameter:

Number of fibres in each lamina or spherical coat	2,500
——— teeth in each fibre	12,500
——— ——— in each spherical coat	31,250,000
——— fibres in the lens	5,000,000
——— teeth in the lens	62,500,000,000

or, to express the result in words, the lens of a small cod con-

* The radius being $\frac{6}{30}$ or ·2, we have $3{\cdot}1416 \times {\cdot}4 \times 2000 = 2513$.

tains five millions of fibres, and sixty-two thousand five hundred millions of teeth. A transparent lens exhibiting such a specimen of mechanism may well excite our astonishment and admiration!

The magnitude of the fibres and of their teeth varies in different animals. In the lens of a South American fish, for example, called the sheep-head, there are only 613 fibres in a spherical surface*; but it will be seen in a subsequent paper, that there are other animals in which the fibres are still more minute than those in the lens of the cod.

By means of very fine microscopes, with high magnifying powers, I have succeeded in detecting the same structure in the lenses of birds and quadrupeds; but in these classes of animals it is much less distinctly developed than in fishes. The teeth are much shorter; and when the lens belonged to an aged animal, the toothed structure was extremely indistinct and irregular, and in some parts of the fibres it entirely disappeared.

The fibrous structure represented in fig. 1, and in which the fibres converge to two opposite poles, is never found, in so far as my observations extend, in any of the Mammalia or Cetacea. It is the universal structure in the lenses of all the birds that I have examined; and though it is the most common structure in fishes, it is not the only one which they exhibit. In the following table, I have given the names of the different animals in whose lenses I have found the structure shown in fig. 1.

FISHES.

Cod.	Loch-Leven Trout.	Mackerel.
Haddock.	Turbot.	Coal Fish.
Serpent Fish.	Sole.	Gold Fish.
Diver.	Sayd.	Great Lamprey.
Mullet.	Grey Dog.	Bull-head.
Herring.	Flying Fish.	Whiting, English.
Holibut.	Frog Fish.	Pout.
Fresh-water Fluke.	Sea Cat.	Dab.
Salt-water Fluke.	Eel.	Plaice.
Spirling.	Pike.	Lumpsucker.

BIRDS.

Cassowary.	Turkey.	Curlew.
Albatross.	Barnacle Goose.	Chaffinch.
Pelican.	Sea Eagle.	Thrush.
Crane.	Snowy Owl.	Magpie.

* The diameter of the lens was [illegible]th of an inch, and there were 975 fibres in an inch at the equator.

Plover.	Cock.	Adjutant.
Common Pigeon.	Hen.	Peacock.
Wood Pigeon.	Green Linnet.	Kite.
Emberiza.	[Ornithorhynchus.]	Raven.
Alauda arvensis.	Red-tailed Hawk.	Gray Parrot.
Tringa.	Trumpeter.	Nicobar Pigeon.
Black Grous.	Vulture, Indian.	Crested Curassow.
Red Grous.	Circaetes brachydactylus.	Mandarine Duck.
Partridge.	Crested Guan.	Chukar Partridge.
Wild Duck.	Condor.	Owl, Indian.
Pheasant.	Rhea Ostrich.	[Macacus Cynomolgus*.]
Penguin.		

Lizards.

Lacerta striata. Lacerta Calotes.

For many of the lenses in the preceding table, and for others which I shall have occasion to enumerate in subsequent communications, I have been indebted to the kindness and zeal of Captain Basil Hall, Captain Robertson, Professor Grant, Mr. George Swinton, Dr. Knox, Mr. William Clark, and Mrs. Green of Cumberland Island.

In the Philosophical Transactions for 1816, I have described and represented by drawings the singular structure of the crystalline lens of the cod, as exhibited by transmitting through it a beam of polarized light. I have there shown that it consists of three different structures; viz. the nucleus, which has negative double refraction like calcareous spar; the external strata, which have the same kind of double refraction; and the intermediate strata, which have positive double refraction like zircon. The axis of vision, or that of the spheroidal lens, is the axis of double refraction for these three structures. I have discovered the same structure in the lens of the haddock, the salmon, the frog-fish, the skate, and the whiting, and indeed, with more or less distinctness, in all the lenses of fishes which were large enough to show the polarized tints.

A doubly refracting structure, related to the axis of vision, is distinctly seen in the human lens, and in that of quadrupeds and birds; but it differs considerably, both in its character and intensity, from that which exists in the lenses of fishes. In the paper to which I have already referred†, I have stated

* For the lenses of most of these birds, &c. and many others which will be described in subsequent papers, I have been indebted to the liberality of the Zoological Society.

† Philosophical Transactions 1816, p. 315.

that in the lenses of sheep and oxen there is only one series of luminous sectors, or one structure, corresponding with the intermediate set in the crystalline of fishes. This, however, is a mistake. By nicer means of observation I can distinctly see all the three structures in the lens of the sheep, the ox, and the horse; but, what is very singular, the nucleus and outer structure have positive double refraction, while the double refraction of the intermediate structure is negative; a result exactly the reverse of that which I have obtained from the lenses of fishes*. If this triple structure is intended, as I have already conjectured, to correct aberration, or to improve vision, it will be a curious problem to determine how this is effected, and to connect the one structure with the existence of a spheroidal lens without the aqueous humour, and the other with the coexistence of a flat lens and an aqueous humour.

When the lens of a cod is prepared so as to indurate and retain its form and transparency, the process of induration confounds all the three structures in one; viz. a negative doubly refracting structure many times more intense than that which exists in the recent lens. When a lens thus indurated was exposed to polarized light, I observed round the axis of the lens a beautiful system of negative uniaxal rings, seven in number, and intersected with a well-defined black cross. When the polarized light passed through any equatorial diameter of the lens, a system of seven biaxal rings was beautifully displayed, the principal axis being of course negative. This system of rings was intersected by the black cross, when the plane of the equator coincided with that of primitive polarization, and they displayed the two dark hyperbolic branches when the inclination of these planes was 45°. These phænomena I observed most distinctly in the lens of the boneta; and I have seen them also in the lenses of the cod, the shark, and the flying-fish, which happened to have been preserved without any fissures or loss of transparency.

In looking at a candle through the indurated lens of a cod,

* Since this paper was printed in the Philosophical Transactions, I have been able to examine the lenses of the Sheep, the Horse, and the Cow, at various ages, and to discover differences in the polarizing structure depending upon the age of the animal; but, what is very strange, I have seen the polarizing structure change after death, and when one lens was placed in distilled water, and exhibited only *two* series of sectors, both *positive*, I have observed the formation and development of *two* additional series of sectors, *both negative*, the one between the two positive series of sectors, and the other at the external margin, thus making four series, viz. +, −, +, −, commencing from the centre of the lens.

held at a distance from the eye, and which had been preserved for three years, I observed the candle encircled with three beautiful concentric rings, red on the inside and green without. These colours were no doubt those of mixed plates.

In examining the laminæ of the crystalline lens under the microscope, we are often perplexed with a variety of colours apparently spread over the surface of the plate. These colours are not the effect of chromatic aberration, but arise from the interference of the coloured pencils produced by the two surfaces of the laminæ. If we take a thin lamina, and, holding it opposite to a candle, look at its surface with a lens about an inch in focus, we shall see the whole of it covered with the most brilliant and varied colours, not inferior to those of the richest opal. These colours vary as we incline the laminæ in a plane cutting the fibres of it perpendicularly; and when the portion of the lamina is flat, they form a series of rectilineal serrated fringes perpendicular to the direction of the fibres. When the portion of the lamina is much curved, the fringes are irregular, and form, occasionally, returning curves of every variety of form and of every imaginable tint. If we immerse one of the surfaces of the lamina in a fluid of the same refractive power, we remove, as it were, the fibrous structure of that surface, and the serrated fringes immediately disappear. This observation led me to imitate these fringes by combining two series of grooves cut on the surface of separate strips of glass *. The effects were precisely similar and highly beautiful; and in the prosecution of the subject I was led to the observation of a series of very curious phænomena, which will form the subject of a separate communication.

XXXIX. *Observations on the Compound Eyes of Insects. By* Rudolph Wagner, *Professor in Erlangen.*†

On examining the works of John Müller on the Eyes of Insects, with a view to the 2nd Part of my Manual of Comparative Anatomy, I have only been able, as might be expected, to confirm the greater part of his statements. But with respect to the very interesting structure of the compound eye I have arrived at a different opinion. Strauss, indeed, represents small knob-shaped, or rather cup-shaped, ganglions of the fibres of the optic nerve, which Müller and Dugès

* These phænomena are much more splendid, when the grooves are formed upon thin plates of isinglass, by taking an impression of them from a steel surface.

† From Wiegmann's *Archiv für Naturgeschichte*, 3rr Heft, 1835; communicated by Mr. William Francis, Berlin.

deny. In *Sphinx Atropos*, however, I first observed how the fibres of the optic nerve surround the apex of the conical lenses, like a cup, and pass forwards over the lens to its anterior facet and to the cornea. The nerve, therefore, forms a true retina which surrounds the crystalline lens like a capsule. The nervous fibril is readily broken off under the apex of the cone, but the retina is also always perceptible at this part. The reason why this has been overlooked appears to me to be that they used too weak magnifiers; with a magnifying power of 300 times my statements will be confirmed. I have often repeated the observations on Beetles, (for example, on *Melolontha*,) on Papiliones, (for example, on *Pap. Urticæ*,) and others. Whether the crystalline lenses are cylindrical or hexagonal is difficult to say; and even where they may be 6-angular, as in *Melolontha*, of which I am still doubtful, the angles must be much rounded. In *Mel. vulgaris* (Plate II. fig. 3.), and more distinctly in *Mel. Fullo* (fig. 5.), each lens appeared to me to consist of six three-sided prisms, whose bases are directed outwards, so that they with their apices rejoin one another convergent in the axis of the lens. At *a* is the lens, inclosed by the sheath of its retina and entire, at *b* bisected; two prisms cover the third, which lies under them. *c* is a single prism from the side; *d* a similar one seen from the side opposite to the basis, (fig. 4. of *Sphinx Atropos*).

If this statement be true, the eyes of insects will appear only single ones inwardly aggregated; behind each cornea lies a crystalline lens with retina and choroidea; the layer of pigmentum takes the place of the latter. The rays of the axis, therefore, would not alone, as Müller supposes, fall on the nerves, nor the other rays be absorbed by the pigment, but would fall on the retina exactly as in the human eye. I have not yet examined the construction in the pyriform crystalline lenses without the cornea subdivided into facets.

We thus discover every day a greater complexity of apparently simple organs, and a closer analogy with the human construction, even in the Invertebrated animals. I think I find a greater perfection in the organization of the eyes in the *Annelida*, particularly in *Hirudo*, for instance. See my *Lehrbuch der Vergleichende Anatomie*, 2te Abtheilung, p. 428.

[*Note.*—Since these observations on the compound eyes of the Lepidopterous Insects were published by Wagner, a similar structure has been detected by Burmeister in the compound eyes of some of the Branchiopodous Crustacea.—Edit.]

XL. *On the Formula for the Dispersion of Light derived from M. Cauchy's Theory. By the Rev.* BADEN POWELL, *M.A., F.R.S., Savilian Professor of Geometry, Oxford.*

(Continued from p. 24.)

IN a former Number of this Journal I stated in a general way the nature of some considerations of importance respecting the formula of dispersion deduced from M. Cauchy's theory, promising at the same time to give some further account of the other researches of Sir W. R. Hamilton, with which it is connected, and of which the author has kindly allowed me to make use. In saying that that eminent mathematician had "taken up the subject," I beg it to be understood (and I mention it at his request,) that I allude to no other investigations than what are here to be given. In the present paper, therefore, I shall proceed to explain his method of comparing, in the first place, the degree in which the approximate and exact formulas accord with each other; and in the second, the analysis by which he deduces a process of computation by the exact formula: the numerical results, as derived from the two methods, will then be compared. Some illustrations of other points will form the subject of a future communication.

Upon the principles explained in the last paper, if we take the development of the sine divided by the arc, and square the polynomial, using the approximate formula,

$$\frac{1}{\mu^2} = H_1^2\left(\frac{\sin\theta_1}{\theta_1}\right)^2 \qquad (1.)$$

we shall have

$$\frac{1}{\mu^2} = H_1^2 - \frac{1}{3} H_1^2\,\theta_1^2 + \frac{2}{45} H_1^2\,\theta_1^4 -, \text{\&c.} \qquad (2.)$$

But if we use the exact formula,

$$\frac{1}{\mu^2} = S\left\{H^2\left(\frac{\sin\theta}{\theta}\right)^2\right\} \qquad (3.)$$

and substitute for θ its value $\frac{\pi\Delta\varrho}{\lambda}$,

we shall in like manner have,

$$\frac{1}{\mu^2} = S\,(H^2) - \frac{1}{3}\left(\frac{\pi}{\lambda}\right)^2 S\,(H^2\Delta\varrho^2) + \frac{2}{45}\left(\frac{\pi}{\lambda}\right)^4 S\,(H^2\Delta\varrho^4)$$
$$-, \text{\&c.} \qquad (4.)$$

Now, to compare this with the approximate development (2.) we may assume

$$H_1 = \sqrt{S(H^2)}$$

$$\theta_1 = \frac{\pi}{\lambda}\sqrt{\frac{S(H^2 \Delta \varrho^2)}{S(H^2)}} \qquad (5.)$$

which being substituted in (2.) will give

$$\frac{1}{\mu^2} = S(H^2) - \frac{1}{3}\left(\frac{\pi}{\lambda}\right)^2 S(H^2 \Delta \varrho^2) + \frac{2}{45}\left(\frac{\pi}{\lambda}\right)^4 \frac{[S.(H^2\Delta\varrho^2)]^2}{S(H^2)}$$
$$-, \&c. \qquad (6.)$$

Hence it is manifest that by our assumption of H_1 and θ_1 in the approximate formula, the two first terms of both developments are identical; but the third and subsequent terms will differ. We thus obtain an idea of the degree in which the approximation deviates from the truth.

In the next place, adopting the exact development (4.) we may proceed to the important discussion of a method of determining the coefficients, and of actually computing the value of μ in any given case. The development may be expressed in the following form, writing single letters for the coefficients:

$$\frac{1}{\mu^2} = A_0 - A_1\left(\frac{1}{\lambda}\right)^2 + A_2\left(\frac{1}{\lambda}\right)^4 -, \&c. \qquad (7.)$$

Now, if τ be the time of a vibration, or, what is the same thing, of the propagation of a wave, μ being the reciprocal of the velocity, we shall have

$$\frac{1}{\lambda} = \frac{\mu}{\tau}$$

Thus the series (7.) becomes

$$\frac{1}{\mu^2} = A_0 - A_1\left(\frac{\mu}{\tau}\right)^2 + A_2\left(\frac{\mu}{\tau}\right)^4 -, \&c. \qquad (8.)$$

Again, we might substitute other letters for the coefficients by making them include the powers of μ so as to express the series in powers of $\left(\frac{1}{\tau}\right)$ only. By extracting the root, and developing the reciprocal of the polynomial, it will be easily seen that the series resulting will be still one of the same powers of $\left(\frac{1}{\tau}\right)$, and may be expressed in this form,

$$\mu = a_0 + a_1\left(\frac{1}{\tau}\right)^2 + a_2\left(\frac{1}{\tau}\right)^4 +, \&c. \qquad (9.)$$

Now, if we substitute this value of μ in each term of the following expression derived from (8), viz.

$$0 = -1 + A_0 \mu^2 - A_1 \frac{\mu^4}{\tau^2} + A_2 \frac{\mu^6}{\tau^4} - \&c. \qquad (10.)$$

we shall have

$$0 = \left\{ \begin{array}{l} -1 + A_0 [a_0 + a_1 \tau^{-2} + \&c.]^2 \\ \quad - A_1 [a_0 + a_1 \tau^{-2} + \&c.]^4 \cdot \tau^{-2} \\ \quad + A_2 [a_0 + a_1 \tau^{-2} + \&c.]^6 \cdot \tau^{-4} \\ \quad \&c. \end{array} \right. \qquad (11.)$$

If in this expression we collect the coefficients of the same powers of τ and equate them respectively to zero, we shall at length obtain,

$$\begin{aligned} a_0 &= A_0^{-\frac{1}{2}} \\ a_1 &= \tfrac{1}{2} A_1 A_0^{-\frac{5}{2}} \\ a_2 &= \tfrac{7}{8} A_1^2 A_0^{-\frac{9}{2}} - \tfrac{1}{2} A_2 A_0^{-\frac{7}{2}} \end{aligned} \qquad (12.)$$

We might continue the process: but confining ourselves to the three first terms, we find, 1st, that a_0 a_1 are positive; and a_2 may probably be so; 2ndly, that, since the first coefficients A_0 A_1, &c. are all independent of μ, and since we have values of the second set a_0 a_1 in terms of these only, therefore these last are also *independent of* μ, that is, are *constant for all rays in the same medium*, but differ for different media. Thus in the series (9.) the value of μ will differ only by the change in the factor τ, that is, for rays whose times of vibration are different, or, in other words, for rays whose lengths of waves are different, that is, for the different primary rays. We shall thus obtain the means of calculating the value of μ for each ray independently of the medium, supposing we may restrict ourselves to the three first terms. For we have the three constants a_0 a_1 a_2 the same in the series for each ray; thus if we take such serieses for any four rays we can eliminate the three constants.

Let us then consider the four rays (in Fraunhofer's notation) B, D, F, H; for any one medium, we shall have this system of equations, viz.

$$\left. \begin{aligned} \mu_B &= a_0 + a_1 \tau_B^{2} + a_2 \tau_B^{-4} \\ \mu_D &= a_0 + a_1 \tau_D^{-2} + a_2 \tau_D^{-4} \\ \mu_F &= a_0 + a_1 \tau_F^{2} + a_2 \tau_F^{-4} \\ \mu_H &= a_0 + a_1 \tau_H^{-2} + a_2 \tau_H^{-4} \end{aligned} \right\} \qquad (13.)$$

Between these it is possible to eliminate the three medium-constants $a_0\ a_1\ a_2$, and thus to deduce a general relation, valid for all media, between the four indices $\mu_B\ \mu_D\ \mu_F\ \mu_H$, and the four periodic times $\tau_B\ \tau_D\ \tau_F\ \tau_H$.

Now, a little consideration will show that this relation can only involve the two ratios of the three differences of the four indices, and the two ratios of the three differences of the four reciprocals of the squares of the periodic times. Taking these we may for abridgement write,

$$\frac{\mu_D-\mu_B}{\mu_H-\mu_B} = s_D \quad \frac{\mu_F-\mu_B}{\mu_H-\mu_B} = s_F \tag{14.}$$

$$\frac{\tau_D^{-2}-\tau_B^{-2}}{\tau_H^{-2}-\tau_B^{-2}} = t_D \quad \frac{\tau_F^{-2}-\tau_B^{-2}}{\tau_H^{-2}-\tau_B^{-2}} = t_F \tag{15.}$$

Then it will appear that the result of elimination will be a relation between the four quantities $s_D\ s_F\ t_D\ t_F$ only; and will not involve the four other quantities $\mu_B\ \mu_H\ \tau_B\ \tau_H$, if we previously substitute for $\mu_D\ \mu_F\ \tau_D^{-2}\ \tau_F^{-2}$ their respective expressions deduced from (14.) and (15.), which are

$$\left.\begin{aligned} \mu_D &= \mu_B + s_D\ (\mu_H-\mu_B) \\ \mu_F &= \mu_B + s_F\ (\mu_H-\mu_B) \end{aligned}\right\} \tag{16.}$$

$$\left.\begin{aligned} \tau_D^{-2} &= \tau_B^{-2} + t_D\ (\tau_H^{-2}-\tau_B^{-2}) \\ \tau_F^{-2} &= \tau_B^{-2} + t_F\ (\tau_H^{-2}-\tau_B^{-2}) \end{aligned}\right\} \tag{17.}$$

This result of elimination will therefore be the same as if in the equations (13.) we had supposed

$$\left.\begin{array}{ll} \tau_B^{-2} = 0 & \tau_H^{-2} = 1 \\ \mu_B = 0 & \mu_H = 1 \\ \tau_D^{-2} = t_D & \tau_F^{-2} = t_F \\ \mu_D = s_D & \mu_F = s_F \\ a_0 = 0 \quad a_1 = b & a_2 = c \end{array}\right\} \tag{18.}$$

that is, the same as if we eliminated any two new quantities b and c between the three new equations

$$\left.\begin{aligned} s_D &= b\,t_D + c\,t_D^2 \\ s_F &= b\,t_F + c\,t_F^2 \\ 1 &= b + c \end{aligned}\right\} \tag{19.}$$

This last elimination is easy, and gives as the relation sought the following:

$$\frac{s_D}{t_D}\;\frac{1-t_F}{t_D-t_F}+\frac{s_F}{t_F}\;\frac{1-t_D}{t_F-t_D}=1 \qquad (20.)$$

This relation may be expanded by substituting the values (14.) and (15.) so as to put it under the form,

$$0=\left\{\begin{array}{l}(\mu_D-\mu_B)(\tau_H^{-2}-\tau_B^{-2})(\tau_F^{-2}-\tau_B^{-2})(\tau_H^{-2}-\tau_F^{-2})\\ -(\mu_F-\mu_B)(\tau_H^{-2}-\tau_B^{-2})(\tau_D^{-2}-\tau_B^{-2})(\tau_H^{-2}-\tau_D^{-2})\\ +(\mu_H-\mu_B)(\tau_F^{-2}-\tau_B^{-2})(\tau_D^{-2}-\tau_B^{-2})(\tau_F^{-2}-\tau_D^{-2})\end{array}\right\} \qquad (21.)$$

and in this way the relation (20.) may be verified, as it will then be found to be satisfied independently of the three medium-constants $a_0\ a_1\ a_2$ by the expression (13.) for the four indices.

Now, to proceed to the actual calculation, we have Fraunhofer's values of λ for the standard rays; these are obtained from interference, and are absolutely independent of any medium. Now if t' the time which light takes to traverse a given length l' *in vacuo*, will obviously have

$$\frac{t'}{l'}=\frac{\tau}{\lambda}.$$

If then we take t' as the unit of time, we have for the time of a vibration *in vacuo*

$$\tau=\frac{\lambda}{l'}.$$

Thus if $l'=\frac{1}{10000}$ inch, since by Fraunhofer's observations we have $\lambda_B = \cdot00002451$ inch, it follows that we have $\tau_B = \cdot2451$, and similarly

$$\tau_D = \cdot2175 \qquad \tau_F = \cdot1794 \qquad \tau_H = \cdot1464. \qquad (22.)$$

Now, there is a circumstance which may be remarked among these numbers, which affords a considerable facility in our calculation. The square of τ_F will be found to be almost exactly an harmonic mean between the square of the extreme values $\tau_B\ \tau_H$: or we have

$$\tau_F^{-2}=\tfrac{1}{2}\left(\tau_H^{-2}+\tau_B^{-2}\right) \qquad (23.)$$

so that in the notation of (15.)

$$t_F=\tfrac{1}{2} \qquad (24.)$$

Availing ourselves of this circumstance we may put the relation (20.) or (21.) under the simpler form

$$4\,s_F t_D(1-t_D)-s_D = t_D\,(1-2\,t_D) \qquad (25.)$$

or, what is equivalent,

$$\mu_D-\mu_F = a_D\,(\mu_F-\mu_B) + b_D(\mu_H-2\,\mu_F+\mu_B) \qquad (26.)$$

whence $a_D = -\,(1-2\,t_D) \qquad b_D = -\,t_D(1-2\,t_D)$

which are wholly functions of the values of τ, viz.

$$a_D = -\frac{\tau_H^{-2}-2\,\tau_D^{-2}+\tau_B^{-2}}{\tau_H^{-2}-\tau_B^{-2}} \qquad (27.)$$

$$b_D = -\frac{-\,\tau_B^{-2}}{\tau^{-2}-\tau_B^{2}}\cdot\frac{\tau_H^{-2}-2\,\tau_D^{-2}+\tau_B^{-2}}{\tau_H^{-2}-\tau_B^{-2}} \qquad (28.)$$

Thus employing the values (22.) of $\tau_B\ \tau_D\ \tau_H$, the following numbers result:

$$\log(-a_D) = \bar{1}\cdot80441$$

$$\log(-b_D) = \bar{1}\cdot06281$$

Now, to take an example of a particular medium; for flint glass, No. 13, Fraunhofer found

$$\mu_B = 1\cdot6277 \qquad \mu_F = 1\cdot6483 \qquad \mu_H = 1\cdot6711.$$

Hence by (26.) and the above logarithms, we may calculate the value of μ_D which will be found to result

$$\mu_D = 1\cdot63492;$$

by Fraunhofer's observation it was

$$\mu_D = 1\cdot6350.$$

Such is the method of Sir W. R. Hamilton: he has, however, not only calculated this example, but has gone through the values of the index, for the same ray D in all the media examined by Fraunhofer. These results I will subjoin, adding a column of the same values as computed by myself, by a tentative method with only the approximate formula, from my paper in the Philosophical Transactions.

Medium.	μ_D Observed by Fraunhofer.	μ_D Calculated by the exact Formula.	μ_D Calculated by the approximate Formula.
Flint-glass 13.	1·6350	1·63492	1·6355
Do. 23.	1·6337	1·63350	1·6335
Do. 30.	1·6306	1·63051	1·6305
Do. 3.	1·6085	1·60825	1·6079
Crown-glass M.	1·5591	1·55901	1·5593
Do. 13.	1·5281	1·52788	1·5279
Do. 9.	1·5296	1·52945	1·5296
Oil of turpentine.	1·4744	1·47444	1·4746
Solution of potash.	1·4028	1·40270	1·4029
Water.	1·3336	1·33346	1·3333

I shall not here enter on any detailed remarks or comparisons of the results exhibited in this table. From it the reader will be enabled to form a correct judgement of the degree in which the approximate method is comparable with the exact; at least for media of no higher dispersive power than those examined by Fraunhofer. Meanwhile we may just observe, that the results here given by the exact formula are invariably a little *in defect* compared with those of observation; whereas the approximate numbers are sometimes in defect and sometimes in excess. To this circumstance, and some further investigations connected with it, I shall recur in a future communication.

In the last Number (p. 113) I alluded to the calculations of M. Rudberg. It may be worth while to observe that such a formula as that which he adopted empirically, may give results nearly coinciding with those of the formula derived from theory which I have used, as will appear by the following considerations.

M. Rudberg's formula, in my notation, becomes

$$\frac{1}{\mu} = a\ \lambda^{(m-1)}.$$

Now (writing $a^{\left(\frac{1}{m-1}\right)} = a'$), let us suppose a quantity H′ so assumed that we have

$$H' = \frac{a'\ \lambda^3}{\lambda^2 - 1},$$

or,

$$a'\ \lambda = H'\left(1 - \frac{1}{\lambda^2}\right),$$

then (writing $H'^{(m-1)} = H$) we shall have

$$\frac{1}{\mu} = H\left[1-(m-1)\frac{1}{\lambda^2}+\frac{(m-1)(m-2)}{2}\;\frac{1}{\lambda^4}-, \&c.\right]$$

This may obviously coincide with approximate development

$$\frac{1}{\mu} = H\left[1-\frac{\theta^2}{6}\;\frac{1}{\lambda^2}+\frac{\theta^4}{120}\;\frac{1}{\lambda^4}\;-\&c.\right],$$

especially if we confine ourselves to the first two terms, (which is usually sufficient,) making $(m-1) = \frac{\theta^2}{6}$.

XLI. *Remarks on a Note on a Pamphlet entitled "Newton and Flamsteed" in No.* CX. *of the Quarterly Review. By the Rev.* W. Whewell, *M.A. F.R.S., Fellow and Tutor of Trinity College, Cambridge.**

To the Editor of the Quarterly Review.

My dear Sir, Trinity College, Cambridge, Feb. 3, 1836.

I HAVE just seen No. 110 of the Review; and I perceive that the reviewer of Mr. Baily's account of Flamsteed, in No. 109, has done my remarks on his article the honour of writing a note respecting them, which you have inserted.

As I do not see in this note any new arguments on the reviewer's side of our controversy, I do not conceive that I have occasion to add much to what I have already said, for I presume your readers do not look for an answer to mere hard words. A few additional remarks will, I think, enable competent judges to decide between us.

I asserted, and assert, that Flamsteed never fully comprehended or accepted Newton's theory;—never understood the difference between the Newtonian theory of the *causes* of the celestial motions, and the *empirical laws of phænomena* which he himself called theories;—in short, the difference between a formula and an explanation—between the discovery of *what* occurred, and the discovery *why* it occurred—between an observer and a philosopher. I quoted a letter which proved this; nor does the reviewer venture to deny the clear inference which irresistibly follows from this quotation. But he takes refuge in "the whole tenour of the correspondence," without quoting a single passage. To any one capable of understanding the distinction which I have pointed out, the whole tenour of the correspondence shows Flamsteed to have had no glimpse of this difference. For example, he says (Account of Flam-

* From the 2nd edition of Mr. Whewell's Pamphlet. See our last Number, p. 139—147.

steed, &c., p. 211,) of the theory, "*I call it mine*, because it consists of my solar and lunar tables corrected by myself, and shall own nothing of Mr. Newton's labours till he fairly owns what he has had from the Observatory;" and (p. 214) he says, that Newton "would needs question the observations when they agreed not with his theories, *or rather conceptions*." The book is full of such expressions. The Edinburgh reviewer, wiser than his brother, has pointed out this.

When my opponent has produced any one passage which shows that Flamsteed understood the difference between the nature of his own labours and those of Newton, (which these passages and many others prove he did not understand,) we shall be able to appreciate his claims to use language like that which he has applied to my opinions. Till then, such expressions as "audacious *dictum*," and "we must beg our non-undergraduate public to consider," must, I think, pass for bold words used to supply the lack of proofs.

I repeat also, that Flamsteed's complaining that the English nation was robbed, because Newton's theory of comets was confirmed by French observations, is another proof that Flamsteed did not understand what the nature, interest, or value of a true theory was.

With regard to the hard terms alleged by Flamsteed to have been used by Newton, I should, I think, have conveyed more exactly the impression which Flamsteed's angry statement leaves on calm consideration, by saying that it is probable that when Flamsteed had talked of the Royal Society as the robbers of his property, Newton did, in some way, employ the term "puppy"; but that it is certain that this was the hardest word which he was provoked to use; for it is abundantly clear that if anything worse had been said, Flamsteed was not in a temper, or of a character, to abstain from recording it. The reviewer's argument amounts to this:— that an angry man cannot exaggerate or misrepresent, because a clergyman ought not to lie. I do not think this will avail him.

On the subject of the sealed packet, I will put the issue in the form of a question. What does the reviewer take to have been the *purpose* of depositing the observations in Newton's hands? My answer is simple. From Flamsteed's known irritability, it was thought necessary to require this deposit, in order to secure the publication, in case Flamsteed should refuse to proceed. The case provided for arrived: the remedy was applied. I want to hear of any *other* interpretation of the deposit.

The exclamatory way in which the reviewer disposes of the account given by Arbuthnot of this step, appears to me rather tragical than logical. "The Queen's command. What a paltry, pitiful subterfuge! The Queen's command! How often is the name of royalty thus abused!" The evidence that it had been abused in this case is, I believe, only Flamsteed's opinion—"This I am persuaded was false" (p. 294)—which I hold to be altogether insufficient, even if he had been an uninterested and reasonable person.

The note quotes a passage of my remarks, in which I had said that I *left it to the reader to decide* "whether the reviewer had not shown an extraordinary ignorance of that part of scientific history," &c. As I wrote with the wish of avoiding anything offensive, I have once or twice since been disposed to regret that I had not left this decision to the reader, without *saying* that I had done so. I feel much less of this regret after reading the reviewer's acknowledgement respecting the preface to the first edition of the Observations, that "he certainly is ignorant of this preface;" and after his speaking of it as a want of *candour* to call it Halley's, which no person at all acquainted with the history of astronomy needs to be informed. As to the statement made in this preface, I need not inform those who have read my Remarks, that I did not put it forward as unquestionable authority, but as the case on one side, in opposition to the ex-parte statement made by the reviewer on the other. There is, however, this material difference;—that this statement of Halley's was published to the world, and challenged contradiction; that adopted by the reviewer is found in the moody soliloquies and querulous effusions of a weak man, which did not see the light till a hundred and thirty years later. As to Flamsteed's charges against Halley's edition, I can hardly suppose that the reviewer will carry any unprejudiced reader with him when he adopts them; though this proceeding is certainly in the general spirit of his treatment of the subject.

I did not argue the question of right in my Remarks; but I must now say that I am very far from assenting to the statements on this subject which have been published. The question of the kind of constraint which the nation has a right to exercise over the publication of the astronomer royal's Observations, I conceive to be a very difficult one: but Halley's statement that the Observatory had existed for thirty years and that nothing had been published, is a strong *primâ facie* case; for it would be absurd to suppose that the Observer was at liberty to lock up his observations for ever. What would be the use of such an Observatory? or the meaning of its having visitors? I must observe here that the reviewer has, very unwarrantably, transformed the statement that nothing was *published*, into a charge that nothing was *done*. The complaint was, that though much was done, nobody but the observer could profit by it.

I do not think it a reasonable infliction either on the reader or the writer, that a discussion of the character of one man should ramify into controversies on the merits of several others; and therefore I shall say as little as possible respecting Halley and Whiston. Halley, an eminent and vigorous philosopher, who devoted himself to science in the most liberal and useful manner during a long life, I hope to see vindicated, by some one acquainted with the history of those times, from the aspersions which the childish spleen and gall of an irritated rival threw upon him, and which have been so strangely and precipitately adopted by men of the present day. I lament his or any one's errors; but when the reviewer reminds us of the exclusion of Halley from the Savilian professorship on the ground of his want of religion, we may, perhaps, allow ourselves to hope that his subsequent election to the office implies that such unhappy opinions had been discarded. The charges of ignorance and immoral conduct are utterly at variance with all we know of him; and rest on nothing but Flamsteed's extravagant prejudices and passions servilely adopted by the reviewer. The friend of Newton, the favoured servant of King William, Queen Anne, Queen Caroline, to whom the offer was made of being appointed preceptor to the Duke of Cumberland, was never by any other person accused of want of respectability: and the man whom Lalande termed the greatest of English astronomers, and whom the severe-judging Delambre calls one of the most eminent men of science that Europe has produced, can suffer little from Flamsteed's disparagement of his knowledge.

I hold Whiston's testimony to be of small value (not that *he himself* was a *worthless person*, as the reviewer takes the liberty of misquoting me), from the extraordinary inconsistency, prejudice, and self-conceit, which I find in his memoirs of himself. That he had some mathematical knowledge is little to the purpose; though, even in such subjects, I suppose the reviewer is not prepared to admire the judgement which led him to recommend the scheme of finding the longitude by having ships moored all over the surface of the ocean, each to fire a gun at midnight, so as to be heard and seen at any place.

The reviewer states that Halley also kept his observations of the moon long unpublished, in order to have a chance of obtaining the reward for the longitude; and asks, "What does Mr. Whewell think of private property now?" To which I answer, that I think of Halley's property as I think of Flamsteed's. Halley did publish, and with dispatch, his other observations. I have never either defended or blamed his holding back the lunar observations; but I may observe that the crisis which gave the peculiar importance to the publication of Flamsteed's was past; and I do not think Halley's motive at all reprehensible. In all such cases it is difficult to decide what constraint may be applied so as to produce publication. There may be a fault of procrastination and fastidiousness, which was Flamsteed's. The attempt to expedite publication in the manner which may be most advantageous to astronomy is meritorious; and this merit was Halley's and Newton's. Whether in pursuit of this object they went beyond the limits which it is so difficult to define, I do not pronounce; but I am sure that Flamsteed was no judge of those limits; and his evidence is so far damaged by his circumstances and character, that it hardly helps us in deciding the point.

When you reviewers condescend to controversy, you have an overwhelming advantage in being advocate and judge at the same time. I presume it is in a momentary usurpation of the latter capacity that my opponent calls my pamphlet "rash," "unworthy," &c. And when, moreover, to the circulation and authority of the Quarterly, you add the rapid reply of a monthly periodical, as in the present case, a poor pamphleteer has no chance of being heard in opposition to you. I shall therefore take the vehicle nearest at hand for this letter, and send it to the Cambridge paper; by which means it may, I hope, come to the knowledge of several of those who care most about the question.

Believe me, my dear Sir, yours very faithfully,

W. Whewell.

To the Editor of the Cambridge Chronicle.

Sir,

I shall be much obliged by your publishing this letter as a postscript to that addressed to the editor of the Quarterly Review, which you did me the favour of inserting in last week's Chronicle.

Some of my friends, feeling that strong interest in the fair fame of Newton, which those cannot fail to feel who love to contemplate the union of intellectual and moral excellence, have expressed regret at my not having answered the charge that Newton neglected to acknowledge his obligation to Flamsteed for the observations by which the numerical elements of the lunar theory were determined; and that in the second edition of the *Principia* he erased the acknowledgement he had made in the first. I had passed over this point, as not bearing materially on the dispute respecting the publication of Flamsteed's observations, which appears to have attracted the largest share of the notice of the public; and with a view of abridging, as much as justice would permit, this unprofitable discussion of the errors and weaknesses of those whom we have been accustomed to admire: but a few words on the subject just mentioned may serve to show how much of mistake there is in such statements.

That the Newtonian lunar theory was published the second time without any acknowledgement of what it owed to Flamsteed, is not true. Newton's "Theory of the Moon," on its first appearance after the use of Flamsteed's observations, and on the only occasion (so far as I know) when it was published with that title, was inserted in David Gregory's *Astronomiæ Physicæ et Geometricæ Elementa*, printed in 1702. It is there stated (p. 332) that the illustrious author had made the calculations agree very nearly with the phænomena, "as he had proved by very many places of the moon observed by the celebrated Mr. Flamsteed." And the elements of the theory are there by Newton referred to Greenwich. With this book, Flamsteed was on various accounts much discontented. One great reason was, that Gregory had said, "The most solid walls, and even rocks and mountains, are not absolutely steady;" "This," says Flamsteed, "is a fling at my wall-arc." (Flamsteed, p. 204.) But I do not see that he here complains of any omission of his name in the Lunar Theory. Newton had previously communicated his theory to Flamsteed, in the shape in which the observer could understand and use it (Flamsteed, p. 72); and though Flamsteed speaks contemptuously and disparagingly of it, he employed it in constructing lunar tables, which he called a Theory. It is of this that he says, a little before the publication of Gregory's work, (p. 211,) "I call it mine, and shall own nothing of Mr. Newton's labours, till he fairly owns what he has had from the Observatory." The obligations of the theory of universal gravitation to Flamsteed, were of the same nature as its obligations to Tycho Brahe, who believed that the sun went round the earth. The observations were highly useful; but it would have been an absurd perversion of the truth to have called the observer one of the authors of the theory. Yet it is probable that nothing less than this, and probably not this, would have satisfied the discontented and morbid mind of Flamsteed. What was stated in Gregory's book was just; and I do not see what more could have been briefly said.

By the time of the publication of the second edition of the

Principia in 1713, (the year before the *sacrifice to Heavenly Truth*), the impossibility of noticing Flamsteed in any manner which would not disgust and irritate him, must have been very clear. Newton appears therefore only to have acted with common prudence and forbearance in avoiding such notice as much as possible. Flamsteed is not quoted as authority for the Lunar Theory, of which he rejected a great part. (See Account of Flamsteed, pp. 304, 305, 309.) His observations of the comet are quoted as the best. In several other points, as the observations of the satellites of Jupiter, Newton refers to published observations of other astronomers, instead of the private communications of Flamsteed. It was proper to reason upon published rather than upon unpublished observations; and the terms on which Flamsteed had put himself with Newton were probably felt by the great philosopher to be such as rendered it undesirable to make use of the private letters of his perverse correspondent.

So far as the published letters of Flamsteed prove anything, they show, that not only he did not feel himself injured by not being mentioned in those parts of the second edition of the *Principia* which refer to the moon, but that he entertained such an opinion of the work as would have made him angry at being so introduced. Thus, soon after the publication, he says, (p. 305,) "I think his new *Principia* worse than the old." And (p. 309) he writes to his friend Abraham Sharp, "I have determined to lay these crotchets of Sir Isaac Newton wholly aside; and I think if you purchase not the new edition of his book [of which the price was 18*s.*] you will be at least 17*s.* a saver by it; for I know not whether all the alterations and additions be worth 12*d.*"

So much for the wrong done to Flamsteed by not being sufficiently mentioned in the second edition of the *Principia.* I have been told also that I ought to have noticed more particularly some of the extravagant expressions of assumed authority and intemperate accusation which occur in the note in the Quarterly Review: but as these can affect only the character of the anonymous reviewer, I do not see how it can be worth while to make them the subject of remark.

I will again *leave it to the reader to decide*, after looking at the passages I have just produced, whether the writer of the note, in appealing to "the whole tenour of the book," as proving that Flamsteed comprehended and accepted Newton's Theory, was not asserting at random, and taking the chance of the impression he might produce, without having read the work which was under his review, or understanding the question on which he undertook to pronounce.

I suppose that if the vilifier of Newton has nothing to support him but rhetoric of this kind, the admirers of that great man will not feel any permanent inquietude; and my sole object will be answered.

I am, Sir, your very obedient servant,

Trinity College, Feb. 6, 1836. W. Whewell.

XLII. *Observations on a Note respecting Mr. Whewell, which is appended to No.* CX. *of the Quarterly Review. By* S. P. Rigaud, *Esq. M.A. F.R.S., Savilian Professor of Astronomy, Oxford.*

To the Editors of the Philosophical Magazine and Journal.

Sirs, Oxford.

The following remarks were, for the most part, drawn up before I saw the letters which Mr. Whewell has printed in the Cambridge Chronicle of the 6th and 13th of February*. Some parts of what had been written were found, in consequence, to be unnecessary; but leaving these to his able defence, I am still induced to offer the remainder to your consideration. Irritation is so great an obstacle to the attainment of truth, that I deeply regret the tone which the writer has assumed. That, however, I leave to his better feelings; my business is with his facts and his arguments.

S. P. Rigaud.

The reader is most probably acquainted with the Note in question; it seems unnecessary, therefore, to occupy his time with introductory explanations of the parts which have been thought to require correction. The topics, though examined separately, are taken nearly in the order which the original suggested.

Whiston was an honest and laborious man, but very deficient in judgement. As he advanced in life he became more pertinacious in error; he had sacrificed the world to his sincerity, and, conscious of moral rectitude in his purpose, he persuaded himself that he must be equally right in his opinions. Bishop Hare's own character adds no weight to the sentiments which he may express on this subject, but the few words which have been quoted from him are not contradictory to what is here said. Sir Isaac Newton, therefore, may be equally justified in his early patronage of his successor in the Lucasian Professorship, and in afterwards shunning his society. This change Whiston was unwilling to consider as just; and in

* See the preceding article of our present Number.

speaking of the man whose friendship he had lost, he says indeed what he thinks, but his thoughts, which at best were often inaccurate, were now warped by his feelings of disappointment.

I have not the slightest wish to take in any way from what may be justly due to Flamsteed; on the contrary, I honour his self-devotion to that department of science in which he was qualified so eminently and so usefully to excel; I honour his independence and noble application of his own property to his great (and it ought to have been national) object; I respect his religion, but I fear that I do not adopt so high a view of it as some of his undiscriminating admirers. I do not mean to express any doubts of his opinions on the great truths of Revelation, or of his general intention to conform his conduct to the dictates of Christianity; but his unhappy temper, irritated by disease, was suffered to become ungovernable. "If any man seem to be religious and bridleth not his tongue, but deceiveth his own heart," the apostle has told us the state to which he may be reduced. I presume to judge no one or to pronounce that "his religion is vain"; but, with every allowance for the weakness of human nature, I must say, that professions of forgiveness too frequently repeated, and constant assumption of the special favour of Heaven, are, when unaccompanied by kind thoughts and mild language, the sources of very painful impressions.

To enter fully into the character of Halley would require more time and space than can now be assigned to it; but there is one point which must not be passed over. To call him a "self-convicted infidel" is, to say the least, strong language, which when applied to the mighty dead, should not have been used without mature consideration. The authority, from which it is derived, was probably Whiston's account of the election in 1691 to the Savilian Professorship. The application, that Whiston makes of it to his own case, might have suggested the possibility of some bias in the direction which he gives to the story; and as the question is now about Halley's own view of his opinions, we have much better evidence in a letter which he wrote on the 22nd of June, in the same year, to Mr. Abraham Hill, which proves that, so far from submitting of necessity to an examination, in which he was likely to bear himself, as Whiston reports, with unbending defiance towards Bentley, *he courted the inquiry in confidence of being able to clear himself from the charge which was brought against him.* The letter likewise supplies us with the definite nature of this charge; for it mentions a *caveat* having been entered against him till he could show that he was "not guilty of asserting the eternity of the world." This objection necessarily* involved his being an atheist, and not merely a sceptic as Whiston says, which shows again the inaccuracy of his relation. It may be from the fault of a bad memory, it may be from a limited extent of reading, but I can at this moment recall to my recollection no one passage, in which Halley has published anything profane; and I may add that in some disquisitions on the general deluge, which he published in the Philosophical Transactions, he treats the Scripture account with all due respect. These disquisitions seem also to supply a clue to the cause of the *caveat*; for having reasoned on the dislocations visible on the earth's surface, he subjoined an explanation of his hypothesis, because it was suggested to him that those changes might rather have happened in times before the Mosaic creation, (when a former world was possibly reduced to chaos, out of whose ruins the present might be formed,) than at the period of the Deluge. This, in the eyes of many religious persons, may then have amounted to a heinous offence; but whether it did so with justice may now be safely left to the determination of Christian geologists. The passage immediately referred to occurs indeed in the Philosophical Transactions for 1724; but Halley had treated of the Deluge in the 190th number of the same collection, which, having been published in 1687, makes it not improbable that he may then, in discussing the subject among his friends, have used the same topics, and have thus raised the storm which burst on him in 1691. But to return to the term originally objected to: it was proposed, in 1691, to send in testimonials of Halley's character to the electors of the Savilian Professor; and the form, in one part, said that his friends recommended him from their "own long experience of his mathematical genius, probity, sobriety, and good life."

* This, perhaps, should not be assumed as a necessary consequence, lest injustice should be done to those philosophers, both heathen and Christian, who, *salvâ pietate*, have entertained the notion of the eternity of the world as the coexistent effect of an Eternal Intelligent Cause; the Stoics, for instance, Volkelius, &c.

Writers on Natural Theology now considered as of the highest authority, following the example set by Crellius, are, we believe, disposed to place most reliance upon the arguments to be derived from the course of nature daily presented to the view, as being of the greatest efficacy, both with ordinary minds and with those to whom abstruse questions respecting the *materia prima*, &c. may have suggested themselves.

"Nunc id," says Crellius, "quod tota Peripateticorum, imo et Platonicorum schola, non modò fatetur, sed et urget, probabimus, nempe res hujus universi omnes *finis gratiâ* existere; sed ita, ut *controversiam de materiâ primâ*, quæcunque tandem ea sit, *non faciamus nostram*."—Crellius, *De Deo et ejus Attributis*, cap. iii., in which work he was assisted by Stanislaus Lubjeniecius, a Polish nobleman, the author of the *Theatrum Cometicum*.—R. T.]

This passage is copied from a paper in Halley's own handwriting, and shows that "self-convicted" is the last term which can with propriety be applied to him. I hope that I feel as much as any man a deep abhorrence of irreligion, and I would not say a word to palliate its baneful nature; but to overload accusations of this kind with unsupported prejudice seems to me to be the surest way of destroying their effect.

That anything should have induced Newton to use harsh language to Flamsteed is sincerely to be deplored; but there are circumstances not to be neglected which may be gathered from Flamsteed's own account of what passed on the 26th of October 1711. His ironical thanks and recommendation to restraint of passion, were no soothers of irritation. While the accusation of robbery was dwelt on, it must be remembered that Newton was under the persuasion of Flamsteed having "called him an atheist"; that Flamsteed, when this was mentioned, left him, without the slightest notice, in error on so grave a point; and though he denies that he had uttered it, he does not deny that he had entertained the suspicion; for he only adds, "I hope he is none." If Newton, under such provocation, had remained unmoved, he would have been not merely (as he was) one of the first of men, but he must have been more than man; if the mildness of his natural temper had not wholly unfitted him for personal altercation, he never could have used such an unappropriate appellation as 'puppy' —how he would have expressed himself if more familiar with the language of reproach, I am unwilling to inquire.

When Newton called for the catalogue of stars, "It would neither be prudent nor safe," Flamsteed said, "to trust a copy of them out of my own keeping. He [Newton] answered, "that I might put them into his hands sealed up; whereby I understood they were to be so kept by him till I had finished the whole, and was ready to print it." Here then was no "solemn pledge"; not even any express conditions or precise explanation are said to have accompanied the delivery. Now Newton's undoubted object was to secure the publication of the catalogue, and as Flamsteed had taken his own view for himself, Newton may, on his side, have understood that the precaution of the seal was only to make the papers "safe" until the time came for printing them. There are difficulties about the story of this seal being broken, for it is told (I do not mean intentionally) without sufficient precision. Every honest mind revolts against a breach of trust; but we ought to be well convinced of the character of the act and of the criminality of the person against whom it is alleged, before we pour out our indignation against him. The description (in p. 294) seems to refer to the packet which was put into Newton's hands in 1705, and in another place (No. 163) Flamsteed says that the seal was broken when the catalogue was returned to him in 1708; but neither in his personal narrative (p. 86) nor in his letter to Sharp (No. 135), does he make any such complaint as he probably would, if the circumstance had occurred at that time. The sextant observations were completely printed in 1707, and the managers decided on the expediency of immediately proceeding with the catalogue; they may, therefore, have then considered the time to have arrived when it was necessary to open and examine the document; but there are particulars which seem rather to indicate that they had not broken the seal till a later period. Whether they were right or wrong in the proposed arrangement of the publication does not affect the question of the fact, and it is clear that nearly four years having elapsed, during which they could not overcome Flamsteed's opposition to their intentions, they determined to wait no longer for his concurrence. The Queen's order to proceed with the publication appears to have been issued in the beginning of 1711, and this seems to be the probable time when the seal was broken. It is inconceivable that Newton would have pleaded the authority of the Queen's order for what had taken place in 1708; and if he had, it is highly improbable that Flamsteed would have failed to notice so obvious a contradiction. By comparing Nos. 100, 104, and 199, it may be seen that, when irritated, Flamsteed could forget what he had written, and in the hurry of vexation he has here made a confusion in his narrative. Surely, therefore, it would be unjust, without more complete knowledge of particulars, to condemn Sir Isaac Newton and all his friends on such an accusation, which is neither explained nor corroborated by any concurring evidence. In such a case it would be more fair to judge of the story by his established character, than to sacrifice his character for the establishment of the story. One thing, however, may be fairly presumed,—that the Queen's order justified what was done; for Flamsteed in his reflections does not appeal from it, but confines his complaint to the authority not having been really obtained, or not till after the offence had been committed, (which latter supposition is introduced as if the first broader assertion was immediately accompanied by some doubts of its accuracy).

In the reference to what Halley says on the thirty years of Flamsteed's life, at Greenwich, the writer would have done well to have looked to the original. It is indeed said, in the preface, that during that time "nihil prodierat"—and nothing had been published; but, as Mr. Whewell had observed, it is added immediately after, "tot annos non effluxisse otiosos, schedasque Grenovicenses in haud modicam crevisse molem." The whole, therefore, together is a plain statement of an undeniable truth.

The work which is regularly done in the execution of any employment belongs of course to the employer, and his having made a hard bargain in no way affects his right. Any one, therefore, engaged in a great scientific work, was entitled to apply to the Astronomer Royal for assistance from his unpublished observations, when they had accumulated for years and there was no immediate prospect of their publication. A discretionary power certainly rested with the observer, but it referred to the nature and object of the application, and whether, if not immediately sanctioned by the Crown, it was such as to imply a fair presumption of the Royal approbation: the power did not extend to an arbitrary refusal. Flamsteed may be considered as obliging Newton whenever he readily communicated his official labours to him, but the greatest part of what he specifically "worked for Sir Isaac Newton" consisted in the reduction of his observations, an operation, in which he appears to have persisted contrary to the expressed wishes of Newton (No. 30).

"The sacrifice to heavenly truth" was not a holocaust of 300 copies of the book, for 388 pages of each were retained by Flamsteed, and form a part of the 1st vol. of the *Historia Cœlestis.* The whole that was burnt was the title and preface, with the catalogue, and 120 pages extracted from the later observations—about one fourth of what had been printed by the referees.

That 100*l.* per annum was too small a payment to the astronomer royal does not admit of a doubt; but his office existed long before the importance of it was rightly understood, and Burstow was a Crown living, which was given to Flamsteed by Lord Keeper North to set him more at his ease. This is not the manner in which the astronomer royal ought to be remunerated for his services; but in those days it was probably thought an easy method of saving the public money. This in no degree diminishes the injustice of not supplying him with what was necessary for the Observatory; and, although he certainly looked to some return from the sale of his observation, this was a miscalculation of what the market was likely to produce.

Newton, in 1691, (No. 14,) had said to Flamsteed, "If you and I live not long enough, Mr. Gregory and Mr. Halley are young men." The office of astronomer royal was a fair object of honourable ambition, but those who accuse Halley of the endeavours to supplant his predecessor, are bound to bring forward direct facts, not surmises, in support of the charge. With such an object, it was the more disinterested in him to hold that the salary ought not to be augmented. He may have done so in Flamsteed's time, but I am not acquainted with the authority for it. I have always heard that the objection was made by him to Queen Caroline, when she visited the Observatory, and expressed a wish for the inadequate payment being increased. From a document in the British Museum it is clear that this could not have taken place before September 1729. Halley, then, for nearly ten years continued himself to receive only the original "pitiful salary"; the report was erroneous, which Crosthwait heard, of his having in 1728 got an addition of 100*l.* per annum (No. 279.); and after all, he only obtained the further pay of the rank which he had held in the navy.

There are some particulars respecting Halley's observations which ought to be added to the writer's account, because they bear immediately on the present question. It was on the 2nd of March 1727 that Sir Isaac Newton reminded the Council of the Royal Society that they had neglected their duty by not having of late demanded, in obedience to the Queen's order, the fair copy of the annual observations. We see, therefore, that Newton's earnestness on this point did not originate in any personal feeling against Flamsteed, and the minute shows that he took the opportunity of Halley's being present to make the representation. The whole is given by Mr. Baily (in the Memoirs of the Royal Astronomical Society, vol. viii. p. 188.), and he adds, "It is worthy of remark, that this was the last meeting of the Royal Society at which Sir Isaac Newton was present, as he died on the 20th of the same month." It is not indeed improbable that his death was hastened by this exertion of the good old man in the execution of what he considered to be a duty. Hearne says, in one of his memorandum books, "Some time before he died, a great quarrel happened between him and Dr. Halley.......This 'tis thought so much discomposed Sir Isaac as to hasten his end." Sir David Brewster, in his Life of Newton, has alluded (p. 339) to this circumstance, but he does not seem to have noticed the time to which it refers. Halley, it must be admitted, in this case was wrong. His withholding the required documents and taking up Flamsteed's idea of the observations being private property were possibly, after Newton's death, never interfered with; and by the tacit acquiescence of the Government, not only the rights of the Crown were virtually abandoned, but the claims of the astronomer royal were confirmed by long-continued usage.

*** I have much regretted the line which has been taken by the Reviewers. The public mind will be made up on the differences between Newton and Flamsteed, and after a time this history will be left to the few who are curious about such subjects; but while new, there was something exciting in it, and it has been put prominently forward, while the British Catalogue, as republished by Mr. Baily, has been noticed with merely transient praise. Now this is certainly not the least valuable part of a very valuable volume. It is a work of useful and lasting reference for the astronomer, which possibly no one would have undertaken excepting the person to whom we are indebted for it, and which no one could have executed who had not, with the advantages of modern science, been, like him, for years familiar with the *Historia Cœlestis.*

XLIII. *On* Whiston, Halley, *and the Quarterly Reviewer of the "Account of Flamsteed."* *By* A Correspondent.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, Manchester, Feb. 20.

THE Note on Mr. Whewell in the late Quarterly Review is sufficiently revolting on account of its coarseness, and the insulting imputation on that gentleman of having presumed upon his official station in the University, and treated the subject of Newton and Flamsteed as if he were palming his opinions upon undergraduates. Now I leave it to the readers' of Mr. Whewell's letter to judge if ever imputation could be more unfounded, and if his letter be not altogether free from all appearance of assumption of the authority either of his office or (what is much more) of his high scientific reputation.

But what is still more reprehensible is the barefaced disingenuousness which the writer displays. What can be a more palpable misrepresentation than that contained in the following passage relating to Whiston: "If, therefore, he was the worthless, shallow person that Mr. Whewell would have us to believe...."? Now what Mr. Whewell really says of Whiston is, that his *judgement* is worthless. What is this, but an attempt to deceive the reader?

Another instance of this utter want of principle is displayed in the writer's reviling Halley for the very same conduct which he had in the preceding page eulogized in Whiston, namely, that he would not dissemble his religious opinions. "The secret history," says he, "of the enmity against Whiston, is his *conscientious departure* from the doctrine of the Church of England, and his adoption of the principles of Arianism." While of Halley he says, "Mr. Whewell cannot be ignorant that Halley was a *self-convicted infidel*, and that he lost an honourable and lucrative situation by being so;—and therefore, it seems more than probable that Flamsteed was disgusted with him."

It must be evident to everybody that the opprobrious term "*self-convicted*" must have been meant to impute to Halley a consciousness of *guilt*, of *moral depravity**: and his deviation from orthodoxy, whatever it was, and ingenuous acknowledgement of it, are, to suit the purposes of detraction, stigmatized as a disgraceful crime, while Whiston's, in order to make him an auxiliary, is justified and even praised as "a *conscientious departure.*" Let us try the question by making the terms change sides. Why did he not call Halley's "a *conscientious departure,*" and Whiston "a *self-convicted Arian*"?—evidently to serve the cause of falsehood by insinuating a prejudice. As for the term *infidel*, we know how vaguely and inconsiderately, and malignantly, it has often been used; and that Newton himself was even called an atheist by some of his contemporaries†. The character and extent of the deviation of these distinguished men from any standard of opinion is wholly another consideration: but the moral quality of the fact of their entertaining and avowing their convictions is the same. With regard to Halley, Whiston's account bears direct testimony to his sincerity and disinterestedness.

I will only add, that the Note is, with regard to honesty, of the same stamp with the article which it vainly attempts to defend; and remain, Gentlemen, yours, &c. C. S.

* Sirrah, 'tis conscience makes you squeak.
So saying, on the fox he flies.
The *self-convicted* felon dies.—*Gay's Fables*, ii. 1.

† Even in our own time a venerable and pious divine and distinguished naturalist has not escaped similar malignity from one who aspired to be a competitor; see Phil. Mag. and Annals, N.S. vol. x. p. 373: and in the Morning Chronicle, a journal pretending to great liberality, those philosophers, who from their ascribing to the Creator the power of enduing matter with life and thought, are denominated materialists, have also lately been stigmatized as atheists.

XLIV. *An Abstract of a Memoir on Physical Geology; with a further Exposition of certain Points connected with the Subject.* *By* W. Hopkins, *Esq., M.A., F.G.S., of St. Peter's College, Cambridge.**

IN a memoir entitled "Researches in Physical Geology," lately printed for the Transactions of the Cambridge Philosophical Society, I have endeavoured to develop, by reasoning founded on mechanical principles, and by mathematical methods, the effects of an elevatory force acting simultaneously at every point beneath extensive portions of the crust of the earth, in producing in it dislocations and elevations such as we now recognise. I have not there, however, attempted to give any exposition of the mechanical principles on which the investigations are founded, beyond what was necessary to make the subject intelligible to persons familiar with investigations of a similar character; but, with the hope that the interest which the subject of elevations must always possess in the estimation of the speculative geologist may appertain in some measure to any new theoretical views respecting it, I have now been induced to attempt a somewhat more detailed and popular exposition of the mechanical considerations which have entered into my own investigations, and which must in some measure, I conceive, enter into all others on similar points possessing any claim to a demonstrative character. I cannot expect to remove difficulties inherent in such investigations, and which must be felt to be considerable even by those best prepared to enter upon them; but if I should succeed in so far diminishing them as to render the subject more accessible by the only way in which, in my opinion, it can be successfully approached, my object will be accomplished. What I have now written may be considered as an abstract of a considerable portion of my memoir, with a somewhat more detailed exposition of several points connected with the subject of it.

When natural phænomena, characterized by general laws, have suggested to us a general cause to which they may be referred, our first object must be to investigate the consequences of this cause acting under certain conditions, and to compare our results with those deduced from observation. Observation, however, unaided by theory, can rarely accomplish more than to detect approximations, more or less accurate, to those perfectly definite laws which the phænomena would

* Communicated by the Author.

accurately follow under the influence of the principal cause alone to which they are referrible. The coincidence between these perfectly definite laws and those deduced from our assumed general cause, independently of perturbing ones, must afford the strongest test of the truth of our assumption. The strength of the evidence thus derived, will of course depend in such cases upon the accuracy of the approximation to definite laws in the observed phænomena; but it is important to observe, that this first approximation must always be the most important one, and that it must be made the instant we begin to speculate on the causes of such phænomena as I have alluded to, if the slightest value is to attach to our speculations; and also that accurate (or what is synonymous in all, or at least in all but the simplest cases,) mathematical methods of investigating the effects which would result from our assumed general cause, are just as necessary in the case we are supposing, as if the observed phænomena presented accurate coincidences with the general laws to which they only approximate.

These remarks (sufficiently trite perhaps) are made with the view of meeting directly the vulgar objection of the uselessness of applying mathematical investigations to geological problems. To assert this is, in fact, equivalent to the assertion that that branch of the science with which we are immediately concerned presents no phænomena characterized by general laws, or referrible to a definite and simple cause. Such however is not the case. The phænomena do distinctly approximate to obvious geometrical laws, and there is a simple cause to which they may be referred, the effects of which it has been my object in the memoir in question to investigate on mechanical principles, in order that we may compare the laws obtained from these results with those to which the observed phænomena are found to approximate.

The phænomena with which we are chiefly concerned in these investigations are those dislocations of the crust of the globe, which we recognise more particularly in *faults* and *mineral veins*, or rather in the narrow fissures in which what is properly termed the mineral vein is deposited. The latter phænomena might, in fact, be almost entirely comprehended in the former, since it is found very generally, where mineral veins occur in stratified masses, that the strata are somewhat higher on one side of the vein than the other. In general this difference of level (not exceeding, perhaps, a few feet) is not sufficient to be designated as a *fault*, though it sometimes increases so much as to be considered such. In these cases it would appear absurd to suppose that the fissure of the mineral vein and the fault are not to be referred to the same mechanical origin, or that other veins in the same district should not be referred to the same cause as such an one as that just described, from which, except where the above-mentioned difference of level becomes great, they differ in no respect. It is also highly important to observe, that (as far as investigation has yet proceeded,) where faults and mineral veins co-exist in the same district, they follow, with reference to their positions, precisely the same laws.

I do not mean, however, to maintain that all mineral veins are necessarily to be referred to the same mechanical cause. I conceive that some of the Cornish veins—those, for instance, of St. Austle Moor—are clearly referrible to some cause quite distinct from that in which the veins of our limestone districts have originated. The latter possess, I believe, universally the characters which lead us to regard them as having originated, like faults, in dislocations produced by mechanical violence, while the former are almost totally destitute of these characters. It would, therefore, be absurd to conclude that these two classes of veins have necessarily had the same origin. It is not, however, from *à priori* considerations that these points are to be finally decided: but since the evidence of dislocation afforded by a fault is independent of its vertical magnitude, I cannot but regard the mineral veins of our limestone districts as indicative of dislocations in the masses in which they exist, equally with the faults with which they are so frequently associated. I therefore regard them in this point of view; the correctness of our doing so must, of course, be ultimately tested by the harmony which may exist between our theoretical deductions involving this hypothesis, and the phænomena which these veins actually present to us.

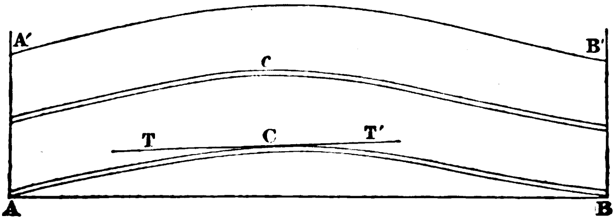
The planes of these dislocations approximate, in the first place, to verticality; and, secondly, their horizontal directions bear distinct relations to the general configuration of the elevated district in which they exist. If there be a central *axis* of elevation, the directions of dislocation are approximately parallel or perpendicular to it, as is the case in most of our mining districts; and if there be a central *point* of elevation, these directions diverge from it as a centre. Such appears to be the case in Mount Etna, and the groups of the Cantal and Mont Dor. The lake district in this country probably affords a similar instance.

These are the laws established by observation, so far as it has yet extended. Many anomalous cases may possibly exist, but they will not invalidate the conclusion, that, so far as the phænomena are characterized by these laws, they are attributable to the action of some general cause, at least as extensive in its operation as the district throughout which the phænomena are observed to follow the same law without breach of continuity. This cause is assumed to be that which naturally suggests itself to the mind of every geologist, viz. an elevatory force acting simultaneously at every point of a portion of the earth's crust, of at least the extent just intimated, and of any assigned thickness. It is manifest, that the elevation of this mass must produce in it *extension*, and consequent *tension*, which, if of sufficient intensity, will cause those dislocations or *fissures* which we now recognise in the phænomena already alluded to. These fissures must, according to this theory, be regarded as the *primary* phænomena, with which all the other phænomena of elevation, as faults, mineral veins, anticlinal lines, &c., are connected as *secondary* ones.

I have carefully abstained in my memoir from any speculations on the causes which might produce this elevatory force—I merely assume its existence. It is easy, however, to conceive such a force to act as above supposed, if we assume the existence of a cavity beneath the elevated mass, either originally coextensive with it, or rendered so by the action of the elevatory force itself. Any vapour or matter in a state of fluidity from heat, forced into this cavity, or expanded there, will produce the elevatory force which I assume to have acted. This appears to be the simplest mode in which we can conceive such a force to be produced; and if we choose to set out from the more remote hypothesis of the earth's having been originally fluid, it might probably be shown that the formation of cavities such as above supposed, would, under simple conditions, be the necessary consequence of that process of cooling by which we must then suppose the crust of the globe to have assumed its present solidity. Instead, however, of assuming the existence of a cavity, we might suppose a portion of the solid matter of the earth, at a certain depth beneath its surface, to become by some means expanded, and by its expansion to elevate the superincumbent mass. This hypothesis, as far as my investigations are concerned, would equally suffice, as, in fact, would any other by which we could account for the simultaneous action of an elevatory force upon a portion of the earth's crust of sufficient extent. For many reasons, however, independent of my immediate object, I should not hesitate to reject this latter hypothesis as generally insufficient to account for observed phænomena, and as involving serious physical difficulties. If we adopt the hypothesis of internal cavities, we may observe that there is no reason why we should not suppose them to exist, not only at different depths in different places, but also along the same vertical line, so that one shall be placed under another. It might, I conceive, be shown to be highly probable, if we should again recur to the hypothesis of the original fluidity of the globe, that the deeper cavities would in such case be the more extensive.

The immediate consequence of the elevatory force, as already remarked, will be to produce *extension*, and consequent *tensions*, in the elevated mass. Our first object must be to determine the directions of these tensions, for the purpose of ascertaining those of the resulting fissures. We shall afterwards consider the influence of the constitution of the elevated mass; at present it is only necessary to regard it as admitting of a certain small extension without rupturing.

I. For the greater simplicity let us first suppose the elevated mass to be of indefinite length, of uniform depth, and bounded laterally by two vertical parallel planes, beyond which the disturbance does not extend. Let A B B′ A′ be a section of the mass by a vertical plane perpendicular to the axis of ele-



vation, A C B originally coinciding with A B; and let us also suppose that every such section is precisely similar and equal. Then it is manifest that there can be no extension perpendicular to these sections*, and that, consequently, the whole extension must lie in directions perpendicular to the axis of elevation. Now let us conceive for a moment the elevated mass to consist merely of a very thin continuous lamina of it, A C B. Then it is evident that the extension, and therefore the tension, at any point, as C, in the section, must be in the direction T C T′ of a tangent to the curve line A B C. Let us now conceive another lamina, similar to the first, but without any adhesion to it, superposed upon it. It is clear that its extension, and, therefore, its tension, must be precisely the same as that of the first lamina, always supposing the original

* The hypothesis of indefinite length in the elevation is equivalent to that of its being terminated by sections equal and similar to the one described in the text, so far as relates to the absence of longitudinal extension.

unextended dimensions of each to have been the same. Again, suppose a third lamina superimposed in the same manner, and then a fourth, and so on, till a mass of any assigned thickness shall have been thus composed. It will then follow, from what has been shown, that the tension at any point *c* of the mass in this state must lie in the plane of the section, and in the direction to the tangent of the curve-line *a c b*, formed by the intersection of the vertical plane of the section with the lamina in which the point *c* may be situated.

The only difference between this hypothetical mass and any proposed actual mass of the same form and dimensions, will consist in this—that in the former there is no cohesion whatever between the successive laminæ of which we have supposed it to be formed. If, however, our laminæ should be superposed on each other in their unextended state, and made to cohere firmly together, (in which case the mass would differ in no wise from any actual mass,) and then elevated to the position represented in the diagram, it is easily seen that the position of each point of the mass would be exactly the same as in the hypothetical case above stated. Consequently, the *extension* of any portion of the mass (and therefore the tension) must be the same in the two cases. Hence then it follows that if A B B′ A′ represent any actual elevated mass, the direction of the tension at any point *c* will be that of the tangent line at that point as above described.

There is no difficulty in extending reasoning precisely similar to the above to any more complicated form of the elevated mass, of which the upper and lower surfaces were originally parallel, and horizontal, and we shall arrive at this conclusion.—*If we conceive the mass, previous to its elevation, to be composed of horizontal laminæ (or thin strata) the directions of the tensions at any proposed point of the mass when elevated but still unbroken, will lie in the tangent plane to the curved surface formed by that originally horizontal lamina in which the proposed point may be situated; and the intensity of the tensions will be the same*, in different laminæ at points similarly situated in each.*

If the mass in its undisturbed position be not of uniform depth, (*i. e.* if the upper and lower surfaces be not parallel,) the above reasoning would not be accurately applicable. The case, however, we have considered may be taken as the standard one to which others will approximate with more or less accuracy, particularly as physical reasons might be assigned

* There are causes why this should be only very approximately true. (See Memoir, p. 42.)

why an extensive cavity within the earth should be nearly horizontal. Adhering then to this case, it is manifest that the extension of each component lamina of the mass will depend on the *form* assumed by it when the mass is elevated, since its boundaries, by hypothesis, remain immoveable. Consequently the direction of the tension *in the tangent plane* before mentioned must also depend upon the form of the lamina. This direction is not generally horizontal, but since it will usually be nearly so, and will always determine the horizontal direction, or azimuth, of a vertical plane drawn through it, we shall be understood when it may be convenient to speak of the *horizontal* tensions.

It is manifest then that the determinations of the *directions* of the tangential tensions in the elevated mass, must in cases such as the above be a purely geometrical problem, as may be easily elucidated by a few instances. In the elevation already described (of which the segment of a cylinder, by a plane parallel to its axis, may be regarded as the approximate type, and which may therefore be termed *cylindrical*) it has been shown that this tension lies entirely in a vertical plane perpendicular to the axis. If the elevation approximate to the form of a cone (which may be conceived to be formed by the superposition of similar conical shells), it may be shown*, that if each lamina remain unbroken, the direction of the only tension will be parallel to the slant side of the cone, and will pass through its axis; but that if a dislocation exist along the vertical axis, the principal tension at any proposed point (particularly near the vertex) will be perpendicular to the vertical plane passing through that point and the axis, there being also another tension in that plane. If again the form of the elevation should approximate to the segment of a sphere, there will be two tensions at each point of the mass, one of which will lie in the plane through the proposed point and the vertical axis of the elevation, the other being perpendicular to that plane.

The above are some of the most simple forms which the elevated mass can be conceived to assume; they may, however, be taken as the approximate types of many of the general elevations which present themselves to our observation, considered independently of their local irregularities. When the superficial boundary of the elevated mass is very irregular, (particularly if the superficial extent be not very great,) the directions of greatest extension, or of greatest tension, will be very different in different points; and it may become very dif-

* See Memoir, p. 47.

ficult to calculate with any precision the resulting phænomena. Cases however may easily be conceived without such difficulty, though more complicated than the simple ones above alluded to. Suppose, for instance, recurring to our hypothesis of internal cavities, one cavity of great extent to exist at a certain depth, and another smaller one within the mass above the former, and communicating with it, so that any fluid pressure acting in the lower should be communicated immediately to the upper one. That portion of the elevated mass which lies directly above the upper and smaller cavity, may manifestly be subjected simultaneously to the tension impressed upon the whole mass from the action of the elevatory force in the larger cavity, and to that produced by the partial elevation above the smaller one. These two sets of tensions may be conceived to be superimposed the one on the other, in the same manner as any two sets of forces in equilibrium may be so superimposed *. Their intensities and directions will depend on the forms of the general and partial elevations respectively. Thus we may have a partial elevation of which a cone or segment of a sphere should be the approximate type, superimposed upon a general one of which the type should be the segment of a cylinder. Other combinations might be formed in a similar manner.

Should it appear preferable to consider the subject independently of the hypothesis of internal cavities, we have only to conceive our partial elevations to be produced by a more intense action of the elevatory force at those points. As regards the resulting state of tension, it is perfectly immaterial which hypothesis we adopt.

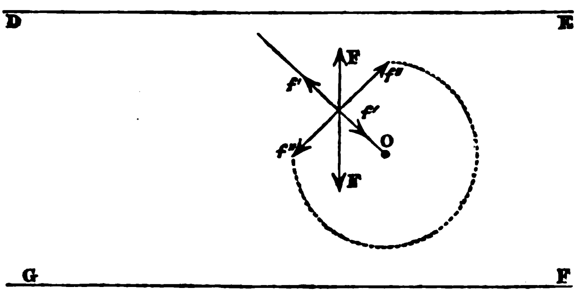
The states of tension above described refer to the mass in its elevated but unbroken state, *i.e.* previously to the formation of those fissures which must of course be formed when the tension shall become greater than the cohesive power of the mass. The tension will begin to be produced at the instant the act of elevation commences, and will increase till it acquires the intensity just mentioned. *Time* will be necessary for this, but it may possibly be so short as to give to the action of the elevatory force the character of an *impulsive* action, which would probably produce the most irregular phænomena, and such as would be altogether without the sphere of calculation. I exclude therefore the hypothesis of this kind of action, not as involving in itself any manifest improbability, but as inconsistent with the existence of distinct approxima-

* One of these sets of tensions may possibly modify the other, but in a general explanation, or in a first approximate calculation, this modification may be neglected.

tions to general laws in the resulting phænomena. It would appear probable however that the time above mentioned will be short, and I therefore assume it to be so, and that consequently the tensions increase *rapidly* but *continuously* from zero to that degree of intensity which is necessary to overcome the cohesive power of the elevated mass. This assumption has also the advantage of facilitating some parts of the mathematical investigation*.

It will, perhaps, be somewhat more convenient for our further investigations, if we conceive the tensions at different points of one of our elevated, but still continuous and unbroken, component laminæ, transferred to corresponding points of a plane lamina †. For this purpose, imagine each point of the curved lamina projected on a plane horizontal one, and that the same tension exists at each point of the latter, as at the point of the former, of which it is the projection; the direction of each tension in the horizontal lamina being the projection upon it of that of the corresponding tension in the curved one. Now one of our ultimate objects will be, to determine the horizontal directions of the fissures which must result in the elevated mass, when the tensions become of sufficient intensity to produce them, and these directions may be considered as coinciding with those which would be produced in our hypothetical horizontal lamina. Consequently our investigation will be reduced to the determination of these latter directions.

To elucidate this, suppose our general elevation to be such as first mentioned above, or what I have termed cylindrical. Its projection on a horizontal plane will be a parallelogram,



represented by D E F G. Suppose also a partial elevation

* See Memoir, p. 21.

† We may remark that the vertical elevation of the disturbed mass, in the state above described, is always extremely small compared with its horizontal extent.

approximately spherical, superimposed upon the general one, such that O shall be the projection of its vertical axis, and the dotted circle that of the circumference of its base. Then taking P as the projection of any proposed point in the partial elevation, we must suppose applied there, first, a tension (F) impressed on the mass generally perpendicular to D E; secondly, a tension ($f_{\prime}$) in a direction passing through O (see p. 233); and thirdly, another tension $f_{\prime\prime}$ perpendicular to P O. From these data the directions of the fissure through P, when the tensions become sufficient to produce it, must be determined. And here we may remark, that since one lamina of our elevated mass will be similar to another, the tensions F, $f_{\prime}$, and $f_{\prime\prime}$, will be very approximately the same for each; and that consequently the direction of the fissure just mentioned will equally determine the horizontal direction of the fissure which shall pass through any point of which P is the projection. The extensibility of the mass being assumed to be small, the intensities of the tensions F, $f_{\prime}$, $f_{\prime\prime}$ will be proportional to the extension each would produce in the mass at P, if it acted separately, or to the additional extension produced by each when acting simultaneously. The accurate determination of these intensities would in most cases present great difficulties. In general, however, it will be sufficient to consider such tensions as $f_{\prime}$ and $f_{\prime\prime}$ (belonging to the partial elevation) merely as forces producing modifications in the effects of F, the nature of which can be determined with sufficient accuracy for practical purposes.

[To be continued.]

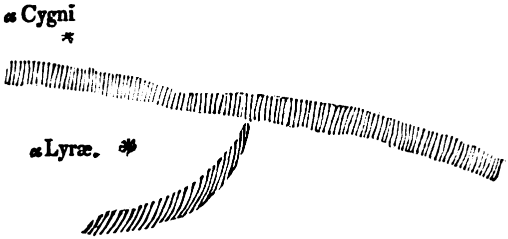
XLV. *On the Aurora of November* 18*th*, 1835. *By the Rev.* T. R. Robinson, *D.D.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

MR. STURGEON's notice of the aurora of ☿ November 18th (not 16th as misprinted,) induces me to send you the notes which I made of its appearance, as from his positive statement "that he saw no appearance of aurora to the south of the zenith, though frequently looked for," this seems to be one of the very rare cases where auroral phænomena can be proved to occur in a low region of the atmosphere. They are as follows, the time being reduced to Greenwich.

"Nov. 18. Sky strongly illuminated, but covered with clouds till 9^{h} 20^{m}, when two arches were visible, which broke suddenly into streamers. The largest was nearly straight, but was met by another band of streamers making an angle with it, thus:



South of the zenith, however, there is a permanent arch, its lower edge on α and γ Orionis, and at the crest its altitude is 30° 13′ *. Its upper edge passes through the Pleiades, but it is broader there than at the vertex. This greater breadth seems to be a fragment of another arch coalescing with the principal one, and is fading away.

"9h 41m. The lower edge is on Aldebaran. The upper still on the Pleiades.

"9h 48m. The arch suddenly becomes more luminous. The altitude of its lower vertex is now 34° 21′. A splendid yellow streamer darts along 30° of its upper edge, parallel to it, (which is new to me, for all that I have noticed hitherto were perpendicular to the arches or nearly so;) with little intermission clouds and rain, but the arch when last seen unchanged.

"10h 35m. Clear. The arch has disappeared, but the whole sky is covered with flashes which are brightest to the north."

I may add, that the arch gave sufficient light to read the seconds of my chronometer and note them down, so that it seems impossible that Mr. Sturgeon could have failed to observe it, had it been visible at Woolwich. If this was not the case, then probably the appearance which he describes was the dissolution of my arch, and this meteor must have been lower than any which I have seen. Perhaps some of your correspondents may be able to afford additional information.

Armagh Observatory, Feb. 5, 1836. T. R. Robinson.

* These altitudes were taken by the sextant, bringing the visible horizon's image to the arch, and measuring its altitude (in this case 0° 51′) by a circle in the daylight.

XLVI. *An Account of Experiments made at Constantinople on* Drummond's *Light, for the purpose of Lighthouse Illumination in the Black Sea. By* W. H. Barlow, *Esq., Civil Engineer. Communicated by* P. Barlow, *Esq., F.R.S., in a Letter to the Editors of the Lond. and Edinb. Philosophical Magazine and Journal of Science.*

Gentlemen, Royal Military Academy, Feb. 4, 1836.

I CAN hardly tell how far the following account of experiments made on Drummond's light at Constantinople may be considered deserving a place in your scientific Journal: it is to me highly interesting, on account of the ingenuity and perseverance it displays in the pursuit of a scientific object, under very difficult circumstances; and I think that it must be gratifying to scientific men generally to know that the Turks, hitherto so bigoted to old maxims and religious prejudices, are availing themselves of the most refined discoveries of modern philosophy.

It may be well to state, as introductory to the following letter, that Mr. W. H. Barlow has been a resident for some time in Constantinople, for the purpose of constructing a brass-foundery and boring-apparatus, upon a large scale, with a view of remodelling the Turkish artillery; and that on the return of Namik Pasha from this country, (who had examined with a scrutinizing eye many of our manufacturing and scientific establishments,) Halil Pasha, the sultan's son-in-law, sent for Mr. Barlow, and spoke to him on the subject of restoring some dilapidated lighthouses in the Black Sea, and requested to know if he was acquainted with a very remarkable light which was known in England under the name of Drummond's lamp. He was answered that he knew of it generally, and that if he could find any description of it in any of his books, he would furnish him with the particulars. Fortunately, on referring to an ingenious Armenian physician, Dr. Zohrab, who had studied at Edinburgh, he fell upon a number of the Nautical Gazette in which an account was given of the light, and on the ground of the information thus obtained the experiments detailed in the following letter were undertaken.

I am, dear Sirs, yours very truly,

Peter Barlow.

Letter to Peter Barlow, Esq.

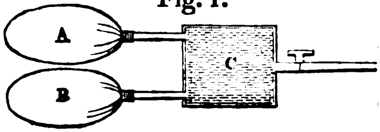
Constantinople, Jan. 6, 1836.

"I have already informed you of my first experiments on Drummond's light, and the astonishment it produced in the

Turks when it first shone forth in all its brilliancy. 'Mash-allah allah gunez boo!' was heard on all sides, and I must acknowledge that my astonishment and delight were no less when I first found my attempts successful, in which Dr. Zohrab equally participated, neither of us having ever seen it in England. I promised you that on my return from examining and reporting on the state of the lighthouses in the Black Sea, I would give you a detailed account of my proceedings, a promise which I now propose to redeem as far as the extent of a letter will permit.

"When Halil Pasha first mentioned the Drummond's light, having searched my own library in vain for any description, I applied to Dr. Zohrab, who, having studied in Edinburgh, and being in the habit of reading English works, I thought might possess the desired information; and fortunately he had a number of the Nautical Gazette in which was given several particulars of the light, with drawings, and as we were reading of its beauties, a sudden thought struck us of trying to make it. I set to work that night, and made a drawing of the simplest apparatus I could conceive capable of producing the desired effect, which was as follows. In fig. 1, A and B are two bladders, one containing oxygen, the other hydrogen. C is the mixing-box, to which they are attached by being firmly tied upon the two projecting pipes.

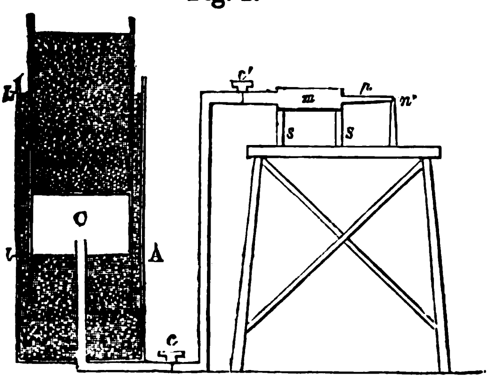
Fig. 1.



In this box were placed about thirty pieces of wire gauze, which, by the by, we were sadly at a loss to obtain till we accidentally fell upon two wire-gauze masks which had been used at the last carnival; these were instantly cut up and arranged in the mixing-box, at the upper end of which we attached the small pipe and stopcock as in the figure. The stopcock belonged to an apparatus of Dr. Zohrab's, and the small pipe was made by an ingenious Armenian at Galeta. Thus prepared, we filled the bladders with the proper gases (after only one unsuccessful attempt), and a piece of lime placed on a lump of clay was put before the jet: a board was then placed on the bladders with a weight on it. We then lighted the jet, and to our inexpressible joy a light instantly burst forth so intense that it was impossible to look directly at it. This being accomplished, and our apparatus appearing safe, I determined to exhibit the light itself to the Pasha, instead of the drawing of it which I had promised him. The astonishment and approbation were, as I have stated, very great, and I was immediately dispatched to the Black Sea, to examine and report on the state of the lighthouses. On my return I was requested to make a larger and more complete apparatus, in which I have succeeded to the full extent of my expectation. This last light burns for an hour; it is described below; but I must here first mention a circumstance attending our first exhibition. After this was over, Dr. Zohrab and myself removed our apparatus, and there being still some gas in the bladders, we lighted it again for our own amusement in my drawing-office, when it exploded with great violence while I was pressing the bladders with my hands. You remember the explosion of my gases in my little room at Rushgrove Cottage, but that was nothing; this was so sharp that I lost the sensibility of my right ear for nearly a month, and the explosion forced pieces of the bladders quite through the cloth of my trowsers; and yet, excepting my ear, I escaped without injury.

In my large lamp it was necessary to have recourse to gasometers instead of bladders. These, according to Drummond's description, were to act under a pressure of 30 inches of water; and our explosion had taught us that this pressure must be very equable to prevent the mixing of the gases in any great quantity. Many were the schemes I had, and rejected, but at last I adopted the following:—A, fig. 2, is a cylinder of tin two feet in diameter, and four feet six inches high, closed at the bottom, and open at the top; B is another cylinder, one foot nine inches in diameter, of the same height, having a diaphragm at one foot eight inches from the bottom; this formed the hydrogen gasometer, and was used as follows: From the bottom of the larger cylinder rose a pipe D, to the

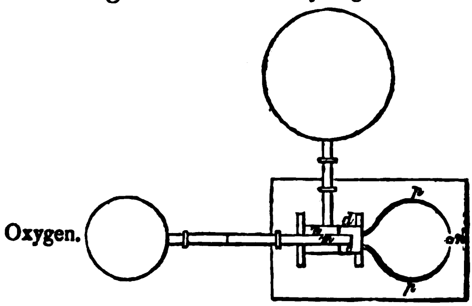
Fig. 2.



height of one foot nine inches, and a small recess was made an inch deep in the diaphragm of the inner cylinder to receive its end; the inner cylinder, therefore, being placed within the other, its edge rested on the bottom of the latter. To fill the gasometer, the interior cylinder was taken out, and water poured into the other to the level *l*; the former was then replaced, the stopcock *c* opened, and the air expelled till the diaphragm reached the surface of the water; the gas was now introduced at the stopcock, and the gasometer thereby raised: twenty-seven inches of water were now poured into the part B, which, together with the weight of the tin, made up the whole pressure of thirty inches. This forced part of the water in D up the sides of the vessel, and other water was added till the external water rose to the level L, which is twenty-nine inches above the top of the pipe; and consequently restored the water in the lower part of the gasometer to its original level *l*. It is now evident that as many inches of gas as are let off are supplied by the upper part descending; and the surface of the upper and lower diameter being the same, the level of the water at *l* and L always remained the same, and consequently the pressure. There is, moreover, very little friction, and the action is soft and equal. The oxygen gasometer was constructed in the same manner; but being only required to hold half the quantity, its area of bottom was made only half the former, the height being the same. The other parts are easily comprehended: *c'* is another cock; *m*, the mixing-box; *s*, *s*, its supports; *p*, the emission pipe; and *n'*, the lime-ball. Fig. 3 is a plan of the whole, showing both gasometers. The mixing-box is made by soldering the pipe *m* into the outer pipe *n*, which has a diaphragm pierced with holes; the part of the pipe *m* projecting through it has also holes round its side.

The lamp is lighted thus: the hydrogen being let on by

Fig. 3. Hydrogen.



its stopcock being opened, passes into *n*, and through the diaphragm d into c, and passes out through the pipes $p\,p$; this is now lighted, and it burns with a red unsteady flame; then the oxygen stopcock is turned gradually, when this gas passes through the holes into c, where it mixes with the hydrogen, and they come out in perfect union at the pipes p, p. The hydrogen cock is now fully opened, and the other cock gradually opened and adjusted till the lime-ball gives out its most brilliant light, when the hydrogen flame entirely disappears.

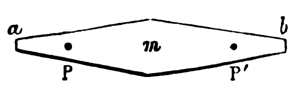
The difficulties we encountered and the extraordinary shifts we were put to would be very amusing to you, but they are too long for a letter; suffice it to say, that in the end the experiment succeeded beyond our most sanguine expectation. The Pasha was delighted with its performance, and has taken the apparatus to his palace. I have since exhibited to him coal-gas light, which I managed much easier, and have drawn out my estimates for this light and oil; but no doubt the latter will be preferred, and I soon expect to be at work in putting in proper repair the lighthouses of Fanaraki. I am anxiously waiting your further description of Beale's light, which I will also show to the Pasha, who takes great interest in all these matters.

W. H. Barlow.

XLVII. *Additional Remarks on the Law of Magnetic Attractions and Repulsions. By the Rev.* William Ritchie, *LL.D. F.R.S.**

As Mr. Fox still seems to think that the law of magnetic attractions is inversely as the distance between the *ends* of the attracting magnets, *without any reference whatever to their form*, the following considerations will, I think, convince him and every impartial inquirer that the supposed law has no existence in nature.

Let two magnets be formed, of plate steel, into the annexed figure, having the poles at P, P′, and consequently *further* from the ends a, b than if the bar were rectangular; then the attraction between those magnets will follow very *different* law from that which exists when the bars are equally broad. The fact is, the supposed law obtained by measuring from the *ends* of the magnet will *change* with the *length* of the *magnets*, their *form*, and even with the *uniformity* of the *temper*.



* Communicated by the Author.

The law in question, then, being a *function* of so many *variable* quantities, must be one of extreme complexity, perhaps beyond the powers of the most refined analysis to unfold.

XLVIII. *On the Theory of Gradients on Railways. By Mr.* W. S. B. Woolhouse.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

As Dr. Lardner and Mr. Barlow, in your Numbers for January and February, hold out conflicting opinions on the theory of gradients on railways, and have left the subject in a state more calculated to create doubts in the minds of the less informed of your readers than to lead them towards the formation of settled conclusions, perhaps you will favour me with the insertion of a few words, by way of explanation, as far as the philosophy of the question presents itself to my mind. Mr. Barlow, without absolutely saying which of the two solutions is wrong, though probably quite conclusive in his own view of the matter, first states his objection to the arithmetical results of the formula employed by Dr. Lardner for the velocity, in certain cases, then gives an outline of his principle of investigation, and finally expresses himself "quite content to leave the decision to those whose minds have not already received a bias from preconceived notions of the forces." Whatever sentiments may prevail as to the competency of my opinions on such a subject, it will at least be acknowledged that I possess the qualification of being free from the bias here alluded to, and I am induced to hope that your readers will, on this very ground, acquit me of any imaginable interference in thus undertaking, voluntarily, the examination of a point that has already had the attention of such distinguished individuals. By close and continued application of particular opinions to particular subjects, it is indeed surprising how they fix themselves in the mind, and become ultimately, whether true or false, of almost a fundamental character. But I do not consider this observation to be applicable to the present case. It is my wish to simplify and expose the truth as far as I can perceive it. I do not, however, intrude the present remarks in elucidation of the subject without some degree of hesitation, although quite free from apprehension as to their theoretical soundness. To many of your readers, who must be far from satisfied with the present situation of the question, I nevertheless feel myself justified in submitting them.

According to Dr. Lardner, the subject is "totally distinct from the consideration of accelerating forces"; he considers it to be essential that the velocities be continued uniform, and therefore discards everything in the shape of an accelerating force. Now, in order that such a theory may be sustained, it is a well known elementary principle of forces, that the power employed must be always precisely equal to the resistance, or the amount of friction combined with the proper resolved effect of gravity along the railway, observing, however, that in the term friction, we must include the resistance to the motion experienced by the carriages, &c., in passing through the atmosphere. We shall not here discuss the practicability of preserving this exact balance between the forces at the various changes of inclination; nor shall we offer any serious objection to the principle that the friction is the same for all velocities, which has received the sanction of general practice, though doubtless inaccurate, as far as regards the effect of the atmosphere.

Continuing the notation of the preceding letters, we have t for the moving power that will keep the load moving at a uniform speed V along the level plane; $t + \sin \epsilon$ for the moving power to keep the load moving at the same uniform speed up the inclined plane; and $t - \sin \epsilon$ for the moving power to sustain the same uniform speed down the inclined plane. To the truth of this there cannot be any doubt, if we assume, as Dr. Lardner has done, that the friction t is not altered by the slight inclination of the plane. By following Dr. Lardner's reasoning, we are hence fairly led to the result that the same amount of mechanical force will be expended in ascending and descending the inclined plane, as in drawing the same load backwards and forwards along the level plane of the same length L.

Though Dr. Lardner is certainly justified in stating this conclusion to be a plain result of first principles, it should at the same time be remembered, that it rests solely on the hypothesis that the power in each case is to be precisely adapted to the amount of resistance, so as to preserve throughout the the same uniform velocity V. This hypothesis has not been admitted by Mr. Barlow, and it must necessarily fail in determining the effect produced by the deflection of a rail during the transitory passage of the carriages. In this way, it appears to me that the principle advocated by Dr. Lardner carries with it a restriction that entirely unfits it for an objection to what has been advanced by Mr. Barlow, in his Second Report, addressed to the directors of the London and Birmingham Railway Company. On the other hand, "however, I can only come to Mr. Barlow's conclusion, that it is altogether erroneous, both in theory and practice," when the assumed maintenance of uniform motion is objectionable, as it most certainly is, in the case of the deflections of rails. Contenting myself at present, then, with the opinion that the contending parties thus view the question of power expended, on different suppositions as to the way in which it is applied, I shall just take a very brief sketch of the question of velocity, when the motion is not assumed to continue the same through planes of different inclinations.

Dr. Lardner supposes that in cases of uniform velocity, the resistance into the velocity is constant, and on this assumption deduces the equations stated by Mr. Barlow in page 97, viz.

$$(t - \sin \varepsilon)\, v = t\, V \qquad v = \frac{t\, V}{t - \sin \varepsilon}$$

This assumed principle is, in my opinion, decidedly inaccurate, more especially when it is contemplated that the carriages will pass along with the uniform velocity so expressed. For uniform motion can only be continued when the moving force continues equal to the resistance; and assuming with Dr. Lardner that the amount of friction is independent of the velocity, the speed will in such a case be quite indeterminate; or, in other words, the power so applied will sustain uniformly *any velocity* that may have been previously communicated. If the friction were *really* independent of the velocity, while a moving force which exactly balances the resistance would maintain uniformly *any previously imparted motion*, a moving force which exceeded the resistance would transmit the carriages with a velocity continually accelerated, in conformity with what has been said by Mr. Barlow: but as the portion of resistance arising from the atmosphere at least, increases with the velocity, it is evident that the resistance will gradually augment till it balances the moving force, and so a uniform motion will eventually succeed. If the carriages be so acted upon as to retain a uniform velocity v along a level plane, and with such velocity and moving power they arrive at the upper end of, and proceed down, an inclined plane, the investigation given by Mr. Barlow, pages 98—100, will be strictly accurate on two suppositions, viz. 1. That the friction is independent of the velocity and inclination of the plane; 2. That the action of the moving power is not diminished by the increase of velocity. The former supposition is sanctioned by Dr. Lardner; the latter, as Mr. Barlow justly observes, if not true, will have the effect of giving the velocity and space passed over, rather in excess of the truth, and therefore the more favourable for a comparison with Dr. Lardner's velocities, which are so much in excess. There can be no doubt as to the inaccuracy of the preceding formula, from which the last-mentioned velocities are calculated, as the principle from which it is derived is not founded in theory.

Yours, &c.

February 20, 1836. W. S. B. WOOLHOUSE.

XLIX. *Note respecting the Undulatory Theory of Heat, and on the Circular Polarization of Heat by Total Reflexion. By* JAMES D. FORBES, *Esq., F.R.SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WHEN a subject so vast and so little explored as that of radiant heat is undergoing investigation, it is hardly to be expected either that experimentalists should abstain from speculation, or, on the other hand, that such speculations should be, in all cases, happily devised by their authors, or fully apprehended by men of science generally. The more immediate results of M. Melloni's researches as to the nature of heat, do not seem to me to have been very philosophically stated in such expositions of them as I have seen (at least in English); but it is not of this that I at present mean to speak. M. Melloni lately read a paper to the Academy of Sciences stating certain objections to the undulatory theory of heat, on which M. Ampère has lately published some ingenious speculative views, but which (so far as I know) has received little or no experimental support except that which I have given in investigating the laws of its polarization. I wish to point out what I conceive to be the present state of the subject, speculatively regarded, and to mention an additional discovery which I have recently made in confirmation of these views.

The arguments which M. Melloni adduces to prove that light and heat are not the same modification of matter all amount to this,—that they may be separated, often in the most irregular and capricious manner, as when the action of a coloured medium absorbs certain rays of the luminous spectrum and yet leaves unaltered the symmetry of the heating spectrum *. Such experiments, or many simpler ones, show that *heat is not light*, but nothing more. If M. Ampère really meant that the light of the solar spectrum *is* the same thing with the heat of the solar spectrum, nothing is easier than to refute it, and I pointed out as distinctly as words can express the fact, that light and heat are apparently separable in my

* *L'Institut* (Journal), 23rd Dec. 1835.

paper on Polarization, Art. 25. "all our experiments point to the first [conclusion], namely, that heat, though intimately partaking of the nature of light, and accompanying it under certain circumstances, is capable of almost complete separation from it under others." This is all that can be said as to the matter of fact, and includes within it all the experiments quoted in M. Melloni's paper. For it will be found that the difficulties which beset the undulation theory of heat are all addressed to our *ignorance*, not to our *knowledge*; they are *negative* rather than *positive*; they refer entirely to dispersion and absorption, the two great difficulties of the theory of Young and Fresnel; and it would be equally presumptuous and unreasonable to expect to find at once in the new and obscure subject of heat a solution of doubts which the far more complete knowledge which we have of the subject of light has been unable to resolve. The objection of the impermeability of one substance to heat which is permeable to light, cannot prove light and heat to be "two essentially distinct modifications of the condition of the ethereal fluid *;" for the same objection applies to different *kinds* of light; a *red* glass is impermeable to *yellow* light, though it is perfectly transparent for *red* light. To say that this conclusion results from the phænomena of *partial* absorption by coloured glasses, is taking advantage of the total ignorance we are in with regard to luminous absorption, as a sort of negative argument. The only result is what I have already stated, (and I agree with M. Melloni that it is unanswerable,) that one and the same undulation does not *invariably* impress the senses of sight and feeling at once. The great difficulty is this—to account for the equal refrangibility of two waves having different properties. This I conceive is the *whole* difficulty at present. Now I argue that this cannot be urged as an *ultimate* difficulty until the undulatory theory of dispersion is complete, which, notwithstanding the most remarkable investigations and experiments of Cauchy and Powell, I scarcely think can be admitted to be accomplished. *The difference between heat and light must be such that the law of refrangibility shall either be independent of it, or shall admit of one result corresponding to several values of the distinguishing element.* Thus, if the *length* of the wave be the sole distinction, the *velocity* in a dense medium must admit of a single value for several values of the length; a very supposable case, as such functions are frequently periodical †.

* *L'Institut*, p. 411, note.

† Though M. Cauchy's expression contains a trigonometrical function, it could not *physically* apply to this supposition.

Or the distinction may be founded on the extent of displacement of the ethereal particles, or on a want of coincidence with the law of force produced by displacement as commonly assumed, or on a thousand other causes, on which I do not wish to dwell because I see little advantage in presenting premature hypotheses which a year or two may demolish. I cannot help observing, however, in an experimental point of view, that if Sir D. Brewster's analysis of the solar spectrum be adopted, we have a difficulty in the case of light identical with that in the case of heat. It surely would have been unreasonable to urge against that analysis that it could not be true, because it is contrary to the assumption that colour depends on frequency of vibration;—and refrangibility solely upon the velocity of a wave:—these are the very points to be proved, and if we have no breach of analogy between light and heat but on ground still debateable as regards the former, the supporters of calorific waves have little to tremble for.

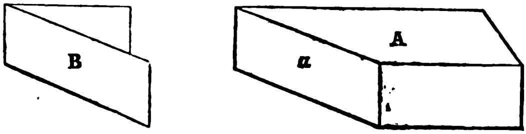
Since the experiments of M. Matteucci respecting the interference of calorific rays have been treated (and I am inclined to think justly) as inconclusive, the proof of the polarization and double refraction of heat is the only one to which we can refer with any confidence as a basis of analogical reasoning. The phænomena of polarization and depolarization of perfectly dark heat I have now succeeded in making as obvious as most of the more ordinary experiments on transmission, and I have lately succeeded in completing the analogy in one case which seems to put the nature of the calorific emanations beyond a doubt. Fresnel's marvellous prediction of the circular polarization of light by two internal total reflexions at certain angles, is justly appealed to as one of the most conclusive evidences in favour of a theory which could foresee so singular a result. By employing a rhomb of rock salt I have obtained precisely analogous results in the case of heat wholly unaccompanied by light*. The loss is so trifling in passing through this amazing substance (the discovery of whose properties I hold to be the most valuable part of M. Melloni's valuable labours), and the total reflexion so far complete, that this curious and complex experiment is almost as easily tried as any of those in common polarization.

With such evidence before me I cannot for a moment doubt that the waves (if such there be either in light or heat) produced by non-luminous hot bodies are *identical in character* with those producing light, *that is, that the vibrations are transversal.*

Before concluding, I beg to mention briefly a decisive ex-

* Communicated to the Royal Society of Edinburgh, 1st Feb. 1836.

periment which I have made to show that conduction has no influence in producing the appearance of polarization. For a statement of the objection I refer to my note inserted in this Journal for November last. It is thus obviated:

P. 

I had a tin vessel constructed of the shape shown at A, which had one surface *a* similar in size and position to the first or polarizing plate used in my experiments, and of which the secondary radiation to the analysing plate was supposed to produce the variations observed. The analysing plate B was placed between the thermo-electric pile P and the vessel A, which was filled with *boiling* water, and which therefore produced on an enormously exaggerated scale the effects attributed to my mica plate. The vessel A was then turned into the various rectangular positions as regarded B, without any decided difference of effect on the pile being observable; indeed, if any, that effect indicated a *maximum* of heat reaching the pile when the position of the surfaces was unsymmetrical, or when in polarizing, it is *least*. This experiment was also repeated for the case of polarization by reflexion.

By a particular process (which I will take another opportunity of describing,) I have been enabled to prepare mica plates, which, whilst they polarize more effectively than my former ones, are of *extreme tenuity*, so that they are almost incapable of becoming sensibly heated. With such plates I can polarize about 50 per cent. of heat wholly unaccompanied by light, and readily polarize the heat of boiling water.

I am, Gentlemen, yours truly,

Edinburgh, 12th Feb. 1836. JAMES D. FORBES.

L. *Reply to some Remarks contained in* Dr. John Davy's *Life of* Sir Humphry Davy. *By* CHARLES DAUBENY, *M.D., Professor of Chemistry, &c., Oxford.*

MR. EDITOR,

IN Dr. Davy's lately published book, entitled "Memoirs of the Life of Sir Humphry Davy," occurs a passage reflecting on myself, on which I feel myself called upon to offer a few remarks.

After noticing his brother's change of opinion with respect to the cause of volcanos, Dr. Davy proceeds as follows:

"It would hardly be supposed, that my brother's motives for modifying his views respecting the nature of volcanic action, as above stated, and for giving up in part a brilliant hypothesis, could be misinterpreted, and referred to an unworthy feeling; yet this, to my surprise, has been done, and even by Dr. Charles Daubeny, Professor of Chemistry in the University of Oxford. This Gentleman, in defending the hypothesis which he advocates, and which is precisely my brother's early hypothesis, comparing Sir H. Davy's early views with his later, says, 'The authority of Sir H. Davy may, I conceive, on this occasion, be fairly pleaded against himself, and the weight of his *ipse dixit* in the two latter years of his life be viewed as counterbalanced by the contrary judgement he had pronounced, apparently on the same evidence, at an earlier period; neither is it inconsistent with what we know of his character, to suppose that he should have acquired a distaste for the theory in question, when he found it seized upon and illustrated by an humble [humbler] class of inquirers.'"

"This I would remark is neither generous nor just, nor even reasonable criticism. It is not generous to assign to unworthy motives, a meritorious act; for so surely may be viewed the relinquishing such an hypothesis by the author of it, when he found it not sufficiently supported by facts. It is not just, because not true, that he merely gave his *ipse dixit* against his early hypothesis; in my brother's observations on volcanos, as I have mentioned, he assigned his reasons for so doing, consisting chiefly in want of the positive evidence which he expected to have met with in examining into the phænomena of active volcanos, provided the chemical theory were true. And least of all, is the criticism reasonable: it is almost absurd to suppose that my brother would relinquish his hypothesis because approved of and advocated by others. Dr. Daubeny might as well have fancied that he would have changed his views respecting chlorine, and the metallic bases of the fixed alkalies, as soon as they were seized upon and illustrated by an humbler class of inquirers."

The asperity of the above remarks seems but little warranted by the occasion which has called them forth.

Had Dr. Davy been aware of the sentiments I have always expressed relative to his deceased brother, he would have acquitted me of any wish to depreçiate his memory, and would have felt that even in the absence of any other mode of accounting for this change of opinion, I should have abstained from suggesting one which would have seriously disparaged it.

But the whole amount of the charge (if charge it can be called) which I had brought against Sir H. Davy, consisted in attributing to him some degree of fickleness or caprice in the abandonment of a preconceived opinion, apparently without sufficient reason.

How far the motive suggested for this change of opinion may be consistent with the character of the individual himself, (which is now a matter of history, and not a fit subject for indiscriminate panegyric,) will best be appreciated by those who were most in his intimacy.

For my own part, as a warm admirer of his genius, though gathering my impression of his sentiments and disposition from public report; without any recollections from personal acquaintance to correct the impressions thus received, but with every disposition to extenuate the foibles of so great a philosopher; I shall sincerely rejoice, if the book now published by his brother, a small part alone of which I have as yet perused, should succeed in its proposed object of elevating the personal reputation of the individual, and thus convince the world that my interpretation of his conduct in this trivial particular has been erroneous.

Still, however, Dr. Davy must excuse me, if, from all that has yet appeared, I persist in regarding his brother's change of opinion in this instance a matter rather of taste than of judgement.

In the memoir on volcanos referred to, Sir Humphry distinctly admits that his previous theory is fully competent to explain all the phænomena*, although he concludes by assigning a preference to the other explanation as recommended by greater simplicity; a sentence which, as his biographer Dr. Paris justly observes (Life, p. 347), must be admitted to be rather equivocal. In his Consolations of a Philosopher he is somewhat more explicit, yet even there the only reason he assigns for preferring the theory of central heat is vague enough, being, as he thinks, "more agreeable to the analogies of things."

Having, therefore, looked in vain in either of these records of his sentiments for any attempt to show in what way "this

* "Assuming the hypothesis of the existence of such alloys of the metals of the earths as may burn into lava in the interior, the whole phænomena may be easily explained from the action of the water of the sea and air on these metals; nor is there any fact, or any of the circumstances which I have mentioned in the preceding part of this paper, which cannot be easily explained, according to that hypothesis."—Memoir on the Phænomena of Volcanos, by Sir H. Davy, Phil. Trans. 1828. [or Phil. Mag. and Annals, N.S. vol. iv. p. 85—94. Edit.]

simpler hypothesis" will account for the chemical phænomena accompanying volcanic action, and Dr. Davy himself not having supplied this desideratum, I cannot view his adoption of it in any other light at present than as a matter of taste on his part. Dr. Davy, indeed, makes his brother say, though I have not yet lighted upon the passage in which this sentiment occurs, that the chemical theory does not rest on sufficient evidence.

This however, although a ground for scepticism as to the truth of the one, would afford no reason for adopting the other; for granting that of two hypotheses both competent to explain the facts, the simpler one ought to be preferred, no competition surely can exist between them, when this can be predicated only of one.

That the chemical theory will enable us to account for the phænomena, has been shown in the memoir which called forth Dr. Davy's animadversions, and since more fully elsewhere*, and is admitted, as has been seen, in the fullest manner by Sir Humphry in the very paper to which allusion is made. Neither do I see the force of the negative evidence which Dr. Davy has produced to impugn it, for he is too conversant with volcanic operations to be ignorant that sulphuretted hydrogen is amongst its commonest products, and is too good a chemist to admit the possibility of substances like potassium or calcium in their unoxidized condition finding their way upwards in the midst of the steam, which always accompanies volcanic ejections†. What, then, becomes of the objection, that if the hypothesis were correct, inflammable gas might probably be detected issuing from the volcano, or that some pure or uncombined alkaline or earthy inflammable basis might be discovered entangled in the lava, when the former is seen to be actually present, and the latter can so little be expected? And, whilst the presence of hydrogen, combined as it naturally would be with the sulphur which we know to exist in such situations, furnishes a striking confirmation of Sir Humphry Davy's original views, neither he, nor any other chemist, has succeeded in accounting for it according to the opposite ones.

The same may be said of the sal ammoniac, the nitrogen, and according to the simplest form of the hypothesis as ex-

* *Encyclop. Metrop.*, art. Geology.

† This objection, at least, cannot have originated with Sir Humphry, but must be the exclusive property of his brother, for in the memoir referred to we find Sir Humphry distinctly asserting, "That the extreme facility of oxidation belonging to these bodies, must prevent them from ever being found in a pure combustible state in the products of volcanic eruptions."

pounded by Cordier, even of the water and the muriatic acid, which are noticed by Davy himself as issuing from the volcano, whose phænomena he describes.

Whatever ground, therefore, may exist for his scepticism on the subject, none certainly has been assigned for his adoption of the rival hypothesis, which, without effecting the object of explaining the facts, is saddled with assumptions equally gratuitous; the existence of the alkaline and earthy bases in the interior of the earth, being not more unsupported by direct evidence, than that of a central fluid mass; seeing that the increasing temperature detected in descending into the bowels of the earth, may be explained quite as well by chemical processes carried on at the requisite depths, as by the hypothesis of central fluidity.

I trust I have now said enough to justify my having stated that Sir Humphry only gave his *ipse dixit* in support of his new hypothesis, a point which I was at that time more particularly anxious to establish, from a wish to obtain for the theory I had advocated an unprejudiced hearing, and being well aware of the weight which the *deliberate* judgement of such an authority as that of Sir H. Davy on a question of science would obtain with most readers. Since that time the favourable opinion expressed by the present as well as by the late President of the Geological Society with respect to the chemical theory, will have secured it a candid reception amongst naturalists; whilst the authority of one of the most distinguished of Sir Humphry Davy's living cotemporaries and rivals in science, Mons. Ampère, will vindicate its claim to respect amongst chemical philosophers.—One more word with respect to the reasonableness of imagining that Davy might choose to abandon his former hypothesis without deliberate consideration.

In the first place, considering the numberless applications of which his great discovery of the alkaline and earthy bases admitted, it is not necessary to suppose that he would regard this one with any peculiar favour. And indeed the only allusion I find to it at all in any of his earlier publications consists of four lines in a note appended to his Memoir on the Decomposition of the Earths.

Secondly, the solid character of the discoveries on which the reputation of Davy was based, would naturally make him indifferent as to the fate of a theory resting on assumptions which, whether probable or not, were such as could themselves neither be substantiated nor set aside by direct experiment.

The higher, indeed, we estimate the fame of Sir H. Davy, the less difficult will it appear to us to account for his abandonment of his original views, and for his preference as a matter of taste for others which were calculated, from their very vagueness, to allow full scope to that imagination, which, as appears from his Consolations of a Philosopher, continued in unimpaired vigour to the last. There is, therefore, no analogy between the motives of his conduct in this case and in the question with respect to the nature of chlorine, in which Sir H. Davy might feel a just pride, as having recalled the scientific world from theory to a simple expression of facts, and thus corrected the logic of chemistry, in quite as great a degree as he extended our knowledge of this particular class of combinations.

It may be readily inferred from these remarks that I regard the chemical theory of volcanos, which it has been my humble endeavour to elucidate and to confirm, chiefly valuable by erecting a standard to which volcanic operations may be compared, and thus encouraging more minute attention to the phænomena they present. This the mere vague and general statement of their originating in central heat is not so likely to do, and hence it may perhaps be regretted, if the preference for a *simpler hypothesis*, or the authority of great names, should so prepossess the minds of men of science as to prevent their entertaining the views I have advocated, and to induce them to dismiss the subject as altogether beyond the reach of probable conjecture *.

It is on this latter ground chiefly that I have chosen to address you, for with respect to that part of the subject which concerns myself I should have been content perhaps to leave the question at issue to the candour of the public, and to the impression which most persons will entertain, that I at least can have no desire to attribute unworthy motives to Sir H. Davy.

Oxford, Feb. 23, 1836.

* In Dr. Thomson's Outlines of Mineralogy, Geology, and Mineral Analysis just published, I find this sentiment expressed, but the only objections stated to the chemical theory are, 1st, The specific gravity of the earth; 2ndly, The nature of the elastic fluids emitted by volcanos. I regret, therefore, that the learned author, who has done me the honour of quoting and commending the work on volcanos I published in 1826, had not also consulted the article on Geology in the *Encyclop. Metrop.*, to which I contributed the portion relating to volcanos, as he would have there seen the first objection fully, and I hope fairly, treated, and the latter shown to be quite in accordance with the theory.

The low specific gravity of the metals of the alkalies appears to operate against the reception of the theory in the minds of many; yet if it can be shown that the bases of those volcanic products which appear upon the surface have collectively a greater specific gravity than the mass resulting from their union with oxygen, I cannot see wherein the force of this objection resides.

LI. *Proceedings of Learned Societies.*

LINNÆAN SOCIETY.

Jan. 19, 1836.—READ, Descriptions of the species of *Lacis* found growing in the River Essequibo, and of the fish called Pacou, which feeds upon these plants. By Robert H. Schomburgh.

Feb. 2.—Read, Observations upon a supposed new species of *Veronica* found in Staffordshire, in a letter to Mr. Sowerby; by Mr. George Luxford.

Also, descriptions of two species of the genus *Pinus* from the Himalaya Alps. By Professor Don, Libr. L.S.

The first of these, which belongs to the group of spruce firs, has been described and figured by Dr. Wallich, in the 3rd volume of his splendid work on Indian plants, under the name of *Pinus Smithiana*, in honour of the late eminent President of the Linnæan Society. It is nearly related to *Pinus orientalis*, a native of Armenia and the western parts of Georgia, and has been cultivated for more than ten years in our gardens, and was at first supposed to belong to the Indian cedar (*Pinus Deodara*). Khutrow, Morinda, and Raga, are the names by which it is known in its native mountains.

The second species belongs to the group of silver firs, and is nearly allied to *Pinus Webbiana*, but is essentially distinguished from it by its longer acutely bidentate leaves, of nearly the same colour on both surfaces, by its shorter and thicker cones, with trapeziform scales, and rounded notched bracteolæ. Dr. Wallich, who had neither seen flowers nor fruit, has doubtfully referred it to *Taxus*, under the specific name of *Lambertiana*, in his Catalogue. Several travellers have noticed the tree, but Mr. Royle appears to be the only one who has been fortunate enough to meet with it in flower and fruit.

The author has noticed a remarkable peculiarity in the seeds of the species belonging to this group, which consists in the rupture or separation at the inner side of the external integument, leaving the nucleus with its inner covering exposed at that part.

The following are the characters of these two species:

Sp. 1. Pinus Smithiana. *Wall.*

P. foliis solitariis compresso-tetragonis rectis subulatis pungentibus, strobilis oblongis cylindraceis: squamis obovato-rotundatis coriaceis rigidis margine lævissimis, antherarum cristâ subrotundâ erosè crenulatâ.

Sp. 2. Pinus Pindrow. *Royle MSS.*

P. foliis bifariàm versis linearibus planis utrinque concoloribus apice bidentatis, antherarum cristâ bicorniculatâ, strobilis ovalibus: squamis trapezoideo-cordatis, bracteolis subrotundis emarginatis erosè crenulatis.

Feb. 16.—Read some observations on the *Nephrodium rigidum*. By Professor Don, Libr. L.S.

For this valuable addition to the British *Filices*, we are indebted to the Rev. W. T. Bree, who discovered it many years ago on Ingleborough, and it has since been published in the Supplement to English Botany. The British specimens accord entirely with foreign ones, and with the accurate figure given by Schkuhr (*Kryptog. Gew.* t. 38.).

The author has proposed the following character of the species:

N. rigidum, fronde lanceolatâ bipinnatâ: pinnulis oblongis pinnatifidis: laciniis argutè dentato-serratis: venulis inconspicuis, soris biseriatim contiguis, indusio scarioso dilatato, stipite rhachique densè paleaceis.

The species ranks next to *dilatatum* and *spinulosum*, but differs from both by its larger and more crowded sori, broader and more depressed indusium, and by the stipes and rhachis being copiously clothed with narrow ramentaceous scales as in *Aspidium aculeatum*. The more delicate fronds, with pinnatifid pinnulæ, having the lobes serrated with sharp-pointed, incurved teeth, essentially distinguish it from *Nephrodium Filix Mas*, between which and *spinulosum* it appears to be intermediate in its habit and characters.

Read also remarks on some varieties of *Erica ciliaris* and *Tetralix*. By Professor Don, Libr. L.S.

The extreme states of these two species are easily recognised at first sight; but it must be admitted that varieties do occur in which the characters of both appear blended. The normal form of *ciliaris* is characterized by its flat, ovate, ternary leaves, elongated axis of its inflorescence, oblong and slightly curved corollas, and naked anthers; and that of *Tetralix* by its quaternary, linear leaves, revolute at the edges, capitate inflorescence, globular corollas, and aristate anthers. Some of the varieties of *ciliaris* exhibited to the meeting, for which the author was indebted to Mr. Hewett C. Watson, had the axis of their inflorescence quite as much depressed as in *Tetralix*, along with the narrow quaternary leaves of that species. Another specimen, clearly referrible to *Tetralix*, had the corolla nearly as long as in *ciliaris*. Another variety of *Tetralix*, lately discovered in Ireland, and which by some botanists is regarded as constituting a distinct species, has entirely the habit of *ciliaris*, but with the depressed inflorescence, globular corollas and aristate anthers, of the former species; and it differs from both in the entire absence of the short pubescence from the upper surface of the leaves.

The only permanent mark by which *ciliaris* and *Tetralix* can be separated is by the absence or presence of the awn-like appendages at the base of the anthers.

A comparison of Irish specimens of *Gypsocallis mediterranea* with others contained in the Smithian Herbarium, shows that they agree in every essential point; and although the two plants when grown together in a garden exhibit a somewhat different aspect, there cannot remain any doubt as to their identity. *G. carnea* is readily distinguished by the much greater length of its anthers and ovarium.

GIBRALTAR SCIENTIFIC SOCIETY.—NEW OBSERVATORY AT CATANIA.

It is truly gratifying to see that activity in scientific pursuits is fast spreading from Britain to her colonies. The institution at Quebec has already distinguished itself by the publication of some able papers on American geology, topography, and statistics; and we now find that a new society has started up at Gibraltar, which, we trust, may prove as stable as the Rock itself, for the gradual changes in the members of so respectable a garrison are ever likely to renew its spirit. We are alluding to "The Gibraltar Scientific Society," of which Dr. Burrow, D.D., F.R.S., is the worthy president; and we hope soon to learn the names of the Council. One of that body, Captain W. H. Shirreff, R.N., and a Fellow of the Royal Astronomical Society of London, possesses a well-situated observatory, mounted with excellent instruments, in the use of which he has long been expert. This gentleman introduced two young officers of great merit to the December meeting, Lieut. Graves and Lieut. Stanley, of the Navy, on which occasion they were elected honorary members, as a mark of consideration for their hydrographical labours in the Archipelago. We look forward to the proceedings of this promising association with much interest.

The respected correspondent to whom we are indebted for the foregoing notice adds the following:

"From a letter from Sig. Cacciatore, of Palermo, I find that the University of Catania are about to build and equip an Observatory, partly at their own expense, and partly at that of the King of Naples. I have been applied to respecting instruments, &c."

LII. *Intelligence and Miscellaneous Articles.*

ON NITRO-BENZIDE AND SULPHO-BENZIDE. BY E. MITSCHERLICH.

Nitro-benzide.—WHEN hot and fuming nitric acid is gradually added to benzine, action ensues, accompanied with the evolution of heat; and a peculiar substance is formed, which remains dissolved in the hot nitric acid; but when cooled it partly separates, and floats on the surface. If the acid is then diluted with water, this product falls to the bottom of the vessel. By washing, and then distilling this substance, it may be obtained perfectly pure, as a yellowish liquid, possessing a very sweet taste and peculiar odour, somewhat between that of the volatile oil of almonds and oil of cinnamon. Its specific gravity is 1·209 at 59° Fahr., it boils at 415·4° Fahr., and distills unchanged. At 37·4° Fahr. it solidifies, affording acicular crystals.

This substance may be distilled unchanged with nitric acid. Diluted sulphuric acid does not act on it; but when the concentrated acid is boiled with it, it is decomposed, with the disengagement of sulphurous acid gas, and the solution becomes highly coloured. When heated with potassium, it detonates so violently as to break the vessel. It is almost insoluble in water; neither æther nor alcohol act on it. The strong acids, such as the nitric and sulphuric, readily dissolve it, better at a high than a low temperature. It is composed of

12 volumes of the vapour of carbon,
10 ——— ——— hydrogen,
2 ——— ——— azote,
4 ——— ——— oxygen.

The specific gravity of the vapour is about 4·4.

1 volume of nitro-benzide is composed of

3 volumes carbon,
$2\frac{1}{2}$ —— hydrogen,
$\frac{1}{2}$ —— azote,
1 —— oxygen.

The formation of nitro-benzide may be explained by supposing that a volume of nitric acid gas combines with a volume of benzine, whilst there separates $\frac{1}{4}$ vol. of hydrogen and $\frac{1}{4}$ vol. of oxygen.

Sulpho-benzide.—If benzine is mixed with anhydrous sulphuric acid it is not decomposed, nor is any sulphurous acid gas liberated; but a thick liquid, very soluble in water, is obtained, which, when diluted with water, affords a crystalline substance equal to about five or six parts for every 100 of benzine employed. This substance is very slightly soluble in water, and may be purified by washing with water. To completely purify it, it may be dissolved in æther, filtered, the solution crystallized, and the crystals distilled. At 212° Fahr. this substance melts, forming a transparent and colourless liquid, and boils at a temperature between the boiling-points of sulphur and mercury. It is inodorous, insoluble in the alkalies; but soluble in the acids, where it separates the water. Heated with sulphuric acid, it forms a particular acid, which forms a soluble combination with barytes. The other acids do not alter it.

It is composed of 12 carbon,
10 hydrogen,
1 sulphur,
2 oxygen.

It thus appears that nitro- and sulpho-benzide are formed by the union of nitric acid and sulphuric acid with benzine, and that during this combination water is separated. It is owing to this circumstance that the union of these substances is so stable as to resist the ordinary methods of separating the acids. M. Mitscherlich, from the analogy of these bodies with the *amides*, has proposed to call them nitro- and sulpho-benzide.—*Journal de Pharmacie, Juin* 1835.

FORMATION OF ÆTHER. BY M. MITSCHERLICH.

The decomposition of alcohol into æther and water is not interesting merely by the production of æther, but is especially so as an example of a particular kind of decomposition, which cannot be so well followed with any other substance, and which is manifested in the formation of some important products, for example, in that of alcohol itself. M. Mitscherlich has endeavoured to elucidate the phænomena of this decomposition by the following experiments: Take a mixture of 100 parts of sulphuric acid, 20 of water, and 50 of anhydrous alcohol, and heat it gradually until its boiling-point becomes 284° Fahrenheit. Alcohol is then allowed to fall gradually into the vessel which contains the mixture, and the current is to be so regulated that the heat of the mixture remains constantly at 284°. If, for example, the operation be conducted with a mixture of six ounces of sulphuric acid, one ounce and one fifth of water, and three of alcohol, and if the density of each two ounces of product as it is obtained be taken, it will be observed that this density passes gradually from 0·780 to 0·788 and 0·798, and afterwards remains constantly at the last-mentioned density, which is exactly that of the alcohol employed. If the operation be properly conducted, an unlimited quantity of alcohol may be converted into æther, provided that the sulphuric acid does not change. The distilled liquor is formed of two distinct fluids; the upper one is æther, containing a little water and alcohol; the lower one is water, with a little alcohol and æther. Its weight is nearly equal to that of the alcohol employed, and it is composed of

Æther	65
Alcohol..........	18
Water	17—100

If into six ounces of concentrated sulphuric acid six ounces of pure alcohol are suffered to flow gradually, a product of constant density is not obtained until the sulphuric acid has taken its proportion of water. Take, on the contrary, three ounces of sulphuric acid and two ounces of water, and let alcohol be added, drop by drop; the first two ounces distilled are merely spirit, if wine of specific gravity 0·926, containing scarcely a trace of æther. The density decreases until the quantity of water of the sulphuric acid is reduced to its proportion, and the product of the distillation has acquired the density of the alcohol.

If concentrated sulphuric acid be added to anhydrous alcohol in excess, pure alcohol distils at first; but when the temperature reaches nearly 260°, the first traces of æther begin to appear; the production of æther is at its maximum between 284° and 302°.

It results, from the preceding observations, that alcohol, when in contact with sulphuric acid, is converted into æther and water at a temperature of about 284°. A great number of analogous decompositions and combinations are known, which may be attributed entirely to the influence of the contact of bodies. The most remarkable example of this kind is that of the conversion of oxygenated water into water and oxygen, by the slightest trace of the peroxide of manganese and some other substances. The decomposition of sugar into alcohol and carbonic acid, the oxidizement of alcohol when it is changed into vinegar, are phænomena of the same kind; and so also is the conversion of starch and sugar by means of sulphuric acid. M. Mitscherlich, observing that in the preparation of carburetted hydrogen by means of sulphuric acid and alcohol water is formed at the same time, attributes this decomposition of alcohol to the influence of mere contact, and not to the affinity of sulphuric acid for water.—*Journal de Pharmacie, Juin* 1835.

ON THE SEPARATION OF BARYTES AND STRONTIA.—BY MR. J. D. SMITH.

The great analogy existing between the salts of barytes and strontia, may render an observation on the difference of solubility in water of their chromates worthy of notice; and the more so, as it adds one to the few methods already devised for the analysis of substances containing both these earths. I had remarked some time before, that when a solution of neutral chromate of potash was added to one of muriate of strontia considerably diluted, no precipitation took place until the mixed solutions were boiled, and even then that a large quantity of strontia was still held in solution; whilst, on the other hand, the action of the neutral chromate of potash on a solution of muriate of barytes was widely different; for let the solution of barytes be ever so largely diluted, yet chromate of potash invariably produced precipitation; so much so that wherever a sulphate was capable of detecting this earth, chromate of potash also indicated its presence. Wishing to examine some minerals supposed to contain both strontia and barytes, it occurred to me that the property possessed by a diluted solution of muriate of strontia of not precipitating with chromate of potash, might be made available for analytical purposes. I therefore made a few experiments to ascertain the fitness of this salt as an agent for separating the salts of these earths when dissolved in a large quantity of water. These experiments at first did not afford very exact results; for the precipi ated chromate always appeared to indicate rather more barytes than was originally taken: but this was found to be owing to the chromate, like the sulphate of barytes, requiring ignition before weighing, to expel a little water which obstinately adheres to it when dried at low temperatures; this error was entirely obviated by heating the chromate to redness previous to weighing it.

The cause of the error being thus ascertained, 20 grs. of carbonate of strontia and 5 grs. of carbonate of barytes were dissolved in dilute muriatic acid; the solution was carefully evaporated to dryness to expel the excess of acid; the dry salt was redissolved in distilled water, and the solution diluted to a pint and a half; to this was added a dilute solution of chromate of potash, made with transparent crystals, in order to prevent the otherwise possible admixture of sulphate or carbonate. After standing for a short time it was filtered, and the chromate of barytes washed, dried, and ignited; weight 6·53 grs. = 5 grs. of carbonate. The solution and washings were then evaporated to reduce the liquor to a smaller compass, and a solution of sesquicarbonate of ammonia added, which precipitated carbonate of strontia; this when collected and dried weighed 19·19 grs.

Another experiment, in which the quantity of barytes exceeded that of the strontia, was conducted in a similar manner, with the exception of the employment of less water ($\frac{3}{4}$ pint) to dissolve the dry salt before the addition of the chromate of potash. In this case there were obtained from 12 grs. of carbonate of barytes, and 8 grs. of carbonate of strontia, 15·8 grs. of chromate = 12·09 grs. of carbonate of barytes, and 7·26 grs. of carbonate of strontia.

In both the above experiments it will be remarked that there is less carbonate of strontia obtained than was originally taken: this is owing to strontia not being entirely precipitated by a solution of sesquicarbonate of ammonia; for when this salt and muriate of strontia are mixed, the former being in excess, the filtered liquor will become slightly turbid on the addition of oxalate of ammonia, and stirring the solution.

In the following experiment, in which 10 grs. of each carbonate were taken, and oxalate substituted for sesquicarbonate of ammonia, the results were chromate of barytes 13·04 grs. = 10 grs. of carbonate, and 11·9 grs. of oxalate = 10 grs. of carbonate of strontia; thus showing the superiority of oxalate of ammonia as a precipitant for strontia: the only precaution necessary is to have the solution neutral.

Note. Pyroxylic spirit produces a more intense crimson flame with a small quantity of muriate of strontia than alcohol does, and consequently is of greater service as a test for recognising strontia when occurring in minute quantity.

St. Thomas's Hospital,
Feb. 1836.

COMPOSITION OF CARBONATE OF ZINC.—BY MR. J. D. SMITH.

When solutions of sesquicarbonate of ammonia and sulphate of zinc are mixed, a white, bulky, and gelatinous precipitate is produced; this after repeated washings with hot water, by which carbonic acid gas is plentifully evolved, falls as a white powder. 80 grs. of this powder lost by ignition 22·5 grs., and 80 grs. dissolved in a counterpoised bottle of dilute sulphuric acid, lost 12·5 grs. of carbonic acid. From these experiments it appears that 80 grs. of this powder are composed of 57·5 grs. oxide of zinc, 12·5 grs. of carbonic acid, and 10 grs. of water; which numbers indicate a compound of $2\frac{1}{2}$ eqs. of oxide of zinc, 2 eqs. of water, and 1 eq. of carbonic acid; which may be viewed either as a $\frac{1}{4}$ carbonate of zinc with 4 eqs. of water, or as 1 eq. of hydrated subsesquicarbonate of zinc united to 1 eq. of hydrate of zinc. Its equivalent number being in former case 280, in the latter 140,

or $2\frac{1}{2}$ eqs. of oxide of zinc	100
1 eq. of carbonic acid	22
2 eqs. of water......	18—140.

St. Thomas's Hospital,
Feb. 1836.

ON *RIOLITE*, A SUPPOSED BISELENIURET OF ZINC, AND *HERREBITE*, SUPPOSED TO BE CARBONATE OF TELLURIUM.—BY PROFESSOR DEL RIO.

An account is given by Mr. Del Rio, in vol. iv. of the Phil. Mag. and Ann., p. 113, of two new minerals found in Mexico; one supposed to be biseleniuret of zinc and sulphuret of mercury, which in honour of Mr. Del Rio I have named Riolite; the other, considered a biseleniuret of zinc and bisulphuret of mercury, I have named Culebrite, from the place in which it occurs.

By the last mail I have received the following letter from Mr. Del Rio relative to the first of these substances, and to another mineral, supposed to be carbonate of tellurium, which I shall be obliged to the editors of the Phil. Mag. and Journal of Science to insert in that journal.

Feb. 13, 1836. H. J. Brooke.

"Dear Sir, "Mexico, November 14, 1835.

"I have again examined the mineral you have been so kind as to name *Riolite*, and have found it to be not a seleniuret of zinc, but a *native selenium* ore with a variable mixture of *sulphoseleniuret* of mercury, and *seleniurets* of cadmium and iron.

I put in a retort $53\frac{1}{2}$ grs. which I washed to separate the carbonate of lime: as some particles were attached to the sides of the retort, I washed it down with some water, and at the moment many round little lumps of selenium arose to the surface, which was covered with a film of the same, proving that it was not combined. There were sublimed by the distillation 38 grs. of selenium and $1\frac{1}{2}$ of mercury, which was also amalgamated with selenium; and there remained in the retort 10 grs. of a yellow and grey powder.

1. I treated the 10 grs. with muriatic acid, which dissolved the iron and the cadmium, and the selenium was precipitated as a black powder, which amounted to $\frac{1}{4}$ gr.

2. I precipitated the diluted solution (1.) with a small bar of zinc: the grey and voluminous cadmium was easy to be distinguished from the iron: both amounted to $1\frac{1}{4}$ gr.

3. The iron was dissolved in diluted muriatic acid, and the cadmium in concentrated; and the last was precipitated again with a bar of zinc in a crucible of platinum, to which some cadmium was attached as a silver white metal, and some was precipitated as a dark grey powder, which deposited upon the charcoal at the blowpipe a reddish brown ring.

4. Together with the black powder (1.) I observed another precipitate lighter in colour and heavier, which I separated by washing, and treated with hydrochloromuriatic acid. All was dissolved immediately, and red selenium arose to the surface, which was reduced to selenious acid by the addition of nitric acid: there remained only a melted globule of sulphur. At the same time some sulphate of lime was precipitated, which amounted to $1\frac{1}{4}$ gr.

5. I precipitated the last solution with hydrosulphate of ammonia, and I obtained $6\frac{1}{2}$ grs. of sulphuret of mercury.

6. The selenium (1.) was put in a little capsule over the lamp in a dark corner of my room with some sulphuric acid, and gave many small flashes of light on the surface of the liquid of the selenious acid which sublimed; I smelled some sulphurous acid, and the decanted solution precipitated red selenium with water; the remaining solution precipitated the same without water; and there was at the bottom selenious acid as a white powder.

As the quantities of mercury and cadmium are variable, because I found more in other specimens, I think this mineral is nothing else than a mixture to which no formula can properly be applied.

In the beginning, when I thought it was a seleniuret of a fixed base, I treated it twice at the blowpipe with some iron and borax, and after washing the charcoal I found both times the next day very small double rhomboidal, oblique pyramids of seleniuret of iron of a dark grey colour: the iron was dissolved in muriatic acid, and the black selenium was precipitated.

I take profit of this opportunity to let you know that the *Herrerite* is not a carbonate of tellurium, as it was announced by my pupil, but of zinc and nickel, which gives to it the pretty pistachio and grass-green colour: it contains also some cobalt; and the apple-green, fibrous, and very soft substance, which accompanies it, and I supposed erroneously to be a species of the preceding, shows to be at the blowpipe an arseniate of nickel.

As soon as I get some pieces of your *Culebrite*, I will examine it, and give you notice.

ANDREA DEL RIO.

METEOROLOGICAL OBSERVATIONS FOR JANUARY 1836.

REMARKS.

Chiswick.—January 1. Snow. 2. Severe frost: slight snow at night. 3. Thawing: cloudy and fine. 4. Cloudy: stormy. 8. Hazy: fine. 9. Frosty. 10. Stormy, with some snow. 11. Overcast: heavy rain. 12. Sharp frost: clear and calm. 13. Frosty: cloudy. 14. Cloudy and windy. 15. Heavy rain: clear and windy at night. 16, 17. Clear and frosty. 18. Cloudy and cold. 19. Fine. 20. Frosty haze: overcast. 21. Fine: clear. 22. Slight rain. 23. Boisterous. 24—26. Very fine. 27. Fine: stormy at night. 28. Fine: slight rain: windy. 29. Cloudy and cold. 30. Clear and frosty: fine but cold. 31. Rain.

The plan which is followed in regard to the meteorological observations made at the Garden of the Horticultural Society, is in accordance with that recommended by Professor Daniell in his excellent Meteorological Essays. A full account of the instruments employed is given by Mr. Booth in vol. vii., p. 97, of the First Series of the Hort. Soc. Transactions. It will be proper, however, to mention such circumstances as are connected with the abstract which appears in the Phil. Mag. and Journal of Science.

The barometer is situated about fourteen feet above the mean level of the Thames, at Chiswick. The observations are taken at 8 A.M., 1 P.M., and 10 P.M. A correction is made for every observation for the capacity of the cistern, the neutral point of the barometer, and the temperature of the mercury; so that the column of mercury is reduced to that which, at a temperature of 32°, would balance the atmosphere. The thermometers indicating the *max.* and *min.* of temperature are self-registering, and of Rutherford's construction; they are placed in an open space in the Arboretum, and are protected from the rays of the sun by a sort of umbrella of painted canvas. They are attached to the post which supports the umbrella, a little below the level of the margin of the latter, and about four feet from the ground.

The rain-gauge is made according to Mr. Howard's directions, in his work upon the Climate of London. The quantity is registered every morning, when there is any, at 8 A.M. The direction of the wind is noted at 1 P.M.

Boston.—January 1, 2. Cloudy. 3. Cloudy; rain P.M. 4. Cloudy. 5. Fine. 6. Cloudy. 7, 8. Fine. 9. Cloudy. 10. Cloudy: snow P.M. 11, 12. Cloudy. 13. Fine. 14. Fine: snow melted: stormy night, with rain. 15. Fine: rain P.M. 16, 17. Fine. 18. Cloudy: rain P.M. 19. Fine. 20, 21. Cloudy. 22. Cloudy: rain P.M. 23. Stormy. 24. Fine: rain P.M. 25. Foggy. 26. Cloudy. 27. Fine. 28. Fine: rain P.M. 29. Fine: snow A.M. 30. Cloudy and stormy: snow A.M. 31. Snow.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1836. January.	Barometer. London: Roy. Soc. 9 A.M.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ A.M.	Thermometer. London: Roy. Soc. Fahr. 9 A.M.	Thermometer. London: Roy. Soc. Self-registering. Min.	Thermometer. London: Roy. Soc. Self-registering. Max.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ A.M.	Wind. London: Roy. Soc. 9 A.M.	Wind. Chisw. 1 P.M.	Wind. Bost.	Rain. London: Roy. Soc. 9 A.M.	Rain. Chisw.	Rain. Boston.	Dew-point. Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
F. 1.	30·184	30·431	30·363	30·04	32·6	31·4	34·6	30	13	32	ESE.	E.	SE.	...	...	...	30
S. 2.	30·493	30·733	30·672	30·30	19·6	17·5	33·9	30	27	24	E.	S.	calm	...	·06	...	12
○ ⊙ 3.	30·311	30·553	30·410	30·12	35·8	19·2	43·4	44	37	36	SSW.	SW.	calm	...	·02	...	29
M. 4.	30·045	30·283	30·241	29·73	45·2	34·2	47·2	51	45	47	SW.	SW.	W.	·063	...	·07	35
T. 5.	30·071	30·271	30·263	29·77	46·3	43·6	48·8	52	42	47	SW.	SW.	W.	...	...	...	40
W. 6.	30·075	30·272	30·137	29·79	46·3	44·9	46·6	47	31	46	SW.	S.	calm	...	...	...	42
Th. 7.	29·780	29·985	29·948	29·54	38·2	34·9	40·2	41	37	37·5	SE.	S.	SE.	·030	...	...	33
F. 8.	29·823	30·167	30·004	29·61	41·4	37·3	41·3	42	29	39	ESE.	SE.	calm	...	...	...	37
S. 9.	29·831	30·025	29·895	29·70	31·5	30·3	...	35	31	34	E.	NE.	calm	...	...	...	30
⊙ 10.	29·465	29·680	29·229	29·43	32·2	...	...	34	29	33	E.	E.	E.	...	...	...	29
☾ M. 11.	29·126	29·343	29·233	28·92	33·3	...	43·7	38	28	32	SW.	S.	calm	...	·83	...	30
T. 12.	29·277	29·700	29·488	29·04	30·7	28·6	35·2	38	27	31	SW.	S.	calm	·625	...	...	26
W. 13.	29·683	29·976	29·831	29·47	30·7	28·0	43·4	41	34	31	SW.	S.	calm	...	·01	...	27
Th. 14.	29·752	29·971	29·698	29·46	43·7	29·0	48·4	49	41	41	S.	SW.	W.	...	·27	·20	35
F. 15.	29·314	29·854	29·505	29·03	43·2	42·6	44·2	45	31	40	W.	W.	calm	·280	·01	·28	41
S. 16.	29·982	30·306	30·164	29·73	35·6	33·3	37·3	41	28	32	N.	NE.	NW.	·041	...	·05	31
⊙ 17.	30·095	30·330	30·427	29·86	33·2	30·0	40·3	44	29	33·5	SW.	W.	calm	...	...	...	27
● M. 18.	30·075	30·295	30·031	28·80	36·7	31·0	44·2	45	33	37	SW.	W.	SW.	...	...	...	29
T. 19.	30·093	30·389	30·288	29·93	35·8	35·2	37·7	41	25	33	N.	NE.	calm	...	...	·10	30
W. 20.	30·115	30·328	30·140	29·88	38·2	31·3	41·5	43	35	36	SSW.	S.	calm	...	...	...	32
Th. 21.	29·709	29·936	29·694	29·48	37·3	35·5	44·0	44	39	40	ESE.	S.	calm	...	·01	...	31
F. 22.	29·386	29·669	29·574	29·17	44·6	36·3	52·3	53	45	42	S. var.	S.	SE.	...	·03	...	39
S. 23.	29·297	29·927	29·463	28·80	52·2	43·7	53·4	55	44	53·5	SW. var.	S.	W.	...	...	·02	44
⊙ 24.	29·881	30·205	30·061	29·55	44·6	42·8	48·6	51	38	41	S.	SW.	W.	...	...	...	41
☽ M. 25.	30·203	30·409	30·385	29·90	42·8	41·3	44·4	47	33	37	S.	S.	calm	0·33	...	·13	40
T. 26.	30·085	30·273	30·137	29·68	42·4	38·6	45·3	50	39	41·5	S.	SW.	calm	...	...	...	39
W. 27.	29·909	30·099	29·907	29·54	44·3	39·0	46·3	47	41	37	SW.	SW.	calm	...	·02	...	38
Th. 28.	29·623	29·811	29·476	29·30	44·3	41·2	48·8	50	35	41	SW.	SW.	W.	...	·05	...	39
F. 29.	29·346	29·555	28·978	29·07	38·0	35·2	42·5	43	33	35	SW.	W.	W.	·158	·32	·15	35
S. 30.	28·922	29·642	29·099	28·62	36·2	32·5	43·2	45	31	34	SW.	W.	W.	·319	·06	...	31
⊙ 31	29·267	29·523	29·324	29·	39·0	33·3	48·2	50	38	34	S. var.	SW.	SE.	·050	·32	·11	34
	29·781	30·733	28·978	29·49	38·6	34·5	43·8	55	13	37·3				Sum 1·599	1·79	1·11	33·4

THE

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[THIRD SERIES.]

APRIL 1836.

LIII. *Observations upon the Habits of the* Plecotus auritus, *or Long-eared Bat.* *By* J. DE C. SOWERBY, *Esq., F.L.S.**

ABOUT the beginning of August last, a living specimen of the Long-eared Bat was given to my children. We constructed a cage for him by covering a box with gauze and making a round hole in the side fitted with a phial cork. When he was awake we fed him with flies introduced through this hole, and thus kept him for several weeks. The animal soon became familiar, and immediately a fly was presented alive at the hole he would run or fly from any part of the cage and seize it in our fingers, but a dead or quiet fly he never would touch. At other times dozens of flies and grasshoppers have been left in his cage, and waking him by their noise, he dexterously caught them as they hopped or flew about, but uniformly disregarded them while they were at rest. The common Blatta, hard Beetles, and Caterpillar he refused, even after he had been induced by their moving to attack them. As we became still more familiar our new friend was invited to join in our evening amusements, to which he contributed his full share by flitting round the room, at times settling upon our persons and permitting us to handle and caress him. He announced his being awake by a shrill chirp,

* Read at the first Philosophical Meeting of the Camden Literary and Scientific Institution, January 28, 1836: and now communicated by the Author.

which was much more acute than that of the Cricket. Now was the proper time for feeding him. I before stated that he only took his food alive: it was also observed that not only was motion necessary, but that generally some noise on the part of the fly was required to induce him to accept it; and this fact was soon discovered by the children, who were entertained by his taking flies from their fingers as he flew by them, before he was bold enough to settle upon their hands to devour his victims. They quickly improved upon their discovery, and by imitating the booming of a bee, induced the bat, deceived by the sound, to settle upon their faces, wrapping his wings round their lips and searching for the expected fly. We observed that if he took a fly while on the wing, he frequently settled to masticate it; and when he had been flying about a long time he would rest upon a curtain, pricking his ears and turning his head in all directions, when if a fly were made to buzz, or the sound imitated, he would proceed directly to the spot, even on the opposite side of the room, guided, it should appear, entirely by the ear. Sometimes he took his victim in his mouth, even though it was not flying; at other times he inclosed it in his wings, with which he formed a kind of bag-net; this was his general plan when in his cage, or when the fly was held in our fingers or between our lips.

From these observations I should conclude that many of the movements of the Bat upon the wing are directed by his exquisite sense of hearing. May not the sensibility of this organ be naturally greater in those animals whose organs of vision are too susceptible to bear daylight, when those organs, from their nature, would necessarily be of most service? such as the cat, who hunts much by the ear, and the mole, who feeding in the dark recesses of his subterranean abode is very sensible of the approach of danger and expert in avoiding it. In the latter case large external ears are not required, because sound is well conveyed by solids and along narrow cavities. In the cases of many bats and of owls the external ears are remarkably developed. Cats combine a quickness of sight with acute hearing; they hunt by the ear, but they follow their prey by the eye. Some bats are said to feed upon fruits; have they the same delicacy of hearing, feeling, &c. as others?

LIV. *Observations on the frequent Presence of Lead in English Chemical Preparations; on the Cause of that Presence; and other Remarks relative thereto.* By GUSTAVUS SCHWEITZER*.

THE examination of the purity of chemical preparations, in which I have been engaged for some time, convinces me that many of them are impure and contain lead. In several which I have examined I have found subcarbonate of magnesia containing lead in the proportion of 2·40 grains subcarbonate of lead in 1000 grains of subcarbonate of magnesia. Bicarbonate of potash contained a similar proportion; bicarbonate of soda, subcarbonate of ammonia, &c. showed the same impurity. It is clear, when these substances, so universally used, contain lead, that many other combinations which are prepared from them must be equally impure. The cause of this impurity arises greatly from the manner in which these substances are prepared. Leaden vessels are too often used for the crystallization and precipitation of them, and how easily alkaline substances act on lead is too well known to need comment. But another cause of this impurity, although the portion present is but very small, is the white glass used in this country, which must be an object of great consequence to practical chemists and druggists. I know not whether any direct experiments have been made to show what influence alkalies, acids, and salts may have on white glass. I have therefore endeavoured to ascertain this point by the following experiments. White glass bottles, such as are used for medicine, were taken and filled, some with distilled water and others with common water. No lead was imparted to the water in either case, even after immersion in it for a few weeks exposed to a common temperature; but when the distilled water was impregnated with carbonic acid gas, after a few days the fluid gave, with the proper tests, ample proof of the existence of lead; and when boiled to expel the gas, no indication of lead was obtained, proving that a bicarbonate of lead was formed by the action of the carbonic acid gas on the glass. Acetic acid, nitric acid, muriatic acid also take up lead from white glass. Diluted sulphuric acid, after standing some time in these glasses, shows no indication of dissolved lead, but after pouring off the acid and rinsing the bottle with nitric acid the presence of lead was detected. Neutral salts showed an equal action when they contained such acids as produce with oxide of lead insoluble combinations, or com-

* Communicated by the Author.

binations of very sparing solubility, and produced more or less a film on the glass, which film was dissolved by nitric acid;—as the phosphates, oxalates, chromates, sulphates. Chloride of lead is but slightly soluble in pure water, and according to my analysis 100 parts of distilled water will dissolve 0·74 part of chloride of lead. Solutious of chlorides will also dissolve chloride of lead, more or less, according to their strength, but still less than distilled water, because when to a concentrated solution of chloride of lead in distilled water a few drops of chloride of calcium of 0·2 strength are added, the greater part of the chloride of lead will be separated, but by chloride of calcium in excess the chloride of lead will be retaken up. (Bischof, *Neues Jahresb. d. Chemie und Physick.*) This I found to occur with the chlorides of ammonium, iron, lithium, magnesium, potassium, sodium, and zinc, and most likely will be proved to be the case with all chlorides of a corresponding strength. Therefore chloride of lead will be imparted to a solution of a chloride when kept in white glass bottles according to the strength of the solution of the chloride; the more chloride the solution may contain the less will be taken up of the chloride of lead. The chlorides will take up by boiling a considerable quantity of chloride of lead, a portion of which will crystallize when the fluid is cooled down.

Caustic alkalies act very powerfully on white glass, and much oxide of lead will be dissolved. Caustic ammonia acts very slightly on the glass; subcarbonate of potash, soda, and ammonia also take up lead, but considerably less than the caustic alkalies. A strong solution of the subcarbonates will take up less than a diluted one. Volatile oils show no action on the glass. These experiments prove that the white glass bottles commonly used are not fit for chemical and medical purposes; which fact is worthy of the attention of the Medical Board. The great addition of oxide of lead in the manufacture of glass to make it more fusible must be avoided. According to the analysis of Faraday, the ordinary flint glass contains 33·28 per cent. of oxide of lead, whereas for all chemical or medical purposes a glass free from lead should be used.

A piece of lead perfectly clean and bright on the surface was kept in distilled water in a closed vessel, and after some time showed a white crystalline coating of subcarbonate of lead; the fluid was also filled with little crystalline scales. The fluid turned red litmus-paper blue, and tests indicated freely the presence of lead in the fluid; but when it was carefully filtered through paper which had been freed by weak nitric acid from its impurity, no indication of lead whatever was perceived, showing that the carbonate of lead was merely dispersed in the water and not dissolved. A similar effect was shown by oxide of lead treated with pure water, but no solution of it was perceptible if it was kept with the water, whether in an open or a closed vessel;—a fact which is opposed to the received opinions*. Well-water and mineral water corrode lead, forming a coating of oxide of lead on the metal without taking up a particle of the oxide; but mineral waters strongly impregnated with carbonic acid gas I found to contain faint traces of lead, when they had been for some time in contact with it. Mr. Walker according to his analysis found in the mineral water of Bath, lead originating from the pipes or pump used for the conveying of the water. (Quarterly Journal of Science, Literature, and Art, January to March, 1829.) Might not the lead in these instances be dispersed mechanically in the water? The result of my experiments induces me to believe so.

Volatile oil dissolves lead freely. Alcohol and æther, when pure, do not act on that metal. When an alkaline fluid contains a trace of lead, the best test to apply is the hydrosulphuret of ammonia, as this reagent will detect $\frac{1}{300,000}$ gr. of crystallized acetate of lead; but this is almost the limit of its dilution, as the observation must be made by the light falling upon the surface of the liquid, which must have a diameter of not much less than one inch. In a neutral fluid, or in one which is only slightly acid, the presence of lead may be shown by the application of sulphuretted hydrogen gas; but it is advisable to avoid the use of nitric acid, as by a little surplus of it faint traces of lead will be easily overlooked. Acetic acid is preferable because its surplus does not affect the delicacy of the hydrosulphuretted gas. Very good tests also are soluble sulphates and chromates, particularly to decide on the nature of the metal, although not to such an extent as the tests before mentioned. Chromate of potash will indicate traces of lead, when sulphate of soda ceases to do so. Sulphate of lead will be partly dissolved by concentrated nitric acid; muriatic acid shows traces of lead, acetic acid only faintly shows them. Chromate of lead when treated with strong sulphuric acid will be changed into sulphate of lead, and the decanted acid will contain no lead. Nitric acid dissolves traces of lead from the chromate; muriatic acid changes the chromate of lead into chloride of lead and the chromic acid into oxide of chrome by developing chlorine, particularly by the application of heat. Acetic

* *Handbuch der theoretischen Chemie von Leopold Gmelin*, 1 Band, 2 Abth. p. 1073. [See on this subject Capt. Yorke's paper in Lond. and Edinb. Phil. Mag., vol. v. p. 81.—Edit.]

acid acted on chromate of lead and took up some lead, particularly when the acid was for several days in contact with it: according to Mans, (Poggendorff's *Annalen*, band ix. p. 127.) it is not soluble in acetic acid.

Royal German Spa, Brighton, November 29, 1835. GUSTAVUS SCHWEITZER.

LV. *Further Researches in the Undulatory Theory of Light.* *By* JOHN TOVEY, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

OBSERVING that my paper on the relation between the length and velocity of a wave of light, inserted in your Number for January last, has received, in your Number for February, a favourable notice from your eminently scientific correspondent Professor Powell, I venture to send you a continuation of my researches.

My object now is to transform the general equations (3.) of that paper, into others adapted to any case of undulation in which the directions of the coordinates can be so taken that the displacements ξ, η, ζ, may be regarded as functions of x and t.

On the condition just stated, we have, by Taylor's formula,

$$\Delta \xi = \frac{d\xi}{dx} \Delta x + \frac{d^2 \xi}{dx^2} \cdot \frac{\Delta x^2}{2} + \frac{d^3 \xi}{dx^3} \cdot \frac{\Delta x^3}{2.3} + \frac{d^4 \xi}{dx^4} \cdot \frac{\Delta x^4}{2.3.4} + \&c.$$

$$\Delta \eta = \frac{d\eta}{dx} \Delta x + \frac{d^2 \eta}{dx^2} \cdot \frac{\Delta x^2}{2} + \frac{d^3 \eta}{dx^3} \cdot \frac{\Delta x^3}{2.3} + \frac{d^4 \eta}{dx^4} \cdot \frac{\Delta x^4}{2.3.4} + \&c.$$

$$\Delta \zeta = \frac{d\zeta}{dx} \Delta x + \frac{d^2 \zeta}{dx^2} \cdot \frac{\Delta x^2}{2} + \frac{d^3 \zeta}{dx^3} \cdot \frac{\Delta x^3}{2.3} + \frac{d^4 \zeta}{dx^4} \cdot \frac{\Delta x^4}{2.3.4} + \&c.$$

Now, suppose the arrangement of the molecules in the state of equilibrium to be such that for every molecule on one side of m, within the sphere of its influence, there is another at an equal distance on the opposite side; then, if we substitute these expressions for $\Delta \xi$, $\Delta \eta$, $\Delta \zeta$, in the first of the equations (3.), the sums $\Sigma . \varphi(r) \Delta x$, $\Sigma . \varphi(r) \Delta x^3$, $\Sigma . \psi(r) \Delta x^3$, $\Sigma . \psi(r) \Delta x^5$, $\Sigma . \psi(r) \Delta y \Delta x^2$, $\Sigma . \psi(r) \Delta y \Delta x^4$, $\Sigma . \psi(r) \Delta z \Delta x^3$, $\Sigma . \psi(r) \Delta z \Delta x^4$, &c., in which the degrees of the products of the variations are odd, will vanish; because, whatever be the signs of Δx, Δy, Δz for any molecule, these signs will all be changed for the corresponding molecule on the opposite side of m; consequently the signs of the odd products of the variations will be changed, while the absolute values of the variations and of their products remain the same for both molecules. This supposition respecting the arrangement of the molecules is due to M. Cauchy, and appears very probable; because it seems impossible to conceive how the equilibrium could subsist unless it were true. It is also probable that the sphere of the influence of each molecule comprehends a great number of other molecules; and accordingly we shall assume this as an hypothesis. Now, as we cannot suppose the molecules, in their state of equilibrium, to be more crowded in one part of the sphere than another, it follows that the terms of the other sums $\Sigma . \psi(r) \Delta y \Delta x^3$, $\Sigma . \psi(r) \Delta y \Delta x^5$, $\Sigma . \psi(r) \Delta z \Delta x^3$, $\Sigma . \psi(r) \Delta z \Delta x^5$, &c., in which there are odd powers of the variations, will be about half of them positive and half negative, and will nearly destroy each other, and consequently these sums will nearly vanish. Neglecting them as well as the former, the first of the equations (3.) becomes, by the substitution,

$$\frac{d^2 \xi}{d t^2} = m \Sigma . \left\{ \left(\phi(r) + \psi(r) \Delta x^2 \right) . \left(\frac{d^2 \xi}{d x^2} . \frac{\Delta x^2}{2} + \frac{d^4 \xi}{d x^4} . \frac{\Delta x^4}{2.3.4} + \&c. \right) \right\}$$

The second and third of the equations (3.) are of the same form as the first; consequently, if we transform them in the same manner, and, for the sake of abridgement, put

$$\begin{array}{ll}
\frac{m}{2} \Sigma . (\phi(r) + \psi(r) \Delta x^2) \Delta x^2 = s^2 & \\
\frac{m}{2.3.4} \Sigma . (\phi(r) + \psi(r) \Delta x^2) \Delta x^4 = s'^2 & \\
\quad \&c. \qquad\qquad \&c. & \\
\frac{m}{2} \Sigma . (\phi(r) + \psi(r) \Delta y^2) \Delta x^2 = s_{\prime}^2 & \quad (1.) \\
\frac{m}{2.3.4} \Sigma . (\phi(r) + \psi(r) \Delta y^2) \Delta x^4 = s_{\prime}'^2 & \\
\quad \&c. \qquad\qquad \&c. & \\
\frac{m}{2} \Sigma . (\phi(r) + \psi(r) \Delta z^2) \Delta x^2 = s_{\prime\prime}^2 & \\
\frac{m}{2.3.4} \Sigma . (\phi(r) + \psi(r) \Delta z^2) \Delta x^4 = s_{\prime\prime}'^2 & \\
\quad \&c. \qquad\qquad \&c. &
\end{array}$$

we shall have

$$\frac{d^2\xi}{dt^2} = s^2 \cdot \frac{d^2\xi}{dx^2} + s'^2 \cdot \frac{d^4\xi}{dx^4} + \&c.$$

$$\frac{d^2\eta}{dt^2} = s_{\prime}^2 \cdot \frac{d^2\eta}{dx^2} + s_{\prime}'^2 \frac{d^4\eta}{dx^4} + \&c. \qquad (2.)$$

$$\frac{d^2\zeta}{dt^2} = s_{\prime\prime}^2 \cdot \frac{d^2\zeta}{dx^2} + s_{\prime\prime}'^2 \frac{d^4\zeta}{dx^4} + \&c.$$

These equations show that the displacements in the three rectangular directions are, to the extent to which we have carried the approximation, independent of each other.

We have supposed the masses of the molecules to be all equal; but if the medium be composed of two fluids uniformly mixed, and if the masses of the molecules of one fluid be all equal to m, and of the other all equal to m', the equations (2.), which we have just obtained, will still be of the same form; because each of the sums Σ may then be divided into two parts, one of which parts, multiplied by m, will embrace the molecules of one of the fluids, while the other part multiplied by m', will comprehend the molecules of the other fluid; and the molecules of each fluid may be conceived to be arranged in the manner which we have supposed. In this way the equations may be extended to the case of any compound medium in which the elementary media are uniformly mingled.

In another communication I propose to deduce the integrals of the equations (2.), and point out the extent of their application. I am, Gentlemen, yours, &c.

Evesham, Feb. 9, 1836. JOHN TOVEY.

P.S. There are three typographical errors in my last paper. At page 9, line 12, for x read Δx; page 10, line 3, for dz^4 read Δz^4; and line 22, same page, for s'^2 read $\frac{s'^2}{3.4}$.

LVI. *An Abstract of a Memoir on Physical Geology; with a further Exposition of certain Points connected with the Subject. By* W. HOPKINS, *Esq., M.A., F.G.S., of St. Peter's College, Cambridge.*

[Continued from p. 236.]

II. HAVING now reduced the determination of the horizontal directions of the fissures produced in the elevated mass to that of the fissure which would be produced in a plane lamina every point of which is subjected to known tensions, we may proceed with this latter problem. Our first object is *to determine the direction in which the tensions have* *the greatest tendency to cause a fissure to begin at any proposed point.* To give all requisite generality to the investigation, let us suppose there to be any number of these tensions, and let F, f_1, f_2, &c. denote their respective intensities at any proposed point, β_1, β_2, &c. the angles which their directions make with that of F;

$$\mu_1 = \frac{f_1}{F}, \ \mu_2 = \frac{f_2}{F}, \text{ \&c.};$$

$$\Sigma\mu \cos 2\beta = \mu_1 \cos 2\beta_1 + \mu_2 \cos 2\beta_2 +, \text{ \&c.}$$

ψ the angle which the required direction makes with that of F; we shall have for the determination of ψ,

$$\cot^2 \psi + \frac{1 + \Sigma\mu \cos 2\beta}{\Sigma\mu \sin 2\beta} \cot \psi - 1 = 0.\text{*}$$

This equation will determine the direction in which the tensions have the greatest tendency to cause a fissure to *begin* at any assigned point, but when its formation has begun, it is obvious that the state of tension in its immediate vicinity must be altered, and that the tensions thus modified may not have a tendency to *continue* the fissure in the same direction as that in which it was the tendency of the original tensions to make it begin. I have shown†, however, that with our hypothesis as to the mode of action of the elevatory force (see p. 234) the above equation will be very approximately applicable to the action of these modified as well as to that of the original tensions.

The actual direction in which the fissure will be formed will not in all cases depend solely upon this tendency of the tensions, but partly also on the constitution of the elevated mass. If, however, its cohesive power be perfectly uniform, it is manifest that this direction will be determined by the tensions alone, or will coincide with that given by the above equation. It will appear also that this is equally true in certain other cases; when it is not so, the effect of any peculiar constitution of the elevated mass must be investigated. I shall now proceed with these points.

Let us still confine our attention to a simple lamina of uniform thickness. Its cohesive power at any proposed point may be estimated in exactly the same manner as the intensity of the tension at that point‡. Let the point of the lamina be designated by P, and draw through it, in any direction in the plane of the lamina, a straight line whose length is unity. Then conceive two equal and opposite forces (f, f) acting

* See Memoir, p. 18. † Memoir, pp. 20, 21. ‡ Memoir, p. 13.

uniformly along this line perpendicular to it, and in the plane of the lamina, on the contiguous particles situated respectively on opposite sides of the line, thus tending to form a fissure along it. The cohesive power opposes this tendency, and if it be uniform along the line just mentioned it will be measured by that value of f which is just sufficient to overcome it. If the cohesive power along this line be variable, f will manifestly not be a measure of it with reference to the single point P. In such case we must conceive the cohesive power to be equal (for the unit of length) at every point of the line to that at P, and then that value of f (which we may designate by Π) which would, under such circumstances, just overcome the cohesive power, may be taken as a measure of it *at the point* P, when estimated in the direction perpendicular to the above line through that point.

In the first place let us suppose the value of Π the same for every direction of this line; then is it manifest that the direction in which a fissure may be formed immediately at the point P cannot be determined in any degree by the cohesive power, since its value is the same for every direction through P. The same conclusion will clearly apply to every point where the value of Π is independent of angular direction, and equally so whether Π be the same or different for different points, *i.e.* whether the cohesive power be uniform or variable, so long as its variation depends solely on the *position* of the point P; or, in mathematical language, the above conclusion will hold whenever Π is a function only of the coordinates of P. In such case then, the fissure will be formed through P in that direction in which the tensions there have the greatest tendency to form it, and our equation will be as strictly applicable for the determination of this direction as if the lamina were perfectly homogeneous. We shall be able shortly to extend still further the conditions under which this equation will be similarly applicable.

It is easy to extend the above reasoning from a lamina to the general elevated mass.

If, however, the value of Π be different for different angular positions of our line of a unit of length through P, (as, for instance, when a laminated or jointed structure prevails in the mass, or any accidental line of less resistance passes through the proposed point,) it is manifest that the direction of the fissure there will depend on the tensions and this variable value of Π conjointly, and the equation above given will no longer suffice generally for its determination. The case of laminated or jointed masses I professedly exclude from these investigations, since their lines of dislocation will necessarily be principally determined by their peculiar structure, and will therefore be in great measure independent of the causes whose effects I am investigating. The case however of the existence of partial and irregular lines of less resistance, regarded as modifying, and not as principal causes, comes within the sphere of our investigations. We may now proceed to this point.

Recurring again to the simple case of a lamina, it is easily shown* that if a fissure in its continuous propagation through consecutive points meet a line of less resistance, it will be propagated across it without change of direction, or along it, according as a certain condition is or is not satisfied, this condition depending on the angle at which the fissure meets the line of less resistance, and the cohesive power along that line estimated in a direction perpendicular to it. If this angle be a right angle the condition is necessarily satisfied, as it must be also if the angle do not deviate much from a right angle, unless the cohesive power just mentioned be extremely small, so that in such cases the line of less resistance will have no effect on the direction of the fissure. If the angle just mentioned deviate too much from a right angle, the fissure will be propagated along the line of less resistance; but I have shown† that when this ceases to be the case it will almost immediately resume the direction determined by our equation, so that if these lines of less resistance exist only partially and irregularly, and be of limited extent, they will only produce partial deviations in the direction of the fissure, without very materially affecting its general bearing. This reasoning again is easily extended to the general mass.

We shall now be able to arrive (as intimated above) at another and important condition respecting the constitution of the elevated mass, with which our equation will be strictly applicable to determine the direction of a fissure. If a single tension act at a point of a lamina, it is easily shown‡ (and in fact is in itself sufficiently obvious,) that the resulting fissure will be perpendicular to the direction of the tension, the cohesive power being such as above shown (p. 274) to be consistent with the strict application of our equation. In like manner it may be easily conceived, that since all the tensions act in the planes of their respective laminæ, whatever their horizontal directions may be, the resulting fissure, whatever may be its horizontal direction, must necessarily (independently of perturbing causes,) lie in a plane perpendicular to each lamina at the points where it intersects it. Hence, then, it

* Memoir, p. 24. † Memoir, p. 23. ‡ Memoir, p. 14.

follows that however small the cohesion may be between two successive laminæ or strata, this will produce no effect on the position of the fissure. In such case then its horizontal direction will still be accurately determined by our equation. This is important, because in a stratified mass the cohesion between different beds must probably be often much less than that between the constituent particles of each bed. The same conclusion will hold with respect to any accidental planes of less resistance which do not deviate too much from horizontality; but if they be vertical, or nearly so, they will produce the accidental and partial deviations which have already been noticed.

In forming a judgement of the probable extent of these planes of less resistance, we must be careful not to be too much influenced by the impressions produced by the examination of a disturbed district, since we are now speaking of the existence of these planes in the *undisturbed* mass. I would also observe, that we are only concerned with this kind of discontinuity in the cohesive power, so far as it depends on local and irregular, and not on general causes, since, as already stated, I exclude those cases in which any regularly jointed or laminated structure may be supposed to have existed in the mass previously to its elevation. Now as far as the planes we are speaking of might be caused by accidental circumstances in the constitution or deposition of the mass, it would seem necessary to suppose them irregular in position and partial in extent; in which case, as we have seen, partial deviations only would be produced by them in the vertical or horizontal directions of the fissure.

It appears then from what has preceded, that the equation above given will accurately determine the direction of a fissure at any proposed point, produced by tensions such as we have supposed, not only in a homogeneous mass, but also in a mass in which there may be any number of planes of less resistance, provided they do not deviate too much from horizontality, and notwithstanding any variation in the cohesive power of the mass depending on the difference of position of one point and another. From the interpretation of the equation, it appears that the fissure (or rather its intersection with a horizontal plane) will in general be rectilinear only in the particular case in which the ratios $\frac{f_1}{F}$, $\frac{f_2}{F}$, &c. are the same for every point through which it passes, supposing the directions of the tensions at one point respectively parallel to those at another. There is, however, one important exception, viz.

the case in which there are two tensions only, and these tensions at right angles to each other. The direction of the fissures will then be always perpendicular to that of the greater tension. If therefore the directions of this tension at different points be parallel to each other, the fissure will be rectilinear, whatever be the ratio of the two tensions. The case of a single tension is a particular case of the above, when the smaller tension vanishes. If there be two tensions making an acute angle with each other, the direction of the fissure will be within the exterior or obtuse angle between the directions of the tensions; and if one tension be considerably greater than the other, or if the angle between their directions do not deviate materially from a right angle, the fissure will lie much nearer to the direction of the smaller tension* than to that of the greater.

III. Having thus explained how a single fissure may be formed, and its direction determined, let us consider the formation of a number of similar fissures all following the same law, and not remote from each other, thus forming a *system of fissures*. In the greater number of cases in which such systems have been recognised the lines of dislocation have been approximately rectilinear and parallel. It will suffice, therefore, to take this case, which will be somewhat the most simple to explain.

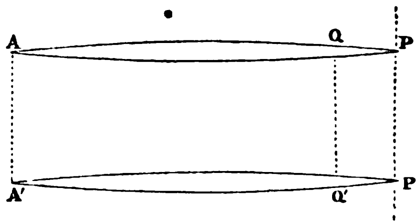
In the first place I have considered in my memoir, how far it would be possible that the fissures of a system should be formed *consecutively*. For this purpose I have examined the modification which would be produced in the tension of the mass by the existence of a rectilinear fissure extending for any assigned distance, assuming, for the greater simplicity, the mass to be acted on by one system of tensions perpendicular to the fissure; and it appears that if we draw a line perpendicular to the fissure and meeting it at a point P, not too near its extremities, the tension at any proposed point of this line and in its direction (or perpendicular to the fissure) will be less than that which will be caused by the existence of the fissure, in a direction parallel to itself, provided the distance of the proposed point from the fissure be less than the radius of curvature at P of the curve formed by the intersection of the vertical side of the fissure with a horizontal plane†. Now it has been before stated that when there are two tensions at any point in directions perpendicular to each other, if they produce a fissure it must be perpendicular to the greater of

* Considerably nearer to it than the resultant of two forces respectively equal in intensity to the two tensions, and in the same directions.

† Memoir, p. 33.

these tensions, and therefore, in the present case, perpendicular to the former fissure. Consequently, since the radius of curvature above mentioned will, if the fissure be of considerable length, be very large at every point, it will be impossible for a second fissure to be formed parallel to the first and not very remote from it, by the general tensions to which the mass is supposed to be subjected*.

We may conceive, however, any number of parallel fissures in the case we are considering to be formed *simultaneously.*



Thus suppose two parallel fissures, AP, A′P′, produced by tensions acting perpendicularly to their directions, to begin simultaneously at A and A′, and also to arrive at P, P′ at the same instant, P P′ being perpendicular to A P and A′ P′. There will manifestly be no reason why they should not in such case be continued simultaneously from P, P′, just as they began at the same instant at A and A′. If, however, the relaxation produced by the opening of A P be communicated through the distance P P′ *instantaneously*†, it is clear that as soon as A P should have advanced in its progressive formation by the smallest quantity further than the other fissure, the formation of this latter would be instantly arrested. Under such circumstances, then, the possibility of the simultaneous formation of two or more fissures would be rather a mathematical than a physical possibility. The fact is, however, that the relaxation produced by the one fissure is not communicated instantaneously to the distance of the other. *Time* will be necessary for this purpose, and this removes the difficulty of conceiving this mode of formation, since it is no longer necessary that the velocities of propagation of the two fissures should be mathematically equal. For, suppose one fissure to have reached P when the other has reached Q′,

* If there were another system of tensions perpendicular to the first, this conclusion would be true for still greater distances from the existing fissure. We may remark that these two cases of tensions would seem to be the only ones in which systems of rectilinear parallel fissures near each other could in any way be produced. See Memoir, p. 36.

† This would be the case if the mass were absolutely *inextensible.*

then it is easily seen that if the velocity of propagation of the first fissure should be such as to continue it from Q to P, in less time than the relaxation of the tension would be communicated from Q to Q′, (Q Q′ being parallel to P P′,) the continued formation of A′ Q′ would not be arrested. Now I have shown* that the velocity of propagation will be extremely great, so that the distance Q′ P′ may be large, and all physical impossibility is therefore entirely removed. Let us suppose, for instance, a system of parallel fissures to begin simultaneously along the lower surface of the elevated mass, and to be propagated upwards †. If the mass be nearly homogeneous, the velocity of propagation will be nearly infinite; and if the fissures be not too near together, it is very possible that the time requisite for the relaxation of the tension to be communicated from one fissure to the distance of the next, may be greater than that which is necessary to propagate the fissures to the upper surface of the mass. In this case it is manifest that every fissure will necessarily be continued to that surface. It seems most probable, however, that in actual cases, similar to that just stated, a part only of the fissures commencing below would reach the higher portion of the mass. If its thickness should be very great, the fissures reaching the surface would probably be at a proportionally greater distance from each other.

In this manner, then, the formation of systems of parallel fissures presents no difficulty. Adopting this view of the subject, we are immediately led to the conclusion, *that the whole of any disturbed district, characterized by a continuous system of parallel dislocations, must have been elevated simultaneously.* It is not, however, here meant to be asserted that the whole elevation must have taken place at once, but that that movement which determined the positions of the principal and characteristic dislocations by causing the commencement of their formation, must have been a great movement, and must have extended at least as far as such dislocations may be observed to follow continuously the same law. Subsequent efforts of the elevatory forces might take place in any number, but it is evident that they would have but little effect in producing new fissures parallel to the former, (since the mass would generally yield along the old ones,) though they may be very instru-

* Memoir, p. 22.

† I have shown (Memoir, p. 43) that fissures must generally commence in the lower portion of the mass; and we may remark, that according to our hypothesis respecting the rapid increase of intensity of the elevating force, from the instant the elevation commences, the formation of the fissures must begin almost accurately at the same instant.

mental in extending those existing previously. Partial elevations, or subsidences, may be easily conceived to be thus produced; but whatever alteration may take place in this manner, in the general conformation of the district, must be under the guidance, as it were, of the fissures previously existing.

Nothing perhaps will tend more to corroborate the views I have been explaining on this important point of the formation of systems of fissures, than the attempts we may make to account for it otherwise, assuming always that the phænomena are due to the action of mechanical causes extraneous to the mass itself, and independent of that kind of internal molecular action to which the existence of joints, or of a laminated structure, may possibly be owing. In the first place, I have shown that two parallel fissures not remote from each other could not be formed consecutively by a repetition of the elevatory action extending to the whole elevated mass; this consecutive formation, if it should take place at all, must therefore be owing to consecutive partial efforts of the elevatory force at different points of the mass. But I have shown* that, if the elevatory force be confined to a portion of the mass of comparatively small superficial extent, fissures must either be formed diverging from it in all directions, such as have been recognised in Mount Etna, and in the groups of the Cantal and Mont Dor, or concentric about the vertex, so that it is mechanically impossible that systems of parallel fissures could be thus produced. In fact, I can in no way conceive this successive formation of parallel fissures, without hypotheses respecting the mode of action of the elevatory force which are infinitely too arbitrary to be admitted for an instant.

After one system of fissures is formed, there is no difficulty whatever in conceiving the formation of a second system perpendicular to the former. The existence of two rectilinear parallel fissures must evidently destroy all tension in the portion of the mass between them, but will have no effect on the extension, or therefore on the tension which may exist in a direction parallel to the fissures, the only one in fact in which any tension can be impressed on a part of the mass so situated. Consequently whatever tendency there may be to form a second system of fissures, it must necessarily be in a direction perpendicular to that already existing. This second system might be formed by any forces, however partial or irregular their action, (always assuming it not absolutely *im-*

* Memoir, p. 47.

pulsive,) since the only direction in which the mass could admit of any tension being impressed upon it would be, as just stated, that parallel to the first system. It seems to be mechanically impossible that any second system of parallel fissures could be thus formed except in the direction here stated.

[To be continued.]

LVII. *On the lately proposed Logarithms of Unity, in Reply to Professor* De Morgan. *By* John T. Graves, *of the Inner Temple, Esq., M.A.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the Philosophical Transactions for 1829, and in the abstract of a memoir printed in the Fourth Report of the British Association, it is proposed to modify the ordinary formulæ for the logarithms of unity and of numbers in general by certain rather startling extensions. These innovations Prof. De Morgan is not disposed to admit, as appears from sections (158.) and (245.) of his able and useful Treatise on the Calculus of Functions, recently published in the *Encyclopædia Metropolitana.* His principal difficulty seems to be founded on his dissent from the proposition that $e^{\frac{2i\pi}{2i\pi-\sqrt{-1}}} = 1$, and accordingly he challenges the supporters of the new theory to the proof of that equation. Now in one sense I do not either assert or admit its accuracy; for $e^{\frac{2i\pi}{2i\pi-\sqrt{-1}}}$ has many values, while 1 has only one. I am bound, however, to show that 1 is *among* the values of $e^{\frac{2i\pi}{2i\pi-\sqrt{-1}}}$. To show this would be to prove, that, according to my understanding of the term, $\frac{2i\pi}{2i\pi-\sqrt{-1}}$ is a Neperian* *logarithm* of 1. I call $\frac{1}{2}$ an e-log. of $-\sqrt{e}$ as well as of $+\sqrt{e}$. If I am told that logarithm ought to be so defined that x ought to be called an a-log. or a logarithm to base a only of the

* I have been accustomed to write "Neperian" instead of "Napierian," because the inventor of logarithms, in the title of his original work on the subject, signs his name in Latin, "Neperus"; because the Scottish mode of spelling the name was unfixed in his time, and because foreigners have generally adopted the Latin orthography.

arithmetical value of a^x, I must say that I should not approve such a restriction. It would, if the proposed theorems be correct, arbitrarily exclude from the *name* of logarithms orders of functions which enjoy the same fundamental and characteristic *properties* as those that are favoured with that name. It would also in some cases be difficult of application. The expression $(m - \sqrt{-1}\, n)^{\frac{1}{2}}$ has two values. If we adopt the restricted definition, of which of those values shall we be allowed to call $\frac{1}{2}$ a logarithm with reference to the base $m - \sqrt{-1}\, n$? On the other hand, there are cases where a^x has an infinite number of real and positive values. But I proceed to prove that 1 is among the values of $e^{\frac{2 i \pi}{2 i \pi - \sqrt{-1}}}$ from postulates which I believe will be conceded by Professor De Morgan.

For all values of x, real or imaginary, by cos x, I denote

$$1 - \frac{x^2}{1.2} + \frac{x^4}{1.2.3.4} - \frac{x^6}{1.2.3.4.5.6} + \dots \qquad (1.)$$

and by sin x I denote

$$\frac{x}{1} - \frac{x^3}{1.2.3} + \frac{x^5}{1.2.3.4.5} - \frac{x^7}{1.2.3.4.5.6.7} + \dots \qquad (2.)$$

I assume that

$$\left\{1 + \frac{x}{1} + \frac{x^2}{1.2} + \frac{x^3}{1.2.3} + \dots\right\}$$

$$\times \left\{1 + \frac{y}{1} + \frac{y^2}{1.2} + \frac{y^3}{1.2.3} + \dots\right\}$$

$$= 1 + \frac{(x+y)}{1} + \frac{(x+y)^2}{1.2} + \frac{(x+y)^3}{1.2.3} + \dots \qquad (3.)$$

that the complete and correct equation for e^x is

$$e^x = 1^x \left(1 + \frac{x}{1} + \frac{x^2}{1.2} + \frac{x^3}{1.2.3} + \dots\right) \qquad (4.)$$

and that, if w denote in succession O and all integers positive and negative, the values of 1^x are properly represented by the formula

$$\cos(2 w x \pi) + {}^{*}\sqrt{-1}\, \sin(2 w x \pi). \qquad (5.)$$

* I have found that upon the whole it tends to superior clearness of notation to place $\sqrt{-1}$ foremost where it occurs as a factor.

From these premises it follows irresistibly that

$$e^x = \left\{1 + \frac{(\sqrt{-1}\, 2\, w\, x\, \pi)}{1} + \frac{(\sqrt{-1}\, 2\, w\, x\, \pi)^2}{1.2} + \dots\right\}$$

$$\times \left\{1 + \frac{x}{1} + \frac{x^2}{1.2} + \dots\right\} \qquad (6.)$$

but by (3.) this product is equal to

$$1 + \frac{(\sqrt{-1}\, 2\, w\, \pi + 1)}{1}\, x + \frac{(\sqrt{-1}\, 2\, w\, \pi + 1)^2}{1.2}\, x^2$$

$$+ \frac{(\sqrt{-1}\, 2\, w\, \pi + 1)^3}{1.2.3}\, x^3 + \dots \qquad (7.)$$

The development (7.) therefore is complete and correct, having as many values, and only as many, as e^x.

To apply this result, on substituting $\frac{2\, i\, \pi}{2\, i\, \pi - \sqrt{-1}}$ for x in (7.), we find that the values of $e^{\frac{2\, i\, \pi}{2\, i\, \pi - \sqrt{-1}}}$ are represented by the formula

$$1 + \left(\frac{2\, w\, \pi - \sqrt{-1}}{2\, i\, \pi - \sqrt{-1}}\right) \frac{(\sqrt{-1}\, 2\, i\, \pi)}{1}$$

$$+ \left(\frac{2\, w\, \pi - \sqrt{-1}}{2\, i\, \pi - \sqrt{-1}}\right)^2 \frac{(\sqrt{-1}\, 2\, i\, \pi)}{1.2}$$

$$+ \left(\frac{2\, w\, \pi - \sqrt{-1}}{2\, i\, \pi - \sqrt{-1}}\right)^3 \frac{(\sqrt{-1}\, 2\, i\, \pi)^3}{1.2.3} + \dots \qquad (8.)$$

Now, among the values of (8.), which are found by giving to w in that formula all its values in succession, there is one value (which I denote by $e_i^{\frac{2\, i\, \pi}{2\, i\, \pi - \sqrt{-1}}}$) that will correspond to the case of $w = i$, in which case the development (8.) reduces itself to

$$1 + \frac{(\sqrt{-1}\, 2\, i\, \pi)}{1} + \frac{(\sqrt{-1}\, 2\, i\, \pi)^2}{1.2} + \frac{\sqrt{-1}\, 2\, i\, \pi)^3}{1.2.3} + \dots$$

$$= \cos(2\, i\, \pi) + \sqrt{-1} \sin(2\, i\, \pi) = 1. \qquad \text{Q. E. D.}$$

It may be remarked that, tested by formula (7.), the expression $e^{\sqrt{-1}\, 2\, i\, \pi}$ will be found to have an infinite number of real values, which are successively equal to 1, and to the ascending integral powers of $e_0^{-4 i \pi^2}$ with their reciprocals. The expression $e_0^{-4 i \pi^2}$ is equivalent to $\cos(\sqrt{-1}\, 4 i \pi^2) + \sqrt{-1} \sin(\sqrt{-1}\, 4 i \pi^2)$. In these investigations there is often much convenience in thus putting imaginary quantities after *sin* and *cos*, and the want of familiarity with this practice has, perhaps, been an obstacle to the full comprehension of the new theory.

There is another objection started by Professor De Morgan but not pursued. He says that after supposing $a = f\theta$, where $f\theta = \cos\theta + \sqrt{-1} \sin\theta$, I find $a^x = f(xf^{-1}a)$, but that I should have obtained precisely the same expression, if I had assumed $f\theta = e^{c\theta}$, c being any quantity whatever. By $e^{c\theta}$, I here understand the Professor to mean *that particular value* of $e^{c\theta}$ which is equal to $1 + \frac{c\theta}{1} + \frac{(c\theta)^2}{1.2} + \frac{(c\theta)^3}{1.2.3} + \ldots$, and which I should denote by $e_0^{c\theta}$. On this understanding I fully agree with his statement, but not with the inference which I suppose him to draw from it. I suppose him tacitly to infer, without minute examination, that when $f\theta = \cos\theta + \sqrt{-1}\sin\theta$, or $e_0^{\sqrt{-1}\,\theta}$, the expression $f(xf^{-1}a)$ cannot have so general a meaning as it would have if the meaning of $f\theta$ were generalized in the mode he mentions. This is an inference which, if the functional definition of a^a adopted by me were admitted to be proper, would at most only charge my solution with not being *sufficiently general*. I acknowledge, indeed, that the impropriety of that definition would be evinced, if it fairly led to a meaning of a^x which subverted any established exponential theorems more fundamental than those which that definition embodies, or if by the inconvenient generality of the values it would comprise, the expediency of a further limiting equation of condition were pointed out. The supposed inference, however, as above stated, is not correct, for it happens that even if $f\theta$ were assumed equal to $1 + \frac{c\theta}{1} + \frac{(c\theta)^2}{1.2} + \frac{(c\theta)^3}{1.2.3} + \ldots$, the expression $f(xf^{-1}a)$ would be unaffected by any variation of c, since it would indicate operations which would eliminate c from the result. That result would in consequence be identical with mine, and if examined with a little attention, would be perceived entirely to coincide with what is ordinaily understood by a^x in all cases where the meaning of a^x has been fixed by usage, and to confer a reasonable analogical meaning on a^x where usage has been silent. The definition of a^x which I adopt is this: a^x denotes in succession every function $\phi\, x$ of x, which, independently of x and y, fulfils the following conditions:

$$\phi\, x \,.\, \phi y = \phi\,(x+y) \qquad (9.)$$

$$\phi\, 1 = a \qquad (10.)$$

This definition is, perhaps, the simplest that could be proposed, the most extensively applicable and the most accordant of any with other less fundamental properties of a^x which have been observed to hold good within certain limits.

The abstract contained in the Fourth Report of the British Association does not contain the reasoning by which I find $f\,(x f^{-1} a)$ to be the general solution for a^x, $f\theta$ being equal to $\cos\theta + \sqrt{-1}\,\sin\theta$. My reasoning is as follows:

I first proceed to show that if $f x$ and $f^{\backprime} x$ denote two functions of x, each of which fulfils condition (9.), we shall have $f^{\backprime} x = f(c\,x)$, c being some constant.

Let $f^{\backprime} x = f\,\psi\, x$, $\psi\, x$ being some unknown function of x, the form of which is sought: then by the assumed property of $f^{\backprime}$, we shall have $f\psi x . f\psi y = f\psi\,(x+y)$; but by the same property of f, we have $f\,\psi\, x\,. f\psi y = f\,(\psi\, x + \psi y)$. Hence $f\,\psi\,(x+y) = f(\psi\, x + \psi y)$.

Now, if $\psi\ (x+y)$ differ from $\psi\, x + \psi y$, let $\theta + \psi\,(x+y) = \psi\, x + \psi\, y$; then we shall have $f\{\theta + \psi\,(x+y)\} = f(\psi\, x + \psi y) = f\psi\,(x+y)$; but by the property of f, $f\,\{\theta + \psi\,(x+y)\} = f\theta f\,\psi\,(x+y)$; hence $f\theta\,.f\psi\,(x+y) = f\psi\,(x+y)$; hence $f\theta = 1$.

Hence the general equation to find $\psi\, x$ is

$$\theta + \psi\,(x+y) = \psi\, x + \psi\, y, \qquad (11.)$$

θ being some quantity such that $f\theta = 1$.

$$\text{Let}\quad \psi\, x - \theta = \psi^{\backprime} x \qquad (12.)$$

By this substitution we obtain from (11.)

$$\psi^{\backprime}\,(x+y) = \psi^{\backprime} x + \psi^{\backprime} y \qquad (13.)$$

I consider equation (13.) the purely algebraic part of the best definition that can be given of what is meant by multiplication in its extended sense, since that definition is based on the most characteristic formal property of multiplication in arithmetic, the science suggestive of symbolic rules. In my view of algebra, if we presuppose arithmetic in general, and algebraic addition, we may at once, on having obtained equation

(13.), assume $\psi' x = c x$, c being some algebraic multiplier, since it is impossible to arrive at any simpler proof than the mere *form* of the proposition.

For those to whom this assumption may not seem satisfactory, we may make the proof that $\psi' x = c x$ rest upon different data. Whatever be the form of ψ' and value of h, the expression $\dfrac{\psi'(x+y+h) - \psi'(x+y)}{h}$ remains unaltered, whether h be regarded as the increment of x or y. We have therefore in general

$$\frac{d\,\psi'(x+y)}{d\,x} = \frac{d\,\psi'(x+y)}{d\,y} \tag{14.}$$

or, in this particular case, performing the respective partial differentiations on the equivalent expression in (13.), we obtain

$$\frac{d(\psi' x+\psi' y)}{d\,x} \text{ or } \frac{d\,\psi' x}{d\,x} = \frac{d\,(\psi' x+\psi' y)}{d\,y} \text{ or } \frac{d\,\psi' y}{d\,y} \tag{15.}$$

but since $\dfrac{d\,\psi' x}{d\,x}$ is equal to $\dfrac{d\,\psi' y}{d\,y}$, it must be independent of x, and therefore constant*. Let $\dfrac{d\,\psi' x}{d\,x} = c$, then $\psi' x$ must be equal to some quantity $\int c\,d\,x$. We assume that the general form of $\int c\,d\,x$ is $c' + c\,x$, but 0 is the only value of c' in the equation $\psi' x = c' + c\,x$, which will be found to satisfy equation (13.). Hence $\psi' x = c\,x$.

Having satisfied ourselves in whatever way, that $\psi' x = c\,x$ is the general solution of (13.), we have, by (12.), $\psi x = \theta + c x$. Hence $f\psi x$ or $f' x = f(\theta + c\,x)$; but $f(\theta + c x) = f\theta . f(c\,x) = 1 . f(c\,x) = f(c\,x)$. Hence $f' x = f(c x)$.

Q. E. D.

Hence, if *any* function $f x$ could be found to satisfy (9.), $f(c\,x)$, c being wholly arbitrary, would be the general solution of (9.). Equation (10.), which defines the base of the system, limits the otherwise arbitrary c to such values that $f c$ may be equal to 1. Hence $a^x = f(c\,x)$, c assuming in succession all the values of $f^{-1} a$, and none other.

The next step in the investigation is to find *some* function (*no matter what*) that fulfils condition (9.), and to determine the general form of its inverse. Such a function I find in $\cos\theta + \sqrt{-1}\sin\theta$.

* By similar considerations we might find at once from equation (9.), without having recourse to Taylor's theorem, that $\dfrac{d\,\phi\,x}{d\,x} = c\,\phi\,x$.

I have perused with interest Professor De Morgan's observations on the impossibility of ever proving that we have arrived at the *most* general solution of a functional equation. It seems to me, however, that our only limit to the meaning of the general symbols employed in the solution of such equations is the necessity of their compliance with certain formal conditions, which must by tacit or express convention be considered elementary and definitional. Now, I think, we may sometimes show that given functional equations can be solved by the solution of certain others expressing such elementary conditions, and by such solution only. When we can show this, we are at liberty, in my opinion, to substitute the symbols which the latter equations define, and to pronounce ourselves in possession of the *most* general solution. This subject deserves further consideration.

There is an error in the abstract of my last memoir. I there appear in substance to define *cosine* and *sine* to mean respectively such functions ϕ and ψ as simultaneously fulfil thefollowing conditions:

$$\phi x \phi y - \psi x \psi y = \phi (x + y) \quad (16.)$$

$$\phi x \psi y + \psi x \phi y = \psi (x + y) \quad (17.)$$

$$(\phi x)^2 + (\psi x)^2 = 1 \quad (18.)$$

These conditions do not constitute a sufficiently limited definition to coincide with the ordinary acceptation of *cosine* and *sine*. The general solution of (16.) and (17.) gives $\phi x = \frac{f(c x) + f(c' x)}{2}$, and $\psi x = \frac{f(c x) - f(c' x)}{2 \sqrt{-1}}$, where $f \theta$ means, as before, $\cos \theta + \sqrt{-1} \sin \theta$, $\cos \theta$ and $\sin \theta$ being defined by equations (1.) and (2.). The third condition (18.) only requires that c' should be equal to $-c$, and so only limits ϕx to $\cos (c x)$ and ψx to $\sin (c x)$. If instead of the third condition we were to substitute the following, viz.

$\phi x = \frac{d \psi x}{d x}$, we should have a good definition coinciding with (1.) and (2.).

I am anxious to embrace the present opportunity of correcting a former involuntary misrepresentation with respect to Professor Ohm. I find that his logarithmic formulæ are not only coincident in principle with mine, but coextensive in their applicability to imaginary as well as real quantities.

On some future occasion, Gentlemen, I shall be happy, with your permission, to communicate my *investigations* relating to the limits of the possibility of finding a base x, such that a particular specified value (x_i^a) of x^a may be equal to c, a and c being given. My *results* are shortly stated in the Fourth Report of the British Association, p. 528. The question bears closely on the subject of the solution of equations involving surds and their "*chance*" of representable roots, a subject which was treated in an interesting and logical manner by Mr. W. G. Horner in a letter to be found, p. 43, of the January Number of your Magazine for this year. I hope also that you will be able to find room for a statement of the restrictions which various ordinary exponential theorems require, and for a few useful equations and developments.

With sentiments of sincere respect, I have the honour to be,

Gentlemen, yours, &c.,

Inner Temple, Feb. 12, 1836. JOHN T. GRAVES.

LVIII. *On the Phænomena of Drops of Oil floating on Water. By the Rev. Professor* CHALLIS.*

I AM not aware that the following facts, connected with the subject of capillary attraction, have been before observed. A single drop of salad oil was let fall on the surface of water contained in a glass tumbler, and was seen to spread immediately on the water surface. Another drop let fall shortly after on a part of the surface not reached by the spreading of the first, was not observed to spread in the least degree like the other, but instantly assumed a well-defined circular shape. The first drop also collapsed by degrees into a circular form; and this, it was found by repeating the experiment, was most likely to happen when the drop was not of very small size. When two drops fell in very quick succession, both of them were observed to spread, but that which reached the surface last, spread in less degree than the other, and sooner assumed a circular shape. The smaller the size of the spreading drops, the greater appeared to be their tendency to spread. In one instance a very small drop was seen to be succeeded by another at a considerable interval, which also spread, but in much less degree. These phænomena were uniformly presented in a great number of trials, fresh water being put into the glass after each. The chief thing to remark is, that without any visible connexion between the first drop and the succeeding ones, the manner in which the latter are affected on coming into contact with the water is influenced by the previous contact of the first.

The explanation I propose to give of this fact will be drawn

* Communicated by the Author.

from the theory of the molecular forces of fluids contained in my communication to the February Number of this Journal. It is there supposed that the sphere of action of the attractive molecular forces of fluids is much greater than that of the repulsive, and that the latter increase so rapidly with any decrement of the mutual distances of the molecules, as to be taken account of without sensible error by considering the fluid incompressible. On this supposition the angle of actual contact between a solid and a fluid, or that between two fluids, is determined by the hydrostatical equilibrium resulting from the molecular *attractions* of the two substances, the solid like the fluid being treated as incompressible. It thence appeared that this is an exceedingly small angle in cases in which the bodies in contact are not of very different specific gravities. Hence in the instance before us, the angle of contact, that is, the angle which the surface of contact of the oil and water makes with the upper free surface of the oil, is very small. But since the drop is convex both at its upper and under surfaces, this is apparently an angle of considerable magnitude. In fact the *theoretical* angle of contact, or that which the upper surface of the oil makes with an imaginary surface drawn parallel to its under surface and just beyond the sphere of the molecular action of the water, would be found by calculation to be of sensible magnitude. Consequently, that the angle of actual contact may be exceedingly small, the portion of the upper surface of the oil that lies within the sphere of the molecular action of the water must undergo a flexure near the visible periphery of the drop. Now in fulfilling this condition it seems probable that a very thin film of the oil spreads over the whole water surface, (as there is no force to counteract,) and gives rise at the same time to the visible spreading of the first drop. The film itself being of less thickness than the radius of the sphere of the molecular action of the water, will not be perceptible to the senses. Such a circumstance having happened to the drop that first comes in contact with the water, will prevent any that succeed from being similarly affected.

I take this opportunity of adverting to the editorial note (signed E. W. B.) in the February Number, (p. 172,) on my communication in that Number, and thanking the author of it for correcting the erroneous assertion that mercury is incapable of adhering to solids, which was inconsiderately made of solids in general, when I was more particularly referring to glass. In accordance with the authorities quoted in the note, the theory I was explaining would lead to the inference that mercury is capable of moistening substances of greater or not much less specific gravity than itself, by showing that the angle of actual contact with them may be exceedingly small.

With respect to the kind of molecular force to which the mathematical reasoning was intended to apply, I may observe that in strictness it is applicable only to that which is usually called the attraction of aggregation, a familiar instance of which, wholly independent of chemical affinity, is seen in *water* adhering to *ice*. I was unacquainted with Mr. Faraday's observations on this subject referred to in the note, but having since perused them, I quite agree with him in thinking that in the contact of two dissimilar substances this force is modified by chemical affinity, even when no chemical action takes place between them. There are, however, no means at present of estimating this effect mathematically. It is probably greatest in the state bordering on chemical action. Analysis applied to the case of perfect contact caused by the attraction of aggregation alone, (which is a simple instance of the *statics* of molecular forces,) leads to the inference that the same fluid will rise to the same height in different capillary tubes: and Link's experiments show, in fact, that water rose to the same height between glass, copper, and zinc plates; sulphuric acid, between glass and copper plates; muriatic acid, between glass and copper; liquid caustic alkali, between glass and zinc; liquid ascetic* alkali (sp. gr. 1·145), between glass and zinc. The deviations from the law in the other instances may therefore be owing to chemical affinity, perhaps also to chemical action. The same causes would affect the heights of ascent of different fluids in the *same* tube. But I am disposed to think that in addition to these causes, the difference of heights depends on the different natural conditions of the fluids. For instance, the most volatile fluids, which are probably those that are most perfectly fluid, appear by the experiments to rise least. A small degree of viscidity, it will perhaps be admitted, would tend to increase the height of ascent, if the condition of perfect contact be maintained. To separate the effect of chemical affinity from that of the attraction of aggregation, requires experiments more numerous and varied than any that have hitherto been made.

Observatory, Cambridge, March 11, 1836.

* [Carbonated?]

LIX. *Remarks on Lieutenant* Lecount's *Treatise on Iron Rails. By* Peter Barlow, *Esq., F.R.S.**

AN amusing but not a very accurate critique of my Reports to the Directors of the London and Birmingham Railway Company having been recently published by Lieutenant Lecount, R.N., which must, I suppose, be considered as the last expiring groans of the fish-bellied rails, in which critique many of my formulæ are made to suffer woful transformations, allow me in their defence to make a few observations, and they shall be very few. The author commences his inquiry at page 20, and as an earnest of what is to follow, his very first step is to correct a simple trigonometrical expression I have given, (*which is perfectly right as it stands,*) and by his correction to render it ambiguous. With this corrected formula, however, after another forty pages, he contrives to prove what I have stated at page 19 of my Report, viz. that by taking a most injudicious form of parallel rail, we may get one inferior to the fish-bellied rail of the same weight. Now my object has been to prove, on the other hand, that by choosing a judicious section we may get one as decidedly superior; and I have no doubt that thus far both conclusions are just, notwithstanding the ambiguity of his formula.

As it stands in my Report, the expression is

$$\surd\,(r^2 + d^2 - 2\,d\,r\cos x);$$

and Mr. Lecount, not recollecting that the cosines in the second quadrant are negative and that "minus into minus produces plus," has thought it necessary to make the alteration in question:—any student in trigonometry will judge with what propriety.

The next 47 pages are employed to prove that all my rules for the neutral axis are unfounded; which of course they ought to be, if all Mr. Lecount says about them be correct. I will not even suspect that he has designedly misrepresented and misapplied my investigations, but I must say that the result he conceives he has arrived at, page 107, is very far from a correct statement. It would seem from what he says, that I give the ratio of 1 to 9 for all cases. Now, if he had properly understood what I had done, and if he had wished to have properly represented it, he would have informed the reader, that I had given a rule which was general for all bars; and that as an approximate rule only, sufficiently exact for all practical purposes, I had stated that taking the neutral axis in the middle of the head was nearly correct for all the usual forms of rails, and, as it happens, (the rail in question being

* Communicated by the Author.

five inches deep and the head an inch deep,) the ratio in that particular case is 1 to 9.

The worst, however, is what follows in the subsequent chapters, where he compares my computed, or rather his computed, results with my experiments, and where by a very unaccountable blunder he mistakes through the other 87 pages my columns of index readings for deflections, and pays me and my rules some very awkward compliments because the two do not agree. Now, I should have wondered very much if they had, for they might as well be compared with the column of sunrisings in any page of an almanac as with the column of numbers he has mistaken for deflections.

I have explained, (I should have thought sufficiently clearly,) at p. 36 of my First Report, what these numbers are, and how the deflections in the adjacent columns are obtained from them; and I must think that Mr. Lecount is the only person who has yet misunderstood them. I have called them in the head of the column, to mark the distinction, *deflections by index* in some places, and in others *index readings*; but in all the tables the adjacent column is headed *deflections for each ton*, and it is this column alone with which comparisons can be made; and I must repeat that I cannot help thinking that Mr. Lecount is the only person who has yet fallen into this singular error. If I had not a better opinion of his integrity, I should be almost inclined to think it was a designed mistake to make out a case in favour of the fish-bellied rail, but of this I most fully acquit him; but then to what am I to attribute it? I know but of one other explanation.

As an example or two of the kind here referred to, the reader will excuse my quoting the following. At page 109 he says, "Mr. Barlow gives the mean deflection per ton at ·015, and the deflection for 7½ tons ·107; whereas in the very same table, and only three lines above this deduction of ·107 deflection for 7½ tons, it is shown in the experiment that at 7 tons it was actually ·335, or three times greater than that which is deduced by this mode of proceeding for 7½ tons *There is some mistake here evidently.*"

Evidently there is, Mr. Lecount, and it is this; you have mistaken my index readings for deflections: if you will look again you will find that you could not have found a better proof of the correctness of my deductions.

Again, page 151, Mr. Lecount says: "Mr. Barlow himself, p. 103, Second Report, states the deflection by computation, &c. to be from ·051 to ·055 with 11 tons, although in the same page, and only three lines above, the experimental deflection is registered from actual observation ·0717. What have we here to do with calculation or hypothesis? We see the thing before our eyes; the *rail does deflect* ·0717; and why are we told that it only deflects ·055?" Now, I say, the rail does *not* deflect ·0717: if Mr. Lecount will turn again to page 103, he will find that what he takes for "deflections by computations, &c. from ·051 to ·055," are the experimental deflections; and that ·0717, the number "before our eyes", is only the index reading.

Mr. L. thus passes through all my pages from 36, First Report, to 103, Second Report, with a total misapprehension of my tables. I am sure, therefore, his readers will readily excuse his having occasionally misunderstood my deductions from them.

I might, if I had leisure, amuse myself and perhaps the reader with many other specimens of the author's ingenuity; indeed, I really think he has subjected himself to prosecution for the torture which he has inflicted on my differential equations; but I have, perhaps, said enough to show that my rules are not *quite* so ill-founded as Mr. Lecount would lead his readers to believe; at the same time I will readily admit that with all the varieties of iron only mean results can be expected, and "that two bars of the same weight and form will have different degrees of strength," &c.; but if I have fitted them to what iron of a good quality (not the best) ought to bear, it is all that I profess; and from many experiments made since my Report was published, I have reason to believe I have succeeded.

Mr. Lecount concludes his preface by saying: "It requires a man of some nerve to face such a leviathan as Professor Barlow on mathematical points, but it was necessary *that some person should do it*, and it appears the lot has fallen on Jonah, with what advantages others must judge." Perhaps a little more attention to what he was reading with a view to criticise it, would have been better than mere nerve to have contended with his supposed formidable opponent. As to the advantages, I must leave that question to be settled between Jonah and his readers.

LX. *On the Solar Eclipse of May* 15*th*, 1836, *particularly as it will be seen at Alnwick, in Northumberland. By* Thomas Squire, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Of all the anticipated celestial phænomena of the present year, the large solar eclipse which happens on Sunday, May 15, P.M., must be considered to rank foremost in a popular point of view. This eclipse, it is well known, will be annular in the North of England, and central across Northumberland. It also further appears that this central track will run very nearly over the town of Alnwick, and on looking at the line of its course, it is clear that this place will be found the most convenient and proper for observing this eclipse under its greatest magnitude; with this impression I have been induced to send you the results of my computations of the same for that place, with some other trifling matters relating to this subject, which I hope you will find a corner for in the next Number of the Philosophical Magazine.

Particulars of the large Solar Eclipse of May 15, *p.m.*, 1836; *computed for the Latitude and Longitude of Alnwick.**

☉ eclipsed May 15th, P.M.
comp. for Alnwick.

	h	m	s	
Beginning..........	1	41	58·1	at 45° 56′ 58″ west of the ☉'s l. l.
Beg^s of annulus	3	5	54·4	☽'s centre N. of ☉'s = 5″·1646;
Middle............	3	8	13·3	then will the greatest breadth of the annulus be 56″·8446,
Visible ☌☉☽	3	8	14·7	and least 46″·5154.
End of annulus	3	10	32·2	
End of eclipse......	4	28	2·1	at 34° 54′ 46″ from the ☉'s vertex.

For the basis of these calculations, I have supposed the geographical latitude of Alnwick to be 55° 25′ 22″ N. or its geocentric 55° 14′ 49″, and longitude 1° 28′ W. of the Royal Observatory, Greenwich.

The rare phænomenon of a central eclipse in England, and the opportunity it offers for scientific inquiry, will no doubt be an inducement for many gentlemen to visit Alnwick, and its neighbourhood, for the purpose of making such observations on the present eclipse as may be conducive to the extension of our knowledge in astronomy and philosophy. Should the atmosphere be favourable the observer must not only be careful to observe the beginning and end with the greatest accuracy, but also the immersions and emersions of the solar spots; the inflection of light about the beginning and ending of the annular formation; and it will also be interesting to observe what stars are visible. The barometer and thermometer should be attended to, and experiments made on the calorific effects of the solar rays on different bodies, regard being had to the magnitude of the eclipse at the same time. Moreover, the colour and shade of objects should be at-

* The instants are given in mean solar time according to the meridian of that place.

tended to; and it may be proper to notice what effect the gloom has upon animals and plants, &c.*

I remain, Gentlemen, yours, &c.

Epping, March 15, 1836. THOMAS SQUIRE.

P.S. During the annular observation the light and heat will be about $\frac{1}{10}$ of that of the full sun.

LXI. *Observations upon* Mr. Woolhouse's *Theory of Vanishing Fractions.* *By* J. R. YOUNG, *Esq., Professor of Mathematics in Belfast College.*†

IT was a remark of D'Alembert, that in all subjects except in the mathematical sciences, there was room for difference of sentiment. This exception, however, in favour of mathematics was unadvisedly made by D'Alembert, as his own disputes with Euler, on the subject of imaginary logarithms, fully prove. Nor is this the only mathematical topic upon which very considerable difference of sentiment has prevailed. The doctrine of vanishing fractions, a subject of far higher interest and importance, has been the source of much more keen and frequent controversy among mathematicians, and respecting which doctrine there is by no means harmony of opinion even at the present day; and this is a circumstance doubtless to be regretted, because of the frequent and unavoidable occurrence of these fractions in various departments of analytical research. To the aspiring student such conflicting theories in a part of the "exact sciences" must be a source of much perplexity. It must be embarrassing to feel that if he assent to the reasoning of the profound Waring, he must oppose himself to that of the cautious Maseres; and that if he adopt the views of Professor Woodhouse, he must discard the arguments of Dr. Hutton. It cannot, however, be denied that the opinions of Waring and Hutton are those which most accord with the ordinary views of modern analysts, in reference to this subject; and it was scarcely to have been expected that any mathematical theory should now be promulgated condemnatory of conclusions which, in the works of our ablest modern analysts, wear all the aspect of mathematical certainty. An essay has, however, been recently published, by a very ingenious and able mathematician, embody-

[* Particular directions for observing an Annular Solar Eclipse, adapted to every class of observers, and to the use of instruments of every degree of perfection and power, will be found in Mr. Baily's Memoir on the Annular Eclipse of Sept. 7, 1820,—Phil. Mag., First Series, vol. lv. p. 85.—EDIT.]

† Communicated by the Author.

ing statements and positions, in reference to this important inquiry, of a very peculiar kind, and which appear to me to be not only opposed to well-established truths, but calculated, under the protection of his name, to retard—what I am sure that gentleman is most anxious to promote—the spread of pure scientific truth. It is from the same anxiety for truth that I here venture, very briefly, to examine the more prominent of the positions adverted to, and this I do with the same sentiments of respect and regard which I have long entertained for his talents and friendship. I cannot, perhaps, in strictness, say that my own defence requires that I should reply to the animadversions which Mr. Woolhouse has made upon the views which, in common with so many others, I entertain on the subject of vanishing fractions, although I think I am privileged to show that my friend has not supplied the place of these views, which he has very unsparingly censured, by others that will bear the test of careful examination. With many of the observations in Mr. Woolhouse's Essay I am disposed entirely to agree, as being in strict accordance with the usual notions of this doctrine; but the new theory, for which the Essay is chiefly remarkable, seems to me to have been much too hastily framed; it is embodied in the very general propositions which follow:

I. "If, in any investigation of a geometrical problem, the unknown quantity is expressed by a fraction which, in a particular case, becomes a vanishing one, the problem in that case will resolve itself into a porism, and the value of the fraction, or unknown quantity, will then admit of arbitrary assumption; and a similar result will follow in all such cases, whatever be the nature of the investigation."

II. "Whenever, in an analytical investigation, the resulting expression for a quantity resolves itself into a vanishing fraction, we may observe, as a general rule, that either one of the original conditions of the inquiry becomes destroyed, or that two or more of them become dependent, and, consequently, whichever way it be, that there is at least one condition less to fulfill, and that the vanishing fraction is not restricted to any determinate value." (Gentleman's Diary, 1836, pp. 25, 26.) That these propositions are fallacious, Mr. Woolhouse would, I think, have soon seen, if he had attempted their demonstration, instead of contenting himself with testing their accuracy by two particular examples, which, as far as they go, seem indeed, at first sight, to corroborate their truth, although upon examination such will not be found to be the case; and if we were to interpret every vanishing fraction agreeably to this theory, we should frequently be involved in the most palpable errors. Indeed it is remarkable that my friend did not reflect that $\frac{0}{0}$, occurring in an analytical result, was as likely to be the symbol of *absurdity*, that is, of no value at all subsisting under the proposed conditions, as the symbol of multiple values.

When we are operating with equations of the first degree, containing several unknown quantities, the symbol $\frac{0}{0}$ is, in fact, the very form which the result usually takes when the proposed equations involve incompatible conditions; so that the foregoing theory would lead us to infer an unlimited variety of values, when in reality not one exists. The theory which Mr. Woolhouse condemns could never lead to such absurdity. But even the examples which Mr. Woolhouse adduces do not appear to accord with the doctrine which they are intended to illustrate and enforce; nor do they furnish any ground of objection to the theory they are designed to oppose. Of these two examples the following is the one upon which, I believe, Mr. Woolhouse places the most importance.

"To find a point in the arc of an elliptic quadrant, such that, a tangent being drawn through it, the perpendicular drawn from the centre to the tangent may be a mean proportional between the two semiaxes a, b." Now by putting x, y for the coordinates of the required point, we easily obtain the following equations, embodying the proposed conditions, viz.

$$\frac{x^2}{a^4} + \frac{y^2}{b^4} = \frac{1}{ab}, \quad \frac{x^2}{a^2} + \frac{y^2}{b^2} = 1$$

"and we find

$$x^2 = \frac{a^3(a-b)}{a^2-b^2} = \frac{a^3}{a+b}, \quad y^2 = \frac{b^3(a-b)}{a^2-b^2} = \frac{b^3}{a+b}$$

so that the coordinates of the required point are

$$x = a\sqrt{\frac{a}{a+b}}, \quad y = b\sqrt{\frac{b}{a+b}}."$$

Now, although most persons would say that these results furnish *all* the values of x and y legitimately deducible from the preceding expressions for x^2 and y^2, yet Mr. Woolhouse adds, "When $b = a$ the elliptic quadrant becomes a circular one, and these last expressions give for the position of the required point $x = a\sqrt{\frac{1}{2}}$, $y = a\sqrt{\frac{1}{2}}$, or the point which bisects the arc of the quadrant. But in the case of the circle, it is obvious that *all* its points will answer the proposed condition; and if we take the expressions which are *immediately* deduced in the investigation, viz.

$$x^2 = \frac{a^3 (a-b)}{a^2-b^2}, \quad y^2 = \frac{b^3 (a-b)}{a^2-b^2},$$

we see that they become vanishing fractions in the case of the circle, and do not limit the required point." Now, I submit that the values $x = a \sqrt{\frac{1}{2}}$, $y = \sqrt{\frac{1}{2}}$, are the true, and the *only* values, fairly deducible from these vanishing fractions; and that the fact of the problem admitting multiple solutions, under the proposed change of hypothesis, is altogether deduced from other, and distinct, considerations. It is, in fact, information which the analytical result is quite incompetent to supply; and is derivable solely from an examination, not of the *conclusion*, but of the *original conditions* of the problem. From this examination it appears that, in the proposed hypothesis, the two conditions merge into one, and thus a restriction being removed, the problem becomes indeterminate; but the mere merging of the final result into the form $\frac{0}{0}$, could never have made known this; the information is obtained quite independently of the slightest reference to this result, and from a directly opposite source. It is no doubt true, that *when* conditions disappear, in certain hypotheses the results will assume the form $\frac{0}{0}$, but it is not true *conversely*; that when the results assume the form $\frac{0}{0}$, conditions must have disappeared, and thus the values of $\frac{0}{0}$ become innumerable, as Mr. Woolhouse contends. What would my friend say of the sum of a geometrical series, viz. $S = \frac{a(r^n-1)}{r-1}$, when $r = 1$? According to his second proposition above, this sum is *anything*! It appears to me that Mr. Woolhouse's oversight, in his interpretation of $\frac{0}{0}$ in the foregoing problem, is analogous to that sometimes committed in physics; and which consists in taking for cause and effect, two phænomena, not invariably connected, yet both having a common antecedent. The very thing (viz. the hypothesis $a = b$) which causes x^2 to become $\frac{0}{0}$, causes also, *in this case*, one condition to disappear; and it is thence presumed that there is an *invariable* connexion between these two events; whereas that connexion is purely accidental.

Belfast, March 16, 1836.

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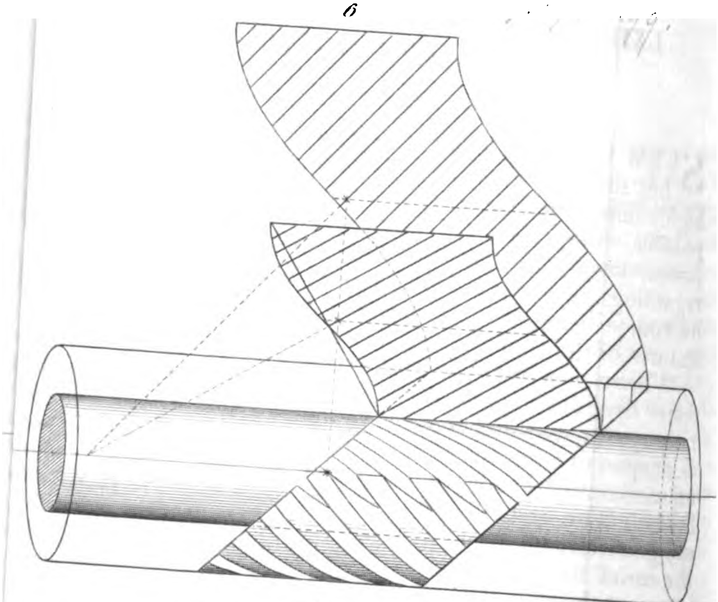

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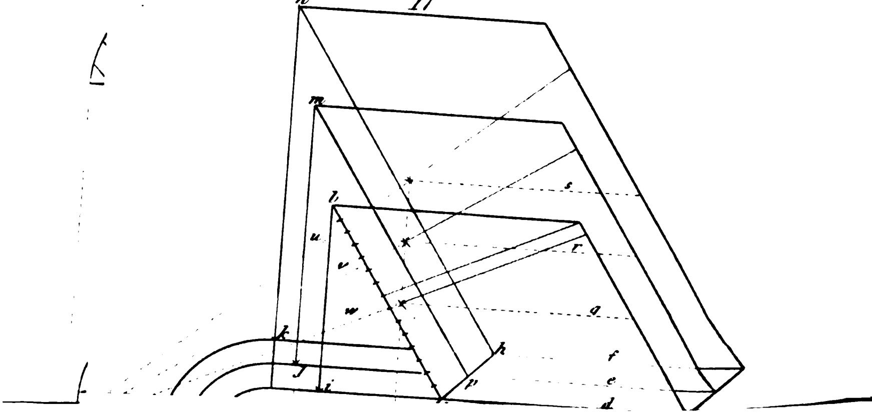

LXII. *On the Construction of Skew Arches.* By Charles Fox, *Esq.**

[With a Plate.]

SKEW bridges have hitherto been comparatively little used; but since railways have been introduced, in which it is highly important to preserve as direct and straight a line as possible, they are very frequently required, as a railway passes through the various districts without the possibility of regarding the angle at which it may cross canals and roads, its course being in great measure controlled by the natural features of the country.

Wherever a canal is thus crossed at an angle, we must either divert the canal, so as to bring it at right angles to the railway; or we must build a common square bridge of sufficient span to allow the canal, its course being unaltered, to pass uninterruptedly under it; or we must erect a proper skew bridge. The first of these is often impracticable, as provisions are generally inserted in the Acts of Parliament, for preserving the canal from any alteration in its course; and even if this were not the case, the diversion of a canal causes great expense, and is attended with much inconvenience to its traffic: the second is a most unscientific mode of overcoming the difficulty, and would also involve very serious expense, arising from the necessity of making use of an arch of much larger dimensions than would be required were the proper oblique arch erected in its stead. By referring to Plate III. figs. 1 and 2, this will be apparent: for this diagram I have selected the angle at which the London and Birmingham railway crosses the Grand Junction Canal, being an angle of 30 degrees. It is for the above reasons that oblique arches are now so frequently erected; and a good method of building them is, therefore, of considerable importance.

As many practical men with whom I am acquainted have experienced considerable difficulty in the construction of skew bridges, I was led to turn my attention to the subject; and have at length succeeded in rendering the principles of it easy to be understood.

All persons are acquainted with the manner in which common square arches are built, where all the courses are square to the face, and parallel both to the direction and surface of the road or river running under it, by which means the thrust or strain is always at right angles to the joints or beds of the

* Communicated by the Author.

individual stones composing the arch; hence the whole thrust of ordinary arches, which is brought in upon the abutments, is exerted in the direction of the bridge itself, *i. e.* of the road passing over it.

To devise some simple mode of setting out and working the courses of stone in a skew arch, so as to bring in the thrust in the proper direction, was the great object to be obtained. All practical men are aware of the vast difference between having to deal with straight and with twisted lines; and the necessity of introducing twisted lines in the construction of skew bridges will soon be seen.

In skew bridges, in order to keep the thrust in the proper direction, it is necessary to place the courses of stones at an angle with the abutment, whereby each stone loses its parallelism with the surface of the road, and is therefore laid on an inclining bed.

In a common semicircular arch each course of stones is parallel with the axis of the bridge, and all the beds are wrought so as to point to the axis: the inclination of the stones varies in every course; but although the inclination of the stones varies in every course, both ends of the course have the same inclination, both ends are equally high in the arch, and both ends point to the centre. This is the case in the ordinary bridge; but in a skew bridge, as the courses run obliquely across the arch, one end of the course is necessarily higher up the arch than the other, and therefore would no longer point to the centre; but only *make this point to the centre*, and we immediately get the twisted form, that is, we make each bed of the courses of stones a true spiral plane.

The principle which I have adopted is, to work the stones in the form of a spiral quadrilateral solid, wrapped round a cylinder, or in plainer language the principle of a square threaded screw; hence it becomes quite evident that the transverse sections of all these spiral stones are the same throughout the whole arch. It will be obvious that the beds of the stones should be worked into true spiral planes; but I am not aware that any rule has yet been published that would enable the stones to be wrought at the quarry into the desired form, or of any rule by which the true angle at which the courses cross the axis of the bridge is determined. Fig. 3. is a representation of the courses of the stones, each alternate course being omitted in order to show their form more distinctly; and the course forming the key-stone is carried out so as to show that it really is the thread of a square threaded screw wound round a cylinder, the cylinder being indicated by the two dotted lines. If the threads are cut at right angles to the cylinder, the section would appear as in fig. 4; if cut at right angles to the courses, or as nearly so as the case will admit of, as they are really cut to form the face of the bridge, the section would appear as in fig. 5.

In order that these principles may be understood, it is necessary to have a clear idea of the nature of a spiral plane; and perhaps, the best definition of it is, to consider it as being produced by the twofold motion of the radius of a cylinder, *i. e.* let a radius revolve upon its axis at an uniform velocity, and at the same time impart to it a progressive motion along the axis itself, and then by apportioning these two motions to the particular case you will obtain any spiral you may desire; hence it is apparent that the outer edge of a spiral plane is produced by a straight line wound round a cylinder everywhere forming the same angle with the axis, while the inner edge actually merges into the axis itself, which of course is a straight line. The question which now naturally suggests itself is how to decide at what angle to place these spiral stones with respect to the axis of the bridge, or in mechanical language, what traverse must we give the screw?

In entering upon the investigation of this subject, my first idea was to develop upon a plane surface all the superficies connected with a skew arch.

If a semi-cylinder be cut obliquely, the section is a semi-ellipsis, and if the semi-cylinder be then unfolded, the edge of the developed ellipsis will not be a straight line but a spiral one; and some builders not being aware of this fact, have squared a course from the face of the centring, and having drawn in the remaining courses parallel with this, have taken it for granted that all the courses would be square with the face, which it will be seen is impossible by referring to the development of the intrados, or under surface of the arch, which is the development of the centring itself: they have hereby been led into very serious and perplexing difficulties.

Having shown the impossibility of making *all* the stones square to the face, I will now give the mode of deciding in what direction they should be placed. When the soffit is developed, the edge which formed the face of the arch gives a true spiral line: my first plan was to lay the courses of stone at right angles to a line extending between the two extreme points of the spiral line of the developed soffit (see fig. 6); this line I shall afterwards speak of as the approximate line, as it is the nearest approximation to the line of the face that can be obtained by a straight line.

On further consideration I discovered a far more eligible mode of laying out the lines.

It is evident from fig. 7, that if spiral planes are considered as composed of spiral lines placed at various distances from the centre of the cylinder, each of these lines will form a different angle with the axis; and therefore, as an arch has always some thickness, that although we have the inner edge of the spiral plane placed at right angles to the thrust, yet every other portion is gradually departing from a right angle, and is, therefore, exerting its force in an improper direction: thus an arch of this description can never exert its thrust in the direction of the bridge, but is endeavouring to push the abutments obliquely.

To get the thrust strictly correct, I have supposed the arch to be cut into two rings of equal thickness (see fig. 8); and having considered the external ring as removed, have proceeded to develop the outside surface of the remaining one: this I shall hereafter speak of as the intermediate development, as it is the development of a surface midway between the extrados and soffit or intrados.

Upon this intermediate development I place the approximate line, and then draw all the courses square to it; by which means we obtain a line in the *centre* of each stone exerting its force in the true direction, and thus get rid of the disadvantage of twisted beds to the stones, as in proportion as the one half of this bed exerts its force in an oblique direction on the one hand, the other half acts in the opposite direction, and is therefore always producing a balance of effect, which resolves the various forces into one exerting all its power in the true direction, which is the object to be obtained.

Having explained the mode of setting out the beds of the stones, a little may now be said on the situation of the cross-joints: by these will be understood the joints between the various stones constituting a complete course.

Where an arch is built of stone throughout, the situation of these joints is of minor importance; but where stone is expensive, it is common to make the faces of the arch only of stone, filling in the intermediate space with brick-work; as in these instances the cross joints form the boundary between stone- and brick-work, it becomes a point of considerable importance. This is the case in the Watford viaduct; each stone here is equal in thickness to five courses of bricks, so that there are five thicknesses of mortar in the brick-work to one in the stone. Mortar always is compressed into a smaller compass when the centring is struck, and the full weight of the arch comes upon it. In consequence of this tendency, that portion of arches constructed of brick-work, always subsides much more than the stone. In an arch where stone- and brick-work are combined, little reliance should be placed on their connexion, as this is always more or less disturbed after the centring is removed, so that we should endeavour to construct each portion of the arch with its bearing surfaces or beds as nearly equal as possible.

In the first models the soffits of all the stones were made of an equal length, considering that this would present the best appearance; but this method rendered the bearing surfaces very unequal, as will be seen by fig. 9; the equal lengths being indicated by the dotted lines.

This difficulty is overcome by this simple means: instead of having the stones of equal length on the soffit, they are made so on the intermediate development, and then the areas of the bearing surfaces or beds of the stones are all equal. See fig. 10.

Having given the mode of laying out the lines, I will now proceed to the practical part, viz. the working of the individual stones.

My first idea was to commence by working the soffit; and this was the mode employed.

Having obtained an elastic mould cut to the angle at which the joints of the soffit cross the axis of the bridge, the workman by means of this gets an oblique line on that surface of the stone which he intends for the soffit. It will be understood from fig. 11, that this oblique line thus obtained will be parallel with the axis of the bridge. The workman then proceeds to chisel out a groove (or what is by masons called a chisel-draught) along this line, of sufficient depth for what he knows will be required for the hollowing of the stone.

He then takes two wooden moulds (one of which is shown in fig. 12), which are portions of the same circle as the soffit itself. A mark being placed upon the centre of each of these moulds, the workman then proceeds to sink them into the stones at right angles to this chisel-draught, (see fig. 11,) and in such a manner that the centre marks shall be in the chisel-draught, and the upper edges of the moulds, which are straight, shall be in the same plane, or what is commonly called, out of winding. It will now be obvious that these two last grooves will form true portions of the soffit itself, and therefore, that the workman has nothing to do but to work out the remainder of the stone with a straight edge, always kept parallel with the first draught, and sunk to the bottom of the two draughts which were worked by the curved moulds. Having obtained this hollowed surface, an elastic mould, of the exact size of the soffit of each stone, is pressed into it, by which the stone being marked, we obtain all the lines of the soffit itself.

It will now be quite evident that the beds may be obtained by making use of a square, one limb of which shall be made to the curvature of the soffit, and the other the radius of this curve; always taking care that this square is kept at right angles to the axis, as will be seen in figures 13, 14, and 15.

The first few stones were wrought in this manner; but finding it very difficult to prevent the workman from getting his soffit a little on one side, by which means he wasted much of the stone on one bed and rendered the other deficient, I had recourse to a method which I will describe. Having provided two straight edges, the one parallel and the other containing the angle of the twist, (see fig. 16,) we proceeded to work one of the beds by chiselling two draughts along the stone, so that these straight edges being kept at a proper distance from each other were let into the stone until they were out of winding on their upper edges.

Having finished one bed by straight edges, we then obtained the soffits and other beds by means of the square before mentioned. By working a bed first instead of the soffit, the best will always be made of a block of stone.

As we have before seen that all the stones constituting a skew arch are portions of the same square threaded screw, the workman having finished one stone has only to repeat the same operations with every other.

Any stone in the face of the arch, taken from one side, and applied to the corresponding one face to face, will continue the true spiral plane: this fact enabled us to work all the stones for one bridge in pairs; that is, one stone having been wrought with the proper twist, and of sufficient length to make two stones, was accordingly sawn in two at the proper angle: but of course this cannot be done advantageously when the stone is of a very hard nature.

It has been shown that by developing all the various surfaces, instead of having to think of complicated spiral lines, they are at once reduced to straight ones; and I will now very briefly show how simply the data necessary for the construction of a skew arch may be obtained (see fig. 17).

Let A represent the curvature of the intrados, and C the extrados, B being a line midway between A and C. Let D D, E E, F F represent the boundaries of three cylinders of which A, B, C are the transverse sections; let these cylinders be cut by the straight line G, H, at the angle of askew, that is, the angle formed by the two roads crossing each other; and from the points I, J, K, draw three straight lines at right angles to the axis, and of such lengths that I L shall be of equal length to the semicircle A, and J M equal to B, and K N equal to C; from the point O draw the straight line O L, and also from P to M: it will be seen that O L is the approximate line of the developed soffit, and P M that of the intermediate development. Add Q, R, and S, which are the centre lines of the three developments.

It will be seen that when these developments are placed as in an arch, these three lines Q, R, S being parallel with the axis, will be in a plane perpendicular to the axis, and, therefore, that all the points in each spiral will be vertical with the axis, and also with one another.

Through any point in P M draw a straight line V at right angles with P M, which straight line shall extend to the axis of the cylinder.

At the point where it intersects R, a line T perpendicular to the axis intersects R also: this last perpendicular line cuts the three lines Q, R, S at the points where the lines U, V, W, which meet in X, intersect Q, R, S.

The joints are then drawn upon the three developments parallel with the lines U, V, W, and at such distances that the lines Q, R, S shall be cut into equal parts. Of course, care must be taken to divide the approximate line of the soffit into a given number of stones. The angle X will be that which the intrados form with the axis of the cylinder, and the angle U W will give the wind of the bed. On this principle and by the rules here given, it is nearly as easy to work the stones of a skew bridge as those of any other.

Park Village East, London, March 17, 1836.

LXIII. *Further Observations on* M. Cauchy's *Theory of the Dispersion of Light. By the Rev.* Baden Powell, *M.A., F.R.S., Savilian Professor of Geometry, Oxford.*

(Continued from p. 28.)

I PROCEED to illustrate the further researches to which I alluded in my last paper; relative to the development of the theory of dispersion, and simplifying the process of M. Cauchy.

In order to consider the subject in its simplest form, let us confine our attention to a plane wave perpendicular to the axis of x, with vibrations parallel to the axis of y. Then the displacements ξ and ζ will vanish, and the differential equation of motion deduced upon M. Cauchy's principle (in my analysis, eq. (12.),) will be reduced to

$$\frac{d^2 \eta}{d t^2} = S \left\{ m \frac{\mathrm{f}(r) + \cos^2 \beta f(r)}{r} \Delta \eta \right\}; \qquad (1.)$$

where η is the value, at the time t, of the varying displacement of the molecule m, whose rectangular coordinates when in equilibrium are $x\ y\ z$; $\eta + \Delta \eta$ is the displacement at the same moment t, of another molecule m, which has for its rectangular coordinates when in equilibrium

$$x + \Delta x \qquad y + \Delta y \qquad z + \Delta z, \quad \text{while}$$
$$r = \sqrt{\Delta x^2 + \Delta y^2 + \Delta z^2},$$

or the distance between these two molecules in their positions of equilibrium; β is the angle between this distance r and the axis of y; and, finally, $\mathrm{f}(r)$ and $f(r)$ are functions of r, of which the former (if positive) expresses the law of attraction, or (if negative) the law of repulsion, and the latter is derived from it by the rule

$$f(r) = r\,\mathrm{f}'(r) - \mathrm{f}(r).$$

S, the sign of summation, is relative to the actions (attractive or repulsive) of all the molecules m.

I have recapitulated thus far in reference to what was established at the outset of M. Cauchy's investigations. Now this analysis is thus far devoid of all difficulty or intricacy; the whole difficulty of the subject lies in the *integration* of these equations of motion. The integration given by M. Cauchy is of an extremely general kind: but for the purpose we have now more immediately in view, it will be readily allowed that if a particular solution were proposed, such as to include the establishment of the relation between μ and λ, it would suffice. A valuable instance of a method of effecting such a simplification has been laid before the readers of this Journal, in the excellent paper of Mr. Tovey in the Number for January, p. 7. But another such particular solution has been pointed out by Sir W. R. Hamilton, the nature of which I now proceed to describe; and this will be most perspicuously done in the following manner:

It will be easily seen that all the conditions of a wave for the ordinary phænomena are fulfilled by such a function as

$$\eta = A + B \cos\left(\frac{2\pi}{\tau}(\mu x - t)\right) + C \sin\left(\frac{2\pi}{\tau}(\mu x - t)\right); \quad (2.)$$

which, merely by the assumption of the coefficients and trigonometrical operations, is easily put under the form

$$\eta = \eta_0 + \eta_1 \cos\left(\frac{2\pi}{\tau}(\mu x - t + t_0)\right), \qquad (3.)$$

t_0 being entirely arbitrary, and $\eta_0\ \eta_1$ being also arbitrary, but small; η_0 is introduced only for greater generality.

Differentiating in respect of t, we shall have

$$\frac{d^2\eta}{dt^2} = -\left(\frac{2\pi}{\tau}\right)^2 \eta_1 \cos\left(\frac{2\pi}{\tau}(\mu x - t + t_0)\right). \qquad (4.)$$

Also, by the method of finite differences, we have

$$\Delta\eta = \begin{cases} -2\eta_1 \cos\left(\frac{2\pi}{\tau}(\mu x - t + t_0)\right)\left(\sin\frac{\pi\mu\Delta x}{\tau}\right)^2 \\ -\ \eta_1 \sin\left(\frac{2\pi}{\tau}(\mu x - t + t_0)\right)\left(\sin\frac{2\pi\mu\Delta x}{\tau}\right) \end{cases} \qquad (5.)$$

Now (on precisely the same grounds as those adverted to in the analysis of Cauchy for deducing the equations (22.),) it will be seen that this expression is of such a form that if it were introduced in a summation, since we may assume half the values of Δx as positive and half as negative, the second member involving the first power of the sine of a function of Δx, and the first member the square, the sums of all the values in the second member will destroy each other, but not those in the first.

Thus on substituting this value of $\Delta\eta$ in the differential equation (12.), or that above, (1.), we shall only have to take into account the first member, multiplied by the function of (r); and it will thus easily appear that that equation (1.) is satisfied by these values derived from the assumed equation of the wave (3.), provided we suppose

$$\left(\frac{2\pi}{\tau}\right)^2 = \mathrm{S}\left\{2m\,\frac{\mathrm{f}(r) + \cos^2\beta f(r)}{r}\left(\sin\frac{\pi\mu\Delta x}{\tau}\right)\right\}; \quad (6.)$$

or, in other words, the equation (3.) coupled with this last condition (6.) is a particular solution of the differential equation of the motion of a system of molecules (1.).

But also, this equation (6.) involves the relation between τ and μ, (or between λ and μ, since we have $\frac{\lambda}{\tau} = \frac{1}{\mu}$,) which is expressed by writing, for abridgement,

$$\left.\begin{aligned} \frac{\pi\,\mu\,\Delta x}{\tau} &= \theta \\ \text{and} \qquad \frac{m}{2}\,\frac{\mathrm{f}(r) + \cos^2\beta\, f(r)}{r}\,\Delta\,x^2 &= \mathrm{H}^2 \end{aligned}\right\} \qquad (7.)$$

(though H² is not necessarily positive), which, since

$$\frac{2\,\pi}{\tau} = \frac{\theta}{2\,\mu\,\Delta\,x},$$

will give the relation

$$\left(\frac{1}{\mu}\right)^2 = \mathrm{S}\left\{\mathrm{H}^2\left(\frac{\sin\theta}{\theta}\right)^2\right\}, \qquad (8.)$$

the same as that formerly deduced.

Such is the outline of the simplification proposed: I have only to regret that these and the other researches connected with the same subject have not been brought before the public by the author himself in the more complete form in which he could have clothed them. But, as they are, I trust neither he nor the mathematical reader will think I have done wrong in adopting this mode of availing myself of his permission to make use of them. On the same ground I will add another brief investigation from the same source connected with the fundamental formula of dispersion.

I have before observed that for low dispersive substances, at least, the simple approximate formula appears quite sufficient. As it may, therefore, be useful for a very large number of cases, it will not be unimportant to dwell upon it, and to state a very simple practical rule resulting from it, which completely removes the difficulties of the computation as conducted by the methods I formerly adopted.

In the first place, from the nature of the formula the following considerations will be readily evident. Taking any two rays whose indices are $\mu\ \mu_1$, and length of waves $\lambda\ \lambda_1$, let us write the arc

$$\frac{\pi\,\varrho\cos\delta}{\lambda} = \theta.$$

Then we have

$$\frac{\pi\,\varrho\cos\delta}{\lambda_{\prime}} = \theta\,\frac{\lambda}{\lambda_{\prime}};$$

and by the approximate formula

$$\frac{1}{\mu} = \mathrm{H}\left(\frac{\sin\theta}{\theta}\right),$$

$$\frac{1}{\mu_{\prime}} = \mathrm{H}\left\{\frac{\sin\left(\theta\frac{\lambda}{\lambda_{\prime}}\right)}{\left(\theta\frac{\lambda}{\lambda_{\prime}}\right)}\right\}.$$

Then developing the sine and dividing by the arc

$$\frac{\mu}{\mu_{\prime}} = \frac{1 - \frac{1}{6}\left(\theta\frac{\lambda}{\lambda_{\prime}}\right)^2 +,\&c.}{1 - \frac{1}{6}\quad(\theta)^2 \quad +,\&c.}$$

And for a first approximation, neglecting the terms above two dimensions, this will be easily reduced to

$$\frac{\mu}{\mu_{\prime}} = 1 + \frac{\theta^2}{6} - \frac{\theta^2}{6}\frac{\lambda^2}{\lambda_{\prime}^2},$$

whence we obtain

$$\theta^2 = \frac{6\left(1 - \frac{\mu}{\mu_{\prime}}\right)}{\left(\frac{\lambda}{\lambda_{\prime}}\right)^2 - 1}.$$

Hence the practical method referred to will be as follows:

Let there be assumed a subsidiary arc ϕ such that

$$\frac{\mu}{\mu_{\prime}} = \cos^2\phi,$$

then by substituting in the last formula, we have

$$\theta^2 = \left\{\frac{6}{\left(\frac{\lambda}{\lambda_{\prime}}\right)^2 - 1}\right\}(\sin^2\phi),$$

and

$$\log\theta = \tfrac{1}{2}\log\left\{\frac{6}{\left(\frac{\lambda}{\lambda_{\prime}}\right)^2 - 1}\right\} + \log\sin\phi.$$

And since $\frac{\mu_{\prime}}{\mu} = \sec^2\phi$, we have also

$$\log.\sec.\phi = \tfrac{1}{2}(\log\mu_{\prime} - \log\mu).$$

These logarithmic formulas enable us to perform the approximate calculation with the greatest ease.

LXIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

Anniversary Proceedings, February 19th, 1836.

AFTER the usual Reports had been read, (which are printed in the "Proceedings" of the Society,) the President announced the award of the Wollaston Medal and Proceeds for the past year; and, in doing so, said,

Gentlemen,

You have learnt from the Report of the Council that the Wollaston Medal has been awarded to Mr. Agassiz of Neuchatel for his work on Fossil Ichthyology, and that the sum of 25*l.* from the Donation Fund has been awarded by the Council to Mr. Deshayes in promotion of his labours in fossil conchology. I shall now proceed to request Mr. Broderip to communicate this adjudication to his friend Mr. Agassiz, and I shall deliver in charge to our Foreign Secretary, Mr. De la Beche, the sum which has been voted to Mr. Deshayes.

The President then addressed Mr. Broderip in these words:—

Mr. Broderip,

I have great pleasure in requesting you to inform Mr. Agassiz, of Neuchatel, that the Council of the Geological Society have awarded the Wollaston Medal to him for his work of last year on Fossil Ichthyology. On a former occasion we presented the proceeds of the Donation Fund for one year to the same distinguished naturalist, to assist him in the publication of the early part of his great work, the importance of which was then only beginning to be known to the scientific world.

It will ever be a subject of gratification to us to have learnt that this small pecuniary aid was not without its influence in accelerating the publication of his "Researches on Fossil Fish," arriving as it did opportunely at a moment when the funds which could be appropriated for the undertaking were nearly exhausted. Mr. Agassiz acknowledged at the time his obligation to us for a mark of sympathy and regard which he received so unexpectedly from a foreign country, and which cheered and animated him to fresh exertions. You will have the kindness to acquaint him that the Council in now awarding the Medal to him, are desirous that he should possess a lasting testimony of their esteem and of the high sense which they entertain of the merit of his scientific labours.

Mr. Broderip replied,—

Sir.—I accept the trust: and permit me, on the behalf of Professor Agassiz, to offer his best thanks to the Society for the seal which it has this day set on the powerful zoological lever which he has placed in the hands of Geologists.

This crowning gift will be doubly precious to him when he reflects on the high character of those who have awarded it, and hears of the expressions with which you, Sir, have been pleased to accompany it.

These, he will feel, are the incentives

"that the clear spirit do raise
To spurn delights and live laborious days."

He will look upon the illustrious head that gives dignity to the gold—upon the representation of "that dark eye" before whose glance, as it has been eloquently said by one of your predecessors, all false pretensions withered—and the sight will inspire him with new energies.

The President then addressed Mr. De la Beche in these words:—

Mr. De la Beche,

It is now my duty to deliver into your hands as Foreign Secretary of this Society the sum of 25*l.*, and it is with great satisfaction that I request you to inform Mr. Deshayes of Paris that this portion of the proceeds of the Wollaston Donation Fund has been awarded to him by the Council for the promotion of his labours in Fossil Conchology. I beg that you will express to Mr. Deshayes at the same time how highly we appreciate the services which he has already rendered to Geology by his description of the fossil shells of the strata above the chalk, to which he has chiefly, although not exclusively, devoted his attention; and we rejoice to hear that he is now engaged in the investigation of the fossil shells of the older formations.

We are not ignorant that he has prosecuted his scientific studies with zeal and enthusiasm under circumstances of considerable difficulty; and we trust that the notice thus taken of his labours may encourage him to persevere in devoting the powers of his mind and his great acquirements to a department of science so eminently subservient to the advancement of Geology.

Mr. De la Beche on receiving the donation expressed the pleasure which it gave him to be requested to communicate the intelligence to Mr. Deshayes, and the satisfaction which he felt in publicly avowing his approbation of the award of the Council.

The following gentlemen were elected the Officers and Council for the ensuing year.

Officers.—*President,* Charles Lyell, jun. Esq. F.R.S. & L.S.: *Vice-Presidents,* Rev. William Buckland, D.D. F.R.S. Professor of Geology and Mineralogy in the University of Oxford; Sir Philip de Malpas Grey Egerton, Bart. M.P. F.R.S.; George Bellas Greenough, Esq. F.R.S. & L.S.; Edward Turner, M.D. F.R.S. L. & E. Professor of Chemistry in the University of London: *Secretaries,* William John Hamilton, Esq.; Woodbine Parish, jun. Esq. F.R.S.: *Foreign Secretary,* Henry Thomas De la Beche, Esq. F.R.S. & L.S.: *Treasurer,* John Taylor, Esq. F.R.S.

Council.—Francis Baily, Esq. F.R.S. & L.S.; William John Broderip, Esq. F.R.S. & L.S.; William Clift, Esq. F.R.S.; Sir A. Crichton, M.D. F.R.S.; William Henry Fitton, M.D. F.R.S. & L.S.; Henry Hallam, Esq. F.R.S.; Robert Hutton, Esq.; Roderick Impey Murchison, Esq. V.P.R.S. F.L.S.; Viscount Oxmantown, F.R.S.; John Forbes Royle, Esq. F.L.S.; Rev. Adam Sedgwick, Woodwardian Professor in the University of Cambridge, F.R.S. & L.S.; Lieut.-Col.

W. H. Sykes, F.R.S. & L.S.; Henry Warburton, Esq. M.P. F.R.S.; Rev. William Whewell, F.R.S. & L.S.

The President subsequently delivered the following

ADDRESS.

GENTLEMEN,

You have learnt this morning, from the annual report of the Council, that the financial affairs of the Society continue to flourish; and that since our last anniversary we have published the concluding part of the third volume of our Transactions, and the first part of a fourth volume. Another part of the same volume is nearly ready, and the Council have directed their thoughts seriously to the means of preventing, in future, the accumulation of such heavy arrears of unpublished memoirs. The delays have hitherto arisen from a desire to print all papers containing original and valuable matter in the order in which they were presented; but many have been sent to us in so unfinished a state as to retard the printing of the rest, and, as the science advances rapidly, and new facts pour in daily, the authors even of the most finished memoirs soon require to make additions and corrections, and thus the evil is continually augmenting. The Council have therefore resolved, for the future, to print at once those memoirs which are in the most complete state, without waiting for others which are imperfect.

During the last year there have been elected into the Society 45 new members, and we have lost 4 by resignations and 12 by deaths. Among the names of the deceased Fellows I may mention those of Mr. Goodhall and Mr. Mammatt as having zealously contributed to the progress of our science. Mr. Goodhall was an active collector of British fossils, and to his labours we owe many valuable contributions to our museum, and the discovery of shells of new species figured in Sowerby's Mineral Conchology. The work of Mr. Mammatt, on the Coal-field of Ashby-de-la-Zouch, has been honourably mentioned by my predecessor Mr. Greenough, in his last anniversary speech. Mr. Mammatt had superintended, for more than thirty years, the working of extensive coal mines, and kept a record of the details of various sections with which he was practically acquainted. To these documents he has added several plans of remarkable faults which intersect the carboniferous strata of Leicestershire. He has shown that on one side of one of these faults the beds rise to the height of 500 feet above the corresponding beds on the other side, yet the mass of uplifted strata does not project above the general level of the country. He infers, therefore, that it has been removed by denudation, and that the wreck of it alone now remains on the surface in the shape of sand and boulders. Mr. Conybeare has drawn similar conclusions respecting analogous phænomena observed on a still greater scale in the Newcastle coal district.* Whether the denudation was sudden or gradual, or whether the faults were produced at once or were the result of a series of movements, are points which the limits of this discourse will not

* Report on Geology to the British Association, 1832.

allow me to discuss at present. Mr. Mammatt contends that these enormous shifts were not effected by volcanic convulsions, but simply by a quiet and uniform operation accompanying the desiccation, shrinking, and induration of dense masses of argillaceous and other rocks, an opinion which, however ingenious, seems irreconcileable with the evidence of violent disruption with which this and other coal-fields abound. Mr. Mammatt's volume is illustrated by more than one hundred plates of fossil plants, but it is much to be regretted that before executing such costly illustrations the author did not obtain the assistance of a skilful botanist, who might have selected the most important and might have added descriptions, without which mere figures can scarcely ever convey accurate information.

Early in the spring of last year an application was made by the Master General and Board of Ordnance to Dr. Buckland and Mr. Sedgwick, as Professors of Geology in the Universities of Oxford and Cambridge, and to myself, as President of this Society, to offer our opinion as to the expediency of combining a geological examination of the English counties with the geographical survey now in progress. In compliance with this requisition we drew up a joint report, in which we endeavoured to state fully our opinion as to the great advantages which must accrue from such an undertaking, not only as calculated to promote geological science, which would alone be a sufficient object, but also as a work of great practical utility, bearing on agriculture, mining, road-making, the formation of canals and rail-roads, and other branches of national industry. The enlightened views of the Board of Ordnance were warmly seconded by the present Chancellor of the Exchequer, and a grant was obtained from the Treasury to defray the additional expenses which will be incurred in colouring geologically the Ordnance county maps. This arrangement may justly be regarded as an economical one, as those surveyors who have cultivated geology can with small increase of labour, when exploring the minute topography of the ground, trace out the boundaries of the principal mineral groups. This end, however, could only be fully accomplished by securing the cooperation of an experienced and able geologist, who might organize and direct the operations: and I congratulate the Society that our Foreign Secretary, Mr. De la Beche, has been chosen to discharge an office for which he is so eminently qualified.

At the same time that measures are thus in train for completing a Geological Map of England on a magnificent scale, the Map of Scotland, by Dr. MacCulloch, which has been so long and impatiently expected, is at length on the eve of publication. But at the moment when I can announce this welcome intelligence we have to deplore the sudden loss of this distinguished philosopher. The first paper in the first volume of our Transactions was from the pen of Dr. MacCulloch, and subsequent volumes contain no less than eighteen of his memoirs*. It would lead me far beyond

* [Three of these papers by Dr. Macculloch will be found at large in Phil. Mag. First Series: viz. "On the Sublimation of Silica," in vol. lxiv. p. 441; "On Staffa," ibid. p. 445; and "On certain Products obtained in the Distillation of Wood," in vol. xlv. p. 203.]

my present limits were I to attempt to give a general analysis of these, and of his numerous other works on geology, such as his Western Islands and his Classification of Rocks. The influence exerted by them on the progress of our science has been powerful and lasting, yet they have been less generally admired and studied than they deserve. Their popularity has been impaired by a want of condensation and clearness in the style, a defect which no one could more easily have remedied than the author, had he been willing to submit to the necessary labour. Another blemish has also contributed to give a repulsive character to some of his later productions, especially his System of Geology, the absence, or apparent absence, of all enthusiasm and love for his subject, and a disposition to neglect or speak slightingly of the labours of others, and even to treat in a tone bordering on ridicule some entire departments of science connected with geology, such as the study of fossil conchology. I attribute these imperfections principally to habitual ill health acting upon a sensitive mind, for certainly, Dr. MacCulloch's spirits were much depressed by bodily sufferings when I had first the pleasure of knowing him, about the year 1825. His imagination was then haunted with the idea that his services in the cause of geology were undervalued, and it was in vain to combat this erroneous impression. After that period he almost entirely withdrew himself, even when residing in London, from all personal intercourse with the most active geologists; and to those who knew him this seclusion from scientific society was a subject of frequent regret. Having expressed myself thus unreservedly on some of the peculiarities and defects of his style, I may affirm that as an original observer Dr. MacCulloch yields to no other geologist of our times, and he is perhaps unrivalled in the wide range of subjects on which he displayed great talent and profound knowledge. For myself I may acknowledge with gratitude that I have received more instruction from his labours in geology than from those of any living writer.

One of the most important communications which we have received for many years is an essay by Professor Sedgwick on the changes of structure produced in stratified rocks after their deposition. Respecting the magnesian limestone, he has confirmed by new arguments the conclusions which he formerly drew, in proof that the complicated concretions of this rock have been produced since the original deposition of the beds. But the principal part of his memoir is devoted to the description of the cleavage or slaty structure of rocks, and those partings which have been called joints. The author first shows the analogy of the Cumbrian zone of green slate and porphyry with the structure of the principal chain of North Wales. In these regions, as in part of the slaty series of Westmoreland and Lancashire, occur many beds exhibiting a slaty cleavage, which the Professor distinguishes from a jointed structure. Joints, he says, are fissures placed at definite distances from each other, the masses of intervening rock having no tendency to cleave in a direction parallel to such fissures: whereas in the planes of cleavage, the rock is capable of indefinite subdivision in a direction parallel to such planes. The planes of stratification, on the other hand, are perfectly distinct from both, and throughout the district alluded to have never been found to coincide with the lines of cleavage, dipping sometimes to the same point and sometimes to opposite points of the compass, but being always inclined to them at an angle of from 10° to 30° or 40°, and in no instance at 90°. There are regions in North and South Wales thirty miles in extent, and many miles in breadth, where the cleavage planes preserve an undeviating dip and direction notwithstanding that they traverse strata which are greatly contorted.

In that variety of slate-rock which is used for roofing, all traces of original deposition or stratification are often obliterated; yet in many quarries, a number of parallel stripes are discovered, sometimes of a lighter and sometimes of a darker colour than the general mass. These stripes, says the Professor, are universally parallel to the true beds, whenever such beds can be discovered, whether by organic remains, by the alternations of similar deposits, or other ordinary means. Many of these beds are of a coarse mechanical structure, others are fine chloritic slate; but the coarser beds and the finer, the twisted and the straight, have all been subjected to one change, a crystalline cleavage passing alike through all. Some of the sections given show the cleavage planes preserving an almost geometrical parallelism while they pass through curved strata, of which the sedimentary origin is obvious. In another place it is said that where the slaty cleavage is very perfectly brought out the rocks always make an approach to homogeneity, but where the coarse beds predominate the slaty structure almost entirely disappears. Dr. Boase in his comments on these passages has remarked that they seem inconsistent with each other, and I confess that at first they struck me in the same light; but the Professor has explained to me that although the coarse beds are not slaty, they have a grain parallel to the cleavage planes of the finer beds, this grain being exhibited when they are struck with the hammer; and it is only when the materials of the beds are very coarse that the cleavage planes entirely vanish.

In regard to the origin of these phænomena, the author supposes that crystalline or polar forces must have acted on the whole mass simultaneously in given directions, and that the action being carried on at once through a very large mass of matter may have acquired an accumulated intensity of crystalline action in each part, so that the whole intensity of crystalline force, modifying the mass, may not have been equal to the sum of the forces necessary to crystallize each part independently, but may have been some function of that sum whereby it may have been increased almost indefinitely.

I regret that I have not space to do justice to this ingenious speculation, nor have I yet had sufficient opportunities of observation to know whether we shall be able to distinguish generally, with precision, those slates which are diagonal to the strata, from those flagstone-slates, as it is proposed to term them, which are parallel to the layers of deposition. During the last summer I observed in the Swiss Alps that the fissile roofing-slate and drawing-slate of the Niesen, in the Canton of Berne, divides into extremely thin laminæ, which are *parallel* to the true planes of stratification. The direction of the beds is shown by alternations of coarse and clearly mechanical strata of a kind of greywacke, the whole series belonging to the Green Sand or fucoid grit formation. If it be said that these slates may owe their laminated texture to extremely minute flakes of talc, mica, or some other foliated mineral which may have fallen as sediment and have been all deposited on their flat surfaces, I reply, that in that case they would exemplify the exact similarity of certain acknowledged slates of deposition to others which have originated in crystalline forces independent of sedimentary action. Mr. Murchison, after confirming the truth of the Professor's observations as applied to all those regions of Wales which have come within his survey, has pointed out what might by some be considered an exception to the rule in a part of the slate-rocks of Pembrokeshire, where the planes of slaty cleavage are coincident with the true laminæ, as proved by colour and the alternation of various layers of deposit. Mr. Murchison states, however, that although these rocks are quarried as roofing-slates, and are a part of the older system, they may be classed by Mr. Sedgwick as fine flagstones.

Some confusion will, I fear, arise from attempting to restrict the term slate to those cases alone where the cleavage is oblique to the stratification; but whatever nomenclature we adopt, it is clear from the excellent paper of the Professor, that three distinct forms of structure are exhibited in certain rocks throughout large districts: namely, first, stratification; secondly, joints; and thirdly, slaty cleavage; the last having no connexion with true bedding, and being superinduced by a cause absolutely independent of gravitation. These different structures must have different names, even though there may be cases, and I believe there will be many, where it is impossible, after carefully studying the phænomena, to decide upon the class to which they belong.

One curious consequence, but slightly alluded to by the author, appears to follow from the facts described, namely, that the slaty structure must have commenced at a period posterior to the last series of violent movements which dislocated the strata and threw them into anticlinal and synclinal lines. Such disturbances would have deranged the parallelism of the cleavage planes. If, therefore, there are proofs, as I believe there are, of the elevation or subsidence of these rocks since they assumed the slaty structure, the whole country must have been moved bodily, or the separate masses, if they changed their relative position, must have moved in such directions as to allow the dip of the cleavage planes to remain unaltered.

It is with pleasure that I next call your attention to the investigations which Mr. Murchison has been steadily pursuing in the older fossiliferous rocks of Wales and the bordering counties of England. He has at length brought his survey of five years to a successful termination; and his work will form a most important step in the progress of geological science, not merely as elucidating the history of a portion of the sedimentary formations of our island, but as fixing the characters of a succession of normal groups to which the strata of other parts of Europe, and perhaps of America, may be referred. A large and beautifully illustrated treatise, in which he intends to give a detailed description of his original observations and views, will soon be published. In the mean time we have tasted, as it were by anticipation, the fruits of his labours, having, year after year, received at our meetings the earliest intelligence of his discoveries, and having freely discussed and criticized them long before it has been possible for him to lay the whole in a matured and digested form before the public. You are aware that the system of rocks, which have been the chief object of his research, constitutes the upper part of what was formerly called the transition or greywacke series. In these strata, which had previously remained in a state of obscurity and confusion, he has distinguished several formations. The old red sandstone rests conformably on the uppermost of these, while the lowest of them repose both conformably and unconformably on the ancient slate-rocks of Wales. Mr. Murchison proposes the general name of "Silurian" for this whole system, as the strata may best be studied in those parts of England and Wales once occupied by the ancient British nation the Silures.

The necessity of a new term has arisen from the uncertain latitude with which the word "transition" had been applied, some authors including in it the carboniferous rocks, and also from the still greater confusion introduced by the word "greywacke," a term which can only be employed conveniently, in a mineralogical sense, to designate a peculiar kind of rock which has been formed at many successive epochs. Thus, for example, in the memoir now under review, it is shown that in Pembrokeshire grits, which have passed for greywacke, occur in the true coal-measures, in the old red sandstone, in the Silurian, and in the still older systems of rock.

Below the Silurian strata are slate-rocks of older date, in which traces of organic remains have been again detected; and Professor Sedgwick has suggested the name of Cambrian for this more ancient system, which is conterminous over a wide territory with the Silurian formations, the relative position of both being clearly seen.

Mr. Murchison has recently traced the Silurian system running in zones through Pembrokeshire, and there rising out in the coast cliffs from beneath the old red sandstone as conformably as in the interior of the country,—an important verification of the accuracy of his previous determinations. Great lithological changes are, however, observed to take place in these localities, so distant from the best types of the system; thus, the "Ludlow and Wenlock" formations are no longer distinctly separated by subordinate limestones, and are therefore simply termed the "upper Silurian rocks," and these, changing their soft argillaceous characters of "mudstone," become hard sandstones, yet contain some well-known organic remains; whilst the lower Silurian rocks, or Caradoc and Llandeilo formations, not only maintain their usual fossil distinctions, but exhibit limestones of much greater thickness than in any other part of their course. Mr. Murchison has also shown that rocks occupying a large coast tract in Pembrokeshire, which from their mineral aspect had been laid down as "greywacke", consist of true coal-measures. After noticing a ridge of intrusive rocks in Caermarthenshire, between the Towey and the Taf, as connected with certain great lines of dislocation, he points out, in the Cambrian System of Pembrokeshire, examples of the existence of two classes of trap rock, one bedded or contemporaneous, the other amorphous and of posterior intrusion. He further shows that the main directions of the stratified deposits of this county are parallel to divergent zones of trap.

In another paper the same author states that he has lately discovered to the north-west of Shrewsbury, proofs of an eruption of trap posterior to the new red sandstone, and probably to the lias. This line of fissure along which he has observed the new red sandstone affected for a distance of thirty miles is on the precise prolongation of a linear eruption in the Breiddin Hills, which he had previously pointed out as having been in progress during and after the epoch of the deposition of the Silurian strata. The more modern trap is made up of a peculiar felspathic rock identical with some of those at the great vent of eruption fifteen miles distant, where they both alternate with and are intruded into the more ancient deposits.

It appears from these observations that volcanic operations were renewed along the same line after a wide interval of time, showing that we must be on our guard against inferring the synchronism of coincident lines of derangement. The repetition also in the same spot and at two distant periods of a trap identical in mineral character is curious, and reminds me of an opinion lately mentioned to me by Mr. Von Buch, that the composition of lava is often determined by that of preexisting volcanic rocks near the point of eruption. Thus on two opposite sides of the same volcano, as on Teneriffe for example, a trachytic flow of lava will issue from a mass of trachyte, and a basaltic flow from rocks of basalt.

Mr. De la Beche has shown that the trappean rocks are associated in such a manner with the new red sandstone of part of Devonshire,—among other places, near Tiverton and Exeter,—as to indicate that the trap and the sandstone were each in the course of formation at the same period. Some beds of sand present every appearance of having been of volcanic origin, and ejected from a crater, but the sand became mixed with common detrital matter then in process of deposition at the bottom of the sea. Numerous angular fragments, some of them even one or two tons in weight, of quartziferous porphyry with a felspathic base, are intermingled with the conglomerates of the old red sandstone, and do not resemble any trappean rocks discovered in place in this district. Mr. De la Beche conjectures with much probability that these fragments were ejected from volcanic vents, and that they fell upon the sand and pebbles then in the course of deposition around such vents, and were thus included. The author has not failed to show that the original features of the bed of the sea, of the period of eruption alluded to, have been obliterated by subsequent denudation; and I may suggest that this cause has often prevented geologists from recognising the analogy of trappean phænomena to those of submarine and insular volcanos now active *.

In another communication Mr. De la Beche informs us that the "Cornish grauwacké," in which term he here comprises the slates of that country and their associated sandstones and conglomerates, contains in some places organic remains. Specimens of these fossils have been presented by him to our museum. He also states that this greywacke formation, which extends into Somerset and Devon, is older than Mr. Murchison's Silurian system, and may be subdivided into natural sections, coinciding perhaps with some observed by Professor Sedgwick in the Cambrian group. The slates of Tintagel, long since known to be fossiliferous, belong to the same age as this greywacke of Cornwall.

A joint paper by Professor Sedgwick and Mr. Williamson Peile has made us acquainted with the carboniferous limestone flanking the primary Cumbrian mountains, and with the coal-fields of the north-west coast of Cumberland. These carboniferous strata rest unconformably on the primary Cumbrian slates. The carboniferous series is divided into four groups: 1st, The great scar limestone; 2nd, Alternations of limestone, shale, and coal; 3rd, Millstone grit; 4th, Great upper coal formation. It appears that the structure of the carboniferous limestone is nearly the same as that of the Yorkshire chain so admirably described by Professor Sedgwick in the first part of our fourth volume just published.

Mr. Griffith, who has for so many years been preparing a geological map of Ireland, has described to us the position of some veins of syenite which traverse the mica-slate and chalk near Fair Head in the county of Antrim. The syenite is composed of dark green crystallized hornblende and brownish red felspar, with occasional grains of quartz; and the chief point of interest consists in the circumstance that the syenitic veins have the appearance in general of being regular beds in the mica-slate, being for the most part conformable both in strike and dip. They are found, however, when more closely examined and traced for some distance, to deviate from the stratification of the mica-slate, and to have an indented and saw-like edge at their junction. Similar syenitic veins also penetrate through the chalk in the neighbouring part of the coast, and near their contact with the chalk nodules or spheroidal masses of syenite are occasionally observed so isolated and surrounded by chalk that had not the intruding veins clearly proved its posteriority, the syenite might be mistaken for the older rock, rounded fragments of which had been imbedded in the calcareous stratum. These phænomena remind us of the isolated nodules of granite which in Cornwall, the Valorsine, and other countries, occur in the immediate vicinity of granite veins.

* [See on this subject Lond. and Edinb. Phil. Mag., vol. vii. p. 515.]

I have next to call your attention to an able sketch of the geology of Denmark, which you will find at some length in our Proceedings, from the pen of an eminent Danish naturalist, Dr. Beck, of Copenhagen. He describes in Bornholm, besides the granitic and Silurian rocks, certain strata which appear to agree with our Wealden group in mineral character and fossil plants, some of these being the same as those found in the Hastings sands, although the shells are marine. In Bornholm this formation is characterized by containing coal. The most remarkable feature in the geology of Denmark Proper is the great development of the cretaceous system above the white chalk with nodular flints. In the island of Seeland the ordinary white chalk is covered with a hard yellowish limestone containing some fossils of the white chalk and others peculiar to itself, especially univalves of the genera Trochus, Fusus, Voluta, Oliva, Cypræa, and Nautilus. At Faxoe this rock consists of an aggregate of corals of unknown depth, but certainly more than forty feet thick. When I myself visited the Faxoe quarries in 1834 in company with Dr. Forchhammer, the rock struck me as agreeing with the description usually given of the limestone in recent coral reefs. The fossil zoophytes of Faxoe are often cemented together by white chalk, which may recall to your recollection the recent chalk which Lieut. Nelson has presented to our museum from the coral reefs of the Bermudas. This recent substance is not distinguishable from some of the white marking chalk of England, and like it is composed of pure carbonate of lime. It is in fact a white earthy mud, known to be derived from the decomposition of the softer corallines, such as Eschara, Flustra, and Cellepora. These observations support an opinion which has long been entertained by some geologists that all chalk may be derived from the decomposition of shells and zoophytes.

While on this subject I may mention a discovery made by Mr. Lonsdale during the last summer, and which he has permitted me to announce. In arranging our collection he has found that our common white chalk, especially the upper portion of it, taken from different parts of England, (Portsmouth and Brighton among others,) is full of minute corals, foraminifera, and valves of a small entomostracous animal resembling the Cytherina of Lamarck. From a pound of chalk he has procured, in some cases, at least a thousand of these fossil bodies. They appear to the eye like white grains of chalk, but when examined by the lens are seen to be fossils in a beautiful state of preservation.

According to Dr. Beck there is a whitish and hard chalk above the Faxoe beds almost entirely made up of pulverized zoophytes including bivalves and Echini, chiefly of the same species as those of the white chalk with flints, and with corals like those of Faxoe. There are layers of flint or chert in this upper division. These conclusions, drawn from a careful examination of an extensive series of the Danish fossils, are very important, for it was formerly imagined by Dr. Forchhammer that the Faxoe beds and the overlying chalk belonged to the calcaire grossier, an idea suggested by the generic resemblance of the shells to those of the tertiary deposits. But none of the species, according to Dr. Beck, agree with any known tertiary fossils, and the secondary genera Ammonite and Baculite occur among the Faxoe shells. Some of the Faxoe corals agree with those of Maestricht, and the newest of the cretaceous formations of Seeland and Jutland agree more nearly with those commonly called the Maestricht beds than with any previously known. Dr. Beck, however, says that the organic remains differ on the whole from those of Maestricht, and are more analogous to those found at Künruth near Liege.*

The cliffs of Möen, one of the Danish islands, are composed of white chalk with nodular flints. The fossils agree with those of the chalk of England and France, as was shown in the year 1827 by the list of more than one hundred species of them given by Dr. Beck in Leonhard's Taschenbuch der Mineralogie. Two years before, Dr. Forchhammer had published in the Transactions of the Royal Danish Academy his opinion respecting Möen, and extracts from his paper afterwards appeared in the Edinburgh Journal of Science for July 1828. He then considered the Möen chalk to be an integral part of the same tertiary deposit of sand and clay which contains erratic blocks in Denmark; and in confirmation of this opinion he gave sections representing an alternation of chalk with beds of tertiary sand, clay, and loam. Being desirous of inquiring into this singular phænomenon I visited the Möen cliffs in company with Dr. Forchhammer in 1834, and came to a different conclusion. I have explained to the Society my reasons for inferring that the association of the cretaceous and tertiary deposits may be referred to the violent disturbances which the chalk strata have undergone. The cretaceous beds are curved, vertical, or shifted, and, upon the whole, more deranged than the chalk in Purbeck or the Isle of Wight. In fact the movements have been on so great a scale that masses of the overlying clay and sand have subsided bodily into large fissures and chasms, intersecting the chalk to the depth of several hundred feet. Some of the intercalations of clay and sand in the midst of great masses of unconformable chalk can only, I think, be explained by supposing engulfments of superincumbent matter, such as are described to occur during earthquakes. These appearances are analogous to those exhibited by masses of chalk nearly enveloped in crag near Trimmingham in Norfolk, although the Danish phænomena are on a much grander scale. Dr. Forchhammer did not fully concur in these opinions in 1834, but he appears to have since adopted them for the most part, in an excellent memoir on the geology of Denmark, a copy of which has been lately sent by him to the Society, accompanied by a small coloured map of the whole of Denmark and Bornholm.

As the fossils of the upper cretaceous series of Denmark are very peculiar, and of so much interest from their position, I have pleasure in stating that figures and descriptions of them are in the course of publication by Dr. Beck, and I may add that we owe this work

* On this subject see Lond. and Edinb. Phil. Mag., vol. vii. p. 413, *note*.]

to the liberality and the zealous interest taken in our science by an illustrious member of our Society, the Crown Prince of Denmark. The collection of recent shells formed by His Royal Highness and now in his private cabinet,—more extensive perhaps than any other in Europe,—has afforded Dr. Beck the most ample facilities of comparing fossil and recent shells, and from the opportunities thus enjoyed we may look, at no distant period, for results which will materially advance the general progress of fossil conchology.*

Few communications have excited more interest in the Society than the letters on South America addressed by Mr. Charles Darwin to Professor Henslow. Mr. Darwin has devoted four years, from 1832 to 1835 inclusive, to the investigation of the natural history and geology of South America. From the position of the tertiary deposits which exist on both sides of the southern Andes, he concludes that the primary chain must have had a great elevation anterior to the tertiary period. A transverse section from Rio Santa Cruz to the base of the Cordilleras, and another on the Rio Negro exhibit the structure of what Mr. Darwin calls the great southern tertiary formations of Patagonia, which may be separated into groups of distinct periods analogous to those already established in Europe. The lowest group is of great extent and thickness, and in one instance was observed to alternate with a bed of ancient lava, which seemed to mark the commencement of the eruptions from the craters of the principal chain of the Andes. Among the shells and corals, even of this lowest deposit, are some which are supposed to belong to species now living in the neighbouring Pacific. Overlying this is a stratum of rolled porphyry pebbles, which the author traced for 700 miles. Scattered over the whole, and at various heights above the sea, from 1300 feet downwards, are recent shells of *littoral* species of the neighbouring coast, so that every part of the surface seems once to have been a shore, and Mr. Darwin supposes that an upheaval to the amount of 1300 feet has been owing to a succession of small elevations, like those experienced in modern times in Chili.

The principal section described is one transverse to the Andes, extending from Valparaiso to Mendoza. The Cordillera consists here of two separate and parallel chains, the western being composed of stratified sedimentary rocks resting on granite. The strata are violently dislocated and contorted along parallel north and south lines, and become crystalline as they approach the gra-

* Having been led to speak of cretaceous fossils, I may state that it has been a question whether certain fossils found in the English chalk, and called by Mr. Mantell *Hippurites Mortoni*, are truly referrible to the genus Hippurite. When I first saw one of these fossils in the collection of Mr. Robert Hudson, I conjectured that it might belong to the family of Conia and Balanus; but I regret that this opinion has been published as mine in Loudon's Magazine, as it was abandoned by me as soon as I had opportunities of minutely examining the specimens. (See Loudon's Mag., No. 58.) Without being able to decide whether they are truly Hippurites, I may state that I believe them to belong to the family of Rudistes of Lamarck, and that they are not allied to Conia.

nite. Some of the slates and limestones, probably referable to the transition period, contain organic remains at an elevation of 13,000 feet above the sea. In the eastern chain are sandstones and conglomerates, and associated felspathic rocks regularly bedded, and more recent than the rocks of the western chain, being partly made up of their debris. After much investigation Mr. Darwin convinced himself that these were of the same age with certain tertiary deposits of Patagonia, Chiloe, and Conception, resembling them in mineral character and in the lignite and fossil wood which they contain. In one escarpment is seen a sandstone of this system in which there is a wood of petrified trees in a vertical position, some of the trees being perfectly silicified and of dicotyledonous wood, others consisting of snow-white columns of coarsely crystallized carbonate of lime. They appear to have formed a clump of trees which had grown on lava and was then submerged, so that layers of fine sandstone were quietly deposited between the trunks. The enveloping sandstone rests on lava, and is again covered by a bed of black augitic lava about 1000 feet thick. Over this there are at least five other grand alternations of similar rocks and aqueous deposits, amounting in thickness to several thousand feet. The same sedimentary strata, or the continuation of them, are not only altered by granite, but are traversed by dikes of granite proceeding from the mass, and also by numerous metallic veins of iron, copper, arsenic, silver, and gold, all of which can be traced to the underlying granite. A gold mine has been worked close to the clump of silicified trees.

From these observations I am led to suspect that, as in some parts of the Alps, the metamorphic structure has been assumed by strata high up in the secondary series, so in the Andes the same structure has been superinduced on certain tertiary deposits which have been also penetrated by granitic and by metalliferous veins.

Dr. Daubeny has analysed a new thermal spring discovered near the town of Torre del Annunziata in the Bay of Naples, and he refers the origin of nitrogen gas in this and other springs in the volcanic region of Naples and Mount Vultur to a process of subterranean oxygenation analogous to combustion. In the excavations made in volcanic tuff and lava near Torre del Annunziata for gaining access to the spring, vestiges of walls and buildings with fresco paintings, and other traces of human art were discovered, and vegetable mould containing the stems of reeds, similar to those now growing in the neighbourhood, and a fir and cypress tree in an upright position. The buildings must have been overwhelmed before the soil existed on which the fir and the cypress grew, as this soil was formed upon the materials which enveloped the town.

Mr. H. E. Strickland and Mr. Hamilton have examined a cavity below the level of the sea in Cephalonia adjoining the coast, into which a constant stream of sea-water is flowing, and has been flowing for years. This singular phænomenon had previously attracted the attention of Mr. Martin and of Lord Nugent and others, some of whom had speculated, like Mr. Strickland, on the probability of the water thus descending through crevices being converted into vapour in subterranean hollows, and then carried off in other directions in the form of *stufas* or hot springs. I forbear to enlarge on this subject at present, as a description of the facts drawn up by Mr. Martin before Mr. Strickland's visit, will shortly be read to the Society.

We have received from Capt. Belcher a suite of geological specimens from various parts of the west coast of Africa, with remarks on the reefs and sand-banks of that coast; and a collection from the Rev. W. Hennah of recent calcareous limestone and volcanic products from the island of Ascension.

I shall next consider some papers relating more or less exclusively to fossil zoology, which have been read at our meetings during the last session. We are indebted to Mr. Broderip for a description of some new species of fossil Crustacea and Echinodermata, which were discovered by Lord Cole and Sir P. Egerton in the lias of Lyme Regis. One of these crustaceans belongs to a genus intermediate between the Palinurus and the Shrimp. It is of a gigantic size compared to any recent species, and belongs to a division of which the living types have been only met with in the arctic regions.

Sir P. Egerton has described some peculiarities of structure in the occipital bone of an Ichthyosaurus, observed in the skeleton of a new and gigantic species recently discovered by Miss Anning at Lyme Regis. He also states that the axis and atlas in this genus are usually found adhering firmly together, and they are connected by an auxiliary bone, showing that strength rather than freedom of lateral motion was required in the neck of these animals. These observations have been confirmed by Mr. Owen and Mr. Clift.

It has often been a question whether the bones of birds had ever occurred in strata below the chalk, some of the thin fragile bones found at Stonesfield, and formerly considered to be those of birds, having been ascertained to belong to Pterodactyls. In order to elucidate this point, Mr. Mantell lately placed all his specimens from the Wealden, supposed to be those of certain Grallæ, or waders, in the hands of Mr. Owen, and the result of his examination has confirmed Cuvier's opinion that they are true ornitholites. They seem, therefore, to be the oldest authenticated fossils of this class hitherto found in Great Britain. The rarity of such remains in geological formations, especially in the marine, cannot surprise us; for in the recent shell marl of Scotland, formed in lakes much frequented by water-fowl up to the moment of their drainage, no bones of birds have as yet been detected amongst the numerous relics of deer, ox, pig, and other quadrupeds occurring in the marl.

Mr. Darwin, in his travels in South America before alluded to, found, in crossing the continent from the Rio Negro to Buenos Ayres, many large bones of Mastodons, and other remains of the Mastodon at Port St. Julian, 50° S. lat., at a distance of more than six hundred miles from the former. He also saw, in the gravel of Patagonia, many bones of the Megatherium, and among the remains of five or six species of quadrupeds associated with them, he detected those of a species of Agouti.

Our museum has just been enriched by a truly magnificent present of fossil bones from India, more valuable than any which have reached England since those obtained by Mr. Crawfurd and Dr. Wallich from Ava. They were collected and presented to us by a gentleman whom we last year elected a Fellow of this Society, Capt. Cautley of the Bengal Artillery, and their existence seems to have been first distinctly recognised by Dr. Falconer, superintendent of the Botanic Garden at Saharunpore. These organic remains come from the range of hills formerly called Sewalik, which skirt the base of the Himalayan mountains from the Ganges to the Sutluj rivers, or from north lat. 30° to 31°. They abound in part of the range to the westward of the Jumna river, and belong to the genera Mastodon, Elephant, Hippopotamus, Rhinoceros, Hog, Anthracotherium, Horse, Ox, Deer, Antelope, Canis, Felis, Gavial, Crocodile, Emys, Trionyx, besides fish and shells. Among the fossils there are some considered to be new genera, and one which Messrs. Cautley and Falconer have called Sevatherium.

We have also received a splendid collection of specimens of rocks from the Himalayas, illustrating the two sections published by Mr. Royle in his work on these mountains, from the plains to the snowy passes, and his section across the central range of India.

Several new facts have been brought to light in fossil ichthyology during the last year. Sir P. Egerton has found in the coal-field of North Staffordshire, among other remains of fish, some scales of the Megalichthys, that large sauroidal fish first described by Dr. Hibbert as occurring at Burdiehouse, near Edinburgh. I have lately seen a large tooth of this fish in a mass of Cannel coal found in Fifeshire by Mr. Horner and described by him in a paper read before the Royal Society of Edinburgh. It will be remembered that these teeth were formerly referred to saurians, to which, in fact, the Megalichthys had a much nearer affinity, according to Mr. Agassiz, than has any fish now living. Sir P. Egerton has also published a catalogue of the fossil fish in his cabinet at Oulton Park, and in that of Lord Cole, at Florence Court; two collections which are described by Mr. Agassiz as unrivalled in England in this department of organic remains, and only equalled by two others in the rest of Europe, that of Count Munster, at Baireuth in Bavaria, and that of the Royal Museum of Paris *. In this catalogue Sir Philip has given the names and localities of about 200 ichthyolites, British and foreign, and has indicated the geological position of each.

Remains of fishes have been found by Mr. Prestwich in a formation of sandstone and red conglomerate which overlies the old red sandstone in Banffshire. He supposes the deposit to be of the age of the coal-measures, an opinion which is in accordance with the characters of the ichthyolites as determined by Mr. Agassiz.

One of the most perplexing enigmas in palæontology has lately been solved by Dr. Buckland, who has discovered that some curious fossils of the oolitic and cretaceous strata, which had long

* Agassiz, *Poiss. Foss.*, 4me livr. p. 45.

baffled the skill of comparative anatomists, are in fact the upper and lower jaws of extinct species of Chimæra, a rare genus of living fish. These fossils had been found by Sir P. Egerton in the Kimmeridge clay, by Mr. Townsend in the Portland stone, and by Mr. Mantell in the chalk. They belong to four distinct species, of which the characters are given by Mr. Agassiz. The scientific world is indebted to the splendid museum of comparative anatomy at Leyden for the opportunities enjoyed by Dr. Buckland of comparing the skeleton of the recent Chimæra with the fossils alluded to.

Mr. Agassiz has described two very singular genera of fossil fish from the lias, one of which has been known under the name of Squalo-raia from Lyme Regis; the other from Whitby, called *Gyrostris mirabilis*, probably the largest known fish.

Hitherto the new red sandstone in Great Britain had been destitute of all organic remains, but some distinct impressions of fish of the genus Palæoniscus, *Ag.*, have now been observed in this formation near Dungannon in Ireland. The geological position of these has been pointed out by Mr. Murchison, and a slab of sandstone presented to the Society by Mr. Greer exhibits on a single surface only two feet square, impressions of about 250 fishes.

I have already had occasion to allude more than once to the name of Agassiz, on whom the Council have this day conferred the Wollaston Medal. I may say with pleasure, that in his second visit to England, as in that of the preceding year, he has given an impulse to the study of fossil remains in various departments which will long be felt in this country. It is not merely sound knowledge which he has freely communicated to all who have enjoyed his society, but what is even of more lasting profit, a generous enthusiasm for the study of every department of natural history and particularly of fossils. The great work on which he is now engaged yields not in importance to any that has ever been undertaken for the illustration of organic remains, and the progress which he has already made at so early an age, holds out the most encouraging prospects of his future success.

When we consider the strong ties of affinity which unite together all animals of the vertebrate classes, and reflect that man himself, viewed in reference to his organization, belongs to this great division of the animal kingdom, we cannot but feel the highest interest in tracing the remains of the vertebrate animals through geological formations of every age, from the newest to the most ancient. In a small part of Europe alone more than 800 species of ichthyolites have already been determined. They are distributed through strata of all epochs; no less than 54 species have already been discovered in the carboniferous rocks, and five or six have been met with in the still older Silurian formations.

The museums of Great Britain alone have afforded to Mr. Agassiz no less than 300 new species of ichthyolites, 50 of which have been added since our last anniversary. He had previously pointed out as a general law that particular generic types are strictly confined to certain groups of strata, and it is remarkable that so vast an accession of new species offers but few exceptions to the rule. In the chalk two species have come to light belonging to genera before observed in the oolitic series only, and a distinct species of one of these genera extends even into the lower or Eocene tertiary deposits.

The labours of Mr. Charlesworth have thrown much light on the structure of the crag of Suffolk and Essex, and on the fossils of that deposit. He proposes to divide the crag into the upper or red crag, and the lower or coralline crag, the last of which consists for the most part of calcareous sand, derived chiefly from the decomposition of zoophytes and shells, and in which many very perfect corals and testacea are preserved. Among other places this coralline crag may be well examined at Tattingstone, Ramsholt, Orford, and Aldborough. It is now many years since Mr. Wood, of Heskerton in Suffolk, formed a large collection of crag fossils, amounting in number to no less than 450 species of the classes Annulata, Cirrhipeda, Conchifera, and Mollusca. Out of 370 species of shells found in the lower crag, Mr. Wood identifies 150 with those found in the red crag. Of these 150 species, common to the two deposits, Mr. Charlesworth suggests that many may have belonged to the lower bed and have been washed into the newer one, in the same manner as some fossil shells of the chalk have been evidently imbedded in the crag*.

Such accidental mixtures have doubtless occurred, and they have been occasionally remarked by geologists in other places under analogous circumstances. But I continue to believe that these upper and lower divisions of the crag should be referred to the same geological period. The determination of that period or the exact place which the crag should occupy in the chronological series of European strata is a more difficult question. When I first submitted 111 species of crag shells to the examination of M. Deshayes, he was of opinion that 66 of them were extinct, and that the others belonged to recent species now inhabitants of the German Ocean. I lately laid before him 60 species from the coralline crag with which Mr. Charlesworth had favoured me, and he was still of opinion that the proportion of recent species was equally great.

But I should add that the suites of individuals of each species were not so full and complete as might have been desired, to enable these identifications to be placed beyond all doubt. Dr. Beck has lately seen 260 species of crag shells in Mr. Charlesworth's cabinet in London, and informs me, that although a large proportion of the species approach very near to others which now live in our northern seas, he regards them as almost all of distinct species, and unknown as living. Both he and M. Deshayes have declared the shells to be those of a northern climate, and according to Dr. Beck the climate may even have resembled that of our arctic regions.

* [See Mr. Charlesworth's paper on the Crag, in L. and E. Phil. Mag. vol. vii. p. 81; also p. 413, *note*, and p. 464 of the same volume.]

In regard to the discordance in the results at which these eminent conchologists have arrived, it may arise not only from the unequal opportunities which they have enjoyed of examining the necessary data, but also, in part, to the different estimate which they have formed of the amount of variation necessary to constitute a distinct species. One example will sufficiently illustrate my meaning. Those naturalists who agree with M. Deshayes in referring all the living varieties of *Lucina divaricata* brought from different countries to one and the same species, will identify many more fossils with recent shells than those who agree with Dr. Beck in dividing the same recent individuals of *Lucina divaricata* into six or eight distinct species. Provided, however, each zoologist is consistent with himself, and provided the distinctive characters relied on as specific by each are commensurate one with another, no confusion will arise.

In reviewing the proceedings of the Society during the last year, I find that the remaining memoirs, numerous as they are, may be all referred to one great class of subjects, for they either relate to changes now going on upon the surface of the earth as attested by man, or to geological proofs of similar changes since the rivers, lakes, and seas were inhabited by the existing species of testacea. Under these heads I shall be led to consider the effects of modern earthquakes in upheaving and depressing the land; the gradual rising of land in one region and the lowering of its level in another; the rolling in of great waves of the sea upon the coast during earthquakes; the transportation of rocks by floating ice; the signs of upraised beaches containing marine shells; erratic blocks; alluvial deposits of different ages; and other kindred topics on which a variety of new facts have been collected.

The last year has been signalized in South America by one of those terrific convulsions which have so often desolated the western coast since the discovery of the new world. A brief notice of this catastrophe was sent me by Mr. Alison, written immediately after the event. He mentions that on the 20th of February, 1835, when Conception, Chillan, and other towns were thrown down in ruins, the sea first retired from the shores of the Bay of Conception, and then returning in a wave about twenty feet high, rolled over several of the towns, and completely destroyed whatever the earthquake had left uninjured. He also states that the coast of the bay was reported to have been heaved up, and that a rock off the landing-place at the port of Talcahuano, which before the shock was nearly level with high water, stood afterwards three feet above that mark. Large fissures were made in the earth, and water burst from some of them.

In these and other particulars Mr. Alison's letter agrees with the more circumstantial account sent to the Royal Society by Mr. Caldcleugh, who was resident at Valparaiso, but who drew his information in great part from eye-witnesses. He mentions that a great number of the volcanos of the Chilian Andes were in a state of unusual activity during the shocks, and for some time preceding and after the convulsion. Among others, Osorno, of which the cone rises 3900 feet above the sea, and which is situated on the mainland north-east of the island of Chiloe was in eruption, lava being seen to flow from its crater. Several others are also noticed, and the lava emitted from one of them is stated to have covered an area eight leagues in circumference and to the depth of $3\frac{1}{2}$ yards. The ashes reached to the distance of 300 leagues. I refer you to these statements because it is rare to meet with any recent descriptions of the emission of lava and ashes from the high cones of the Andes.

The same writer was informed that the strata of clay-slate, forming the shore of the Bay of Conception, were elevated from three to four feet, whereas the rise at San Vicente, south of Talcahuano, amounted to only $1\frac{1}{2}$ feet. Mr. Caldcleugh was also informed that the island of Santa Maria, in the Bay of Conception, was upheaved about eight feet.

At the same time the island of Juan Fernandez, distant 360 miles from Chili, was violently shaken and devastated by a great wave. A dense column of vapour issued from the sea about a mile from the coast, and flames were seen at the same spot in the night which illumined the whole island. At this point in the sea whence the flames were emitted the depth of water was afterwards ascertained to be no less than 69 fathoms.

At a court-martial, lately held at Portsmouth, in consequence of the wreck of the Challenger frigate on the coast of Chili, in May 1835, some notes of Capt. FitzRoy were read, and afterwards communicated by Capt. Beaufort to the Society, in which he describes some remarkable alterations produced by the earthquake of February in the direction of the currents on the Chilian coast. A more detailed account of the convulsion has just been received at the Admiralty from the same officer, with a sight of which I have been favoured, but no allusion is here made to the currents. There are, however, other facts perfectly new and of the highest importance attested in this memoir, and as they come from an observer of great experience in hydrographical surveying, who examined the Bay of Conception immediately after the shocks, they will remove all doubts from the minds of those who have questioned the power of earthquakes to cause the permanent upheaval of land.

Capt. FitzRoy states, that on the 20th of February, 1835, the earthquake was felt at all places between Copiapo and Chiloe from north to south, and from Mendoza to Juan Fernandez from east to west. Conception and other towns were thrown down. After the shock the sea retired; the vessels in the bay grounded, even those which had been lying in seven fathoms water; all the shoals in the bay were visible; and soon afterwards a wave rushed in and then retreated, and was followed by two other waves. The vertical height of these waves does not appear to have been greater than from 16 to 20 feet, although they rose to much greater heights when they rushed upon a sloping beach. During the shocks the earth opened and closed rapidly in numerous places. The direction of the cracks was not uniform, though generally from south-east to north-west. The earth was not quiet during three days after the great shock, and more than three hundred shocks were counted between 20th February and 4th of March. The loose earth of the valley of the Bio Bio was everywhere parted from the solid rocks which bound the plain, being separated by cracks from an inch to a foot in width.

In the Bay of Conception two explosions or eruptions were seen in the sea while the great waves were coming in. One beyond the island of Quiriquina appeared to be a dark column of smoke in shape like a tower; another rose in the Bay of San Vicente like the blowing of an immense imaginary whale. Its disappearance was followed by a whirlpool which lasted some minutes. It was hollow and tended to a point in the middle, as if the sea was pouring into a cavity of the earth. The water in the bay appeared to be everywhere boiling, bubbles of air or gas were rapidly escaping, and dead fish were thrown ashore in quantities.

For some days after the 20th February the sea at Talcahuano did not rise to the usual marks by four or five feet vertically. "Some thought that the land had been elevated, but the common and prevailing opinion was that the sea had retired. This difference gradually diminished till, in the middle of April, there was only a difference of two feet between the existing and former high-water marks. The proof *that the land had been raised* exists in the fact that the island of Santa Maria was upheaved nine feet; but of this presently. When walking on the shore, even at high-water, beds of dead mussels, numerous chitons and limpets, and withered sea-weed still adhering, though lifeless, to the rocks on which they had lived, everywhere met the eye—the effects of the upheaval of the land."

From the above extracts, then, it appears that in the opinion of Capt. FitzRoy some of the land was first raised in February four or five feet, and that it afterwards gradually returned towards its former level, so that in about two months the temporary increase of its height was diminished by more than one half.

The observations which follow respecting Santa Maria, an island seven miles long and two broad, in the Bay of Conception, deserve particular attention, and I shall give them in Capt. FitzRoy's own words; for although in so doing I anticipate a communication which I trust will hereafter be given in full to the Society*, I am only supplying the proofs of the elevation which was asserted as a fact in Capt. FitzRoy's notes read before you during the last year.

"It appeared that the southern extreme of the island had been raised eight feet, the middle nine, and the northern end upwards of ten feet. The Beagle visited this island twice, at the end of March and in the beginning of April. At her first visit it was concluded, from the visible evidence of dead shell-fish, water-marks, and soundings, and from the verbal testimony of the inhabitants,

* Since the above was written the whole memoir has appeared in the Nautical Magazine for March 1836.

that the land had been raised about eight feet. However, on returning to Conception, doubts were raised, and to settle the matter beyond dispute, or the possibility of mistake, the owner of the island, Mr. Salvador Palma, accompanied us. An intelligent Hanoverian, who had lived two years there and knew its shores thoroughly, was also a passenger in the Beagle. His occupation upon the island was sealing. When we landed, the Hanoverian, whose name was Antonio Vogelborg, showed me a spot from which he used formerly to gather Choros by diving for them at low water. At dead low water, standing upon that bed of choros, and holding his hands up above his head, he could not reach the surface of the water. His height is six feet; on that spot when I was there the choros were barely covered at high spring tide.

"Riding round the island afterwards with Mr. Palma and Vogelborg, many measures were taken in places where no mistake could be made. On large steep-sided rocks, where vertical measures could be correctly taken, beds of dead mussels were found ten feet above the present high-water mark. A few inches only above what was taken as spring-tide high-water mark were putrid shell-fish and sea-weed, which evidently had not been wetted since the upheaval of the land. One foot lower than the highest bed of mussels, a few limpets and chitons were adhering to the rock where they had grown. Two feet lower than the same, mussels, chitons, and limpets were abundant.

"An extensive rocky flat lies around the northern parts of Santa Maria. Before the earthquake this flat was covered by the sea, some projecting rocks only showing themselves. Now the whole flat is exposed. Square acres (or many *quadras*) of this rocky flat were covered with dead shell-fish, and the stench arising from them was abominable. By this elevation of the land the southern port of Santa Maria has been almost destroyed; there remains but little shelter, and very bad landing. The soundings have diminished a fathom and a half everywhere around the island."

The author then goes on to inform us that at Tubul, to the south-east of Santa Maria, the land has been raised six feet. At Mocha two feet. No elevation has been ascertained at Valdivia, northward of Conception; at Maule, according to the assertion of the governor, the chief pilot, and other residents, the land instead of being elevated had sunk two feet, for they said there were two feet more water on the bar after the shock, and the banks of the river were lowered. Capt. FitzRoy, however, suggests that a rush of water might have shifted the loose sands of the bar; so that he doubts the subsidence at Maule, and only feels certain that the land had not risen there.

It is scarcely necessary for me to advert to the striking analogy of the phænomena observed by Capt. FitzRoy and those which were formerly described by Mrs. Maria Graham (now Calcott), and published in our Transactions, respecting the Chilian earthquake of 1822. The coast of Valparaiso, Quintero, and other places was then stated to have undergone unequal elevations, the greatest amounting only to a few feet, and banks of sea-shells were laid dry above high-water mark. But these statements, given on the authority of Mrs. Graham's personal observation, and confirmed by others to which I shall presently allude, have been met by a direct counter-statement so circumstantial and explicit as to deserve the fullest consideration. Mr. Cuming, well known to you by his numerous researches in conchology, declares that being at Valparaiso before and during the earthquake of 1822, and residing there constantly until 1827, he could never detect any proofs of the rise of the land, although his pursuit of conchology and natural history in general caused him to visit frequently the rocks and inlets with which the northern and southern parts of the bay abound. These rocks were covered with Fuci, Patellæ, Chitons, Balani, &c., yet he never perceived the least difference in their appearance from the date of his arrival to his finally quitting Valparaiso, nor observed any trace of them except in situations covered by the tide. He also remarked that the water at spring tides rose after the earthquake to the same point on a wall near his house which it had reached before the shocks. He imagines that the idea that a change had taken place in the relative level of land and sea originated in the gain of land opposite Valparaiso, occasioned by the accumulation of detritus at points where the tide had flowed previously to the earthquake. Mr. Cuming first heard of the notion of the land having been elevated at Valparaiso when Mrs. Graham's paper read to the Geological Society in 1824 was talked of at Valparaiso. Neither he nor his friends were then able to subscribe to the opinion expressed in that communication.

On the other hand, Lieut. Freyer, R.N., in a letter read to you during the last session, observes, that being at Valparaiso after the earthquake of 1822, he saw a shelly beach to the east of the town, above the reach of the tides; and rocks, which was pointed out to him as being less under water than it had been before the convulsion. Dr. Meyen also, a Prussian traveller, who visited Valparaiso in 1831, says he examined the coast there and found appearances in corroboration of Mrs. Graham's statements. I may also repeat what I have elsewhere recorded, that some years after the event I applied to Mr. Cruckshanks, an English botanist, who resided in Chili at the time of the earthquake, whether he had seen any signs of the alleged change of level. He said that he examined the coast at Quintero after the shocks, and satisfied himself that it had been uplifted several feet, and that the fishermen told him that the ocean had gone down and was lower than before, in confirmation of which they pointed to some rocks of greenstone at Quintero, a few hundred yards from the beach, which were always under water previously to the great shock of 1822, but were afterwards uncovered when the tide was at half ebb.

Without pretending that I can reconcile this contradictory evidence, I may suggest that some discordance in the accounts may have arisen from a want of uniformity in the movement at different places, and still more from a subsequent sinking down of some of the land which was first raised, in the manner described by Capt. FitzRoy as having taken place near Talcahuano in the spring of last year. In perusing Mr. Cuming's account we must all feel that the author has had no object in view but that of establishing the truth; and the doubts which he has raised will call for a reinvestigation of the phænomena; but after hearing all objections, even before the late convulsion of 1835, I expressed myself satisfied with the proofs in favour of the elevation of 1822 *. If I had still cherished any scepticism, it would now be removed by the coincidence of the facts related by Capt. FitzRoy. To suppose that a set of imaginary phænomena, which appeared at first sight very improbable, and which no geologist could explain, should have been invented, in Chili, in 1822, by several intelligent observers, and that thirteen years afterwards nature should realize, in the same country, the same phænomena, or others strictly analogous, so as to lend countenance to all the previous misconceptions, is to imagine a combination of circumstances almost as marvellous as the upheaval of a continent itself.

We are indebted to Mr. Woodbine Parish for a collection of historical notices respecting the effects of the earthquake waves of the Pacific, which have repeatedly caused great inundations on the coast of Chili and Peru. The earliest date to which he has traced back these memorials is the year 1582. The sea usually retired in the first instance, and then rolled in upon the land, carrying ships far inland and levelling towns to the ground. Such floods must have left great banks of sand and gravel, mingled occasionally with broken and entire shells, upon dry land, considerably above the level of the highest tides, but they will by no means account for the very elevated position of recent marine shells on various parts of the maritime country of Patagonia, Chili, and Peru †.

Mr. Freyer, to whom I have before alluded, states that he observed in many parts of Peru, especially near Arica and in the Isle of San Lorenzo, in the Bay of Callao, lines of shingle and sand, with shells of existing species, at various elevations above the level of the sea. The rocks of sandstone and gypsum south of the bold promontory called the Morro of Arica are shaped into distinct terraces towards the shore, and on these terraces the rock, wherever it is exposed, is seen to be incrusted with balani and millepores. At the height of about twenty or thirty feet above the sea, these shells and zoophytes are as abundant and almost as perfect as on the shore; at upwards of fifty feet they still occur, but in an injured state, for although there is no rain in this district to hasten their decay, by alternate moisture and desiccation, still they are abraded by the sand which is constantly blown over them. Some of the recent shells occurring at considerable heights in the island of San Lorenzo retain their colour almost as freshly as those living in the adjacent sea. Mr. Darwin has also observed in different parts of Patagonia and Chili beds of recent shells at various heights above

* Principles of Geology, 4th edit. vol. ii. p. 331.
† [See Mr. Woodbine Parish's paper in our last Number.]

the sea, and among them mussels which retained their blue colour, and emit a strong animal odour when thrown into the fire.

I shall now turn from the modern changes observed in South America to the evidences of recent alterations in the level of the land in high latitudes in the northern hemisphere. Dr. Pingel, a Danish mineralogist and naturalist, has communicated some facts showing the gradual sinking of part of the west coast of Greenland. It is now more than fifty years since Arctander inferred that this coast had subsided, having noticed some buildings in the Firth called Igalliko, on a low rocky island near the shore, almost entirely submerged at spring tides. From this point, which is in lat. 60° 43′ north, to Disco bay, extending to nearly the 69th degree of north latitude, Dr. Pingel has traced various signs of the depression of the land, ancient settlements of the Greenlanders and Moravians being now overflowed by the sea. In one case the Moravians were obliged to move inland the poles upon which their large boats were set, and the old poles still remain beneath the water as silent witnesses of the change. It is also mentioned that no aboriginal Greenlander builds his hut near the water's edge. Having conversed with Dr. Pingel, at Copenhagen, on this subject, I am convinced that the phænomena cannot be explained away by reference to a rise of the tides at particular points, the advance of the sea being general for more than 600 miles from north to south, and caused not by the undermining of cliffs and the denudation of land, but by submergence of what was before above water.

I am the less inclined to question the probability of a general subsidence of the land in Greenland, because I now believe that an equally slow and gradual movement is taking place, but in an opposite direction, throughout a large part of Sweden and Finland. I ventured formerly to controvert the proofs adduced in favour of such an upheaval of land in those countries, although the fact had been advocated by Celsius, the Swede, and in later times by Playfair and Von Buch. But after visiting, in 1834, several parts both of the eastern and western coasts of Sweden, I became satisfied that an elevation is in progress, more rapid at Stockholm than further to the south, and greater at Gefle than at Stockholm. The rate of rise appears in some places to have amounted only to a few inches in a century, in other places to several feet, but as far as I could learn from the report of pilots, travellers, fishermen, and traders, the alteration extends to the North Cape, and is probably felt over a space more than 1000 miles in length from north to south, and several hundred miles in breadth. The evidence is derived from many sources, partly from tradition and from the recollection of the oldest inhabitants and seafaring men, partly from the position of ancient buildings on the coast, and partly from marks chiselled at different periods on rocks bordering the sea, for the express purpose of indicating the ancient standard level of the waters. As the details of my own observations have been published in the Philosophical Transactions of last year*, I need only add that at one

* [See Lond. and Edinb. Phil. Mag., vol. vi. p. 297.]

spot to the south of Stockholm I saw what appeared to me a conclusive proof of an alternate rising and sinking of the same land since this region was inhabited by man, first a depression of the ground of at least 50 feet below its former level, and then a re-elevation of the same amounting to at least 50 feet.

The probable cause of the prolonged and insensible movements of large masses of land opens a wide and inviting field for speculation. As we know that volcanic action is never dormant in some parts of the interior of the globe, it seems most natural to imagine that an alternate expansion and contraction of the earth's crust may arise from a gradual increase or diminution of its temperature. Mr. Babbage has suggested that as many common kinds of stone have been shown by experiment to augment in volume when heated, and decrease in bulk when slowly cooled, a great thickness of subjacent rock may cause the surface to rise or sink according to the variations experienced in the subterranean temperature. We have also to consider the effects which might result from the slow cooling and crystallization of large reservoirs of melted matter, on which subject we have unfortunately as yet few experiments to guide our conjectures. We know not, for example, whether the passage from a fluid to a solid state would uplift or let down an incumbent mass of rock. A dense fluid, subjected to immense pressure, may, perhaps, on crystallizing into a rock like granite, occupy more space in its state of solidity. I need not remind you that as ice floats in water, so a bar of cast iron floats on the surface of melted iron.

But however obscure the origin of the movements in question, their reality if admitted affords a key to the interpretation of a variety of geological appearances, some of which I shall now proceed to consider.

Dr. Beck has mentioned that the oldest strata in Denmark are often covered by deposits of gravel, sand, and loam, several hundred feet thick, in which, but more commonly upon them, lie erratic blocks. The sand and gravel beds rarely contain any fossils, but when shells do occur they are absolutely identical with living species. He has also found, in the lower valleys of Jutland, more than seventy species of shells now living in the German Ocean. These facts agree precisely with others which I observed in different parts of Sweden, and which I have described in the memoir before alluded to. On the west coast, between Uddevalla and Gothenborg, the beds of sand, gravel, and clay, containing recent oceanic shells, are seen at various heights from 100 to 300 feet above the sea. M. Alex. Brongniart formerly pointed out those which rest on the gneiss, near Uddevalla, and like him I saw Balani still attached to the rocks at the height of more than 150 feet above the sea-level. I ought, however, to state that at the points where I discovered them they had not been exposed to decomposition in the atmosphere ever since their emergence. On the contrary, the adhering shells had been protected by a covering of shelly sand only removed of late years for road-making. I need scarcely insist upon the obvious inference that the Balani and corallines which also cover the rocks, and which are of the same species as those found on the shells of the recent strata in contact with the rocks, prove that the gneiss was long submerged beneath the waters, and that the shells were not washed up by an inroad of the sea upon the land. In the island of Orust, opposite Uddevalla, I found similar appearances, and on other parts of the western coast; but on the eastern shores of Sweden or those bordering the Baltic, both to the north and south of Stockholm, a marked distinction is recognised. In the assemblage of fossil shells which there occur in beds of upraised gravel, sand, and clay, the testacea belong to recent species, yet not to that assemblage which inhabits the ocean, but to a confined number of mixed freshwater and marine species characteristic of the brackish waters of the Baltic. Such deposits rise near Stockholm to the height of 200 feet above the sea, and show that the relative level of land and sea has greatly changed, not only since the existing testacea were in being, but also since the Baltic was divided off from the ocean as an inland sea freshened by a superabundance of river water.

It is well known that these parts of Sweden are densely strewed over with huge erratic blocks, many of the largest of which occur in the highest part of ridges of sand and gravel, finely stratified or made up of a continued series of thin layers of sand, loam, and gravel. In one of these ridges, at Upsala, I found layers of marl, containing perfect shells of recent species, such as live in the Baltic. The ridge was about 100 feet high, and on the summit of it were blocks of gneiss and granite, measuring from eight to ten feet in length. I saw similar boulders but inferior in size overlying some deposits of recent shells in Orust and near Uddevalla *. Hence it is evident that the transportation of these rocky fragments into their present position continued after the period when the modern shelly formations of both the coasts of Sweden were accumulated.

In addition to the facts enumerated in my paper on Sweden in the Philosophical Transactions for 1835, in regard to the agency of ice-islands, I may mention a fact observed by Dr. Beck on the coast of Jutland. He has ascertained that on the breaking up of the fringe of ice which encircles the coast there during winter, small islands of ice float off and carry with them not only small gravel from the beach but stones four feet in diameter firmly frozen into the solid mass. These ice-floes are sometimes driven eastward into the Cattegat, and have been known to stop up the narrow part of the passage of the Great Belt, and to cause new reefs of rocks thus transported on which vessels, and a few years ago a Danish man-of-war, have been stranded. If such power can be exerted by ice-islands, only a few hundred feet in diameter, in latitudes corresponding to those of England, we may be well prepared to find that islands several leagues in circumference may remove blocks of the magnitude of small houses.

Capt. Bayfield, in commenting on the inferences which I had drawn as to the transporting power of ice in the Baltic, communi-

* Phil. Trans., 1835, p. 33.

cated to me several interesting facts observed by him both on the lakes of Canada and in the St. Lawrence. In the river last mentioned the loose ice, when the water is low in winter, accumulates on the shoals, the separate fragments being readily frozen together into solid masses in a climate were the temperature is sometimes 30° below zero. In this ice boulders become entangled, and in the spring, when the river rises after the melting of the snow, the packs are floated off, frequently conveying away the boulders to great distances. Heavy anchors of ships lying on the shore have in like manner been closed in and removed. He also states that immense ice-islands, detached far to the north, perhaps in Baffin's Bay, are brought by the current in great numbers down the coast of Labrador every year, and are frequently carried through the Straits of Belle-isle between Newfoundland and the continent of America, which, after passing through the Straits, sometimes float for several hundred miles to the south-west up the Gulf of St. Lawrence. In one of these icebergs which Capt. Bayfield examined, he found heaps of boulders, gravel, and stones, and he saw other ice-floes discoloured by mud. Capt. Belcher also informs us that in 1815, when in His Majesty's ship Bellerophon he fell in with field-ice off Newfoundland, near St. John's Harbour, in which there were muddy streaks, gravel, and even stones: it was in the heat of summer and torrents of water were shooting off the ice. The importance of these phænomena will be duly appreciated by the geologist who reflects that they relate to the annual transportation of rocks from high latitudes probably corresponding to those of the northern parts of Norway and Sweden, and that the points sometimes reached by the ice are further south than any part of Great Britain. It is therefore by no means necessary to speculate on the former existence of a climate more severe than that now prevailing in the Western Hemisphere in order to explain how the travelled masses in Northern Europe may have been borne along by ice. We know from independent evidence that large parts of the lands bordering the Baltic, and now strewed over with erratics, have constituted the bed of the sea at a comparatively modern period.

It may be asked whether I refer all erratics, even those of Switzerland and the Jura, to the carrying power of ice. In regard to those of Switzerland I have elsewhere endeavoured to show that a combination of local causes might have contributed to their transfer; for repeated shocks of earthquakes may have thrown down rocky fragments upon glaciers, causing at the same time avalanches of snow and ice, by which narrow gorges would be choked up and deep Alpine valleys, such as Chamouni, converted into lakes. In these lakes, portions of the fissured glaciers, with huge incumbent or included rocks might float off, and on the escape of the lake, after the melting of the temporary barrier of snow, they might be swept down into the lower country*.

M. Charpentier has lately proposed another theory which he in-

* Principles of Geology, vol. iii. p. 149, 1833, enlarged in later editions.

forms us is merely a development of one first advanced by M. Venetz. The Alpine blocks, according to these writers, were not carried by water, for had that been the case the largest would be either in the Alpine valleys or near the base of the great chain, and we should find their size and number diminish as we receded from their original point of departure. But the fact is otherwise, many of the blocks on the Jura, or those farthest removed from the starting-place, being of the largest dimensions. They suppose, therefore, in accordance with the opinion of M. de Beaumont and others, that the elevation of the Alps occurred at a comparatively modern epoch, and that when these mountains were first upheaved they were more lofty than now, and more deeply covered with snow and glaciers. After the principal movement had ceased, a lowering of the Alps took place, the dislocated and shattered beds requiring time to settle down into their present more solid and stable form. According to this hypothesis, therefore, the erratic blocks are monuments of the greater magnitude and extent of the ancient glaciers under a different configuration of the surface. I have not space for all the ingenious arguments adduced, after a minute examination of the ground by M. Charpentier in support of this theory, but must refer you to the original memoir *.

Before leaving this subject I may observe, that although it is rare, in modern times, to meet with icebergs in the northern hemisphere so far south as the Azores, in north latitude 42°, yet they have been seen there, and not unfrequently in north lat. 44°, within the present century, thus reaching the parallel of Southern Italy and Central Spain. In the southern hemisphere we learn from Capt. Horsburgh that some large ones were carried, in 1828, still nearer to the equator as far as lat. 35° south, or within about forty miles of the Cape of Good Hope. I do not remember, when examining alluvial deposits, to have seen any blocks in Sicily nor in Italy till I approached the foot of the Alps; and in Sweden I found them increasing in number and size as I advanced northwards, where I saw some between thirty and forty feet in diameter. The erratics, therefore, as far as my experience extends, are a northern phænomenon; and M. Charpentier states, on the authority of Humboldt, that there are no such fragments at the eastern foot of the equatorial Andes, where, notwithstanding the altitude of the mountains, there are no glaciers.

But assuming that ice could have transported into their present position those myriads of angular blocks which cover the low countries bordering the Baltic, in what manner and by what force could these masses have been detached from the mountains of which they once formed a part? Now the granitic rocks in Sweden sometimes consist of large tabular masses, traversed by numerous horizontal and vertical joints; and entire hills may be said to be broken up, in

* *Sur les Blocs Errat. de la Suisse*, Ann. des Sci., tom. viii. p. 219. Mr. Bakewell has also in some one of his works alluded to the carrying of Alpine blocks by ice.

situ, into blocks of the same forms and dimensions as the erratics of the Baltic. I remarked this particularly in Ostrogothland, near Lake Roxen. Whether this fissuring of the rocks has been due to earthquakes, or the expansive power of ice in northern regions, or to what other causes I cannot pretend to decide; but reefs of such jointed rocks before they emerged from the sea might have afforded an inexhaustible supply of detached fragments, over and around which the ice would freeze in winter. One block after another might be buoyed up and floated off on the rise of the Baltic when the snows melted, or of the ocean during high tides.

It has been suggested that large blocks may have been pushed far over the bed of the sea and over the land by a succession of waves raised by earthquakes or by hurricanes. Without denying that such agency may explain some facts in geology, I may remark that we cannot be too much on our guard against assuming violent catastrophes where the effects may have been brought about tranquilly, and even with extreme slowness. Let us imagine, for example, a sunken reef of granite in Baffin's Bay, in about 75° north lat., divided into fragmentary masses as above described, and these masses becoming year after year involved in packed ice. In a few months they may be drifted more than 1800 miles to the southward, through the Straits of Belleisle, to the 48° north lat., the ice moving perhaps at a slow rate—no more than a mile an hour. We might even land upon such ice-fields and be unable to determine whether they were in motion or not. After a repetition of these operations for thousands of years, the uneven bed of the ocean far to the south may be strewed over with drift fragments which have either stranded on shoals or have dropped down from melting bergs. Suppose the floor of the ocean where they alight to be on the rise as gradually as the bottom of the Baltic in our own times. The change may be so insensible that pilots may suspect, and yet scarcely dare to insist upon the fact till its reality is confirmed by the experience of centuries. At length a submarine ridge, covered with the travelled fragments, emerges, and first constitutes an island, which at length becomes connected with the main land,—in time, perhaps, the site of a university like Upsala. Here the question is agitated whether the land is stationary, or continually rising beneath their feet. Perchance they decide that it is motionless, and yet it continues to move upwards, "E pur si muove," till by a growth as imperceptible as that of the forest tree, what was once a submarine reef becomes the summit of an inland mountain. Here the geologist admires the position, number, and bulk of the transported fragments; identifies them with the parent mountains, a thousand miles distant to the north; and in speculating on the causes of the phænomena, imagines mighty deluges and tremendous waves raised by the shock of a comet, or the sudden starting up of a chain like the Andes out of the sea, by which huge rocks were scattered over hill and dale as readily as shingle is cast up by the breakers on a sea beach.

But it is time to return from these digressions and to consider the other memoirs treating of these and similar subjects which have been lately read to the Society. There is perhaps no class of geological phænomena in Great Britain which has hitherto remained in more obscurity than that relating to the distribution and origin of superficial gravel, sand, and mud, especially that which has been called diluvium. Mr. Murchison, in his examination of the older rocks of part of Wales and England, has made a great step towards reducing these phænomena to order, and has thrown so much light upon them that his treatise may be considered not only as one of much local interest, but as likely to contribute powerfully towards the establishment of a general theory of these deposits. He has distinguished between the local drift, or the gravel and alluvium of South Wales and Siluria, and that which he terms the northern drift of Lancashire, Cheshire, North Salop, and parts of Worcester and Gloucester. The surface of the Welsh and Silurian territories is exempt from the debris of far-transported rocks, the alluvium there being derived from the adjacent mountains, while Herefordshire is chiefly covered with debris of the old red sandstone. The author, after giving a detailed description of the drainage of the Teme, Onny, Lug, and Wye, shows that in the valleys of these rivers the loose materials change with each successive range which they traverse, the fragments becoming smaller in proportion as they have been carried to greater distances towards the valley of the Severn. It is also demonstrated that there is an evident connexion between the distribution of this ancient gravel or drift and the strike and dip of the strata in the Welsh and Silurian mountains; and hence it is inferred that the scattering of certain fragments took place during the original upheaving of the mountains. But there are other wide-spread accumulations of sand and gravel in the valleys of the same region, which have partly been due to the existing rivers, and partly to lakes which were drained long after the first emersion of the country from the sea.

The above-mentioned alluvia differ entirely from another kind of detritus, which is spread over parts of Lancashire, Cheshire, and North Shropshire, and which consists of granites, porphyries, and other hard rocks, similar to those of Cumberland and some of the Scotch mountains. To these, with their associated clay and sand, the author gives the name of the northern drift. It has two distinguishing features: first, the occasional occurrence in and upon it of large blocks or boulders of northern origin, sometimes of great size, like the erratics of the Baltic, and none of which ever enter into the region of the Welsh drift; secondly, the association with it of marine shells of existing species. This last fact was formerly noticed by the author and Mr. Gilbertson, at Preston in Lancashire, at heights of 350 feet above the sea. Sir Philip Egerton has since observed the same shells in sand and gravel, north of Tarporley, in Cheshire, at the height of 70 feet, where they occur at the western base of the Forest Hills, about nine miles from the nearest point of the estuary of the Mersey. But what is still more remarkable, Mr. Trimmer found similar recent marine shells on Moel Tryfane, near the Menai Straits, at the height of 1392 feet above the level of the sea. The same author also reported to us that he had discovered similar gravel with recent marine shells overlying a peat bog near Shrewsbury, in which were the remains of a submerged forest. Mr. Murchison, however, having examined this spot, has shown us that the supposed trees were stakes with sharpened points driven into the ground, forming a woodwork which supported an old road, and over these piles the shelly gravel or northern drift had been afterwards spread artificially. I understand that Mr. Trimmer is now fully aware of the mistake into which he had fallen.

From the evidence afforded by the shells, as well as by the indication of several newly discovered localities where they occur sixty miles from the nearest sea-coast, Mr. Murchison infers that the tracts covered by them must have formed the bed of the sea during the modern period, and as the granitic drift occupying the high grounds east of Bridgnorth rises to the height of 500 or 600 feet, and thence descends in a deltoid form into the Vale of Worcester, he conceives that the sea also extended over the valley of the Severn from Bridgnorth to the Bristol Channel, so that there was then a strait separating Wales and Siluria on the one side from England on the other. The deposits observed by Mr. Strickland at Cropthorne and at other points in the valley of the Avon, an eastern tributary of the Severn, and which contain fluviatile and land shells, with the bones of extinct quadrupeds, must, according to Mr. Murchison, have been accumulated at the mouth of a river which flowed from the east, or from the Cotteswold Hills, into the ancient straits above alluded to, and into which the northern drift was prolonged.

There are sections near Shrewsbury from which Mr. Murchison has been enabled to deduce the relative age of the two alluvial formations, the local or Welsh drift having in those places been found covered by the clay and boulders of the northern drift. The latter is, therefore, evidently of newer origin. As to the mode in which the erratic blocks were transported, Mr. Murchison adverts to the possible agency of ice-floes, and to the difficulty of imagining that currents of water alone, whether of rivers or the ocean, could have exerted a force adequate to their removal to such great distances; many boulders of several tons in weight having been transported to more than 100 miles from the nearest possible source of their origin. He also infers from the position of the shells, gravel, and boulders, that they were not washed, as has sometimes been imagined, by one or more diluvial waves over preexisting lands, but were all deposited during the same period in the bed of the sea, which bed was afterwards uplifted to unequal heights by movements of elevation of unequal intensity—movements which, though so largely affecting the physical geography of our island, must have taken place within the modern æra.

Mr. Edward Spencer has communicated to us the result of his examination of the "diluvium" near Finchley, and the summits of the neighbouring hills of Highgate and Hampstead. The gravel there contains water-worn boulders of granite and porphyry, together with fragments of secondary rocks with their characteristic fossils from the mountain limestone to the chalk inclusive. Mr. Spencer supposes that the current which brought these materials into their present situation must have flowed from the north. The diluvium here alluded to seems to correspond to that which covers the crag of Norfolk, and which is in some places intimately connected with that deposit. I may add that I have seen a similar formation on the banks of the Elbe, below Hamburgh, and in other parts of Denmark, with erratic blocks included in it in some places.

Our Secretary, Mr. Hamilton, has described a bed of marine shells, of recent species, on the southern coast of Fifeshire, near Elie, part of the deposit being twelve or fourteen feet above the level of high tide. Similar marine shells have been observed above the sea-level in many of the low lands bordering the estuaries of the Forth and Tay; and in the memoirs before mentioned Mr. Murchison has described a raised beach at the mouth of Carlingford Bay, Ireland, which he lately examined in company with Professor Sedgwick. Mr. De la Beche also informs us that he has lately discovered proofs of two movements of the land of Somerset, Devon, and Cornwall, one to a height of about thirty to forty feet above the present sea-level, and another to an uncertain depth beneath it, both subsequent to the period when the vegetation of the land and the molluscous inhabitants of the neighbouring sea were the same as they now are.

The evidence, therefore, is annually augmenting in favour of considerable alterations in the relative level of land and sea having been brought about in northern Europe at a comparatively modern epoch. For this reason I am more than ever disposed to refer to great movements of elevation and depression, the origin and present position of the loess of the valley of the Rhine, of which I gave some account in a former year. I have lately had occasion to recall your attention to this ancient silt in which terrestrial and aquatic shells are preserved of species still living in Europe. It is found from below Cologne to the neighbourhood of the Falls of Schaffhausen, exhibiting almost everywhere the same mineralogical character and fossils, forming sometimes low hills which cover the gravel of the great alluvial plain of the Rhine, sometimes rising up on the flanks of the mountains which border the great valley to an elevation of 300 or 400 feet above the river, or more than 1200 feet above the sea. I discovered lately, in the neighbourhood of Basle, the first remains of fossil fish which have been detected in this silt; and Mr. Agassiz recognized them as the vertebræ of a small species of the Shark family, perhaps of the genus Lamna. They were associated with the usual fresh-water and terrestrial shells, and the fact appeared anomalous, but the celebrated ichthyologist informs me that species of this family and of the Skate tribe have been known to ascend from the sea up the mouths of the rivers Senegal and Amazon to the distance of several hundred miles.

Some have imagined that a great lake once extended throughout the valley of the Rhine, which sent off large branches up the courses of the Mayne, Neckar, and other tributary valleys, in all of which large patches of loess are occasionally met with. The barrier of such a lake has been placed in the narrow gorge of the Rhine between Bingen and Bonn; but this theory is untenable, as there are proofs of the loess having once filled that gorge, and of its having overspread the adjoining hills of the Lower Eifel; also that it reached to the flanks of the hills bounding the valley of the Rhine as far down as Cologne and still further.

Instead of supposing one continuous lake of sufficient extent and depth to allow of the simultaneous accumulation of loess at all heights and throughout the whole area where it now occurs, I conceive that subsequently to the period when the countries now drained by the Rhine and its tributaries, acquired nearly their actual form and geographical features, they were again depressed gradually by a movement like that now in progress on the west coast of Greenland. In proportion as the whole district was lowered, the general fall of the waters between the Alps and the ocean was lessened, and both the main and lateral valleys, becoming more subject to river inundations, were partially filled up with fluviatile silt containing land and freshwater shells. After this operation, when a thickness of many hundred feet of loess had been thrown down slowly, and in the course of many centuries, the whole region was once more upheaved gradually, but perhaps not equally, throughout the whole region. During this upward movement most of the fine loam was carried off by denudation to such an extent that the original valleys were nearly re-excavated. The country was thus restored to its pristine state, with the exception of those patches of loess still remaining, and which, from their frequency and their remarkable homogeneousness of composition and fossils, attest the original continuity and common origin of the whole. By introducing such general fluctuations of relative level, we may dispense with the necessity of erecting and afterwards removing a great barrier more than 1200 feet high, sufficient to exclude the ocean from the valley of the Rhine during the accumulation of the loess.

Dr. Fitton has again brought before us those curious phænomena in the Island of Portland from which the former alternate existence of sea, of dry land, and lastly, of a body of fresh water in the same place, all anterior to the formation of the chalk, has been clearly inferred. In the ancient soil, called in Portland, the "Dirt bed," the silicified trunks of trees and their roots are still preserved. Some curious facts are just published on this subject in the new Part of our Transactions, in a memoir by Dr. Buckland and Mr. De la Beche. After Mr. Webster had first made known the nature and existence of the dirt bed, Professor Henslow ascertained that between this and the marine oolite of Portland there were two other beds of carbonaceous clay, and in one of these Dr. Fitton has now found the remains of Cycadeæ, from which it appears that the forest of the dirt bed was not the first vegetation which grew on this tract. First there must have been the sea of the oolite, then land which supported Cycadeæ, then a lake or estuary in which freshwater strata were deposited, then again land on which other Cycadeæ and a forest of dicotyledonous trees flourished; then a second submergence under fresh water, in which new strata were formed; and finally, a return of the ocean in the South-east of England, when the greensand and chalk were superimposed upon the Wealden. The appearances in Portland alluded to by Dr. Fitton may be explained either by the alternate rising or sinking of the same ground, or by simply supposing one gradual and continuous subsidence in a region where a large and turbid river entered the sea. The conversion of certain tracts into land several feet high might be caused in a single year by river-inundations, and there might be sufficient time for a forest to grow upon these before the continued sinking down of the land (assuming it to have been constant) had time to cause the tract to be again submerged. I have before adverted to the petrified forest described by Mr. Darwin, in Chili, where the trees have grown on a bed of lava, and have then been covered by sand and sedimentary and volcanic matter 2000 feet thick. These facts seem to prove that the region of the Andes, instead of having been raised up suddenly and at once, a few thousand years before our time, as some have conjectured, has undergone, even since the commencement of the tertiary period, vast movements of depression as well as of elevation.

Among the modern changes of the surface of the globe which have been attributed to a depression of the earth's crust, I may mention the great cavity in Western Asia spoken of by Humboldt in his Asiatic Fragments. The supposed existence of a region of dry land 18,000 square leagues in area, surrounding the Caspian Sea, and below the mean level of the ocean, naturally excited the most lively curiosity. The fact was regarded for twenty years as established by a series of barometrical measurements made in 1811 by Professors Engelhardt and Parrot. The difference of level which these travellers assigned to the Caspian and Black Seas amounted to about 350 feet. But Professor Parrot, having revisited the tract in 1829 and 1830, soon found reason to doubt the accuracy of his former conclusions. He learnt that some Russian engineers had ascertained by careful measurements that the Don, at the place called Katschalinsk, where it is only sixty wersts distant from the Wolga, is 130 Paris feet *higher* than the latter river, and as the Don flows with much greater rapidity to the Black Sea than the Wolga does to the Caspian, the difference of level between the two seas, if any, must be considerably less than 130 feet. Parrot accordingly made a series of levellings from the mouth of the Wolga to Zarytzin, 400 wersts up its course, and from the mouth of the Don to the like distance; and these observations gave as a result that the mouth of the Don was between three and four feet *lower* than that of the Wolga! So that, according to this measurement, if there is any difference between the levels of the two seas, the Caspian is the highest! Baron Humboldt, who with other geographers had given full credit to the former statement of Parrot, very naturally refused to admit the validity of these new observations, unless the Professor was prepared to show that his former ones were less worthy of confidence. In reply to this, Professor Parrot, in his Appendix, admits that the barometrical instruments used in 1811 were imperfect, and that his former calculations also were in some respects inaccurate.

It appears to me perfectly natural that Baron Humboldt, M. Arago, and others, should have willingly admitted the supposed fact of a considerable variation between the levels of the Caspian and Black Seas. It is well known that the Mediterranean sustains its level at nearly the same height as the ocean, by drawing largely from the Atlantic on one side and from the Black Sea on the other. But if these constant supplies of water were cut off, if the Straits of Gibraltar and Constantinople were closed, and the Mediterranean became an inland lake isolated like the Caspian, its level must immediately fall. Its loss, by evaporation, would not be counterbalanced by the influx of river water, and there would then exist around its borders a tract of dry land lower than the ocean. It is true that we have no data for deciding to what extent this depression of level would reach; but it would present, at least on a small scale, a phænomenon analogous to that supposed to have been established in the case of the Caspian.

With every inclination to acknowledge and duly to appreciate the honest zeal with which Professor Parrot has laboured to correct his first error, I may remark that it does not yet appear why three or four years were lost after 1829 in putting the scientific world on their guard, and above all why the author of the Asiatic Fragments, published in 1831, was allowed to remain in ignorance of results previously obtained.

Gentlemen, I have now endeavoured to lay before you a brief sketch of the principal subjects referred to in the papers and in the discussions which have engaged the attention of the Society during the last year. I have confined myself exclusively to our own Proceedings; for the limits of this address would not allow me to give an analysis even of all the English works on Geology which have appeared since our last Anniversary, still less of all those which have been published on the Continent. A brief notice of these last would indeed require a volume, and this fact alone should inspire us with a feeling of strength and confidence in the future progress of Geology, which although it had scarcely obtained a recognised place among the sciences towards the close of the last century, has already risen into such importance as to excite a general interest in every nation throughout the world where the works of nature are studied.

LINNÆAN SOCIETY.

Dec. 15, 1835.—A communication from Charles C. Babington, Esq., M.A., F.L.S., on several new or imperfectly understood British and European Plants, was read.

Among the species whose history is elucidated in this paper are the following, viz.

1. *Crepis virens*, L. This is the *Crepis tectorum* of British authors, which must henceforth be removed from our Flora. *C. virens* has the leaves even at the margin, the achenia smooth, and shorter than the pappus. *C. tectorum* has the leaves revolute at the edges, and the achenia scabrous, equalling the pappus in length.

2. *Habenaria chlorantha*, and *bifolia*. The former is the *Orchis bifolia* of Fl. Brit. and Engl. Bot., and the latter is the *bifolia* of Linnæus, as is proved by the specimen in his Herbarium, and is identical also with the *Platanthera brachyglossa* of Reichenbach.

Among the additions to the British Flora we may reckon the following: 1st, *Herniaria ciliata*, a species hitherto confounded with *glabra*; from which it is principally distinguished by its ovate ciliated leaves. The Cornish habitat for *glabra* belongs to this species. 2ndly, *Polygonum maritimum*, L., found by Mr. Borrer on the sandy shore near Muddiford. 3rdly, *P. Raii*, the *marinum* of Ray's Synopsis, and the *aviculare* ε of Smith. 4thly, *P. dumetorum*, L., an interesting addition discovered by Mr. Hankey in a wood near Wimbledon. 5thly, *Euphorbia corallioides*, L., found at Slinfold, Sussex, and perhaps scarcely to be reckoned *indigenous*. It is the *pilosa* of the 1st edit. of Hooker's Brit. Flora. 6thly, *Erica Mackaii*, a species discovered on Craigha Moira, Cunnemara. In its essential characters it approaches to *E. Tetralix*; but in habit it resembles *E. ciliaris*.

March 1.—His Grace the Duke of Somerset, President, in the chair.

Some account of a species of *Agave* introduced accidentally into the Deccan; by Lieut.-Col. Sykes, F.R.S. &c.,—was read.

A number of young plants of this species came up accidentally in the garden of the collector at Poonah, in a border that had been appropriated the year before to a collection of bulbous roots that had been obtained from the Cape of Good Hope. One of the plants flowered in the fifth year after their first appearance. The height of the flower-stem was 25 feet. Although the flowers were apparently perfect, no seeds were produced. After the flowers had fallen, a multitude of small bulbs were produced on the branches. The species proves to be identical with the *Agave cubensis*, a plant discovered by Jacquin in the island of Cuba. It belongs to Ventenat's *Fourcroya*, a group of species distinguished from the normal Agaves by their dilated filaments, and by the thickened base of the style.

March 16.—Read a continuation of Dr. Hamilton's Commentary on the Hortus Malabaricus.

ZOOLOGICAL SOCIETY.

November 10, 1835.—At the request of the Chairman, Mr. Gould exhibited a specimen of the true *Lanner Hawk, Falco Lanarius*, Linn., and entered into some details with respect to its distinguishing peculiarities. Its real characters, he stated, have hitherto been so imperfectly understood as to have led to very general doubts as to its existence as a distinct species.

Mr. Gould also exhibited specimens of two species of *Pheasant*, both of very great rarity, which had recently come into his possession: they were the *Phasianus Sœmmeringii*, Temm., and the *Phas. versicolor*, Ej. He accompanied the exhibition by some remarks on the subdivisions which appear to him to be required among the *Phasianidæ* generally; and more especially on the position, among that extensive group, of the species exhibited.

Mr. Bell read "Some Account of the *Crustacea* of the Coasts of South America, with Descriptions of New Genera and Species; founded principally on the Collections obtained by Mr. Cuming and Mr. Miller. (Tribus 1, *Oxyrhynchi*.)"

The skeleton was exhibited of a *Coypus, Myopotamus Coypus*, Comm., together with preparations of some of the *viscera* obtained from the same individual, which recently died at the Society's Gardens. With reference to them some notes by Mr. Martin were read, which are given in the Proceedings.

Mr. Christy subsequently exhibited several skins of the *Coypus*, for the purpose of directing the attention of the Meeting to the position of the *mammæ* in the female, which are situated extremely high up the sides.

Nov. 24.—Mr. Yarrell exhibited a specimen of the *Syngnathus Acus*, Linn., with the view of again* calling the attention of the Society to the fact that the males in this species of *Pipe-fish* are furnished with a pouch under the tail, in which they bear about with them the *ova* until the young have escaped from the capsule; and which probably serves also as a place of shelter to which the young can, for some time after their exclusion, retreat in case of danger. In this individual the opened *abdomen* exhibited the preparatory organs of the male; and the displayed subcaudal pouch showed many eggs contained in it, the young of which were fully developed and ready to escape from the capsules, while from others the young had actually escaped. As a guide to those observers who may be desirous of procuring specimens equally illustrative of the peculiarity of this fish, Mr. Yarrell mentioned that the individual exhibited was obtained on the 20th of July.

Mr. Yarrell read some "Notes on the Economy of an Insect destructive to Turnips"; which he prefaced by adverting to the importance to agriculture of an attentive collection of those entomological facts which relate to species injurious to the ordinary crops of the farmer. He then proceeded to remark that the turnip crop is in this country usually infested in every season by two species of *Haltica*; and that another destroyer has been, in the dry summer of this year, superadded to them, especially on the light and chalky soils. To the history of this latter pest, which has been known to occur in those seasons only in which there has been an almost total absence of rain, Mr. Yarrell's paper is directed. A good account of a similar visitation in 1782, as it was observed in Norfolk by Mr. William Marshall, was published in the 'Philosophical Transactions' for the following year.

Early in July last the "yellow fly" was seen upon the young tur-

* See Lond. and Edinb. Phil. Mag., vol. vi. p. 383.

nips. It was remembered by some farmers that this was the fly which prevailed in the year 1818, and which was followed by the caterpillars known by the name of the blacks. The eggs being deposited by the perfect insect in the leaf of the plant, the black caterpillar or turnip-pest speedily makes its appearance, feeding on the soft portions of the leaves of the turnips and leaving the fibres untouched; and finally, casting its black skin and assuming one of a more slaty or grey colour, it buries itself in the earth. Lodged there, it forms for itself, from the soil, a strong oval cocoon; from which some of the earlier broods pass almost immediately into the perfect state, filled with *ova*, and ready quickly to supply another generation of destroyers. So complete and so rapid was the destruction in some instances, that a whole field was found, in two or three days, to present only an assemblage of skeletonized leaves; and this too when the turnips had attained a considerable size.

The insect whose proceedings have been thus briefly noticed, belongs to the *Hymenopterous* family *Tenthredinidæ*; it is the *Athalia Centifoliæ*, a species first noticed by Panzer. Mr. Yarrell describes the perfect insect and the caterpillar; and then recurs to the damage effected by the latter. By their repeated broods the devastation was continued for so long a time that even the third sowing did not in all cases escape destruction; and it was not until the occurrence of the heavy rains in September, terminating the unusually dry summer, that the mischief ceased. The destruction of the leaves caused, in most instances, the loss of the root also; and where the leaves suffered from the attacks of the black caterpillar, but not sufficiently to occasion the death of the plant, the turnip itself became pithy and of little value. It has become necessary, Mr. Yarrell states, to import the root largely from the Continent to supply the deficiency of the home crop.

The remedial measures adopted on a former visitation were the turning into the infested fields of a large number of ducks, who greedily devoured the caterpillars as they were brushed from the leaves by a boy with a long pole; the passing of a heavy roller over the ground at night, when the caterpillars were at their feed; and the strewing of quick lime by broad cast over the fields, renewing it as often as it was dispersed by the wind. The latter mode was generally considered as the most effectual preservative.

PROCEEDINGS AT THE FRIDAY-EVENING MEETINGS OF THE ROYAL INSTITUTION.

Jan. 22, 1836.—Mr. Faraday on silicified plants and fossils, and the proposed theories of silicification.

Jan. 29.—Dr. Ritchie. A view of the differential and integral calculus.

Feb. 5.—Mr. Brande on the manufacture of paper-hangings.

Feb. 12.—Dr. Grant on the structure of fishes, considered with reference to their aqueous element.

Feb. 19.—Mr. Faraday on the magnetism of metals as a general character (see page 177).

Feb. 26.—Dr. Lardner on steam communication with India.

March 4.—Mr. Fox on a mode of laying out and working oblique or askew bridges. (See p. 299 of the present Number.)

March 11.—Dr. Arnott on warming and ventilating buildings.

March 18.—Mr. Wheatstone on the means of investigating the structure of crystalline bodies by their sonorous vibrations.

LXV. *Intelligence and Miscellaneous Articles.*

ON THE ATTRACTIVE AND REPULSIVE FORCES OF MAGNETS AT VERY SMALL DISTANCES.

Note applicable to the Correspondence between Professor Ritchie and Mr. R. W. Fox, (see our last four Numbers,) on the attractive and repulsive forces of magnets at very small distances. Extracted by a Correspondent from a paper by W. Snow Harris, Esq., F.R.S., in the Trans. R.S. Edinburgh, 1831; dated July 1, 1827.

"IN the following table are the results of a series of experiments with the attracting and repelling poles. The magnets employed are indicated by the letters *a*, *b*, *c*, *d*, *e*, their dimensions being as follows:

a, A small cylindrical magnet 2 inches long, 0·2 of an inch in diameter, and similar in every respect to the suspended magnet *x* [on which its force was exerted].

b, 4·5 inches long, and 0·4 of an inch square.

c, 7·0 inches in length, and 0·7 of an inch diameter.

d, 9·0 inches long, 0·8 of an inch wide, and 0·3 of an inch thick.

e, 14·0 inches long, 1·0 inch wide, and 0·5 of an inch thick.

D signifies the distance; whilst the letters *a*, *b*, *c*, *d*, *e* are placed over the respective forces."

D	Dissimilar Poles.						Similar Poles.				
	a	*b*	*c*	*d*	*e*	5° of attraction equal one grain.	*a*	*b*	*c*	*d*	*e*
4·	...	...	...	...	3·+		...	...	...	...	3·+
3·5	...	...	...	...	4·+		...	...	...	...	4·+
3·	..	...	...	...	6·−		...	...	...	...	6·−
2·5	...	...	...	...	8·5		...	...	...	...	8·+
2·	...	...	2·5	3·	13·		...	...	2·	2·5	13·
1·8	...	...	3·+	3·5+	16·5		...	...	2·5	3·+	15·+
1·6	...	...	4·	4·5	21·		...	...	3·	4·+	18·5
1·5	...	...	4·5	5·5	23·		...	...	4·	5·	20·
1·4	...	...	5·5−	6·+	28·		...	...	4·5	5·5	23·
1·2	...	...	7·	8·5	38·		...	...	5·5	7·	28·
1·	1·5	2·	10·	12·	49·		1·5	2·	7·	9·	33·
0·8	2·+	3·+	15·	21·	...		2·	3·	10·	11·	42·
0·6	4·	6·−	25·+	32·	...		3·+	5·	14·	14·	56·
0·5	6·	8·	33·	40·	...		4·	6·5	15·5	14·+	60·
0·4	9·	11·5	...	...	...		6·	9·	17·	13·*	58·*
0·3	15·	18·	...	...	...		8·	11·	11·*	...	..

"These experimental results are quite consistent with the operations of the inductive influence [before explained]. We immediately perceive, by referring to the attractive forces, that the law of the in-

* At these distances the repulsive force was superseded by attraction.

verse square of the distance is manifest through all the approximations, except a few of the last, the occasional irregularities observed being very inconsiderable; so that when the magnets are very nearly approximated in relation to their respective intensities, the increments in the forces begin to decline,—a circumstance of considerable importance in our endeavours to investigate the laws of magnetic attraction; for it may be supposed that the inductive influence which thus begins to vary, may at last so far vanish, even *before contact, that the absolute force, at near approximations, may in some instances, as already stated, be in an inverse* SIMPLE RATIO OF THE DISTANCE*, and which was observed to happen with the bars marked *d* and *e*. For although the cylindrical counterpoise employed in these experiments did not admit of the forces being examined at nearer approximations than those marked in the table, yet by substituting one of large dimensions the forces may be carried on nearly up to the point of contact, so as to be estimated in terms of the preceding progression, since the degrees of attraction may be always compared and valued in grains of absolute weight."

"In the following table are the results of the experiments so continued with the magnets *d* and *e*; the counterpoise being 1·0 inch in diameter, 1° of attraction corresponding to 10° of the former, and being equal to two grains of absolute weight:"

Dissimilar Poles.		
D	*d*	*e*
0·4	6·	18·
0·3	8·5	24·
0·2	13·	36·

"It may be perceived in this table, that the corresponding forces at near approximations, do not materially vary from *a simple inverse ratio of the distance.*"

"This deviation from the law of the inverse square of the distance observed in all the near approximations of the magnets may happen either in consequence of the distant polarities having passed a certain limit, or otherwise from the inductive action not going on with the same freedom at some point approaching saturation. The latter would seem to be extremely probable; for it has been already shown, that when two dissimilar polarities are opposed to each other, their free action becomes more or less neutralized."—*Trans. of Royal Society of Edinburgh*, vol. xi. p. 310—312. W. J. H.

ON THE AURORA OF THE 18TH NOVEMBER LAST.

Communicated by Professor Rigaud.

During the beautiful aurora which took place on the 18th of last November, it is remarkable that Mr. Sturgeon was not able to see any of its light excepting in the north. Dr. Robinson, in the last number of the Phil. Mag. (p. 236.), has drawn an important conclusion from this circumstance: it may be right, therefore, to state

* The Italics and capitals in this passage are our Correspondent's.—EDIT.

that in Oxford the streams of light rose and continued for a considerable time to pass far beyond the zenith. A little, also, after nine o'clock, an arch like a long, luminous, narrow cloud extended from about the E.N.E. or N.E. by E. nearly across the heavens. It appeared to have an altitude of about 60° where it cut the southern meridian. The approximation of this situation to that of the magnetic equator would have given great value to this phænomenon, if the arch had been more definite in its form, and the place could have been more accurately determined in which it reached the horizon; but this last circumstance could only be collected from its passing near Jupiter.

The Oxford Herald of the 21st mentioned that this arch had been also observed at Banbury.

NOTE ON MR. ATKINSON'S PAPER INSERTED IN THE LAST NUMBER OF OUR JOURNAL, PAGE 188.

We regret that we should have given currency to a paragraph in the above-mentioned paper, which seems to imply a degree of negligence on the part of the Assistant Secretary at the Royal Society's, whose duty it is to make and record the Meteorological Observations there. If Mr. Atkinson had taken the precaution to make inquiries at the apartments of the Royal Society, he would have found that the anomalies and apparent errors to which he alludes are not owing to want of care and attention on the part of the Assistant Secretary, but to the position in which the instruments are placed; which position, although evidently not a good one, is the best which the *locale* of the Society presents. The instruments are all of the best kind, and of superior accuracy and workmanship.—Edit.

METEOROLOGICAL OBSERVATIONS FOR FEBRUARY 1836.

Chiswick.—February 1. Slightly overcast: fine. 2. Hazy: rain: barometer extremely low. 3. Rain. 4. Wet and stormy. 5. Hazy and cold. 6. Very fine. 7. Drizzly: cloudy and mild. 8, 9. Overcast. 10. Showery. 11. Clear and cold. 12. Cold and windy. 13. Sharp frost: fine. 14. Fine. 15. Clear and frosty. 16. Frosty. 17, 18. Clear, cold and windy. 19, 20. Sharp frost: fine but cold. 21. Frosty: clear. 22, 23. Overcast. 24. Overcast: fine. 25. Frosty: fine. 26. Sleet; rain. 27. Drizzly. 28. Hazy: cloudy and fine. 29. Overcast.

During the first three days of the month the barometer fell remarkably low, and particularly on the 2nd, on which day it was lower than it has probably been for many years in the vicinity of London. The fall of rain was not remarkably great, nor was the temperature at nights below freezing; but in the country the fall of snow was, at the same time, unusually deep, and the storm so excessively violent that the mails were in many instances obstructed in consequence.

Boston.—February 1. Cloudy and stormy: rain early A.M. 2. Fine: snow P.M. with rain. 3. Cloudy: rain early A.M. 4. Cloudy and stormy: rain P.M. 5. Cloudy. 6. Fine. 7. Cloudy: rain early A.M.; rain P.M. 8. Cloudy: rain P.M. 9. Fine. 10. Rain: rain P.M. 11. Fine and stormy. 12. Stormy. 13. Fine. 14. Cloudy. 15. Fine. 16. Cloudy. 17. Stormy: snow A.M. and P.M. 18. Stormy. 19, 20. Fine. 21. Cloudy. 22. Fine. 23—25. Cloudy. 26. Cloudy: rain P.M. 27. Cloudy. 28. Cloudy: rain P.M. 29. Rain.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; and by Mr. VEALL at Boston.

Days of Month. 1836. February.	Barometer.				Thermometer.						Wind.			Rain.			Dew-point.
	London: Roy. Soc. 9 A.M.	Chiswick.		Boston. 8½ A.M.	London: Roy. Soc.			Chiswick.		Boston. 8½ A.M.	London: Roy. Soc. 9 A.M.	Chisw. 1 P.M.	Bost.	London: Roy. Soc. 9 A.M.	Chisw.	Boston.	Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
		Max.	Min.		Fahr. 9 A.M.	Self-registering. Min.	Max.	Max.	Min.								
M. 1.	29·136	29·347	29·312	28·88	40·5	37·3	45·0	36	35	38·5	SW.	SW.	W.	·175	·01	·06	36
○ T. 2.	28·768	28·907	28·688	28·59	38·2	35·5	40·7	40	36	33	ESE.	E.	S.	...	·48	...	33
W. 3.	28·740	29·456	28·937	28·78	36·7	35·8	39·3	40	34	37	N.	N.	calm	·481	·34	1·04	35
Th. 4.	29·598	30·176	29·764	29·55	38·3	35·2	38·6	38	37	40	NE. var.	NE.	E.	.300	·10	...	36
F. 5.	30·014	30·227	30·134	29·91	38·2	36·5	38·9	40	32	38	N. var.	NE.	calm	·038	...	·04	35
S. 6.	29·825	30·038	30·015	29·51	39·0	33·2	47·5	52	37	42	W.	SW.	W.	...	·01	...	34
☉ 7.	29·614	29·964	29·826	29·36	43·2	37·8	46·7	50	31	41·5	WSW.	NW.	calm	...	·03	·09	37
M. 8.	29·885	30·095	29·911	29·62	39·2	37·2	47·8	49	43	36	E.	SW.	calm	·047	·01	·02	35
T. 9.	29·754	30·065	29·937	29·35	47·8	38·2	50·6	52	46	48	SSW.	SW.	W.	...	...	·03	39
☾ W. 10.	29·739	29·937	29·815	29·37	47·7	45·8	50·3	51	36	44	S. var.	SW.	calm	...	·10	·04	45
Th. 11.	29·897	30·389	30·101	29·55	39·2	36·4	41·2	42	31	36	SW.	NW.	NW.	·202	...	·20	35
F. 12.	29·946	30·250	30·104	29·56	39·7	32·8	47·6	49	26	42·5	SSW.	W.	W.	...	...	...	32
S. 13.	30·213	30·429	30·409	29·95	35·7	33·3	42·7	46	35	33	W.	SW.	calm	...	...	...	33
☉ 14.	30·255	30·506	30·146	29·91	42·6	34·2	48·6	50	28	43	SW.	W.	NW.	...	...	...	37
M. 15.	30·319	30·527	30·483	29·98	38·5	34·2	47·7	53	25	37	SW.	SW.	calm	...	...	...	33
● T. 16.	30·178	30·399	29·909	28·80	38·9	34·5	47·0	48	32	39	SW.	SW.	calm	...	...	...	35
W. 17.	29·711	29·934	29·877	29·43	34·2	31·8	39·0	40	36	34	S. var.	N.	N.	·222	·02	...	23
Th. 18.	29·893	30·312	30·080	29·80	38·2	33·3	38·2	41	31	41	N. var.	E.	E.	...	...	·11	32
F. 19.	30·119	30·376	30·327	30·03	33·6	31·2	36·2	38	23	35	S.	N.	calm	...	...	...	27
S. 20.	30·231	30·474	30·450	30·05	30·2	26·5	36·2	39	19	31	N.	E.	calm	...	...	...	25
☉ 21.	30·180	30·407	30·218	29·92	29·1	25·3	38·3	42	28	34·5	NW.	SW.	calm	...	...	...	25
M. 22.	29·802	30·045	29·837	29·54	39·2	27·8	37·7	45	28	36	SSW.	SW.	calm	...	...	...	30
T. 23.	29·481	29·700	29·603	29·27	38·0	34·2	43·2	43	26	40	SSW.	SW.	calm	...	...	...	33
☽ W. 24.	29·219	29·537	29·160	29·12	38·6	32·9	44·3	46	30	35	NE. var.	S.	calm	...	...	...	33
Th. 25.	28·883	29·098	29·068	28·73	34·7	32·4	40·0	45	26	33	SSW.	SW.	calm	...	...	...	33
F. 26.	28·786	28·996	28·980	28·80	35·6	31·7	37·9	37	34	34	E.	E.	calm	·061	·46	...	33
S. 27.	28·730	29·177	28·950	28·64	36·6	34·2	37·7	37	30	38·5	NW.	SW.	calm	·494	·01	·17	35
☉ 28.	29·103	29·379	29·316	28·96	34·2	33·2	38·5	40	32	34	SW.	NW.	calm	...	·04	...	32
M. 29.	29·287	29·530	29·488	29·15	37·4	33·3	39·8	33	33	36	SSW.	N.	calm	·036	...	·07	34
	29·631	30·527	28·688	29·38	38·0	34·0	42·3	53	19	37·5				Sum 2·056	1·61	1·87	33·3

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LXVI. *On the Action of Hydrochloric Acid on certain Sulphates, and particularly on the Sulphate of Copper.* By ROBERT KANE, *M.D. M.R.I.A.**

THE following experiments were instituted in consequence of some results casually arrived at, whilst engaged on another subject; and as they possess a certain interest with regard to the theory of the hydracids, and are also additions to the number of real facts in chemistry, I have considered them deserving of the notice of the Royal Irish Academy.

When bluestone $(\dddot{S} + \dot{Cu}) + 5\dot{H}$, is dissolved in liquid muriatic acid, there is produced a considerable reduction of temperature, viz. from about 65° to about 35°. The solution becomes deep grass green, and by evaporation yields needles of hydrated chloride of copper. If there is taken a quantity of sulphate of copper corresponding to the atomic weight, and a quantity of liquid muriatic acid corresponding to an atom of dry acid, and the solution be effected by heat, on cooling, the whole solidifies into a fibrous mass of hydrated chloride of copper, and there is no bluestone remaining undecomposed. The sulphuric acid remains in the water. When the atomic proportions are not accurately preserved, small crystals of bluestone are scattered through the mass of chloride; but the latter can be obtained pure by carefully attending to this point. In this reaction we have evidently a complete in-

* Communicated by the Author.

version of the ordinary rules of chemical affinity. The sulphuric acid ranks much higher in affinitary [?] power than the muriatic acid, and yet is completely displaced by the latter from its state of union with the black oxide of copper. The theory of this reaction is very easily understood. These are

$(\dddot{S} + \dot{Cu} + 5\dot{H}) + Cl\,H$, and there are formed $(Cl + Cu)$

$+ \dddot{S} + 6\dot{H}$. The sudden liberation of the larger quantity of water from its state of solidity in bluestone produces the remarkable reduction of temperature.

I have sometimes observed that when the crystallized chloride of copper is allowed to remain for a long time in contact with the strongly acid mother-liquor, a reverse action is set up, and small crystals of sulphate begin to appear disseminated through the mass. I several times analysed these crystals, in order to ascertain whether a sulphate of the chloride of copper, like Peligot's chromate of the chloride of potassium, would be formed, but without effect; no definite compound could be detected.

The interesting nature of this reaction made it important to ascertain the action of sulphate of copper upon dry muriatic acid gas. The experiments for this purpose were conducted in the following manner. A bulb tube was connected at the ends with strong glass tubes containing fragments of dried chloride of calcium. The one tube was by its other extremity connected to a retort, in which muriatic acid gas was disengaged by the action of oil of vitriol on fused chloride of sodium. The other desiccating tube was of much smaller size, so as to allow of being weighed in a delicate balance; to the remote extremity of this a small quill tube was attached, by which the excess of gas made its escape. The sulphate of copper in fine powder was introduced into the weighed bulb tube, and the whole then weighed to determine the quantity employed; the desiccating tubes were then attached, and the muriatic acid gas disengaged. Having been dried in its passage over the first chloride of calcium, it came into contact with the bluestone, by which it was rapidly absorbed; and any water that was formed or disengaged, carried away by the current of dry gas in excess, was deposited in the small desiccating tube, where its quantity could be accurately determined.

When the crystallized bluestone $(\dddot{S}\,\dot{Cu} + 5\,\dot{H})$ in fine powder is put into the tube, it absorbs rapidly the muriatic acid gas, and becomes grass green: great heat is produced. Drops of moisture appear on the cold portions of the tube. It loses its pulverulent texture, and is converted into a mass of silky pale green crystals: on the heated portions of tube, points of a chocolate brown matter are produced. The current of gas being continued until all action ceased, and the tube and its contents had cooled to the ordinary temperature of the room, the apparatus was weighed, and the bluestone was found to have absorbed rather more than one atom of muriatic acid, the excess being attributed to the quantity absorbed by the water disengaged.

The mass of green crystals thus obtained is very deliquescent, excessively acid, and gives fumes, arising probably from some muriatic acid in excess. Dissolved in water it yields by crystallization the hydrated chloride of copper in long needles.

When there is used sulphate of copper, either quite dry, or retaining one atom of water, the effect is so nearly similar as to allow of the same description serving for both.

$\dddot{\mathrm{S}}\,\dot{\mathrm{Cu}}$ or $\dddot{\mathrm{S}}\,\dot{\mathrm{Cu}}\,\dot{\mathrm{H}}$ absorb muriatic acid rapidly, and assume a dark chocolate brown colour. The mass becomes slightly coherent as if some water became free; but the second desiccating tube does not increase in weight in any perceptible degree. The process is accompanied by the evolution of so much heat as occasionally to crack the tubes; but the passage of the gas must be continued for a long time after the whole has become cold. The amount of gas absorbed then approximates very closely to one atom, but it seldom absolutely attains the theoretical quantity; it can approach, however, within one per cent., and we may consequently consider that one atom is the quantity absorbed.

This brown matter is possessed of interesting properties. When heated it gradually and readily parts with its muriatic acid gas, leaving behind the sulphate of copper unaltered. Exposed to the air it rapidly absorbs water, with the evolution of heat, and becomes apple green, a change which occurs instantaneously if a few drops of water be allowed to fall upon it. Dissolved in water it forms an apple green solution; and by crystallization gives the crystallized chloride of copper, sulphuric acid remaining in the liquor.

Two theories may be conceived of the nature of the body thus formed: One, that the chloride of hydrogen is absorbed by the sulphate of copper and combines with it as water would do,—that, in fact, the so-called muriatic acid is capable of replacing the water of crystallization of salts as ammonia and phosphuretted hydrogen have been shown to do by Rose and Graham: The other, that the chloride of hydrogen reacting on the oxide of copper forms water and chloride of copper, while the latter with the sulphuric acid constitutes a sulphate of a chloride. The general nature of its properties inclines me to believe the former to be the true idea, that the chloride of hydrogen exists as such in the brown powder, and that chloride of copper is only formed when the decomposition is effected with the presence of much water.

The singular results of the reaction just described rendering an examination of the influence of muriatic acid on the sulphates in general highly interesting, experiments were instituted, of which the results shall be very briefly stated.

Dry muriatic acid was passed over sulphates in the apparatus before described. With the sulphates of potash, soda, zinc, magnesia, iron, alumina, and lead, no action was observed; these salts did not change in weight or in appearance. On the other hand, the sulphates of nickel and of quicksilver absorb muriatic acid very gradually, with the evolution of heat, the absorption ceasing when half an atom has been taken up. These compounds lose the gas they had absorbed, by exposure to the air during some time, and immediately on being heated. If they be put into water, the sulphate is deposited pure, the muriatic acid remaining in the water.

Sulphate of potash dissolves in liquid muriatic acid with some evolution of heat; and if by means of heat two atoms of sulphate of potash be dissolved in a quantity of liquid containing an atom of real muriatic acid, there separate on cooling finely formed crystals (rhomboidal plates) of bisulphate of potash, with opake cubes of chloride of potassium. A great number of analyses of the crystals obtained by such reaction was made to determine whether the sulphate of chlorkalium corresponding with the chromate had any existence, but no trace of its being formed could be obtained. Bisulphate of potash crystallizes from its solution in liquid muriatic acid unaltered. Sulphate of ammonia similarly treated gives precisely similar results.

It has been long known that Glauber's salt treated with muriatic acid constitutes a powerful freezing mixture, the theory of which is at once explained by the results of the experiment. When sulphate of soda is dissolved in liquid muriatic acid there are formed bisulphate of soda and chloride of sodium, and as the former salt crystallizes only with four atoms of water, the remaining quantity of the water of crystallization of the Glauber's salt is disengaged, to the amount of sixteen atoms: thus, $2\{(\dddot{S}+\dot{Na})10\,H\}+(Cl+H) =$

$$\{(2\,\dddot{S}+\dot{Na})+4\,\dot{H})\}+(Cl+Na)+17\,H.$$

This large quantity of water suddenly separated from a state of combination in which it had been solid, produces, by its absorption of caloric of liquidity, the frigorific property.

The sulphates of zinc and magnesia dissolve in muriatic acid, and by cooling or evaporation are obtained unaltered. The muriatic acid does not appear to produce any change of nature.

When protosulphate of iron is dissolved in muriatic acid, the liquor furnishes by crystallization quantities of unaltered sulphate and of chloride of iron. Sometimes the sulphate retains its common quantity of water of crystallization, but at others I have obtained a salt giving by analysis:

Sulphuric acid	18·7	$\dddot{S}$ =	19·5
Protoxide of iron	16·7	$\dot{Fe}$ =	17·3
Water and loss	14·6	$3\dot{H}$ =	13·2
	50·0		50·0

The crystals were always so aggregated that their form could not be accurately determined; they are transparent, harder, and of a much lighter green than ordinary copperas; they are quite permanent, and when dissolved in water give sulphate of iron with the ordinary quantity of water.

The sulphate of alumina crystallizes unaltered from its solution in muriatic acid, but in more beautiful plates than from water. From the solution of sulphate of nickel or of mercury in muriatic acid, these salts are deposited by crystallization unchanged.

23, Lower Gloucester Street, Dublin: March 25, 1836.

LXVII. *An Abstract of a Memoir on Physical Geology; with a further Exposition of certain Points connected with the Subject. By* W. Hopkins, *Esq., M.A., F.G.S., of St. Peter's College, Cambridge.*

[Continued from p. 281, and concluded.]

IV. THE two systems of fissures which I have described are those which must be regarded according to this theory as *primary phænomena*, from which, as before stated, the *secondary phænomena* of mineral veins, faults, anticlinal lines, &c., must be derived. For this second part of the subject, I must refer to the second Section of my Memoir, where I have entered in detail into the manner in which these latter phænomena may be conceived to be derived from the former. The number of phænomena which we are thus enabled to account for, as the consequences of a simple cause from which they are deducible by strict mechanical reasoning, appears to me, in the present state of our speculations and practical knowledge, to give to the theory I have been attempting to develop the strongest claim to the attention of geologists.

It will be observed, that a most essential part of this theory consists in the relation which it assigns between the directions of dislocation and the general configuration of the elevated mass *at the instant previous to its rupture.* It may, at first sight, appear impossible to ascertain what this form may have been, now that we can only examine the mass in its dislocated state; but this difficulty, though it must always exist in a greater or less degree, will not appear so serious a practical one, when we consider that since necessary relations must exist between the form of the mass at the instant above mentioned, and the lines of dislocation, and again between these lines and the actual disturbed form of the mass, some such relations must also exist between this latter and the previous form. Thus if the actual form be approximately conical, we may conclude it to have been also conical at the instant of dislocation; and if the disturbed district be of great length as compared with its width, and presents a well-defined axis of elevation, we may conclude the unbroken elevation to have been approximately cylindrical. Partial elevations which may have been superimposed upon a general one, as already described, at the instant previous to dislocation, must generally be more difficult to detect, since it may frequently be impossible to distinguish present indications of them from similar elevations which may have been produced by the elevatory force entirely *subsequently* to the formation of the fissures. On such points the observer must of course exercise his discrimination and judgement. Deviations from rectilinearity or parallelism in the lines of dislocation are not to be regarded necessarily as anomalies. We frequently speak, it is true, of the law of parallelism in such phænomena, as if that were their essential characteristic, but it is manifest that, according to our theory, this is only a secondary property in them, depending on the rectilinearity of the general axis of elevation. In conical elevations such as those before alluded to (p. 233) there is no approximation to parallelism in the observed fissures; and if the general axis of elevation be curvilinear, the longitudinal fissures preserving their parallelism with it (according to theory) will be also curvilinear, while the transverse fissures perpendicular to the former at their points of intersection will no longer be parallel. These deviations from rectilinearity and parallelism are due to the action of the general elevatory force, and to the nature of the general elevation. Others more limited may be due to partial elevations. Where they are observed, the local configuration of the mass must be examined, and thence the directions of the consequent tensions superimposed on that of the general mass must be inferred. The equation of page 273 will then determine the resulting directions of the fissures, and therefore the nature of the deviations. In simple cases, the remarks in page 276–277 will enable us to do this with sufficient accuracy to compare the theoretical deduction with the observed phænomena.

V. It has already been stated, that all consideration of those portions of the earth's crust, in which a regularly jointed or laminated structure may have prevailed previous to their elevation, has been excluded from the investigations contained in my memoir, because I wished to keep them distinct from any speculations respecting the operation of other causes than the one whose effects I proposed to develop. In our general speculations, however, on theories of elevation, it is necessary to consider how far these dislocations which, according to the theory I have been discussing, must be regarded as primary phænomena, are anything more than secondary ones, depending on lines of less resistance, produced by some such particular structure as that above mentioned. Much valuable information respecting joints may be expected from the forthcoming work of Professor Phillips on the Geology of Yorkshire; but at present we know but little accurately about this important feature in the structure of rocks, and of its cause, I conceive, absolutely nothing. We have, therefore, no positive reason to conclude that this peculiarity of structure has generally been superinduced after the elevation of the mass in which it exists, though in some cases there appears little doubt of such having been the fact. It is highly important, however, to determine whether any perfect coincidence does exist in the directions of dislocation, and of joints in the same district; and it is to be hoped that geologists will direct their earnest attention to this subject. Should this coincidence be established generally in districts where the fractures are parallel and at right angles to each other, it will still be important to ascertain how far it exists in elevations approximating to the conical form; and, in all cases, whether the directions of joints bear any relations to the configuration of the mass, as modified by partial elevations. All these are points of interest which we may hope by accurate and careful observation to determine. Should the coincidence, however, between joints and lines of fracture be perfectly established, we shall still have to consider whether the joints, by their prior existence in the undisturbed mass, have determined the lines of fracture, or [whether] these latter phænomena have exercised an influence in determining the positions of the joints, supposed to be subsequently formed in the elevated mass. Now, assuming the coincidence just mentioned, there must of course be the same relations between the general conformation of the elevated mass and the directions of joints, as those which have been already stated to exist between that conformation and lines of dislocation; and therefore, if we assume the prior existence of joints, and also that the lines or axes of elevation have been principally determined by the points of application of the elevatory force in the lower portion of the elevated mass, we must necessarily conclude, that some relation must exist between the causes which have produced the jointed structure, and the action of the elevatory force; *i. e.* between the action of a force extraneous to the mass, and that internal molecular action, to which it would seem absolutely necessary to refer the formation of joints in the undisturbed mass. To assert such relation to be physically impossible, would, in the present state of our knowledge, perhaps, be absurd; but it does appear to me that the difficulty of conceiving it is so great as to form a most serious, if not a fatal objection, to any theory in which it should be involved as a necessary consequence. To avoid this objection, we might proceed on the hypothesis, that the lines or axes of elevation have been principally determined by the lines of less resistance along the joints, rather than as above supposed; and such might be the case, if the principal line of action of the elevatory force* should not deviate materially from either of the two directions at right angles to each other in which we are assuming joints to exist. If, however, that principal line should approximate to an angle of 45° with these rectangular directions, and should be of considerable length, the hypothesis would be, I conceive, altogether inadmissible.

Supposing then the accurate coincidence of the directions of joints and those of fracture to be hereafter established, it would appear that the hypothesis of the laws discoverable in lines of fracture being generally due to the prior existence of some regular structure in the undisturbed mass, would still involve serious difficulties, on account of the relations existing so generally between the lines of fracture and the configuration of the elevated mass, for which, with the above hypothesis, it

* It must be recollected that we can only judge of the superficial form and dimensions of the mass to which the elevatory force has been applied, by those of the actual elevations. These appear unquestionably, I conceive, to justify the notion of sufficiently determinate *lines of action* of the elevatory force, at least in a sufficient number of instances to give due weight to the argument in the text.

seems almost impossible to conceive any efficient physical cause. On the other hand (still supposing the coincidence of the directions of joints and of lines of fracture), if we assume the formation of joints to have been posterior to the elevation of the mass, this coincidence will still remain to be accounted for. Our ignorance, however, of the process by which this structure may have been superinduced, will not at present allow us to do this. The fact must continue to offer a theoretical difficulty, but one, I conceive, very different in its nature to that above stated, since it would appear, I think, probable that, taking a portion of the elevated mass bounded by adjoining fissures, the position of the joints subsequently formed in it should have some relations to the boundaries of that portion. The fact, therefore, of the coincidence (or rather parallelism) of direction above mentioned, while it presents a difficulty, does not seem to offer any *à priori* objection to the theory which involves it. It is proper, however, to observe that though it should appear, for the reasons now stated, that a preference may generally be due to the theory which would assign the production of fissures to the elevatory force alone, we should by no means be justified in the rejection in every instance of that which attributes the directions of those fissures to the previous structure of the mass, and especially in those cases in which we fail to recognise distinct lines of elevation, or the usual relations between them and the lines of dislocation. And here it may be remarked as a striking fact, that the only mining district in this country in which there is any difficulty (as far as I have yet ascertained) in tracing these relations, is that in which, for independent reasons, it appears most necessary to recognise the influence of a previously veined or jointed structure on the directions of its dislocations. I allude to the mining district of Cornwall.

In the above reasoning I have assumed the *accurate* coincidence of the directions of joints and of lines of fracture, and it is important to observe that this accuracy of coincidence is essential to the theory which would assign the latter phænomena to the prior existence of the former. A difference of a few degrees in the angular positions of the above lines would, if clearly established, be fatal to this theory, because, as I have already explained, although a fissure produced by an elevatory force would cross a line of less resistance under a certain condition, without change of direction, that condition cannot be generally satisfied when the angle between the fissure and line of less resistance is small, and in such case the fissure will be propagated exactly along the latter line. Observations on this point would therefore demand great care and accuracy.

It is also important to remark, that the accurate coincidence above spoken of will require two coexisting systems of joints to be at right angles to each other, since such is the law recognised in lines of dislocation. Observation, however, so far as it has proceeded, appears in many instances, I believe, to contradict this law in the directions of joints. Should any other laws be established hereafter, or very frequent deviations from the one just mentioned, it will probably be found necessary to abandon altogether the notion that lines of dislocation have been principally determined by the directions of joints, rather than by the mode of action of the elevatory force.

The theory which it has been the object of my memoir to develope, enables us to account for nearly all the more important phænomena of elevation; but before we finally decide on its relative claims to our adoption, we are manifestly called upon to remove as far as possible, by accurate observation, the uncertainty which still remains respecting the possible influence of a jointed structure in producing what I have termed the primary phænomena of elevation. These speculations are thrown out with the hope of indicating some of the more critical points of inquiry on which the ultimate determination of this question must turn, and which are generally best indicated in such cases by theoretical discussion. The necessary relations which I have shown must exist, according to one of these theories, between the directions of dislocation and the general, and in some cases local, conformation of the elevated mass, will probably do much towards enabling us ultimately to decide between them; and it is therefore of the first importance that the observer who may hereafter wish to elucidate this subject, should remark these relations as carefully as those which may exist between the dislocations themselves, or the joints with which they may be associated *.

We may here observe, that the only difference between the two theories we have considered, consists in the cause which they assign for what I have termed, with reference to the

* It would be important, as before intimated, to observe the directions of joints in a conical elevation with lines of dislocation diverging from its vertex. I am not aware that the existence of a similarly diverging system of joints has ever been suspected. It would also be highly desirable to observe whether there be any continuity in the joints of two contiguous but distinct formations, and particularly when one formation is primary and the other sedimentary. The perfect continuity of the veins in Cornwall, in passing from the killas to the granite, forms a curious feature in the geology of that district, if we are to regard the former as a sedimentary deposit. In such case, it would clearly demonstrate that the regular structure to which, I conceive, many of those veins must be referred, was superinduced in that district after the great dislocations which must have accompanied the injection of the granite.

theory with which I have been more particularly occupied, *primary* phænomena. The *secondary* phænomena of faults, mineral veins, anticlinal lines, valleys, &c., will be deducible from the primary ones just in the same manner in both theories, so that nearly the whole of the investigations contained in the second section of my memoir will be equally applicable to both these theories.

In that section I have entered, as before intimated, with considerable detail into an examination of the secondary phænomena of elevation, such as anticlinal lines, longitudinal and transverse valleys, ejected and injected horizontal beds of trap, veins of trap and granite; and also the different phænomena of mineral veins, such as the *throw* and depth of a vein, the comparative widths of the best bearing veins and cross courses, and the *shifts* or *heaves* so frequently recognised at the intersections of veins. I have also stated reasons for concluding that the fissures of mineral veins must have been filled by some process of infiltration or segregation (which I profess not further to define) from the surrounding mass; and here, viewing this point with reference to the subject of joints, I would further observe, that the formation of a vein (by which is here meant the matter contained in the fissure) might take place along an open joint, exactly in the same manner as along a fissure produced by any other means. If, therefore, we find veins (such as those before alluded to in Cornwall) which cannot be supposed to originate in the dislocating effects of an elevatory force, we should carefully examine how far the directions of these veins appear to coincide with those of the leading joints. From a hasty inspection of the Cornish veins, I have a strong impression that this coincidence will be found to exist in that district. It would be important to ascertain this fact by careful and detailed observation; for, should it be established, it will immediately destroy the hypothesis of the *contemporaneous formation* of such veins as a necessary alternative, and at least remove one inconceivable process from the speculations of geologists, more especially with respect to those who may at once be disposed to allow this mode of formation of the Cornish veins, while they contend for the fact of the mass in which they are found being a sedimentary deposit. The difficulty of the theory of all similar veins will be reduced by my hypothesis to that of the formation of joints, a process hard enough to conceive, but which has its analogy in that of crystallization, and must of necessity be recognised. The process of contemporaneous formation of veins, without the previous formation of fissures as receptacles for the segregated or infiltrated matter, appears to me inconceivable in itself, and unsupported by any analogies drawn from the known operations of nature.

VI. There is another point on which I have touched incidentally in the conclusion of my memoir—the application of the principles already explained to the theory of Elie de Beaumont, respecting the parallelism of mountain-chains of contemporaneous elevation. I have before stated, that in whatever manner we may conceive an elevatory force to be produced, there seems no reason why we should not suppose it, in some cases, to have acted at a much greater depth than in others. Now I have already explained (p. 278) the reason for concluding that the formation of a system of fissures, according to our theory, must be simultaneous; and also how the simultaneous formation is facilitated by the circumstance of *time* being necessary for the transmission of the relaxation of the mass produced by the opening of a fissure. From that explanation it will easily be seen, that if a number of fissures commence simultaneously in the lower portion* of an elevated mass of great thickness, and great superficial extent, it is most probable that those only will reach the upper surface which are remote from each other. These fissures will be large, and all the phænomena resulting from them may be expected to be on a proportionate scale. Anticlinal lines † will almost necessarily be formed along them; and thus it is as easy to account for two parallel mountain ranges, as for two neighbouring anticlinal lines on a scale of comparatively small magnitude; and our theory will thus assign a physical cause for the law of parallelism in mountain chains of contemporaneous elevation, as contended for by M. Elie de Beaumont, if, at least, the application of that geologist's theory be restricted within certain limits. I have no intention, however, of now insisting on this extensive action of the physical cause I have been considering; but I would observe, that the extent of this action can only be determined by that of those portions of the earth's surface throughout which the laws of observed phænomena may be continuous, and in accordance with our theoretical deductions.

To persons not habituated to the investigation of the accurate relations between mechanical causes and their effects, much of the previous reasoning may appear too refined to be applicable to our subject; but it must always be recollected, that this reasoning is immediately applied to hypothetical pro-

* I have shown that these fissures must generally commence in some lower portion of the elevated mass. See Memoir, p. 43.

† See Memoir, p. 51.

blems, to which it is strictly applicable, and which are chosen so as to bear the closest analogy to the corresponding ones which nature presents to us; and it is simply on the strictness of this analogy that we are called upon to decide, in judging of the admissibility of our mode of investigation. It is important to have a clear conception of this principle, on which the application of strict analysis to the problems of nature must always be made. In fact, this is the principle on which every one must tacitly (sometimes perhaps unconsciously) proceed, in forming a distinct idea of the necessary relations between any physical cause acting under complicated conditions, and its remoter consequences. We must form our conclusions from the consideration of some comparatively simple but strictly analogous case, and apply them to the actual one, with such limitations as circumstances may require. The advantage which the mathematician possesses, consists in this, —that the standard case to which he refers his more complex problem, is a definite one, from which he has means of deducing his results free from that uncertainty which necessarily attends other modes of investigation. It is a standard case of this kind, which I have endeavoured to supply for geological theories of elevation; nor am I without hopes, that the attempt may at least so far succeed as to remove some of that indefiniteness on this subject, by which the earlier speculations in every science must almost necessarily be characterized. More particularly, perhaps, may this be asserted of geology, which, notwithstanding the rapidity of its growth, is yet hardly strong enough to emerge from the cloudiness in which its phraseology alone, with reference to the phænomena of elevation, by addressing itself more to the imagination than the judgement of the student, has sometimes been sufficient to involve it. An impression has thus been too frequently created, that little hope exists of elevating the science to any rank among the stricter physical sciences. Such a notion, however, appears to me most fatal to its healthy progress. The author of the *Principles of Geology*, whatever may be thought of some of his theoretical views, must be allowed by all to have set us an example well calculated to improve in this respect the tone of geological speculation, in as much as he has boldly grappled with the difficulties of his problems in detail, and not been content to meet them with indeterminate generalities. In these investigations I have endeavoured to act upon the same principle, as the only one on which, if we are to speculate at all, we can speculate with safety; and if, perchance, a somewhat vague and misty sublimity which has appertained to this branch of the science should thus be diminished, ample compensation will be made if we should in return confer upon it a portion, however small, of the more naked dignity of demonstrative truth.

St. Peter's College, Jan. 7, 1836.

LXVIII. *Catalogue of Fossil Fish in the Collections of Lord Cole and Sir Philip Grey Egerton, arranged alphabetically; with References to the Localities, Geological Positions, and published Descriptions of the Species.* By Sir PHILIP DE MALPAS GREY EGERTON, *M.P., F.R.S., F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following "Catalogue of Fossil Fish" was printed for private distribution, but I am induced to solicit its insertion in your widely-circulated Journal in the hopes that it may prove of interest beyond the immediate pale of my personal friends and acquaintance;—to the geological adept, as exhibiting in a tabular form the stratigraphical position of two hundred and twenty-seven species,—to the student in fossil ichthyology, as affording a clue to the depositories of many new and rare specimens destined to appear in the forthcoming numbers of Dr. Agassiz's "Recherches sur les Poissons Fossiles."

To the discriminating eye and classic orthography of that distinguished naturalist I am indebted for the diagnosis and nomenclature of the new genera and species enumerated in the Catalogue, as well as for the identification of such as were raleady known. I remain, Gentlemen, yours, &c.

Oulton Park, Feb. 6, 1836. PHILIP GREY EGERTON.

The letter C or E prefixed to a species denotes the Collection to which it belongs when not common to both.

	GENUS AND SPECIES.		STRATA AND LOCALITIES.
	Acanthoderma spinosum		Engi, canton Glaris.
	Acanus oblongus*		Engi, canton Glaris.
C	——— *new species not yet named*		Engi, canton Glaris.
E	Acipenser, *new species not yet named*	(*Lias.*)	Lyme Regis.
	Acrodus nobilis	(*Lias.*)	Lyme Regis.
C	——— gibberulus	(*Lias.*)	Lyme Regis.

* This genus is found in the Pläner Kalk.

	GENUS AND SPECIES.	STRATA AND LOCALITIES.
c	Acrodus Bronnii	(*Lias.*) Brunswick.
c	——— Guilliardetti	(*Lias.*) Brunswick.
	——— *not yet named*	(*Lias.*) Lyme Regis.
	Amblypterus eupterygius. *Agass.* vol. ii. p. 36	(*Coal Formation.*) Lebach.
	——— lateralis. *Agass.* vol. ii. p. 39	(*Coal Formation.*) Lebach.
	——— latus. *Agass.* vol. ii. p. 37	(*Coal Formation.*) Lebach.
	——— macropterus. *Agass.* vol. ii. p. 31	(*Coal Formation.*) Lebach.
	Anenchelum dorsale	Engi, canton Glaris.
	——— Glarisianum	Engi, canton Glaris.
	——— heteropleurum	Engi, canton Glaris.
	——— isopleurum	Engi, canton Glaris.
	——— latum	Engi, canton Glaris.
	Aspidorhynchus acutirostris	(*Oolite.*) Solenhofen.
	——— mandibularis	(*Oolite.*) Solenhofen.
	Asteracanthus semiradiatus	(*Oolite.*) Stonesfield.
	——— ornatissimus	(*Kimmeridge Clay.*) Shotover Hill.
E	Atherina macrocephala	(*Tertiary Beds.*) Monte Bolca.
	Belonostomus leptosteus	(*Oolite.*) Stonesfield.
	Beryx ornatus *. *Geology of Sussex*	(*Chalk.*) Sussex.
c	Carangopsis dorsalis. *Agass.* vol. v. pl. 8	(*Tertiary Beds.*) Monte Bolca.
	Carcharias grossiserratus	(*Tertiary Beds.*) Malta.
	——— megalodon. *Agass.* vol. iii. pl. 29	(*Tertiary Beds.*) Malta.
	——— productus	(*Tertiary Beds.*) Malta.
c	——— macrodon	North America.
c	——— megalotis. *Agass.* vol. iii. pl. 28	North America.
	——— minor	North America.
c	——— polygyrus	North America.
c	——— subserratus	(*London Clay.*) Sheppey.
	Caturus† macrodus	(*Oolite.*) Solenhofen.
	——— microchirus	(*Oolite.*) Solenhofen.
	——— maximus	(*Oolite.*) Solenhofen.
c	——— pachyurus	(*Oolite.*) Solenhofen.
E	Chimæra‡ Egertonii	(*Kimmeridge Clay.*) Shotover Hill.
	Clupea Beurardi	(*Tertiary Beds.*) Mount Lebanon.
	——— brevis	Engi, canton Glaris.
	——— megaptera	Engi, canton Glaris.
	——— Scheuchzeri, a doubtful species.—*Agass.*	Engi, canton Glaris.

* *Zeus Lewesiensis*, Mantell.

† Originally named *Uræus* by Agassiz.

‡ Discovered and described by Dr. Buckland. [See our present volume, pp. 4, 5.—EDIT.]

	GENUS AND SPECIES.	STRATA AND LOCALITIES.	
	Clupea catopygoptera	(*Tertiary Beds.*)	Monte Bolca.
	——— tenuis	(*Tertiary Beds.*)	Sicily.
c	Cobitis cephalotes. *Agass.* vol. v. pl. 50	(*Tertiary Beds.*)	Œningen.
c	Cœlacanthus, *new species*	(*Magnes. Limest.*)	East Thickley.
	Cottus brevis	(*Tertiary Beds.*)	Œningen.
E	Ctenacanthus tenuistriatus	(*Mountain Limestone.*)	Bristol.
c	Cybium macropomum	(*London Clay.*)	Sheppey.
	Cyclurus crassus		Engi, canton Glaris.
	——— nemopterus		Engi, canton Glaris.
	Dapedius Colei. *Agass.* vol. 5. p. 195	(*Lias.*)	Lyme Regis.
	——— granulatus. *Agass.* vol. ii. p. 190	(*Lias.*)	Lyme Regis.
	——— politus *. *Geol. Trans.* 2nd series, vol. i. *Agass.* vol. ii. p. 185	(*Lias.*)	Lyme Regis.
	——— punctatus. *Agass.* vol. ii. p. 192	(*Lias.*)	Lyme Regis.
	Dercetis elongatus †. *Geol. of Sussex*	(*Chalk.*)	Sussex.
	Dipterus macrolepidotus ‡. Geol. Trans. 2nd series, vol. iii. *Agass.* vol. ii. p. 114	(*Schist.*)	Caithness.
	Ductor leptosomus. *Agass.* vol. v. pl. 12	(*Tertiary Beds.*)	Monte Bolca.
	Esox lepidotus	(*Tertiary Beds.*)	Œningen.
c	Eugnathus chirotes	(*Lias.*)	Lyme Regis.
	——— ornatus	(*Lias.*)	Lyme Regis.
E	Fistularia magnifica		Engi, canton Glaris.
	——— tenuirostris	(*Tertiary Beds.*)	Monte Bolca.
	Galeus aduncus. *Agass.* vol. iii. pl. 26	(*Molasse.*)	Switzerland.
	——— serratus	(*Molasse.*)	Switzerland.
c	——— pristodontus §. *Agass.* vol. iii. pl. 26. *Geol. of Sussex,* pl. 32	(*Chalk.*)	Maëstricht.
	——— *not yet named*	(*Chalk.*)	Sussex.
	Gasterocnemus rhombeus. *Agass.* vol. v. p. 20	(*Tertiary Beds.*)	Monte Bolca.
	Gyrolepis Albertii. *Agass.* vol. ii. p. 173	(*Muschel Kalk.*)	Bayreuth.
	——— tenuistriatus. *Agass.* vol. ii. p. 174	(*Muschel Kalk.*)	Bayreuth.
	Hemipristis serra. *Agass.* vol. iv. pl. 14	(*Molasse.*)	Switzerland.

* De la Beche. † *Muræna Lewesiensis,* Mantell.
‡ This includes the four species of Sedgwick and Murchison.
§ Also found in the chalk of Sussex.

	GENUS AND SPECIES.	STRATA AND LOCALITIES.
	Holocentrum pygæum. *Agass.* vol. iii. pl. 27	(*Tertiary Beds.*) Monte Bolca.
	Hybodus curtus*	(*Lias.*) Lyme Regis.
	——— incurvus	(*Lias.*) Lyme Regis.
C	——— grossispinus	(*Lias.*) Lyme Regis.
	——— ornatus	(*Lias.*) Lyme Regis.
	——— reticulatus	(*Lias.*) Lyme Regis.
	——— minor	(*Lias.*) Old Passage, Bristol.
C	——— marginalis	(*Lias.*) Keynsham.
	——— grossiconus	(*Iron Sand & Oolite.*) Tilgate and Stonesfield.
	——— polyprion	(*Oolite.*) Stonesfield.
	——— dorsalis	(*Oolite.*) Stonesfield.
E	——— acutus	(*Kimmeridge Clay.*) Shotover Hill.
C	——— plicatilis	(*Muschel Kalk.*)
C	——— *new species not yet named*	(*Oolite.*) Purbeck.
	Isurus macrurus	Engi, canton Glaris.
	Labrax schizurus	(*Tertiary Beds.*) Monte Bolca.
	Lamna acuminata †. *Geol. of Sussex*, pl. 32	(*Chalk.*) Sussex.
	——— Mantellii. *Geol. of Sussex*, pl. 32	(*Chalk.*) Sussex.
	——— appendiculata. *Geol. of Sussex*, pl. 32	(*Chalk.*) Sussex and Maëstricht.
	——— contortidens	(*Molasse.*) Switzerland.
	——— cuspidata	(*Molasse.*) Switzerland.
	——— denticulata	(*Molasse.*) Switzerland.
	——— hastalis ‡	(*Molasse.*) Switzerland.
	——— quadrans	(*Molasse.*) Switzerland.
	——— obliqua	(*London Clay.*) Sheppey.
	——— plicata	(*London Clay.*) Sheppey.
	——— trigonodus	(*Tertiary Beds.*) Malta.
	——— xiphodon	(*Tertiary Beds.*) Malta.
	——— *new species not yet named*	(*Molasse.*) Switzerland.
E	Lebias crassicaudus	(*Tertiary Beds.*) Sinigaglia.
	Leptacanthus semistriatus	(*Oolite.*) Stonesfield.
	Lichia prisca. *Agass.* vol. v. pl. 11, 11 a.	(*Tertiary Beds.*) Monte Bolca.
	Lepidotus fimbriatus	(*Lias.*) Lyme Regis.
	——— Fittonii §. *Agass.* vol. ii. pl. 30, c. b.	(*Tilgate Beds.*) Tilgate Forest.
	——— Mantellii §. *Agass.* vol. ii. pl. 30, c.	(*Tilgate Beds.*) Tilgate Forest.

* This Ichthyodorulite probably belongs to *H. reticulatus.*

† Also from Quedlinburg.

‡ Also from Malta.

§ *Lepisosteus Fittonii,* Mantell.

	GENUS AND SPECIES.	STRATA AND LOCALITIES.
E	Lepidotus maximus	(*Oolite.*) Stonesfield.
	——— *new species not yet named*	(*Oolite.*) Stonesfield.
	——— minor. *Agass.* vol. ii. pl. 34	(*Oolite.*) Portland.
	——— notopterus. *Agass.* vol. ii. pl. 34	(*Oolite.*) Solenhofen.
C	——— *new species not yet named*	(*Kimmeridge Clay.*) Boulogne.
	Leptolepis Bronnii	(*Lias.*) Lyme Regis.
	——— caudalis..........	(*Lias.*) Lyme Regis.
C	——— contractus.........	(*Oolite.*) Solenhofen.
	——— dubius	(*Oolite.*) Solenhofen.
	——— Knorrii	(*Oolite.*) Solenhofen.
	——— sprattiformis	(*Oolite.*) Solenhofen.
C	Leuciscus gracilis	(*Tertiary Beds.*) Wurtemberg.
	——— macrurus	(*Papier Kohl.*) Rhine.
	——— papyraceus. *Agass.* vol. v. pl. 56	(*Papier Kohl.*) Rhine.
	——— Œningensis. *Agass.* vol. v. pl. 57—58	(*Tertiary Beds.*) Œningen.
	Macropoma Mantellii *. *Geol. of Sussex*, p. 239	(*Chalk.*) Sussex.
	Mallotus villosus fossilis ?	Greenland†.
	Megalichthys Hibbertii. Trans. Royal Soc. Edin. v. 13	(*Coal Formation.*) North Stafford.
E	Megalops priscus	(*London Clay.*) Sheppey.
	Microdon hexagonus	(*Oolite.*) Solenhofen.
C	——— *new species not yet named*	(*Oolite.*) Stonesfield.
C	Mugil princeps	(*Tertiary Beds.*) Aix.
	Myliobates angustus..........	(*London Clay.*) Sheppey.
	——— subarcuatus.......	(*London Clay.*) Sheppey.
	——— Studeri	(*Molasse.*) Switzerland.
C	Myriacanthus paradoxus	(*Lias.*) Lyme Regis.
	Myripristis homopterygius	(*Tertiary Beds.*) Monte Bolca.
E	Notidanus‡ primigenius. *Agass.* vol. iii. pl. 27	(*Molasse.*) Switzerland.
	Odontaspis rhaphiodon§	(*Chalk.*) Maëstricht.
E	Ophiopsis dorsalis	(*Oolite.*) Purbeck.
C	Osmeroides Mantellii ‖. Geol. of Sussex, p. 235	(*Chalk.*) Sussex.
E	Osteolepis arenatus. *Agass.* vol. ii. p. 122	(*Coal Series.*) Gamrie.
	Palæoniscus comptus. *Agass.* vol. ii. p. 97	(*Magnesian Limestone.*) East Thickley.

* *Amia Lewesiensis*, Mantell.
† [See Lond. and Edinb. Phil. Mag., vol. v. p. 461.—EDIT.]
‡ This genus is also found in the chalk of Sussex.
§ Also from the chalk of Sussex.
‖ *Salmo Lewesiensis*, Mantell.

	GENUS AND SPECIES.	STRATA AND LOCALITIES.
	Palæoniscus elegans. *Agass.* vol. ii. p. 95	(*Magnesian Limestone.*) East Thickley.
C	——— longissimus. *Agass.* vol. ii. p. 100	(*Magnesian Limestone.*) East Thickley.
	——— Freieslebeni. *Agass.* vol. ii. p. 66	(*Kupfer Schiefer.*) Eisleben.
	——— magnus. *Agass.* vol. ii. p. 78	(*Kupfer Schiefer.*) Eisleben.
E	——— Egertonii	(*Coal Shale.*) North Stafford.
	——— Blainvillei. *Agass.* vol. ii. p. 48	Muse, near Autun.
	——— Duvernoy. *Agass.* vol. ii. p. 45	(*Coal Formation.*) Zweibrücken.
	——— macropomus. *Agass.* vol. ii. p. 81	(*Zechstein.*) Ilmenau.
C	Palæorhynchum Colei	Engi, canton Glaris.
	——— Egertonii	Engi, canton Glaris.
	——— Glarisianum	Engi, canton Glaris.
	——— latum	Engi, canton Glaris.
	——— medium	Engi, canton Glaris.
	——— microspondylum	Engi, canton Glaris.
	——— longirostris	Engi, canton Glaris.
	Palimphyes brevis. *Agass.* vol. v. pl. 20	Engi, canton Glaris.
	——— longus. *Agass.* vol. v. pl. 19	Engi, canton Glaris.
	Pholidophorus Bechei. *Geol. Trans.* 2nd series, vol. i.	(*Lias.*) Lyme Regis.
	——— latiusculus	(*Lias & Bit. Schist.*) Seefeld, Tyrol.
	——— limbatus	(*Lias.*) Lyme Regis.
	——— onychius	(*Lias.*) Lyme Regis.
	——— latimanus	(*Oolite.*) Solenhofen.
E	——— radiatopunctatus	(*Oolite.*) Solenhofen.
C	——— taxis	(*Oolite.*) Solenhofen.
	——— uræoides	(*Oolite.*) Solenhofen.
	Placodon gigas	(*Muschel Kalk.*) Bayreuth.
E	——— Münsteri	(*Muschel Kalk.*) Bayreuth.
C	Platysomus gibbosus. *Agass.* vol. ii. p. 164	(*Kupfer Schiefer.*) Mansfeld.
E	Pleiocnemus macrospondylus	Engi, canton Glaris.
	Pleuracanthus serratus	Engi, canton Glaris.
	Psammodus magnus	(*Oolite.*) Stonesfield.
	——— tenuis	(*Oolite.*) Stonesfield.
	——— reticulatus	(*Kimmeridge Clay.*) Shotover Hill.
	——— porosus	(*Mountain Limestone.*) Bristol.
E	Pterygocephalus paradoxus	(*Tertiary Beds.*) Monte Bolca.
C	Ptycholepis Bollensis	(*Lias.*) Lyme Regis.
	Ptychodus altior. *Geol. of Sussex,* pl. 32	(*Chalk.*) Sussex.

	GENUS AND SPECIES.	STRATA AND LOCALITIES.	
	Ptychodus decurrens. *Geol. of Sussex*, pl. 32	(*Chalk.*)	Sussex.
	—— latissimus. *Geol. of Sussex*, pl. 32	(*Chalk.*)	Sussex.
	—— mammillaris. *Geol. of Sussex*, pl. 32	(*Chalk.*)	Sussex.
	—— polygyrus. *Geol. of Sussex*, pl. 32	(*Chalk.*)	Sussex.
	Pycnodus Bucklandii	(*Oolite.*)	Stonesfield.
	—— parvus	(*Oolite.*)	Stonesfield.
c	—— *new species not yet named*	(*Oolite.*)	Stonesfield.
	—— microdon	(*Tilgate Beds.*)	Tilgate Forest.
	—— gigas		Jura.
c	Pygæus, *new species not yet named*	(*Tertiary Beds.*)	Monte Bolca.
	Semionotus rhombifer	(*Lias.*)	Lyme Regis.
	—— *new species not yet named*	(*Bit. Schist.*)	Seefeld, Tyrol.
	Smerdis micracanthus. *Agass.* vol. iv. p. 33	(*Tertiary Beds.*)	Monte Bolca.
	—— minutus	(*Tertiary Beds.*)	Aix.
	Sphærodus gigas	(*Oolite.*)	Stonesfield.
c	—— *new species not yet named*		Jura.
c	Sparnodus altivelis	(*Tertiary Beds.*)	Monte Bolca.
E	—— macrophthalmus	(*Tertiary Beds.*)	Monte Bolca.
E	—— micracanthus	(*Tertiary Beds.*)	Monte Bolca.
c	Sphyræna gracilis	(*Tertiary Beds.*)	Monte Bolca.
c	Spinacorhinus polyspondylus*	(*Lias.*)	Lyme Regis.
c	Tetragonolepis confluens. *Agass.* vol. ii. p. 199	(*Lias.*)	Lyme Regis.
	—— heteroderma	(*Lias.*)	Lyme Regis.
	—— Leachii	(*Lias.*)	Lyme Regis.
	—— pholidotus	(*Lias.*)	Lyme Regis.
c	—— pustulatus	(*Lias.*)	Lyme Regis.
	—— speciosus. *Agass.* vol. ii. p. 199	(*Lias.*)	Lyme Regis.
E	—— radiatus	(*Lias.*)	Lyme Regis.
	Tetrapterus priscus	(*London Clay.*)	Sheppey.
c	Thrissops salmoneus	(*Oolite.*)	Solenhofen.
	Tinca furcata. *Agass.* vol. v. pl. 52	(*Tertiary Beds.*)	Œningen.
E	Vomer longispinus. *Agass.* vol. v. p. 28	(*Tertiary Beds.*)	Monte Bolca.

Specimens undetermined.

New genus allied to Palimphyes.. Engi, canton Glaris.

* *Squaloraia dolichognathus*, Dr. Riley. [See Lond. and Edinb. Phil. Mag., vol. iii. p. 369.—EDIT.]

GENUS AND SPECIES.	STRATA AND LOCALITIES.
New genus allied to Scopelus	Engi, canton Glaris.
E Placoid fish not yet named....	(*Coal Shale.*) North Stafford.
E Placoid fish not yet named....	(*Coal Shale.*) North Stafford.
E Placoid fish not yet named....	(*Coal Shale.*) North Stafford.
E Placoid fish not yet named....	(*Coal Shale.*) North Stafford.
Scomberoid not yet named ...	(*London Clay.*) Sheppey.
c Ten species not yet named....	(*London Clay.*) Sheppey.
c New genus not yet named....	(*Tertiary Beds.*) Œningen.

LXIX. *On a new Method of reducing Lunar Observations for the Determination of the Longitude.* By CHARLES RUMKER, *Esq., F.R.A.S.**

To Lieut.-General Sir Thomas Macdougall Brisbane, K.C.B., Pres. R.S.E., F.R.S.L., F. Ast. S., &c.

SIR,

THE lively interest which you took in the determination of the longitude by lunars, and the success which attended your observations, animate me to submit the following method, admitting of greater accuracy when either altitude is low, to your approval and patronage. Allow me to remind you here, that already when I had the honour of accompanying you on your passage to Australia, I had occasion to allude to the necessity of a more strict method, suitable to the circumstances. This subject has since been treated by a celebrated astronomer. His method appears to me, however, not likely to be generally adopted by seamen, and requires, moreover, a particular ephemeris, which becomes useless on ordinary occasions, when the usual methods answer every purpose.

I hope, therefore, that the following method, which requires no other tables than the Nautical Almanac, and may, according to circumstances, be computed with more or less precision, will meet your approbation.

The object of the present lines is to correct the error committed in the usual methods of clearing the apparent distance between sun and moon by taking out the refraction for the central altitude of both bodies, whereas it is the refraction for those points of their limbs, the distance of which is actually measured, that should be used in the calculation.

As introduction, it may not be useless to remark, that the usual methods of clearing the distance, supposing the altitudes known from observation, can be classed into direct and approximative ones. For thus, as all the former methods are derived from the equation existing between the sides of

* Communicated by Sir Thomas M. Brisbane, K.C.B., &c. A portion of this paper was inserted in our Number for October last: this having required some corrections, we now give the Communication entire.

the two triangles formed by the apparent zenith distances and distance, and the true zenith distances and distance, and only differ by slight variations in the manner of finding thence the true distance, or side of the latter triangle opposite to the angle at the zenith common to both triangles, the latter methods unite all in computing in the apparent triangle the angle at the sun and moon, and the sides adjacent thereto in two right-angled triangles having for hypothenuses the corrections of the sun's and moon's altitudes. These sides are the corresponding corrections of the distance.

Let D designate the apparent distance of the centres, S and M the above angles at the sun and moon, $\varrho - \pi$ the difference between parallax and refraction or correction of altitude, then is the true distance of centres $= D + \text{cosine } S . (\varrho - \pi) + \text{cosine } M (\varrho' - \pi') + \alpha (\varrho - \pi')^2 + \beta (\varrho' - \pi')^2 + +$.

The moon's parallax being greater than her refraction, the second correction becomes negative; and when one of the angles is obtuse, its cosine takes the opposite sign. The fundamental formula of all approximative methods is,

$$\text{true distance} = D + \left(\frac{\sin h - \sin H \cos D}{\cos H \sin D}\right)(\varrho - \pi)$$

$$+ \left(\frac{\sin H - \sin h \cos D}{\cos h \sin D}\right)(\varrho' - \pi') + + +, \ldots$$

where ϱ and π refer to that altitude h or H which stands first and by itself in the parenthesis. Lyons obtains by executing the division,

$$D + (\sin h \sec H \operatorname{cosec} D - \tan H \cot D)(\varrho - \pi)$$
$$+ (\sin H \sec h \operatorname{cosec} D - \tan h \cot D)(\varrho - \pi) + +.$$

If we make $\left(\frac{\sin h - \sin H \cos D}{\cos H \sin D}\right)(\varrho - \pi)$

$$= \left(1 - 1 + \frac{\sin h - \sin H \cos D}{\cos H \sin D}\right)(\varrho - \pi)$$

$$= \left(1 - \frac{2 \cos \frac{1}{2}(D + H + h) \sin \frac{1}{2}(D + H - h)}{\cos H . \sin D}\right)(\varrho - \pi),$$

we obtain the one part of Mendoza Rios' approximative formula to which the other is analogous; and by separating in Lyons's method the moon's parallax from the refraction, thence results the correction of the distance for the moon's parallax

only $= \text{hor. par.} \times \left[\frac{\sin ☾ \text{ alt.}}{\tan D} - \frac{\sin ☉ \text{ alt.}}{\sin D}\right]$,

which is identical in Elford's, Thomson's, and Lynn's tables, is found graphically by Kelly, and by Norie in his linear tables, and which Thomson finds with his lunar scale; and it is only in the manner of allowing for the refraction and the sun's parallax that the above authors differ; all of whom, however, with the exception of Lynn, have erred in using in the calculation the apparent altitude, whereas the apparent altitude corrected for refraction should have been used. On the same grounds the usual tables exhibiting the moon's correction by inspection, including parallax and refraction, are erroneously computed.

By approximative methods are not to be understood methods that admit of less accuracy, but such as approach the truth by a series of which the last terms vanish, or, which answers the same purpose, by a gradual substitution in the calculation of terms found by a former approximation. The approximative methods have, particularly to seamen, that advantage above the direct ones, that the trigonometrical calculation need only be executed to the nearest minute; and as the correction of the distance never can exceed that of the altitude, and as its sign, as well as amount, can nearly be estimated from the position which the observed bodies occupy, essential errors may easily be avoided; moreover, they offer an easy means of reducing the refraction to the points of the limbs brought into contact when observing their distance, as shall be shown in what follows: To deduce from the observed altitude of the upper or lower limb the apparent altitude of the centre, the horizontal semidiameter must be diminished for refraction by a correction to be taken out from the table at the end of this paper: in order to find the vertical diameter, the moon's semidiameter requires moreover the usual augmentation on account of parallax. From another table, too extensive for our limits here, the reduction from the apparent to the true zenith must be taken, which may also be computed after the following formula:

$$\frac{\sin\varphi \sin h - \sin\delta}{\cos h} \times \frac{2\sin\phi}{\frac{a^2+b^2}{a^2-b^2}} + \cos 2\varphi = \text{tang. reduction},$$

neglecting higher powers of a^2-b^2. This reduction assuming the ratio of axes $\frac{302\cdot8}{303\cdot8}$ is very nearly $= 1360''. \frac{\sin\varphi}{\cos h}$ $(\sin\varphi \sin h - \sin\delta)$, where h denotes the true altitude, φ the latitude, δ the declination, attention being had to its sign. Tables for the correction of the horizontal parallax on account of the spheroidical figure of the earth are contained in several nautical works, or supplied by a simple calculation. Let us designate by,

D, the apparent distance of centres;

d, the distance of the points of contact or observed distance of limbs;

H, the greater, h the less, apparent altitude of centres;
H′, the greater, h' the less, apparent altitude of the points of contact;
r, the semidiameter parallel to the horizon (augmented if the moon's) of the upper body;
$\varrho-\pi$, the correction of the upper altitude;
$\varrho'-\pi'$, the correction of the lower altitude;
θ, the true distance of the limbs, to which the equatorial horizontal semidiameters must be added to obtain the true distance of the centres.

Let A be the distance of the middle of the apparent distance of the centres, and A′ the distance of the middle of the apparent distance of the limbs from the highest point in the apparent distance or its prolongation, that is, from that point in a great circle drawn through sun and moon where the effect of parallax and refraction is 0,—then is

$$\tan A = \frac{\tan \frac{1}{2} (H-h)}{\tan \frac{1}{2} (H+h) \tan \frac{1}{2} D} \text{ and } A' = \tfrac{1}{2} d + r + A - \tfrac{1}{2} D$$

whence we obtain the apparent altitudes of the points of contact,

$$\sin H' = \frac{\cos (\frac{1}{2} d - A') \sin H}{\cos (\frac{1}{2} D - A)} \text{ and } \sin h' = \frac{\cos (\frac{1}{2} d + A') \cos h}{\cos (\frac{1}{2} D + A)}$$

which are also found by $H'-H = r \tan H \tan (\frac{1}{2} D - A)$ and $h'-h = r' \tan h \tan (\frac{1}{2} D + A)$ and adding $H'-H$ and $h'-h$ to the apparent altitudes of the centres above the sensible horizon and subtracting from the sum the change of refraction from H′ to H and h' to h. But if $A > \frac{1}{2} D$, $H'-H$ is to be subtracted from the greater altitude, and the change of declination to be added thereto; the same method is to be followed when the distance of the moon's remote limb from a star is observed. It being, however, not so much an error in the altitude as an error in the refraction that materially affects the calculation, and this refraction not being sensibly altered by a few seconds of difference of altitude, the change of refraction may safely be neglected. For the apparent altitudes of the points of contact of the sun and moon above the horizon, compute strictly the refractions ϱ and ϱ' with regard to barometer and thermometer, and add the sum of these refractions to the observed distance of the limbs. From the same apparent altitudes of the points of contact above the sensible horizon, find, by applying thereto the above-stated reduction, the altitudes H′ and h' with respect to the true zenith, and deduct from each the corresponding refraction found before, and compute for the rest the parallaxes in altitude π and π', and subtract their sum $\pi+\pi'$ from the observed distance augmented by the refractions, and call the remainder $d+\varrho+\varrho'-\pi-\pi' = \delta$.

Find also for each altitude the corrections $\varrho-\pi$ and $\varrho'-\pi'$; then is

$$\theta=\delta-\frac{\cos(H'+\frac{1}{2}d-A').(\varrho-\pi)}{\cos H'\cos(\frac{1}{2}d-A')}-\frac{\cos(h'+\frac{1}{2}d+A').(\varrho'-\pi')}{\cos h'\cos(\frac{1}{2}d+A\ )}$$

$$+\frac{\cos(H'+\frac{1}{2}d-A')\cos[H'-(\frac{1}{2}d-A')](\varrho-\pi)^2.\sin 1''}{2\tan d\cos^2 H'\cos^2(\frac{1}{2}d-A')}$$

$$+\frac{\cos(h'+\frac{1}{2}d+A)\cos[h'-(\frac{1}{2}d+A')](\varrho'-\pi')^2.\sin 1''}{2\tan d\cos^2 h'\cos^2(\frac{1}{2}d-A')}--++$$

If we call the first correction α, the second β, then is:

$$\theta=\delta-\alpha-\beta+[(\rho-\pi-\tfrac{1}{2}\alpha)\alpha+(\rho'-\pi'-\tfrac{1}{2}\beta)\beta]\frac{\sin 1''}{\tan(\delta-\alpha-\beta)}$$

to which both horizontal semidiameters are to be added to find the true distance of the centres. The square of the sun's correction can always be neglected, and that even of the moon's correction disappears when d is near 90°, or when the moon's correction is small. All four corrections disappear when the distance is the supplement of the sum of the altitudes; but when the distance is equal to the difference of the altitudes, all other corrections vanish except the first one, which becomes $= 2(\rho-\pi)$.

The first two corrections only, however, need be computed, as the others can be taken as a small third correction from a table contained in most nautical works, *with its sign*, which becomes negative when $\delta>90$; so that this method has the advantage that no difference of cases need be attended to, as in that of Witchell's, whose example I have followed in the computation of it. For if we call, of the two first corrections, the one proceeding from the sun S and that from the moon M, then is,

$$\theta=\delta-S+M\pm\text{ third correction,}$$

which *latter one*, in the absence of the tables, may be found by the formula (☾'s corr. $-\frac{1}{2}$ M) . M cot θ sin 1″.

To the semidiameters, which are to be added to the observed distance to obtain the apparent distance of the centres, a correction should be applied on account of inclination to the horizon, for which Mendoza Rios has given a table. This inclination to the horizon is found by sin. inclination = sin H sec ($\frac{1}{2}$ D — A). In the subsequent example this inclination is = 90°; so that the vertical semidiameters have been used. As however the distance of the centres enters only into the approximative calculation, the correction for inclination may be omitted in this method.

To illustrate this method we shall choose the example given by Professor Schumacher in his Ephemeris for 1835, which from 21 feet elevation above the level of the sea would become the following:

1st Example. June 18, 1835, in 23° 57′ N. latitude, and 172° 22′ E. longitude, the following observations were made. Therm. 90 Fahr.; barom. 28·6 Engl.

⊙ Lower Limb.	83 55 35
Dip	− 4 21
	83 51 14
⊙'s semid.	+ 15 45
App. alt.	84 6 59
Red. to true zen.	+ 35
H =	84 7 34

☾ Upper Limb.		5 15 50
....		− 4 21
		5 11 29
☾'s hor. semid.	15 4	
Refraction	−20	−14 45 vert. semid.
Parallax	+ 1	
Ap. alt. above hor.		4 56 44
Reduct. to true zen.		+1 23
h =		4 58 7

Observed Distance of Limbs.

d =	90 17 16	$\frac{1}{2} d$ =	45 8 38
semid. ⊙ = r	15 45	r =	15 45
semid. ☾ = r'	14 45	A =	39 37 57
	90 47 46 = D	$\frac{1}{2}$ D =	45 23 53
		A′ =	39 38 27

Calculation.

H	84 7 34				
h	4 58 7				
H+h	89 5 41	$\frac{1}{2}$(H+h)	44 32 50	cot	0·006864
H−h	79 9 27	$\frac{1}{2}$(H−h)	39 34 43	tan	9·917318
		$\frac{1}{2}$ D	45 23 53	cot	9·993966
		A	39 37 57	tan	9·918148

$\frac{1}{2}$ D−A	5 45 56	tan	9·0042	$\frac{1}{2}$D+A	85 7 50	tan	1·0607
H	84 7 34	tan	0·9877	h	4 58 7	tan	8·9392
r	15 45	log	2·9754	r'	15 5	log	2·9567
H′−H	15 27	log	2·9673	h'−h	15 5	log	2·9567
App. alt.	84 6 59			App. alt.	4 56 44		
	84 22 26	refr. ρ = 5″			5 11 49		
Reduc. zen.	35	Change refr. for h'−h =			20		
H′	84 23 1				5 11 29	refr. R = 8′ 19″	
Refract.	5			Red. zen.	1 23		
	84 22 56	par. π = 1			5 12 52		
				Ref.	8 19		
					5 4 33	par. P =	55 1
						P−R =	+46 42

π−ρ =	−4
P−R =	+ 46 42
P+π−ρ−R =	46 38
d =	90 17 16
δ =	89 30 38

$\frac{1}{2}d$	45 8 38		$\frac{1}{2}d$	45 8 38	
A′	39 38 27		A′	39 38 27	
$\frac{1}{2}d-$A′ =	5 30 11	sec 0·00201	$\frac{1}{2}d+$A′	84 47 5	sec 1·04144
H′	84 23 1	sec 1·00936	h'	5 12 52	sec 0·00180
H′$+\frac{1}{2}d-$A′	89 53 12	cos 7·29623	$h'+\frac{1}{2}d+$A′	89 59 57	cos 5·16270
$\varrho-\pi=$	4″	log 0·60206	R′−P′	−46′ 42″	log 3·44747
S =	− 0″·08	log 8·90966	M =	+ 0″·45	log 9·65341
M =	+ 0 ·45				
δ =	89 30 38 ·00				
θ =	89 30 38 ·37				
r =	15 45				
r' =	15 4				

		Diff.	
True cent. dist.	90 1 27·4 90 2 52·0	0° 1′ 24″·6	pr. log. 2·1065
From Nautical Alm. prop. log. for midnigh			0·3250
Mean time at Greenwich	12 2 58·5		pr. log 1·7815
Mean time on board	23 34 52·0		
Longitude	11 31 53·5 East.		

2nd Example. October 16, 1835, in 53° 33′ N. latitude, and 9° 58′ E. longitude, at 10 A.M. mean time, the following altitudes of the sun's and moon's lower limb and distance of their limbs were observed. Elevation of the eye 20 feet. Barom. 30·2; therm. 40.

	☉		☽
	23 41 31		45 30 0
Dip	− 4 15	dip	− 4 15
	23 47 16		45 25 45
☉ semid. 16 4·6 Dim. for refr. 2·0	+ 16 2·6	☾'s hor. sem. 15 18·7 aug. for par. 10·5 dim. for ref. 0·4	15 28·8
	23 53 18·6		45 41 13·8
Reduction	+ 9 31·0	Reduction	+ 4 53
h =	24 2 49·6	H	45 46 6·8

Distance.

d =	72 41 13	$\frac{1}{2}d$ =	36 20 36·5
	15 29·2	r =	15 29·2
	16 4·6	A =	20 18 38·0
D =	73 12 46·8		56 54 44·0
		$\frac{1}{2}$D	36 36 23·0
		A′ =	20 18 21·0

$H = 45$ 46 7				
$h = 24$ 2 50				
$H+h = 69$ 38 57	$\frac{1}{2}(H+h)$	34 54 28	cot 0·1562619	
$H-h = 21$ 43 16	$\frac{1}{2}(H-h)$	10 51 38	tan 9·2829749	
	$\frac{1}{2}$ D	36 36 23	cot 0·1291056	
	A	20 18 38	tan 9·5683424	

$(\frac{1}{2}D-A)$	16 17 49	tan 9·46589		$(\frac{1}{2}D+A)$	56 55 1	tan 0·18611
H	45 46 7	tan 0·01165		h	24 2 50	tan 9·64954
r'	15 29	log 2·96801		r	16 5	log 2·98453
$H'-H$	4′ 39″	log 2·44556		$h'-h$	11′ 1″	log 2·82018
	45 41 14				23 53 19	
	45 45 53	refr. 0′ 58″·1			24 4 20	ref. cor. 2′ 13″·2
Reduction	+ 4 53				+ 9 31	
$H' =$	45 50 46			h'	24 13 51	parallax 7·7
Refraction	58				$\rho'-\pi''$	2 5·5
	45 49 48	parall. 39′ 4″·1				

$$\rho-\pi = -\ 38'\ 6''{\cdot}0$$
$$\rho'-\pi' = +\ 2\ \ 5{\cdot}5$$
$$\rho-\pi+\rho'-\pi' = -\ 36\ \ 0{\cdot}5$$
$$d = 72\ 41\ 13{\cdot}0$$
$$\delta = 72\ \ 5\ 12{\cdot}5$$

$\frac{1}{2}d =$	36 20 36			$\frac{1}{2}d$	36° 20′ 36″	
$A' =$	20 18 21			A'	20 18 21	
$(\frac{1}{2}d-A')$	16 2 15	sec 0·01724		$\frac{1}{2}d+A'$	56 38 57	sec 0·25982
$H' =$	45 49 48	sec 0·15690		h'	24 13 51	sec 0·04005
$H'+\frac{1}{2}d-A' =$	61 52 3	cos 9·67349		$h'+\frac{1}{2}d+A' =$	80 52 48	cos 9·20064
$\rho-\pi =$	38 6	log 3·35908		$\rho'-\pi'$	2′ 5″·5	log 2·09864
$M = +$	26 49·5	log 3·20671		$S =$	39″·68	log 1·59855

$S = -$	39·7
$\delta =$	72 5 12·5
	72 31 22·3
3rd corr.	+ 3·6
$\theta =$	72 31 25·9
$r =$	15 18·7
$r' =$	16 4·6

		Diff.	
True dist. of centres	73 2 49·2		
	73 12 39·0	0° 9′ 49″·8 …	pr. log. 1·2627
	From Nautical Almanac		pr. log. 0·3111
Mean time at Greenwich	21 20 7 ……		pr. log. 0·9516
Mean time at ship ……	22 0 0		
Longitude in time ……	0 39 53 East.		

The third correction is to rectify the error committed in assuming one side of a spherical triangle equal to the adjacent segment of its base cut off by a perpendicular from the vertex of the opposite angle, which error will diminish with the angle contained between the above side and the base.

Suppose a and c to be two sides of a triangle, and b and d their adjacent segments, and $\mu = a - b$, then is $\tan \frac{1}{2}\mu$ $= \frac{\tan\frac{1}{2}(c-d)\,.\,\tan\frac{1}{2}(c+d)}{\tan(b+\frac{1}{2}\mu)}$, whence μ may be found by approximations, supposing it in a first one $= 0$. But $\tan\frac{1}{2}(c-d)\,.\,\tan\frac{1}{2}(c+d) = \tan^2\frac{1}{2}\lambda$, if we denote by λ the perpendicular. And $\text{tang}\,\frac{1}{2}\mu = \frac{\text{tang}^2\,\frac{1}{2}\lambda}{\text{tang}\,(b+\frac{1}{2}\mu)}$.

Whence we find the following general expression for $\frac{1}{2}\mu$, which may be reduced accordingly as circumstances allow:

Suppose,

$$= \cfrac{\cfrac{\text{tang}\,\frac{1}{2}\lambda}{1+\text{tang}^2\,\frac{1}{2}\lambda} - \left[\cfrac{\tan^2\frac{1}{2}\lambda}{\tan b+\cfrac{\tan^2\frac{1}{2}\lambda}{\tan b+\cfrac{\tan^2\frac{1}{2}\lambda}{\tan b+}}}\right]^2 \tan b + \left(\cfrac{\tan^2\frac{1}{2}\lambda}{\tan b+\cfrac{\tan^2\frac{1}{2}\lambda}{\tan b+\cfrac{\tan^2\frac{1}{2}}{\tan b+}}}\right)}{1+\tan b\left(\tan b+\cfrac{\tan^2\frac{1}{2}\lambda}{\tan b+\cfrac{\tan^2\frac{1}{2}\lambda}{\tan b+\cfrac{\tan^2\frac{1}{2}\lambda}{\tan b+\dots}}}\right) - \tan b\left[\cfrac{\tan^2\frac{1}{2}\lambda}{\tan b+\cfrac{\tan^2\frac{1}{2}\lambda}{\tan b+\cfrac{\tan^2\frac{1}{2}\lambda}{\tan\dots}}}\right]^2 + - \text{\&c.}}$$

then is: $\text{tang}\,b\;\text{tang}\,\frac{1}{2}\mu = N - N^2 + N^3 - N^4 + N^5 - + - \dots$ μ becomes, therefore, negative when $b > 90$, and is $= 0$ when $b = 90°$.

Walbeck, who proposed computing for the time of observation, reduced for the estimated longitude from the first meridian, the apparent distance as seen from the place of observation; and to derive, by a comparison of this computed distance with the observed one by means of the moon's apparent horary motion, the error of the estimated longitude, has remarked already the necessity of computing the refraction for the points of contact when the altitudes are low, in the note to page 15 of his *Dissertatio de Modo reducendi Distantias*, Abo 1817: "Si rigorose calculaveris, refractio non pro Centro lunæ sed puncto limbi quo distantia capitur sumenda est. Inutile vero est, calculum talibus minutiis molestum reddere, quæ præterea, nisi sit luna vel sol horizonti proximus nullius sunt momenti. Ex hac etiam caussa minimæ altitudines evitari debent." But low altitudes are better than none, and cannot always be avoided.

Walbeck found from latitude, declination and horary angle, the altitudes, parallactic angle and the corrections of the altitudes, and thence the apparent declinations and right ascensions of both bodies, and computed thence, with true difference of right ascensions and true declinations, the true distance, and with the apparent difference of right ascensions and apparent declinations, the apparent distance, and the differences $\Delta' - \Delta$ of these apparent and true distances for three successive equally distant periods t_1, t_2, t_3, and denoted by ω_1, ω_2, ω_3, the remainders left by a subtraction of these differences $\Delta' - \Delta$ from one another.

Suppose now $\omega_1 = A + Bt_1 + Ct_1^2$, $\omega_2 = A + Bt_2 + Ct_2^2$, and $\omega_3 = A + Bt_3 + Ct_3^2$, then is,

$$C = \frac{\omega_1(t_3 - t_2) + \omega_2(t_1 - t_3) + \omega_3(t_2 - t_1)}{(t_2 - t_1)(t_3 - t_1)(t_3 - t_2)}$$

$$B = \frac{\omega_2 - \omega_1}{t_2 - t_1} - C(t_2 + t_1) \text{ and } A = \omega_1 - t_1(B + t_1 C),$$

whence any other $\omega = A + Bt + Ct^2$ for any given t may be found, provided A is assumed proportional to the given t; Then $\omega \pm$ sum of apparent semidiameters applied to the distance taken from the Naut. Almanac for the time reduced for longitude to the first meridian, gives the apparent distance of limbs, which by a comparison with the observed distance will show the error of the assumed longitude. This rather laborious proceeding may be simplified by taking also from the Nautical Almanac for the same reduced time, together with the other elements, the true distance of the centres, and finding by $\frac{\text{differ. Æ cosin declin.}}{\text{sine distance}}$, the sines of the angles of position for the sun, as well as for the moon; then a subtraction of the parallactic angles from the angles of position will give the above-mentioned angles S and M, whence will be found, $\Delta' - \Delta = (\pi' - \varrho') \cos M - (\varrho - \pi) \cos S +$ third correction, where S and M are considered acute; any doubts whether the angles of position are obtuse or acute, are easily decided, and the reduction of the refraction to the points of contact is accomplished in the same manner as before. This method offers advantages when by a series of observations the longitude of a place on shore is to be determined. At sea it would be unwise to neglect the opportunity of observing the altitudes above the visible horizon, considering that the latitude entering into the calculation of the altitudes rests upon no firmer base than the contemporaneously observed altitudes, and requires moreover a very unsafe reduction by dead-reckoning to the time and place of lunar observation, not to mention that the error of the estimated longitude affects the elements entering into the computed altitudes taken from the Nautical Ephemeris, of which the observed altitudes are independent. Immediately before or after new moons the faint image of the moon when she is high may be difficult to bring down to the horizon, and occupy such a position in respect of it, that neither upper nor lower limb can be correctly observed. The computed altitude of a fixed star is also more to be depended on than its observed one; in these cases it is better to compute, particularly the high altitudes, with the reduced geocentric latitude, as the refraction corresponds thereto, which, by rights, ought to have been taken from the tables for the angle of the ray of light with the normal or with its complement; the altitude above the sensible horizon then requires only a trifling correction.

When the lower object is a star, and the moon's altitude is not too small, the usual methods are sufficiently correct, provided allowance is made for barometer and thermometer.

Here follows a specimen of the table of the contraction of the vertical diameter on account of the refraction, which is calculated for the mean diameter of the sun, and for mean refraction.

Altitude.	Correction for		Altitude.	Correction for		Altitude.	Correction for	
	Lower Limb.	Upper Limb.		Lower Limb.	Upper Limb.		Lower Limb.	Upper Limb.
10° 0'	−8"	−8"	7°20'	−14"	−15"	4°50"	−26"	−28"
9 50	9	9	7 10	14	16	4 45	26	29
9 40	9	9	7 0	15	16	4 40	27	29
9 30	9	9	6 50	16	17	4 35	27	30
9 20	9	10	6 40	16	18	4 30	28	31
9 10	10	10	6 30	17	18	4 25	28	32
9 0	10	10	6 20	18	19	4 20	29	33
8 50	10	11	6 10	18	20	4 15	30	34
8 40	11	11	6 0	19	21	4 10	31	35
8 30	11	11	5 50	20	22	4 5	32	36
8 20	11	12	5 40	21	23	4 0	33	37
8 10	11	12	5 30	22	24	3 55	34	38
8 0	12	13	5 20	23	25	3 50	36	40
7 50	12	13	5 10	24	26	3 45	38	42
7 40	13	14	5 0	24	27	3 40	40	44
7 30	13	14	4 55	25	27	3 35	42	45

LXX. *Observations on the Lines of the Solar Spectrum, and on those produced by the Earth's Atmosphere, and by the Action of Nitrous Acid Gas.* *By* Sir David Brewster, *K.H., V.P.R.S.Ed.**

IN a paper on the Monochromatic lamp, &c., read before the Royal Society of Edinburgh on the 15th April 1822, and published in their Transactions, I recorded some of my earliest experiments on the action of coloured media on the solar spectrum. These experiments were continued at irregular intervals, with the view of obtaining distinguishing characters of coloured media, of investigating the cause of the colours of natural bodies, and of examining more correctly the phænomena of the overlapping colours of equal refrangibility, which I had announced in the paper already referred to. The results to which I was conducted on the two last of these subjects have been already published, in two papers, one on the analysis of solar light, and the other on the colours of natural bodies.

The first and the principal object of my inquiries, namely, the discovery of a general principle of chemical analysis, in which simple and compound bodies might be characterized by their action on definite parts of the spectrum, still remained to be pursued. The coloured juices of plants—artificial salts and their solutions, and various glasses and minerals—had afforded me many beautiful examples of this species of action; and after determining the locality of these actions in reference to Fraunhofer's principal lines, and their intensity, as depending on the thickness of the absorbing medium and the brightness of the spectrum, I was able to distinguish all such compounds, by merely looking through them at a well-formed spectrum. Even in those cases where the eye could recognise no difference between the colours of two substances that exercised different specific actions upon light, their discrimination was instantly effected by viewing them through a standard coloured medium.

As some of these bodies attacked the spectrum at *two*, *three*, *four*, and even *five* or more points at once, it became probable that the number and intensity of such actions depended on the number and nature of the elements which entered into the composition of the body, or, what is nearly the same thing, that it was the sum of all the separate actions of such elements; and hence the next step in the inquiry was, to determine the action of elementary bodies on the solar spectrum. This inquiry was not limited to coloured bodies,

* From the Transactions of the Royal Society of Edinburgh, corrected.

for it is quite possible that a body may transmit light perfectly white, and yet exercise a definite action in absorbing various parts of the spectrum. The only physical condition which is necessary in this case is, that the sum of all the rays thus absorbed, shall constitute white light.

The first substances which I examined were *sulphur* and *iodine* vapour. The *sulphur* attacked the *violet* end of the spectrum with great force, and, when combined with arsenic, in the form of native orpiment, its absorptive power for the same colours was greatly increased. Even with the thinnest film that I could detach, and not exceeding the two-hundredth part of an inch, the spectrum was, as it were, cut sharply in two near the boundary of the *green* and *indigo* spaces, and this body possessed the very uncommon property of having nearly the same colour at small as at great thicknesses. By increasing the thickness, the absorption advances almost imperceptibly from the remaining blue border, and if the transparency continued, the transmitted light would certainly become *red* at great thicknesses,—a property which may be communicated transiently to the thinnest plates, merely by an increase of temperature.

The iodine vapour acted powerfully upon the middle of the spectrum, and, by an increase of thickness, gradually extended its absorption towards both extremities; but more rapidly towards the violet one, so as to show that the final colour must be a homogeneous red*.

In so far as these two experiments went, they were highly favourable to the speculation which had at first presented itself to me. My attention was now directed to the action of gaseous bodies, and the first trial which I made was with *nitrous acid gas*†. The result of this experiment completely destroyed the hypothesis which had appeared so plausible, and presented me with a phænomenon so extraordinary in its aspect,—bearing so strongly on the rival theories of light,—extending so widely the resources of the practical optician, and lying so close to the root of atomical science, that I am persuaded it will open up a field of research, which will exhaust the labours of philosophers for centuries to come.

The spectrum of Newton, and of all the philosophers of the 18th century, was a parallelogram of light, with circular ends, in which the *seven* colours gradually shaded into each other without any interruption. The illumination was a maximum in the yellow rays, and the light decayed by insensible

* [See Lond. and Edinb. Phil. Mag., vol. ii. p. 362.]
† [See Lond. and Edinb. Phil. Mag., vol. ii. p. 381.]

degrees towards the red and the violet extremities. In the year 1808, Dr. Wollaston conceived the happy idea of examining a beam of light, that passed through an aperture only the twentieth of an inch wide, and he was surprised to see it crossed by seven dark lines, perpendicular to its length.

About ten or twelve years afterwards, the celebrated optician Joseph Fraunhofer, without knowing what had been done by Dr. Wollaston, observed the spectrum formed by the sun's light transmitted through small apertures; and by applying a telescope behind the prism, he discovered about 600 parallel dark lines traversing the spectrum. As no such lines appeared in the spectra of white flames, Fraunhofer considered them as having their origin in the nature of the light of the sun. The strongest of these lines were seen in the spectra of the Moon, Mars, and Venus, and, by means of very fine instruments, he was able to detect one or two of them with other new lines in the spectra of Sirius and Castor.

Such was the state of the subject, when I made the experiment already referred to on nitrous acid gas. Upon examining with a fine prism of rock-salt, with the largest possible refracting angle, (nearly 78°,) the light of a lamp transmitted through a small thickness of the gas, whose colour was a very pale straw yellow, I was surprised to observe the spectrum crossed with hundreds of lines or bands, far more distinct than those of the solar spectrum. The lines were sharpest and darkest in the violet and blue spaces, fainter in the green, and extremely faint in the yellow and red spaces. Upon increasing, however, the thickness of the gas, the lines grew more and more distinct in the yellow and red spaces, and became broader in the blue and violet, a general absorption advancing from the violet extremity, while a specific absorption was advancing on each side of the fixed lines in the spectrum. It was not easy to obtain a sufficient thickness of gas to develop the lines at the red extremity, but I found that heat produced the same absorptive power as increase of thickness, and, by bringing a tube containing a thickness of half an inch of gas to a high temperature, I was able to render every line and band in the red rays distinctly visible.

The power of heat alone to render a gas, which is almost colourless, as red as blood without decomposing it, is in itself a most singular result; and my surprise was greatly increased when I afterwards succeeded in *rendering the same pale nitrous acid gas so absolutely black by heat, that not a ray of the brightest summer's sun was capable of penetrating it.* In making this experiment, the tubes frequently exploded, but, by using a mask of mica, and thick gloves, and placing the tubes in cylinders of tinned iron with narrow slits to admit the light, there is little danger of any serious accident.

When the gas is in the liquid state, it produces none of the fixed lines which I have described, and exercises no other action upon the spectrum than any ordinary fluid of the same orange colour.

In examining the structure of the solar spectrum, Fraunhofer seems to have put forth all his strength in determining the position of the principal lines, A, B, C, D, E, *b*, F, G and H *, which he had selected as equidistant as possible, for the purpose of measuring their angular distances in different media, and thus obtaining the most accurate data for the construction of the achromatic telescope. These measures he has given with the greatest exactness for various kinds of crown and flint glass and for a few fluids, and he has thus put it into the power of the practical optician to construct achromatic object-glasses, with a degree of certainty and perfection hitherto unknown.

This method, however, notwithstanding its high value, is not easily applicable in practice, and from the nice observations which it involves, we have reason to believe that it has not been used by any other artist than Fraunhofer himself. The difficulty of procuring out of the mass of glass to be employed, prisms sufficiently pure to show such narrow lines as E, or the two which constitute D†,—of obtaining the sun when his light is wanted, and of observing and measuring the distances of the fixed lines in a spectrum constantly in motion, are insurmountable obstacles to the general adoption of so refined a method of measuring dispersive powers.

From all these difficulties, the discovery of lines in the nitrous acid gas spectrum completely relieves us. As the lines whose distances are required, may be made as broad and black as we please, prisms of ordinary purity are sufficient to exhibit them in perfect distinctness. The artificial light of a lamp can be commanded at any hour, and as its rays are absolutely fixed, the least experienced observer can have no difficulty in measuring the distances of the fixed lines, and thus obtaining, with extreme accuracy, all the data for the construction of achromatic instruments.

But it is not merely to this practical purpose that the gaseous lines are singularly applicable. Among the various solids and fluids in nature, there are very few sufficiently pure and transparent, to enable us to see through them the lines of

* Six of these, viz. B, D, *b*, F, G, and H, were discovered by Dr. Wollaston.

† These lines are also the most important, as the most luminous part of the spectrum lies between them.

the solar spectrum, so as to enable us to measure their refractive and dispersive powers with minute accuracy, whereas the gaseous lines can be rendered visible, however imperfectly the spectrum may be formed. In determining the various elements of double refraction and polarization, and in all optical researches where the phænomena vary with the refrangibility of the rays, the gaseous lines will hereafter perform a most important part.

Had the solar lines been much broader than they are, we might have been able, by means of minute thermometers, to have ascertained the temperature of all those parts of the spectrum where there was no light, and thus to have determined whether or not the rays of light and heat are separate and independent emanations. The phænomena of the nitrous acid gas spectrum, the lines of which can be widened at pleasure, enable us to perform this and other interesting experiments, and thus to decide many important questions in the theory of radiant matter.

From the various experiments which I had made on the absorptive action of coloured media, I was led to a general principle, which, in that stage of the inquiry, appeared to possess considerable importance. The points of maximum absorption exhibited a distinct coincidence with some of the principal dark lines in the solar spectrum, and thus indicated that these lines marked, as it were, weak points of the spectrum, on which the elements of material bodies, whether they existed in the solar atmosphere or in coloured solids and fluids, exercised a particular influence. These actions, however, were so indefinite, that, with the exception of the oxalate of chromium and potash*, a salt of most remarkable properties, they never appeared in the form of lines or distinct bands. The light which was left shaded into the dark spaces, and therefore, notwithstanding the general coincidence which I had observed, the phænomena of ordinary absorption could not be identified with those of the definite actions by which the solar lines are produced.

This point of similarity, however, led me to institute a diligent comparison between the solar lines and those of the nitrous acid gas spectrum; and it did not require many experiments to prove, that there existed between these two classes of phænomena a most remarkable coincidence. In order to afford ocular demonstration of this fact, I formed the solar and the gaseous spectrum with light passing through the same aperture, so that the lines in the one stood opposite

* [See Lond. and Edinb. Phil. Mag., vol. ii. p. 362; and vol. vii. p. 436.]

those on the other, like the divisions in the vernier and the limb of a circle, and their coincidence or uon-coincidence became a matter of simple observation. I then superimposed the two spectra, when they were both formed by solar light, and thus exhibited at once the two series of lines, with all their coincidences, and all their apparent deviations from it. Professor Airy, to whom I showed this experiment, remarked, that he saw the one set of lines through the other, which is an accurate description of a phænomenon, perhaps one of the most splendid in physical optics, whether we consider it as appealing to the eye or to the judgement.

The general coincidence, thus cognisable by the eye, requires to be more particularly explained. Though some of the larger lines in the gaseous spectrum coincide with some of the larger ones in the solar spectrum, yet, in many cases, faint and narrow lines in the one coincided with strong and broad lines in the other; and there were some strong gaseous lines, and even broad bands, to which I could discover no counterpart in Fraunhofer's map of the spectrum, which, at this stage of my inquiry, was the standard to which I appealed. This discrepancy at first embarrassed me, and, as I observed it in parts of the spectrum where Fraunhofer had laid down every line which he had seen with his finest instruments, I abandoned all hopes of being able to establish the general principle of their identity. I was therefore obliged either to renounce this principle as one contradicted, or rather not confirmed by observation, or to consider Fraunhofer's delineation as in fault, and to enter upon the Herculean task of making a better map of the spectrum.

The magnificence of Fraunhofer's instruments,—the means of nice observation which he had at his command,—and his great skill as an observer, were considerations which long deterred me from even attempting to repeat his examination of the spectrum. Possessing such inferior means, and situated in so unfavourable a climate, I should have felt the attempt as presumptuous; but in the comparison which I had already made of the gaseous and solar lines, I had detected grave errors, and inexplicable omissions, in Fraunhofer's map, and was disposed even to adopt the suggestion of Mr. H. F. Talbot, (to whom I mentioned the fact, and who had the same confidence that I had in Fraunhofer's accuracy,) that a change might have taken place in the light of the sun itself, and that the delineation of the Bavarian philosopher might have been perfectly accurate at the time when it was executed. This supposition, however, became less and less tenable as I proceeded in the identification of the two classes of lines; but even if it had been otherwise, it would have added a still more powerful motive, while it afforded the best apology for undertaking a new delineation of the spectrum.

The apparatus which I had at my command for this investigation were two very fine rock-salt prisms, executed by myself; a large hollow prism made of plates of parallel glass for holding fluids; a fine plate glass prism, by Fraunhofer, and which I owe to the kindness of Mr. Talbot; a copious supply of oil of cassia and oil of cinnamon, which Mr. George Swinton transmitted to me from Bengal with his usual liberality; a good achromatic telescope, by Berge; and an excellent wire micrometer by Troughton. To this apparatus Mr. Robison made two important additions, which he executed with his own hands, the one a brass stand with a variable aperture for admitting the incident light, and the other a stage for holding and adjusting the prisms in front of the object glass; and I have recently been favoured by Sir James South with the use of his fine five-feet achromatic telescope, executed by Dollond.

After a little practice in the observation of the solar spectrum, I discovered most of the lines, which I had in vain sought for, in Fraunhofer's map, as the counterpart of those in the gaseous spectrum. I saw well-marked groups, of which he had only given one of the lines, and shaded bands, and well-defined lines, which his methods of observation had not permitted him to discover. After I had laid down all the principal features in the spectrum, I was able to examine the two classes of lines *pari passu.* The action of the gas upon invisible lines in the spectrum rendered them visible by slightly enlarging them, and this enlargement of a solar line indicated the existence of a corresponding line in the gaseous spectrum.

By this double process, and by methods of observation which I believe have never before been used in optical researches, I have been able to execute three different maps of the spectrum; *first*, a map of the lines in the solar spectrum; *secondly*, a map of the same spectrum, exhibiting at the same time the action of nitrous acid gas upon solar light, previously deprived of a number of its definite rays; and, *thirdly*, a map showing the action of the gas upon a continuous and uninterrupted spectrum of artificial white light. The general scale of these delineations is *four* times greater than that of Fraunhofer, but some portions of them are drawn on a scale *twelve* times greater, which became necessary from the impossibility of representing in narrower limits the numerous lines and bands which I have discovered. The length of Fraunhofer's spectrum is 15½ inches. Mine, upon the same scale, is nearly 17 inches. The length of the general spectrum, which I have delineated, is about *five* feet 8 inches, and the length of a spectrum, corresponding to the scale on which I have delineated parts of it, is *seventeen* feet.

Fraunhofer has laid down in his map 354 lines, but in the delineations which I have executed, the spectrum is divided into more than 2000 visible and easily recognised portions, separated from each other by lines more or less marked, according as we use the simple solar spectrum, or the solar and gaseous spectrum combined, or the gaseous spectrum itself, in which any breadth can be given to the dark spaces.

The suggestion of Mr. Talbot induced me to watch narrowly the state of the defective solar lines at different seasons of the year, in order to observe if any change took place in the combustion by which the sun's light is generated, or in the solar atmosphere through which it must pass. Such changes I have found to be very general in every species of terrestrial flame. The definite yellow rays which exist in almost all white lights, flicker with a variable lustre; and analogous rays in the green and blue spaces proceeding from the bottom of the flame, exhibit the same inconstancy of illumination. In the course of the winter observations, I observed distinct lines and bands in the *red* and *green* spaces, which at other times wholly disappeared; but a diligent comparison of these observations soon showed, that these *lines and bands depended on the proximity of the sun to the horizon, and were produced by the absorptive action of the earth's atmosphere.* I have no hesitation, therefore, in affirming, that during the period of my own observations, no change has taken place either in the dark lines or luminous bands of the solar spectrum; a result which seems to indicate, that the apparent body of the sun is not a flame in the ordinary sense of the word, but a solid body raised by intense heat to a state of brilliant incandescence.

The atmospheric lines, as they may be called, or those lines and bands which are absorbed by the elements of our atmosphere, have their distinctness a maximum, when the sun sinks beneath the horizon. The study of them, consequently, becomes exceedingly difficult in a climate where this luminary, even in a serene day, almost always sets in clouds; but as I have availed myself of every favourable moment for observation, I have been able to execute a tolerably accurate delineation of the atmospheric spectrum.

It is a curious circumstance, that the atmosphere acts very powerfully round the line D, and on the space immediately on the least refrangible side of it. It develops a beautiful line in the middle of the double line D, and by enlarging a group of small lines on the red side of D, it creates a band almost as dark as the triple line D itself. It widens generally all the lines, but especially the darkest one which I call *m* between C and D. It develops a band on the least refrangible side of *m*, and it acts especially upon several lines, and develops a separate band on the most refrangible side of C. The lines A, B, and C are greatly widened, and lines and bands are particularly developed between A and B, and generally throughout all the red space.

Most of the lines thus widened by the atmosphere are faint lines previously existing in the spectrum, and I have no doubt that they would be seen in the spectrum of the lime ball light condensed by a polyzonal lens, and acted upon by thirty miles of atmosphere.

The absorptive action of the atmosphere shows itself in a less precise manner in the production of dark bands, whose limits are not distinctly defined. A very remarkable narrow one, corresponding to one produced by the nitrous acid gas, is situated on the most refrangible side of C. Another very broad one lies on the most refrangible side of D, close to a sharp and broad band of yellow light, displayed by the general absorption of the corresponding part of the superimposed blue spectrum. There is also an imperfectly defined atmospheric action, corresponding to a group of lines where Dr. Wollaston placed his line C.

This general description of the atmospheric lines, while it indicates the remarkable fact, that the same absorptive elements which exist in nitrous acid gas exist also in the atmospheres of the sun and of the earth, leads us to anticipate very interesting results from the examination of the spectra of the planets. Fraunhofer had observed in the spectra of Venus and Mars, some of the principal lines of the solar spectrum. This, indeed, is a necessary consequence of their being illuminated by the sun, for no change which the light of that luminary can undergo, is capable of replacing the rays which it has lost. But while we must find in the spectra of the planets and their satellites, all the defective lines in the solar spectrum, we may confidently look for others arising from the double transit of the sun's light through the atmospheres which surround them.

Allerly, April 12, 1833.

LXXI. *On the Theory of Vanishing Fractions, in reply to the Observations of* Professor Young. *By* W. S. B. Woolhouse.*

IN my short but comprehensive essay on the principles of the differential and integral calculus, printed in the Appendix to the Gentleman's Diary for the years 1835–36, my expressed object was to remove, as far as was practicable, the perplexing difficulties usually experienced by those students who very naturally desire to bring the subject under the guidance and dominion of their reasoning faculties. The theory of vanishing fractions is well known to be the chief source of these difficulties; and it so happens, unfortunately for beginners, that writers have hitherto paid little or no attention to the strict interpretation that ought to be given, in the general sense, to a fraction when the values of its numerator and denominator have both absolutely disappeared or become equal to *zero*. This part of the subject has been diligently examined in the second part of my essay, where it is shown that a fraction in such a state may consistently possess any value whatever, if it be not limited by a special condition, but that one particular value only will fulfill the law of continuity assumed by the successive values immediately before or after the disappearances take place. It appears, however, that my explanations, there given, are insufficient to satisfy the scruples of Prof. Young, who has, in opposition to the principle I have adopted, entered into a very general statement of his own views at page 295 of the last Number of the Philosophical Magazine. The opinions of one so deservedly eminent as my esteemed friend are entitled to high consideration, and I am duly sensible of the respectful and condescending manner in which his observations are expressed. In a mathematical discussion of this kind, however, it would have been more desirable had Prof. Young attached less weight to his supposed evidence of authority, and applied himself more closely to the demonstration of his statements, nearly all of which are at direct variance with my judgement, and therefore, to me, far from being satisfactory. I here propose a brief and explicit examination of the most important points that Professor Young has advanced, and, I hope, with the same earnest anxiety for the spread of scientific truth that he has been pleased to ascribe to me.

To any person unacquainted with the inquiry, Professor Young's assertions would seem to imply that my views were of a strange and revolutionary description; that they were, in re-

* Communicated by the Author.

ality, adverse to the results of our ablest modern analysts, and directly opposed to well-established truths;—indeed it would almost appear that Professor Young himself had contracted that notion. No idea, however, could be imagined more contrary to the fact. The new line of theory, adopted and pursued in my essay, leads to precisely the conclusions subscribed to by all modern analytical writers, and varies only in the substitution of strict reasoning in place of the illogical and mysterious mode of deduction that has all along rendered this most important branch of mathematics a popular paradox. Professor Young has quoted two of my most general principles, which, with one or two more extracts, will convey a pretty correct idea of the particular view I have taken of the subject. As these extracts will very much facilitate the present discussion I shall here annex them.

I. As a principle, we have no right to reduce a fraction by dividing its numerator and denominator by *absolute nothing*, as the process removes from the fractions the indeterminate character which they previously possessed, and which they ought to retain. (Gentleman's Diary, Appendix, page 26.)

II. If, in the investigation of a geometrical problem, the unknown quantity is expressed by a fraction which in a particular case becomes a vanishing one, the problem in that case will resolve itself into a porism, and the value of the fraction, or unknown quantity, will then admit of arbitrary assumption; and a similar result will follow in all such cases, whatever be the nature of the investigation. (Page 25.)

III. Whenever, in an analytical investigation, the resulting expression for a quantity resolves itself into a vanishing fraction, we may observe, as a general rule, that either one of the original conditions of the inquiry becomes destroyed, or that two or more of them become dependent, and, consequently, whichever way it be, that there is at least one condition less to fulfill, and that the vanishing fraction is not restricted to any determinate value. (Pages 26, 27.)

IV. When a fraction, which in a particular case becomes a vanishing one, expresses the value of a quantity which we previously know, from the nature of the subject, does not become discontinuous in that case, or generally when such a fraction enters in any equation, the other terms of which are not discontinuous, the fraction is, under such circumstances, necessarily limited to continuous values, and consequently, when the terms vanish, it must take the particular value, (described in the essay,) or the ordinary result deduced either by the method of limits or the usual process of differentiation. (Page 29.)

The paragraphs II. and III., which embody the main principle, are those extracted by Professor Young, who labours under a misapprehension if he supposes that I contented myself with testing their accuracy by two particular examples. Has Professor Young read the remark on page 26 that immediately follows my examples? Speaking of the examples, I there add, that "these are not adduced as curious instances, but merely as examples of what *always* takes place in such cases." It is here evident that I had not contented myself with testing the accuracy of the propositions by the two particular examples. On the contrary, my conviction of their truth was founded on the solid evidence of mental demonstration, and the examples were adduced for the purpose of illustration without any reference to the proof of the principle itself, which, in common with the others, may be established without much difficulty. I shall now proceed at once to the demonstration of these principles.

First, then, it is required to be shown that, logically, we have no right to reduce a fraction by dividing its numerator and denominator by *absolute nothing*. Let θx, ϕx be two functions of a variable x which do not vanish when $x=a$; and suppose another variable y to be so connected with x as to always fulfill the condition

$$(x-a)^{\alpha}\,\theta x - y\,(x-a)^{\beta}\,\phi x = o \;\ldots\ldots\; (1)$$

in which α, β, are two positive numerical indices and either whole or fractional. The value of y deduced from this condition is

$$y = \frac{(x-a)^{\alpha}\,\theta x}{(x-a)^{\beta}\,\phi x} \;\ldots\ldots\ldots\ldots\; (2)$$

and takes the most general form of a vanishing fraction. Suppose it to be reduced by dividing the numerator and denominator by $(x-a)^{\beta}$, and it becomes

$$y = \frac{(x-a)^{\alpha-\beta}\,\theta x}{\phi x} = (x-a)^{\alpha-\beta}\,.\,\frac{\theta x}{\phi x} \;\ldots\ldots\; (3)$$

Let x now be taken equal to a and the expression (2) will become $y = \frac{o}{o}$, while (3) will give

$$y = \left\{\begin{matrix} o \\ \dfrac{\theta a}{\phi a} \\ \infty \end{matrix}\right\} \text{ if } \left\{\begin{matrix} \alpha > \beta \\ \alpha = \beta \\ \alpha < \beta \end{matrix}\right\} \ldots\ldots\; (4)$$

But if we refer back to the original condition (1), it is plain that it will be satisfied with $x = a$, independently of the value of y, that in this case it imposes no limit whatever on the value of y which is therefore completely indeterminate. It follows therefore that the result of (2), when $x = a$, viz. $y = \frac{o}{o}$, must have the same indeterminate acceptation; and that the process of dividing the numerator and denominator of (2) by $(x-a)^{\beta}$, ($=$ *zero* when $x = a$,) which produces (3), and so determines a *particular* value for y, is inadmissible when $x = a$, and ought not in that case to be performed. And as the expression (2) comprehends every possible case of vanishing fractions, the reasoning is general, and fully establishes the position occupied in the first extract.

It is here evident that the same objection will apply to the division by *zero* of an equation involving two variables, or that the equation resulting from a division by a common factor is inadmissible when that factor absolutely vanishes. Thus the equation (1), when divided by $(x-a)^{\beta}$, gives

$$(x-a)^{\alpha-\beta}\,\theta\,x-y\,\phi\,x=0,$$

which would, for $x=a$, give to y the *particular* value in (3), a circumstance quite inconsistent with the nature of the condition involved in the antecedent equation (1), which, in the case $x=a$, places no restriction on the value of y. It is also obvious that the multiplication of an equation, or of the numerator and denominator of a fraction, by *zero*, is equally objectionable, as regards propriety of reasoning, since, by that process, we might pass from conditions that determine particular values to others of a totally indeterminate character. Before quitting this point it will be well to draw a general and necessary inference that may, in conjunction with the fourth extract, contribute in some degree towards the elucidation of the present inquiry. It is this:—That when a quantity, which we know from other considerations ought to have a determinate value, comes out in a vanishing fraction, or, *vice versa*, when a quantity, which we know to be indeterminate, comes out in a determinate form, we may be assured that at least one of the steps, in the process of solution, fails in the manner here explained.

The proof of the principles contained in the other extracts immediately follows from the preceding demonstration. Suppose the equation (2) to express the result of an analytical investigation in which the reasoning throughout is admissible when $x=a$, so that no multiplication or division by a power of $x-a$ occurs in the process. We proceed to show that the resulting vanishing fraction (2), when $x=a$, must be indeterminate in value. The equation (1), which is antecedent to, and corresponds in signification with, the equation (2), is satisfied with $x=a$, without any reference to the value of y, because that equation is divisible by a positive power of $x-a$. Since, therefore, in the investigation, no multiplications or divisions have been made by $x-a$ or any power of it, it is conclusive that the series of equations, which precede the equation (1) in the course of reduction, must likewise be divisible by the same power of $x-a$, and therefore be satisfied with $x=a$, independently of the value of y. The primitive equation from which the expression (2) is deduced will consequently, when $x = a$, be also satisfied by any value of y. If this primitive equation expresses an original condition of the problem, that condition, therefore, when $x = a$, cannot limit the variable y, or the expression (2), to any particular value. If, however, this equation is produced by a combination of two or more leading equations of the problem, the circumstance of its wholly disappearing when $x = a$, will necessarily lead us to the conclusion that for this particular value of x some dependency exists among those leading equations, and therefore that one of the original conditions of the problem becomes, in that case, virtually destroyed. In addition to this proof we may remark that the expression (3) is legitimately deduced from the equation (1) in every case in which $x - a$ does not absolutely vanish, or in which the value of x differs from the quantity a, however small that difference may be; that since it holds good when x is taken as nearly equal to a as we please, and is in itself continuous as x approaches and arrives at that value, it is evident that, when x becomes exactly equal to a, it will express, as in (4), that particular value of y, or of the vanishing fraction (2), which unites in the law of continuity observed by all its other successive values.

Having attempted, and I expect successfully, the demonstration of the principles laid down in the extracts from my essay, without discovering them to be "fallacious," it now remains for Professor Young, since the truth is our common object, either to subscribe to my views or to point out wherein consists the inaccuracy of the reasoning here employed; and, without any wish to prolong our discussion, I unhesitatingly pledge myself to devote my most respectful and candid consideration to whatever arguments or explanations he may be pleased to offer. But it will be useless to pursue the subject any further unless Professor Young will enter more into the theoretical merits of the question and make up his mind to support every general statement with some kind of evidence.

In Professor Young's present letter he thinks it remarkable that I did not reflect that $\frac{0}{0}$ was as likely to be "the symbol of *absurdity*" as the symbol of multiple values, and he follows up the same idea by observing that "when we are operating with equations of the first degree, containing several unknown quantities, the symbol $\frac{0}{0}$ is, in fact, the very form which the result usually takes when the proposed equations involve incompatible conditions." If, however, subjects of absurdity are not to be absurdly treated, I apprehend it will not require any extraordinary degree of reflection to be convinced of the incorrectness of such a notion. On the other hand, it is rather remarkable that Professor Young did not consider that $\frac{a}{o}$ was the usual symbol of absurdity or of incompatible conditions, and that $\frac{o}{o}$ could never be so, in the result of an investigation logically conducted. Thus, the corresponding antecedent equation to the result $x = \frac{o}{o}$, when cleared of fractions, is $o\,x = o$ or $o = o$, an equation that is very obviously satisfied without any limitation to the value of x, and that cannot fail therefore to be compatible with other equations or conditions; but the corresponding antecedent equation to the result $x = \frac{a}{o}$ is $o = a$, an equation evidently indicating the presence of absurdity or of incompatible conditions, unless the nature of the investigation will admit of infinite results.

The query respecting the geometrical series is dismissed at once by a reference to the fourth extract from my essay. By putting for S the series it represents, the equation is

$$a + a\,r + a\,r^2 + \ldots\ldots + a\,r^{n-1} = \frac{a\,(r^n - 1)}{r - 1}$$

and as the left-hand member is not discontinuous when $r = 1$, the vanishing fraction, which forms the right-hand member, must be limited to its continuous value, viz. $n\,a$. The very circumstance of the equation involving both a determinate and an indeterminate quantity, when $r = 1$, indicates the existence of a fallacy in the process by which it has been deduced. We first have

$$S = a + a\,r + a\,r^2 + \ldots\ldots + a\,r^{n-1} \ldots\ldots (a)$$

and multiplying by $r - 1$, we get

$$(r - 1)\ S = a\,r^n - a = a\,(r^n - 1) \ldots\ldots (b)$$

which divided by $r - 1$, gives

$$S = a\,\frac{r^n - 1}{r - 1} \ldots\ldots\ldots\ldots (c)$$

In the case $r = 1$, and $r - 1 = o$, we have therefore committed the fault of multiplying by *absolute nought* in passing from (a) to (b); but the equation (c) is a true deduction from (b), for the mere placing of $r - 1$ in the denominator of a fraction is not an actual performance of division. The equation (a) becomes $S = a + a + a + \ldots\ldots$; the equation (b) entirely vanishes, and (c) becomes $S = \frac{o}{o}$.

After the foregoing discussion it will be needless to offer any special observations on the obvious inaccuracy of Professor

Young's views of the ellipse question. It may, however, be worth while to take the opportunity of adding a single remark on an erroneous principle which he appears to entertain regarding the general theory of analytical results. I never before heard of the incompetency of an analytical result to afford any positive information that an investigation could admit of. It is plain that the original equations, which express the analytical conditions of a problem, cannot include any extraneous conditions with those expressed in the enunciation, and that they must therefore comprehend, in their analytical results, every solution that the problem is capable of receiving. The equations, however, may not include certain other implied conditions, dependent on the peculiar nature of the inquiry, and therefore may yield some additional solutions incompatible with the conditions so implied. For instance the nature of a problem may be such as to exclude from the results not only imaginary values but negative values and values which fall beyond certain limits, though they will be unavoidably comprehended in the analytical solution. The exclusion of inadmissible solutions, therefore, rests with the nature of the problem and not with the forms of its analytical conditions. It is hence evident that Professor Young involves himself in a palpable inconsistency, when he arrives at the fact of the ellipse question admitting multiple solutions, by an examination of the original analytical conditions, and at the same time alleges that the analytical result is quite incompetent to supply that information; for the true analytical result must necessarily present every solution capable of satisfying the analytical conditions from which it has been deduced. If we refer back to the nature of the problem, as originally presented, which is the proper source of rejective information, we perceive that the only condition it imposes on the results is the limitation which requires the coordinates $x\,y$ to fall within the bounds of the ellipse, or of the circle that represents it in the indeterminate case.

I have thus unreservedly enumerated the principal reasons on which I found my sincere and firm conviction of the incorrectness of the various statements contained in Professor Young's letter. To avoid the possibility of being misunderstood, I have also given a concise analysis of the most important of the principles maintained in my essay; and, in conclusion, I may be permitted to add, that instead of their being "condemnatory of conclusions which, in the works of our ablest modern analysts, wear all the aspect of mathematical certainty," they establish the truth of those very conclusions on a firmer and more intelligible basis,—that instead of the mere aspect of certainty, in favour of those conclusions, they substitute certainty itself.

London, April 9, 1836.

LXXII. *Further Experiments on Conducting Power for Electricity.** By Edward Solly, *Jun., Esq.*

15. In my former communication I said that I had found iodine when solid to be a nonconductor, but I did not describe any experiments made with it in the melted state. This perhaps may have appeared an omission, the more so after Dr. Inglis's note (the contents of which had not, however, been communicated to me,) had been appended to my paper; but I had been advised to lose no time in describing such of my experiments as were in opposition to Dr. Inglis's statement that "iodine is a conductor".† What follows now will explain that apparent omission.

16. In all my original experiments I had found iodine to be a nonconductor in the fluid as well as the solid state; but on the present occasion, when I was led to repeat them by the above-mentioned statement, I was not a little surprised to find the iodine, when rendered fluid by heat, become a conductor. That a substance acting as iodine does should not be similar as to conducting power when fluid to what it is when solid, (as all known substances that have been as yet examined are, excepting only such as are electrolytes, and also perhaps the periodide of mercury,) but should appear a conductor upon assuming the liquid state, was so singular, and so contrary to my previous results and preconceived views, that I was induced to multiply my experiments; they continued unsatisfactory, and they were the more so as the iodine did not always appear a conductor but sometimes a nonconductor, and then, when it did appear a conductor it did in a very feeble manner, and with great uncertainty.

17. I was therefore led to doubt the purity of the iodine which I was using, and this seemed the more probable as it was from a different source from that which I had employed in the original experiments; and in the means which I had before described for examining conducting power it was impossible the wires could touch, and therefore the objection which the use of loose wires would have introduced was avoided. In consequence I procured some perfectly pure iodine sublimed at a very low temperature, and ascertained that that which I had

* Communicated by the Author: see our Number for February, p. 130.

† Lond. and Edin. Phil. Mag., No. 43. p. 129.

been previously using contained some impurity, most probably the iodide of iron, which is not unfrequently present in the iodine of the shops. The pure substance which I now used proved equally a nonconductor when fused as it had proved to be when solid.

18. When iodine is distilled with five times its weight of chlorate of potassa, a liquid comes over, which, according to Wöhler, is a chloride of iodine: it proved to be a very good conductor. Its solution in æther was also a good conductor, æther being as is well known a non-conductor. The chloride which I used was purified by being twice distilled off chloride of calcium. After the electric current had passed, upon examining the tube which had contained the chloride of iodine, I found that the one platinum wire, or that which had been the anode, was very much corroded, but still quite clean; the other, or that which had been the cathode, was encrusted with black matter very like iodine in appearance. So good a conductor indeed was this fluid, that the spark of a voltaic battery was hardly visibly impaired by interposing a small portion of it in the circuit. Great heat was evolved during the passage of the current, so that the liquid soon boiled.

19. The chloride of bromine and its solutions in water and æther were all good conductors.

20. I prepared iodic acid by Connel's process and then heated it up to its boiling point. I kept it fused and boiling for about a minute, and then allowed it to cool; by this means more than half was decomposed and volatilized, but what remained was I believe pure iodic acid. I used it immediately after this to prevent absorption of moisture from the atmosphere. I then found it a most distinct insulator when solid, but a very good conductor when fused, so much so that a spark might be easily taken from its melted surface. Its aqueous solution was also a very good conductor, and when strong, iodine was precipitated at the cathode.

21. It is very interesting and curious that iodic acid should behave thus, for as in all hitherto described experiments oxygen and iodine were both found to go to the same electrode, and as in order to the decomposition of a body the two composing ions must go to opposite electrodes*, it seems very unlikely that iodic acid should be an electrolyte: besides this, it is not composed of one proportional of each of its elements, which Mr. Faraday has shown to be the case with all known electrolytes†.

* Experimental Researches in Electricity, by Mr. Faraday, No. 828.—[Lond. and Edinb. Phil. Mag., vol. v. p. 425.—Edit.]

† Ibid. No. 679.—[vol. v. p. 167.]

If, however, it be an electrolyte, which is very improbable, it will be a proof that in the electrolyzation of a substance the evolution of the one ïon depends entirely on the nature of the other ïon with which it is combined; and thus the terms anïons and catïons will only be relative. If, however, iodic acid is not electrolyzed, still this experiment furnishes us with another exception to the law of liquido-conduction * similar to the periodide of mercury†.

22. Unfortunately, however, iodic acid is decomposed by the same degree of heat which is required to melt it, and the vapours of iodine entirely prevent the acid being examined during the experiment: it is also decomposed by almost all substances which can be used as electrodes, and therefore the advantage which can sometimes be taken of observing which of the electrodes is corroded, is here of no avail. I was therefore quite unable to ascertain whether iodic acid was electrolyzed or not; but when the electrodes were immersed in the fused acid, much stronger ebullition seemed to take place than before.

23. It was impossible to ascertain whether the oxides of bromine and chlorine were conductors or not, and I therefore had not the advantage of comparing iodic acid with the bromic and chloric acids.

I had at first some hopes of being able to add further experiments in relation to these last described, but finding that not in my power, I no longer delay sending the above.

7, Curzon Steeet, 15th April, 1836.

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

On the Theory and Solution of Algebraic Equations; with the Recent Researches of Budan, Fourier, and Sturm on the Separation of the Real from the Imaginary Roots of Equations: by J. R. Young, Professor of Mathematics in Belfast College. Souter, *London.*

WE have more than once dwelt upon the remarkable perspicuity of Mr. Young's writings. In this respect they are, one and all, models of the very best kind for the elementary writer, and far better adapted than any which we are acquainted with, for the purposes of actual study. In saying this, we mean no ordinary praise; for of all kinds of writing on science, and especially on mathematical science, the development of elementary principles in a perspicuous and logical manner is the most difficult. If Mr. Young had succeeded only in this, beyond any other author in our language, he would have achieved much, and have effected sufficient towards a diffusion, not only of

* Exp. Res. in Electricity, by Mr. Faraday, No. 402.—[Lond. and Edinb Phil. Mag., vol. iii.]

† Ibid. No. 691.—[vol. v. p. 169.]

mathematical knowledge, but of taste for mathematical pursuits in the younger branches of the community, to claim the gratitude of every sincere friend of science and of man. How many have turned away in disgust from the illogical statements (for arguments they deserve not to be called, nor, scarcely, even sophisms,) of the general mass of writers on analysis, under the impression, justly entertained so far as any impression could result from such works, that it was composed of a mere set of *hocus pocus* triflings! or in despair of ever acquiring even a glimpse of the promised land that lay beyond the elemental mountain-range, darkened as it was by the symbolical mists in which ignorant or injudicious compilers had involved them! Not such is the effect of Professor Young's volumes. He leads the student on by easy steps, generalizing the particulars, one after another, in a way that not only commands our assent, but interests the attention too deeply to allow of our being turned aside from the further pursuit of science. His algebraical reasonings are not less convincing than those of the Euclidian logic; and the hold which the elegant formulæ and elegant results he derives take upon the fancy, is not less strong than that which his compact and unsophisticated reasoning takes upon the understanding. So much may be said of *all* Professor Young's writings: but his present work, in addition to this, has many and peculiar claims upon the attention of the mathematical world, as well as upon the young and aspiring class of mathematical students.

From the time that our distinguished countryman Harriot transposed the "absolute term" to the same side of the equation with the other terms, algebra has taken a new aspect,—a totally new character. He was thus enabled to show that an equation of the nth degree may be compounded, from n simple equations having n roots, which may be any numbers whatever; and he inferred (not so illogically as has been often represented by *foreign* historians of algebra *, and too implicitly admitted by our own,) that every equation of the nth degree has also n roots. From that time the great problem of algebra became the determination of those roots by a *practicable process*. Certain cases of it had been already solved, so far as the fourth degree inclusive, by more than one person; the simple and the quadratic equation at a very early period, the cubic by Tartaglia and Cardan, and the biquadratic by Ferrari. All these were solved by exhibiting a general formula in terms of the 2nd and 3rd roots of certain assigned functions of the coefficients; and the ambition of the earlier inquirers was to find analogous expressions for the roots of the fifth and higher degrees. The inquiry under this form has been *altogether unsuccessful*; and the most signal mistakes, and, in many cases, the most ludicrous ones, have been made in the progress of such attempts.

* A lithographic specimen of a manuscript page of Harriot's work, published by that eminent mathematical antiquary Professor Rigaud of Oxford, in his Supplement to the works of Dr. Bradley, sets this question quite at rest. He distinctly understood the nature both of negative and imaginary roots. The "Ars Praxis Analyticæ," we would add, *is rather to be taken as a specimen of Warner's power to comprehend Harriot's views, than as a standard of those views themselves.*

The problem, on the authority of very careful researches into the relation that must subsist amongst the roots themselves in the composition of the coefficients, and the degree of the subsidiary equations to which the algebraical expression of those relations conducts us, is now known to be incapable of solution by a general formula. If this be established satisfactorily (and to our own minds it is so), the inquiry is ended in this direction; and the only ground to hope for a solution is in the discovery of some process which shall evolve the several roots by one continuous series of operations, figure after figure, till either the whole of them are assigned; or, when the roots are irrational, till so many figures shall be assigned as are necessary for the purpose had in view in the problem in which the equation originated *. This was the method followed by Newton, whose sagacity led him to see the hopelessness of a general formula of solution, if not its essential impossibility,—one instance amongst many of his extraordinary prescience of the history of science in after ages. Nor was this done after a casual view of the subject, but after careful investigations of its character, as is evident from the researches which he made respecting the relations between the roots and the coefficients of a literal equation—researches, to the results of which, much as they have been since pursued, the labours of his successors have made comparatively unimportant additions. His method of approximation, however, with which we are now more immediately concerned, was characteristic of his great mind, and remained till our own time, except under peculiar circumstances, not only the briefest, but the best that had been proposed. Still it had its difficulties and imperfections, even after the initial figure of a root had been found, and these were fully exposed by Lagrange in the 5th note to his *Traité de Resolution des Equations* as far back as 1798; and though they have been in some degree removed by Mr. Horner (in the Annals of Philosophy,) and Baron Fourier (in his *Analyse des Equations Determinées,*) the method is on many accounts incumbered with difficulties that are of a serious *practical* nature, and essentially inherent in the principle of the process.

A method of approximation, very elegant in theory, and though not rapid in execution, yet free from some of the defects incident to Newton's method, was given by the celebrated analyst just referred to, Lagrange, by which the root was exhibited in a continued fraction. This, too, besides its practical tediousness, had other inconveniences, several of which, by the labours of Mr. Horner, published in the Annals of Philosophy and the Journal of the Royal Institution, were almost entirely removed. Still the tediousness which is essential to its first principle of operation is such as to render it useless in practice, except where some very important object arises to justify the employment of the great length of time which its practice requires.

It is to Mr. Horner that we owe a *simple, rapid, easy,* and *complete* method of continuous approximation, disencumbered of all extraneous

* The investigation is here referred to cases in which the equation is reduced to a *rational form*, as indeed are all the general conclusions which are deduced respecting equations. A valuable dissertation on irrational or surd equations is given by Mr. Horner in the present volume of this Journal, p. 43.

operations and symbols, and arranged in a form so condensed as rather to resemble, in appearance, the extraction of the square or cube root of a numerical quantity, than the solution of a complete equation, but considerably less complicated than even that operation as it is generally laid down. All the work is visible to the eye, and is arranged in a series of columns *beneath the coefficients**: and these, step by step, formed by the multiplication of the result in one column by a single digit, and added to the next throughout the series, till the new subtrahend is found beneath the absolute term. By a repetition of the same process, a new trial divisor is found and verified, and the coefficient of a new equation, having its roots reduced by the quantity brought out already, has its coefficients standing in the same columns, instead of the original coefficients at the head. A continuance of this simple process evolves the root, figure after figure, till the whole of them if rational, or as many as are requisite if irrational, are determined. The process, moreover, instead of becoming more complicated, and the determination of the next figures more uncertain, becomes more simplified in the first respect, and more certain in the other; and the last half of the whole number of figures (save one) are obtained by mere division. Moreover, it presents at the end of the process, the coefficients of a new equation, which contains the remaining roots of the original equation. It thus, whilst the root is actually assigned, presents us with the depressed equation simultaneously produced,—an additional advantage of the method. Nor are these all; but our space does not allow of our entering into further particulars.

This substitution of a *general method* for a *general formula* is not, indeed, the object after which the lovers of mere symbols have been straining; and perhaps such formulæ, though in their employment, a hundred times the work would be required, would be more accordant to such prejudices: nevertheless, the mathematician who looks at the question with a philosophic eye, will see, that as this process is requisite even in the *extraction of the roots* which any such formulæ must involve, the search after those formulæ is a matter of mere trifling. Though it strains at the naked gnat, it can swallow the symbolized camel,—the more smoothly, the more incumbered it is with these useless and unintelligible hieroglyphics! By the mathematician who values a result, less by its extreme algebraical complication than by the elegant facility of its application to the main purposes for which the formulæ were devised, a method like this must be hailed as one of the greatest boons that has ever been conferred upon the scientific community. Its influence will soon be felt in every department of philosophy which involves considerations respecting mensurable quantity, by whatever means the measures can be effected; and in many cases even theories will be brought to a decisive test, the numerical results upon which this testing depended having been hitherto involved in equations which no ardour or perseverance could resolve by any of the methods heretofore proposed.

Though this discovery was published in the Philosophical Transac-

* No letters are introduced, and the whole process is *purely numerical.*

tions in 1819, and was immediately pirated by others, yet it has unaccountably been neglected amongst mathematicians in general, in England. For this we cannot, nor do we pretend to account; but it is unfortunate, less for Mr. Horner's sake than for the sake of mathematical science. We hope, however, that the elegant exhibition of the principles and the processes of the method which are given in this work by Professor Young (together with Mr. Horner's own illustrations of the subject in Leybourn's Mathematical Repository, vol. v.,) will have the effect of familiarizing at least the younger and more inquiring English mathematicians with this beautiful system of numerical solution. No work could be better calculated to produce such an effect, and we doubt whether any (even a minor) improvement can be made upon it as it stands in this work, and in Mr. Horner's papers.

Even amongst those who have felt disposed to do justice to Mr. Horner's labours, there are few, it appears to us, who are fully aware of the extent of the applicability of his theorems. He has, indeed, only *applied* them in one particular direction; and it is, perhaps, too much to expect that others will be hasty in making applications of them which he has not suggested. The unaccountable neglect with which his past labours have been received, may well discourage the most ardent and persevering mind. We sincerely trust, however, that he has yet many years of activity and usefulness before him, and that he may still be able to accomplish some of the purposes, to which his previous investigations directly lead.

As in the extraction of the square and cube roots, so in this general evolution of the roots of an equation, the first figure is to be found tentatively; and as in them, so here, the successive figures are determined with greater certainty at every successive step. The divisional portion also of neither one nor other commences till after the first step: or in other words, the initial figure of the root and its sign are required as a separate and preparatory step to the operation of the method. The difficulty of effecting this first step has always been found to be great. Lagrange proposed a method which, though *theoretically perfect*, was yet, from the immense labour which it involved, *utterly incapable of application to practice*, except, indeed, in cases in which the necessity for its application was partially superseded by other methods, namely, in the equations of the first four degrees *. It was reserved for Budan to overcome this difficulty in his *Nouvelle Méthode*, published in 1803, with the high approbation of Lagrange himself. Most unaccountably, this valuable work lay neglected in France till after Navier's publication of Fourier's *Anal. des Eq. Det.* in 1831, and in England till it was made known to the readers of our Magazine and of Leybourn's Repository by Mr. Horner. It was soon perceived

* Common justice requires that the laborious researches of the Abbé de Gua (*Mém. de l'Acad.*, 1741) should not be overlooked in any history of this problem. Though only in a very limited degree successful, he yet opened the road of inquiry, and deduced several very important results. In his expression for the *number of conditions* necessary to render all or any number of the roots of an equation real, he is certainly wrong; but this is not the place to discuss the source of his error, or to give the true formula.

by the Continental mathematicians that Fourier's was but a slight modification of Budan's method, and accordingly the French elementary writers since that time have invariably given the name of Budan, not that of Fourier, to the method. The few English mathematicians who have spoken on the subject, following Navier and Fourier, or rather Mr. Peacock's account of the matter, have designated them as "Fourier's rules." Professor Young ascribes them rightly to Budan; and we hope that, as his work must of necessity obtain an extensive circulation, the mistake will be gradually corrected. We hope it is not too late, though we well know how difficult it is to eradicate a familiar epithet, however unjust; as, for instance, in the case of "Cardan's Rule" for cubics, and "Mercator's Projection" of the Sphere, neither of which, it is well known, was the invention of the persons whose names they bear, whilst the names of their authors, Tartaglia and Wright, are almost unknown, except to well-read mathematicians. This rule was an immense advance in the progress of actual solution, as it enables us to discover the number of roots which lie between any assigned limits, *a* and *b*, and to determine whether they be real or imaginary. The initial (or, if need be, any number of figures,) of the real roots may be successively determined; and hence the methods of actual approximation, whether that of Newton, of Lagrange, or of Horner, may be immediately commenced, and the determination of it gradually and systematically effected*. This method, however, though in comparison of Lagrange's "Equation of the Squares of the Differences of the Roots" such as to induce any one to rejoice in its discovery, and value it as perfect, still a simpler, more direct, and effective method has been since discovered by M. Sturm, already alluded to. It was read to the French Institute in 1829, before the publication of Fourier's *Traité*, but was not published in its *Mémoires* till a few months ago. It was, however, printed in Crelle's *Journal für die reine und angewandte Mathematik*, about a year after, and was introduced in an abridged form into the works of Lacroix, Bourdon, and Lefebre de Fourcy, soon and successively. *No allusion to it*, however, appeared in *any English work* till the publication of Professor Young's treatise. It is the more extraordinary that Mr. Peacock should have overlooked it when writing his "Report", as it was so easy of access from so many quarters. The memoir has since been accurately and elegantly translated into English, as we noticed in a late Number, by Mr. W. H. Spiller. The best and most simple of all the abstracts of this important paper that we have seen is that of Mr. Young, in the work before us. Mr. Horner has well termed it the "gem of the book,"—well, as modestly coming from *him*; but still, to our thinking, not more a gem than the version of his own methods in the same work: and to follow the metaphor, we would add that it is here cut, polished, and set in the most tasteful and elegant manner of which it seems capable. Even to accomplished mathematicians, to whom the subject is new, we strongly recommend the reading of Mr. Young's chapter before taking up the original memoir, as it will

* Fourier employs the Newtonian, though he has put it in almost the worst form, perhaps, of which it is susceptible for actual working.

greatly facilitate the study of Sturm's details to have Young's general view of its essential parts already in the mind: to younger and less experienced students this course is indispensable, whilst to those who are but arrived at the threshold of the subject, by their previous acquirements, no inducement to pursue the obvious and natural course is necessary. The process itself is, we may add, in application, only the method of finding the greatest common measure of two algebraical expressions.

The *real roots*, both positive and negative, being successively evolved, an equation is left in which all the roots *are imaginary*. For all the purposes of actual calculation, the problem then is perfectly solved. Still, for many reasons, it is desirable to be able to assign the quadratic factors of which the depressed equation is composed. Is it too much to hope that another Horner or another Sturm may rise up in our own day to render the solution, *in every sense*, complete?

The process of Sturm is the same as that which leads, in the usual operation itself, to the detection of the equation which is composed of *equal roots*, and terminates there at once, so that we cannot but detect them as we proceed. This is a great advantage, in as much as we cannot pass over this circumstance *unknowingly*. We have, it is true, to depress the equation, and proceed anew; but we have removed all ambiguity as to equal roots, and done very much towards their determination. This advantage is peculiar to the method of Sturm. If real roots, in the reduced equation, lie between narrow limits, we know they are not *equal* ones, and therefore proceed to their separation with *certainty*.

We would not, however, conceal from our readers the fact that, advantageous as Sturm's rule generally is, in comparison with that of Fourier and Budan, still the great facility with which the derived polynomes are formed in the latter method, compared with the tedious calculations which the former often requires, is a great and decided advantage in this stage of the work. Wherever, from the want of some visible relation amongst the coefficients of the given equation existing, it is probable that the derivation of Sturm's V_1, V_2, etc. (or X_1, X_1, etc. of Young's notation) will give high numbers as coefficients of these derived polynomes, we think it better to defer the application of *either* method, till, by successive substitutions in the usual manner, it is rendered evident that some test will be required, by the appearance of "a doubtful interval." Should there be but *one* such doubtful interval, or even a small number of them, compared with the degree of the equation, then we think Fourier's method will be the less operose: but if several, then unquestionably it will be most simple to have recourse to Sturm's in preference to the other. The *directness* of Sturm's process, and the less danger of interchanging the additive and subtractive signs of the result, is an advantage, however, which furnishes great relief to the attention during the operation, and which, to calculators who are not in the almost daily use of either the one or the other, will be duly valued as an important feature of this process. It may, moreover, often be abbreviated in practice by using only a few of the higher places of figures, instead of all which result from finding the coefficients of the derived polynomes; as the ratios alone are sought, and these can hardly ever be required to extreme exactness, since the general character, not the particular values, are generally sought. At the same time some experience, and the foresight which experience alone can give, is requisite to distinguish when, and under what circumstances, this abbreviation can be used with perfect safety. If we take, for instance, the biquadratic equation $32x^4 + 41x^3 - 184x^2 - 24x + 1 = 0$, the application of Sturm's method brings us to products (and in these cases logarithms cannot be used with perfect safety, except where the contractions can be used) of *sixteen figures*; and as the coefficients of equations which arise out of any inquiry to which algebra may be made subservient, and under all conditions of the data, are generally less likely to be so simple as the example given above, some subsidiary methods of lessening the actual trouble are yet not only desirable, but necessary. Cannot Mr. Horner so apply or modify his principle of "Synthetic Division", as to furnish a more direct and easy algorithm for Sturm's Rule? We think we see more than one way by which this may be accomplished; but we leave it in better hands, when we refer over the problem into his*.

The space which we can devote to a review will not allow us to give even a general analysis of Professor Young's treatise. It is sufficient to say that it contains all that can be interesting to the student on the subject of equations, developed with his usual perspicuity and elegance; and that it is brought, in all essential points, to the state of science at the present hour; and though principally intended for the use of students who have only mastered the first principles of algebra, and happily adapted to their wants, yet as a syllabus for recalling to the minds of the most extensively read mathematicians on the subjects of equations, the essentials of what they already know, we are persuaded that it will be of considerable utility. With this conviction we take our leave of the work, happy if our favourable notice shall be the means of rendering it more extensively known, and that less for the sake of Professor Young than of the numerous persons who may derive advantage from his labours.

* We have often wondered that the method of working with the "detached coefficients" in algebraic multiplication and division has never been introduced into practice, and even into elementary works: and that the beautiful contrivance of what its inventor has called "Synthetic Division" (see Leybourn's Repos., vol. v.,) has not also become a school-boy practice ere now. This is also one amongst the many valuable improvements in algebra and arithmetic conferred on mathematicians by Mr. Horner. The *eleventh* edition of Hutton's course, edited by Dr. Gregory, is the only *elementary* work in which it has yet appeared.

Herpetologia Mexicana, *seu Descriptio Amphibiorum Novæ Hispaniæ, quæ itineribus Comitis de Sack, Ferdinandi Deppe et Chr. Guil. Schiede in Museum Zoologicum Berolinense pervenerunt.* Pars I. *Saurorum Species amplectens, adjecto Systematis Saurorum Prodromo, additisque multis in hunc Amphibiorum ordinem observationibus, edidit Dr. Arend Friedericus Augustus Wiegmann. Accedunt tabulæ lithographicæ decem, novorum generum typos exhibentes.* Berolini sumptibus C. G. Lüderitz, 1834. London, W. Wood, Tavistock-street.

This Work is intended to form two volumes, of which the first contains the order *Sauri*, and the second will contain the *Ophidii*, *Chelonii*, and *Batrachii*. The author treats of the *Sauri*, on which he has founded the Prodromus of his system, at large, giving careful characteristic definitions of all known genera, appending the Crocodiles (*Loricati*, Merr.) and the *Amphisbænoides*, as he has done in a former work (*Handbuch der Zoologie*, Berlin 1832), as aberrant suborders. The anatomical characters of the typical subdivision (*Squamati*) and of both the aberrant (*Loricati* and *Annulati*) are investigated at length. The typical subdivision (*Squamati*) is divided into the series *Leptoglossi*, *Rhiptoglossi*, and *Pachyglossi*, which are developed in a Synopsis. The central group is formed by the *Chamæleontes* alone; the *Leptoglossi* and *Pachyglossi* form the lateral divisions, and are subdivided into two sections.

Leptoglossi.		Rhiptoglossi.	Pachyglossi.	
Sect. I. (aberrans.) (*a.*) *Brevilingues.*	Sect. II. (typica.) (*b.*) *Fissilingues.*	Sect. I. *Vermilingues.*	Sect. I. (typica.) (*b.*) *Crassilingues.* (Agamæ.)	Sect. II. (aberrans.) (*a.*) *Latilingues.*
Fam.	Fam.	Fam.	Fam.	Fam.
1. Lacertæ.	1. Monitores.	Chamæleontes.	1. Dendrobatæ.	Ascalabotæ.
2. Stychopleuri.	2. Trachydermi.		2. Humivagæ.	
3. Chamæsauri.	3. Ameivæ.			
4. Scinci.				
5. Gymnophthalmi.				

The families of the aberrant sections inhabit both hemispheres, those belonging to the typical only one, or the tribes belonging to the Old and New World show at least a great difference in their dentition. The families are well characterized according to the peculiarities of their outward form and their osteological peculiarities. The author has added several observations to the genera, and has described a great number of new genera and species from all parts of the world. He refers, in describing the four last families of the *Brevilingues*, to a Synopsis of the genera. In the description of the *Sauri* of Mexico, which begins at p. 22, all the living genera are described with great accuracy, and he has often given a complete view of the anatomy of the typical species, also a Conspectus of all the species of the genus, with short diagnoses and descriptions of the Mexican species. The author has also endeavoured to show their relation to those from other parts of the world. The coloured plates surpass those of Wagler in accuracy, and give not only a true copy of the scaly covering of the animal, but so represent their habit and physiognomy, that they appear to be drawn after living specimens.

W. F.

FLORA METROPOLITANA; *or Botanical Rambles within Thirty Miles of London. Being the results of numerous Excursions made in* 1833, 1834, 1835, *furnishing a List of those Plants that have been found on the different Heaths, Woods, Commons, Hills, &c., surrounding the Metropolis (more particularly in the Counties of Surrey and Kent,) chiefly from actual Observation and the latest Authorities. Intended for the Student in practical Botany: with a List of the Land and Fresh-water Shells of the Environs of London.* By Daniel Cooper, London: S. Highley, 32, Fleet street.

After a long, wet, and dismal winter, in the dirt and smoke and noise of the City, there is something inspiring in the title of this little work,—"*Botanic Rambles* within Thirty Miles of London." At sight of it the sky seems at length to brighten, the air becomes mild, and our imagination carries us to many a delightful spot as we glance over the *habitats* which Mr. Cooper has recorded. Nor will the botanist of the provinces smile at his brethren in the Capital when they exult in the opportunities which are afforded them for their favourite pursuit, if he considers the beauty and variety of the country within a circuit of thirty miles of London, and the innumerable means of conveyance ready at every moment of leisure or fine weather to transport them to the scene of their investigations. Of this district also, how considerable is the portion in which Nature has maintained her undisturbed sovereignty in spite of inclosure-acts, corn-laws, and those artificial prices which have too often brought the crooked ploughshare to violate tracts that mock at cultivation; but which, when unappropriated and unperverted, used to yield spontaneously a rich feast to our nobler appetites! The lover of heath and thicket and forest, down and marsh and wood, gliding stream, and shady lane, may certainly go further and fare worse; nor will the pedestrian generally meet with more comfortable and reasonable entertainment than the inns within this circuit afford. To all these advantages we may now add that London possesses admirable schools for regular botanical instruction, since that important step in our social progress, the foundation of the University of London, and the consequent establishment of King's College: here the labours of such eminent botanists as Professors Lindley and Don cannot fail to be attended with extensive usefulness, not to mention other meritorious teachers connected with our medical schools. Highly valuable, however, as such aids unquestionably are, Botany, as Professor Martyn has well observed, "is not to be learned in the closet; you must go forth into the garden or the fields, and there become familiar with Nature herself,—with that beauty, order, regularity, and inexhaustible variety which is to be found in the structure of vegetables, and that wonderful fitness to its end which we perceive in every work of creation." It has also been justly said by another writer, that "the plants which adorn and characterize a picturesque country, impressed on the recollection by that attention which the botanist is led to bestow on them while enjoying his rambles, contribute largely to the stock of delightful associations which he carries away with him, and often call up the remembrance of the scenes in which he observed them. And in the intervals of rest, or of unfavourable weather, he may furnish himself with agreeable occupation in examining such as are new to him."—*Flora Vectiana*, Pref.

As the season has we trust arrived when we may exclaim, in the words of the Royal Botanist,

> Lo, the winter is past,
> The rain is over and gone.
> The flowers appear on the earth,
> The time of the singing of birds is come:

we shall gladly recommend this little volume to those who are disposed to connect the study of nature with the purest enjoyment. Both what it contains and what it lacks may give them pleasing occupation, especially if they will endeavour to supply Mr. Cooper with contributions for a new edition. And if they would add to their pleasure by giving some attention to a kindred pursuit, we shall recommend to them another companion in their excursions: namely,

The Entomologist's Useful Compendium; or an Introduction to the Knowledge of British Insects; comprising the best Means of obtaining, preserving, studying, and arranging them; with a Calendar of their times of appearance, &c., illustrated with Plates: Part I. Longman and Co.

The publication has been seasonably commenced in monthly parts, each part containing, in addition to a portion of the work, a Calendar of the times of appearance of insects for the ensuing month, the places where they may usually be found, and directions for collecting them, which will afford great assistance to the student.

The merits of the work are well known from the former edition, which was soon exhausted; and Mr. Samouelle has been long engaged in improving it, and adapting it to the advanced state of natural history.

LXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 156.]

Dec. 10, 1835.—THE following papers were read: "Memoranda taken during the continuance of the Aurora Borealis of November 18, 1835." By Charles C. Christie, Esq. Communicated by Samuel Hunter Christie, Esq., F.R.S.

The appearances described were seen from Deal, on the day mentioned in the title, from 9 to 20 minutes past 10 o'clock in the evening; and consisted chiefly of a bright arch of light, of which the lower edge was sharply defined, surmounted on a dark cloud below, while the upper edge was shaded off into the cloudless and starlight sky, emitting large but faint luminous streaks, which issued upwards with great rapidity, exactly imitating flames agitated to and fro by a violent wind.*

"Démonstration complète du Théorème dit de Fermat: par François Paulet, de Genève, ancien élève de l'E'cole Polytechnique." Communicated by P. M. Roget, M.D., Sec. R.S.

The theorem of which the author professes to give, in this paper, the complete demonstration, is the following: "No power, beyond the second degree, of any quantity, can exist, capable of being resolved into the sum, or the difference, of two other powers of the same degree:" or, as it may still more generally be expressed, "If the exponents of three powers be multiplied by the same number, provided that number be greater than 2, neither the sum, nor the difference, of any two of the resulting quantities can ever be equal to the third quantity."

Dec. 17.—"Researches towards establishing a theory of the Dispersion of Light, No. II." By the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford.

The author, in a preceding paper, published in the last part of the Philosophical Transactions†, commenced a comparison between the results of M. Cauchy's system of undulations, expressing the theoretical refractive index for each of the standard rays of the spectrum, and the corresponding index found from observation in different media. Since that paper was communicated, he has received the account of a new series of results obtained by M. Rudberg, and comprising the indices for the standard rays in a prism of calcareous spar, and in a prism of quartz, both for the ordinary and the extraordinary rays; and also the ratios of the velocities in the direction of the three axes of elasticity, respectively, in Arragonite and Topaz. The author was accordingly led to examine this valuable series of data, and the comparison of them with the theory forms the subject of the present paper. He finds the coincidences of theory and observation to be at least as close as those already obtained from Fraunhofer's results, and to afford a satisfactory extension of the theory to ten new cases, in addition to those already discussed; and a further confirmation of the law assigned by the hypothesis of undulations.

A paper was in part read, entitled, "On the action of Light upon Plants, and of Plants upon the Atmosphere." By Charles Daubeny, M.D., F.R.S., Professor of Chemistry and of Botany in the University of Oxford.

Jan. 7, 1836.—A paper was read, entitled, "Meteorological Journal kept at the Royal Observatory, Cape of Good Hope, from the 1st of June to the 31st of December, 1834." Communicated by Capt. Beaufort, R.N., F.R.S., Hydrographer to the Admiralty.

* Other particulars respecting this Aurora have been given in our late Numbers: by Mr. Sturgeon; p. 134; Dr. Robinson, p. 236; and Prof. Rigaud, p. 350.—Edit.

† An abstract of Prof. Powell's preceding memoir will be found in Lond. and Edinb. Phil. Mag., vol. vi. p. 374: various papers on the subject, by Prof. Powell and Mr. Tovey, have appeared in our last and present volumes.—Edit.

The observations recorded in this Journal are those of the barometer, and of two thermometers, one in, and the other out of doors; taken at sunrise, noon, sunset, and midnight, in each successive day from the 1st of June, 1834, to the end of the year.

"Some Account of the Volcanic Eruption of Coseguina in the Bay of Fonseca, commonly called the Coast of Conchagua, on the Western Coast of Central America." By Alexander Caldcleugh, Esq., F.R.S.

The particulars recorded in this narrative are derived partly from a voluminous collection of official reports transmitted from the authorities in various towns to the government of Central America, and partly from the information of intelligent eye-witnesses of the phænomena. The eruption occurred on the 19th of January, 1835, and was preceded by a slight noise, accompanied with a column of smoke issuing from the mountain, and increasing till it took the form of a large and dense cloud, which, when viewed from a distance of ten leagues to the southward, appeared like an immense plume of white feathers, rising with considerable velocity and expanding in every direction. Its colour was, at first, of the most brilliant white; but it gradually became tinged with grey; then passed into yellow; and finally assumed a beautiful crimson hue. In the course of the following days several shocks of an earthquake were felt, the last of which were most terrific. On the morning of the 22nd, the sun had risen in brightness; but a line of intense darkness denoted the presence of the same cloud which had before presented such remarkable appearances, and which, extending with great rapidity, soon obscured the light of day; so that in the course of half an hour the darkness equalled in intensity that of the most clouded night: persons touched without seeing one another; the cattle hurried back to their folds; and the fowls went to roost, as on the approach of night. This atmospheric darkness continued with scarcely any diminution for three days; during the whole of which time there fell a fine impalpable dust, covering the ground at St. Antonio to the depth of two inches and a half, and consisting of three layers of different shades of grey colour: and for ten or twelve succeeding days the sky exhibited a dim and murky light. At Nacaome, to the northward of the volcano, the same degree of darkness was experienced, and the deposit of ashes was from four to five inches in depth, and exhaled a fetid sulphureous odour, which penetrated through every interstice in the buildings. The complete obscurity was only occasionally broken by the lightning, which flashed in every direction, while the air was rent with loud and reiterated explosions like the discharges of artillery, which accompanied each eruption of volcanic matter, and conspired to strike the deepest terror, and to spread among the inhabitants a universal panic that the day of judgement was arrived. On the 24th the atmosphere became clearer, and the houses were found covered to the depth of eight inches with ashes, in which many small birds were found suffocated. Deer and other wild animals flew to the town for refuge, and the banks of the neighbouring streams were strewed with dead fish. In Segovia, and as far as eight leagues from the volcano, the showers of black sand were so abundant as to destroy thousands of cattle; and many were subsequently found whose bodies exhibited one mass of scorched flesh.

Within the Bay of Fonseca, and two miles from the volcano, it is stated that two islands, from two to three hundred yards in diameter, were thrown up, probably from the deposit of masses of scoriæ on previously existing shoals.

Jan. 14.—Dr. Daubeny's paper entitled, "On the action of Light upon Plants, and of Plants upon the Atmosphere," was resumed and concluded.

The objects of the experimental inquiries of which the author gives an account in this paper were, in the first place, to ascertain the extent of the influence of solar light in causing the leaves of plants to emit oxygen gas, and to decompose carbonic acid, when the plants were either immersed in water, or surrounded by atmospheric air. The plants subjected to the former mode of trial were *Brassica oleracea, Salicornia herbacea, Fucus digitatus, Tussilago hybrida, Cochlearia armorica, Mentha viridis, Rheum rhaponticum, Allium ursinum*, and several species of *Gramineæ*. Geraniums were the only plants subjected to experiment while surrounded with atmospheric air. Comparative trials were made of the action on these plants of various kinds of coloured light, transmitted through tinted glass, of which the relative calorific, illuminating, and chemical powers had been previously ascertained; and the results of all the experiments are recorded in tables; but no general conclusion is deduced from them by the author. He next describes a few experiments which he made on beans, with a view to ascertain the influence of light on the secretion of the green matter of the leaves, or rather to determine whether the change of colour in the chromule is to be ascribed to this agent. The third object of his inquiries was the source of the irritability of the *Mimosa pudica*, from which it appeared that light of a certain intensity is necessary for the maintenance of the healthy functions of this plant, and that when subjected to the action of the less luminous rays, notwithstanding their chemical influence, the plant lost its irritability quite as soon as when light was altogether excluded. He then examines the action of light in causing exhalation of moisture from the leaves; selecting Dahlias, Helianthuses, Tree Mallows, &c., as the subjects of experiment. The general tendency of the results obtained in this series is to show that the exhalation is, *cæteris paribus*, most abundant in proportion to the intensity of the light received by the plant. He also made various comparative trials of the quantity of water absorbed, under different circumstances, by the roots of plants, and chiefly of the *Helianthus annuus, Sagittaria sagittifolia*, and the *Vine*. From the general tenor of the results of these and the preceding experiments, he is inclined to infer that both the exhalation and the absorption of moisture in plants, as far as they depend on the influence of light, are affected in the greatest degree by the most luminous rays; that all the functions of the vegetable economy which are owing to the presence of this agent, follow, in this respect, the same law; and that in the vegetable, as well as in the animal kingdom, light acts in the character of a specific stimulus. The author found that the most intense artificial light that he could obtain from incandescent lime produced no sensible effect on plants.

The latter part of the paper is occupied by details of the experiments which the author made with a view to ascertain the action of plants upon the atmosphere, and more especially to determine the proportion that exists between the effects attributable to their action during the night and during the day; and also the proportion between the carbonic acid absorbed, and the oxygen evolved.

His experiments appear to show that at least 18 per cent. of oxygen may be added to the air confined in a jar by the influence of a plant contained within it. He also infers that the stage of vegetable life at which the function of purifying the air ceases, is that in which leaves cease to exist. The author shows that this function is performed both in dicotyledonous and in monocotyledonous plants, in evergreens as well as in those that are deciduous, in terrestrial and in aquatic plants, in the green parts of esculents as well as in ordinary leaves, in Algæ and in Ferns as well as in Phanerogamous families. Professor Marcet has shown that it does not take place in Fungi*.

The reading of a paper, entitled, "On the Anatomical and Optical Structure of the Crystalline Lenses of Animals, being the continuation of the paper published in the Philosophical Transactions for 1833." By Sir David Brewster, K.H., LL.D., F.R.S.,—was commenced.

Jan. 21.—Sir David Brewster's paper, entitled, "On the Anatomical and Optical Structure of the Crystalline Lenses of Animals, being the continuation of the paper published in the Philosophical Transactions for 1833," † was resumed and concluded.

The author has examined the structure of the crystalline lens of the eye of a great variety of animals belonging to each of the four classes of Vertebrata; and has communicated in this paper a detailed account of his observations, arranged according as they relate to structures more and more complex. In a former paper, published in the Philosophical Transactions for 1833, the lens of the Cod fish was taken as the type of the simplest of these structures, in as much as all the fibres of which it is composed converge, like the meridians of a globe, to two opposite points, or poles, of a spheroid or lenticular solid; both of which poles are situated in the axis of vision. The structure which ranks next in respect of simplicity is that exhibited in the Salmon, among fishes; in the Gecko, among reptiles; and in the Hare, among Mammalia. It presents at each pole two septa placed in one continuous line, in different points of which all the fibres proceeding from the one surface to the other have their origin and termination. A structure somewhat more complex is met with in the lenses of most of the Mammalia, and is particularly exemplified in the lion, the tiger, the horse, and the ox. Three septa occur at each pole in the form of diverging lines inclined to

* A notice of the results obtained by Prof. Marcet will he found at p. 82 of the present volume.—Edit.

† Sir D. Brewster's former paper on this subject was given entire, with additions, in our Number for March last, p. 193.—Edit.

one another at angles of 120°. The next degree of complexity is presented in the lens of the whale, the seal, and the bear, which contain, instead of three, four septa on each side, placed at right angles to each other in the form of a cross. In some specimens of lenses of whales and seals the author observed two septa from each pole, forming one continuous line, from each of the extremities of which proceeded two others, which were at right angles relatively to one another: so that there were in all five on each surface. The most complex structure is that of the lens of the elephant, which exhibits three primary septa diverging at equal angles from the pole, and at their extremities bifurcating into two additional septa, which are inclined to each other at angles of 60°, these latter being the real septa, to which the fibrous radiations are principally related. In some lenses of the elephant the author found the three septa immediately proceeding from the poles exceedingly short, and approaching to evanescence; so that he has no doubt that occasionally they may be found to have disappeared, and that the other six septa will then all diverge from the poles, like the radii of a hexagon, at angles of 60°.

In all the preceding cases, where the arrangement of the fibres is symmetrical on the two sides, the septa on the opposite surface of the lens occupy positions which are reversed with respect to one another; thus in the simple case of the double septa at each pole, the line formed by those of the posterior surface is situated at right angles to that formed by the septa of the anterior surface. Where there are three divergent septa at each pole, the direction of those on the one side bisect the angles formed by those on the other side; and again, where the septa form a rectangular cross, those of one surface are inclined 45° to those of the other surface.

It follows as a consequence of this configuration of the series of points which constitute the origins and terminations of the fibres, that all the fibres, with the exception only of those proceeding in a direct line from the extremities of any of the septa, must, in their passage from the one surface to the other, follow a course more or less contorted; and must form lines of double curvature; that is, curves of which none of the portions lie in the same plane.

The fibres of the lenses of quadrupeds gradually diminish in size from the equator or margin of the lens, where they are largest, to their terminations in the anterior or posterior septa. They are united together by small teeth like those of fishes; but, generally speaking, the teeth are smaller and less distinctly pronounced, and sometimes they are not seen without great difficulty.

In the lens of the turtle, as well as in that of several fishes, the arrangement of the fibres, instead of being symmetrical on the two sides, as is the case in all the preceding instances, is different on the anterior and posterior surfaces; there being two septa on the former, but none in the latter, which presents only a single polar point of convergence.

The author has directed much of his attention to the optical properties of these structures. The lens of the salmon depolarizes three series of luminous sectors; the inner and outer series being negative, and the intermediate series positive. The polarizing structure of the cornea is negative, and it depolarizes very high tints at its junction with the sclerotic coat. When a slice cut from the sclerotica nearly perpendicularly to the surfaces, and with parallel faces, is exposed to polarized light, it exhibits the system of biaxal rectilineal fringes, exactly like those in a plate of glass heated by boiling water or oil, when in the act of rapid cooling. The same alternation of properties with regard to polarization in the successive strata of the substance of the crystalline lenses is exhibited by other fishes which the author examined.

With respect to the final cause of these highly complicated arrangements, it is reasonable to conceive that the gradually increasing density of the fibres in each successive stratum from the surface to the centre is intended to correct spherical aberration: but the design of the other properties resulting from the arrangement of the fibres with reference to septa, in all their variations of number and position, and more especially the alternations of positive and negative structures, as exhibited by the action of the different strata in polarized light, has not even excited the ingenuity of conjecture, and will probably remain among the numerous problems destined to exercise the sagacity of another age.

Jan. 28.—A paper was read, entitled, "Discussion of Tide Observations made at Liverpool." By J. W. Lubbock, Esq., F.R.S.

The chief purpose which the author has in view in presenting the tables accompanying this paper, which are a continuation of those published in the Philosophical Transactions for 1835, and are founded on the observations instituted by Mr. Hutchinson at Liverpool, is to exhibit the diurnal inequality in the height of high water, which is scarcely sensible in the river Thames, but which at Liverpool amounts to more than a foot. The diurnal inequality in the interval appears to be insensible.

The author has further ascertained that Bernoulli's formulæ expressing the height of the tide, deduced from his theory of the tides, present a very remarkable accordance with observation.

Feb. 4.—"Geometrical Investigations concerning the Phænomena of Terrestrial Magnetism: Second Series,—On the number of points at which a magnetic needle can take a position vertical to the Earth's surface." By Thomas Stephens Davies, Esq., F.R.S. Lond. and Edin., F.R.A.S., of the Royal Military Academy, Woolwich.

This paper is intended as a continuation of the one by the same author published in the last volume of the Philosophical Transactions*; in which it was proposed to investigate the mathematical consequences of the hypothesis of the earth being a magnet with two poles, or centres of force, situated anywhere either within, or at the surface, and of equal intensity, but of contrary characters: with the ultimate view of verifying this hypothesis by comparing its results, so deduced, with the phænomena furnished by observation.

* An abstract of Mr. Davies's former paper appeared in Lond. and Edinb. Phil. Mag., vol. vi. p. 302–305.—Edit.

In his former paper the author had shown that on this hypothesis the magnetic equator, or the locus of the points at which the magnetic needle takes a horizontal position, is one single and continuous line on the surface of the earth. In this paper his object is to prove that there are always two, and never more than two, points at the earth's surface, at which the needle takes a position vertical to the horizon.

At the close of his former paper the author had deduced the equation of the curve of verticity, that is, of the curve at any point of which an infinitesimal needle being placed, it will always tend towards the centre of the earth, and consequently be vertical to the horizon at its point of intersection with the surface of the earth: but, owing to circumstances over which he had no control, he was unable, at that time, to write out an account of his investigations of the peculiar character of that curve, or to apply its properties to the determination of the latter problem: and these are more especially the objects to which the present paper is devoted.

The processes to which he has had recourse, with this view, are the following. He first transforms the rectangular equation of the curve into a polar equation, and finds that in the result the radius vector is involved only in the second degree; and hence that for every value of the polar angle there are two values of the radius vector, and never more than two; or, in other words, that no line drawn from the centre of the earth can cut the curve of verticity in more than two points. But as no means present themselves of ascertaining whether the values of (r), the polar ordinates of the curve of contact, be always real or not, or how many values of ($\theta_{,}$), the other co-ordinate to that curve, are possible for any given value of r; he abandons this method of inquiry, contenting himself with a few deductions respecting the general form of the locus, and proceeds to employ a different method.

The general system of his reasonings proceeds on the principle that as the magnetic curve itself, and the curve of verticity have one common and dependent genesis, a knowledge of the properties of the former must throw considerable light on those of the latter; and he is accordingly induced to enter into a more minute examination of the magnetic curve than had before been attempted. As both the polar and the rectangular equations of this curve are much too complex to afford any hope of success in their investigation, the author has recourse to a system of co-ordinates, which he terms the "*angular system*," and which was suggested to him originally by the form under which Professor Playfair exhibited this equation in Robison's Mechanical Philosophy. But as he has not yet published his investigations of the differential coefficients, and other formulæ necessary in the application of this system, he puts his results in a form adapted to rectangular co-ordinates; each rectangular co-ordinate being expressed in terms of his angular co-ordinates and the constants of the given equation; and by these means deduces the characters of the magnetic curve throughout its whole course.

The angular equation being

$$\cos\theta_{,} + \cos\theta_{,,} = 2\cos\beta,$$

he finds, 1°, that the two equations, the convergent and the divergent, or that in which the poles are unlike, and that in which they are like, are both expressed by this equation, and essentially included in it: 2°, that the divergent branches on one side of the magnetic axis are algebraically and geometrically continuous with the convergent branches on the other side; the parameter (β) being the same in both cases: 3°, that the divergent branches are assymptotic, and the assymptote is capable of a very simple construction; 4°, that the continuous branches have the poles as points of inflexion, and that these are the only points of inflexion within finite limits: 5°, that a tangent at any point of the curve, or, which is the same thing, the direction taken by a small needle placed there, admits of easy construction: 6°, that when the parameter (β) is such as to cause the convergent and divergent branches to intersect, they do so in a perpendicular to the magnetic axis drawn from the poles: 7°, that the convergent branches are always concave, and the divergent always convex, to a line at right angles to the magnet, drawn from its middle,—besides other properties not less interesting, though less capable of succinct enunciation.

Having separated the branches belonging to the case of like poles from those belonging to the unlike ones in the magnetic curve, the author proceeds to a similar separation of the corresponding branches in the curve of verticity. In the former case the curve is composed of two branches infinite in length, having the magnetic axis for assymptotes, lying above that axis, and emanating from the poles to the right and left; and of two finite branches, continuous with those just described, and lying below the magnetic axis; one of which passes through the centre of the earth, and meets the other in the perpendicular from the middle of the axis; so that the whole system is constituted by one continuous curve, extending from negative infinite to positive infinite, and having the lines drawn from the centre of the earth to the magnetic poles as tangents at the poles; and no part of the curve lies between these tangents. It bears in form some general resemblance to a distorted conchoid; this curve not having either cusp or loop. In the second case, the curve is also composed of four branches, two finite and two infinite ones; the latter having the line drawn from the centre of the earth through the middle of the magnet as assymptotes, and both lying on the same side of it as the more distant pole; and the finite branches joining these continuously at the poles, and each other in the middle of the magnetic axis; the one from the nearer pole lying above the axis, and the one from the remoter pole lying below it. The branches, where they unite at the poles, have the lines drawn from the centre of the earth to the poles as tangents, and the lower infinite branch passes through the centre. The whole system of branches is comprised between the polar tangents; and the two systems are mutually tangential at the poles, and intersect each other at the centre; but they have no other point in common.

Lastly, the author proceeds to demonstrate that a circle (namely, the magnetic meridian) described from the centre of the curve of verticity, will always cut the convergent system in two points, but can never cut it in more than two. He remarks, however, that if we could conceive two poles of like kinds to exist without any other whatsoever, we might have either four points of verticity, or only two, according to circumstances; but he waves the discussion of this particular case, as being irrelevant to the purpose of his present inquiry.

Mr. Davies announces his intention of shortly laying before the Society a continuation of these researches; devoting the next series to the points of maximum intensity.

"Memoir on the Metamorphoses in the *Macroura*, or Long-tailed Crustacea, exemplified in the Prawn (*Palæmon serratus*)." By John V. Thompson, Esq., F.L.S., Deputy Inspector-General of Hospitals. Communicated by Sir James Macgrigor, M.D., F.R.S., &c.

The author gives descriptions, illustrated by outline figures, of three different stages of growth of the Prawn; the first being that of the larva immediately on its exclusion from the egg; the second, at a later period, when it has acquired an additional pair of cleft members, and a pair of scales on each side of the tail; and the third, at a still more advanced stage of development, when it presents the general appearance of the adult Prawn, but still retains the natatory division of the members, now increased to six pair. The author thinks it probable that an intermediate stage of metamorphosis exists between the two last of these observed conditions of the animal.

Feb. 11.—A paper was in part read, entitled, "On Voltaic Combinations." In a letter addressed to Michael Faraday, Esq., D.C.L., F.R.S. Fullerian Professor of Chemistry in the Royal Institution of Great Britain, &c., &c. By John Frederick Daniell, Esq., F.R.S., Professor of Chemistry in King's College, London.

Feb. 18.—The reading of Mr. Daniell's paper, entitled, "On Voltaic Combinations," in a letter to Michael Faraday, Esq., D.C.L., F.R.S., &c., was resumed and concluded.

The author, after expressing his obligations to Mr. Faraday for the important light which his late researches in electricity have thrown on chemical science*, proceeds to state that in pursuing the train of inquiry which has thus been opened, he has obtained further confirmations of the truth of that great principle discovered and established by Mr. Faraday, namely, the definite chemical action of electricity; and has thence been led to the construction of a voltaic arrangement which furnishes a constant current of electricity for any required length of time.

For the purpose of ascertaining the influence exerted by the different parts of the voltaic battery in their various forms of combi-

* The greater part of Mr. Faraday's Experimental Researches in Electricity will be found entire in Lond. and Edinb. Phil. Mag., vol. iii., vol. v., vol. vi.; and of his other Series abstracts have been given in Phil. Mag. and Annals, N.S., vol. xi., and in the succeeding volumes of Lond. and Edinb. Phil. Mag. Mr. Faraday's Seventh Series, on the definite chemical action of Electricity, appeared in Lond. and Edinb. Phil. Mag., vol. v. p. 161; and his Tenth Series, on the construction of the Voltaic Battery, in our present volume, p. 114.—Edit.

nation, he contrived an apparatus, which he designates by the name of *the dissected battery*, and which consists of ten cylindrical glass cells, capable of holding the fluid electrolytes, in which two plates of metal are immersed; each plate communicating below, by means of a separate wire, which is made to perforate a glass stopper closing the bottom of the cell, with a small quantity of mercury, contained in a separate cup underneath the stopper, and with which electric communications may be made at pleasure through other wires passing out of the vessel on each side. The active elements of the circuit, which were adopted as standards of comparison, were, for the metals, plates of platinum and amalgamated zinc three inches in length by one in breadth; and for the electrolyte, water acidulated with sulphuric acid, in the proportion of 100 parts by volume of the former to 2·25 of the latter; this degree of dilution (giving a specific gravity of 1·0275,) being adopted, in order to connect the author's experiments with those of Mr. Faraday.

This dilute acid exerts scarcely any local action on amalgamated zinc; because the surface of the metal becomes covered with bubbles of hydrogen gas, which adhere strongly to it; and this force of heterogeneous adhesion appears to have an important influence on the phænomena both of local and of current affinity, and soon puts a stop to the decomposition of the water by the zinc. When a small quantity of nitric acid is added to the acidulated water, the same plate which in the former experiment resisted the action of the diluted sulphuric acid, is, in a few hours, entirely dissolved, without the extrication of any gaseous matter. This result is explained by the author on the supposition that the elements of the nitric acid enter into combination with the hydrogen as it is evolved, and that the opposing attraction of this latter substance is thus removed. The author finds, in like manner, that nascent hydrogen deoxidates copper, and precipitates it from its solutions upon the negative plate of the voltaic circuit.

A series of experiments performed with the dissected battery is next described; illustrating, in a striking manner, the difference of effects with relation to the quantity and the intensity of the electric current, consequent on the different modes of connecting the elements of the battery: the former property being chiefly exhibited when the plates of the respective metals are united together so as to constitute a single pair; and the latter being exalted when the separate pairs are combined in alternate series. The influence of different modifications of these arrangements, and the effects of the interposition of pairs in the reverse order, operating as causes of retardation, are next inquired into.

In the course of these researches, the author, being struck with the great extent of negative metallic surface over which the deoxidating influence of the positive metal appeared to manifest itself, as is shown more especially in the cases where a large sheet of copper is protected from corrosion by a piece of zinc or iron of comparatively very small dimensions, was induced to institute a more careful examination of the circumstances attending this class of phenomena; and was thus led to discover the cause of the variations and progressive decline of the power of the ordinary voltaic battery, one of the principal of which is the deposit of the zinc on the platina [or copper] plates; and to establish certain principles from which a method of counteracting this evil may be derived. The particular construction which he has devised for the attainment of this object, and which he denominates the *constant battery*, consists of a hollow copper cylinder, containing within it a membranous tube formed by the gullet of an ox, in the axis of which is placed a cylindrical rod of zinc. The dilute acid is poured into the membranous tube from above by means of a funnel, and passes off, as occasion requires, by a siphon tube at the lower part; while the space between the tube and the sides of the copper cylinder is filled with a solution of sulphate of copper, which is preserved in a state of saturation by a quantity of this substance suspended in it by a cullender, allowing it to percolate in proportion as it is dissolved. Two principal objects are accomplished by this arrangement; first, the removal out of the circuit of the oxide of zinc, the deposit of which is so injurious to the continuance of the effect of the common battery; and, secondly, the absorption of the hydrogen evolved upon the surface of the copper, without the precipitation of any substance which would lead to counteract the voltaic action of that surface. The first is completely effected by the suspension of the zinc rod in the interior membranous cell into which fresh acidulated water is allowed slowly to drop, in proportion as the heavier solution of the oxide of zinc is withdrawn from the bottom of the cell by the siphon tube. The second object is attained by charging the exterior space surrounding the membrane with a saturated solution of sulphate of copper, instead of diluted acid; for, on completing the circuit, the electric current passes freely through this solution, and no hydrogen makes its appearance upon the conducting plate; but a beautiful pink coating of pure copper is precipitated upon it, and thus perpetually renews its surface.

When the whole battery is properly arranged and charged in this manner, it produces a perfectly equal and steady current of electricity for many hours together. It possesses also the further advantages of enabling us to get rid of all local action by the facility it affords of applying amalgamated zinc; of allowing the replacement of the zinc rods at a very trifling expense; of securing the total absence of any wear of the copper; of requiring no employment of nitric acid, but substituting in its stead materials of greater cheapness, namely, sulphate of copper, and oil of vitriol; the total absence of any annoying fumes; and lastly, the facility and perfection with which all metallic communications may be made and their arrangements varied.

LINNÆAN SOCIETY.

April 5.—A paper was read, entitled, "On the Ovula of *Santalum album*; by William Griffith, Esq., Assistant Surgeon in the Madras Medical Service: communicated by R. H. Solly, Esq., F.L.S."

In this paper are detailed minute observations on the fecundation of *Santalum album*, carried on through all the stages of that process;

and throwing additional light on the subjects investigated by Mr. Brown, in his papers on the Sexual Organs and mode of Impregnation in *Orchideæ* and *Asclepiadeæ,* of which abstracts were given in Phil. Mag. and Annals, N.S., vol. x. p. 437; and Lond. and Edinb. Phil. Mag., vol. i. p. 70.

A paper was also read, containing particulars of the lives of the two eminent botanists of the early part of the last century, named Sherard,—especially of William Sherard, who founded the Professorship of Botany at Oxford which bears his name,—and of Dillenius, who was appointed the first professor upon that foundation; derived from the papers of the celebrated Peter Collinson, and communicated to the Society by Aylmer Bourke Lambert, Esq., V.P.L.S.

William Sherard was appointed English consul at Smyrna, where he made large collections of plants, which he brought to England with him, and with the assistance of his brother, James Sherard, commenced the preparation of his *Pinax.* James Sherard afterwards became possessed of a botanical garden at Eltham, in which he cultivated the plants of the *Hortus Elthamensis.*

It is mentioned as a point of some curiosity, that Dillenius, though attached to the study of mosses, which are among the most diminutive of plants, was himself " tall and clumsy." Notices are also given of Catesby, author of the "Natural History of Carolina;" among these it is stated that the plates illustrating the works of both Dillenius and Catesby were all drawn as well as engraved by the authors themselves, and the works produced under circumstances of great discouragement.

ROYAL SOCIETY OF EDINBURGH.

Feb. 15.—The award of the Keith Prize to Professor Forbes having been announced by the Council on the 18th of January, the medal was presented by Dr. Hope, the Vice-President in the Chair, accompanied by an address to the following effect.

The prize founded by our late estimable associate Mr. Keith, whose ingenious contrivances for self-registering thermometers and barometers are recorded in our Transactions, is, by the regulation of his Trustees, to be adjudged biennially for the most important discovery communicated to the Royal Society, or in the event of such being wanting, for the best paper which shall have been presented to the Society in the space of two years on a scientific subject. The Council, in discharge of the powers vested in them, have awarded unanimously the Keith prize for the last biennial period, to Professor Forbes, for his paper " On the Refraction and Polarization of Heat," which they consider to come under that class of communications, which contain discoveries important to science.

The Vice-President then observed, that the subject of heat is one so important to man, and so intimately connected with a variety of natural phænomena, that it has not failed to command a great degree of attention in all ages:—That an intimate connexion subsists between Heat and Light, and that much discordance of opinion has subsisted respecting the nature of both. He next stated the various opinions entertained concerning them, and particularly respecting heat, and in historic order presented the views of Bacon, Boyle, Boerhaave, Stahl, and Black, and adverted to the discoveries of Black respecting latent and specific heat, and the successive labours of Irvine, Crawfurd, Wilke, Magellan, Lavoisier and Laplace, Dulong and Petit, in the same field.

Heat presents itself in two very different conditions; first when combined with matter, pervading bodies slowly, either by communication and conduction through and among its particles, or by the movements of the particles themselves; secondly, when radiated, moving through elastic fluids or empty space with vast velocity.

The first of these had been studied by the philosophers already named, and not long after by Rumford. To the second of these, viz. radiant heat, the subject of Professor Forbes's discovery called upon him more especially to allude, and to present a brief historic view.

The radiation of cold, and its reflection by metallic mirrors, was known to Baptista Porta in the sixteenth century; and observations were made on the radiation of heat, by the Florentine academicians, towards the middle of the seventeenth century, and by Marriotte in 1682. About the middle of the 18th century, Lambert published his works on pyrometry and photometry, which contained some of the first accurate experiments on this subject; and the facts of the difficult transmission and reflection of heat by glass, were pointed out by the Swedish chemist Scheele. Pictet of Geneva extended his experiments on the radiation and the reflection of the heat derived from boiling water; and our venerable associate Professor Prevost of the same place, established the doctrine of the mobile equilibrium of heat, in 1802. The triumph of this theory was found in the beautiful experiments of Dr. Wells, on dew, in 1813.

Meanwhile, the experiments of Rumford and Leslie were corroborating and extending these general views, even although the doctrines of radiation were denied by the latter philosopher in all his writings. The passage of radiant heat through solid substances, such as glass, and through fluids, such as water, had long been admitted, in the case where light accompanied heat. But in the case of non-luminous heat, it was strenuously denied by Leslie, and others. The experiments of De la Roche proved that such was the fact, at least in the case of heat derived from terrestrial sources, and at the same time luminous. But this subject has received a vast enlargement by the recent experiments of Melloni, who has shown that substances differ surprisingly in their permeability to heat, and that while some, such as alum, stop almost every incident ray, others, as rock-salt, transmit almost the whole of the heat, and that from whatever source derived.

The connexion of light with heat was too obvious and important to be overlooked. To Sir W. Herschel the world is indebted for the first great step in this curious inquiry. He examined the thermometric qualities of the spectrum formed from the sun's rays by a common prism of glass; and in 1800 announced the curious fact, that the heating power increases, not only from the violet to the red end of the spectrum, but *even beyond the latter*, indicating the existence of dark calorific rays. These experiments, though at first denied by some authors, were afterwards fully confirmed, and some anomalies which they presented, explained, by Robison, Englefield, Berard, Seebeck*, and Melloni.

Heat, then, even unaccompanied by light, appears to be capable both of reflection and refraction. But new modifications of light, discovered of late years, require us to investigate how far the analogy may be pursued. In 1802, Dr. Young announced his remarkable discovery of the interference of the rays of light, or the power of two luminous rays, properly disposed, *to produce darkness* by their union. About the year 1808, Malus, a most eminent French philosopher and mathematician, discovered the remarkable modification which light undergoes by reflection from certain substances at certain angles. This modification may be easiest conceived by stating the fact, that light so reflected becomes incapable of undergoing a second reflection in certain positions of the reflecting surface, when common light would be reflected.

The corresponding experiment in the case of heat was tried by Berard, along with Malus, about the year 1811, and an account of them was published in 1817, in the *Mémoires d'Arcueil*. They found, that when the solar beam was twice reflected in the manner just stated, the heat and light refused simultaneously to be reflected in certain positions of the second reflector. The same experiment was repeated with incandescent bodies, with the same result; and even, as stated by Berard, with bodies having temperatures beneath that of visible incandescence. These experiments were probably discontinued in consequence of the death of Malus, and the details were never published, if, indeed, they were ever carried to any great extent. The result has been, that Berard's conclusion seems not to have been generally adopted by the scientific world. The polarization of heat has remained amongst the doubtful facts in science. It has been adopted in scarcely any systematic works, whether British or foreign: and, of late years, direct evidence seemed to be entirely against it. Professor Powell of Oxford, repeatedly and fruitlessly, attempted to obtain Berard's result. Nobili of Florence (whose recent loss science has to deplore) attempted it likewise with the aid of his thermo-multiplier, an instrument admirably adapted for the measurement of small quantities of heat; and Melloni having failed to polarize even luminous heat by tourmalines, concurs in the conclusions of Powell and Nobili. The Vice-President then observed, that it was under these circumstances that the subject was undertaken by Professor Forbes, who, by means of arrangements differing from any that had before been used, has succeeded in completely establishing the polarization of heat under all the circumstances in which light is polarized, namely, by Reflection, Transmission, and Double Refraction, and that it is for the establishment of these facts that the Keith Prize has been awarded by the Council†.

* Seebeck's memoir on this subject will be found in Phil. Mag., First Series, vol. lxvi. p. 330. *et seq.*—Edit.

† Prof. Forbes's paper establishing these facts will be found at large in Lond. and Edinb. Phil. Mag., vol. vi. p. 134, *et seq.* See also vol. vii. p. 349.—Edit.

Dr. Hope then stated that, in the ordinary case of the publication of papers, the Society holds itself in no degree responsible for the truth of the facts stated therein; but, in the adjudication of prizes, the case is different; and that, with regard to them, the Council are bound to be satisfied of the truth of the statements for which they award their prize. Several members of the Council had seen and satisfied themselves of the accuracy of Mr. Forbes's leading experiments before the Keith Prize was awarded; and, some days ago, he deemed it right to request Mr. Forbes to show him the more important of these experimental demonstrations. This he succeeded in doing in a way which left upon his mind not the slightest doubt as to the truth of his results; the variations of temperature being so obviously displayed, as to prevent the slightest ambiguity as to the true source from which they are derived. The instrument employed in the research is the thermo-multiplier, of which the invention is due to Nobili, though it has been greatly improved for experimental purposes by Melloni. Professor Forbes has likewise increased greatly its power of indicating the more delicate effects by employing a telescopic apparatus, which enables him to measure a quantity of heat, perhaps not exceeding *one fifteen hundredth* part of a degree of Fahrenheit.

That the Society may fully understand the nature of the proofs afforded by Mr. Forbes's experiments, reference must be made to the correlative facts observed in the case of light.

When light undergoes reflection from glass at an angle of 56°, its physical character is found to be thus far altered, that it refuses to be a second time reflected by another plate of glass placed to receive the ray at the same angle of 56°, if the plane of incidence on the second glass be perpendicular to the plane of incidence on the first. The light is then wholly transmitted by the second plate. If the plane of incidence be the same for the two plates, complete reflection takes place at the second plate. This illustrates polarization by *reflection*.

If a number of glass plates be used, and light *transmitted* obliquely through such a bundle of plates, it is in like manner found, that the emergent light is wholly transmitted by a second similar bundle placed parallel to the first, but is almost wholly reflected, and therefore *not* transmitted, when the second bundle is placed so that whilst the ray falls upon it at the same angle as upon the first, the plane of incidence on the second bundle is perpendicular to the plane of incidence upon the first bundle. This is polarization by *transmission* or *refraction*.

Lastly, It was observed before the close of the 17th century by Huyghens, that certain bodies, as Iceland spar, endowed with the property of double refraction, alter at the same time the character of the light in the two refracted rays. So that, if two sections similarly cut from a crystal of Iceland spar be placed upon one another in *conformable* positions, or the respective positions which they occupied on the crystal, the two rays will proceed through the second slice as they did through the first, and be refracted according to the same laws. But if the second slice be placed *unconformably* upon the first, or turned round a quarter of a circle, the ray, which at first was ordinarily refracted, is now extraordinarily refracted; and the ray, which at first was extraordinarily, is now ordinarily refracted. Now, it has been found that some crystals, such as tourmaline, possess the property, first, of dividing these rays, *and then of suppressing or absorbing one of them*; the result of which is, that when two tourmalines, cut as we have supposed, are placed *conformably*, the ray which was not suppressed by the first slice, still makes its way through the second; but, when placed *unconformably*, the ray transmitted by the first plate is wholly suppressed by the second. In the latter case, therefore, not a ray of light can penetrate the two plates. This is polarization produced by *double refraction*.

Now, all these modes of polarization have been recognised by Mr. Forbes in the case of heat, and even in the case of heat wholly unaccompanied by light. The Vice-President announced that he had witnessed this in the most satisfactory manner in the case of heat polarized by reflection and transmission, for which purposes, instead of glass, (which permits scarcely any non-luminous heat to penetrate it,) Mr. Forbes employs plates of mica, divided by a peculiar process into extremely thin laminæ.

But the analogies which he has established between light and heat do not stop here. It has been found in the case of light, that, when the two reflecting plates before spoken of, or the two crystals, are placed in *unconformable* positions, so that little or no light reaches the eye, we may, by interposing between the plates or the crystals a thin lamina of a doubly refracting substance (such as mica) in a certain position (relatively to its internal structure), cause a portion of light, which before was incapable of reaching the eye, to become capable of so doing. In other words, the polarized light, which at first was incapable of reflection or transmission at the second plate or crystal, now becomes capable of it; it has lost, to a certain extent, its character of polarization, or it is said to be depolarized.

Dr. Hope stated, that he had seen this to be most completely effected in the case of heat, by Mr. Forbes. A lamina of mica is interposed between the bodies used to polarize heat unconformably placed. When the lamina of mica has a certain position, no effect is produced beyond stopping a small portion of the heat, which would otherwise reach the thermometer; but when this interposed lamina is turned 45° in its own plane, a portion of the heat which before was incapable of reaching the thermometer in consequence of its polarization, is now capable of doing so, and the influx of heat is instantly indicated. The most striking exemplification of this result is found in the fact, which excited so much interest when communicated more than a year ago to the Society, that in certain cases the mere interposition of a piece of mica (in the proper situation), will cause an immediate indication of increased temperature, the mica *depolarizing* more heat than it *stops*. Since depolarization takes place only in consequence of double refraction, we have here another undoubted proof of the double refraction of heat.

The Vice-President terminated his general and rapid sketch, in which he alluded to the brilliant discoveries of Brewster, Arago, and Fresnel, respecting the polarization of light, by observing that it would be needless for him to point out the important bearing of these facts on the question of the nature of heat, and its connexion with light. He concluded in the following terms:—"It now only remains for me to present to Professor Forbes the medal which has been awarded to him for these discoveries. I believe that I shall be joined cordially by every member of the Society who now hears me, in the fervent wish that it may be the will of the Almighty Ruler, that his life may be long protracted, with vigour of mind and health of body to pursue the career in which he has made an advancement so honourable to himself, and reflecting lustre upon those great establishments, the University and the Royal Society, with which he is connected. I cannot doubt that he will persevere in this happy path with the same ardour and success which have hitherto accompanied his researches. Indeed, we have a gratifying proof that his zeal will not be impaired, nor his success less brilliant, from the discovery in the same field announced by him at the last meeting of the Society, of the Circular Polarization of Heat*."

CAMBRIDGE PHILOSOPHICAL SOCIETY.

(Continued from p. 80.)

Feb. 22.—A paper was read by Mr. Kelland, of Queen's College, "On the application of the hypothesis of finite intervals to the explanation of the phænomena of dispersion." The object of this paper was to show, that by supposing, as M. Cauchy has done, the distance between two consecutive particles of the medium of light to bear a finite ratio to the length of wave, the phænomena of dispersion are satisfactorily accounted for. Numerical calculations are entered into for the purpose of verifying the formula in all the cases which M. Frauenhofer has examined. The fact that a star appears to us as a point, and not a spectrum, compels the author to the conclusion that the medium of light is more dense *in vacuo* than in refracting media, a conclusion in opposition to generally received opinions. It is also a consequence of the above circumstance, as applied to the author's formula, that the forces which the particles exert on each other follow the law of the *inverse square of the distance*, and also that the vibrations must be transversal. The author added, that by the formula he had investigated, a marked difference was found in the results when applied to M. Frauenhofer's seven solids and three

* This discovery is announced in the Proceedings of the Society for Feb. 1, 1836, in the following terms:—"Professor Forbes verbally communicated to the Meeting, that he had succeeded in proving the Circular Polarization of Heat, whether accompanied or unaccompanied by Light, when polarized heat is made to undergo two total reflections within a rhomb of rock-salt; the plane of total reflection being inclined 45° to the plane of primitive polarization."

Prof. Forbes also announced his discovery in our Number for March last, at p. 248 of the present volume.—Edit.

fluids; for the former a particular function of the *forces* was always *negative*—for the latter always *positive*; which remarkable circumstance the author thinks will lead to the most important consequences in the theory of *molecular actions*.

The Rev. Mr. Whewell made some remarks on the present state of our knowledge of the tides. He stated that recent researches have completely changed the position of this subject; observation is now in advance of theory, as, a little while ago, theory was in advance of observation. It has been shown that the inequalities depending on the moon's hour of transit, declination, and parallax follow with great exactness the laws resulting from the hypothesis of a spheroid of equilibrium, slightly modified. In addition to this, it has recently been discovered that the diurnal inequality of the tides agrees in general circumstances with the equilibrium hypothesis, and that there is a solar inequality also agreeing with the same hypothesis. The observer may now, therefore, call upon the mathematician to investigate the result of some theory agreeing more nearly with the state of the case than those of Bernoulli and Laplace, and thus to bring the calculation into accordance with the observed quantities. It was remarked further, that this must be solved as a problem of hydrodynamics, not of hydrostatics; but that it does not appear likely that a satisfactory solution will be obtained, except we take into account the retarding forces, as well as the attractive forces and the condition of perfect fluidity. This being almost the only mechanical problem yet unsolved, which is requisite for the completion of the theory of universal gravitation, was put forward as a subject well worthy the attention of mathematicians.

March 7.—Mr. Whewell gave an account of the recent discoveries made by Prof. Forbes, and other philosophers, with respect to the polarization of heat. He stated that Prof. Forbes had recently obtained an additional confirmation of this discovery, by finding that heat, by two internal reflexions in a rhomb of rock-salt, resembling Fresnel's rhomb, becomes circularly polarized under the same circumstances as light. It was also mentioned that Biot and Melloni have very recently ascertained that heat acquires circular polarization by transmission along the axis of a crystal of quartz.*

The Rev. Mr. Willis then explained his views respecting the composition of the entablature of Grecian buildings. He observed that this feature in the architecture of Egypt consisted of two members, arising from the mode there adopted of roofing a building with beams of stone, resting on the pillars, and supporting transverse slabs. The upper member being resolved into two, the three divisions of architrave, frieze, and cornice were produced; and the portion of the mass which belongs to each of these members may be determined by observing in what manner they are managed when the entablature is resolved into parts by cross-trabeation. It appears in this way (and also by the principles which Vitruvius implies in giving his rules) that each member consists of a vertical face capped by

* See the report of proceedings of the Royal Society of Edinburgh, in the preceding page.

some projecting mouldings; the term *cymatium* denotes this group of mouldings in all cases; and not, as has hitherto been supposed, a particular form of moulding. The entablature in the simplest cases consists of *architrave*, *frieze*, *corona*, each with its *cymatium*, and the *sima* above; in more complex cases there are inserted also the *denticulus*, and the *modillion-band*, each of which has likewise its cymatium.

March 21.—A memoir was read by S. Earnshaw, Esq., of St. John's College, "On the Integration of the Equation of Continuity of Fluids in Motion;" also a memoir by Professor Miller on the Measurement of the Axes of Optical Elasticity of certain Crystals. This memoir contained various determinations, from which it appears that the law concerning the connexion of the crystalline and the optical properties of crystals suggested by Professor Neumann, namely, that the optical axes are the axes of crystalline simplicity, is false; but that it is true, in many of the cases hitherto examined, that one of the optical axes coincides with the axis of a principal crystalline zone.

Afterwards Mr. Webster, of Trinity College, made some observations on the periodical and occasional changes of the height of the barometer, and on their connexion with the changes of temperature arising from the seasons and from the condensation of aqueous vapour.

CAMDEN LITERARY AND PHILOSOPHICAL INSTITUTION.

January 26th.—A very perfect specimen of the *Ornithorhynchus paradoxus* was shown to the meeting, and its peculiarities described.

Mr. Saxton exhibited a very ingenious and simple piece of machinery, by which the rolling of a ship labouring in a heavy sea was perfectly imitated.

Mr. J. de C. Sowerby, F.L.S., in laying before the members some cases of fossil shells from the London clay, a recent donation to the Institution, suggested a plan for the advantageous arrangement of fossils in reference to the strata in which they are found; and presented a specimen of an undescribed fossil Nautilus from the green sand.

Mr. Wilson addressed the meeting on the characters of two fine skulls of the African* Orang Outang. By reference to the skulls of other animals he pointed out the comparative peculiarities of the head for the accommodation of the senses of sight, smell, hearing, and taste. The extraordinary development of the teeth and jaws in the African* Orang, in harmony with the nature of its food,

* Mr. Wilson seems inadvertently to have transposed these local designations: the Chimpanzee (*Troglodytes niger*, Geoff.) is the *African*, and the ordinary Orang Outang (*Simia Satyrus*, Auct.) the *Asiatic* animal; the specimen of the former recently living in the menagerie of the Zoological Society, was brought from the Gambia coast. See our last volume, p. 161, also p. 72; and vol. vi. p. 457. Should the subject require further explanation, perhaps Mr. Wilson will have the goodness to supply it.—E. W. B.

and serving also as instruments of offence and defence, gave rise to the necessity for immense spines and ridges for the attachment of the muscles; impressing on the animal an aspect of ferocity. The skull of the female was more smooth, the prominences less produced; characteristic of the milder nature of the animal. In some of the lower monkeys, viewed horizontally, the bulk of the face concealed entirely the arch of the skull; as the animals assumed a more docile disposition the vault of the skull rose gradually, until a forehead of considerable dimensions was perceived. The skull of the male was narrow above, broad and expanding at the base, and obtusely flattened behind; evincing a destructive and ferocious disposition, and the absence of regard for offspring. In the female, the arch of the skull was broad above, narrower below, and lengthened out behind; displaying less ferocity of disposition, more circumspection, and tenderness for offspring.

Compared with the Chimpanzee or Asiatic* Orang at present in the Zoological Gardens, these skulls, although much larger in size, were very inferior in perfection of development to that animal.

Mr. Sowerby then read a paper upon the "Habits of the *Plecotus auritus*," the Long-eared Bat, which was followed by an interesting discussion. The paper itself was inserted in our last Number, p. 265.

LXXV. *Intelligence and Miscellaneous Articles.*

Views on Scientific and General Education, *applied to the proposed System of Instruction in the South African College.* By Sir John F. W. Herschel, M.A., F.R.S., &c.

It is with great pleasure that we observe from the local publications of the British Colony in Southern Africa, that in that distant region, as in his own country, Sir John Herschel,—while devoting his main attention and energy to the advancement and extension of that branch of Astronomy of which his revered father and himself may be considered at once the founders and to a very great extent the finishers also,—yet directs his powerful and accomplished mind to more general objects, and especially to the improvement of Education, and the application to that purpose of the resources derivable from the most recent advances which science and literature have made. The following letter addressed by Sir John to the Rev. Dr. Adamson, relative to the proposed scheme of instruction in the South-African College, will prove we think as interesting to our readers as we have found it, and it will amply justify the remarks with which we have now introduced it to their attention.

A good practical system of public education ought, in my opinion, to be more real than formal; I mean, should convey much of positive knowledge with as little attention to mere systems and conventional forms as is consistent with avoiding solecisms. This principle, carried into detail, would allow much less weight to the study of languages, especially of dead lan-

* See the note in the preceding page.

guages, than is usually considered its due in our great public schools, where, in fact, the acquisition of the latter seems to be regarded as the one and only object of education. While on the other hand it would attach great importance to all those branches of practical and theoretical knowledge whose possession goes to constitute an idea of a well-informed gentleman, as, for example—a knowledge of the nature and constitution of the world we inhabit—its animal, vegetable, and mineral productions, and their uses and properties as subservient to human wants. Its relation to the system of the universe, and its natural and political subdivisions; and last and most important of all, the nature and propensities of man himself, as developed in the history of nations and the biography of individuals; the constitutions of human society, including our responsibilities to individuals and to the social body of which we are members. In a word, as extensive a knowledge as can be grasped and conveyed in an elementary course of the actual system and laws of nature both physical and moral.

Again, in a country where free institutions prevail, and where public opinion is of consequence, every man is to a certain extent a legislator; and for this his education (especially when the Government of the country lends its aid and sanction to it) ought at least so far to prepare him, as to place him on his guard against those obvious and popular fallacies which lie across the threshold of this as well as of every other subject with which human reason has anything to do. Every man is called upon to obey the laws, and therefore it cannot be deemed superfluous that some portion of every man's education should consist in informing him what they are. On these grounds it would seem to me that some knowledge of the principles of political œconomy—of jurisprudence—of trade and manufactures—is essentially involved in the notion of a sound education. A moderate acquaintance also with certain of the useful arts, such as practical mechanics or engineering—agriculture—draftsmanship—is of obvious utility in every station of life;—while in a commercial country the only remedy for that proverbial short-sightedness to their best ultimate interest which is the misfortune rather than the fault of every mercantile community upon earth, seems to be, to inculcate as a part of education, those broad principles of free interchange and reciprocal profit, and public justice, on which the whole edifice of permanently successful enterprise must be based.

The exercise and development of our reasoning faculties is another grand object of education, and is usually considered, and in a certain sense justly, as most likely to be attained by a judicious course of mathematical instruction—while it stands if not opposed to, at least in no natural connexion with, the formal and conventional departments of knowledge (such as grammar, and the so-called Aristotelian logic). It must be recollected, however, that there are minds which, though not devoid of reasoning powers, yet manifest a decided inaptitude for mathematical studies,—which are *estimative* not *calculating*, and which are more impressed by analogies, and by apparent preponderance of general evidence in argument than by mathematical demonstration, where all the argument is on one side and no show of reason can be exhibited on the other. The mathematician listens only to one side of a question, for this plain reason, that no strictly mathematical question *has* more than one side capable of being maintained otherwise than by simple assertion; while all the great questions which arise in busy life and agitate the world, are stoutly disputed, and often with a show of reason on both sides, which leaves the shrewdest at a loss for a decision.

This, or something like it, has often been urged by those who contend against what they consider an undue extension of mathematical studies in our Universities. But those who have urged the objection have stopped short of the remedy. It is essential, however, to fill this enormous blank in every course of education which has hitherto been acted on, by a due provision of some course of study and instruction which shall meet the difficulty, by showing how valid propositions are to be drawn, not from premises which virtually contain them in their very words, as is the case with abstract propositions in mathematics, nor from the juxtaposition of other propositions assumed as true, as in the Aristotelian logic, but from the broad consideration of an assemblage of facts and circumstances brought under review. This is the scope of the Inductive Philosophy—applicable, and which ought to be applied (though it never yet has fairly been so) to all the complex circumstances of human life; to politics, morals, and legislation; to the guidance of individual conduct, and that of nations. I cannot too strongly recommend this to the consideration of those who are now to decide on the normal course of instruction to be adopted in your College. Let them have the glory—for glory it will really be—to have given a new impulse to public instruction, by placing the *Novum Organum* for the first time in the hands of young men educating for active life, as a text book, and as a regular part of their College course. It is strong meat, I admit, but it is manly nutriment; and though imperfectly comprehended, (as it must be at that age when the college course terminates,) the glimpses caught of its meaning, under a due course of collateral explanation, will fructify in after life, and like the royal food with which the young bee is fed, will dilate the frame, and transform the whole habit and œconomy. Of course it should be made the highest book for the most advanced classes.

Among branches of knowledge purely formal, language of course stands foremost. Its importance is doubtless great as the key to the depositories of knowledge, and as the most powerful instrument of human reason. Of course it must form an essential part of every system of instruction. But it should be studied as a means and not as an end. The books chosen in every language (after its first rudiments are acquired) ought to be vehicles of other than mere verbal instruction, and the attention of the pupil ought to be much more strongly directed to the matter than to the words. Indeed, a foreign tongue can never be said to be in fair train of being mastered, till the sense is seized and the words begin to pass unheeded. Much of course will depend on the tact of the teacher in determining the point where the strictness of literal construction may be relaxed or altogether abandoned, and fluent translation substituted for it. And here I would incidentally remark, how infinitely preferable a close written translation is to any oral construing. A boy should come up "to construe" with his written, or even—in the case of beginning—his printed, translation in his hand; he should read it aloud, and then be called upon to prove by literal construction that such *is* the true sense of the passage. Thus and thus only can we be sure that the sense has not escaped him in the turmoil of words and rules, which it is to be feared is too often the case in the usual method. As for composition, or even translation from the vernacular into a foreign tongue, till the point of fluent construing or translation at sight is attained, I consider it as time mispent*. The usual practice at schools of setting boys who know nothing, or next to nothing, of Latin, to write Latin exercises, has always appeared to me a mere waste of their own and their master's time. One hour spent in acquiring a fluency of rendering at sight is worth a week of such unnatural effort.

* [On this point we venture to express our dissent. We are inclined to think that these two species of exercises, simultaneously practised, assist and test each other.—R. T.]

So soon as any of the pupils, in the opinion of the masters, shall have acquired such a degree of proficiency in a foreign or dead language that it can be done with advantage, I should be disposed to recommend, in pursuance of the principle above laid down, that its study *as a mere language* should be abandoned, and that such proficients as a distinction and a reward, should be drafted into a separate class, and commence the study of some *subject* competent to their age, in that language. This would secure one material advantage, viz. that, in pursuance of a *subject*, a much greater quantity of the half-acquired tongue can be made to pass through the channels of the mind than in the mere conning over of stated passages as exercises, and that a familiarity is thereby acquired with its forms and idioms which can never be attained by the study of rules, or by any assiduity in construing and parsing. Historical works, as exciting the attention, following out a connected story, and requiring the perusal of many pages at a sitting, seem particularly adapted to this purpose. Those of Livy, Cæsar, and even Tacitus, in the Latin; of Schiller in German; and the spirited biographies of Charles and Peter, by Voltaire, in French, may be taken as exemplifying the proposed method.

In this colony, and more especially in Cape Town, two languages are habitually spoken among those classes who may be expected to send their sons to college, and a question may arise which of those should be taught in the vernacular language of the. country, and made the vehicle of instruction in the college. As to the latter point, convenience, of course, must be consulted. It would cripple the institution of half its power to carry on two distinct courses of tuition, under masters exclusively English and exclusively Dutch, besides being otherwise mischievous. Probably no parent would be found so culpably negligent of his child's future comfort and advancement, as to allow him to attain the age of admission entirely ignorant of English. Such entire ignorance ought, I think, to operate as a bar to admission. Considering also that this is, and will in all human probability remain for centuries to come, a British possession; that communications with Britain are constant and increasing; British settlers arriving yearly, and British habits gaining ground, I should conceive that, *cæteris paribus*, so far as can be done without sacrificing what is more important, a preference should be given to the English language as the medium of oral communication, and in the choice of elementary books.

But whether the acquisition of a critical knowledge of either of these languages should be made a feature in the course of instruction, is another question. For my own part I think not, being of opinion that youths should occupy their time at school or in college in learning that which they have not opportunity or means of learning elsewhere, and that provided bad grammar and vulgar expressions are corrected and reprobated whenever they occur—in speech or in writing—no other express provision for learning any language in ordinary use in the country is needed. In fact, however, neither the English nor the Dutch languages can be critically studied without an acquaintance, in the latter case with the German, in the former with both that language and the Latin. A knowledge of the original meaning and mode of derivation of words is of far more importance than that of mere idiom and grammatical nicety, and in this view, as well as by reason of the vast intrinsic utility of the languages themselves, I would strongly urge the propriety of *making both the last-mentioned languages essential parts of the regular College course*, and as such, to be taught indiscriminately to all the pupils, superadding French as highly desirable; but leaving it optional with parents, and loading it with an extra payment. I should hardly think it worth while to have a Greek class, though a small vocabulary of Greek words (in the Greek character) consisting of those whose derivatives have been introduced directly into our terms of art and science (without passing through the Latin) would be no doubt useful.

I confess I do not see any valid reason for deferring the study of Latin till an advanced period. All languages are easiest learnt early, nor am I aware (when artificial difficulties, such as committing to memory the Eton Grammar, &c. are discarded,) that the Latin is more difficult to acquire than any modern language. The known fact of the readiness with which children acquire languages, as well as the degree in which the knowledge of words, both in children and in grown up persons, is often in advance of their acquaintance with their import, may, I should hope, induce you, my dear Sir, to reconsider your position, that the acquisition of general information is so far a necessary or advantageous preparation for that of languages as to render it desirable to postpone the latter in point of time till the former is attained.

Of the purely abstract departments of study, I shall say little, as I do not see how the mathematical course actually established in the college can well be amended, except in so far as the introduction of new branches of physical science into the course of instruction, would naturally lead to a greater development and detail of its applications, to those subjects which admit them in a form not too difficult—at the expense, perhaps, of some sacrifice of more abstruse and technical points.

In what is said I would not be understood as advocating a merely utilitarian course of instruction. Something must be conceded to ornament and elegance. The influence of a tincture of elegant literature, early imbibed, on the tastes and habits of after life is far too important to be lost sight of. The charms of well-chosen poetry, for instance, learnt in youth, take so strong a hold on the imagination, and connect so many pleasing associations with the memory of youthful studies, that it would be a very erroneous system which would banish them as superfluous. Still the selection should be cautiously made, with reference to the matter as well as to the language. It is not easy to say on what defensible grounds the feeble Pastorals of Virgil, or the whining love-letters and wild extravagancies of Ovid, are generally selected as the avenues by which the temple of the Latin Muse is to be approached, when there is quite easy Latin for the beginner, joined with pleasing narrative and far loftier and more poetical diction to be found in the Æneid, or made the vehicle for the soundest good sense, the noblest sentiments, and the most sterling wit in Horace. But the consideration of these subjects would lead to a dissertation on classical literature. I will only observe that neither in the study of the German nor the Latin languages would I begin with poetical works.

In advocating so considerable a range of instruction as I have done, it may be reasonably asked—how is it to be accomplished?

Without descending into a detail of each year's work, or of the proportion in which the several items are to be distributed among the limited number of professors whom the funds of the Institution will support, I would observe, that in many of the subjects proposed, a very limited and extremely elementary course only is contemplated, and in some a true statement of their scope and fundamental principles in the form of an occasional lecture, might suffice. For example, the course of political œconomy might be confined to the reading of a single elementary volume of moderate extent, such as, for example, the admirable 'Conversations,' by Mrs. Marcet. In Ethics, a subject of chief importance, some standard work (such as Paley's Moral Philosophy,) might be distributed over time so as to pervade the whole duration of each pupil's frequenting the institution. For the study of natural history, the proximity of the Museum offers great advantages. An occasional visit to that collection would form an excellent comment on whatever outline of animated nature might be put into the hands of the junior classes. The best mode of disposing of the subject of jurisprudence would perhaps be by lecture, but on a very limited scale. A few lectures also on the useful arts—engineering and manufactures, might, perhaps, satisfy all the requisites of the occasion.

Drawing should, of course, be taught by a drawing-master, and paid for as an extra; but the principles of perspective should be included in the course of geometry. The physical sciences—those especially which most require experimental elucidation (as all do, more or less), could hardly be taught adequately otherwise than by a regular course of lectures. As a single elementary compendium of physical science, I know nothing comparable to the "Physics" of Dr. Arnott; but without the elucidation which experimental lectures afford, the study of this, or any other work must be insufficient to communicate distinct and satisfactory notions. No provision, however, (I believe,) exists for any such course, and as no one can be expected, or indeed ought, in justice, to be suffered to perform so extensive a task gratuitously, there is no course open but one of the following, or a combination of them all:

1st, To establish one or two lecturing professorships, with salaries from the funds of the institution;

2ndly, To provide for their support by fees from the pupils;

3rdly, To apply to the public for support by subscription;

And, lastly, to apply to Government for assistance.

That any, or all of these modes, independent of the last, would prove permanently sufficient, is much to be doubted. But no worthier or more truly useful application of a portion of the public treasure than for the maintenance of a high standard of education, in at least one point, the metropolis of the colony, can be imagined—supposing such an application made, and successful. The professor or professors, being APPOINTED and SALARIED by Government, it would devolve upon the resident masters of the college to *enforce* the attendance of their classes (for which no payment should be required), to aid their progress by a course of reading, prospective and retrospective, and to estimate their proficiency by public and private examination.

But in that case I would by no means confine the benefit of the lectures within the walls of the institution. The doors of the lecture-room should be thrown open, not only to the pupils, but to the public in general, on payment of a small fee in aid of the professor's salary. This would have several highly beneficial effects: 1st, The augmentation of his income would be a motive to the professor to render his lectures intelligible and attractive. 2nd, It would afford an opportunity to many adult persons, tradesmen and others, to acquire knowledge of a kind which must be useful to themselves, and have a direct tendency to develope the internal resources of the colony. 3rdly, It would probably furnish to many an attractive counteractive of intemperate and idle habits, which mainly grow out of the absence of some object of interest enough to engage the attention. 4thly, It would afford to parents and relations of the pupils an authorized and no way invidious opportunity of witnessing in person the actual process of instruction to which they are subjected. Lastly, but not of least importance, should any unforeseen circumstance, such as want of funds, occur, to suspend for a time, or permanently to cripple the efficiency of the institution itself, the lecturing professors being entirely or chiefly supported from without, and *independent as* (in this view of the subject) *they would be* of its *internal arrangements*, would still continue to perform their duties, so that the public instruction, though grievously wounded (as it must be, by any event, so much to be deprecated) would not be entirely annihilated, and a rallying point would always be preserved for a reconstruction of a more extended system, whenever the necessary means should be forthcoming.

I will here recapitulate the heads of the several branches of instruction I have above endeavoured to recommend.

LANGUAGES.—Latin and German, Greek Alphabet and Vocabulary,—French, *extra.*

HISTORY.—1. Ancient Greek, Roman (Jewish ?).
2. Modern—chiefly those of England and Holland; European and General in less detail.

NATURAL HISTORY.—1. General subdivisions of Organic nature.
2. Particular History of the more remarkable Animals and Vegetables.

GEOGRAPHY.—1. Political—Ancient and Modern.
2. Physical—1. Form of the Earth.—2. Traces of its former condition.—3. Natural subdivisions.—4. Climates.—5. Atmosphere. Winds. Seas. Tides.

PHYSICAL SCIENCE.—Mechanics, including Hydrostatics, &c. Astronomy. Chemistry. Optics, &c.

N.B. The climate is remarkably favourable for Optical Lectures, which might be splendid and most attractive.

USEFUL ARTS.—Engineering, including the nature of the Steam Engine. Agriculture and Horticulture. Draftsmanship (extra).

SOCIAL RELATIONS.—Ethics. Jurisprudence. Political Economy.

MATHEMATICS.—Arithmetic, Geometry, Analysis, Applications.

INDUCTIVE PHILOSOPHY.—*Novum Organum* of Bacon, omitting his specimen of the application of his own principles to the Nature of Heat.

A few brief remarks on the subject of public examinations may not be irrelevant, and I should certainly not have hazarded them had I not been requested by you to state my impressions as to what may prove of benefit to the objects of the institution prospectively; and it is in the spirit of that request, and without the slightest wish to criticise anything which I have observed in the only examination at which I have had the honour to be present, that I do so.

First, then, I think it would be desirable that some portion of the examination of the senior classes should be conducted in writing, and with deliberation, not only in mathematics but on other subjects. From what I have been in the habit of observing in such matters, I am disposed to think that a combination of written with oral answers, is necessary to give an effectual trial to the merits of any proficient.

In the next place, I would suggest, that the number and variety of prizes given may quite as easily be too great as too small, and that a certain reserve on this point is essential to keeping up the value of such distinctions in general.

Lastly, I should be disposed to suppress altogether a practice which I have observed to exist, of the successful candidates for prizes returning thanks to their judges. There is no distinction which can possibly be awarded to a youth at college which ought not to have the immediate effect of humbling him in his own sight, and inducing him to retire in silence and meditation on the share which his own good fortune, or the ill-luck or diffidence of his competitors may have had in his success—on the numbers of questions which might have been proposed to him, and which he could not have answered, and on the immeasurable interval which still separates him from excellence—as well as in forming inward resolves, to let his future exertions be greater than his past. Such a frame of mind is incompatible with any kind of public declamation.

I remain, dear Sir, yours, with much esteem,

J. F. W. HERSCHEL.

ON THE AURORA BOREALIS OF NOVEMBER 18TH, 1835, AS WITNESSED AT COLLUMPTON IN DEVONSHIRE. BY N. S. HEINEKEN.

To the Editors of the Philosophical Magazine and Journal of Science.

GENTLEMEN,

In the Number of the Philosophical Magazine for this month (February) there appears to be a mistake in the date given by Mr. Sturgeon for the occurrence of the Aurora Borealis in November last, and that we should read the 18th instead of the "16th of Nov. 1835." If such should be the case, allow me to state that my attention was called, by a friend, to the same phænomenon on the evening of the 18th of November. The aurora was seen here about a quarter before nine o'clock, but I did not observe it until half-past, at which time it presented precisely the appearance described by Mr. Sturgeon. Waves of light appeared to roll, in rapid succession, from the horizon to the zenith, which were succeeded by columns, sometimes of a yellowish and at other times of a lilac tint. The light of the aurora was sufficient to produce a shadow upon a white wall, and to enable me to ascertain the hour by my watch. Several meteors were seen; one of these, at nearly fifteen minutes before ten o'clock, had almost the brilliancy and apparent magnitude of Jupiter. It passed from towards the north to the west. The duration of its course did not much exceed a second, and it left a train of reddish-coloured sparks, the length of which appeared to be equal to one half of the space passed over. Although I listened attentively, I heard no explosion. The state of the thermometer for the preceding day was, max. $= 52°$, min. $= 44°$. For the 18th, max. $= 51\frac{1}{2}°$, min. $= 34°$. The depression in the temperature took place after the appearance of the aurora, for at ten o'clock that night the thermometer stood at 42°. I am, yours, &c.

Collumpton, Devon, Feb. 3, 1836. N. S. HEINEKEN.

[We have annexed to the notice of a paper by Mr. Christie in our report of the proceedings of the Royal Society, at p. 413 of the present Number, references to other communications relative to this aurora.—EDIT.]

LIEUT. LECOUNT'S REPLY TO MR. BARLOW.

We have received a letter from Lieut. Lecount, informing us that he has published a pamphlet in reply to Mr. Barlow's letter in our last Number, p. 291. He states the following as the points at issue between them; (and our readers will have an opportunity of judging how far he has succeeded, by a perusal of his reply, which is advertised on the wrapper of our present Number.

"Mr. Barlow has called an ellipse, which vanishes with respect to depth at one end, a fishbellied rail; and has asserted that it deflects 4, when a parallel rail deflects 3. I have shown that it is the parallel rail which deflects 4, while the fishbelly only deflects 3.

"Mr. Barlow asserts 10 tons to be the longitudinal extension of iron. I assert that his own experiments only show 9 tons.

"Mr. Barlow asserts the neutral axis in rectangular bars to be as 1 to 4. I assert that with his own formula and his own experiments it is as 1 to 10.—Mr. Barlow in another mode of calculating it for railway-bars gives it as 1 to 9. I say that his own experiments and his own formulæ show that it is as 9 to 1.

"Mr. Barlow assigns 7 tons as the strength of certain rails. I say his own formulæ will only give half that strength.

"Mr. Barlow asserts that the deflection of a rail is the same at all velocities of the engines. I assert that it is not."

BOTANICAL SOCIETY OF EDINBURGH.

We rejoice to observe that a Botanical Society has been established in Edinburgh. At a meeting which took place on the 17th of March, the Society was constituted, under the title of "The Botanical Society of Edinburgh,"—the meetings to be held on the second Thursday of every month, from November to July inclusive.

Professor Graham has been elected President, and Drs. Greville and Balfour Vice-Presidents of the Society for the present year.

The advancement of Botanical Science is the object of the Society. Its operations will for some time be confined principally to the holding of periodical meetings, to correspondence, to the formation of an herbarium, and the interchange of specimens. The last is a new feature in the constitution of such a Society, and will be conducted by a committee, in accordance with certain rules embodied in the laws. The desiderata of botanists in all parts of the kingdom will be supplied, as far as possible, from the Society's duplicates, and individuals will secure the important advantage of exchanging the botanical productions of their respective districts for those of others more remotely situated. The benefits resulting to science, as well as to individuals, by this arrangement, will it is hoped be considerable; especially in regard to the Geographical Distribution of Plants in the British Islands and the formation of Local Floras. The Society, besides, contemplates an extension of this plan by promoting an exchange of specimens with botanists in other parts of the world.

The members will be divided into the following classes:—Resident, Non-resident, Foreign, and Associate. Any person wishing to become a non-resident member must be recommended by two individuals belonging to some scientific or literary Society, and pay a contribution of two guineas, which, without any additional payment, will entitle him, as long as he continues annually to send specimens to the Society, to a participation in the duplicates. To become a Foreign Member, it is necessary to transmit 500 specimens, including at least 100 species, or a botanical work of which the candidate is himself the author,—the former alternative, only, entitling him to a share of the Society's duplicates. To continue to participate in these duplicates, he must afterwards contribute annually 300 specimens, including at least 50 species.

The Flora of Edinburgh, which is particularly rich, will afford a constant supply of valuable duplicates, and others will be regularly obtained from other parts of Scotland,—especially the rarer alpine species.

Local Secretaries will be appointed in different parts of the kingdom. In the mean time all communications are to be addressed (postage paid) to the Secretary, W. H. Campbell, Esq., Botanical Society, 21 Brown's Square, Edinburgh.

INQUIRY RELATIVE TO DR. PEMBERTON'S TRANSLATION AND ILLUSTRATIONS OF NEWTON'S PRINCIPIA. BY PROFESSOR RIGAUD.

In the New Memoirs of Literature* for March, 1727, there is advertised, as speedily to be published, "Sir Isaac Newton's Mathematical Principles of Natural Philosophy, translated from the latest edition, with a Comment by H. Pemberton, M.D. F.R.S." In this notice the author says, "I having had a very particular opportunity of being fully informed of his real mind from his own mouth, do intend to proceed in my design with all expedition; wherein I shall present the public with such a translation of Sir Isaac Newton's words as shall comprehend in the fullest manner I am able his true sense. And besides many other occasional remarks, I shall illustrate at large the meaning of the difficult passages by explanatory notes, and shall demonstrate in form those numerous corollaries and scholiums which he, for brevity, has set down without proof." The work is specifically mentioned as intended "for the use of mathematical readers," to distinguish it from the popular "View of Sir Isaac Newton's Philosophy," which Pemberton then had in the press. This came out in 1728, with a preface containing many curious particulars respecting Newton, towards the end of which it is said, "As many alterations were made in the last edition of the Principia, so there would have been many more if there had been a sufficient time. But whatever of this kind may be thought wanting I shall endeavour to supply in my Comment on that book. I had reason to believe he expected such a thing from me, and I intended to have published it in his lifetime. . . . . This Comment I shall forthwith put to press, joined to an English translation of his Principia which I have had some time by me."

Dr. Pemberton died in March, 1771, and in the same year his Course of Chemistry was published, by his old friend Dr. James Wilson. The editor prefixed a biographical preface, from which we learn that Motte's Translation of the Principia, which came out in 1729, put a stop to Dr. Pemberton's intention. Indeed, he expressed in his advertisement the fear of being, in this manner, anticipated in his design, and it is to be regretted that his fears were realized. It is best, certainly, when the reader is able, for him to study the original, and to a mathematician there is no great difficulty in mathematical Latin; but even if a complete edition in English, executed in a manner worthy of such a work, be not considered as a desideratum in British literature, a comment like Pemberton's must

* Vol. v. p. 239.

in all probability have contained much that was valuable. In the Abridgement * of the Philosophical Transactions Dr. Hutton has given an account of Pemberton, and says that "after his death many valuable pieces were found among his papers." In the enumeration of them we find "A Comment on an English Translation of the Principia." This appears to describe the work in question; but although the account is almost wholly taken from Dr. Wilson's Memoir, the original does not express the fact quite so strongly; it only says †, "The Doctor advertized he would publish a Comment on an English Translation of the Principia, and I find in his copy a great number of papers written for that purpose." This seems to indicate that the Comment had not been completely arranged; but at the same time it gives every reason to conclude that materials for it had been collected. It is not impossible that the manuscripts may yet be in existence; and if they are, the best way of bringing them to light appears to be by recalling the attention of the scientific world to the circumstance.

Dr. Pemberton's will was executed August 7, 1769; in it he bequeaths his printed books to Dr. Wilson; but his papers must have been included in the residue of his property, all of which he left to Mr. Henry Miles, whom he describes as a timber-merchant at Rotherhithe‡. This gentleman married Dr. Pemberton's niece, by whom he had "two sons, both of age and in perfect health and strength §," [1771.] If any readers of the Philosophical Magazine should be acquainted with their descendants something might probably be learned from them.

It is well known that Dr. Pemberton undertook the publication of the third edition of the Principia. Newton entertained so high an opinion of his talents "that he even solicited Dr. Mead to prevail on him to assist him" in the work ||; and he was so well satisfied with the care which the editor took in the execution of his task, that, with his accustomed generosity, he nobly rewarded it ¶. This engagement, Dr. Pemberton says **, "obliged me to be very frequently with him; and as he lived at some distance from me, a great number of letters passed between us on this account." It is not likely that these letters should have been destroyed, and if they could be recovered they would form an important addition to our stock of scientific history. The correspondence with Cotes, during the publication of the second edition of the Principia, is preserved in Trinity College, Cambridge ††, and thus we should have the means of following Newton's progress to the completion of his stupendous work.

S. P. R.

* Vol. vi. p. 570.

† Wilson's Preface, p. xvi.

‡ In Manning and Bray's History of Surrey, vol. ii. p. 235, Henry Miles, Esq. is recorded as a subscriber in 1788 of 100*l.* to the charity-school at Rotherhithe.

§ Wilson, p. xxiv.

|| *Ibid.*, p. xiii.

¶ *Ibid.*, p. xiv.

** Preface to View of Newton's Philosophy.

†† Bishop Monk's Life of Bentley, p. 180.

ON SUBERIC ACID AND ITS COMBINATIONS.

Examination of Cork.—M. Chevreul has given the name of *suberine* to cork freed from those substances which can be extracted from it by digestion in water, alcohol, and æther.

Æther digested upon cork acquires a pale yellow colour; this solution affords by evaporation a substance which is deposited in small acicular crystals. This substance resembles a resin, and M. Boussingault has called it resin of cork. Nitric acid converts it into oxalic acid and a substance resembling wax, which M. Chevreul has denominated cerine.

Resin of cork contains

Carbon	0·824	= 32 eqs.
Hydrogen	0·111	= 26 eqs.
Oxygen	0·065	

Suberine partially dissolves in the alkalies; and the alkaline solution affords a brown precipitate on the addition of an acid. The precipitate is converted into suberic acid by treating it with nitric acid.

That part of the suberine which does not dissolve in the alkalies, consists of lignin and a little resin.

It appears most probable that it is the principle soluble in the alkalies, which in the cork gives rise to the production of suberic acid: two facts tend to confirm this opinion, one that M. Chevreul has discovered, that the epidermis of the birch produces a large quantity of suberic acid; and the other, M. John has found that this epidermis is almost entirely soluble in solution of potash.

The results of the analysis of suberic acid by M. Boussingault indicate nearly the same composition as already given by M. Bussy, viz.

Anhydrous acid.			Hydrated acid.		
Carbon	0·612	= 16 eqs.	Carbon	0·557	= 16 eqs.
Hydrogen ..	0·076	= 12 eqs.	Hydrogen ..	0·079	= 14 eqs.
Oxygen....	0·304	= 3 eqs.	Oxygen	0·364	= 4 eqs.

Suberic æther may be prepared by heating a mixture of 4 parts of alcohol, 1 part of muriatic acid, and 2 parts of suberic acid. It is rather heavier than water, of a faint smell, and disagreeable taste. It is colourless, oleaginous, and boils at 450° Fahr. Its composition is

Carbon	0·627	= 24 eqs.
Hydrogen	0·096	= 22 eqs.
Oxygen	0·276	= 4 eqs.

But $C^{24} H^{22} O^{4} = C^{16} H^{12} O^{3} + C^{8} H^{8} + H^{2} O$. Thus suberic æther is subject to the general law which governs the composition of æthers of the same kind.

By distilling suberic acid and lime at a moderate temperature, M. Boussingault has obtained, amongst other products, a volatile oil, which possesses the general properties of essential oils. Its odour is powerful and aromatic. When separated from the hydrocarburets with which it is mixed, it boils at 276·8° Fahr.; it does not become solid at 18° Fahr., and affords by analysis,

Carbon	0·766	= 14 eqs.
Hydrogen....	0·108	= 14 eqs.
Oxygen......	0·126	= 1 eq.

The specific gravity of its vapour ascertained by the method of M. Dumas was found to be 4·392. The formula, $C^{16} H^{14} O$, compared to that of suberic acid, $C^{16} H^{14} O^{4}$, presents a remarkable relation, showing that the essential oil obtained by the action of lime on suberic acid differs from the acid only in containing 3 eqs. less of oxygen: accordingly when this oil is exposed to the air it becomes distinctly acid.

When the essential oil is treated with nitric acid, violent action ensues, and it is converted into suberic acid. It will now be seen that the volatile oil obtained from suberic acid presents a certain analogy to the essential oil of almonds, which MM. Liebig and Wœhler consider as a hydruret of the radicle of benzoic acid. If we suppose the radicle of suberic acid to be $C^{16} H^{12} O$, then the volatile oil, the formula of which is $C^{16} H^{14} O$, may be likewise represented by $C^{16} H^{12} O + H^{2}$; in this case it will be a hydruret of suberyle. The production of a body analogous to hydruret of suberyle by the mode described above is not easily explained. It only appears in a general manner, that under certain influences, an organic acid may be reduced at the expense of its own elements, and may be modified in such a manner that the result of this modification shall be a less oxygenized body, approaching in its nature to the radicle of the acid.—*L'Institut*, Jan. 27, 1836.

PHLORIDZINE.

MM. de Koninck and Stas have discovered a new organic substance in the barks of the apple, pear, and wild cherry, which they call phloridzine, from *φλοιος*, bark, and *ριζα*, root; these chemists having obtained it from the cortical part of the root of these trees. When pure it is of a dead white colour, and commonly crystallized in silky needles; it is very slightly soluble in water, but it increases in solubility by an increase of temperature, dissolving to any extent in water at 212°. Persulphate of iron colours its solution brown, and throws down a yellow precipitate, whilst the protosulphate does not act upon it. Phloridzine may be obtained by boiling the bark in water for 4 or 5 hours, and repeating the boiling for 2 hours. By leaving the solution in convenient vessels for about 36 hours, the phloridzine will be deposited in brown crystals on the sides of the vessel. It may be obtained in larger quantity and in greater purity by digesting the bark with warm alcohol for 7 or 8 hours and distilling the alcohol; by standing for 24 hours the phloridzine will be deposited. Its composition is stated at 14 eqs. of carbon, 9 of oxygen, and 18 of hydrogen. —*L'Institut*, Feb. 3, 1836.

THEBAIA, A NEW ALKALI IN OPIUM.

M. Couerbe discovered this new substance in the solution from which the muriates of morphia and codeia had been separated by Gregory's process. It was separated by its discoverer in the following manner: the mother waters above mentioned were evaporated to the consistence of a syrup; this contains bimeconate of lime, morphia, narceia, meconin, narcotina, and thebaia: muriatic acid is to be added, to separate a black fatty matter containing ulmic acid, which is removed by a skimmer from the surface of the liquid. To the solution thus purified, ammonia is to be added, which occasions a black deposit of morphia and thebaia. This precipitate is to be dried, powdered, and treated with boiling æther, in which the thebaia, though only slightly soluble, dissolves. When the æther is separated by distillation, the thebaia is deposited in small reddish crystals, which are to be purified by boiling in alcohol with animal charcoal. It is then to be dissolved in æther, and by spontaneous evaporation crystals are obtained.

Thebaia, thus prepared, is perfectly white, strongly alkaline, and soluble in alcohol and æther. In the first liquid it crystallizes, like the sugar of grapes, in small mammillated crystals, but in the second, in brilliant flat rhombic crystals. When heated to about 266° it fuses, and does not solidify till its temperature is reduced to 130°; whereas narcotina fuses at 338° and solidifies at 266°. Codeia fuses at 302°, and meconin at 194°. By fusion, thebaia loses 4 per cent., or two equivalents of water. Concentrated acids convert it into a resinous substance, whereas when properly diluted, they combine and form crystallizable salts with it. By friction it becomes negatively electrical.

It is composed, according to M. Couerbe, of

Carbon	71·976	=	25	equivalents	nearly.
Azote	6·385	=	2	do.	
Hydrogen	6·460	=	27	do.	
Oxygen	15·279	=	4	do.	

M. Couerbe gives the following table of the colours produced by agitating the peculiar substances of opium in a bottle with sulphuric acid and air. Nitric acid oxidizes them so rapidly that the progress of the oxidation cannot be followed. The experiment is to be made in a four-ounce phial, with six grains of the substance, with nearly half an ounce of sulphuric acid containing nitric acid: strong agitation is to be employed. At first the colour is not very deep; but it is developed in a few minutes.

Thebaia is rendered instantly red, becoming deeper and deeper by time; when examined in thin portions the colour has a yellowish tint.

Narcotina, at first yellow, and remains so for seven or eight minutes, then becomes red.

Codeia immediately becomes of a very pale green colour, which passes to a *vert-russe* after some time.

Morphia becomes almost immediately of a green colour.

Meconin, no immediate effect, but in 24 hours the mixture becomes of a superb rose colour.

Narceia immediately becomes nearly of a mahogany colour.

When sulphuric acid, which contains no nitric acid, is employed, then

Thebaia gives a rose-colour, with a shade of yellow;

Narcotina, a blood red colour;

Codeia, a green colour;

Morphia, a brown colour;

Meconin, first a turmeric yellow and then red;

Narceia, a chocolate colour.

M. Couerbe obtained from 40 pounds (French) of opium, the following products:

1 ounce of meconin,
$1\frac{1}{2}$ ounce of codeia,
$\frac{3}{4}$ ounce of narceia,
1 ounce of thebaia,
50 ounces of morphia.

The narcotina, which remained in the *marc*, was not extracted.—*Ann. de Ch. et de Ph.*, lix. 136.

NEW RENAL CALCULUS.

There has been recently found in the kidney of a young girl 20 years old, who died of a calculous disorder, several calculi which present some remarkable particulars. The largest of these calculi weighed about 19 grs.; it was rounded, and covered with several excrescences resembling the mulberry calculus. Its composition offers an example not yet noticed of the association of oxalate and carbonate of lime, being composed, according to an analysis of M. Bourchardat, of about 0·4 of oxalate of lime, 0·2 of carbonate of lime, and colouring matter, blood, and loss 0·4. A notable quantity of iron was detected in the organic portion of the calculus.—*L'Institut*, 24 Fev. 1836.

SOLIDIFICATION OF CARBONIC ACID.

M. Thilorier has read to the Academy of Sciences a memoir containing an account of the means by which he rendered carbonic acid solid; and he also gave some details respecting liquid carbonic acid.

He finds the specific gravity of the liquid acid to be ·83, water being 1·; it dissolves in all proportions in alcohol and æther: potassium decomposes it, but the common metals do not. A jet of carbonic acid, directed upon a spirit thermometer, caused it fall to 194°* below zero Fahr. The cold would have been still greater if the bulb of the thermometer could have been entirely covered by the jet.

The solidification of carbonic acid was effected in the following manner: a jet of liquid carbonic acid was received in a glass vial; the expansion which it undergoes is about 400 times its original volume, and by this so intense a cold is produced that one part of the carbonic acid congeals in a white powder and adheres to the glass. This powder exists for some minutes, and without any pressure. If the finger be placed on solid carbonic acid, the heat converts it into gas, the expansion of which repels the finger. A few grains of this powder, closed in a vessel, soon expelled the cork.

Solid carbonic acid contains a little water, which is doubtless derived from the moisture of the air. In order, however, to remove all doubts, it would be necessary to get rid of the hygrometric moisture, both of the air and of the vessels, because it might be supposed that this

* These are lower temperatures than have ever before been artificially produced, and lower also, we believe, than any which have yet been observed in nature.—Edit.

water facilitates the congelation of the acid, as is the case with chlorine.

As to the temperature of this congelation, it was determined by using a spirit thermometer graduated to 187° below zero, to which about 44° must be added for the tube of the thermometer which could not be cooled, so that the cold observed was not less than 231°*.

These experiments were verified by commissioners, among whom were MM. Thenard and Dulong.—*Journal de Chim. Med.*, tome ii. p. 3.

ARSENOVINIC ACID.

M. Felix D'Arcet has found that when arsenic acid is made to act upon alcohol, a new acid, analogous to the sulphovinic and phosphovinic acids, is formed.

Arsenovinate of barytes is composed of

Barium........	27·20
Carbon........	19·21
Hydrogen......	3·33
Arsenic........	15·31
Oxygen	34·95—100·

Arsenovinic acid is stated to be formed of

	Calculated.	Experiment.
C^{16}	25·6	24·93
H^{20}	5·6	4·47
As^{2}	39·4	38·91
O^{7}	29·4—100·	31·69—100·

Journ. de Chim. Med., tome ii. p. 11.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1836.

Chiswick.—March 1. Cloudy: stormy. 2. Fine. 3. Showery. 4. Rain: windy at night. 5. Fine. 6. Rain. 7. Cloudy. 8. Rain. 9. Frosty: fine. 10. Hazy: drizzly. 11. Fine. 12. Stormy and wet. 13. Fine. 14. Stormy and wet. 15. Rain: stormy. 16. Very fine. 17. Cloudy and windy. 18, 19. Hazy: fine. 20. Very fine. 21. Slight haze. 22. Drizzly. 23. Hazy: slight rain: windy at night. 24. Fine. 25. Heavy rain: stormy showers. 26. Cold and windy: slight showers. 27. Frosty: fine. 28. Stormy and wet. 29. Clear and cold: rain. 30. Heavy rain. 31. Stormy showers: clear and windy.

Boston.—March 1. Cloudy: rain early A.M.: rain P.M. 2. Fine: rain P.M. 3. Cloudy. 4. Foggy. 5. Fine: rain early A.M. : rain P.M. 6. Cloudy. 7. Fine. 8. Cloudy: rain early A.M. 9. Fine; rain early A.M. 10. Cloudy. 11. Cloudy: rain P.M. 12. Fine and stormy: rain P.M. 13. Fine: rain P.M. 14. Stormy: rain early A.M. 15. Cloudy: rain early A.M.: rain A.M. 16. Fine. 17. Stormy: rain early A.M. 18, 19. Cloudy. 20. Fine. 21. Cloudy. 22. Fine: rain P.M. 23. Cloudy. 24. Fine: hail-storm P.M. 25. Rain. 26. Fine and stormy. 27. Fine. 28. Rain. 29. Stormy. 30. Cloudy: rain A.M. 31. Stormy: heavy squall with rain and snow A.M.

* See note in the preceding page.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; and by Mr. Veall at Boston.

Days of Month. 1836. March.	Barometer. London: Roy. Soc. 9 A.M.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8¾ A.M.	Thermometer. London: Roy. Soc. Fahr. 9 A.M.	Thermometer. London: Roy. Soc. Self-registering. Min.	Thermometer. London: Roy. Soc. Self-registering. Max.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8¾ A.M.	Wind. London: Roy. Soc. 9 A.M.	Wind. Chisw. 1 P.M.	Wind. Bost.	Rain. London: Roy. Soc. 9 A.M.	Rain. Chisw.	Rain. Boston.	Dew-point. Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
T. 1.	29·013	29·261	28·996	28·94	40·6	35·4	47·0	50	40	39	s. var.	s.	se.		·22	0·02	36
W. 2.	29·285	29·578	29·472	28·94	42·2	38·9	49·4	53	41	42	ssw.	sw.	w.	·194	·02	·28	37
○ Th. 3.	29·477	29·717	29·271	29·27	45·5	40·7	48·4	49	34	37	s.	sw.	calm	...	·12	·04	41
F. 4.	29·532	29·741	29·505	29·35	43·3	40·4	48·7	52	42	35	s.	sw.	calm	·072	·04	...	40
S. 5.	29·314	29·501	29·450	29·04	44·4	42·3	49·4	52	31	42·5	s.	s.	calm	·038	·21	·02	42
⊙ 6.	29·122	29·338	29·192	28·96	43·8	37·7	48·4	49	30	40	ene.	s.	calm	·186	·04	·18	40
M. 7.	29·247	29·445	29·419	29·02	42·2	36·4	48·7	51	30	38	ese.	sw.	calm	...	·01	...	37
T. 8.	29·202	29·518	29·412	29·00	40·3	39·2	42·2	46	30	36	ne. var.	nw.	calm	·033	·15	·06	39
W. 9.	29·287	29·498	29·208	29·13	38·4	33·3	45·5	48	38	35	s.	s.	nw.	·088	·09	...	34
☾ Th. 10.	29·208	29·417	29·205	29·34	43·8	37·2	47·2	50	41	41	s.	sw.	calm	.069	·07	·11	38
F. 11.	28·916	29 238	29·116	29·67	47·7	42·5	51·2	52	41	43·5	sw.	sw.	w.	·052	·07	...	42
S. 12.	29·123	29·377	29·284	28·77	47·6	41·5	51·6	52	39	44	s. var.	s.	w.	...	·14	·10	42
⊙ 13.	29·412	29·673	29·605	29·11	43·0	39·2	50·4	52	43	43	sw.	sw.	w.	·227	·14	·13	40
M. 14.	29·049	29·364	29·234	28·67	50·3	42·2	50·6	48	39	48	sw. var.	sw.	nw.	·166	·47	·24	43
T. 15.	28·891	29·495	29·083	28·67	44·9	40·3	50·7	52	35	40	sw. var.	sw.	calm	·444	·01	·12	41
W. 16.	29·584	29·177	29·736	29·23	41·4	35·3	47·8	50	36	40	sw.	w.	nw.	...	·05	·14	37
● Th. 17.	29·792	30·255	29·991	29·37	48·3	38·7	52·2	53	49	47·5	s. var.	sw.	w.	·061	·01	·07	40
F. 18.	30·322	30·404	30·305	29·74	52·2	47·7	59·4	62	43	51	sw.	w.	calm	...	...	...	47
S. 19.	30·245	30·293	30·142	29·77	47·8	45·5	61·6	69	37	47	ene.	s.	calm	...	...	...	47
⊙ 20.	30·182	30·226	30·182	29·63	53·3	47·2	64·4	69	44	50	sw.	sw.	calm	...	...	...	49
M. 21.	30·113	30·136	29·987	29·46	48·2	46·7	54·2	52	41	51	sw.	sw.	calm	...	...	...	48
T. 22.	29·922	29·943	29·916	29·33	50·8	48·2	52·4	52	35	52	s.	sw.	w.	...	·02	...	49
W. 23.	29·720	29·755	29·561	29·25	48·2	42·3	50·7	51	38	42	s.	sw.	calm	·047	·08	·03	45
Th. 24.	29·544	29·568	29·523	29·04	44·6	39·5	48·7	54	40	44	s. var.	sw.	w.	·050	·11	...	47
☽ F. 25.	28·938	29·195	28·980	28·50	47·7	41·3	51·3	51	36	42·5	s. var.	w.	calm	·105	·07	·20	45
S. 26.	29·239	29·513	29·254	28·73	42·6	37·7	47·6	47	26	41	sw. var.	s.	nw.	·044	·02	·10	37
⊙ 27.	29·556	29·873	29·321	29·14	39·6	31·8	48·2	51	35	40·5	s.	s.	nw.	...	·37	...	38
M. 28.	28·662	29·103	28·655	28·52	40·2	36·2	45·5	45	36	40	e.	e.	ne.	·244	·34	·07	40
T. 29.	29·548	29·784	29·560	29·04	42·3	35·9	48·2	50	40	42	wsw.	sw.	n.	·130	·12	·16	37
W. 30.	29·677	29·744	29·424	29·22	47·2	41·2	53·3	55	43	46	sw. var.	sw.	calm	...	·28	...	42
Th. 31.	29·679	29·898	29·692	29·06	47·4	42·3	49·4	50	36	46	sw. var.	w.	w.	·180	·03	·17	42
	29·445	30·404	28·655	29·12	45·2	40·2	50·5	69	26	42·7				Sum 2·430	3·30	2·24	41·4

Prof. Forbes on the Mathematical Form of the Gothic Pendant

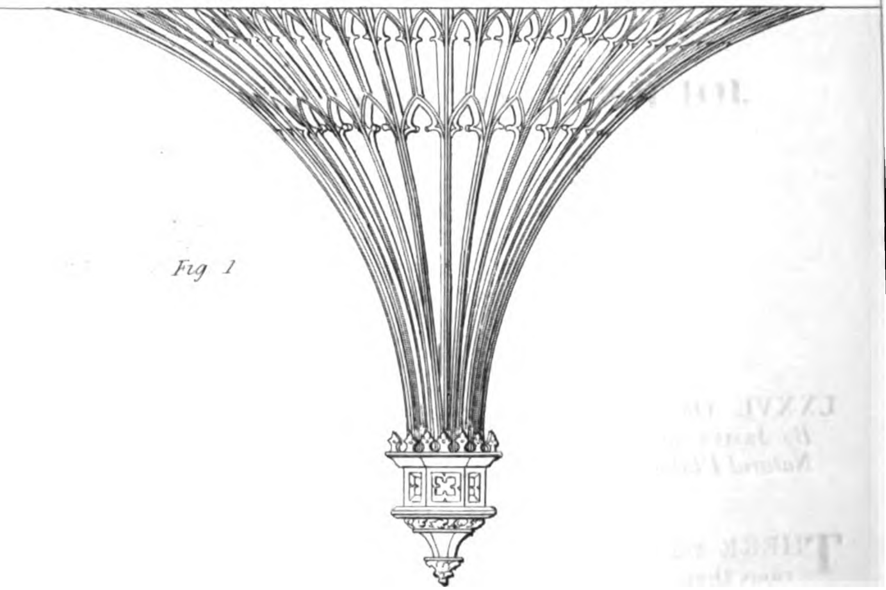

Fig. 1

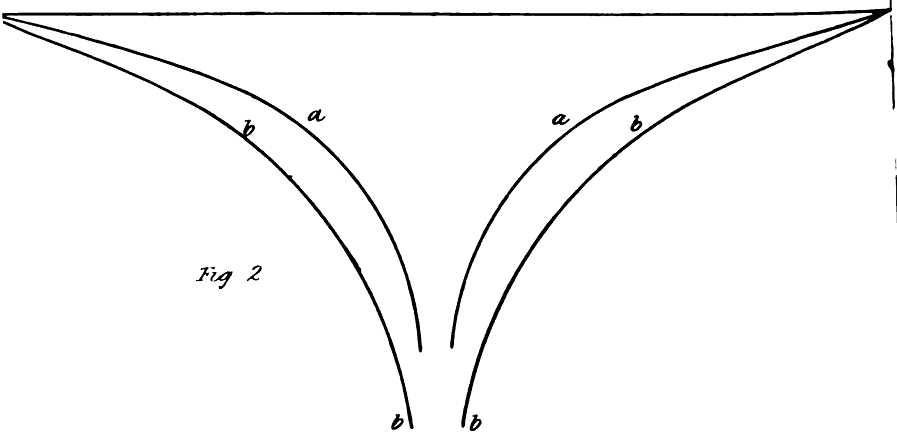

Fig. 2

Pendants of uniform strength of Marble (b) and Portland Stone (a)

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JUNE 1836.

LXXVI. *On the Mathematical Form of the Gothic Pendent.*

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[With a Plate.]

THERE are few points in the history of science more curious than the display of theoretical skill afforded by the masonic works of the darker ages. Wherever the Gothic architects derived their knowledge, it must have been both extensive and sound; and now that the stigma attached to the unfortunate appellation of *Gothic* has in a great measure passed away, and it is admitted that pure taste may be shown in following other than the Grecian models, we may be permitted to gather lessons from these remoter times, tending to show that the basis, at least, of what is pleasing in architecture is not of a capricious or ephemeral character, but reposes upon the immutable substratum of natural laws.

When we select the best works which have characterized the middle ages, including both the Norman and the pointed styles,—but especially the latter, from its earliest introduction into Italy during the Imperial decline down to the sixteenth century,—we are sometimes at a loss to say whether the sound mechanical principles employed in such structures have been more happily displayed or artfully concealed. To con-

* Read before the Royal Society of Edinburgh, Feb. 1, 1836; and communicated by the Author.

fine ourselves to the Pointed style, we have a beautiful accordance amongst the perpetually rising lines of a symmetrical structure. These carry the eye from the base to the summit of a building with a consciousness that such a general disposition of parts is conformable to the particular disposition of details; we have a superposition of less solid upon more condensed parts, retreating buttresses and tapered pinnacles. Then the peculiar form of the pointed arch, which, whilst it leads the eye upwards, has that in it which convinces us of its fitness to be loaded at the summit, and to bear in stately equipoise those spires or towers, which had their especial adaptation to the objects of the sacred edifices with which they were connected. The mutual support afforded by the parts was not only always adequate, but (in the best models) amply enough developed to prove to the eye that it was so. Pillars are placed where they might have been dispensed with, but they are never placed where the eye sees at once their inutility. Spandrels of arches are lightened, though the voussoirs might have sustained the load; open canopies with loaded vertices, though their lightness strikes the eye with a pleasing astonishment, are never suffered to inspire us with a dread of instability.

Yet it often happens that the real sources of security in Gothic architecture have been as carefully kept out of sight, as that amount of protection required by the eye was secured*. We are perfectly capable of admiring the interior of a groined stone roof, without concerning ourselves much with the mode in which the lateral thrust is opposed. The vertical weight is that which chiefly affects our senses, and that the walls should *appear*, as well as *be*, strong enough to sustain it. Yet every carpenter knows that the lateral thrust of his roof must somehow or other be resisted. Much more so when stone is used, and arches which render the employment of tie-beams impracticable. The Gothic architects from a very early period transferred the pressure to individual points of the vertical walls, (for instance, by the beautiful conoidal groining of King's College Chapel, Cambridge,) and sustained the pressures by flying buttresses of the most elegant forms, which conveyed the thrust to the lateral solid buttresses, surmounted by those elegant but ponderous pinnacles, which whilst they appear to be placed but for ornament, are in reality preventing the displacement of these stays, and thus

* I find that Mr. Willis, in his interesting and elegant work on Italian Gothic Architecture, has expressed himself in almost the same terms that I have here used.

conducing to the great end in view. The supports of the towers and spires of churches are in many cases quite different from those which the eye of the spectator is taught to consider as the real sources of stability.

We might say truly of the Gothic architects, "Ars est celare artem"; but we have at present rather to do with the cases in which it is displayed. Though we are very far from thinking that the principles of taste are in all cases referrible to principles of reasoning, we believe that in a vast majority of cases they are so, and frequently to mechanical principles by no means obvious. The *tact*,—as distinguished from *definite knowledge*,—which experience conveys, is one of the most curious of our faculties, and we are often astonished on discovering upon what remote analogies or reasoning our homeliest conclusions are founded. That there is a point beyond which mere logic is unavailable, and where its application would be absurd, few will deny: but we must commence with the clear conception of a design to be answered, and means conspiring to the given end; nor must our superstructure be inconsistent with that design, nor opposed to, if it does not conspire with, those means. The more obvious conditions of stability *must* be fulfilled; and any ornament interfering with them is not only superfluous but displeasing. Every conspicuous part must have its apparent use: no portion must have a greater share of duty assigned to it than it *appears*, as well as *is*, able to sustain. Some of the architects of the middle ages delighted in constructing paradoxes in stone. They violated the rules of good taste, because they violated the rules of common sense. Every one sees that helical pillars, if they be what they appear to be, are incapable of bearing a heavy load. Short dumpy pillars seem disproportioned to the chance of their flexure; very slender ones, unless most skilfully grouped, look as if a touch of the finger would bend them at the middle of their length. Orders of architecture of increasing heaviness as we ascend, stone staircases which seem hung in air, and leaning towers (if we could conceive that it ever occurred to an architect to execute such a monstrosity), would be equal violations of the canons of taste and reason. On the other hand, the most moderately experienced eye cannot look at a well-balanced building, whatever may be its order of architecture, or at a well-trussed roof, however simple its materials, without a degree of conscious satisfaction, of the cause of which we are for a moment ignorant. Though we do not pretend that the eye can detect by mere general experience the concordance between parts which the more refined mechanical problems present, such as the relation between the intrados and extrados of an arch, or the form of an equilibrated dome, yet it so happens that our consciousness of fitness and the accuracy of our theoretical views desert us nearly at the same moment, and that we are obliged to have recourse to that middle path which practical sagacity, long experience, and sound mechanical views point out.

Professor Robison, in one of those admirable articles on applied science with which he enriched the *Encyclopædia Britannica*, and which remarkably exhibit the characteristics just mentioned, after an eloquent appeal on behalf of the dignity of roofs, has the following pertinent remarks.

"The Gothic architecture is, perhaps, entitled to the name of Rational Architecture, and its beauty is founded on the characteristic distinction of our species. It deserves cultivation: not the pitiful, servile and unskilled copying of the monuments; this will produce incongruities and absurdities equal to any that have crept into the Greek architecture: but let us examine with attention the nice disposition of the groins and spandrels; let us study the tracery and knots, not as ornaments, but as useful members; let us observe how they have made their walls like honeycombs, and admire their ingenuity as we pretend to admire the instinct infused by the great Architect into the bee*."

Having had occasion to consider some time ago what should be the form of a depending column of uniform material, such that the area of section should always be proportional to the weight sustained, I was led by an easy analysis to conclude, that it must be the solid generated by the revolution of the logarithmic curve round its axis. The mere imagination of such a depending body reminded me of the beautiful pendents of Gothic architecture, which, though we more frequently see them on a small than a large scale, have always

"* The Greeks were enabled to execute their colossal buildings only by using immense blocks of the hardest materials. The Norman mason could raise a building to the skies without using a stone which a labourer could not carry to the top on his back. Their architects studied the principles of equilibrium; and having attained a wonderful knowledge of it, they indulged themselves in exhibiting remarkable instances. We call this false taste, and say that the appearance of insecurity is the greatest fault. But this is owing to our habits: our thoughts may be said to run in a *wooden* train, and certain simple maxims of carpentry are familiar to our imagination; and in the careful adherence to these consists the beauty and symmetry of the Greek architecture. Had we been as much habituated to the equilibrium of pressure, this apparent insecurity would not have met our eye: we would have perceived the strength, and we should have relished the ingenuity."—Art. ROOF, *Encyclop. Britann.*, Third Edit., vol. xvi. p. 463; 10, 9.

conveyed to my mind a sense of peculiar elegance; and this notwithstanding that they occur only in the later periods of Gothic architecture, and are rather contemptuously passed over by the connoisseur as merely exaggerated bosses.

I have not been able to discover either in practical or descriptive works any indication of the real figure of Gothic pendents. I am perfectly satisfied, however, that if they are not logarithmic spindles, they ought to be so. The gradual modification of the curve from the long finely tapered extremity to the point of greatest curvature, and then the flat receding branch, corresponds to a multitude of Gothic details; and an exact sketch from the best models I have been able to procure has led me to the same conclusion.

It is not to be supposed that the architects could have had a curve in view which was not known until long after the termination of the real Gothic æra; I conceive that it was merely a rude approximation to that figure which might satisfy the eye by exhibiting some parity between the area of the cohering surfaces and the mass to be sustained. When we come to reflect upon extreme cases, this supposition of the judgement exercised by the eye will not appear extravagant. A depending cylinder seems heavy at its lower part, because the area of section is disproportioned to the weight it has to sustain, and hence the upper part will appear weak and contracted; for, if the depending mass be loaded until the limits of cohesion are passed, rupture must take place there. Any body materially increasing inferiorly would be still more displeasing. A uniform cone with the apex downwards will, I believe, strike every one as overloaded near its centre, and every figure having its concavity directed towards its axis would be still more disagreeable. The form, therefore, must be concave outwardly, and we may easily imagine how the abstraction of matter from the middle of the depending cone, and the transfer of it towards its upper and lower extremities, might produce a curve similar to the logarithmic. This figure, in fact, embraces the essential part of what Professor Robison calls rational architecture,—sufficiency without redundancy: the section on which strength depends increases in proportion to the mass to be sustained.

We may observe also that since the lower extremity should be indefinitely extended (the curve becoming asymptotic,) the eye could not be satisfied by an abrupt termination; there is consequently always an inferior expansion which may seem to replace the asymptotic part of the spindle removed, and without which the termination might appear abrupt.

One characteristic of the Gothic architecture is unity of design. We accordingly find the peculiar figure of the pendent carried into the minuter depending ornaments for the sake of symmetry; though the scale is almost too small to require the curve of equal strength to satisfy the eye. It is quite obvious too, that to reverse the case we have described, and to make masses of the form of pendents resting on their smaller bases to sustain weights, is equally repugnant to the principles of good architecture and good sense.

In all cases the strength actually given to pendents enormously exceeds that requisite for their cohesion. It appears from the following simple analysis that the modulus or subtangent of the logarithmic curve, must, in order exactly to prevent rupture, be equal to twice the modulus of cohesion of the substance in feet.

"Required the figure of a depending body which shall be just within the limit of cohesion at every part of its length." Let s^2 represent the area of its section corresponding to any point x in a given vertical ascending line. Since the condition infers that the increase of section shall be in a constant ratio to the increased volume of the solid,

$$a\,d\,.\,s^2 = s^2\,d\,x$$

(a being a constant); and integrating

$$x = a\,.\,\text{hyp. log}\; s^2 + c.$$

If we assume the body to be a solid of revolution, and likewise that the variable radius r shall become equal to unity when $x = 0$, we shall have for the corrected integral

$$x = 2\,a\,.\,\text{hyp. log}\; r.$$

Hence the contour of the pendent will be a logarithmic curve, whose subtangent $= 2\,a$.

Now, since it is required that the increment of cohering surface shall be just capable of supporting the increment of mass, we must have the quantity $\dfrac{s^2\,d\,x}{d\,.\,s^2}$, or a, equal to the *modulus of cohesion* of the substance employed expressed in linear measure. Consequently the subtangent is equal to twice the modulus of cohesion, and for a self-supported body of uniform thickness, the measure of the one and the other would be the same.

In the cases of white marble and Portland stone the moduli of cohesion have been stated at 1542 and 945 feet respectively. The subtangents would, therefore, be 3084 and 1890 feet. We may thence calculate the logarithm of 2 upon those scales, or the vertical height in which the radius of the section doubles itself. This will be found to be 2138 feet in the case of white marble, and 1310 feet in that of Portland stone*. In a pendent n times the necessary strength r will be doubled in the nth part of the above intervals.

Explanation of Plate IV.

Fig. 1. Gothic Pendent.

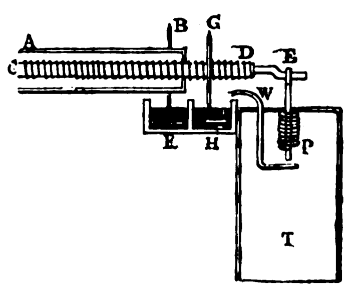
Fig. 2. Pendents of Uniform Strength: *a*, of Portland Stone; *b*, of White Marble.

Edinburgh, January 16, 1836.

LXXVII. *Experimental and Physical Researches in Electricity and Magnetism. By the Rev.* William Ritchie, *LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution and in the University of London.*†

1 AS soon as the magneto-electric spark and shock were obtained, it must have been observed that the *size* of the *spark* increased with the length of the coil employed, and afterwards diminished till it at length disappeared. The physiological effects are also exceedingly feeble with a short coil, and continue to *increase* by increasing the length of the wire long after the spark has attained its maximum brightness. In experimental research, and particularly in public lectures, it is very convenient to obtain both effects from the same magnet and revolving lifter. This is easily and expeditiously accomplished by the following arrangement, which will be understood by simply inspecting the annexed figure.

A B is the hollow axis, C D the solid axis passing through the former, metallic contact being prevented by a cylinder of wood. B is the disc of copper or platina dipping into the mercury contained in the cell F, and G the *star* or point dipping into the cell H. Two copper wires having their ends formed into a close spiral by rolling them round a thick wire are soldered to the hollow and solid axis at B and C. The revolving lifter of soft iron is considerably longer



* If x_2 denote the logarithm of 2 upon the scale in question, and M the modulus of the common system, we shall have

$$x_2 = \frac{2a}{M} \log 2.$$

Whence these numbers are computed.

† Communicated by the Author.

than those commonly employed, and made of a tube of iron instead of a solid mass. A continuous coil of eighty or a hundred yards, or even more according to the effect intended to be produced, is rolled about one of the ends, whilst two or three coils of thirty, forty, or fifty yards long are rolled about the other end. The ends of the last are collected together and soldered to a thick wire which fits into the cylinder formed by the spiral, each end of the single coil being terminated by similar pieces.

When brilliant phænomena of light are required, we fix a star of platina foil on the solid axis, and if we wish to double the effect we fix another similar star on the hollow axis, and connect the ends of the compound coil with the two axes by means of the spiral cylinder. If we wish to exhibit chemical or physiological effects we connect the continuous coil, or employ both coils as a continuous one.

When the short coil is employed the light is exceedingly brilliant and the shock scarcely sensible; with the long coil the light is feeble, but the shock unpleasantly powerful, even without wetting the hands.

The following simple addition to the revolving lifter will supersede the apparatus which I formerly described for detonating a mixture of oxygen and hydrogen by the magneto-electric spark*. D E is a thick copper or brass wire, about the size of a quill, and bent into the annexed form. It is screwed into the end of the brass axis so as to have good metallic contact. E P is a wire having a loop at E through which the wire passes, the other end resting on a small disc of copper connected with the wire W. T is a glass tube open at the lower end and closed at the other by a sound cork, or a piece of wood cemented in it. The wire W dips into the interior compartment F of the cup for holding mercury. A small spiral spring is fixed on the wire a little above P in order to secure good contact with the disc of copper. When the lifter is made to revolve, the end of the wire is raised from the disc at every revolution, and a brilliant spark appears at the point P, which will detonate a mixture of oxygen and hydrogen introduced into the tube.

Though these facts, which I have endeavoured to illustrate by an improved apparatus, are generally known, I am not aware that any theory has been proposed to account for the striking difference between the physical and the chemical or physiological effects.

The undulatory theory of light is already established on so firm a basis, that we may employ it in the explanation of all

* [See Lond. and Edinb. Phil. Mag., vol. iv. p. 105.—Edit.]

phænomena in which light is in any way evolved. It is universally admitted that *nothing* passes from the permanent magnet to the lifter when temporary magnetism is induced on the latter. It is also admitted that *nothing* passes from the lifter to its surrounding coil when *voltaic* electricity is induced on the latter. The polarity of the electricity essentially belonging to the soft iron is rapidly changed by the change of poles in the soft iron horseshoe lifter. The electricity thus thrown into a rapid vibratory state must derange the *stable* equilibrium of the electricity belonging to the coil of copper wire. Hence if this wire or the circuit be suddenly broken, which is the case when one of the points leaves the mercury, the rapid motion of the electricity at the point of separation must communicate a corresponding rapid vibration to the electric fluid contained in the surrounding air, and consequently to the electric fluid contained in the humours of the eye, retina, optic nerve, and brain, which will be followed by the sensation of light.

The appearance and indefinite continuance of the magneto-electric light, without deriving supply from any foreign source, thus affords a powerful argument in favour of the undulatory theory of light, whilst it appears to me an unanswerable objection to the Newtonian doctrine. As long as the lifter is made to revolve, light of the same degree of brilliancy continues to emanate. We can conceive this motion continued for *ever*; so that the light, according to the Newtonian theory, lurking in a small copper wire and actually given out, would ultimately surpass all the light which has been given out by the sun since the creation of the world. For an infinite number of sparks, however minute, will constitute an infinite light; whereas the whole light given out by the sun since the creation is only a very limited quantity. Since gold-leaf placed in the circuit is deflagrated, and a fine platina wire heated red hot, these effects are obviously produced by the rapid vibration of the *electricity* or *æther* essentially belonging to them. The metals then are obviously *heated* by their *own* heat, an unanswerable argument against the *chemical* theory of *caloric.*

2. In order to account for the production of the physical and physiological effects by wires of different lengths, we must take into view the striking difference between good and imperfect conductors of voltaic electricity. The metals not only conduct much *better* than liquids, but also convey the vibratory wave much *quicker.* In the case of a short conductor the whole electricity belonging to it has polarity induced on it in an indefinitely short period; and also returns to its natural

state with extreme rapidity. To produce a sensation the exciting cause must continue to act for a certain length of time depending on the delicacy of the organ. The eye being the most delicate is affected by a series of vibrations continuing during a very short period; and hence a comparatively short wire formed into a coil will exhibit light when the circuit is broken before any sensible shock is experienced.

By continuing to lengthen the coil the series of vibrations will continue during a longer period, but they may not follow each other with sufficient rapidity to constitute light. When any part of the body is placed in the circuit when the metallic contact is broken, the electricity belonging to that part of the body is suddenly forced into a corresponding polar arrangement accompanied by that peculiar sensation termed a shock. Hence in the case of five or six feet of imperfectly conducting substances, such as the liquids of the body, a certain length of time must be required to allow the induction to take place.

3. If these views be correct, the electric fluid instead of being an imponderable agent possesses one of the essential properties of ponderable matter. When a body is put in motion it will *communicate* a portion of its motion to other matter, but not without *losing* a corresponding quantity of its own motion. Hence, agreeably to the experiments of Mr. Faraday, when the electricity of one wire is forced to induce electric polarity on that belonging to another wire, the momentum of the first suffers a corresponding reduction. Again, the *motion* of the electricity of a wire towards a state of polarity will continue after the inducing cause has been removed, thus exhibiting in another point of view the same property of ponderable matter, viz. the inertia of matter, or in this case its tendency to continue in motion after the impulse which first produced the motion has ceased.

If these views be correct we have no right to expect that bodies at different temperatures, or differently electrified or magnetized, will have different weights, since in each of these states they contain exactly the same quantity of *ponderable* and improperly called im*ponderable* matter.

It is a well-known fact that we receive a more powerful shock when electricity is being induced on the body than when the induced electricity is returning to its natural state. This is what might be expected from considering the energy and quantity of the exciting agents employed, these being either a powerful voltaic battery, or the immense quantity of electricity put in rapid motion in a large mass of soft iron.

If these views be correct again, it is obvious that as we *hear* by means of *vibrations*, so we see by means of *vibrations*, we are *warmed* by means of *vibrations*, and we receive an electric shock by the sudden *vibrations* excited in the elastic fluid essentially belonging to our own bodies.

LXXVIII. *On a New Formula for solving the Problem of Interpolation in a Manner applicable to Physical Investigations. By* M. Cauchy.*

IN the application of analysis to geometry, physics, and astronomy, the questions which present themselves for solution are of two kinds. First, it is required to find the general laws of the figures or the phænomena, *that is*, the general form of the equations which exist among the different variables: for instance, between the coordinates of curves and their surfaces; between the velocities, the times, and the spaces described by bodies in motion, &c.: secondly, to determine the numerical values of the arbitrary constant quantities which enter into the expression of these laws, *that is*, the values of the unknown coefficients contained in the equations. Among the variables we usually distinguish, as is well known, those which may vary independently of one another, and are therefore called independent variables, from those which are derived from them by the resolution of the several equations, and which are named functions of the independent variables. Let us consider a particular function of the latter kind, and suppose that it is derived from the independent variables by means of an equation or formula which contains a certain number of coefficients. An equal number of observations or experiments, each of which will afford a particular value of the function answering to a particular system of values of the independent variables, will be sufficient to enable us to determine the numerical values of all these coefficients; and, these values being determined, we may easily obtain such new values of the function as will correspond with new systems of values of the independent variables, and thus solve that which is called the problem of interpolation. If, for example, the ordinate of a curve be expressed as a function of the abscissa by means of an equation containing three coefficients, it will be sufficient to know three points of the curve, that is to say, three particular values of the ordinate corresponding with three particular values of the abscissa, in order to determine the three coefficients. When these are determined the curve may be easily traced by points, if we calculate the coordinates of so many points in the arcs of the curve lying between the given points, as we wish to ascertain.

* Translated from a lithograph circulated by the Author.

Thus, when viewed in its whole extent, the problem of interpolation consists in determining the coefficients or arbitrary constants contained in the expression of the general laws of figures or phænomena, on the supposition that at least an equal number of points is given in the former or an equal number of observations or experiments made upon the latter. In a great number of questions these arbitrary constants enter only in the first degree into the equations containing them. This is precisely what happens when a function is capable of being developed in a converging series arranged according to the ascending or descending powers of an independent variable, or to the sines and cosines of the multiples of an arc. Then the question is, to determine the coefficients of such of the terms of the series as cannot be disregarded without giving cause to fear that a sensible error in the values of the function may be the consequence. Among the small number of formulæ that have been proposed for this purpose the most worthy of notice are,—that derived from the calculus of finite differences but applicable only when the different values of the independent variable are equi-different among themselves,—and that of Lagrange, which, whatever these values be, can be applied to series arranged according to the ascending powers of the independent variable. However, the latter formula itself becomes more and more complicated in proportion as it is found desirable to retain a greater number of the terms of the series in which the function is developed; and what is still more annoying is, that the approximate values of the different orders corresponding with the different cases in which we should keep, first one term of the series, then two, then three, &c., are obtained by calculations almost independent of one another; so that each new approximation, far from being rendered easier, is more tedious and laborious than those which precede. Struck with these inconveniences, and led by my investigations respecting the dispersion of light to turn my attention anew to the problem of interpolation, I have been so fortunate as to find for its solution a new formula, which, both in respect to the certainty of the results and the facility with which they are obtained, seems to me to possess such decided advantages over the others, that I have no doubt it will soon be generally employed by all persons devoted to the cultivation of the physical and the mathematical sciences.

In order to give an idea of this formula, I suppose that a function of x represented by y is developed in a converging series arranged according to the ascending or descending powers of x, or according to the sines and cosines of the multiples of an arc x, or, more generally, according to other functions of x which I shall represent by $\varphi(x) = u$, $\chi(x) = v$, $\psi(x) = w$; so that we have

(1.) $\quad y = au + bv + cw + \ldots\ldots$ where $a, b, c \ldots$ are constant coefficients.

Now, the question is, 1st, how many terms of the second member of the equation (1.) are to be employed, in order to obtain a value of y so approximate that the difference between it and the exact value may be very small, and capable of being compared with the errors to which the observations are liable; 2ndly, to determine in numbers the coefficients of the terms retained, or, in other words, to find the approximate value just mentioned. The data of the problem consist of a sufficient number of values of y represented by

$$y_1, y_2, \ldots\ldots\ldots y_n;$$

and corresponding with an equal number (n) of values of x represented by $x_1, x_2, \ldots\ldots x_n$, and, consequently, with an equal number of values of each of the functions $u, v, w, \ldots\ldots$ These several values of the functions I shall represent by

$$\begin{array}{ll} u_1, u_2, \ldots\ldots u_n & \text{for the function } u; \\ v_1, v_2, \ldots\ldots v_n & \text{for the function } v; \\ w_1, w_2, \ldots\ldots w_n & \text{for the function } w; \text{ \&c.} \end{array}$$

Thus we shall have for the solution of the problem, the number (n) of equations of the first degree among the unknown coefficients $a, b, c \ldots\ldots$

$$(2.) \quad \begin{cases} y_1 = au + bv + cw + \ldots\ldots \\ y_2 = au_2 + bv_2 + cw_2 + \ldots\ldots \\ \vdots \\ y_n = au_n + bv_n + cw_n + \ldots\ldots \end{cases}$$

and if we put i to represent generally any one of the whole numbers 1, 2, ... n, these equations will all be comprised in the general formula

(3.) $\quad y_i = au_i + bv_i + cw_i + \ldots\ldots$

The first approximation will be made by neglecting the coefficients b, c, &c., or, what amounts to the same thing, by reducing the series, which the equation contains, to its first term. Then the general approximate value of y will be

(4.) $\quad y = aw;$

and to determine the coefficient (a) we shall have the system of the equations

(5.) $\quad y_1 = au_1, \; y_2 = au_2 \ldots\ldots y_n = au_n.$

The different values of a that can be deduced from the equations (5.) would, whether considered separately or in combination with one another, be all precisely equal, if the particular values of y which we suppose to be furnished by observation were rigorously exact. But they are not so; for actual observation is inevitably liable to errors confined within certain limits, and this consideration renders it advisable so to combine the equations among themselves that, in the most unfavourable cases, the effect produced on the value of the coefficient a by the errors committed in respect to the values of $y_1, y_2, \ldots y_n$, may be the least possible. Now the different combinations that can be made of the equations (5.) in order to derive from them a new equation of the first degree in reference to a, will all furnish values of a comprised in the general formula

$$(6.) \qquad a = \frac{k_1 y_1 + k_2 y_2 + \ldots\ldots\ldots k_n y_n}{k_1 u_1 + k_2 u_2 + \ldots\ldots\ldots k_n u_n},$$

which we obtain by adding together the equation (5.), member by member, and multiplying them respectively by the constant factors $k_1, k_2 \ldots k_n$. It is still further to be observed, that, as the value of a determined by the equation (6.) does not vary, while we cause the factors $k_1, k_2 \ldots k_n$ to vary simultaneously in the same ratio, it is clear that the greatest among these factors (the sign not being taken into account) may always be considered as reduced to unity.

Finally, let it be observed that if we represent by

$$\varepsilon_1, \varepsilon_2 \ldots\ldots \varepsilon_n,$$

the errors committed in the observations and the values of $y_1, y_2 \ldots y_n$ respectively, the formula (6.) will furnish an approximate value of a, the difference between which and the true value will be

$$(7.) \qquad \frac{k_1 \varepsilon_1 + k_2 \varepsilon_2 + \ldots\ldots k_n \varepsilon_n}{k_1 u_1 + k_2 u_2 + \ldots\ldots k_n \varepsilon_n}.$$

It is now necessary to choose $k_1, k_2, \ldots k_n$ such that, in the most unfavourable cases, the numerical value of the expression (7.) may be the least possible.

Let us represent by

$$\mathrm{S}\, u_i$$

the sum of the several numerical values of u_i, that is to say, what the polynomial $\pm u_1 \pm u_2 \pm \ldots\ldots\ldots \pm u_n$ becomes when we so dispose of each sign in it that each term will be positive.

Let us represent by $\mathrm{S}\,\varepsilon_1$, not the sum of the numerical values of $\varepsilon_1, \varepsilon_2, \varepsilon_3 \ldots \varepsilon_n$, but what the sum $\mathrm{S}\, u_i$ becomes when in it we substitute for each value of u_i the corresponding value of ε_i. If we reduce to $+1$ or to -1 each of the coefficients $k_1, k_2, \ldots k_n$ by so choosing the signs that in the denominator of the fraction (7.) all the terms may be positive, this fraction will be reduced to

$$\frac{S\,\varepsilon_i}{S\,u_i};$$

and it will afford a numerical value, at most, equal to the ratio $\frac{\Sigma}{S\,u_i}$, if we represent by Σ the sum of the numerical values of ε_i, or, in other words, that of $S\,\varepsilon_i$ in the case which is least favourable. On the other hand, by assigning to $k_1, k_2 \ldots k_n$ unequal values the greatest of which (the signs not being taken into account) may be unity, we shall obtain for the denominator the fraction (7.) a quantity whose numerical value will evidently be lower than $S\,u_i$, while that of the numerator may ascend even to the limit Σ: and this will actually happen if the errors $\varepsilon_1, \varepsilon_2, \ldots \varepsilon_n$ be all of no amount, except that one which is multiplied by a factor equal (the sign being disregarded) to unity. Hence it follows that the greatest error to be apprehended in respect to the value of a determined by means of the formula (6.) will be the least possible if we put generally

$$k = \pm 1,$$

choosing the signs in such a manner that, in the polynomial, $k_1 u_1 + k_2 u_2 + \ldots\ldots + k_n u_n$, all the terms may be positive. Then the formula (6.) will give

$$(9.) \qquad a = \frac{S\,y_i}{S\,u_i},$$

($S\,y_i$ being what the sum $S\,u_i$ becomes when in it we substitute for each value of u_i the corresponding value of y_i,) and the equation (4.) will become

$$(10.) \qquad y = \frac{u}{S\,u_i}\,S\,y_i.$$

If, as an abbreviation, we put (11.) $\alpha = \frac{u}{S\,u_i}$, we shall have

$$(12.) \qquad y = \alpha\,S\,y_i.$$

If we supposed generally $u = 1$, the equation (4.) reduced to $y = 0$ would indicate that the value of y is constant; and as we should then have

$\alpha = \frac{u}{S\,u_i} = \frac{1}{n}$, the formula (12.) would give $y = \frac{1}{n}\,S\,y_i$.

We should then take as the approximate value of y the arithmetical mean between the observed values; and the greatest error to be apprehended would be less for this than for any other approximate value. This property of arithmetical means, together with the facility with which they are calculated, completely justifies the preference usually given to them in the valuation of those arbitrary constants which can be determined directly by observation.

Let Δy be now what is wanting to complete the approximate value of y furnished by the equation (12.), so that we have

(13.) $$y = \alpha \, S y_i + \Delta y.$$

Let us also put

(14.) $$v = \alpha \, S u_i + \Delta v, \; w = \alpha \, S w_i + \Delta w, \&c. \ldots$$

we shall derive from the formula (3.)

(15.) $$S y_i = a \, S u_i + b \, S v_i + c \, S w_i, \&c. \ldots$$

then from this last multiplied by α and subtracted from the equation (1.), we obtain

(16.) $$\Delta y = b \, \Delta v + c \, \Delta w + \&c. \ldots$$

Moreover, let us represent by α_i, Δy_i, Δv_i, Δw_i what the values of α, Δy, Δv, Δw, deduced from the equations (11.), (13.), and (14.), become when for x we substitute x_i, i being one of the integers $1 . 2 \ldots n$. If the values of Δy_1, $\Delta y_2 \ldots \Delta y_i$, are very small, and capable of being compared with the inevitable errors of the observations, it will be useless to proceed to a second approximation, and we may rest satisfied with the approximate value of y afforded by the equation (12.). If the contrary takes place, it will be sufficient, in order to obtain a new approximation, if we do with the formula (16.) as in the first approximation we have done with the formula (1.). This being supposed, let us represent by

$$S' \Delta v_i$$

the sum of the numerical values of Δv_i, and by

$$S' \Delta y_i, \; S' \Delta w_i \ldots\ldots \&c.$$

the polynomials into which the sum $S' \Delta v_i$ is changed, when for each value of Δv_i we substitute the corresponding value of Δy_i or of $\Delta w_i \ldots\ldots$.

In fine, let

(17.) $$\beta = \frac{\Delta v}{S' \Delta v_i}.$$

If we can without a sensible error disregard in the series the coefficient (c) of the third term and those of the following terms, we must take as the approximate value of Δy, (18.) $$\Delta y = \beta\, S' \Delta y_i.$$

Let $\Delta^2 y$ be the remainder of the second order which is required to complete this approximate value, and let us therefore put

(19.) $$\Delta y = \beta\, S' \Delta y_i + \Delta^2 y.$$

Let us in like manner put

(20.) $$\Delta w = \beta\, S' \Delta w_i + \Delta^2 w, \text{ \&c. } \ldots$$

We shall derive successively from the formula (16.)

(21.) $$\Delta y_i = b \Delta v_i + c \Delta w_i + \text{\&c.} \ldots$$

(22.) $$S' \Delta y_i = b\, S' \Delta v_i + c\, S' \Delta w_i, \text{ \&c.} \ldots$$

and from this last, multiplied by β and deducted from the equation (19.), we obtain

(23.) $$\Delta^2 y = c \Delta^2 w +, \text{ \&c.} \ldots$$

Let β_i, $\Delta^2 y_i$, $\Delta^2 w_i$ ...... be what the values of β, $\Delta^2 y$, $\Delta^2 w$, derived from the equations (17.), (19.), and (20.), become when for x we substitute x_i, i being one of the integers $1 . 2 \ldots n$. If the values of $\Delta^2 y_1$, $\Delta^2 y_2 \ldots \Delta^2 y_n$ be very small, and capable of being compared with the errors incident to the observations, it will be useless to proceed to a new approximation, and we may be contented with the approximate value of Δy furnished by the equation (18.). If it happen otherwise we shall obtain a third approximation by operating upon the formula (23.) as we have done in the first approximation on the formula (1.). By continuing this process we shall obtain the following rule.

The unknown quantity y, a function of the variable quantity x, being supposed capable of being developed in a converging series

(I.) $$a w + b v + c w + \ldots\ldots$$

in which u, v, w represent given functions of the same variable, if we know n particular values of y corresponding with n particular values $(x_1, x_2, x_3, \ldots x_n)$ of x: if moreover we represent by i any one of the whole numbers $1, 2, 3 \ldots n$ and by y_i, u_i, $v_i \ldots$ what y, u, v, ... become when for x we substitute x_i; then, in order to obtain a sufficient approximation to the general value of y, we shall first determine the coefficient α by means of the formula

(II.) $$u = \alpha\, S u_i,$$

(in which $S u_i$ represents the sum of the numerical values of u_i,) and the difference of the first order Δy by means of the formula

(III.) $$y = \alpha\, S y_i + \Delta y.$$

If the particular values of Δy represented by Δy_1, $\Delta y_2 \ldots$

Δy_n can be compared with the errors of observation, we may disregard Δy and reduce the approximate value of y to

$$\alpha \, S y_i.$$

In the contrary case we shall determine β by means of the formulæ

(IV.) $\quad v = \alpha \, S v_i + \Delta v, \; \Delta v = \beta \, S' \Delta v_i,$

($S' \Delta v_i$ being the sum of the numerical values of Δv_i,) and the difference of the second order ($\Delta^2 y$) by means of the formula

(V.) $\quad \Delta y = \beta \, S' \Delta y + \Delta^2 y.$

If the particular values of $\Delta^2 y$ represented by $\Delta^2 y_1$, $\Delta^2 y_2$, ... $\Delta^2 y_n$ may be set off against the errors of observation, we shall be able to neglect $\Delta^2 y$, and therefore to reduce the approximate value of y to $\alpha \, S y_i + C \, S' \, \Delta \, y_i$; but if they cannot, we shall determine γ by means of the formulæ

(VI.) $\quad w = \alpha \, S w_i + \Delta w, \; \Delta w = \beta \, S' \Delta w_i + \Delta^2 w, \; \Delta^2 w = \gamma \, S'' \Delta^2 w_i.$

($S'' \Delta^2 w_i$ being the sum of the numerical values of $\Delta^2 w_i$,) and the difference of the third order ($\Delta^3 y$) by means of the formula

(VII.) $\quad \Delta^2 y = \gamma \, S'' \Delta^2 y_i + \Delta^3 y$, &c.

Thus, in short, by supposing the coefficients α, β, γ ... determined by the system of the equations (II.), (IV.), (VI.), &c. we shall calculate the several orders of differences represented by Δy, $\Delta^2 y$, $\Delta^3 y$ ...... or, rather, their particular values corresponding with the values (x_1, x_2, x_3 ...) of the variable x, until we arrive at a difference the particular values of which may be set off against the unavoidable errors of observation. Then it will be sufficient to represent as zero the value of this difference deduced from the system of the equations (III.), (V.), (VII.)... in order to obtain a sufficient approximation to the general value of y.

This general value will be then

$$y = \alpha \, S y_i, \text{ or } y = \alpha \, S y_i + \beta \, S' \Delta y_i \ldots \text{ \&c.}$$

according as we shall be able, without a sensible error, to reduce the series (I.) to its first term or its first two terms, &c. Now, if we call the number of terms retained m, the problem of interpolation will be resolved by the formula

(VIII.) $\quad y = \alpha \, S y_i + \beta \, S' \Delta y_i + \gamma \, S'' \Delta^2 y_i +$ &c.,

the second member being continued to the term which contains $\Delta^{n-1} y_i$.

It is necessary to observe, that from the formulæ (II.), (III.), (IV.), (V.), (VI.) (VII.) ... we derive not only

(IX.) $S\alpha_i = 1$, $S\beta_i = v$, $S'\beta_i = 1$; $S\gamma_i = v$, $S'\gamma_i = v$; $S''\gamma_i = 1$, &c.,

but also

(X.) $S\Delta v_i = v$; $S\Delta w_i = v$; $S\Delta^2 w_i = v$; $S'\Delta^2 w_i = v$, &c.,

and

(XI.) $S\Delta y_i = 0$; $S\Delta^2 y_i = 0$; $S'\Delta^2 y_i = 0$; $S'\Delta^3 y_i = 0$...

These latter formulæ are so many equations of condition which must be satisfied by the particular values of α, β, γ... as well as by those of the several orders of differences of $w, v, w \dots y$; and hence it follows that in the calculation of these particular values we cannot commit an error of a single figure without being apprised of it by the bare fact of the equation of condition ceasing to be verified.

The advantages of the new formulæ of interpolation are the following:

1st. They are applied to the development by series, whatever be the law according to which the different terms are deduced from one another, and whatever be the values, equidifferent or not, of the independent variable.

2nd. The new formulæ are of very easy application, especially when logarithms are employed in the calculation of the ratios α, β, γ ... and in that of the product of those ratios by the sums of the several values of the functions or their differences. Then, in fact, all the operations are reduced to additions and subtractions.

3rd. By means of these formulæ the successive approximations are made with a constantly increasing facility, as the several orders of differences continually decrease.

4th. They allow us to introduce at once into the calculation the numbers furnished by all the given observations, and thus to add to the exactness of the results by making a great number of experiments subservient to this object.

5th. They possess this advantage also, that, on every new approximation, the values which they furnish for the coefficients a, b, c are precisely those in which the greatest error to be apprehended is the least possible.

6th. Our formulæ indicate of themselves the moment when the calculation ought to cease by then giving differences comparable with the errors of observation.

7th. The quantities which they determine satisfy equations of condition which do not allow the least fault of calculation to be committed without being almost instantly perceived.

In the new mathematical exercises there will be found numerous applications of the formulæ of interpolation. I shall here quote but one of them.

Let l be the length of a luminous undulation relative to one of the rays of the solar spectrum, and θ the index of refraction of this ray passing from the air into another medium: it follows from the principles established in my memoir on the dispersion of light, that we may develop in a converging series $\left(\frac{1}{l}\right)^2$ according to the ascending powers of $\left(\frac{\theta}{l}\right)^2$, and consequently $\left(\frac{\theta}{l}\right)^2$ and θ^2 according to the ascending powers $\left(\frac{1}{l}\right)^2$. Moreover, a very able observer, Fraunhofer, has determined, in respect to different substances, the indices of refraction of the rays for which the values of l in hundred-millionth parts of an inch, are

2541, 2425, 2175, 1943, 1789, 1585, 1451,

and finds as the corresponding values of θ relative to a certain species of flint-glass, 1·626469; 1·628469; 1·633667; 1·640495; 1·646756; 1·658848; 1·669686: in which case the formula (VIII.) being then reduced to

$$\theta^2 = 2\cdot6112351 - 0\cdot0256298 \left(\frac{l_l}{l}\right)^2 + 0\cdot1081567 \left(\frac{l_l}{l}\right)^4$$
$$- 0\cdot0649226 \left(\frac{l_l}{l}\right)^6 + 0\cdot019115 \left(\frac{l_l}{l}\right)^8 - 0\cdot002139 \left(\frac{l_l}{l}\right)^{10};$$

reproduces exactly, and without the slightest alteration, the preceding values of θ.

September 1835.

LXXIX. *On the Colours of Natural Bodies.* By Sir David Brewster, *K.H. LL.D. F.R.S. Lond. V.P.R.S. Edin.**

THERE are few of the applications of optical science so universally interesting as that which has for its object the explanation of the colours of natural bodies. Sir Isaac Newton was the first person who ventured to refer to one general principle all the variety of colours which are found in nature; and he maintained his opinions on this subject with a confidence in their accuracy which seems to have confounded his adversaries: For while his analysis of light, the most perfect of all his labours, exposed him to the most harassing controversies, his theory of natural colours, the least perfect of his speculations, was allowed to pass without examination or censure.

During the century which has elapsed since the death of

* From the Transactions of the Royal Society of Edinburgh, an unimportant paragraph being omitted.

Newton this theory has been generally received and admired: In our own day it has been ingeniously defended, and beautifully illustrated, by M. Biot; and, with few exceptions, it has been adopted by most of the distinguished philosophers of the present age.

The author of this theory has presented it under the two following propositions, one of which states the general cause of the phænomena, and the other the particular constitution of natural bodies on which their colours depend.

1. " Every body reflects the rays of its own colour more copiously than the rest, and from their excess or predominance in the reflected light, has its colour.

2. " The transparent parts of bodies, according to their several sizes, reflect rays of one colour, and transmit those of another, on the same ground, that thin plates or bubbles do reflect or transmit those rays."

In estimating the truth of the theory which is contained in these two propositions, I do not intend to enter into any examination of the postulates, facts, and reasonings, on which it is founded. The object of the following paper is to analyse one leading phænomenon of colour, and to apply this analysis as an *experimentum crucis*, in determining the true origin of all colours similarly produced.

The colour which I have chosen for this purpose is the *green colour of the vegetable world*, and I have made this selection for the following reasons:—

1. The green colour of plants is the one most prevalent in nature.

2. It is the colour of which Sir Isaac Newton has most distinctly described the nature and composition.

3. Its true composition is almost identically the same in all the variety of plants in which it appears.

Sir Isaac Newton has described this colour in the following manner:—

" There may be good *greens* of the fourth order, but the purest are of the *third*. And of this order the *green* of all vegetables seems to be, partly by reason of the intenseness of their colours, and partly because, when they wither, some of them turn to a *greenish-yellow*, and others to a more perfect *yellow* or orange, or perhaps to *red*, passing first through all the aforesaid intermediate colours. Which changes seem to be effected by the exhaling of the moisture which may leave the tinging corpuscles more dense, and something augmented by the accretion of the oily and earthy part of that moisture. Now the *green*, without doubt, is of the same order with those colours into which it changeth, because the changes are gradual, and those colours, though usually not very full, yet are often too full and lively to be of the fourth order."

Having thus determined that the green colour of vegetables must, according to this theory, be a *green of the third order*, we must inquire into its composition. Sir Isaac has himself stated, that the green of the third order "is principally constituted of original green, but not without a mixture of some blue and yellow." In point of fact, it consists of all the rays of the green space, with the least refrangible rays of the blue space, and the most refrangible rays of the *yellow* space, and it does not contain a single ray of *indigo* or *violet*, nor a single ray of *orange* or *red light*. This is its real composition, whether we deduce it from the theory of periodical colours, or obtain it by direct analysis with the prism.

In order to discover the true composition of the green colour of plants, we may analyse the light which they reflect or transmit, but the best method is to extract the green colouring matter by means of alcohol, and to examine the action of the tingeing corpuscles when suspended in that fluid. For this purpose I have used the leaves of the common laurel, *Prunus Lauro-cerasus*, as a type of this class of colours. The leaves are torn into small shreds and put into absolute alcohol, and the fine green fluid which is thus obtained is either placed in a hollow prism with a large refracting angle, so as to exhibit its composition in its own spectrum, or the light transmitted through the fluid may be analysed by a fine prism, or the spectrum produced by such a prism may be viewed through a portion of the fluid bounded by parallel surfaces. By whichsoever of these methods the experiment is made, we shall observe a spectrum of the most beautiful kind. In place of seeing the *green* space with a portion of *blue* on one side and *yellow* on the other, as the Newtonian theory would lead us to expect, we perceive a spectrum divided into several coloured bands of unequal breadths, and having their colours greatly changed by absorption.

At a certain thickness of the green fluid there are three *red* bands. By increasing the thickness, the violet and blue spaces are absorbed, and the two inner red bands. An absorption then begins near the middle of the green space, and after destroying the more refrangible portion of that space, three bands are left; viz. *one* faint band of the extreme *red*, *one* band almost *white*, corresponding with the most luminous spectrum, and one *green* band contiguous to the white one.

In applying this mode of examination to the green colours of others plants, I have found them to have invariably the same composition. In the following list of plants of various characters, I have given those in which I have made the experiments with most care. Excepting where it is otherwise mentioned, the green fluid was extracted from the leaves:

White Lilac.	Celastrus scandens.
——– Convolvulus.	Viburnum Tinus.
Tulip-Tree.	Prunus Lusitanica.
Mignionette.	Aucuba Japonica.
Common Pea.	Juniperus communis.
Daphne Cneorum.	Camellia Japonica.
Virginian Raspberry.	The green berries of the Convallaria multiflora.
White Jasmine.	The green berries of the Asparagus officinalis.
Thuja occidentalis.	
Arbutus Unedo.	
Hemerocallis flava.	

When the green fluid obtained from these plants has stood for *three* or *four* days, it loses its high green colour, and becomes of an olive-green, which grows more and more of a brownish-yellow, till it becomes almost colourless. During these various changes, the specific action of the fluid upon the spectrum changes also; but neither the change of colour nor the change of action has any relation whatever to the effects of an increase or decrease of thickness in the tingeing corpuscles, by which Sir Isaac Newton explains the changes which take place in the colour of leaves. When the fluid has become almost colourless like water, it still exercises a powerful action upon the middle of the *red* space, and a faint, but still perceptible action, at two points of the *green* band. This curious fact may lead us to expect that transparent media may yet be discovered, which shall absorb different parts of the spectrum, while they themselves are perfectly colourless. This effect of course cannot take place unless the rays absorbed compose white light.

In the course of these experiments, I observed a very remarkable phænomenon, which at first sight appeared to be somewhat favourable to the Newtonian theory. In making a strong beam of the sun's light pass through the green fluid, I was surprised to observe that its colour was a brilliant *red*, complementary to the *green*. By making the ray pass through greater thicknesses in succession, it became first *orange* and then *yellow* and *yellowish-green*, and it would undoubtedly have become blue, if it had been transmitted through a greater thickness of fluid. This mode of producing a spectrum by reflexion from the particles of a fluid, exhibits the phænomenon of opalescence in a very interesting form. Had the green fluid shown the same colour at all thicknesses, or had it absorbed only the red rays, the opalescent beam would have been *red* throughout the whole of its path: but as the different colours are absorbed in different proportions, and, in the present case, in the order of their refrangibility, excepting the blue and violet, the colour of the intromitted beam must vary from red to greenish-yellow, as these colours are successively taken out of it.

The analysis of this experiment is very interesting, but as this is not the place to pursue it, I shall only remark, that I have observed the same phænomenon in various other fluids of different colours, that it occurs almost always in vegetable solutions, and almost never in chemical ones, or in coloured glasses; and that it is a phænomenon of opalescence or imperfect transparency. One of the finest examples of it which I have met with may be seen by transmitting a strong pencil of solar light through certain cubes of bluish fluor-spar. The brilliant blue colour of the intromitted pencil is singularly beautiful.

According to the Newtonian theory of colours, the green of plants is of the same order as the *yellow* and *orange* into which it is changed when it withers, in consequence of an increased density, or an enlargement of size in the tingeing corpuscles. In order to put this opinion to the test of experiment, I extracted the yellow juice from the brilliant yellow leaves of the *common laurel.* This fluid becomes of a deep red at great thicknesses. It attacks the spectrum powerfully towards the extremity of the green space, a place where it is not touched by the green fluid. It then absorbs the *yellow* and *violet*, leaving a bright green, and converting the blue into violet. At greater thicknesses, the violet disappears, and the absorption advances gradually to the red.

For the purpose of varying the experiment, I extracted the juice of the leaves of the privet, which become of a deep black violet when they wither, a colour which has not the most remote resemblance to any periodical tint. The fluid was a deep red colour, much deeper than that of the darkest port wine. It attacked the red part of the spectrum near the line B of Fraunhofer, at the same place that the green juice attacked it, leaving *two red* bands, the innermost of which vanished at an increased thickness. It then absorbed the violet and blue spaces generally; and having obliterated the middle of the green space, the absorption advanced to the orange rays at D.

Now, in both these experiments, the action of the colouring matter of the decayed leaves is decidedly different from that of the green juice, and there is no appearance whatever of the tints having any such relation as that which subsists between adjacent colours of the same order.

From facts like these, which it is impossible to misinterpret, we are entitled to conclude, that the green colour of plants, whether we examine it in its original verdure, or in its decaying tints, has no relation to the colours of thin plates.

I have submitted to the same mode of examination nearly *one hundred and fifty* coloured media, consisting of fluids extracted from the petals, the leaves, the seeds, and the rind of plants,—the different substances used in dyeing,—coloured glasses and minerals,—coloured artificial salts,—and different coloured gases; and in all these cases I have obtained results which lead to the same conclusion. I have analysed, too, the *blue* colour of the sky, to which the Newtonian theory has been thought peculiarly applicable; but instead of finding it a *blue* of the first order, in which the extreme red and the extreme violet rays are deficient, while the rest of the spectrum was untouched, I found that it was defective in rays, adjacent to some of the fixed lines of Fraunhofer, and that the absorptive action of our atmosphere widened, as it were, these lines. Hence it is obvious, that there are elements in our atmosphere which exercise a specific action upon rays of definite refrangibility, and that this, in some of these rays, is identical with that which is exercised over them by the atmosphere of the sun. I have obtained analogous results in analysing the *yellow*, *orange*, *red*, and *purple* light, which is reflected from the clouds at sunset; but it is impossible to convey any correct idea of the composition of these colours, without a reference to the fixed lines of the spectrum, of which we at present possess no distinct nomenclature.

I may mention, however, this general fact, that in the various specific actions exercised upon light by solids, fluids, vapours, and gases, the points at which the spectrum is attacked are generally coincident with the deficient lines of Fraunhofer; and particularly with those which are common to the light of the sun, and that of some of the fixed stars. Hence it appears, that these rays or lines are weak parts of the spectrum, or the parts of white light which have the greatest affinity for those elements of matter, which, while they enter into the composition of sublunary bodies, exist also in the atmospheres of the central luminaries of other systems.

From the preceding experiments, it is impossible to resist the conclusion, that the second and leading proposition of Newton's theory of colours is incompatible with the actual phænomena; and we may demonstrate the incorrectness of the first proposition by simply stating the fact, that there are *red*, *yellow*, *green*, and *blue* media, which are absolutely incapable of reflecting or transmitting certain definite rays of the same colour with themselves.

The true cause of the colours of natural bodies may be thus stated: When light enters any body, and is either reflected or transmitted to the eye, a certain portion of it, of various refrangibilities, is lost within the body; and the colour of the body, which evidently arises from the loss of part of the intromitted light, is that which is composed of all the rays which are not lost; or, what is the same thing, the colour of the body is that which, when combined with that of all the rays which are lost, compose the original light. Whether the lost rays are reflected, or detained by a specific affinity for the material atoms of the body, has not been rigorously demonstrated. In some cases of opalescence, they are either partly or wholly reflected; but it seems almost certain, that in all transparent bodies, and in that great variety of substances in which no reflected tints can be seen, the rays are detained by absorption*.

LXXX. *On the Proposition that a Function of θ and ψ can be developed in* ONLY ONE *Series of* Laplace's *Coefficients; the Function being supposed not to become infinite between the limits* 0 *and* π *of* θ' *and* 0 *and* 2π *of* ψ. *By the Rev.* J. H. PRATT, *B.A.*†

THIS important proposition is, in fact, not proved, but assumed, by Laplace in the *Mécanique Céleste*, III. ii. § 12. Professor Airy pointed out this defect, and gave a proof of the proposition in the Cambridge Philosophical Transactions: but this labours under the restriction of supposing the number of terms in the series *finite*. M. Poisson has considered this among numerous other important questions in a paper in the *Connoisance des Tems* for 1829, and also in his *Théorie Mathématique de la Chaleur*, chap. viii. But I confess it appears to me that the proposition is not proved even in these places; though by a slight addition to the reasoning the objection to the proof may be removed.

M. Poisson shows that if $p = \cos\theta \cos\theta' + \sin\theta \sin\theta' \cos(\psi - \psi')$, and also if $(1 - 2\alpha p + \alpha^2)^{-\frac{1}{2}} = 1 + \alpha P_1 + \alpha^2 P_2 + \ldots + \alpha^i P_i + \ldots\ldots$ then

$$f(\theta, \psi) = \frac{1}{4\pi} \int_0^{\pi} \int_0^{2\pi} \cdot \{1 + 3\alpha P_1 + \ldots + (2i+1)\alpha^i P_i + \ldots\} f(\theta', \psi') \sin\theta' \, d\theta' \, d\psi'. \qquad (1.)$$

* The views on this subject of Sir John Herschel will be found in a paper by that philosopher in Lond. and Edinb. Phil. Mag., vol. iii. p. 401.—EDIT.

† Communicated by the Author.

He then says, that since $\int_0^{\pi}\int_0^{2\pi} . P_i f(\theta', \psi') \sin\theta' \, d\theta' \, d\psi'$ is of the form of Laplace's coefficients, we may put it

$$= \frac{4\pi}{2i+1} Y_i;$$

and hence $f(\theta, \psi) = Y_0 + Y_1 + \ldots\ldots + Y_i + \ldots\ldots$

In the same manner we shall have

$$f(\theta', \psi') = Y'_0 + Y'_1 + \ldots\ldots + Y'_i +,$$

the accents denoting that θ' and ψ' are put for θ and ψ.

Hence $Y_i = \frac{2i+1}{4\pi}\int_0^{\pi}\int_0^{2\pi} . P_i f(\theta', \psi') \sin\theta' \, d\theta' \, d\psi'$ by the above assumption,

$$= \frac{2i+1}{4\pi}\int_0^{\pi}\int_0^{2\pi} . P_i Y'_i \sin\theta' \, d\theta' \, d\psi' \text{ by the}$$

nature of *Laplace's Coefficients.* All so far is clear enough.

But in order to show that $f(\theta, \psi)$ cannot be developed in another series $V_0 + V_1 + \ldots V_i + \ldots$ he says, that if this were possible we should have

$$V_i = \frac{2i+1}{4\pi}\int_0^{\pi}\int_0^{2\pi} . P_i V'_i \sin\theta' \, d\theta' \, d\psi'$$

by what has preceded; and then easily deduces the result desired. But surely this is no less than *begging the question.* All we learn from it is that if we proceed to develop, *as above,* we shall arrive at a series of determinate terms; but it does not follow that another method of development cannot be discovered which would lead to another series. The following demonstration appears to be free from objection.

In the formula (1.) we may evidently interchange θ and θ', ψ and ψ' since $P_1 \, P_2 \ldots P_i \ldots$ are the same functions of θ, ψ and θ', ψ'. Hence from that formula we learn that the definite integral of the product of any given function of θ and ψ, and the function $(1 + 3\alpha P_1 + \ldots. + (2i+1)\alpha^i P_i + \ldots.) \sin\theta$ does not vanish between the limits specified above.

Now, suppose $f(\theta, \psi)$ can be expanded in the two distinct series $Q_0 + Q_1 + \ldots\ldots + Q_i + \ldots..$ and $R_0 + R_1 + \ldots\ldots R_i + \ldots.$ Then by hypothesis $Q_i - R_i$ does not vanish; and consequently,

$$\int_0^{\pi}\int_0^{2\pi} \left(1 + 3\alpha P_1 + \ldots\ldots + (2i+1)\alpha^i P_i + \ldots\ldots\right) \sin\theta \; (Q_i - R_i) \, d\theta \, d\psi \quad \text{does not vanish.}$$

$\therefore \int_0^{\pi} \int_0^{2\pi} \cdot P_i \sin\theta \, (Q_i - R_i) \, d\theta \, d\psi$ does not vanish, since all the other terms do vanish by the nature of *Laplace's Coefficients.*

Again, $(Q_0 - R_0) + (Q_1 - R_1) + \ldots + (Q_i - R_i) + \ldots = 0$. Multiply by $P_i \sin\theta$ and integrate; then we have

$$\int_0^{\pi} \int_0^{2\pi} \cdot (Q_i - R_i) \, P_i \sin\theta \, d\theta \, d\psi = 0:$$

but we showed that this does not vanish if Q_i and R_i are different functions. Hence that hypothesis is not true, and therefore $Q_i = R_i$ and the expansions of $f(\theta, \psi)$ are identical.

Caius College, Cambridge, Feb. 18, 1836.

LXXXI. *On a supposed new Sulphate and Oxide of Antimony.* *By* Michael Faraday, *D.C.L., F.R.S., &c. &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In my Experimental Researches, paragraphs 693. 694. 695. 696.*, I have, in relation to antimony, described what I considered to be a new sulphuret, and expressed my belief that a new and true protoxide existed consisting of single proportions, "but could not stop to ascertain this matter strictly by analysis." Professor Rose when in London informed me that Berzelius objected to my new sulphuret, and I was induced to make more accurate experiments on that point, which showed me my error, and accorded generally with what Rose had described to me. I intended to publish these results in the first electric paper which I might have to put forth; but my friend Mr. Solly has put into my hands a translation of Berzelius's paper, and it is so clear and accurate as to the facts that I now prefer asking you to publish it, adding merely that my experiments quite agree with those described in it, as regards the sulphuret. With respect to the supposed chloride and oxide, I have not anywhere implied that I had made quantitative experiments on them.

On Faraday's supposed Sulphuret of Antimony and Oxide of Antimony: by J. J. Berzelius,—*From his* "Jahresbericht," *No.* 15.

"Faraday has stated, that when sulphuret of antimony is heated with more metallic antimony, a new sulphuret of antimony is formed, which when in the fused state is distinguish-

* See Lond. and Edinb. Phil. Mag., vol. v. p. 170.—Edit.

able from the common sulphuret. According to a few experiments, this sulphuret of antimony is composed of $Sb\,S$, or one atom of each element. When this sulphuret is dissolved in muriatic acid, sulphuretted hydrogen is evolved, and although a little antimony is separated, yet there remains in solution a combination with chlorine $Sb\,Cl$, which when decomposed with carbonate of soda furnishes a new oxide. The mixing of this with the common oxide is said to have given rise to the contradictory views of its composition, and also to the appearance that the fused oxide of antimony is decomposed to a certain extent by the electric current only until the new oxide is reduced.

"Faraday appears convinced of the truth of this statement, but adds that he has not confirmed by analysis the composition of this oxide, because he should thereby have interrupted the course of his main experiments.

"This appeared to me to deserve a nearer investigation, as well for itself as for the importance of its influence on Faraday's electro-chemical views. I have therefore repeated the above-described experiments of Faraday on the three new combinations of antimony with sulphur, chlorine, and oxygen, and I have found that even if they do exist they cannot possibly be formed by the means which he has described, and they are therefore still to be discovered.

"The following is the substance of my examination. I mixed together very carefully and intimately sulphuret of antimony and metallic antimony in the proportions that, through melting, the combination $Sb + S$ must be formed: the mixture was then put into a glass tube; this was drawn out to a capillary end; the air was then expelled by heat, and the tube was hermetically sealed. The tube was then placed in a vessel covered with sand, heated to a full red-heat, and then suffered to cool slowly. When the mass was taken out there was at the bottom a regulus, which contained 63 per cent. of the antimony which had been added after it had been separated from some adhering portions of sulphuret of antimony by boiling with a little muriatic acid.

"This had all the properties of pure antimony. Rubbed to powder and boiled with muriatic acid, it still evolved however a little sulphuretted hydrogen and gave some antimony to the acid. The powder when thus boiled had lost 6¼ per cent.

"From all this it is evident that though the resulting sulphuret of antimony contained more antimony after than before the process, it is not the combination which Faraday thought it was. Even in the cleavage it had not the appearance of a pure sulphuret of antimony. The upper portions had the same radiated structure as the common sulphuret of antimony, and a few larger crystals had shot up into the upper surface of the regulus, where they were surrounded with an irregular mass of a lighter colour. The upper and the lower portions of this so-formed antimony were each separately analysed, in such a manner that a weighed portion was put into muriatic acid and digested in it in the water-bath. The solution went on rapidly. From the lowermost portion crystals fell off one after another, upon which the acid did not act. The same happened likewise with the uppermost portion, only they were smaller and fewer in number. These insoluble parts when well boiled and washed were from the lowermost 15 and from the uppermost 10 per cent. It proved to be pure metallic antimony formed in feathery crystals, and shows, therefore, the interesting fact that sulphuret of antimony can dissolve at a high temperature $13\frac{1}{4}$ per cent. of metallic antimony, which when the solution is suffered to cool sufficiently slowly crystallizes out of the yet fluid sulphuret of antimony before this latter solidifies. By a more rapid cooling the whole mass congeals together, and the cleavage is then quite similar throughout.

"From what has been said it is quite evident that the muriatic acid takes up nothing but the common chloride of antimony. I have examined this behaviour further in detail, and thereby found, that by this method neither with water nor alkali is it possible to obtain any other oxide.

"The above-mentioned experiment of Faraday, that melted oxide of antimony is decomposed by the electric current, clearly proves that the law proposed by him that similar quantities of electricity always evolve equal chemical proportions, only holds good so long as the comparison is made between combinations of proportional composition.

"As for the cause of the appearance, that the decomposition of the oxide of antimony becomes gradually weaker and weaker, and at last ceases, it is evident that Faraday has overlooked the circumstance that the oxide is decomposed into metal at the negative conductor and antimonious acid at the positive conductor, which then soon becomes encrusted with a solid substance, after which the electricity could not have any further action."

With respect to Berzelius's objection in the last paragraph but one of his paper, I will ask you to reprint paragraph 821.* of my series. "All these facts combine into, I think, an irresistible mass of evidence, proving the truth of the important

* See Lond. and Edinb. Phil. Mag., vol. v. p. 344.—Edit.

proposition which I at first laid down, namely, *that the chemical power of a current of electricity is in direct proportion to the absolute quantity of electricity which passes.* (377. 783.) They prove too that this is not merely true with one substance, as water, but generally with all electrolytic bodies; and further that the results obtained with any *one substance* do not merely agree amongst themselves, but also with those obtained from *other substances*, the whole combining together into *one series of definite electro-chemical actions.* (505.) I do not mean to say that no exceptions will appear; perhaps some may arise, especially amongst substances existing only by weak affinity: but I do not expect that any will seriously disturb the result announced. If, in the well-considered, well-examined, and I may surely say, well-ascertained doctrines of the definite nature of ordinary chemical affinity, such exceptions occur, as they do in abundance, yet without being allowed to disturb our minds as to the general conclusion, they ought also to be allowed, if they should present themselves at this the opening of a new view of electro-chemical action: not being held up as obstructions to those who may be engaged in rendering that view more and more perfect, but laid aside for a while, in hopes that their perfect and consistent explanation will finally appear."

With regard to my having overlooked the cause of the diminution and cessation of voltaic action on the oxide of antimony, I do not know how that can well be said, for Berzelius's statement seems in parts to be almost a copy of the reasons I have given: see paragraph 801. of the Seventh Series of my Researches. My explanation is actually referred to in the account of the action on the oxide of antimony at paragraph 693., but by a misprint 802. has been stated instead of 801.* I am, Gentlemen, yours &c.,

M. Faraday.

LXXXII. *Table of observed Terrestrial Refractions.* *By* John Nixon, *Esq.*†

THE following table of mean refractions is founded on measurements obtained at 61 stations on 162 arcs, of various lengths from 1′ 9″ to 21′ 48″, amounting together to 17° 59′. The observations were made on 515 different days of the years 1821 to 1824, 1827 to 1833, and 1835. The average altitude above the level of the sea of the 162 arcs is 1730 feet.

* This reference is correctly made in Lond. and Edinb. Phil. Mag., vol. v. p. 170.—Edit.

† Communicated by the Author.

Table of observed Refractions.

Class.		Sum.	Refr.	Mean Arc.	Refr.	Ratio to Arc.
A.	11 arcs from 16′ 36″ to 21′ 48″	=207′ 31″	761″	18′ 52″	+69″	= $\frac{1}{16 \cdot 4}$
B.	24 arcs from 9′ 8″ to 14′ 0″	=269 28	949½	11 14	+39½	= $\frac{1}{17 \cdot 0}$
C.	77 arcs from 3′ 54″ to 8′ 58″	=467 37	1530	6 4½	+20	= $\frac{1}{18 \cdot 3}$
D.	50 arcs from 1′ 9″ to 3′ 54″	=134 23	184½	2 41	+ 4	= $\frac{1}{44}$
Sum, 162 arcs = 17° 58′ 59″; Refr. 3425″ = $\frac{3425}{64739}$ = $\frac{1}{18 \cdot 9}$						

The class A. forms itself naturally from the total deficiency of observations on arcs from 14′ 1″ to 16′ 36″. That marked B. consists of arcs still large enough to mask those local irregularities of refraction, generally of a negative character, which begin to form an occasional but slight feature of the class C. The arcs marked D, subdivided at first into two classes of about the same number of arcs, were ranged together on account of the anomaly of the smaller arcs presenting a refraction of $\frac{1}{22}$, whilst those of the other class, abounding in negative refractions, averaged no more than $\frac{1}{112}$. Some of the more marked deviations from the mean value may, no doubt, be attributed in inconsiderable distances to the want of sufficient accuracy in the measurement of the height of the eye and pointing the telescope at the base of the signal, yet numerous recent observations have clearly indicated a modification of the average refraction peculiar to the locality. As the ratio of the refraction to the contained arc appears to increase with the arc, it is more than probable that the constant error of the sector, considered as $-20''$, has been estimated in defect.

Let a, c be two arcs, of which c is (considerably) greater than a, and let a third arc b equal their difference $c-a$. Admitting $\frac{1}{r}$ to denote the *constant* proportion of the refraction to the arc, then will $\frac{a}{r}$ be the refraction for the arc a, and $\frac{c}{r}$ that for the arc c; that of the intermediate arc b being equal to $\frac{c}{r} - \frac{a}{r}$. Supposing the instrument to give the elevations in defect by an unknown constant quantity x, the calculated refraction for the arc a will be $\frac{a}{r} - x$, and that for the arc C, $\frac{c}{r} - x$. Hence the *true* refraction for the intermediate arc b must equal the difference of these two quantities, and the constant error of the instrument will be the excess of this proportion of the arc c (or a) above the quantity previously calculated.

The several values of the refraction in terms of the arc, and that of the error of the instrument, deduced from the application of the above formula to various combinations of the four classes of arcs, are subjoined.

		Ref.	Error of Sector.
I.	By the difference of B and A ...	$\frac{1}{133}$	$-24''$
II.	——————— C and A ...	$\frac{1}{137}$	-23
III.	——————— D and A ...	$\frac{1}{15}$	-26
IV.	——————— D and B ...	$\frac{1}{14\cdot 3}$	$-27.$

The true value of the mean refraction is most probably the average of the two first ratios, or $\frac{1}{13\cdot 6}$, and that of the instrumental error $23\frac{1}{2}''$, or about $2''$ more than the quantity derived from actual measurement. (See Lond. and Edinb. Phil. Mag. for 1833, vol. ii. page 334.)

Ilkley, April 27, 1836. JOHN NIXON.

LXXXIII. *Characters of some undescribed Species of* Araneidæ. *By* JOHN BLACKWALL, *Esq., F.L.S., &c.**

Tribe, TUBITELÆ, Latreille.

Genus, *Walckenaëria* } mihi.
Walckenaëria fuscipes, }

CEPHALOTHORAX oval, convex and glossy, with a slight indentation in the medial line of the posterior region; the anterior part, which is prominent and acute, is compressed and deeply indented on the sides, and has also a slight longitudinal indentation above: in front it is divided into two segments by a transverse groove. Mandibles conical, armed with teeth on the inner surface, and inclined towards the pectus, which is broad and heart-shaped. Legs moderately robust; the anterior and posterior pairs, which are the longest, are equal in length, and the third pair is the shortest. These parts, with the maxillæ and lip, are of a brown colour; palpi brown, the fourth and fifth joints being much the darkest; the fourth joint terminates in two apophyses; one, which is large, depressed, and hairy externally, overlaps the base of the fifth joint; the other, which is small, projects on the inner side; the fifth joint is oval, convex and hairy externally, concave within, comprising the palpal organs; they are highly

* Communicated by the Author.

developed, not very complex in structure, and are of a brown colour tinged with red. Eyes distributed in pairs on the anterior eminence of the cephalothorax; one pair is situated on the summit of its superior segment, another on a small prominence on the upper part of the inferior segment, in front; these eyes describe a narrow trapezoid whose shortest side is before; the third and fourth pairs are seated on the sides of the frontal eminence, and are geminated. Each tarsus is terminated by three claws; the two upper ones are pectinated, and the lower one is inflected near its base. Abdomen oval, convex above, projecting over the base of the cephalothorax; it is thinly covered with hair, glossy, and of a brownish black colour. The plates of the spiracles are pale yellow.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{12}$th of an inch; length of the cephalothorax $\frac{1}{20}$; breadth $\frac{1}{40}$; breadth of the abdomen $\frac{1}{32}$; length of an anterior leg $\frac{1}{10}$; length of a leg of the third pair $\frac{1}{12}$.

I found this spider in March 1835, at Oakland, under stones; but obtained specimens of males only.

Walckenaëria depressa.

Cephalothorax of a short oval form, convex, prominent, but obtuse, before, where the eyes are situated, depressed in the posterior region, without any indentation in the medial line. Mandibles moderately strong, concave, and slightly inclined towards the pectus, which is broad and heart-shaped. The anterior and posterior pairs of legs, which are the longest, are equal in length, and the third pair is the shortest. These parts, with the maxillæ and lip, are of a deep brown colour, the cephalothorax, pectus, and lip being much the darkest. Each tarsus is terminated by three claws; the two superior ones are curved and pectinated, and the inferior one is inflected near its base. The third and fourth joints of the palpi are short; the latter is the larger, and has two strong apophyses in front, the outer one of which is the more prominent: the fifth joint is oval, convex and hairy externally, concave within, comprising the palpal organs; they are highly developed, complicated in structure, with a curved, spiny process at the extremity, and are of a deep red-brown colour. Abdomen oval, somewhat depressed, pointed at the spinners, and projects over the base of the cephalothorax; it is thinly covered with hair, glossy, and brownish black. Plates of the spiracles deep brown. Aged individuals have the legs of a dark red-brown colour.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{11}$th of an inch; length of the cephalothorax $\frac{1}{18}$; breadth $\frac{1}{27}$; breadth of the abdomen $\frac{1}{24}$; length of an anterior leg $\frac{1}{11}$; length of a leg of the third pair $\frac{1}{11}$.

The specimens from which the description was made were taken under stones, in a wood at Oakland, in April 1835. Males alone were captured.

Walckenaëria obtusa.

There is a striking resemblance between the male of this species and the male of *Walckenaëria cuspidata* *; the following are the principal points of difference. The male of *Walckenaëria obtusa* is decidedly the larger, its pectus is more elongated, it has a slight indentation in the medial line of the posterior region of the cephalothorax, and has no acute, conical prominence situated within the trapezoid formed by the four intermediate

* For the description of *Walckenaëria cuspidata*, see Lond. and Edinb. Phil. Mag., vol. iii. p. 108.

eyes. Its palpi also differ a little in organization: the third joint is clavate; the fourth is short, terminating in three apophyses, the largest of which curves outwards before the fifth joint; exterior to this occurs the next in size, having a small, pointed prominence at its base, in front; and the smallest is situated underneath: the fifth joint is somewhat oval, convex and hairy externally, concave within, comprising the palpal organs; they are highly developed, complicated in structure, with a strong spine on the outer side curved into a circular form, and are of a brownish black colour tinged with red.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{7}$th of an inch; length of the cephalothorax $\frac{1}{13}$; breadth $\frac{1}{20}$; breadth of the abdomen $\frac{1}{19}$; length of an anterior leg $\frac{5}{24}$; length of a leg of the third pair $\frac{1}{6}$.

I found males of this species under stones at Oakland in February 1835, but I have not been so fortunate as to discover the female.

Tribe, Inequitelæ, Latreille.

Genus, *Theridion*, Walckenaër.

Theridion angulatum.

Cephalothorax inversely heart-shaped, inclining to oval, convex, slightly hairy, prominent before, where the eyes are situated, with an indentation in the medial line of the posterior region; its colour is pale yellow-brown, with a longitudinal band of red-brown on each side, and a broader one of the same hue extending along the middle; the margins are yellowish white. Eyes placed on black spots; four, which are intermediate, form a square nearly, the two in front being seated on a protuberance; the other four are disposed in pairs on the sides of the square; the eyes constituting each pair are placed obliquely on an eminence, and are near together but not contiguous. Mandibles moderately strong, conical, and perpendicular; they are red-brown, with a spot of a darker hue in front, near the base of each. Maxillæ enlarged externally, where the palpi are inserted, obliquely truncated on the outer side, at the extremity, and inclined towards the lip, which is almost semicircular, being a little pointed at the apex. These organs, and the palpi, which are short, and are armed with a curved, pectinated claw at the extremity, are of a red-brown colour. Pectus of an oblong heart-shape and a dark red-brown hue. Legs yellowish-brown banded with red-brown; the first pair is the longest, then the fourth, the third pair being the shortest; the second and third pairs are disproportionally short. Each tarsus is terminated by three claws; the two superior ones are curved, and are slightly pectinated near the base, and the inferior one is inflected near its insertion. The abdomen, which is deeply notched in front, and projects over the base of the cephalothorax, has an angular appearance, occasioned by two, bold, lateral prominences, situated on the upper side, nearer to the posterior than the anterior extremity; the superior surface, from the fore part to the lateral prominences, is of a deep red-brown colour, the margins being the darkest; on each side of the medial line are two minute, yellowish white spots, forming a long narrow quadrangle; the posterior part is pale red-brown, a yellow transverse line connecting the two lateral prominences, from which proceed two obscure, angular bands that converge to the spinners; the whole of the upper part has an irregular border of yellowish-white minutely freckled with red-brown; the sides and under part of the abdomen are dark red-brown, with streaks and minute spots of a lighter shade. Plates of the spiracles yellow.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{7}$th of an inch; length of the cephalothorax $\frac{1}{16}$; breadth $\frac{1}{22}$; breadth of the abdomen $\frac{1}{14}$; length of an anterior leg $\frac{1}{4}$; length of a leg of the third pair $\frac{1}{8}$.

This spider, which, like *Tetragnatha extensa*, frequently extends the first and second pairs of legs forwards, and the third and fourth pairs backwards, in a line with the body, was found in a cleft of a rail at Oakland, in the month of April, 1835. I have not yet seen the male.

Theridion filipes.

This remarkable species has the cephalothorax of an oval form; it is convex and glossy, with an indentation in the medial line of the posterior region. Mandibles powerful, conical, armed with teeth on the inner surface, rather divergent at the extremities, and inclined towards the pectus, which is heart-shaped. Maxillæ enlarged at the base, where the palpi are inserted, obliquely truncated on the outer side, at the extremity, and inclined towards the lip, which is semicircular, and prominent at the apex. Legs and palpi long, slender, and furnished with hairs and some fine, erect spines. These parts are of a brown colour, the mandibles and maxillæ having a tinge of red. Eyes disposed in two transverse rows on the fore part of the cephalothorax; the intermediate eyes of both rows form a trapezoid whose anterior side is considerably the shortest; the lateral ones are placed obliquely in pairs, each pair being seated on a small eminence, and geminated; the posterior eyes of the trapezoid are larger, and the anterior ones much smaller than the rest. Each tarsus is terminated by three claws; the two superior ones are curved and slightly pectinated, and the inferior one is inflected near its base. The first pair of legs is the longest, then the fourth, the third pair being the shortest. Abdomen oval, convex above, projecting over the base of the cephalothorax; it is thinly covered with hair, glossy, and of a blackish brown colour, with a tinge of olive. A long, slender, cylindrical, semitransparent process, directed backwards, is in connexion with the sexual organs. Plates of the spiracles of a deep, dull brown colour. Some specimens have a series of faint, pale, angular lines, whose vertices are directed forwards, extending along the middle of the upper part of the abdomen.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{8}$th of an inch; length of the cephalothorax $\frac{1}{20}$; breadth $\frac{1}{20}$; breadth of the abdomen $\frac{1}{16}$; length of an anterior leg $\frac{5}{24}$; length of a leg of the third pair $\frac{3}{20}$.

The male is rather smaller and darker coloured than the female, but the relative length of its legs is the same; their absolute length, however, is greater, an anterior one measuring $\frac{11}{48}$ths of an inch. The third and fourth joints of the palpi are short; the latter, which is the stronger, being prominent on the inner side and in front; with the frontal prominence several long bristles are connected: the fifth joint is of a long, irregular oval form, having a projection on the outer side, and two smaller ones on the upper part, near its articulation with the fourth joint; it is convex and hairy externally, concave within, comprising the palpal organs, which are highly developed, complicated in structure, and of a red-brown colour; a strong, corneous spine, enveloped in a delicate, transparent membrane, originates in the upper part of these organs, and, bending downwards, extends along their inner side a little beyond the termination of the fifth joint, being curved outwards at its extremity.

This spider is allied to the *Nerienæ* by the disposition and relative size of the eyes, and to the *Linyphiæ* by the length and delicacy of its limbs; indeed, on a superficial view, it bears a striking resemblance to *Linyphia pusilla*; but the structure of the maxillæ and the relative length of the legs have induced me to class it with the *Theridia*. It occurs under stones in the woods at Oakland, where I captured specimens in March 1835.

The first individual I examined under the microscope was a female, and it presented an anomaly in organization which I never before witnessed in this class of animals; it had a supernumerary eye, situated between the two small ones constituting the anterior pair of the trapezoid. An instance of a deficiency of eyes in a female *Thomisus cristatus* has since fallen under my observation. This spider had the two lateral pairs only; the two intermediate, or smaller pairs, were altogether wanting, not even the slightest rudiments being visible.

Genus, *Neriene*, *Neriene rubripes*, } mihi.

Cephalothorax oval, convex, glossy, with furrows on the sides diverging from the upper part to the margins, and an indentation in the medial line of the posterior region. Mandibles powerful, conical, convex in front, divergent at the lower extremities, armed with two rows of teeth on the inner surface, and slightly inclined towards the pectus, which is heart-shaped. Maxillæ strong, and inclined towards the lip, which is semicircular and prominent at the apex. These parts are of a red-brown colour, the mandibles, lip, and margins of the pectus being the darkest. Legs moderately robust, provided with hairs and a few fine spines; the first pair is rather the longest, then the fourth, the third pair being the shortest. These organs and the palpi are of a red colour. Each tarsus has three claws at its extremity; the two superior ones are pectinated about two thirds of their length from the base, and the inferior one is inflected near its insertion. Eyes placed on black spots, and disposed as in the *Nerienæ* generally. Abdomen oval, convex above, projecting over the base of the cephalothorax; it is thinly covered with hair, glossy, and brownish black. Plates of the spiracles pale yellow. A curved process of a red-brown colour is connected with the sexual organs. Some individuals have the abdomen of a yellowish-brown hue, and the other parts, generally, lighter-coloured.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{2}{18}$ths of an inch; length of the cephalothorax $\frac{1}{12}$; breadth $\frac{1}{16}$; breadth of the abdomen $\frac{1}{17}$; length of an anterior leg $\frac{1}{4}$; length of a leg of the third pair $\frac{5}{24}$.

The male is somewhat smaller and darker-coloured than the female, but its legs are longer, an anterior one measuring $\frac{7}{24}$ths of an inch. The maxillæ are remarkably convex externally immediately before the insertion of the palpi. The second joint of the palpi is curved towards the cephalothorax; the third and fourth joints are short, the latter being rather the larger: the fifth is oval, convex and hairy externally, concave within, comprising the palpal organs, which are prominent, highly developed, complex in structure, and are of a dark red-brown colour. The fifth or terminal joints of the palpi have their convex sides directed towards each other.

This species was found at Oakland, under stones, in the autumn of 1834, by Mr. T. Blackwall.

Neriene tibialis.

This spider has the cephalothorax of an oval form; it is convex, glossy, prominent but obtuse before, where the eyes are situated, with an indentation in the medial line of the posterior region. Mandibles moderately powerful, conical, armed with teeth on the inner surface, and somewhat inclined towards the pectus, which is heart-shaped. These parts, with the maxillæ and lip, are of a brownish black colour. The anterior and posterior pairs of legs, which are the longest, are equal in length, and the third pair is the shortest. Each tarsus is terminated by three claws; the two superior ones are slightly pectinated, and the inferior one is inflected near its base. The tibiæ of the anterior pair of legs are disproportionally strong, having the appearance of being swoln. The palpi are slender; the third joint is long and clavate; the fourth is elongated before into a narrow, oval process, hairy externally, which extends obliquely across the upper part of the fifth joint towards the inner side, but is terminated by a short, acute spine curved outwards: the fifth joint is oval, convex and hairy externally, concave within, comprising the palpal organs, which are highly developed and complicated in structure, having several corneous processes, one of which, on the outer side, at the extremity, is curved into a circular form. Abdomen oval, convex above, projecting over the base of the cephalothorax; it is thinly covered with hair, glossy, and of a brownish black colour. The plates of the spiracles are pale yellow.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{12}$th of an inch; length of the cephalothorax $\frac{1}{21}$; breadth $\frac{1}{28}$; breadth of the abdomen $\frac{1}{30}$; length of an anterior leg $\frac{1}{7}$; length of a leg of the third pair $\frac{1}{9}$.

In March 1835 I captured a few specimens of this species, all of which were males, under stones, at Oakland.

Neriene livida.

Cephalothorax oval, convex, glossy, with several furrows on the sides diverging from the middle to the margins, and an indentation in the medial line of the posterior region. Mandibles powerful, conical, convex in front, near the base, armed with a few small teeth on the inner surface, and rather inclined towards the pectus, which is heart-shaped. Maxillæ strong, convex underneath, and inclined towards the lip, which is somewhat of a triangular form truncated at the apex. Legs and palpi robust, and furnished with hairs and fine spines. These parts are of a red-brown colour, the lip, maxillæ, mandibles, and anterior part of the cephalothorax being the darkest. Each tarsus is terminated by three claws; the two superior ones are curved and deeply pectinated, and the inferior one is inflected near its base; the palpi have a curved, pectinated claw at the extremity. Abdomen oval, convex above, projecting over the base of the cephalothorax, and rather broader at the posterior than the anterior extremity; it is thinly covered with hair, glossy, and of a yellowish-brown colour, with a tinge of black. Plates of the spiracles pale-yellow.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{6}$th of an inch; length of the cephalothorax $\frac{1}{14}$; breadth $\frac{1}{20}$; breadth of the abdomen $\frac{1}{12}$; length of an anterior leg $\frac{1}{5}$; length of a leg of the third pair $\frac{1}{7}$.

The male is smaller and darker coloured than the female, but the relative length of its legs is the same. The second joint of the palpi is curved towards the cephalothorax; the third and fourth joints are short. the latter projecting two obtuse apophyses, the larger one situated on the outer and the smaller one on the inner side: the fifth joint is oval, convex and hairy externally, concave within, comprising the palpal organs, which are highly developed, complex in structure, and of a dark red-brown colour.

This species is common on the under surface of stones in the neighbourhood of Llanrwst.

Neriene furva.

The cephalothorax is of an oval figure; it is convex, glossy, with slight furrows on the sides, and an indentation in the medial line of the posterior region. Mandibles powerful, conical, vertical, convex in front, and armed with teeth on the inner surface. Maxillæ enlarged at the base, where the palpi are inserted, and inclined towards the lip, which is semicircular and prominent at the apex. Pectus heart-shaped. These parts are dark brown, with a slight tinge of red, the pectus, lip, and anterior part of the cephalothorax being the darkest. Legs and palpi robust, and of a red colour. Each tarsus is terminated by three claws; the two superior ones are pectinated about half their length from the base, and the inferior one is inflected near its insertion. The third and fourth joints of the palpi are short, the former, which is considerably the stronger, being convex in front; the latter projects two apophyses from its anterior extremity; one before, which terminates in a corneous point, and has a small, acute, corneous prominence on the inner side; the other underneath, which is provided with a corneous point on the outer side: the fifth joint is oval, convex and hairy externally, concave within, comprising the palpal organs; they are highly developed, complicated in structure, with a corneous process at the upper part curved outwards, and are of a dark red-brown colour. Abdomen oval, convex above, projecting over the base of the cephalothorax; it is thinly covered with hair, glossy, and of a brownish-black colour. Plates of the spiracles pale yellowish-white.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen $\frac{1}{5}$th of an inch; length of the cephalothorax $\frac{1}{17}$; breadth $\frac{1}{20}$; breadth of the abdomen $\frac{1}{20}$; length of an anterior leg $\frac{1}{6}$; length of a leg of the third pair $\frac{1}{7}$.

I found one male only of this species, under a fragment of rock in a wood at Oakland, in June 1835.

Tribe, ORBITELÆ, } Latreille.
Genus, *Linyphia*, }

Linyphia nigella.

Cephalothorax oval, convex, glossy, with an indentation in the medial line of the posterior region; it is of a dark brown colour approaching to black. Mandibles long, powerful, armed with teeth on the inner surface, divergent at the extremities, of a deep brown colour tinged with red, and inclined towards the pectus, which is heart-shaped, and of a brownish black hue. Maxillæ strong, longer than broad, with the exterior angle, at the extremity, curvilinear; they resemble the mandibles in colour, and incline a little towards the lip, which is semicircular, prominent at the apex, and brownish black. Legs long and slender, provided with hairs and a few spines; their colour is pale yellowish brown, the thighs having a tinge of red. Each tarsus is terminated by three claws; the two superior ones are curved and pectinated, and the inferior one is inflected near its base. Eyes disposed in two transverse rows on the anterior part of the cephalothorax; the intermediate eyes of both rows form a trapezoid, whose anterior side is considerably the shortest, and the lateral ones are placed obliquely in pairs, each pair being seated on a small eminence, and geminated; the posterior eyes of the trapezoid are the largest, and the anterior ones much the smallest of the eight. The third and fourth joints of the palpi are short, the latter, which is much the stronger, being prominent on the inner side, at the lower extremity: the fifth joint is of an irregular oval figure, convex and hairy externally, concave within, comprising the palpal organs, which are highly developed, complicated in structure, having a small projection at the upper part, in front, and a large corneous spine, originating in the upper part of the under side, extending to the termination of the joint, where it is curved into a circular form, the extremity projecting a little; the colour of these organs is dark reddish brown.

The convex sides of the fifth or terminal joints of the palpi are directed towards each other. Abdomen oval, convex above, projecting over the base of the cephalothorax; it is thinly covered with hair, glossy, and brownish black. Plates of the spiracles pale yellowish brown. Some individuals have a series of obscure, angular lines of a yellowish brown colour, whose vertices are directed forwards, extending along the middle of the upper part.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{8}$th of an inch; length of the cephalothorax $\frac{1}{18}$; breadth $\frac{1}{24}$; breadth of the abdomen $\frac{1}{19}$; length of an anterior leg $\frac{11}{48}$; length of a leg of the third pair $\frac{1}{6}$.

Specimens of this species were procured under fragments of rock in the woods at Oakland, in September 1835, but they were all males.

Linyphia tardipes.

The cephalothorax of this interesting species is oval, convex, glossy, depressed and somewhat rounded before, with an indentation in the medial line of the posterior region; its colour is reddish brown, with a broad band of blackish brown extending along each side. Mandibles powerful, conical, divergent at the extremities, and inclined towards the pectus; they are terminated by a long nail slightly curved at its extremity, and are armed with two rows of teeth on the inner surface, the anterior row being remarkably long and fine. Maxillæ strong, straight, and somewhat quadrate, having the exterior angle, at their extremity, curvilinear. Lip semicircular and prominent at the apex. The pectus, which is heart-shaped, is finely pointed at its posterior extremity. These parts are of a reddish brown colour, the pectus and lip being rather the darkest. Eyes placed on black spots on the anterior part of the cephalothorax; four are intermediate and form a square nearly, the two in front being the largest of the eight; the other four are disposed in pairs on the sides of the square; the eyes constituting each pair are placed obliquely on a small eminence, and are contiguous. The palpi are furnished with spines, and have a slightly curved, slender claw at their extremity; their colour is reddish brown. Legs moderately robust, supplied with hairs and a few fine, erect spines; they are of a reddish brown colour obscurely banded with brownish black; the first pair is the longest, the second and fourth pairs are nearly equal in length, and the third pair is the shortest. Each tarsus is terminated by three claws; the two superior ones are curved, and the inferior one is inflected near its base. Abdomen oval, convex above, rather broader at the posterior than the anterior extremity, and projects over the base of the cephalothorax; it is thinly covered with hair, glossy, and of a reddish brown colour on the upper side with a few minute, whitish spots interspersed, and a series of large, brownish black blotches extending along each side of the medial line; these blotches unite, as they approach the spinners, and form transverse, curved bands; the sides are brownish black, minutely mottled with reddish brown; the under side is dark brown, or brownish black. Plates of the spiracles pale yellow. Connected with the sexual organs is a large and very prominent, curved process of a dark red-brown colour; it is abruptly contracted in the curvature, and is recurved at the extremity, which is enlarged and deeply notched.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{6}$th of an inch; length of the cephalothorax $\frac{1}{17}$; breadth $\frac{1}{16}$; breadth of the abdomen $\frac{1}{17}$; length of an anterior leg $\frac{5}{21}$; length of a leg of the third pair $\frac{1}{6}$.

The male resembles the female in colour, and in the relative length of the legs, but their absolute length is greater, an anterior one measuring

$\frac{7}{24}$ths of an inch. On the anterior part of the cephalothorax, about the region of the eyes, are some longish, black bristles, directed forwards. The third and fourth joints of the palpi are short, the latter being the stronger, and a long, slender bristle projects in front from the anterior extremity of the former: the fifth joint is somewhat oval, being gibbous on the outer margin, and having a large process, or apophysis, curved outwards, and notched at its extremity, directed upwards from its superior part; it is convex and hairy externally, concave within, comprising the palpal organs, which are highly developed, complex in structure, presenting several curved, corneous processes, and are of a red-brown colour. The fifth joints of the palpi have their convex sides turned towards each other.

In the autumn of 1834, I found specimens of this spider at Oakland, under detached pieces of rock imbedded in a light soil, to the inferior surface of which they attach their cocoons, usually two or three in number, by a small, fine web. The cocoon is flat on the side in contact with the rock, and convex, with a small, depressed border, on the opposite one. It measures about $\frac{1}{4}$th of an inch in diameter, is composed of white silk of a fine compact texture, and contains, on an average, between thirty and forty spherical eggs of a pale yellow colour, not agglutinated together, but enveloped in delicately soft silk. This species fabricates a small, compact, horizontal sheet of web in the cavities beneath stones, on the under side of which it takes its station in an inverted position. It pairs in the month of September. An approximation to the *Theridia* may be traced in the disposition and relative size of the eyes.

Tribe, Laterigradæ, Latreille.

Genus, *Thomisus*, Walckenäer.

Thomisus luctuosus.

Cephalothorax inversely heart-shaped, convex, depressed in the posterior region, and broadly truncated before; it is of a brown colour, veined with lines of a deeper shade, and has a fine line of yellowish white on the lateral margin; a short band of a yellowish white hue, bifid before, on each side of which is a spot of the same colour, situated on an irregular, black patch, occupies the medial line of that portion of the cephalothorax which is in contact with the abdomen, and a faint brownish white spot occurs on the inner side of the tubercles on which the anterior eyes of the lateral pairs are seated. Eyes disposed in front, in two transverse, curved rows, forming a crescent; the lateral eyes of both rows are larger than the rest, those of the anterior row being the largest of all, and are situated on projections of the cephalothorax. Mandibles short, strong, vertical, cuneiform. Maxillæ inclined towards the lip, which is triangular. Pectus oblong heart-shaped. These parts, with the legs and palpi, are of a dark brown colour, the legs being streaked and spotted with brown of a deeper shade, and yellowish white at the joints. The first and second pairs of legs, whose dimensions considerably exceed those of the third and fourth pairs, are nearly equal in length, the second pair being slightly the longer; and the longitudinal extent of the fourth pair surpasses that of the third. Each tarsus has two curved, deeply pectinated claws at its extremity. Abdomen oval, depressed, wrinkled, broader at its posterior than its anterior extremity, and projects over the base of the cephalothorax; its colour is dark brown obscurely mottled with pale brown and yellowish white, particularly on the upper part. Plates of the spiracles reddish brown.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{4}$th of an inch; length of the cephalothorax $\frac{1}{8}$th; breadth

$\frac{1}{10}$; breadth of the abdomen $\frac{1}{7}$; length of a leg of the second pair $\frac{1}{4}$; length of a leg of the third pair $\frac{1}{4}$.

I discovered the female of this species, which seems to belong to the section *Cancroides*, in September 1834, in the woods at Oakland, on the trunks of trees which had been felled. In July it constructs a lenticular cocoon of white silk, of a compact texture, measuring about $\frac{1}{4}$th of an inch in diameter, in which it deposits between 80 and 90 spherical eggs, of a pale yellowish white colour, not agglutinated together. The cocoon is often placed between two leaves connected by a slight tissue of silk, forming a kind of sack, usually containing the female, which sits upon the cocoon and is greatly attached to it.

Tribe, CITIGRADÆ, } Latreille.
Genus, *Lycosa*, }

Lycosa exigua.

Cephalothorax large, hairy, somewhat oval, compressed before, with depressed, sloping sides, and a narrow indentation in the medial line of the posterior region; its colour is dark brown, with three longitudinal bands of a pale yellowish brown tint, one extending along each side, and the third occupying the carina. Mandibles strong, conical, armed with a few teeth on the inner surface, reddish brown, and inclined towards the pectus, which is heart-shaped, of a very dark brown colour, approaching to black, and is thinly covered with whitish hairs. Maxillæ short, powerful, straight, enlarged and rounded at the extremity, and of a pale reddish brown colour. Lip quadrate, and of a dark, dull brown colour, being palest at the apex. Eyes unequal in size; four, which are minute, form a row in front, the two exterior ones being the smallest; the other four are placed on the sides of the anterior part of the cephalothorax, and form a square nearly, the anterior pair being the largest of the eight. Legs and palpi long, moderately robust, and provided with hairs and strong spines; they are of a pale reddish brown colour, with spots and longitudinal streaks of a brownish black hue on the upper part and sides; these spots and streaks are most conspicuous on the thighs, and on the second joint of the palpi. The palpal claw is curved and pectinated. Each tarsus has two curved, deeply pectinated claws at its extremity. Abdomen oval, hairy, convex above, projecting over the base of the cephalothorax; it is dark brown on the upper side, with three yellowish white spots in front, the intermediate one, which is the largest, and is faintly bordered with brownish black, extending backwards nearly half the length of the abdomen; on each side of the medial line, on the posterior half of the abdomen, occurs a series of alternate blackish and white spots, the latter being much the smaller; the two series, which are rather obscure in some specimens, converge to the spinners, where they meet; the sides are yellowish brown, spotted with dark brown; the under side is pale yellowish, or reddish brown. Plates of the spiracles very dark brown.

Length, from the anterior part of the cephalothorax to the extremity of the abdomen, $\frac{1}{4}$th of an inch; length of the cephalothorax $\frac{1}{8}$; breadth $\frac{1}{11}$; breadth of the abdomen $\frac{1}{10}$; length of a posterior leg $\frac{11}{24}$; length of a leg of the third pair $\frac{3}{10}$.

The male is rather smaller than the female, and darker coloured, but the relative length of its legs is the same. The third and fourth joints of the palpi are short, the latter being the stronger of the two: the fifth joint is oval and pointed at the extremity, which is armed with a small claw; it is convex and hairy externally, concave within, except at the end, which is solid, and comprises the palpal organs; they are highly developed, complex with corneous processes, and are of a very dark reddish brown colour.

This species occurs in pasture fields in Denbighshire. In the month of June the female spins a lenticular cocoon of yellowish or greenish brown silk, of a compact texture, with a whitish margin of a slighter texture; it contains between 50 and 60 yellowish white eggs of a spherical figure, not agglutinated together. The cocoon, which is always connected with the spinners of the female, and is carried along with her, measures about ⅛th of an inch in diameter; when the young quit it they attach themselves to the body of the mother.

Oakland, Denbighshire, 1836.

LXXXIV. *Of the Conditions of Germination, in reply to* M. DeCandolle. *By the Rev.* P. Keith, *F.L.S.**

NOTHING can be so gratifying to an author as the commendation that comes from a critic of acknowledged talent and learning—"*laudatus à laudato viro.*" But we, the οἱ πολλοι of botanical scribblers, ought, perhaps, to rest satisfied, and to think ourselves very well off if a first- or second-rate wrangler in the science condescends to take notice of us, if it were but for the purpose of giving us a rap on the knuckles.

In my System of Physiological Botany published in 1816†, I enumerated five conditions as necessary to the process of the germination of the seed, and thought I had adduced good grounds for the said enumeration. Yet its accuracy has been impugned by a great botanist, and my five conditions reduced to three. I ought, perhaps, to submit in silence, and take in good part the correction of a great master; but as I am not satisfied of the soundness of the views of my corrector, I will venture to vindicate my original statement.—Proceed we now to the article itself.

I. The first condition necessary to germination is the maturity of the seed. Unripe seeds seldom germinate, because their parts are not yet prepared to form the chemical combinations on which germination depends. This fact M. De Candolle denies, saying that "M. Keith ne s'est pas exprimé avec precision lorsqu'il a posé la maturité de la graine, pour première condition générale et nécessaire à la germination"; and adding that Senebier and Treviranus succeeded in making green peas to germinate a short time before they were absolutely ripe‡. If M. DeCandolle had read to the end of the paragraph which he criticizes, he would have seen that the identical exception which he specifies is mentioned by

* Communicated by the Author.

† Vol. ii. p. 3.

‡ *Phys. Vég.* ii. 662.

Mr. Keith. He would have seen also that radish-seed, which M. Lefébure could not prevail upon to germinate till it was quite ripe, will germinate, when it pleases to do so, before that period arrives. If left long upon the stalk in a wet season it will germinate even in the pod. Also lemon-seed will sometimes germinate in the very centre of its pulpy pericarp, even before the fruit is cut open.

After all, we regard these apparent exceptions as amounting absolutely to nothing. The seeds were not ripe, it is true, in the common acceptation of the term, which supposes them to be as dry and as hard as a bone; but they were ripe in the physiological acceptation of it, and that is enough. The seed that will germinate is, physiologically speaking, ripe; that is, its fluids have been so elaborated in the process of its maturation, and its solids so vitalized in the assimilation of due aliment, as to be now fully and profitably susceptible of the action of the combined *stimuli* of the soil and atmosphere. Hence I contend, notwithstanding the objection of M. DeCandolle, that the maturity of the seed is rightly and legitimately placed in the list of the conditions of germination. I do not speak of the experiments of the chemist in his laboratory; I do not deny that a seed apparently unripe may germinate; but I speak of the operations of the farmer and of the gardener, and ask whether or not it would not be thought most absurd in them if they were to gather and sow their seeds in an unripe state?

II. The second condition necessary to germination, or at least to rapid and healthy germination, is the exclusion of light. The practice of the raking in of the grains, or seeds, sown by the farmer or gardener is founded upon this principle. But it does not seem to have engaged the notice of men of science, or to have been proved by direct and intentional experiment till lately. The first direct experiments that were instituted on this subject are those of Ingenhousz. He found that seeds germinate faster in the shade than in the sun, and hence concluded that light is prejudicial to germination. Senebier, who repeated the experiments of Ingenhousz, had the same result, and drew from them the same conclusion*. The prejudicial effect of light has been thought to be owing to its action on the carbonic acid gas contained in the seed, by which its oxygen is withdrawn too rapidly, its carbon fixed, its mass parched, and the possibility of its germination thus precluded.

But M. DeCandolle denies that the exclusion of light is necessary: "L'exclusion de la lumière est très-loin d'être,

* *Mem. Phys. Chem.* vol. iii. p. 341.

comme on la dit *, une des conditions nécessaires à la germination : il n'y a personne, en effet, qui n'ait vu des graines germer, quoique exposées à la clarté †." Yet this objection is equally invalid with the objection that was made to the maturity of the seed. I do not say that a seed may not germinate if left exposed to the light. I do not say that it may not be made to do so. But is that giving it a fair chance for early and healthy germination? Is that treating it in a way to bring all to a successful issue? For, again, I allude merely to the operations of the farmer and gardener, and not to the experiments of the chemist in his closet; though I am ready to admit that there is, perhaps, no rule without its exception; and on this ground it will be easy to find a flaw in almost any rule whatever. Suppose a writer on agriculture were to say that it is necessary for the cultivator who would farm well to keep his corn-fields clear of weeds; the truth of the rule might be denied by any one who was disposed to be captious. For he may turn round upon the rule-maker, and say,—No such thing! What you advance is not the fact, for I have seen many a good crop of corn in fields where the weeds stood higher than the corn itself. This may be all very true; but would it be a good and valid objection against the keeping of corn-fields clear of weeds? Certainly not. What then are we to think of the objections with which M. DeCandolle combats the accuracy of the above conditions of germination? For in the one case he admits that the grains selected for sowing should be the largest and the best nourished,—but how can they possibly be so, unless they are left upon the stalk till they are fully ripe?—and in the other case he does not deny that the exclusion of light is useful to germination, he only denies that it is necessary. But if it can be shown to be useful, we maintain that it is on that very account practically necessary.

III. A third condition of germination is the access of a certain degree of heat. No seed has ever been known to germinate at or below the freezing-point. Hence seeds do not generally germinate in winter, even though lodged in their proper soil. Yet the potential vitality of the seed is not necessarily destroyed by this exposure. For the seed will germinate still, on the return of spring, when the ground has been again thawed, and the temperature raised to the proper degree. This condition M. DeCandolle admits to be good.

IV. A fourth condition necessary to germination is the access of moisture. Seeds will not germinate if they are kept

* Keith, Phys. Bot., vol. ii. p. 5.

† *Phys. Vég.*, ii. 638.

perfectly dry. Water, therefore, or some liquid equivalent to it, is essential to germination. Hence rain is always acceptable to the farmer or gardener immediately after he has sown his seed; and if no rain falls, recourse must be had if possible to artificial waterings. But the quantity of water applied is not a matter of indifference. There may be too little or there may be too much. If there is too little, the seed dies for lack of moisture; if there is too much, the seed rots. This condition M. DeCandolle admits also to be good.

V. A fifth condition necessary to germination is the access of atmospheric air. Seeds will not germinate if placed in a *vacuum.* Ray introduced some lettuce-seeds into the receiver of an air-pump, which he then exhausted; but the seeds did not germinate. Yet they germinated upon the readmission of the air, which is proved by consequence to be necessary to their germination. Whether the whole of the ingredients of the atmospheric air are necessary, or only part of them, we do not at present inquire; but we are willing, with M. De Candolle, to limit the part necessary to oxygen.

Such are the five conditions which I enumerated as indispensable to the success of the process of germination; and I still persist in maintaining the correctness of the enumeration, notwithstanding all that M. DeCandolle has written about the germinating of immature seeds, or the non-necessity of the exclusion of light. By demolishing the two first of the above conditions M. DeCandolle leaves only three behind. But what would M. DeCandolle say if I were to come forward with an objection professing to demolish one of the three conditions which he has suffered to remain? The presence of water he regards as an essential condition. But I have actually known acorns to germinate, by protruding a radicle several inches in length, though lying in the corner of a dry granary where no water had access to them at all. Such an objection, undoubtedly, would be but frivolous and vexatious, —a light in which some might be apt to regard the objections which have been taken notice of above. For to say that the maturity of the seed and the exclusion of light are not necessary conditions of germination seems to be counter to the opinions whether of the practical cultivator, or of the speculative phytologist. What says Evelyn on the subject of the maturity of the seed; and of the exclusion of light?— "Choose your seed of that which is *perfectly mature*, ponderous, and sound." (*Sylva*, chap. i. sect. 2.) "Keep your newly sown seeds continually fresh, and in the shade, as much as may be, till they peep." (*Sylva*, chap. xxxii. sect. 2.) Now this is quite in keeping with the physiological doctrines of the present day, founded on facts that pneumatic chemistry, as applied to germination, has clearly demonstrated. "It is well known that seeds will not germinate in the light. This is caused by light decomposing the carbonic acid gas, expelling the oxygen, and fixing the carbon, whence all the parts become hardened,—a condition under which vegetation cannot proceed."—*Lindley's Introd. to Bot.* p. 270.

Still we regard the author of the criticism with feelings of the most unfeigned respect. But when it comes to be circulated at second-hand, and advanced by the plagiarists as an original remark of their own, without any intimation whatever of the source from which it came, we can only regard *them* with feelings of contempt. In a late botanico-veterinario-agricultural periodical, the criticism was excerpted from the work of M. DeCandolle, and published, in translation, as the actual *critique* of the editor or writer of the article, without the slightest reference to the original from which it was stolen. But as the periodical in question did not long survive the commission of this piece of plagiarism, and died a natural death (some say a violent death) at a very early age, we have nothing further to say on the subject. *De mortuis nil nisi bonum.*

Charing, Kent, Feb. 17, 1836. PATRICK KEITH.

LXXXV. *Experiments on the Action of Ammonia on the Chlorides and Oxides of Mercury, and on the Composition of White Precipitate.* By ROBERT KANE, *M.D., M.R.I.A.**

The paper of part of which the following is an abstract was read to the Royal Irish Academy at the October and November meetings of 1835, and will appear in the next part of the Transactions of that body. By permission of the Council the present abstract is sent for publication in the Philosophical Magazine.

BY the action of ammonia on a solution of corrosive sublimate there is produced a *White Precipitate*, which has been frequently made the subject of examination by chemists. Nevertheless there prevails considerable doubt as to its real nature. In these countries there has been adopted a theory, founded principally on the experiments of Hennell, that it consists of an atom of red oxide of mercury united to one of sal ammoniac: several other chemists, however, of equal eminence, as Soubeiran, Guibourt and George Mitscherlich, have each formed separate and discordant views. I trust it will be

* Communicated by the Author.

found that the results now brought forward will clear up these discrepancies and enable us to attain to a knowledge of its real nature.

In order to place in evidence the necessity that there exists for a reexamination of this subject, I shall arrange in a tabular form the results hitherto obtained. The first column contains the names of the chemists by whom the analyses of white precipitate have been made, the others the quantities of the constituents which were actually determined: one element, oxygen, which is only ascertained from the loss, has been omitted.

Authors.	Mercury.	Chlorine.	Ammonia.	Where described.
Fourcroy ...	74·1	13·2	6·03	Gmelin, Handbuch.
Hennell......	74·24	13·14	6·31	Quarterly Journal *.
Mitscherlich	76·37	13·82	7·1	Poggendorff, B. 85.
Guibourt	78·31	13·31	4·45	Thenard, Traité.
Soubeiran....	82·1	7·8	5·3	Jour. Phar., vol. xii.

To unravel the sources of error that had evidently led astray so many distinguished chemists was the object of my experiments.

By the theory of Hennell, generally adopted here, corrosive sublimate should give almost precisely its own weight of white precipitate; thus,

$$(2\,Cl + Hg) + 2\,N\,H^3 \text{ gives } . \{\ddot{H}g + (Cl\,N\,H^4)\} + Cl\,N\,H^4:$$

and 273·6 of sublimate gives 272·34 of white precipitate. The first set of experiments was made to ascertain how far that was consistent with the truth.

A. A solution of corrosive sublimate was decomposed by caustic ammonia, and the precipitate, carefully washed with cold water, was dried until preserving its pure white colour it ceased to lose weight. The liquor and washings were then acidulated with nitric acid and precipitated by nitrate of silver. The chloride of silver produced gave the quantity of chlorine abstracted from the sublimate. Of six experiments carefully conducted the mean was, from 100 of sublimate, 93·1 of white precipitate, and 13·0 of chlorine in the liquor.

But 100 grains of sublimate contain

Mercury... = 74·09
Chlorine... = 25·91;

we have therefore in white precipitate,

* An account of Mr. Hennell's experiments on this substance will also be found in Phil. Mag., First Series, vol. lxv. p. 226.—Edit.

In 93·1 grs.		In 100 grs.	
Mercury	= 74·09	Mercury	= 79·57
Chlorine	= 12·91	Chlorine	= 13·87

B. When white precipitate is heated, there is obtained, besides gaseous matter and watery vapour, all the mercury and chlorine united as calomel. This mode was now employed. A quantity of white precipitate, generally from 15 to 25 grains, was introduced into a very small tube retort, and heated cautiously until all the ammonia constituent had been expelled, along with whatever trace of watery vapour appeared, and the calomel completely sublimed *white*, which by a little care can be accomplished without any loss. Four experiments conducted in this way gave results very closely agreeing, of which the mean is,

From 100 of white precipitate, 92·98 of calomel, containing

Mercury... = 79·14
Chlorine... = 13·84.

C. The mercury was obtained in the metallic state by dissolving white precipitate in muriatic acid and decomposing by protochloride of tin. The mean result was 77·7 of mercury from 100 of white precipitate.

D. White precipitate was dissolved in muriatic acid and the liquor decomposed by a current of sulphuretted hydrogen. The sulphuret of mercury was collected on weighed filters, dried until it ceased to lose weight, and its quantity determined. The liquor was evaporated and the quantity of residual sal ammoniac ascertained. In this way the mercury and ammonia are both determined, and the mean result is, in 100 of white precipitate,

Mercury ... = 77·96
Ammonia ... = 7·16.

E. To obtain another value for the ammonia the following processes were used in addition. When white precipitate is boiled with a solution of sulphuret of barium, the mercury is all converted into sulphuret, there are formed chloride and oxide of barium, and ammonia is disengaged. The gaseous ammonia was passed over into a vessel of dilute muriatic acid, and the sal ammoniac obtained dry by evaporation. If a solution of iodide of potassium be digested on white precipitate, the quicksilver is converted into biniodide; and there being formed chloride and oxide of potassium, ammonia is set free, and its quantity determined as above. The mean of the results is that 100 of white precipitate contain 6·53 of ammonia.

F. In all theories of the composition of white precipitate hitherto advanced, oxygen is enumerated as a constituent to a very considerable amount, generally so much as to peroxidize the whole of the quicksilver. The results hitherto obtained in my experiments would not appear to leave room for so much oxygen, and I therefore endeavoured to obtain an estimate of the amount by direct experiment, on the following principle. When white precipitate is heated, there is obtained ammonia and azote, water and calomel, but no free oxygen. Therefore all the oxygen of the substance has been formed into water at the expense of the ammonia, and by collecting the water and determining its weight, the quantity of oxygen present could be ascertained. A small retort was blown of strong glass and a capacity of from 0·2 to 0·3 of a cubic inch; the neck being about two inches long. To this was tightly connected a small tube containing fused potash, and communicating by a small tube with the mercurial trough. The retort was carefully weighed and the white precipitate introduced, then weighed again. The desiccating tube was carefully weighed, and the apparatus having been connected tightly, heat was applied until the chlorine and mercury had all sublimed as calomel. The water was driven over completely into the potash tube, and the mixed azote and ammonia collected over mercury and analysed by water; the corrections for temperature and pressure, air of apparatus, and residual gas having been attentively applied. After the operation the retort was weighed. The residue was calomel, the loss water and gases. The desiccating tube was weighed; the increase of weight was water. There were obtained in four experiments the following quantities of water:

	Material.	Water.	Water per cent.
Exp. 1.	22·21	0·22	0·990
2.	20·47	0·14	0·684
3.	12·14	0·08	0·658
4.	19·42	0·00	0·000

Giving a mean of 0·583 per cent. of water.

Summing up all the means we obtain a final result for the composition of white precipitate, viz.

Mercury	=	78·60
Chlorine	=	13·85
Ammonia............	=	6·77
Water	=	0·58
Loss	=	0·20
		100·00

The ordinary view of white precipitate founded on the experiments of Fourcroy and Hennell give the following numerical results. $\ddot{\mathrm{Hg}} + \mathrm{Cl\,N\,H^4}$

Hg	=	202·8	=	74·46
2 Ox	=	16·0	=	5·88
Cl H	=	36·42	=	13·37
$\mathrm{N\,H^3}$	=	17·15	=	6·29

The fallacy of this view is completely proved by the great variation of the quantity of mercury and the absence of oxygen. Its language has been nevertheless adopted by George Mitscherlich, and has thus caused great confusion. He gives the name *Chlorwasserstoffsäure* to the hypothetic dry [anhydrous] muriatic acid, and his formula $(\mathrm{N\,H^3} + \ddot{\mathrm{M}}) + \ddot{\mathrm{Hg}}$ is so constructed. This is shown by the numbers for muriatic acid and ammonia, 10·7 and 7·1, which he says form sal ammoniac. In order to get his value for the chlorine in the table of results (p. 496), I added to his muriatic acid half the oxygen which he gives to the oxide of quicksilver. In fact his analysis correctly interpreted overturns the very hypothesis it was intended to support, for he obtained

	Mercury	... =	76·37
Muriatic acid...10·70 Oxygen .... 3·12	Chlorine	... =	13·82
	Ammonia...	=	7·10
			97·29

Leaving a vacancy of only 2·71 for all the oxygen to oxidize the whole of the quicksilver.

The simplest view to take of the existence of the chlorine in this substance, is to suppose it united with half the mercury into corrosive sublimate. It is almost the only view which agrees with its reactions. Then in what state is the remainder of the mercury? We may suppose it peroxidized, and the oxide united with the ammonia, giving the formula $(2\,\mathrm{Cl} + \mathrm{Hg}) + (\ddot{\mathrm{H}} + 2\,\mathrm{N\,H^3})$ and the following numerical results.

2 Hg	=	405·60	or, per cent.	77·00
2 Cl	=	70·84	———	13·45
2 Ox	=	16·00	———	3·04
$2\,\mathrm{N\,H^3}$	=	34·30	———	6·51.

This agrees closely with Mitscherlich, and also with some of my own analyses. It differs nevertheless from the mean of my results in the quantities of mercury and chlorine, and particularly in the amount of oxygen; this hypothesis supposing the presence of 3·04 per cent. of oxygen,—a body the existence of which as a constituent I could not determine at all, and which the quantities of the other constituents would appear absolutely to exclude. It is therefore necessary to examine whether any other method of arrangement is more satisfactory.

From the researches on oxamide, benzamide, &c. it follows, that by the action of ammonia on an oxide there may be formed water and a compound of the body $N H^2$ with the base of the oxide. If we consider this to have taken place in white precipitate, we should have the formula

$$(2\,Cl + Hg) + (2\,N H^2 + Hg), \text{ giving}$$

$2\,Hg$	=	405·60	or, per cent.	79·73
$2\,Cl$	=	70·84	———	13·93
$2\,N H^2$	=	32·30	———	6·34;

and this compound should yield on analysis, on being decomposed, 6·73 per cent. of ammonia.

The question whether ammonia in acting on metallic oxides forms water and metallic amides is one of the most interesting now requiring to be examined; but notwithstanding the bearing the results just described have on the question, I do not wish to adopt too positively the opinion that white precipitate is a compound of deutochloride and deutamide of mercury until more extended researches shall enable me to argue from a greater number of facts derived from the reactions with other metals.

LXXXVI. *Researches in the Undulatory Theory of Light, in continuation of former Papers.* By John Tovey, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I NOW proceed, as I proposed at the conclusion of my last paper, p. 272, to integrate the equations

$$\frac{d^2 \xi}{d t^2} = s^2 . \frac{d^2 \xi}{d x^2} + s'^2 . \frac{d^4 \xi}{d x^4} + \&c.$$

$$\frac{d^2 \eta}{d t^2} = s_{\prime}^2 . \frac{d^2 \eta}{d x^2} + s_{\prime}'^2 \frac{d^4 \eta}{d x^4} + \&c. \qquad (1.)$$

$$\frac{d^2 \zeta}{d t^2} = s_{\prime\prime}^2 . \frac{d^2 \zeta}{d x^2} + s_{\prime\prime}'^2 \frac{d^4 \zeta}{d x^4} + \&c.$$

As ξ is, by supposition, a function of x and t, we can put

$$\xi = p \sin k x + p' \sin k' x + p'' \sin k'' x + \&c.$$
$$+ q \cos k x + q' \cos k' x + q'' \cos k'' x + \&c.$$

where p, p', p'', &c., q, q', q'', &c. are functions of t, and k, k', k'', &c. arbitrary constants. (Poisson, *Traité de Mécanique*, No. 514.) Now, if we substitute $p \sin k x + q \cos k x$ for ξ in the first of the equations (1.), it becomes

$$\left\{\frac{d^2 p}{d t^2} + \left((s^2 k^2 - s'^2 k^4 + \&c.\right) p\right\} \sin k x$$

$$+ \left\{\frac{d^2 q}{d t^2} + \left(s^2 k^2 - s'^2 k^4 + \&c.\right) q\right\} \cos k x = 0;$$

and, this equation being true for all values of x, resolves itself into

$$\frac{d^2 p}{d t^2} + \left(s^2 k^2 - s'^2 k^4 + \&c.\right) p = 0,$$

$$\frac{d^2 q}{d t^2} + \left(s^2 k^2 - s'^2 k^4 + \&c.\right) q = 0.$$

The complete integrals of these equations are $p = \mathrm{A} \sin n t + \mathrm{B} \cos n t$, $q = \mathrm{A}' \sin n t + \mathrm{B}' \cos n t$; where A, B, A′, B′, are arbitrary constants, and $n = \sqrt{(s^2 k^2 - s'^2 k^4 + \&c.)}$. Hence the complete integral of the first of the equations (1.) can be expressed by the sum of a series of functions, each of which is of the form

$$(\mathrm{A} \sin n t + \mathrm{B} \cos n t) \sin k x + (\mathrm{A}' \sin n t + \mathrm{B}' \cos n t) \cos k x.$$

This expression is, by the rules of trigonometry, equivalent to

$$\frac{\mathrm{A} + \mathrm{B}'}{2} \cos (n t - k x) + \frac{\mathrm{A}' - \mathrm{B}}{2} \sin (n t - k x)$$

$$- \frac{\mathrm{A} - \mathrm{B}'}{2} \cos (n t + k x) + \frac{\mathrm{A}' + \mathrm{B}}{2} \sin (n t + k x);$$

which, if we put

$$\frac{\mathrm{A} + \mathrm{B}'}{2} = \alpha \sin a, \quad \frac{\mathrm{A}' - \mathrm{B}}{2} = \alpha \cos a, \quad \frac{\mathrm{A} - \mathrm{B}'}{2} = - \beta \sin b,$$

$$\frac{\mathrm{A}' + \mathrm{B}}{2} = \beta \cos b,$$

may, by the same rules, be reduced to

$$\alpha \sin (n t - k x + a) + \beta \sin (n t + k x + b).$$

It follows, therefore, that since the second and third of the equations (1.) are of the same form as the first, and since η and ζ are, like 1, functions of x and t, we can express the complete integrals of these equations by putting

$$\begin{aligned}\xi &= \Sigma . \alpha \sin (n t - k x + a) + \Sigma . \beta \sin (n t + k x + b), \\ \eta &= \Sigma . \alpha_{,} \sin (n_{,} t - k_{,} x + a_{,}) + \Sigma . \beta_{,} \sin (n_{,} t + k_{,} x + b_{,}), \quad (2.) \\ \zeta &= \Sigma . \alpha_{,,} \sin (n_{,,} t - k_{,,} x + a_{,,}) + \Sigma . \beta_{,,} \sin (n_{,,} t + k_{,,} x + b_{,,});\end{aligned}$$

where

$$\begin{aligned}n &= \surd (s^2 k^2 - s'^2 k^4 + \&c.), \\ n_{,} &= \surd (s_{,}^2 k_{,}^2 - s'^2_{,} k_{,}^4 + \&c.), \quad (3.) \\ n_{,,} &= \surd (s_{,,}^2 k_{,,}^2 - s'^2_{,,} k_{,,}^4 + \&c.):\end{aligned}$$

the sums Σ being extended to all the requisite values of the arbitrary constants.

We perceive, by the equations (2.), that the motion of the system may be regarded as compounded of a number of coexisting movements, severally expressed by the terms of the sums Σ. And when we confine our attention to a single term of the first sum in one of these equations, which we may do in a great variety of problems, we have virtually the same expression for the displacement of a molecule of an undulating medium, as is assumed tacitly by Sir Isaac Newton, and expressly by Professor Airy*.

Taking separately the displacement η, and considering only one term of the first sum in the expression for this quantity, we have

$$\eta = \alpha_{,} \sin (n_{,} t - k_{,} x + a_{,}).$$

It is well known that this equation represents a series of equal and continuous waves, the length of each wave being $\frac{2\pi}{k_{,}}$; where 2π is the circumference of a circle whose radius is unity. Now, if we increase x uniformly, so as to make $n_{,} t - k_{,} x$ constant, η remains constant, and $\frac{dx}{dt} = \frac{n_{,}}{k_{,}}$. Hence we perceive that these waves travel, in the direction of x positive, with a velocity equal to $\frac{n_{,}}{k_{,}}$. If $\eta = \beta_{,} \sin (n_{,} t + k_{,} x + b_{,})$, which is a term of the second sum, the movement is similar, except that the waves travel in the contrary direction.

The second of the equations (3.) gives

$$\frac{n_{,}}{k_{,}} = s_{,} \surd \left(1 - \frac{s'^2_{,}}{s_{,}^2} k_{,}^2 + \&c.\right);$$

an equation affording the same theory of dispersion as that which has been so satisfactorily investigated and verified by

* See Airy's Mathemat. Tracts, p. 255.

Professor Powell in the recent Numbers of your Journal. This I have shown more explicitly in your Number for January last, p. 7.

Since, by the last equation, the velocity of the waves, and consequently the refraction of the light at the surface of the medium, depends chiefly upon $s_{\prime}$, while the dispersion depends upon $\frac{s_{\prime}'^2}{s_{\prime}^2} k_{\prime}^2$ and the following terms of the series, we see that the dispersion may be different for different media, though the mean refraction be the same; contrary to the opinion which so long retarded the improvement of refracting telescopes.

The equations (3.) may, perhaps, lead to a theory of absorption as well as of dispersion; since it is obvious that they may become impossible for particular values of k. It should be observed that the sums s^2, $s_{\prime}^2$, $s_{\prime\prime}^2$, s'^2, &c. are not necessarily positive, and I now think it would be better to denote them by s, $s_{\prime}$, $s_{\prime\prime}$, s', &c. I adopted the other notation in order to assimilate the formulæ to those employed in the theory of sound.

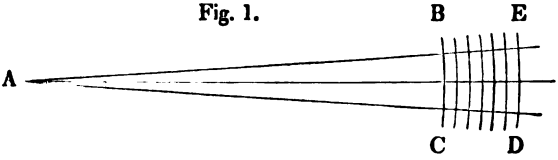
In the case of undulation which we have been considering, the waves are plane waves, perpendicular to the axis of x; we now pass on to the consideration of converging and diverging waves.

Let us take the case of a system of waves going and returning to and from a certain point; calling this point the *centre of agitation*. Then the diameter of the sphere of influence of any molecule being an insensible quantity, it is evident that the minute portion of one of the waves contained within the sphere cannot, at any sensible distance from the centre of agitation, differ sensibly from the same portion of a plane wave. Therefore, as the motion of any molecule is affected only by the molecules within the sphere of its influence, it follows that the equations (3.), which give the velocities $\frac{n}{k}$, $\frac{n_{\prime}}{k_{\prime}}$, $\frac{n_{\prime\prime}}{k_{\prime\prime}}$, of plane waves, will also give, at any point of the system, the velocities with which diverging or converging waves are transmitted in the direction perpendicular to the wave-surface at that point.

When the molecules are so arranged that the sums s^2, $s_{\prime}^2$, $s_{\prime\prime}^2$, &c. are the same for all directions of the rectangular coordinates, the velocities of the waves are the same for every radius drawn from the centre of agitation; and consequently the wave-surfaces are spherical.

If we conceive a slowly tapering cone (fig. 1.) to have its

Fig. 1.



summit A at the centre of agitation of a system of spherical waves, and if we take the axis of the cone for the axis of x, it is clear that the displacements ξ, η, ζ, of the molecules within the frustum B C D E may be regarded as functions of x and t; and may therefore be expressed by the equations (2.), nearly. It is also manifest that the same equations will express the displacements for any other frustum of the medium, by making the arbitrary quantities to vary according to the position of the frustum. Consequently, if we suppose

$$\xi = \alpha \sin (n t - k x + a)$$

for the frustum B C D E, the same equation may be taken to express the value of ξ for any other frustum of the *same cone*, by regarding α, n, k, a as functions of x.

Let ϱ be the radius of the sphere of influence of the molecules: then, if $\frac{\varrho}{x}$ were infinitely small, the minute portion of a wave contained within the sphere would be a plane wave, and α, n, k, a constant. Hence we perceive that these quantities must be functions of $\frac{\varrho}{x}$; and consequently, that we may write

$$\alpha = \mathrm{A} + \mathrm{B}\ \frac{\varrho}{x} + \mathrm{C}\left(\frac{\varrho}{x}\right)^2 + \&c.,$$

$$n = \mathrm{A}' + \mathrm{B}'\ \frac{\varrho}{x} + \mathrm{C}'\left(\frac{\varrho}{x}\right)^2 + \&c.,$$

$$k = \mathrm{A}'' + \mathrm{B}''\ \frac{\varrho}{x} + \mathrm{C}''\left(\frac{\varrho}{x}\right)^2 + \&c.,$$

$$a = \mathrm{A}''' + \mathrm{B}'''\ \frac{\varrho}{x} + \mathrm{C}'''\left(\frac{\varrho}{x}\right)^2 + \&c.:$$

the only variable quantity in these series being x.

Now when x is infinite α must be zero; therefore $\mathrm{A} = 0$: and as $\frac{\varrho}{x}$ is, at all sensible distances from the centre of agitation, an extremely small quantity, we may reject its powers above the first; therefore $\alpha = B\frac{\varrho}{x}$. The quantities n, k, a approach, as x increases, towards the values which they have in the case of plane waves, which values are independent of α. And since the small portion of a wave contained within the sphere of influence of any molecule cannot, at any sensible distance from the centre of agitation, differ sensibly from the same portion of a plane wave, we may regard n, k, a as constant for all parts of the cone. If then we retain α to denote $B\varrho$, the constant part of $\frac{B\varrho}{x}$, we have

$$\xi = \frac{a}{x}\sin(nt - kx + a):$$

and, in general, for any cone taken as we have supposed, we have, from the equations (2.),

$$\xi = \Sigma.\frac{\alpha}{x}\sin(nt-kx+a)+\Sigma.\frac{\beta}{x}(\sin nt+kx+b),$$

$$\eta = \Sigma.\frac{\alpha_{,}}{x}\sin(n_{,}t-k_{,}x+a_{,})+\Sigma.\frac{\beta_{,}}{x}\sin(n_{,}t+k_{,}x+b_{,}), \quad (4.)$$

$$\zeta = \Sigma.\frac{\alpha_{,,}}{x}\sin(n_{,,}t-k_{,,}x+a_{,,})+\Sigma.\frac{\beta_{,,}}{x}\sin(n_{,,}t+k_{,,}x+b_{,,}).$$

When the waves all move *from* the centre of agitation, the second sums in the equations (4.) will vanish: and limiting our view to a single term of one of the first sums, we have an expression for the displacement virtually the same as that which Professor Airy, in his valuable tract on the Undulatory Theory of Optics, has partly assumed and partly borrowed from the theory of sound*.

It may be observed, by the way, that the method adopted in this paper of expressing the displacement of the molecules, is analogous to that employed so successfully in physical astronomy to express the differences between the mean and true places of the planets.

When the molecules are so arranged that the sums s^2, $s_{,}^2$, $s_{,,}^2$, &c. are different for different directions of the coordinates, waves going and returning to and from a centre of agitation will not be spherical. The most simple case of such waves will probably furnish a subject for another paper.

I am, Gentlemen, yours, &c.

Evesham, April 15, 1836. JOHN TOVEY.

P.S. I perceive that throughout my last paper I inadvertently called the differences Δx, Δy, Δz variations.

* Mathemat. Tracts, p. 271.

LXXXVII. *On the former Extent of the Persian Gulf, and on the Non-identity of Babylon and Babel; in Reply to* Mr. Carter. *By* C. T. Beke, *Esq., F.S.A.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

The opinion which was, in the first instance, advanced by me in the Number of your Journal for February 1834*, was to the effect that the low lands of the Euphrates and Tigris have been formed by the gradual deposits of those rivers, and that this operation has been so extensive, that, at the time of the erection of the Babel of Genesis, it must have been physically impossible for that city to be built near the spot where the Babylon of Nebuchadnezzar afterwards stood. This opinion may be considered as embracing two distinct and separate propositions:—the first is that, within the period of history, an advance of the land upon the sea has taken place of sufficient importance to affect materially the geography of the localities in question; the second is that, within the same period, that advance has been so great as (independently of all other arguments,) to warrant my conclusion with respect to the non-identity of Babylon and Babel. If the former of these propositions be untrue, *à fortiori* must the latter be so; but, on the other hand, even if the former be established, it does not follow that the latter is likewise correct.

From Mr. Carter's former arguments I certainly was led to consider, that he not merely disputed the correctness of the first proposition to its entire extent, but that he went yet further, and contended that the changes (if any) which have taken place, are altogether insignificant. In his present remarks he says, however †, "I much object to such expressions in the reply as, 'Mr. Carter has, in fact, asserted the opinion that, since the time of Nearchus, the encroachments on the gulf must be very unimportant,' omitting the words '*to the point in question*, any later encroachments,' &c., as conveying the idea of a mere assertion without proof, and a much broader one than my remarks warrant." I am most anxious that no difference should exist between us on the score of mere misconception of each other's meaning, and I therefore give at length, in the note at foot, an extract of the whole passage from which I made my citation ‡; and I put it to the candour

* Lond. and Edinb. Phil. Mag., vol. iv. p. 108—111.

† *Supra*, vol. vii. p. 195.

‡ "Following the course of Nearchus, as given in his own clear account of the voyage preserved by Arrian, from his arrival at the Arosis, the river

of my opponent himself, whether I was not fairly authorized in the conclusion which I came to with respect to his meaning: indeed I would ask whether, when in his present reply he says, with respect to "the navigation of Alexander and his fleet in the delta streams," that "the ancient canal, the entire circuit, all the points of the navigation then presented by the spot, are still offered for our observation," it must not be understood as his *unqualified* opinion that "since the time of Nearchus the encroachments on the gulf have been very unimportant." If I am so unfortunate as still to misunderstand his meaning, I beg to assure him that I do so most unintentionally.

As regards the observation that my words "convey the idea of a mere assertion without proof," Mr. Carter must allow me to say, that a construction appears to be put upon them which ought not by any means to be adopted in a discussion like the present. Every proposition advanced, or assertion made, on either side, must be presumed to be made upon what are regarded as "proofs;" and it is simply from the considered insufficiency of those *alleged* proofs that the correctness of any such proposition or assertion is questioned on the other side. For my part, I feel that I might have reason to object, not merely to some expressions, but also to the tone generally in which Mr. Carter's last reply is written; but I refrain from doing so, and I sincerely trust that neither of us will have occasion again to refer to any such unpleasant topic.

In order to prevent any future misconception, it is to be understood that the first and principal point in dispute between us is, whether a change of such importance has taken place as *materially* to affect the geography of the localities in

at the N.E. next before coming to the streams of the Delta, in his progress to Kataderbis and the island of Margastana, in his passage through the channel over the shoals to his arrival at Diridotis (by the Khore Abdallah), on the S.W. side of the Delta, and comparing it with the present state of the country, we learn with surprise the small degree of change which the general characters of the coast have undergone during the lapse of so many ages. Dr. Vincent, in his able work on the Commerce and Navigation of the Ancients in the Indian Ocean, adverting to this remarkable fact, observes, that Capt. Howe's chart 'explains the journal of Nearchus as perfectly as if it had been composed by a person on board of his fleet,' (vol. i. p. 423.) and (p. 466.) 'the pilot on board Nearchus's ship steered exactly the same course' (*along the coast of the Delta*) 'as MacCluer's Karack pilot 2000 years afterwards.' The junction of the river called by Arrian the Eulæus (coming from the N. or N.E.) with the Tigris by the still existing ancient Hoffar canal, across which Alexander sent a part of his fleet while he sailed down the Eulæus to the mouths of the Tigris, and so round to meet it (Arrian, Exp. Alex. vii. 7.) further shows that to the point in question any later encroachments on the gulf must be very unimportant." Lond. and Edinb. Phil. Mag., vol. v. p. 247—8. The Italics are Mr. Carter's.

question; that is to say, a change so great as to render the descriptions of ancient writers inapplicable to the *actual* coast-line and state of the neighbourhood generally.

Seeing that my hypothesis precludes the possibility of Nearchus's voyage being made applicable to the present coast of Susiana and the countries at the head of the Persian Gulf, it is scarcely necessary for me expressly to dispute in detail the correctness of the identifications, considered to have been established by Dr. Vincent, of the river Arosis, of Kataderbis, the island of Margastana, Diridotis, &c. &c.* That the river Karoon is not the Eulæus, nor Shuster the representative of Susa, has already been asserted by many geographers of eminence, whose voices are united in favour of Shus and the river Haweeza or Kerrah. Without intending to range myself with these geographers, I believe I am correct in saying that, as between them and Dr. Vincent, the greater show of reason is

* Although I am quite willing to concede that "a few miles of addition to the Delta is not the question" between us, yet, as regards the learned Dean's identifications, I must remark, that a few miles—nay, *a very few* miles indeed—of addition would (I much suspect,) render it impossible that "Capt. Howe's chart should explain the journal of Nearchus as perfectly as if it had been composed by a person on board of his fleet." To establish the correctness of this position, it appears to be necessary, not merely that the coast should have remained unvaried since the time of the Greek navigator, but *that Capt. Howe's chart should accurately represent that coast*: it ought, consequently, to correspond in all points with the trigonometrical survey recently made by Lieuts. Brucks and Haines, of the East India Company's Marine Service.

By the kindness of Capt. Horsburgh I have been furnished with copies of the Company's chart, as also of that of Lieut. MacCluer (by Dalrymple, 1786 and 1788): Capt. Howe's he was not in possession of. Owing to the longitude not being marked in MacCluer's, I am prevented from making an exact comparison of these two charts; still differences of sufficient moment are to be detected between them. For instance, the island of Karack is represented by MacCluer as being 8, and Korgo more than 4 geographical miles long, whereas they are actually just half those lengths respectively: Buna (Derabuna), by the Core Moosah, is made as much as 9 miles long, from north to south, and 3 miles broad, whilst it is only 3 miles long, and less than 1 mile broad, its length being from east to west: Derah, adjoining this last island, is made 7 miles long and 3 miles broad, but it is in fact only a mile and a half each way: the Core Abdallah, represented in the copy of 1786 as being 10 miles broad, with 8 miles of coast between it and the mouth of the Bussorah river, and in that of 1788 as only 6 miles broad, with about 10 miles of coast, is actually 12 miles broad, and the two mouths meet at a point, without any coast intervening. These variations (which are only a portion of what might be pointed out,) may be said to be but trifles with respect to "the general characters of the coast;" still they are more than sufficient to show that MacCluer's chart would have been rather a dangerous guide for Nearchus to have placed implicit confidence in. Capt. Howe's chart, which was adopted by Dr. Vincent, is (I believe) not even so correct as that of MacCluer; but I have not at present the means of referring to the Dean's work, so as to ascertain this positively.

on their side. Under their hypothesis, however, Charax, which was situate at the confluence of the Tigris and Eulæus, will have to be placed not 37 but about 100 miles up the river; so that "the plain fact" by which "even the increase of 35 miles" in the distance of that city from the sea is "annihilated" in so summary a manner, is not quite so manifest. The position of Charax remains, I conceive, yet to be determined; but, let it have been where it may, I confess I do not exactly understand how my "extravagant hypothesis" is to be "at once disposed of," for the reason that, "if the distance of Charax, the port, had increased but 70 miles" (or it may be only 35 miles,) between the times of Alexander and Pliny (400 years), "the whole distance to Babylon could have increased but 70" (or 35) miles in the 2160 years which have elapsed since the voyage of Nearchus down to the present time. Leaving the "extravagant hypothesis" quite out of the question, it appears to me that, assuming the same rate of increase throughout the whole period, the gain would be about 380 (or 190) miles.

The "diversitas auctorum" of which Pliny complains, is a point upon which Mr. Carter makes a great stand; and hence he comes to the strange conclusion, that the distance between Babylon and Charax was "utterly uncertain." Now it may be perfectly comprehensible that the naturalist should have been in difficulty upon the subject, and unable to arrive at any satisfactory result, on account of the apparent discrepancies among the various authorities which were before him; yet it will not, I presume, be thence argued, that either Babylon or Charax was so situate as not to have been perfectly easy of access, so that the distance between them might always have been ascertainable, in the same way as it would be in the present day (and perhaps with less difficulty,) were both cities in existence. There is not the slightest reason, therefore, for imagining that "the distance was *utterly uncertain*." The various authors must be presumed to have made their several statements upon good grounds, and with a competent knowledge of the actual distance; and whatever discrepancies may be found among them, beyond those which will always exist where distances are only *estimated* and not actually measured, are mainly, if not entirely, to be attributed to differences in the standards of measurement employed by them respectively. And this, in fact, is what Pliny himself says: "Inconstantiam *mensuræ*" (the *measure* itself and not the *distance measured*,) "diversitas auctorum facit: *cum Persæ quoque schœnos et parasangas, alii alia mensura determinent* *." This difficulty be-

* *Hist. Nat.*, lib. vi. cap. 27.

comes no slight one when, as was frequently the case, those standards of measurement, although of widely different lengths, *had the same name**; added to which, we must bear in mind that the various distances recorded were, at various times, applicable to different states of the country, in those portions of it which were liable to change. The possible existence of errors of copyists is, of course, not to be lost sight of; but I question much whether we may be authorized to entertain "serious doubts of the authenticity" of passages which do not exactly coincide with our preconceived notions.

In his former paper† Mr. Carter cites various authorities in illustration of the passage from Pliny, in part originally quoted by me‡; which passage he understands (though I cannot conceive how,) to mean that "long before Pliny's time the two rivers *had united* above the embouchure somewhere, not by encroachments on the gulf and formation of delta, but *simply by the labour of hands*;" and in his present reply that gentleman repeats that those various authorities "all harmonize with the *unbroken* sense of this passage:" meaning, of course, as it is interpreted by him. I confess that in my last answer I dismissed these authorities rather summarily, and I did so on account of my not being able to discover their application, and on account also of the "discrepancies" existing among them, which my opponent himself admitted §. And on this point an explanation is due from me to Mr. Carter. In your Number for June last (1835), I stated that "these authorities, according to his (Mr. C.'s) admission, contain 'some discrepancies,' and are not always 'very explicable,'" in which I was thus far wrong: the being not "very explicable" was (as he now observes,) "distinctly applied by him *to Pliny's general account of the two rivers only*," the "discrepancies" having

* We have a precisely analogous case in the various *miles* of the present day, and we may easily conceive the case of a geographer in future ages being strangely perplexed on this account. Take, for instance, the distance between St. Petersburg and Riga, which by a Swede would be said to be 50 miles; by a German, 71 miles; and by an Englishman, 285 or 330 miles; whilst a Frenchman would call it 95 or 118 leagues, and a Russian 495 wersts; to which might be added, perhaps, twenty other measures of modern Europe (principally *miles*), all differing with one another. Here would be ample ground for complaining, as Pliny did, of the "inconstantia *mensuræ*," but certainly none for the conclusion that "the distance was utterly uncertain."

† Lond. and Edinb. Phil. Mag., vol. v. p. 249.

‡ "Inter duorum amnium ostia 25 mill. pass. fuere, aut (ut alii tradunt) 7 mill. utroque navigabili. Sed longo tempore Euphratem præclusere Orcheni, et accolæ agros rigantes; nec nisi Pasitigri defertur in mare."—*Hist. Nat.*, lib. vi. cap. 27.

§ "But notwithstanding some discrepancies, the conclusion from the above authorities surely is," &c.

been intended by him (as will be seen from the last note,) to apply *merely to the rest of the authorities cited by him.* I am most happy to be able thus to correct my error.

As regards these various "harmonizing," "discrepant" authorities, I even now refrain from considering them in detail; for it would only needlessly be taking up much room, since my remarks would be little more than the continued repetition, with respect to each of them individually, of the assertion which I make respecting them collectively; namely, that I am unable to see their applicability, either to Pliny's statement as above explained by Mr. Carter, or to the present condition of the country. It will not be denied that the general conclusion from them is, that the two rivers in question "have, at a very early period, united inland *somewhere*;" but I cannot conceive by what possible means the further conclusion is to be arrived at from them, that "Khorna was the grand confluence in all ages*;" for the two rivers may, by the union of their deltas, have formed a junction at some point much further inland, and yet, for ages afterwards, have still continued their (in part) separate courses to the sea.

Among the many writers thus cited by Mr. Carter, is the geographer Ptolemy, to whom, however, whilst he quotes the particular passages from the other authors which he considers applicable, he refers only in general terms. Yet Ptolemy's description of these rivers, and the countries through which they flow, is that, perhaps, which is the most important of the whole, and which, consequently, requires to be more particularly considered. The purport of this description appears to be as follows: That to the north of Babylon the Euphrates divided itself into two streams, whereof the one flowed southward by that city, and the other eastward past Seleucia: that between these two branches of the Euphrates there was a river called the Basilius, which, on the one hand, fell into the Tigris below Apamea, and, on the other hand, joined the main stream of the Euphrates flowing past Babylon, at some distance below that city: that the Euphrates likewise threw off an arm called the Baarsares; and that both this arm and also the main stream itself, continued their courses southward, and divided themselves into several subordinate branches, with which they formed lakes and marshes towards the head of the Persian Gulf†.

The Alexandrian philosopher's account must, of course, be

* How does such a conclusion tally with the notion that the Orcheni "*united*" the two rivers "*simply by the labour of hands*"? Did they make the junction at Khorna?

† Ἡ τοῦ Εὐφράτου θέσις, καθ' ἣν σχίζεται εἴς τε τὸν διὰ Βαβυλῶνος

taken with all the imperfections in geographical knowledge belonging to his age; but the whole context affords a manifest indication that, so late as about his time, (the beginning of the second century of our æra,) the Euphrates possessed its separate delta, of which the apex was above Babylon, and of which the western branches formed lakes and marshes below that city; whilst (although the junction is not mentioned,) the most eastern branch, as it passed by Seleucia, must have joined the Tigris. The outlet of the lakes and marshes into the sea is also not described; in fact, as Pliny tells us, it was already closed up by the Orcheni: but the authorities cited by Mr. Carter, as also Herodotus to whom I shall presently refer, plainly show that, at an earlier period, the delta streams of the Euphrates had their separate union with the Persian Gulf. In my last paper* I attempted to show how these lakes and marshes at the mouths of the Euphrates would, in the first instance, have been produced, and how, subsequently, the branches of the river which formed them would successively have been stopped and filled up by the operation of natural means, the western branches being those which were first closed. The Orcheni would have finished the work of nature by stopping up the eastern arm, which, till then, discharged itself into the sea, not more than 25 or 27 miles (as stated by Pliny,) from the western mouth of the Tigris; and the lakes and marshes of the Euphrates, having no longer a channel through them, would then gradually have become silted up, in the manner I have further suggested in the same paper. Much light would be thrown upon the subject if, by local examination, it were determined (which it might be without much difficulty,) how far westward the course of any branch of the Euphrates has once extended †.

ῥέοντα, καὶ τὸν διὰ Σελευκείας, ὧν ὁ μεταξὺ καλεῖται Βασίλειος ποταμὸς, οὗ θέσις τῆς ἐκτροπῆς μοῖρας .... οθ λι γ΄.—Lib. v. cap. 18.

Ἀπάμεια .... οθ ς΄ λδ ς΄.

ὑφ' ἣν ἡ τοῦ Βασιλείου ποταμοῦ πρὸς τὸν Τίγριν συμβολὴ, ἐγγὺς μέση χώρα. —Ibid.

Διαῤῥέουσι δὲ τὴν χώραν ὅτε Βασίλειος ποταμὸς, καὶ ὁ διὰ τῆς Βαβυλῶνος ῥέων, καὶ ὁ καλούμενος Βααρσάρης· ὃς τῷ μὲν Εὐφράτῃ συμβάλλει, κατὰ θέσιν ἐπέχουσαν μοί.. οθ λδ ∫ο΄.

Τῷ δὲ διὰ Βαβυλωνίας, ὃς καλεῖται ὁ Βασίλειος ποταμὸς συνάπτων.

οθ λδ γ΄.

Ποιοῦσι τὲ οἱ ποταμοὶ οὗτοι, καὶ αἱ ἀπ' αὐτῶν ἐκτροπαὶ λίμνας καὶ ἕλη, ὧν καὶ αὐτῶν τὸν μεταξὺ ἐπέχει μοί............ οη ς΄ λβ ς΄.—Lib. v. cap. 20. *Edit. Basil.* 1533.

* Lond. and Edinb. Phil. Mag., vol. vii. p. 45.

† The most eastern branch of the Euphrates, which joined the Tigris above Babylon, would appear also to have become closed, unless indeed it

At the present moment (as I have before remarked,) I do not consider that our present knowledge of the countries in question is sufficient to enable us to come to any entirely satisfactory conclusion, or to reconcile the various apparently conflicting statements of antiquity, which evidently cannot be made to apply (under favour of Mr. Carter must it be said,) to the present state of the country, and which it will require much labour and not less caution to adapt to any hypothetical condition of the country. But one point, which is not sufficiently attended to by commentators generally, cannot be too strongly borne in mind by those who may apply themselves to the task. It is, that where *a fact* is expressly asserted by a writer of character, who possessed the means of knowing it, *its correctness must be admitted, until something positive be alleged sufficient to invalidate it.* Mr. Carter appears entirely to neglect this rule, when he cites Arrian as "saying expressly, the Euphrates has a higher channel than the Tigris, which receives the waters of the Euphrates by many streams," and yet, without hesitation, stigmatizes this an "error." Perfectly true it may be, as Col. Chesney reports, that, in the present day, "the Tigris gives a large contribution to the sister stream by the canal of the Hie, about 220 miles above the gulf;" but may it not be equally true, that formerly the two rivers united much higher up, at a point at which their relative levels were as Arrian *so expressly* states them to have been? The mere circumstance that the river Al Huali or Hermas, which at the present day runs in a direction towards the west so as to unite with the Khabour, is considered to have had in former times an eastward course and to have joined the Tigris *, is in entire accordance with such a state of things.

Mr. Carter says, "Xenophon understood this better [than Arrian]: he mentions four canals *by which the latter* [the Tigris] *pours its waters into the Euphrates* †." Did Xenophon really say this, I should be compelled to admit his testimony, as that of a man of unquestioned honour and integrity *and an eye-witness*, even in spite of the express assertion of Arrian to the contrary; but it is far from being the case, and Mr. Carter has evidently been misled from consulting merely some loose

was kept open by artificial means, in which case it would, in the result, have been regarded merely as *a canal.*

* See Rennell's Illustrations of the Retreat of the Ten Thousand, p. 102; see also *Orig. Bibl.*, p. 113, where the opinion is expressed that "at the time when the extent northward of the Persian Gulf was much greater than it is at present......the river Al Huali had its separate course to the sea."

† *Anab.*, lib. i. cap. 7.

and inaccurate version. A reference to the original would have shown him that what the author really says respecting these four canals is simply εἰσβάλλουσι δὲ εἰς τὸν Εὐφράτην: correctly rendered in the Oxford version (edit. 1676), "Iidem in Euphratem *influunt*," and by Spelman "they *fall into* the Euphrates." Smaller streams are commonly said to *fall into* larger ones with which they communicate, so that these words do not *necessarily* convey any idea beyond that of *mere union*; and the writer being near the Euphrates (see the next paragraph of the text,) would naturally describe these canals as tributaries to that river, even had the actual run of the waters been in the other direction. Seeing, however, that these canals were *navigable*, and that they were of course made without locks, it is manifest that no great difference of level between the two rivers could have existed; and whichever way it may have been, most assuredly there was not, in a country which to this day is almost a dead flat, any opportunity for the one river to "pour its waters into" the other.

Mr. Rich tells us in his Memoir on the Ruins of Babylon (2nd edit. p. 18), that during the inundation of the Euphrates "rafts laden with lime are brought almost every day from Felugiah to within a few hundred yards of the northern gate of Bagdad." This must (I have reason to believe) be understood as referring to a canal existing there, which joins the two rivers, and which is filled during the flooded season; but even here, no less than 380 (600—220) miles above the Hie, by which (as Col. Chesney informs us,) the Euphrates receives the waters of the Tigris, the levels of the two rivers so closely correspond as to allow of a navigable communication existing between them! Mr. Carter has discoursed very learnedly respecting the mode in which rivers produce their deltas, but there appears to be a fundamental defect in his reasoning: he takes as a "fact" that the Tigris "can be more rapid [than the Euphrates] *only* through flowing from a higher country down a greater slope." But if we look to what is actually *the fact*, we find that at two distinct points, namely, at Felugiah (opposite Bagdad) and at Khorna, the Tigris and Euphrates are of equal (or nearly equal) heights. Between these two points, however, we have the unquestionable evidence of Col. Chesney that the two rivers are "very different in every respect," the former moving in a rapid and the latter in a dull and lingering stream. This difference in character is clearly not produced by the Tigris "flowing from a higher country down a greater slope," *since at Bagdad that river is no higher than the Euphrates at Felugiah.* Other causes have therefore to be sought for, among which may be noticed the greater length of the

Euphrates between these two points and the breadth of its bed in the lower portion of its course, both which causes must produce a corresponding diminution in its speed, and on the other hand the contraction of the channel of the Tigris, which must be attended with a corresponding acceleration of the motion of its waters.

Col. Chesney is referred to as describing the Euphrates as in the present day flowing in a dull and lingering stream: Herodotus, *also an eye-witness*, in his description of Babylon talks of the "*deep and rapid* streams of the great Euphrates *." No one will for a moment doubt the accuracy of Col. Chesney's observation; but is not credit also due to Herodotus? and is he, in like manner as Arrian, to be "unceremoniously thrown overboard," whilst the facts respecting the *former* condition of these rivers remain unascertained? In the passage last cited, the Halicarnassian traveller further expressly asserts that the Euphrates "discharges itself into the Persian Gulf;" which assertion he confirms in his more detailed statement that that river, "which before flowed in an almost straight line," had its course so turned by Nitocris, that in his time, "those who wished *to go from the sea up to Babylon* were compelled to touch at Ardericca three times on three different days †." Surely such unqualified and unequivocal *assertions of plain matters of fact* are entitled to consideration, and are not to be put aside as *errors* simply because they are not applicable to the present state of things, or rather, perhaps, because they do not coincide with what we have been taught by former commentators to receive as the truth.

[To be continued.]

LXXXVIII. *On the Theory of Vanishing Fractions. By* J. R. Young, *Esq., Professor of Mathematics in Belfast College.* ‡

IN a letter inserted in the April number of this Journal (p. 295) I ventured to offer some objections to certain novel positions, lately advanced by an ingenious mathematician, in an Essay on the Fundamental Principles of the Differential and Integral Calculus. To these objections the author of the Essay has furnished a reply, in the number for May (p. 393); and I am happy to find, from the general tone of it, that Mr. Woolhouse has considered my scruples with the same good feeling in which they were avowedly offered.

* Ἐὼν μέγας, καὶ βαθὺς, καὶ ταχὺς, ἐξίει δὲ οὗτος ἐς τὴν Ἐρυθρὴν θάλασσαν.—*Clio*, 180.

† *Clio*, 185.

‡ Communicated by the Author.

In my former communication, I contented myself with simply pointing out the fallacy involved in the extremely general statements which I extracted from the Essay referred to; and with tracing the source of this fallacy to the circumstance of the author having unguardedly assumed the *converse* of a certain proposition, to be equally general with the proposition itself, which converse holds however only in particular cases.

The *direct* proposition to which I here allude is this, viz. that when in certain hypotheses any of the analytical conditions of a problem disappear, the final result, to which the general process leads, takes the form $\frac{0}{0}$. The *converse* proposition is, that *when the final result takes the form* $\frac{0}{0}$ *original conditions must have disappeared.* This latter is the affirmation distinctly conveyed, without the slightest qualification, in the propositions marked II. and III. in Mr. Woolhouse's reply; and it will be remembered, that against those propositions *only* my objections were directed; for I cheerfully admitted that much of Mr. Woolhouse's Essay was "in strict accordance with the usual notions of this doctrine."

To show that these objections were valid, I adduced an instance (that of a geometrical series) in which the propositions objected to would lead to error; and in adverting to this instance, in his reply, it will be seen that my respected friend has not defended the positions in question from the charge of making the sum of the said geometrical series *anything*, but has shown that another position (Prop. IV.), a position which was never impugned, is competent to supply the correct result. Surely my ingenious friend does not consider it to be a sufficient defence of Proposition III. to prove that its affirmations are neutralized by Proposition IV.; and yet there is no other attempt made to establish its truth. The proposition which Mr. Woolhouse discusses at page 395, does not at all contribute to this object; for that is the *converse* of the one which it behoves him to prove, in order to establish his third principle: this principle requires the proposition stated above, in Italics, and not the one which Mr. Woolhouse has demonstrated in the preceding Number. There is no dispute as to the form of the result *when* conditions vanish; the question is, does this form *necessarily imply* vanishing conditions in the original analytical statement of the problem? Mr. Woolhouse's third principle unequivocally states *it does.* But innumerable examples to the contrary may be adduced. The well-known problem of Clairaut, which has for its object the determination of the spot between two lights, which is equally illuminated by both, is a case in point, and furnishes a satisfactory refutation of the principle in question, as may be seen by a reference to the Algebra of Lacroix, where the circumstances of the problem are discussed at length. The ordinary expression for the radius of curvature of a plane curve, will also furnish other examples of the fallacy of the assumed principle; for when, in any particular example, that expression takes the form of a fraction, as $r = \frac{P}{Q}$, we have, by differentiating,

$$d\,r = \frac{Q\,d\,P - P\,d\,Q}{Q^2} \ldots\ldots\ldots (1)$$

and it is well known that whatever values of x and y render this expression equal to zero, the same values, provided they fulfill the original condition, or equation of the curve, will belong to points in it of maximum or minimum curvature; or to points at which the contact with the osculating circle is above the second order. Now it is plain that the conditions

$$P = 0, Q = 0 \ldots\ldots\ldots\ldots\ldots (2)$$

will cause a value of (1) to be zero; if, therefore, these conditions furnish for x and y values which satisfy the equation of the curve, the points to which they refer will be distinguished from the other points by the order of contact being higher there than elsewhere. Instead of deducing this conclusion from the expression $r = \frac{0}{0}$, we ought, in accordance with Mr. Woolhouse's third principle, to say that at every such point the radii of curvature are innumerable, which is obviously absurd. As an example, let us take the common parabola, of which the equation is $y^2 = 4\,m\,x$. By the usual process we obtain for r the expression

$$r = \left\{\frac{m + x}{x}\right\}^{\frac{3}{2}} \cdot \frac{y^3}{4\,m^2} = \frac{P}{Q};$$

and the conditions (2) are, in this case,

$$(m + x)^{\frac{1}{2}}\, y = 0,\ 4\,m^2\,x^{\frac{3}{2}} = 0,$$

which are satisfied by the values $x = 0$, $y = 0$; and these values, fulfilling the original condition $y^2 = 4\,m\,x$, it follows that the origin of the axes, that is the vertex of the parabola, is a point at which the contact is above the second order, and this we know to be the case from other considerations.

It is unnecessary to multiply examples illustrative of the fallacy of this third principle "as a general rule," and indeed a passage in the reply of my talented friend leads me to suspect that, while writing that reply, he himself had some misgivings about it. The passage I refer to is at page 396, where Mr. Woolhouse, in his reasonings on the form $\frac{0}{0}$, limits his arguments to those comparatively few cases in which the results of that form are obtained in such a way, "that no multiplication or division by a power of $x - a$ occurs in the process." If only results obtained under such restrictions as these are admitted to come under the second and third principles, then the *generality* of those principles is of course at once given up, and my friend and I are thus far agreed. But then so limited a principle of interpretation falls greatly short of a general theory; and moreover requires, in its application, an acquaintance with the texture of the entire process too minute to be generally attainable; it requires, in fact, that we know the *composition* of every multiplier and divisor employed,—an impossible problem beyond certain limits.

At page 399, Mr. Woolhouse enters into a digression upon "the general theory of analytical results," respecting which he considers me to be in error, because in my last letter I had said that the fact of the ellipse question, admitting multiple solutions, was information which the analytical result was quite incompetent to supply; and he observes, "I never before heard of the incompetency of an analytical result to afford any positive information that an investigation could admit of." In this gratuitous admission of paucity of information upon subjects in which he so eminently excels, my friend has done himself a wanton injustice. He is too profoundly acquainted with all the subtleties of the Integral Calculus, and its applications, not to have "heard of" *singular solutions*, which, though not comprised in the resulting integrals which furnish the general solutions to certain differential equations, have, nevertheless, the property of satisfying the proposed conditions. But a more comprehensive view of the results of even common algebra, would, I think, have induced my friend to withhold the remark just quoted. Mr. Woolhouse ascertains the number of admissible solutions from "the nature of the problem." By taking a more enlarged view, it would have occurred to him that the result might furnish solutions, not only contrary to the express stipulations of the problem, but at variance with even the original analytical conditions, although these may have a much wider range. The results after these "*solutions étrangères*" are rejected from them, are those from among which are to be selected the solutions to the problem. In the present discussion it is the connexion between the analytical conditions and the analytical results, which is the matter before us; and it is, I suspect, from not keeping this in mind, that Mr. Woolhouse has been led to say, in mistake, that "Professor Young involves himself in a palpable inconsistency when he arrives at the fact of the ellipse question admitting multiple solutions, by an examination of the original analytical conditions, and at the same time alleges that the analytical result is quite incompetent to supply that information." The mathematical readers of this Journal will however readily perceive, that what is here charged as "palpable inconsistency" is in perfect accordance with the strictest analytical accuracy; and that the "inconsistency" would have been, in inferring the multiple solutions from the analytical result, without reference to the original conditions, as Mr. Woolhouse has done, thus assuming (what is not true) that the *converse* of a certain proposition holds merely because the proposition itself is known to be true. Mr. Horner in the present volume of this Journal (p. 43.) has brought forward whole cluster of instances, in each of which, as he clearly shows, "the analytical result is quite incompetent to supply the information" even as to whether the question admits of a *single* solution, much less as to whether it admits of *multiple* solutions: the information sought must be obtained in all these cases, as I have obtained it in the ellipse question, viz. by a direct appeal to "the original analytical conditions." Without such an appeal how are we to know whether the analytical result to which the condition

$$2x + \sqrt{x^2 - 7} = 5$$

leads, viz.

$$3x^2 - 20x + 32 = 0,$$

will supply values competent to satisfy that condition? The presumption is that it *will* supply such values; upon trial however we find them to fail: and yet these values will satisfy the immediately antecedent equation, but this is not sufficient; *every* anterior step must be satisfied, up to the original equation inclusively; and the error committed in overlooking this would be precisely similar to that which Mr. Woolhouse appears to me to have committed, in inferring the multiple solutions to the ellipse question, merely because these solutions satisfy the final result*. The same mistaken view of the "theory of

* It is but justice to Mr. Woolhouse to state, however, that he admits (p. 399) that "the nature of the problem, as originally presented, is the proper source of rejective information," although he maintains that the original analytical conditions do not furnish the proper source of information, as to whether, in certain hypotheses, one of those conditions becomes destroyed, or two or more of them become dependent; but, on the contrary, that the result $\frac{0}{0}$ is a sufficient indication that one or other of these circumstances *must* take place. (See III. p. 394.) I have endeavoured to show, however, that this result is not competent to furnish any information on the subject.

analytical results" accompanies his animadversions at page 398-9 in the last number of this Journal; he appears to think it sufficient that the *antecedent* equation should be satisfied, for he remarks, "The corresponding antecedent equation to the result $x = \frac{o}{o}$, when cleared of fractions, is $o\,x = o$, or $o = o$, an equation that is obviously satisfied without any limitation to the value of x, and that cannot fail therefore to be compatible with the other equations or conditions." The statement, in connexion with this remark, viz. that "$\frac{o}{o}$ can never be the symbol of absurdity," has a little surprised me, because the contrary is a fact so generally known to analysts. To occupy these pages by examples of this would be quite superfluous, as they abound in most of the Continental books on algebra. In the comprehensive work of Bourdon there is an ample supply of such examples, and from which he deduces the ordinary conclusion, viz. that "le symbol $\frac{o}{o}$ est tantôt un caractère d'*indétermination*, tantôt un caractère d'*absurdité*."

From what has now been said of the symbol $\frac{o}{o}$, it appears that, when it is not the indication of absurdity, or of incompatible conditions, it may arise from either of these two causes: viz. from taking the ultimate, or limiting, value of $\frac{P}{Q}$, the general result of an analytical process; or, without regard to this extreme limit, it may arise from the destruction of one or more of the conditional equations. One or other of these circumstances must take place in connexion with the occurrence of $\frac{o}{o}$, whenever this symbol is at all interpretable. I say whenever the symbol is *interpretable*, for cases may arise in which this symbol is indicative of neither multiple solutions, nor of limiting values, nor of incompatible conditions. In such cases therefore other modes of solution must be sought. The instances to which I now allude are among those in which the vanishing of the numerator is not necessarily accompanied by the vanishing of the denominator; but where each vanishes independently, in virtue of distinct hypotheses introduced among the arbitrary quantities in each. With the exception of these unintelligible results, the occurrence of $\frac{o}{o}$ is always traceable to one or other of the circumstances before mentioned; which circumstances, although having no necessary connexion, *may* nevertheless, as in the case of the ellipse question, both exist simultaneously.

When therefore $\frac{o}{o}$ takes the place of $\frac{P}{Q}$, in any hypothesis, we may be assured that the limiting values of $\frac{P}{Q}$ will always subsist with the original analytical conditions, however they may be modified under the proposed hypothesis; but we can neither deny, nor affirm, that *other* values may also subsist with these conditions; for "this is information which the analytical result is quite incompetent to supply," and which must be derived solely from ascertaining the effect of the proposed hypothesis upon the original analytical restrictions; and that this is a fair and legitimate deduction from the foregoing examination, I think no person who enters into it with unbiassed judgement, will be disposed to deny.

Belfast, May 7th, 1836.

LXXXIX. *On the History of the Condensation of the Gases, in reply to* Dr. Davy, *introduced by some Remarks on that of Electro-magnetic Rotation. By* Michael Faraday, *Esq., D.C.L. F.R.S., &c., in a Letter to Richard Phillips, Esq., F.R.S. L. & E., &c.*

My dear Sir, Royal Institution, May 10, 1836.

I HAVE just concluded looking over Dr. Davy's Life of his brother Sir Humphry Davy. In it, between pages 160 and 164 of the second volume, the author links together some account, with observations, of the discovery of electro-magnetic rotation, and that of the condensation of the gases, concluding at page 164 with these words: "I am surprised that Mr. Faraday has not come forward to do him [Sir Humphry Davy] justice. As I view the matter, it appears hardly less necessary to his own honest fame than his acknowledgement to Dr. Wollaston, on the subject of the first idea of the rotary magnetic motion."

I regret that Dr. Davy by saying this has made that necessary which I did not before think so; but I feel that I cannot after his observation indulge my earnest desire to be silent on the matter without incurring the risk of being charged with something opposed to an *honest* character. This I dare not risk; but in answering for myself, I trust it will be understood that I have been driven unwillingly into utterance.

Dr. Davy speaks of electro-magnetic rotation, and so also must I, for the purpose of showing certain coincidences in dates, &c. between the latter part of that affair and the condensation of chlorine and the gases, &c. Oersted's experiments were publised in Thomson's Annals of Philosophy for October 1820, and from this, I believe, was derived the first knowledge of them which we had in this country. At all events it was the first intimation Sir Humphry Davy and I had of them, for he brought down the Number into the laboratory on the morning of its appearance (October 1st) and we repeated the experiments together. I may remark that this is a proof that Dr. Davy, in the Life* as well as elsewhere†, does not always understand the meaning of his brother's words, and I think that he would never have written the lines which have driven me to the present and a former reply‡ if he had.

Immediately upon Oersted's great discovery, the subject was pursued earnestly, and various papers were written, amongst which is one by Sir Humphry Davy, Phil. Trans. 1821, page 7, read before the Royal Society Nov. 16, 1820, in which, at page 17, he describes the rolling of certain wires upon knife-edges, being *attracted* when the north pole of the magnet was presented under certain conditions of current, and *repelled* under certain other conditions of current, &c.

Another paper was a brief statement by the Editor of the Quarterly Journal of Science, (Mr. Brande,) in which he announces distinctly and clearly Dr. Wollaston's view of the nature of the electro-magnetic force, and its circumferential character. It is in the tenth volume, p. 363, and may be dated according to the number of the Journal, 1st January 1821.

Then there are my historical sketches in the Annals of Philosophy, N.S., vols. ii. and iii. written in July, August, and September 1821, and the paper describing my discovery of the electro-magnetic rotation dated 11th September 1821§, and others; but we will pass on to that of Sir Humphry Davy, read 6th March 1823||, which with its consequents is *synchronous* with the affair of the condensation of gases. This is the paper which Dr. Davy says "he (Sir H. D.,) concludes by an act of justice to Dr. Wollaston, pointing out how the discovery of the *rotations of the electro-magnetic wire round its axis* by the approach of a magnet, realized by the ingenuity of Mr. Faraday had been anticipated, and even attempted by Dr. Wollaston in the laboratory of the Royal Institution¶".

I have elsewhere** done full justice to Dr. Wollaston on the point of electro-magnetic rotation, and have no desire to lessen the force of anything I have said, but would rather exalt it. But as Dr. Davy has connected it with the condensation of the gases, I must show the continual tendency to error which has occurred in both these matters. Dr. Davy, then, is in error when he says I realized Dr. Wollaston's expectation; nor does Sir Humphry Davy say what his brother imputes to him. *I did not realize* the rotations of the electro-magnetic wire

* Vol. ii. p. 143. † Lond. and Edinb. Phil. Mag., 1835, vol. vii. p. 340.
‡ *Ibid.* p. 337. § Quarterly Journal of Science, vol. xii. p. 74.
|| Phil. Trans. 1823, p. 153. ¶ Life, vol. ii. p. 160.
** Quarterly Journal, vol. xv. p. 288.

round its axis; that fact was discovered by M. Ampère, at a later date; and even after I had discovered the rotation of the wire round the magnet as a centre, and that of the magnet round the wire, I could not succeed in causing the wire to revolve on its own axis*. The result which Wollaston very philosophically and beautifully deduced from his principles, and which he tried to obtain in the laboratory, was, that wires could be caused to roll, not by attraction and repulsion as had been effected by Davy†, but by a tangential action, according to the principles which had been already made known to the public as his (Dr. W.'s) by Mr. Brande‡.

What Sir Humphry Davy says in his printed paper § is this: "I cannot with propriety conclude without mentioning a circumstance in the history of the progress of electro-magnetism which, though well known to many Fellows of this Society, has, I believe, never been made public, namely, that we owe to the sagacity of Dr. Wollaston the first idea of the possibility of *the rotations of the electro-magnetic wire round its axis by the approach of a magnet*; and I witnessed early in 1821 an unsuccessful experiment which he made to produce the effect in the laboratory of the Royal Institution." This paper being read on the 6th of March 1823, was reported on the first of the following month in the Annals of Philosophy, N.S., vol. v. p. 304; the reporter giving altogether a different sense to what is conveyed by Sir Humphry Davy's printed paper, and saying that "had not an experiment on the subject made by Dr. W. in the laboratory of the Royal Institution, and witnessed by Sir Humphry failed, *merely through an accident* which happened to the apparatus, *he would have been the discoverer of that phenomenon* ‖."

I have an impression that this report of the paper was first made known to me by Sir Humphry Davy himself, but a friend's recollection makes me doubtful on this point: however, Sir Humphry, when first he adverted to the subject, told me it was inaccurate and very unjust; and advised me to draw up a contradiction which the Editor should insert the next month. I drew up a short note, and submitting it to Sir Humphry he altered it and made it what it appears in the May Number of the Annals of Philosophy, N.S. vol. v. page 391, as from the Editor, all the parts from "but writing only" to the end being Sir Humphry's; and I have the manuscript in *his hand-writing* inserted as an illustration into my copy of Paris's Life of Davy.

* Quart. Journ. of Science, vol. xii. p. 79.

† Phil. Trans. 1821, p. 17.

‡ Quart. Journ., vol. x. p. 363.

§ Phil. Trans. 1823, p. 158.

‖ In justice to the reporter, I have sought carefully at the Royal Society's for the original manuscript, being the paper which he heard read; but it cannot be found in its place.

The whole paragraph stands thus: "*** We endeavoured last month to give a full report of the important paper communicated by the President to the Royal Society on the 5th [6th] of March*; but writing only from memory, we have made two errors, one with respect to the rotation of the mercury not being stopped, but produced, by the approximation of the magnet; the other in the historical paragraph in the conclusion, which, as we have stated it, is unjust to Mr. Faraday, and does not at all convey the sense of the author. We wish, therefore, to refer our readers forward to the original paper, when it shall be published, for the correction of these mistakes.—*Edit.*"

From this collection of dates and documents any one may judge that I at all events was *unjustly* subject to some degree of annoyance, and they will be the more alive to this if they recollect that all these things were happening at the very time of the occurrence of the condensation of gases and its consequences, and during the time that my name was before the Royal Society as a candidate for its fellowship. I do not believe that any one was wittingly the cause of this state of things, but all seemed confusion, and generally to my disadvantage. For instance, this very paper of Sir Humphry Davy's which contains the "act of justice," as Dr. Davy calls it, is entitled, "*On a new phenomenon of Electro-magnetism.*" Yet what is electro-magnetic was not new, but merely another form of my rotation; and the *new phenomenon* is purely electrical, being the same as that previously discovered by M. Ampère. As M. Ampère's result is described for the first time in a paper of the date of the 4th of September 1822†, and Sir Humphry Davy's paper was read as soon after as the 6th of March 1823‡, the latter probably did not know of the result which the former had obtained.

To conclude this matter: in consequence of these and other circumstances, and the simultaneous ones respecting the condensation of chlorine, I wrote the historical statement§, to which Dr. Davy refers ||, in which, admitting everything that Dr. Wollaston had done, I claim and *prove* my right to the discovery of the rotations I had previously described. This paper before its publication I read with Dr. Wollaston; he examined the proofs which I have adduced at p. 291, and after he had made a few alterations which brought it into the state in which it is printed, expressed his satisfaction at the arguments and his approval of the whole. The copy I have preserved, and I will now insert the most considerable and im-

* So far is mine; the rest is Sir Humphry Davy's.

† *Ann. de Chim.*, 1822, vol. xxi. p. 47.

‡ Phil. Trans. 1823, p. 153.

§ Quarterly Journal of Science, vol. xv. p. 288.

|| Life, vol. ii. p. 146. bottom of the page.

portant of Dr. Wollaston's corrections as an illustration. At the end of the paragraph at the bottom of page 291, I had expressed the sense thus: "But what I thought to be attraction and repulsion in August 1821, Dr. Wollaston long before perceived to be an impulsion in one direction only, and upon that knowledge founded his expectations." This he altered to: "But what I thought to be attraction *to* and repulsion *from the wire* in August 1821, Dr. Wollaston long before perceived to arise from *a power not directed to or from the wire, but acting circumferentially round it as axis*, and upon that knowledge founded his expectation." The parts in Italics are in his hand-writing.

With respect to the condensation of the gases, I have long ago done justice to those to whom it was really due, and now approach the subject again with considerable reluctance; for though I feel that there is some appearance of confusion, still I regret that Dr. Davy did not leave the matter as it stood. All my papers on the subject in the Transactions of the Royal Society had passed through the hands of Sir Humphry Davy, who had corrected them as he thought fit, and had presented them to that body. Again, all the facts that Dr. Paris has stated upon his own knowledge* are correct; he made that statement as his own voluntary act and without any previous communication with me, so that I think I might have been left in that silence which I so much desired.

The facts of the case, as far as I know them, are these: In the spring of 1823, Mr. Brande was Professor of Chemistry, Sir Humphry Davy Honorary Professor of Chemistry, and I Chemical Assistant, in the Royal Institution. Having to give personal attendance on both the morning and afternoon chemical lectures, my time was very fully occupied. Whenever any circumstance relieved me in part from the duties of my situation, I used to select a subject of research, and try my skill upon it. Chlorine was with me a favourite object, and having before succeeded in discovering new compounds of that element with carbon, I had considered that body more deeply, and resolved to resume its consideration at the first opportunity: accordingly, the absence of Sir Humphry Davy from town having relieved me from a part of the laboratory duty, I took advantage of the leisure and the cold weather and worked upon frozen chlorine, obtaining the results which are published in my paper in the Quarterly Journal of Science for the 1st of April 1823†. On Sir Humphry Davy's return to town, which I think must have been about the end of

* Paris's Life of Davy, pp. 390, 391, 392.

† Vol. xv. p. 71.

February or the beginning of March, he inquired what I had been doing, and I communicated the results to him as far as I had proceeded, and said I intended to publish them in the Quarterly Journal of Science. It was then that he suggested to me the heating of the crystals in a closed tube, and I proceeded to make the experiment which Dr. Paris witnessed, and has from his own knowledge described*. I did not at that time know what to anticipate, for Sir Humphry Davy *had not told me his expectations*, and I had not reasoned so deeply as he appears to have done. Perhaps he left me unacquainted with them to try my ability. How I should have proceeded with the chlorine crystals without the suggestion I cannot now say, but with the hint of heating the crystals in a close tube ended for the time Sir Humphry Davy's instructions to me, and I puzzled out for myself in the manner Dr. Paris describes, that the oil I had obtained was condensed chlorine. This is all very evident from the paper read to the Royal Society, though it may seem at first to stand opposed to the notes and papers that Sir Humphry Davy communicated in conjunction with and after mine. When my paper was written it was, according to a custom consequent upon our relative positions, submitted to Sir Humphry Davy, (as were all my papers for the Philosophical Transactions up to a much later period,) and he altered it as he thought fit. This practice was one of great kindness to me, for various grammatical mistakes and awkward expressions were from time to time thus removed which might else have remained.

The passage at the commencement of the paper which I shall now quote was of Sir Humphry Davy's writing, and in fact contains everything that, and perhaps rather more than, he had said to me: "The President of the Royal Society having honoured me by looking at these conclusions, suggested, that an exposure of the substance to heat under pressure, would probably lead to interesting results; the following experiments were commenced at his request†." I say "rather more," because I believe *pressure* was not recurred to in our previous verbal communication. However, I proceeded to make the experiment, and was making it when Dr. Paris came into the laboratory as he has described, and my thoughts at that moment are embodied and expressed in my paper in the following words: "I at first thought that muriatic acid and euchlorine had been formed; then that two new hydrates of chlorine had been produced; but at last I sus-

* Paris's Life, p. 391.

† Phil. Trans. 1823, p. 160., [or Phil. Mag., First Series, vol. lxii, p. 413.—Edit.]

pected that the chlorine had been entirely separated from the water by the heat, and condensed into a dry fluid by the mere pressure of its own abundant vapour *." I then describe an experiment entirely of my own, in which I proceed to verify this conjecture, and go on to say, "presuming that I had now a right to consider the yellow fluid as pure chlorine in the liquid state, I proceeded to examine its properties, &c. &c.†"

To this paper Sir Humphry Davy added a note ‡, in which he says, "In desiring Mr. Faraday to expose the hydrate of chlorine to heat in a closed glass tube §, it *occurred to me* that one of three things would happen; that it would become fluid as a hydrate; or that a decomposition of water would occur, and euchlorine and muriatic acid be formed; or that the chlorine would separate in a condensed state." And then he makes the subject his own by condensing muriatic acid, and states that he had "requested" me, (of course as Chemical Assistant,) "to pursue these experiments, and to extend them to all the gases which are of considerable density, or to any extent soluble in water;" &c. This I did, and when he favoured me by requesting that I would write a paper on the results, I began it by stating "that Sir Humphry Davy did me the honour to request I would continue the experiments, which I have done under his general direction, and the following are some of the results already obtained: ||" and this paper being immediately followed by one on the application of these liquids as mechanical agents, by Sir Humphry Davy ¶, he says in it, "One of the principal objects that I had in view in *causing experiments to be made* on the condensation of different gaseous bodies, by generating them under pressure, &c."

I certainly took up the subject of chlorine with the view of pursuing it as I could find spare time, and at the moments which remained to me after attending to the directions of my superiors. It however passed in the manner described into the hands of Sir Humphry Davy, and a comparison of the dates will readily show that I at least had no time of my own to pursue it. My original paper was published on the first of April 1823, that being the first number of the Quarterly Journal which *could* appear after the experiments had been made: but in the short time between the first experiment and the publication much that I have referred to had occurred, for

* Phil. Trans. 1823, p. 162. † *Ibid.* p. 163. ‡ *Ibid.*, p. 164.

§ Observe, not "to heat under pressure." See my remarks in the preceding page.

|| Phil. Trans. 1823, p. 189. [or Phil. Mag., First Series, vol. lxii, p. 417. —Edit.] ¶ *Ibid.* p. 199.

not only had I communicated my results to Sir Humphry Davy, and received from him the hint, but my paper on fluid chlorine had been read (13th of March), and his note also, of the same date, attached to it; and the Editor of the Quarterly Journal, Mr. Brande, had time prior to the printing of my original paper to attach a note to it stating the condensation of chlorine and muriatic acid, and expressing an expectation that several other gases would be liquefied by the same means*. On the 10th of April my paper on the condensation of several gases into liquids was read, on the 17th of April Sir Humphry Davy's on the application of condensed gases as mechanical agents, and on the 1st of May his Appendix to it on the changes of volume produced by heat.

I have never remarked upon or denied Sir Humphry Davy's right to his share of the condensation of chlorine or the other gases; on the contrary, I think that I long ago did him full "justice" in the papers themselves. How could it be otherwise? he saw and revised the manuscripts; through his hands they went to the Royal Society, of which he was President at the time; and he saw and revised the printer's proofs. Although he did not tell me of his expectations when he suggested the heating the crystals in a closed tube, yet I have no doubt that he had them †; and though, perhaps, I regretted losing my subject, I was too much indebted to him for much previous kindness to think of saying that that was mine which he said was his. But *observe* (for my sake) that Sir Humphry Davy nowhere states that he told me what he expected, or contradicts the passages in the first paper of mine which describe my course of thought, and in which I claim the development of the actual results.

All this activity in the condensing of gases was simultaneous with the electro-magnetic affair already referred to, and I had learned to be cautious upon points of right and priority. When therefore I discovered in the course of the same year that *neither I nor Sir Humphry Davy* had the merit of first condensing the gases, and especially chlorine, I hastened to perform what I thought right, and had great pleasure in spontaneously doing justice and honour to those who deserved

* Quarterly Journal, vol. xv. p. 74.

† I perceive in a letter to Professor Edmund Davy, published by Dr. Davy in the Life, vol. ii. p. 166, of the date of *September* 1, 1823, that Sir Humphry Davy said, "The experiments on the condensation of the gases were made under my direction, and *I had anticipated*, theoretically, all the results." It is evident that he considered the subject his own; but I am glad that here, as elsewhere, he never says that he had informed me of his expectations. In this Sir Humphry Davy's negative, and Dr. Paris's positive, testimony perfectly agree.

it*. I therefore published on the 1st of January of the following year (1824) a historical statement respecting the liquefaction of gases†, the beginning of which is as follows: "*I was not aware* at the time when *I first observed* the liquefaction of chlorine gas, nor until very lately, that any of the class of bodies called gases had been reduced into the fluid form; but having during the last few weeks sought for instances where such results might have been afforded without the knowledge of the experimenter, I was surprised to find *several recorded cases.* I have thought it right, therefore, to bring these cases together, and *only justice* to endeavour to secure for them a more general attention than they appear as yet to have gained." Amongst other cases the liquefaction of *chlorine* is clearly described‡. The value of this statement of mine has since been fully proved; for upon Mr. Northmore's complaint ten years after, with some degree of reason, that great injustice had been done to him in the affair of the condensation of gases, and his censure of "the conduct of Sir H. Davy, Mr. Faraday, and several other philosophers for withholding the name of the first discoverer," I was able by referring to the statement to convince him and his friend that if my papers had done him wrong, *I at least* had endeavoured also to do him right§.

Believing that I have now said enough to preserve my own "honest fame" from any injury it might have risked from the mistakes of Dr. Davy, I willingly bring this letter to a close, and trust that I shall never again have to address you on the subject.

I am, my dear Sir, yours, &c.

Richard Phillips, Esq., &c. &c. M. Faraday.

XC. *On the Crag of Suffolk, and on the Fallacies connected with the Method now usually employed for ascertaining the relative Age of Tertiary Deposits. By* Edward Charlesworth, *Esq., F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In former communications I treated of the crag as a tertiary formation consisting of separate marine deposits, and

* Monge and Clouet had condensed sulphurous acid probably before the year 1800. Northmore condensed chlorine in the years 1805 and 1806.

† Quarterly Journal of Science, vol. xvi. p. 229. ‡ *Ibid.*, p. 236.

§ Lond. & Edinb. Phil. Mag. 1834, vol. iv. p. 261.

being desirous that the grounds upon which I have adopted this opinion should be fairly placed before those to whom the geological history of our own island is an object of interest, I propose in the course of the following observations to enter more minutely into the merits of that question.

An attempt has been made to explain the relation which the divisions of the crag bear to each other by assuming that the lower or coralline beds constitute the only original deposit, from which the rest of the fossiliferous strata above the London clay in Suffolk and the adjoining counties have been derived, by the operation of diluvial agents.

It may perhaps appear hardly necessary to enter upon the refutation of a theory which is so irreconcilable with recorded facts, but as it is desirable that no stumbling block should lie in the way of future investigation, I shall advert to some of the points which are especially opposed to its reception.

Until the subject was recently brought before the notice of the Geological Society, our available sources of local information respecting the crag and its organic remains were almost entirely confined to the published observations of Mr. R. C. Taylor and Mr. Samuel Woodward, the former of whom had paid great attention to the tertiary deposits of Norfolk and Suffolk, and to whose exertions, I believe, we are indebted for the first list of their characteristic fossils. I might, perhaps, reasonably inquire how far the diluvial character assigned to the red crag is consistent with the results attending my own personal investigation. For the present, however, I am anxious that your attention should be drawn to several passages occurring in the works of the above-named writers, and which are certainly calculated to throw some light upon the point at issue if the matter be really one requiring elucidation.

Mr. Taylor's interesting memoir on the geology of Eastern Norfolk was published in 1827, but his range of observation was by no means limited to the particular district which he there professes to describe. We find, however, no allusion to the Ramsholt stratum, although he had evidently extended his researches into the adjoining county and explored the coral reefs of Aldborough and Orford. A circumstance which appears to have particularly arrested the attention of Mr. Taylor during his investigation of the crag was the natural distribution of its fossil *Testacea*, the occurrence of which he points out in that part of the formation which we have lately been informed "is *decidedly diluvium or disrupted crag*." At page 15, he remarks, "it is characteristic of the shells and other organic bodies deposited with the crag, that they are by no means diffused in equal numbers and proportions throughout, but occur at intervals in groups and genera. Thus at Cromer the predominant and remarkable shells are *Mactræ*; at Runton, *Cardia*; nearer Clay, *Murex striatus*; at Bawdesey cliff, *Murex reversus* and *Pectunculus*; at the Beacon, *Venus æqualis*; at Felixstow, *Pectunculus* and *Voluta Lamberti*; south of Landguard cottage, *Murex contrarius* and *Mya lata*; at Bramerton and near Norwich are *Murex striatus*, *Tellinæ*, and *Balani*." There is no reference here made to Ramsholt, Sudbourn, or Aldborough; all the localities named in the above extract are those of the red or *diluvial* crag.

At page 23, Mr. Taylor observes, "that after the formation of the chalk the waters deposited the marine exuviæ, and gave existence during *the long period* in which they occupied that portion of its former surface to those remarkable accumulations of crag shells which we now witness." And again, at page 29: "A district bordering a hundred miles upon our eastern coast is occupied by an ancient marine deposit. . . . . . . at one point exhibiting groups of shell-fish allied to those of the neighbouring sea, and at another composed of numerous genera which are neither to be recognised living in any part of our globe or assimilated to the fossil shells of other formations."

I need not pursue Mr. Taylor's views any further, but would refer the reader to his work or to his previous papers in the Philosophical Magazine. The above quotations furnish ample proof that he had not discovered the diluvial nature of the red crag, although it was that part of the formation with which he was so intimately acquainted.

In 1833, Mr. Samuel Woodward published an outline of the Geology of Norfolk, in which we are presented with a brief notice of the crag, confirming the previous observations made by Mr. Taylor. At page 19, Mr. W. mentions that "the crag district is a narrow tract running southward from the coast between Cromer and Weybourn, and passing Norwich in its progress towards the Suffolk coast, the great deposit of this formation." Mr. Woodward, without suspecting that the deposit which he is describing is *diluvium*, proceeds to remark that "this tract appears to us to have been an estuary in the antediluvian period . . . . . .Viewing the thick beds of testaceous remains, we cannot hesitate to admit that the *sea occupied for a long period* the part of Norfolk now under consideration."

Again, at page 21: "Another point worthy of attention is the apparent agreement in the gregarious habits of the original occupiers of these shells with the recent Mollusca, confining

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them to particular spots or habitats; thus we find that the beds of crag shells are not continuous but deposited in patches; and that the shells in the Suffolk beds are in numerous instances generically and in almost all specifically different to those found near Norwich." No traces of the coralline crag have yet been detected in the county of Norfolk; it should therefore be borne in mind that the above observations refer solely to the upper deposit.

We are here furnished with the clearest evidence that Messrs. Woodward and Taylor agree in one important particular; viz. that the fossils of the red crag are not promiscuously jumbled together, but localized very much in the same manner as the Mollusca inhabiting our present seas: both geologists also infer from the great accumulation of these fossils that the ocean must for a long time have remained stationary over that district in which they occur.

In order then to maintain the decisions in reference to this subject which appeared in your Number for November, it will be necessary either to dispute the accuracy of the facts now adduced, or to show that this gregarious distribution of genera and species may exist in a formation resulting from those operations which we designate by the term diluvial. I willingly admit that the views of geologists as to the real nature of these operations are not of the most definite character, and at the present time our opinions respecting the true origin of what are called diluvial deposits are undergoing important modifications; but allowing the utmost latitude for any discordance of this kind, I apprehend that it will require more than ordinary ingenuity to show that the conditions which prevailed at the time when the formation of the crag was going forward can in any way be approximated to that state of things which is generally understood to be the necessary concomitant of diluvial action.

Those who are at all familiar with the geology of Norfolk, cannot fail to have observed that the crag, in common with other formations, has been subjected to the abrasion of diluvial currents. Mr. Taylor remarks that "portions probably from its western edges have been swept away. Their fragments mingled with those of the chalk and preceding formations, piled in enormous heaps, form the cliffs of Cromer and Trimmingham, 250 or 300 feet in thickness upon the original crag which rests *in situ* at their base."

I imagine that it would not greatly increase the reputation of any geological observer to infer the diluvial origin of the Norfolk chalk, because its fragments in the shape of detritus occur in the cliffs at Cromer; but a precisely analogous fact has been brought forward to support a similar opinion regarding the upper division of the crag.

A small series of shells which I had collected at Ramsholt were placed by Mr. Lyell in the hands of M. Deshayes, for the purpose of ascertaining his opinion with regard to the proportion of extinct species. The conclusion he came to was that the per centage of recent shells was the same as in the larger collection, which he had examined when preparing his tables on tertiary fossils, and which were probably obtained from the upper bed.

It is in allusion to this circumstance that a correspondent observes, "If such be the fact, there is an end to the question between my opponent and myself."

Now, the questions which have been under discussion are the presence of corallines in the Ramsholt bed, and the diluvial nature of the red crag. To decide these disputed points by simply ascertaining the per centage of extinct species in the shells of the coralline crag, can only have been effected by a course of induction as novel in its nature as the results which it evolves are important; nor shall I stand alone in anxiously anticipating further information upon the application of a principle, which in some instances may so materially assist the labours of the geologist while prosecuting the investigation of tertiary formations.

I turn however from the consideration of this subject, which is almost devoid of interest from its not having assumed a form that entitles it to serious discussion, to enter upon an inquiry far more comprehensive in its nature and requiring a more profound method of investigation;—an inquiry replete with the highest interest, from the practical suggestions which it offers, and still more so in the field which it throws open for legitimate inductive speculation.

I have on a previous occasion dwelt upon the features which separate the coralline crag from the tertiary strata with which it is connected. The novelty of its general aspect, lithological character, and organic remains when contrasted with the adjacent fossiliferous beds cannot be disputed. But the question may fairly be asked, what is the nature of these changes, and what are the conclusions to be drawn from them? Do they accord with those well-known phænomena which are supposed to register the lapse of ages; or may they not rather be attributed to certain alterations in physical condition, which over a small area may materially affect the existing organization during a comparatively short period?

I am aware that Mr. Lyell in the last edition of his Principles of Geology refers the red and the coralline crag to the same period, from the number of fossils which are common to the two deposits, and this opinion he has subsequently confirmed in the Anniversary Address recently delivered to the Fellows of the Geological Society.

In a former memoir, when describing the stratum at Ramsholt, the opinion I stated was that it formed part of a deposit, older, geologically speaking, than those shelly strata above it with which geologists were already familiar. Subsequent consideration has tended to strengthen the views which I then advocated, and my object at present is that of testing the importance of those facts which are supposed by some to identify the coralline beds with the other fossiliferous strata.

During the summer of 1835, I entered upon a more minute examination of those localities in which the inferior portion of the crag is most advantageously exposed, and my investigation has been attended with results of a highly gratifying and satisfactory nature. I have procured from Ramsholt every species of coral that has yet been obtained from the more extensive excavations at Aldborough and Orford; while above the coral reefs, which occupy so large a portion of the latter district, I have succeeded in discovering the upper deposit, still retaining those well-marked peculiarities which form a striking contrast to the inferior stratum, and from which even the yet unpractised observer would as naturally separate it as he would the beds of the coralline crag from the London clay on which they repose. My anticipations on this subject have therefore been completely realized, and the true geological position of the Orford crag may now be considered fully established.

The relative position and lithological character of the red crag would during a late period of inquiry have probably assigned it a distinct place in a geological series, and under some circumstances the geologist undoubtedly derives considerable assistance in the classification of fossiliferous deposits from a careful observation of these phænomena. To guide our determination in the instance before us, in addition to these sources we have thrown open to our inspection an extensive series of organic remains; it is from their examination that my own opinions have principally been formed, and it now remains for me to show how far they can be justified.

With this view I shall take a cursory survey of the organic remains at present discovered in the tertiary strata which overlie the London clay in Suffolk and the chalk in Norfolk.

In the coralline crag we find few indications of the existence of vertebrated animals; such as are met with belong exclusively to the class of *Fish*; but the nature of this deposit appears to have been by no means well calculated for the preservation of their remains. The only bones of frequent occurrence are those placed within the cavity of the tympanum, and which being of a more solid texture than the rest of the skeleton are found in a very perfect state. These bones belong to an unknown genus, and are peculiar to this part of the crag formation. Teeth of cartilaginous species are occasionally met with, but in the course of my own researches I have never succeeded in obtaining them.

The ocean, however, which deposited the red crag was one evidently swarming with fish; and their mineralized remains, generally consisting of the teeth and portions of the palate, are preserved in great abundance. Among them are the genera *Carcharias*, *Myliobates*, *Galeus*, *Lamna*, *Notidanus*, and *Platax*, &c. Wherever this deposit is detected, some of these genera invariably accompany it. It is here also that we first meet with the higher orders of the animal kingdom. The teeth of the Mastodon, Elephant, Hippopotamus, and other *Mammalia* are deposited with the *Mollusca* of this period, and in addition to them I may mentioned the bones of *Birds*, which I have recently obtained from several localities.

Turning from the groups of vertebrated animals to those of the *Radiata*, we naturally revert to that extensive assemblage of *Polypifera* which characterize so large a portion of the coralline crag, and to which nothing analogous is presented by any other tertiary deposit in this island. The *Echinidæ* too, so sparingly distributed in the London clay and upper beds of the crag, are here met with in comparative abundance; fragments and spines are of constant occurrence, and some of the more perfect specimens which have been obtained exhibit the most elegant forms, and are widely removed from known species. There are one or two spots in the red crag where Echini have congregated in myriads, but the species approximate more nearly to those now existing, and with which they may perhaps be identified. The comparison of the *Crustacea* from the two beds has furnished a corresponding result; but the remains of this group are sparingly met with, and generally in an unfavourable state for examination.

I now proceed to notice that class which among organized beings are thought to furnish the geologist with the most important data in his investigation of tertiary formations, and to which he especially directs his attention when fossiliferous strata of different periods are superposed in the same area, or when he is desirous of ascertaining the probable epoch to which an isolated deposit should be referred.

Mr. Searles V. Wood, who possesses the largest series extant of British tertiary fossils, states that he has collected 450 species of shells from the crag: of these more than 200 were peculiar to the coralline, 80 peculiar to the upper bed, and 150 were found in both deposits.

Before any conclusions are drawn from this statement, it is of the utmost importance to bear in mind the circumstances under which the fossils of one formation may, by the natural process of degradation, have been imbedded in another. I have before alluded to the fact of secondary shells occurring in the red crag where that deposit is in contact with the chalk; and if causes similar to those now in action were operating at æras antecedent to the present, there is nothing to excite our surprise in this phænomenon. I have been particularly struck with the appearance presented by the fossils in those remarkable masses of transported or protruded chalk which are seen on the beach for a few miles east of Cromer. Many of these enormous fragments are half buried in the stratum of blue clay forming the beach, to which level the elevated portion is by the action of the tides gradually reduced. A platform of chalk is thus formed, which is frequently studded in every direction with *Belemnites* and *Terebratulæ*. As its surface wears away the fossils are brought out in relief, and at length being entirely removed are deposited with the recent *Mollusca*. The point principally deserving notice here is the introduction of these fossils into the present deposits completely detached from the matrix in which they were imbedded, and which being removed in a finely divided state, would not at a future period be recognised in the form under which it formerly existed.

The secondary shells in the crag of Norfolk have probably been removed from their original bed by a process similar to that just described. We see no indications of a more violent operation; there are no nodules of chalk accompanying the fossils, which are themselves so completely freed from any adherent matrix that they can only be distinguished from the more recent *Mollusca* with which they are associated by an attention to specific distinctions, and by the chalk locked up within the cavity of the bivalves.

At the time the formation of the red crag was going forward, the surface of the chalk to a great extent was protected from abrasion by overlying deposits, and wherever this was the case the *superior* stratum would be the one exposed to denudation, and from which organic remains would be transported. In this way, undoubtedly, have the fossils of the coralline crag, along with those of the chalk, been introduced into a more recent deposit, and the difficulty is now to ascertain the probable amount of admixture. Connected with this subject there is one circumstance which should not be passed over without consideration: supposing that the disturbing forces were acting with equal intensity over the area of chalk and coralline crag, the effect produced, so far as regards the removal of fossils, would be regulated by their abundance and by the nature of the deposit in which they were imbedded. If, as is really the fact, we find in the red crag six or eight per cent. of fossils belonging to the chalk, we may reasonably infer the presence of a much larger number derived from the coralline beds. Were we to discover fossil shells carried down to the delta of a river the course of which flowed over an equal area of chalk and crag, we should naturally expect that the majority of these transported fossils would belong to the latter formation.

The numerical statements drawn up by Mr. Wood have been made without any reference to the conditions under which a large number of the same fossils have been discovered in the two deposits. However abundant or naturally grouped a shell may occur in the coralline crag, one solitary specimen of that species, or even a fragment having been detected in the upper bed, at once places it on the list of those which are spoken of as common to the two formations; under these circumstances, and taking into consideration the probable extent to which the coralline beds have been broken up, I am only surprised that there should be so large a number as 200 species which are only found in them and have not yet been observed in the rest of the formation.

There are however some *Mollusca* which are either naturally localized, or occur in the same abundance in both divisions of the crag formation; and setting aside the fallacies which may arise from our erroneous identification of species, we are at liberty to infer from these the probable approximation of the two deposits. It appears, however, that a very large proportion of species may be continued through distinct and very remote geological epochs, for on referring to the tables of M. Deshayes, we find that there are not less than 40 per cent. of species common to the crag and to the formations at this time in progress round the British islands.

Mr. Lyell, when speaking of the newer pliocene formations, observes in vol. iii., page 54, "It will be seen that of two hundred and twenty-six species found in the Sicilian beds only ten are of extinct or unknown species, although the antiquity of these tertiary deposits as contrasted with our most remote historical æras is immensely great. In the volcanic and sedimentary strata of the district round Naples, the proportion appears to be even still smaller."

It seems then that if instead of 20 or 30 there were 95 per cent. of species common to the red and coralline crag, even then these deposits might be as widely separated as the Sicilian tertiary strata and the formations of the present period!

I have yet to enter upon the most important stage of the present inquiry, that which relates to M. Deshayes's examination of the coralline crag shells, and to the consideration of how far the result affects the opinion I formerly advanced respecting the antiquity of the Ramsholt stratum.

During the last two or three years I have embraced every opportunity of examining the marine and freshwater deposits in the counties of Norfolk, Essex, and Suffolk, and of late my attention has been particularly directed to those views of chronological arrangement which in so comprehensive and elaborate a manner are advocated in the 'Principles of Geology.' From facts which have fallen under my own notice during the course of my investigation, and from other circumstances which have more recently transpired, I feel confident that a classification of the fossiliferous strata in question, founded upon the proportion of extinct *Mollusca* which they individually contain, would lead to the most erroneous conclusions.

The sources of error which I have in the present instances detected, will, if clearly established, have a general application in the arrangement of tertiary formations, and will probably materially interfere with the confidence which we might otherwise place in the accuracy of those results which are connected with numerical calculations.

To enter upon a full discussion of this most interesting and complicated subject would greatly exceed the limits of the present communication, and I shall therefore confine myself to those points which are particularly connected with the present inquiry. [To be continued.]

XCI. *Theorem respecting Algebraic Elimination, connected with the Question of the Possibility of resolving in finite Terms the general Equation of the Fifth Degree.* Extracted by Permission, from a Communication recently made to the Royal Irish Academy. *By Professor Sir* WILLIAM ROWAN HAMILTON, *Astronomer Royal of Ireland**.

Theorem. IF x be eliminated between two equations, of the following forms, namely, 1st, an equation of the fifth degree, of the form

$$0 = x^5 + D x + E, \quad \text{..................} \quad (1.)$$

* Communicated by the Author.

in which the roots are supposed to be all unequal, and the coefficients D and E to be, both of them, different from 0, and, 2nd, an equation of the form

$$y = \mathrm{Q}\,x + f(x), \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2.)$$

in which $f(x)$ denotes any rational function of x, whether integral or fractional,

$$f(x) = \frac{\mathrm{M}'\,x^{\mu'} + \mathrm{M}''x^{\mu''} + \&c.}{\mathrm{K}'\,x^{\varkappa'} + \mathrm{K}''x^{\varkappa''} + \&c.}; \quad \ldots\ldots\ldots\ldots \quad (3.)$$

and if, in the result of this elimination, which will always be an equation of the fifth degree in y, of the form

$$0 = y^5 + \mathrm{A}'y^4 + \mathrm{B}'y^3 + \mathrm{C}'y^2 + \mathrm{D}'y + \mathrm{E}', \quad \ldots\ldots \quad (4.)$$

we suppose that the coefficients are such as to satisfy, *independently of* Q, the second as well as the first of the two conditions

$$\mathrm{A}' = 0, \quad \mathrm{C}' = 0, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (5.)$$

in virtue of the values of the constants

$$\mathrm{M}', \mathrm{M}'', \ldots\ \mu', \mu'', \ldots\ \mathrm{K}', \mathrm{K}'', \ldots\ \varkappa', \varkappa'', \ldots\ \ldots \quad (6.)$$

in the rational function $f(x)$; I say that then those constants (6.) must be such as to admit of our reducing that rational function to the form

$$f(x) = q\,x + (x^5 + \mathrm{D}\,x + \mathrm{E}).\,\phi(x), \quad \ldots\ldots \quad (7.)$$

q being some new constant, and $\phi(x)$ being some new rational function of x, which does not contain the polynome $x^5 + \mathrm{D}\,x + \mathrm{E}$ as a divisor.

Demonstration.—Let $x_1\, x_2\, x_3\, x_4\, x_5$ denote the five roots of the equation (1.), which are supposed to be all unequal among themselves, and different from 0; and let us put for abridgement

$$\left.\begin{array}{l}
f(x_1) - \dfrac{x_1}{x_5} f(x_5) = h_1, \\[2ex]
f(x_2) - \dfrac{x_2}{x_5} f(x_5) = h_2, \\[2ex]
f(x_3) - \dfrac{x_3}{x_5} f(x_5) = h_3, \\[2ex]
f(x_4) - \dfrac{x_4}{x_5} f(x_5) = h_4, \\[2ex]
\dfrac{f(x_5)}{x_5} = q, \quad \mathrm{Q} + q = \mathrm{Q}'.
\end{array}\right\} \ldots\ldots\ldots\ldots\ldots\ldots \quad (8.)$$

We shall then have

$$f(x_1) = h_1+q\,x_1,\ f(x_2) = h_2+q\,x_2,\ f(x_3) = h_3+q\,x_3,\ f(x_4) = h_4+q\,x_4,\ f(x_5) = q\,x_5, \quad \ldots\ldots\ldots \quad (9.)$$

and the result (4.) of the elimination of x between the equations (1.) and (2.), may be expressed as follows:

$$0 = (y-Q'x_1-h_1)(y-Q'x_2-h_2)(y-Q'x_3-h_3)(y-Q'x_4-h_4)(y-Q'x_5). \quad \ldots\ldots\ldots \quad (10.)$$

Comparing (10.) with (4.), and observing that the form of the equation (1.) gives the relations

$$0 = x_1+x_2+x_3+x_4+x_5, \quad \ldots\ldots\ldots \quad (11.)$$

$$0 = x_1x_2+x_2x_3+x_3x_4+x_4x_5+x_5x_1 + x_1x_3+x_2x_4+x_3x_5+x_4x_1+x_5x_2, \quad \ldots\ldots \quad (12.)$$

$$0 = x_1x_2x_3+x_2x_3x_4+x_3x_4x_5+x_4x_5x_1+x_5x_1x_2 + x_1x_3x_4+x_2x_4x_5+x_3x_5x_1+x_4x_1x_2+x_5x_2x_3, \quad (13.)$$

we easily find these expressions for A′ and C′, namely,

$$A' = -(h_1+h_2+h_3+h_4), \quad \ldots\ldots\ldots\ldots \quad (14.)$$

and

$$C' = -Q'^2(h_1x_1^2+h_2x_2^2+h_3x_3^2+h_4x_4^2) + Q'\left\{\begin{array}{l} h_1h_2(x_1+x_2)+h_1h_3(x_1+x_3)+h_1h_4(x_1+x_4) \\ +h_2h_3(x_2+x_3)+h_2h_4(x_2+x_4)+h_3h_4(x_3+x_4) \end{array}\right\} - (h_1h_2h_3+h_1h_2h_4+h_1h_3h_4+h_2h_3h_4) \quad \ldots\ldots\ldots \quad (15.)$$

If, then, the coefficient C′, as well as A′, is to vanish independently of Q, and consequently of Q′, we must have the four following equations:

$$0 = h_1+h_2+h_3+h_4; \quad \ldots\ldots\ldots \quad (16.)$$

$$0 = h_1x_1^2+h_2x_2^2+h_3x_3^2+h_4x_4^2; \quad \ldots\ldots\ldots \quad (17.)$$

$$0 = h_1h_2(x_1+x_2)+h_1h_3(x_1+x_3)+h_1h_4(x_1+x_4) + h_2h_3(x_2+x_3)+h_2h_4(x_2+x_4)+h_3h_4(x_3+x_4); \quad (18.)$$

$$0 = h_1h_2h_3+h_1h_2h_4+h_1h_3h_4+h_2h_3h_4; \quad \ldots\ldots\ldots \quad (19.)$$

which give, by elimination of h_4,

$$0 = h_1(x_1^2-x_4^2)+h_2(x_2^2-x_4^2)+h_3(x_3^2-x_4^2), \quad (20.)$$

$$0 = h_1^2x_1+h_2^2x_2+h_3^2x_3+(h_1+h_2+h_3)^2x_4, \quad (21.)$$

$$0 = h_2+h_3)(h_3+h_1)(h_1+h_2). \quad \ldots\ldots\ldots \quad (22.)$$

Of the three factors of the last of these equations, it is manifestly indifferent *which* we employ; since the conclusions which can be drawn from the consideration of any one of these three factors can also be drawn from the consideration of either of the other two, by merely interchanging two of the three roots $x_1\,x_2\,x_3$, without altering the other of those three roots, or the two remaining roots $x_4\, x_5$ of the equation (1.). We shall therefore take the first of the three factors of (22.), namely, the equation

$$0 = h_2 + h_3; \quad \text{.....................} \quad (23.)$$

which reduces the two equations (20.) and (21.) to the two following, obtained by elimination of h_3,

$$0 = h_1\,(x_1^2 - x_4^2) + h_2\,(x_2^2 - x_3^2), \quad \text{......} \quad (24.)$$

$$0 = h_1^2\,(x_1 + x_4) + h_2^2\,(x_2 + x_3). \quad \text{......} \quad (25.)$$

These two last equations give, by elimination of h_2,

$$0 = h_1^2(x_1 + x_4)\{(x_1 + x_4)(x_1 - x_4)^2 + (x_2 + x_3)(x_2 - x_3)^2\}; \quad (26.)$$

in which we cannot suppose the factor $x_1 + x_4$ to vanish, because the relations

$$0 = x_1^5 + \mathrm{D}\,x_1 + \mathrm{E}, \quad 0 = x_4^5 + \mathrm{D}\,x_4 + \mathrm{E}, \qquad (27.)$$

give
$$\left.\begin{aligned} \mathrm{D} &= -(x_1^4 + x_1^3 x_4 + x_1^2 x_4^2 + x_1 x_4^3 + x_4^4), \\ \mathrm{E} &= \quad (x_1 + x_4)\,(x_1^2 + x_4^2)\,x_1\,x_4, \end{aligned}\right\} \quad (28.)$$

and we have supposed that E does not vanish; and since, for a similar reason, we cannot suppose that $x_2 + x_3$ vanishes, we see that we must conclude

$$h_1 = 0, \quad h_2 = 0, \quad h_3 = 0, \quad h_4 = 0, \qquad (29.)$$

unless we can suppose that the third factor of (26.) vanishes, that is, unless

$$(x_1 + x_4)\,(x_1 - x_4)^2 + (x_2 + x_3)\,(x_2 - x_3)^2 = 0. \qquad (30.)$$

Let us then examine into the meaning of this last condition, and the circumstances under which it can be satisfied.

If we put, for abridgement,

$$x_2 + x_3 = -\alpha, \quad x_2\,x_3 = \beta, \quad \text{.........} \quad (31.)$$

the condition (30.) will become

$$0 = x_4^3 - x_4^2\,x_1 - x_4\,x_1^2 + x_1^3 - \alpha^3 + 4\,\alpha\,\beta; \qquad (32.)$$

and we shall have, in virtue of the relations (11.) (12.) (13.), two other equations between x_4, x_1, α, β, namely,

$$0 = x_4^2 + x_4\,(x_1 - \alpha) + x_1^2 - x_1\,\alpha + \alpha^2 - \beta, \qquad (33.)$$

and

$$0 = x_1^3 - x_1^2\,\alpha + x_1\,(\alpha^2 - \beta) - \alpha^3 + 2\,\alpha\,\beta; \quad \text{...} \quad (34.)$$

between which three equations, (32.) (33.) (34.), we shall now proceed to eliminate x_4 and x_1. For this purpose we may begin by multiplying (33.) by x_1, and adding the product to (32.); a process which gives, by (34.),

$$0 = x_4^3 - x_4\,x_1\,\alpha + x_1^3 + 2\,\alpha\,\beta, \quad \text{.........} \quad (35.)$$

a relation more simple than (32.). In the next place we may observe that, in general, the result of elimination of any variable x between any two equations of the forms

$$\left.\begin{aligned} 0 &= p' + q' x + r' x^2 + s' x^3, \\ 0 &= p'' + q'' x + r'' x^2, \end{aligned}\right\} \qquad (36.)$$

is

$$\begin{aligned} 0 = {} & p'^2 r''^3 - p' q' q'' r''^2 - 2 p' r' p'' r''^2 + p' r' q''^2 r'' + 3 p' s' p'' q'' r'' \\ & - p' s' q''^3 + q'^2 p'' r''^2 - q' r' p'' q'' r'' - 2 q' s' p''^2 r'' + q' s' p'' q''^2 \\ & + r'^2 p''^2 r'' - r' s' p''^2 q'' + s'^2 p''^3, \quad \ldots\ldots\ldots\ldots \quad (37.) \end{aligned}$$

Applying this general formula to the elimination of x_4 between the equations (35.) and (33.), and making, for that purpose,

$$\left.\begin{aligned} p' &= x_1^3 + 2\alpha\beta, & q' &= -x_1\alpha, & r' &= 0, & s' &= 1, \\ p'' &= x_1^2 - x_1\alpha + \alpha^2 - \beta, & q'' &= x_1 - \alpha, & r'' &= 1, \end{aligned}\right\} \ldots \quad (38.)$$

we find, after some easy reductions,

$$\begin{aligned} 0 = {} & 4 x_1^6 - 4 x_1^5 \alpha + x_1^4 (8\alpha^2 - 6\beta) + x_1^3 (-8\alpha^3 + 14\alpha\beta) \\ & + x_1^2 (6\alpha^4 - 12\alpha^2\beta + 3\beta^2) + x_1 (-2\alpha^5 + 7\alpha^3\beta - 7\alpha\beta^2) \\ & + \alpha^6 - 7\alpha^4\beta + 13\alpha^2\beta^2 - \beta^3; \quad \ldots\ldots\ldots\ldots \quad (39.) \end{aligned}$$

which is easily reduced by (34.) to the form

$$\begin{aligned} 0 = {} & x_1^2 (2\alpha^4 - 2\alpha^2\beta + \beta^2) + x_1 (2\alpha^5 - 7\alpha^3\beta + \alpha\beta^2) + \alpha^6 - 3\alpha^4\beta \\ & + 5\alpha^2\beta^2 - \beta^3. \quad \ldots\ldots\ldots\ldots \quad (40.) \end{aligned}$$

Again, applying the same general formula (37.) to the elimination of x_1 between the equations (34.) and (40.), by making now

$$\left.\begin{aligned} & p' = -\alpha^3 + 2\alpha\beta, \; q' = \alpha^2 - \beta, \; r' = -\alpha, \; s' = 1, \\ & p'' = \alpha^6 - 3\alpha^4\beta + 5\alpha^2\beta^2 - \beta^3, \; q'' = 2\alpha^5 - 7\alpha^3\beta + \alpha\beta^2, \; r'' \\ & \qquad = 2\alpha^4 - 2\alpha^2\beta + \beta^2, \end{aligned}\right\} (41.)$$

we find after reductions,

$$\begin{aligned} 0 = {} & 25\alpha^{18} - 250\alpha^{16}\beta + 975\alpha^{14}\beta^2 - 1850\alpha^{12}\beta^3 \\ & + 1725\alpha^{10}\beta^4 - 700\alpha^8\beta^5 + 100\alpha^6\beta^6, \quad \ldots \quad (42.) \end{aligned}$$

that is,

$$0 = 25\alpha^6 (\alpha^2 - 2\beta)^2 (\alpha^4 - 3\alpha^2\beta + \beta^2)^2. \quad \ldots\ldots \quad (43.)$$

But this condition cannot be satisfied, consistently with the suppositions which we have already made that neither D nor E vanishes; because, by expressions similar to (28.), we have

$$\mathrm{D} = -(\alpha^4 - 3\alpha^2\beta + \beta^2), \; \mathrm{E} = -\alpha\beta(\alpha^2 - 2\beta). \quad (44.)$$

We must therefore reject the supposition (30.), and adopt the only other alternative, namely, (29.); and hence we have, by (9.),

$$\begin{aligned} & f(x_1) = q x_1, f(x_2) = q x_2, f(x_3) = q x_3, f(x_4) \\ & \qquad = q x_4, f(x_5) = q x_5. \quad \ldots\ldots\ldots \quad (45.) \end{aligned}$$

In this manner we find, that, under the circumstances supposed in the enunciation of the theorem, the function

$$f(x) - q\,x$$

vanishes, for every value of x which makes the polynome $x^5 + \mathrm{D}\,x + \mathrm{E}$ vanish; and since these values have been supposed unequal, we must have, therefore,

$$f(x) - q\,x = (x^5 + \mathrm{D}\,x + \mathrm{E}) \, . \, \phi\,(x), \quad \ldots\ldots\ldots \quad (46.)$$

the function $(\phi\,x)$ being rational, like $f(x)$, and not containing $x^5 + \mathrm{D}\,x + \mathrm{E}$ as a divisor; which was the thing to be proved.

Corollary. It is evident that, under the circumstances above supposed, the coefficients $\mathrm{B}'\ \mathrm{D}'\ \mathrm{E}'$ of (4.) will be expressed as follows:

$$\mathrm{B}' = 0, \quad \mathrm{D}' = \mathrm{Q}'^4\,\mathrm{D}, \quad \mathrm{E}' = \mathrm{Q}'^5\,\mathrm{E}; \quad \ldots\ldots \quad (47.)$$

that is, the equation of the 5th degree in y will be of the form

$$0 = y^5 + \mathrm{Q}'^4\,\mathrm{D}\,y + \mathrm{Q}'^5\,\mathrm{E}. \quad \ldots\ldots\ldots\ldots \quad (48.)$$

At the same time the relation between y and x will reduce itself, by (2.) and (7.), to the form

$$y = \mathrm{Q}'\,x + (x^5 + \mathrm{D}\,x + \mathrm{E}) \, . \, \phi\,(x), \quad \ldots\ldots \quad (49.)$$

Q' still denoting $\mathrm{Q} + q$. If, then, we were to establish this additional supposition

$$\mathrm{D}' = \tfrac{1}{3}\,\mathrm{B}'^2, \quad \ldots\ldots\ldots\ldots\ldots \quad (50.)$$

in order to complete the reduction of (4.) to De Moivre's solvible form, we should have

$$\mathrm{Q}'^4 = 0, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (51.)$$

that is,

$$\mathrm{Q}' = 0; \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (52.)$$

the equation of the fifth degree in y would become

$$y^5 = 0, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (53.)$$

and the relation between y and x would become

$$y = (x^5 + \mathrm{D}\,x + \mathrm{E}) \, . \, \phi\,(x); \quad \ldots\ldots\ldots \quad (54.)$$

and thus, although the equation in y would indeed be easily solvible, yet it would entirely fail to give any the least assistance towards resolving the proposed equation of the fifth degree in x.

Observatory, Dublin, May 13, 1836.

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

The Principles of Hydrostatics. By Thomas Webster, M.A., of Trinity College, Cambridge. 1835.

The Theory of the Equilibrium and Motion of Fluids. By the same Author. Cambridge, 1836.

IN the Preface to the first of these works Mr. Webster states that he has "endeavoured to develop the principles of the science of Hydrostatics with the use of none but the most elementary mathematics; so that the student, who now either partially or wholly neglects this beautiful branch of natural philosophy from the uninviting character which analysis presents to those who are not familiar with it, may at once proceed to its study if he is only acquainted with the first principles of algebra and mechanics. It is not from thinking other methods preferable or even comparable with the analytical that I have adopted this plan, but with the view of bringing the subject within the reach of those who have not been initiated in analysis." By pursuing this plan the author has produced a work of more general utility than if he had introduced more analytics, since many that might not have ability or inclination to follow a train of reasoning conducted by mathematical symbols, would take interest in and receive benefit from an exposition of the principles on which such reasoning may be founded, and by a statement of facts and results. Besides which, the nature of the work admits of the introduction of subjects which in the present state of science do not admit of exact mathematical treatment, and which are nevertheless fully as useful in a practical point of view, or instructive as branches of natural philosophy, as many of those that do. These subjects are, Steam and its applications; the mechanical application of the Motion of Fluids; Dalton's law of the diffusion of Gases coexisting in the same space; Winds; Trade Winds; Evaporation; Theory of Rain, &c., which will be found to be clearly and concisely treated in this work. It adds, however, to the confidence we place in works of this description to know that they are written by mathematicians. Mr. Webster establishes a claim to being considered such by the able manner in which the second of the above-named treatises is composed, which is purely mathematical, being intended to carry the student to the highest analytical deductions from the first principles of hydrostatics and hydrodynamics that could be prudently introduced into an elementary work. Accordingly, besides the usual propositions, treated for the most part in the usual manner, there is additional matter on subjects that have scarcely yet acquired a standing in elementary treatises, viz. Laplace's Theory of Capillary Attraction, Specific Heat, and the Law of Cooling, in the statical part; aerial vibrations and their propagation, considerd as the immediate causes of sound, together with musical vibrations in cylindrical tubes, in the dynamical part. In these portions of the work Mr. Webster has drawn largely from recent memoirs both of foreigners and our own countrymen, and has endeavoured to make their productions more accessible to the mathematical student by breaking up the simpler parts into distinct propositions.

As these subjects are inferior to few in the interest that attaches to them, and would be more generally attended to if the mathematical calculation by which the reasoning in most of them is necessarily conducted could be simplified, any attempt, like that in the work before us, to do this, is deserving of our approbation.

XCIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

1836. Feb. 25.—A PAPER was read, "On an artificial Substance resembling Shell; by Leonard Horner, Esq., F.R.S. L. and Ed.: with an account of the examination of the same; by Sir David Brewster, K.H., LL.D., F.R.S., &c."

The author, having noticed a singular incrustation on both the internal and external surfaces of a wooden dash-wheel, used in bleaching, at the Cotton Factory of Messrs. J. Finlay and Co., at Catrine, in Ayrshire, instituted a minute examination of the properties and composition of this new substance. He describes it as being compact in its texture, of a brown colour, and highly polished surface, with a metallic lustre, and presenting in some parts a beautiful iridescent appearance: when broken, it exhibits a foliated structure. Its obvious resemblance, in all these respects, to many kinds of shell, led the author to inquire into its intimate mechanical structure, and into the circumstances of its formation. He found, by chemical analysis, that it was composed of precisely the same ingredients as shell; namely, carbonate of lime and animal matter. The presence of the former was easily accounted for; as the cotton cloths which are placed in the compartments of the wheel, in order that they may be thoroughly cleansed by being dashed against its sides, during its rapid revolutions, have been previously steeped and boiled in lime water. But it was more difficult to ascertain the source of the animal matter; this, however, was at length traced to the small portion of glue, which, in the factory where the cloth had been manufactured, was employed as an ingredient in forming the paste, or dressing, used to smooth and stiffen the warp before it is put into the loom. These two materials, namely lime and gelatine, being present in the water in a state of extreme division, are deposited very slowly by evaporation; and thus compose a substance which has a remarkable analogy to shell, not only in external appearance, and even pearly lustre, but also in its internal foliated structure, and which likewise exhibits the same optical properties with respect to double refraction and polarizing powers.

A letter from Sir David Brewster, to whom the author had submitted for examination various specimens of this new substance, is subjoined; giving an account of the results of his investigations of its mechanical and optical properties. He found that it is composed of laminæ, which are sometimes separated by vacant spaces, and at others, only slightly coherent; though generally adhering to each other with a force greater than that of the laminæ of sulphate of lime, or of mica; but less than those of calcareous spar. When the adhering plates are separated, the internal surfaces are sometimes colourless, especially when these surfaces are corrugated or uneven; but they are almost always covered with an iridescent film of the most brilliant and generally uniform tint, which exhibits all the variety of colours displayed by thin plates or polarizing laminæ. This substance, like most crystallized bodies, possesses the property of refracting light doubly; and, as in agate and mother-of-pearl, one of the two images is perfectly distinct, while the other contains a considerable portion of nebulous light, varying with the thickness of the plate, and the inclination of the refracted ray. Like calcareous spar, it has one axis of double refraction, which is negative; and it gives, by polarized light, a beautiful system of coloured rings. It belongs to the rhombohedral system, and, as in the *Chaux carbonatée basée* of Haüy, the axis of the rhombohedron, or that of double refraction, is perpendicular to the surface of the thin plates. As mother-of-pearl has, like arragonite, two axes of double refraction; this new substance may be regarded as having the same optical relation to calcareous spar that mother-of-pearl has to arragonite.

The flame of a candle, viewed through a plate of this substance, presents two kinds of images; the one bright and distinct, the others faint and nebulous, and having curvatures, which vary as the inclination of the plate is changed: the two kinds being constituted by oppositely polarized pencils of light. On investigating the cause of these phenomena, Sir David Brewster discovered it to be the imperfect crystallization of the substance; whence the doubly refracting force separates the incident light into two oppositely polarized pencils, which are not perfectly equal and similar. In this respect, indeed, it resembles agate, mother-of-pearl, and some other substances; but it differs from all other bodies in possessing the extraordinary system of composite crystallization, in which an infinite number of crystals are disseminated equally in every possible azimuth, through a large crystalline plate; having their axes all inclined at the same angle to that of the larger plate, and producing similar phenomena in every direction, and through every portion of the plate: or this remarkable structure may be otherwise described, by saying that the minute elementary crystals form the surfaces of an infinite number of cones, whose axes pass perpendicularly through every part of the larger plate.

An examination of the phenomena of iridescence afforded by this new substance, leads him to the conclusion that the iridescent films are formed at those times when the dash-wheel is at rest, during the night, and that they differ in their nature from the rest of the substance. These phenomena illustrate in a striking manner some analogous appearances of incommunicable colours presented by mother-of-pearl, which had hitherto baffled all previous attempts to explain them; but which now appear to be produced by occasional intermissions in the processs by which the material of the shell is secreted and deposited in the progress of its formation.

March 3.—A paper was read, entitled, "Researches on the Tides. Fifth Series: On the Solar Inequality and on the Diurnal Inequality of the Tides at Liverpool." By the Rev. William Whewell, F.R.S., Fellow of Trinity College, Cambridge.

The inequality both in the height and time of high water in the morning and evening tides of the same day, which varies according to a law depending on the time of the year, is termed by the author *the diurnal inequality*, because its cycle is one day. The existence of such an inequality has often been noticed by seamen and other observers; but its reality has only recently been confirmed by regular and measured observations; and its laws have never as yet been correctly laid down. The author gives an account of the observations now in progress at different ports, from which he expects they will be ascertained with great precision. He traces the correspondence of the observations of the diurnal inequality already made with the equilibrium theory; and remarks that the semi-diurnal tides, alternately greater and less, which are transmitted from the Southern Ocean to Liverpool, may be compared to the oscillations of a fluid mass: and that they are augmented by the action of the forces occurring at intervals equal to those of the oscillations. Hence the oscillations go on increasing for a considerable period after the forces have gone on diminishing, and reach their maximum a week after the forces have passed theirs.

The remaining sections of this paper are devoted to the investigation of the Solar inequalities at Liverpool. By carefully eliminating the Lunar effects, which the author is enabled to do by the aid of the preceding researches, he has determined the approximate circumstances of the Solar correction for the height. He has also obtained evidence of the existence, and some knowledge of the laws of the Solar inequalities of the times; and these inequalities, as thus discovered, are found to exhibit the same general agreement with the equilibrium theory which has been disclosed in all the inequalities hitherto detected. The results of the extensive observations now obtained are sufficiently precise to indicate the defects of our mathematical theories of hydrodynamics; and some of these are pointed out by the author, who remarks that although a short time ago the theory was in advance of observation, at present observation is in advance of theory; which mathematicians are therefore called upon to remodel and perfect.

The author proceeds to consider the effect of the Moon's declination on the Tides at Liverpool; which, as before observed, it is necessary to eliminate, in order to obtain the Solar inequality; and gives an explanation of various formulæ and tables constructed for that object. He then investigates the laws of the solar inequalities, first, as to the heights; and secondly, as to the times of high water at Liverpool, by applying to them these methods of calculation.

March 10.—"Report of Magnetic Experiments tried on board an Iron Steam-Vessel, by order of the Right Hon. the Lords Commissioners of the Admiralty." By Edward J. Johnson, Esq., Commander, R.N., accompanied by plans of the vessel, and tables showing the horizontal deflection of the Magnetic Needle at different positions on board, together with the dip and magnetic intensity observed at those positions, and compared with that obtained on shore with the same instruments. Communicated by Captain Beaufort, R.N., F.R.S., Hydrographer to the Admiralty; by command of the Right Hon. the Lords Commissioners of the Admiralty.

This report commences with a description of the iron steam-vessel, the "Garryowen," belonging to the City of Dublin Steam Packet Company, and built by the Messrs. Laird, of Liverpool. She is constructed of malleable iron, is 281 tons burthen, and draws only 5¼ feet water, although the weight of iron in the hull, machinery, &c. is 180 tons.

This vessel was placed under the directions of the author, in Tarbert Bay, on the Shannon, on the 19th of October, 1835, for the purpose of investigating its local attractions on the compass. The methods which were adopted with that view are given; together with tables of the results of the several experiments, and plans of the various parts of the Garryowen. The horizontal deflections of the magnetic needle at different situations in the vessel were observed, for the purpose of ascertaining the most advantageous place for a steering compass, and also for the application of Professor Barlow's correcting plate: and the dip and intensity in these situations were, at the same time, noted.

An experiment is detailed, showing that where several magnetic needles, freely suspended, were placed upon the quay, in Tarbert Bay, and the vessel warped from the anchorage towards them, first with her head in that direction and then with her stern, opposite deflections were produced: in the first case all the needles showing a deviation to the eastward, and in the latter to the westward, of the true magnetic meridian.

Considering the height of the general mass of iron in the vessel and also that of the head and stern, together with the distance (169 feet) at which some of the needles indicated a deviation, the author concludes that the respective deflections were caused by the magnetic influence of the iron in the vessel; the combined effect of that about the bows representing the north pole of a magnet, and that about the stern a south pole. He then offers several suggestions for future observation on this subject, and connected with the little oxidation that is reported to have taken place in the vessel.

The experiments having been interrupted by a continuance of wet and stormy weather, the author proceeds to draw the following general practical conclusions, deduced from the series of observations already made, and points out the further experiments which he considers necessary to be tried.

1st. The ordinary place for a steering-compass on board ship is not a proper position for it in an iron steam-vessel.

2nd. The binnacle-compass in its usual place on board the Garryowen is too much in error to be depended upon.

3rd. In selecting a proper position for a steering-compass on board iron steam-vessels, attention should be paid to its being placed, as far as is practicable, not only above the general mass of iron, but also above any smaller portions of iron that may be in its vicinity; or such portions of iron should be removed altogether.

4th. The steering-compass should never be placed on a level with the ends either of horizontal or of perpendicular bars of iron.

5th. The extreme ends of an iron vessel are unfavourable positions, in consequence of magnetic influences exerted in those situations. The centre of the vessel is also very objectionable, owing to the connecting rods, shafts, and other parts of the machinery belonging to the steam-engine and wheels, which are in continual motion; independently of the influence exerted by the great iron tunnel in this part of the ship.

6th. No favourable results were obtained by placing the compass either below the deck, or on a stage over the stern.

7th. It was found that at a position 20¼ feet above the quarter-deck, and at another 13¼ feet above the same level, and about one seventh the length of the vessel from the stern, the deflections of the horizontal needle were less than those which have been observed in some of His Majesty's ships.

The author proceeds to point out various methods of determining, by means of a more extended inquiry, whether the position above indicated, or one nearer to the deck, is that at which the steering-compass would be most advantageously placed.

The concluding section contains an account of some observations made by the author on the effects of local attraction on board different steam-boats, from which it appears that the influence of this cause of deviation is more considerable than has been generally imagined; and he points out several precautions which should be observed in placing compasses on board such vessels.

"Researches on the Integral Calculus. Part I." By Henry Fox Talbot, Esq., F.R.S.

The author premises a brief historical sketch of the progress of discovery in this branch of analytical science. He observes that the first inventors of the integral calculus obtained the exact integration of a certain number of formulæ only; resolving them into a finite number of terms, involving algebraic, circular, or logarithmic quantities, and developing the integrals of others into infinite series. The first great improvement in this department of analysis was made by Fagnani, about the year 1714, by the discovery of a method of rectifying the differences of two arcs of a given biquadratic parabola, whose equation is $x^4 = y$. He published, subsequently, a variety of important theorems respecting the division into equal parts of the arcs of the lemniscate, and respecting the ellipse and hyperbola; in both of which he showed how two arcs may be determined, of which the difference is a known straight line. Further discoveries in the algebraic integration of differential equations of the fourth degree were made by Euler; and the inquiry was greatly extended by Legendre, who examined and classified the properties of elliptic integrals, and presented the results of his researches in a luminous and well-arranged theory. In the year 1828, Mr. Abel, of Christiana, in

Norway, published a remarkable theorem, which gives the sum of a series of integrals of a more general form, and extending to higher powers than those in Euler's theorem; and furnishes a multitude of solutions for each particular case of the problem. Legendre, though at an advanced age, devoted a large portion of time to the verification of this important theorem, the truth of which he established upon the basis of the most rigorous demonstration. M. Poisson has, in a recent memoir, considered various forms of integrals which are not comprehended in Abel's formula.

The problem, to the solution of which the author has devoted the present paper, is of a more general nature than that of Abel. The integrals, to which the theorem of the latter refers, are those comprised in the general expression $\int \frac{P\,dx}{\sqrt{R}}$ where P and R are entire polynomials in x. Next in order of succession to these, there naturally presents itself the class of integrals whose general expression is $\int \frac{P\,dx}{\sqrt[3]{R}}$, where the polynomial R is affected with a cubic, instead of a quadratic radical; but Abel's theorem has no reference to these, and consequently affords no assistance in their solution. The same may be said of every succeeding class of integrals affected with roots of higher powers. Still less does the theorem enable us to find the sum of such integrals as $\int \phi\,(R)\,dx$; R being, as before, any entire polynomial (that is, containing at least two different powers of x), and ϕ being any function whatever. The author then details the processes by which he arrives at the solution of this latter problem.

March 17.—A paper was read, "On the reciprocal attractions of positive and negative electric Currents, whereby the motion of each is alternately accelerated and retarded." By P. Cunningham, Esq., Surgeon R.N. Communicated by Alexander Copland Hutchison, Esq., F.R.S.

The author found that a square plate of copper, six inches in diameter, placed vertically in the plane of the magnetic meridian, and connected with a voltaic battery by means of wires soldered to the middle of two opposite sides of the plate, exhibited magnetic polarities on its two surfaces, indicative of the passage of transverse and spiral electrical currents, at right angles to the straight line joining the ends of the wires. The polarities were of opposite kinds on each side of this middle line, in each surface; and were reversed on the other surface of the plate. The intensities of these polarities at every point of the surface were greatest the greater its distance from the middle line, where the plate exhibited no magnetic action. The author infers from this and other experiments of a similar kind, that each electric current is subject, during its transverse motion, to alternations of acceleration and retardation, the positive current on the one side of the plate and the negative on the other, by their reciprocal attractions, progressively accelerating each other's motions, as they approach, in opposite directions, the edge round which they have to turn. After turning round the edge their motion will, he conceives, be checked by coming in contact with the accelerated portions of the opposing currents to which they respectively owed their former increase of velocity; so that the one current will be retarded at the part of the plate where the other is accelerated. To these alternate accelerations and retardations of electric currents during their progressive motion, the author is disposed to refer the alternate dark and luminous divisions in a platina wire heated by electricity, as was observed by Dr. Barker.

"Meteorological Journal kept at Allenheads, near Hexham." By the Rev. William Walton. Communicated in a letter to P. M. Roget, M.D., Sec. R.S.

This Journal contains a register of the height of the barometer, taken at 9 A.M. and at 3 P.M. during every day in January and February 1836, with remarks on the state of the weather during a few particular days. The station where the observations were made is elevated 1400 feet above the level of the sea.

March 24.—A paper was in part read, entitled "On the Temperatures and Geological Relations of certain Hot Springs; particularly those of the Pyrenees; and on the Verification of Thermometers." By James David Forbes, Esq., F.R.S., Professor of Natural Philosophy in the University of Edinburgh.

The Society then adjourned over the Easter vacation, to meet again on the 14th of April.

April 14.—The reading of Professor Forbes's paper, "On the Temperatures and Geological Relations of certain Hot Springs; particularly those of the Pyrenees; and on the Verification of Thermometers," was resumed and concluded.

The author expresses his regret that notwithstanding the great interest, more especially in a geological point of view, which attaches to every topic connected with the origin, the nature, and the permanence in temperature of the many thermal springs met with in different parts of the world, our information on these subjects is exceedingly deficient. On many points which might easily be verified, and which are of essential consequence towards obtaining a satisfactory theory of the phenomena, we as yet possess but vague and uncertain knowledge. It is evident that the first step towards the establishment of such a theory must consist in the precise determination of the actual temperature of each spring; from which we may derive the means of estimating by comparative observations, at different periods, the progressive variations, whether secular, monthly, or even diurnal, to which that temperature is subject. We have at present, indeed, not only to lament the total absence of exact data on which to found such an inquiry; but we are obliged to confess that, owing to the difficulties which meet us even in the threshhold, we have not, even at the present day, made any preparation for establishing the basis of future investigation, by applying such methods of experiment as are really in our power, and are commensurate with the superior accuracy of modern science. The researches of Fourier would lead us to the conclusion that, if the high temperature of these springs be derived solely from that of the interior portions of the earth, the changes which can have occurred in that temperature, during any period to which history extends, must be so minute as to be inappreciable. On the other hand, the theory of internal chemical changes, which have been assigned as the origin of volcanos, would suggest it as improbable that this temperature has remained constantly the same; and as a more likely occurrence, even were we to suppose that no uniform secular diminution took place, that it would be liable to occasional irregular fluctuations. The influence of earthquakes on the temperature of hot springs is also admitted; and it would be very desirable to learn, from a series of consecutive observations, whether abrupt changes, similar to those which have occasionally been noticed, are not of frequent occurrence.

The author has diligently laboured to collect, by observations made on the spot, materials for supplying this great chasm in the natural history of our globe. As an essential preliminary means of obtaining accurate results, he applied himself to the verification of the scales of the thermometers he employed in these researches: and he describes, in a separate section of this paper, the methods which he adopted for the attainment of this object. He first fixed with great precision the standard points of each thermometer, namely the freezing and boiling temperatures of water, by a mode which he specifies: and afterwards determined the intermediate points of the scale by a method, similar to that of Bessel; namely, that of causing a detached column of mercury to traverse the tube; but simpler in practice. Instead of employing for that purpose columns of mercury of arbitrary length, and deducing by a complex and tentative process the portions of the tube having equal capacities, the author detaches a column of mercury from the rest, of such a length as may be nearly an aliquot part of the length of the scale for 180°; and causes this column to step along the tube; the lower part of the column being brought successively to the exact points which the upper extremity had previously occupied: so that, at last, if its length has been properly chosen, the upper end of the column is found to coincide with the end of the scale: and this being accomplished, it is easy to apply to every part of the actual scale of the instrument the proper corrections, which may, for greater practical convenience, be drawn up in the form of a table.

In the next section, the author gives a detailed account of his observations of the mineral springs of the Pyrenees, made during the months of July and August, 1835, following them in their natural order from west to east, and describing their geological positions, the special circumstances of interest relating to them, and their actual temperatures.

In the third and last section he extends his inquiries to the hot springs met with in some other parts of Europe; and in particular, those of the baths of Mont Dor and of Bourboule, in France; of Baden-Baden, in Germany; of Loèsche, or Leuk, in the Vallais; of Pfeffers, in the canton of St. Gall, in Switzerland; and the baths of Nero, near Naples. The final results of all the observations contained in this paper are presented in the form of a table, with comparative columns of those derived from some unpublished observations of M. Arago, and of those of M. Anglada.

GEOLOGICAL SOCIETY.

(Continued from p. 160.)

Dec. 16, 1835.—A paper, entitled "Notes on the Geology of Denmark," by Dr. Beck of Copenhagen, and communicated by the President, was first read.

The only part of the Danish dominions in which gneiss and granitic rocks like those of Scandinavia appear, is in the north-east of the Island of Bornholm. To the south and south-west of these formations in the same island, are beds considered to be of the age of the Silurian system of Mr. Murchison; and on the eastern side of it are strata of the cretaceous period, all the intermediate groups being wanting.

Respecting the exact age of the lower part of the cretaceous beds in Bornholm, much difference of opinion has existed. By some it has been referred to the old carboniferous formation, on account of the presence of large quantities of coal, and impressions of ferns; by others to a lignite deposit of a very new or diluvial period; by M. Alexander Brongniart to the age of the lias; by Dr. Pingel to the iron sand of Messrs. Conybeare and Phillips; and by Dr. Beck to the English strata, from the Hastings sand to the upper green sand inclusive. The fossil ferns found in these beds belong to the genus Pecopteris, and some of the species have been named by M. Adolphe Brongniart. The seed-vessel of a monocotyledonous plant of common occurrence in these strata, and considered by Dr. Beck to belong to the family Restiaceæ, is identical with one in Mr. Mantell's collection obtained at Heathfield in Sussex. The few shells associated with the ferns which the author has examined are marine; and he conceives that these Bornholm beds were deposited in the sea at some distance from the mouth of the river which formed the Wealden system of England.

To the south of these coal-bearing strata are beds of siliceous and calcareous sand, containing between 30 and 40 species of shells, which also occur in the upper green sand of England: and in the neighbourhood of Arnager is a small patch of greyish white chalk with very few flints, but abundance of fossils, agreeing with those of the lower white chalk without flints at Southerham near Lewes.

In Denmark Proper the oldest formation belongs to beds of the cretaceous series, younger than those in the island of Bornholm. The lowest strata consist of pure white, soft chalk, with many layers of black, nodular flints, and contain more than 300 species of fossils. Among these remains, Ammonites are extremely scarce, Marsupites are unknown; and the remains of fishes, except teeth of the shark family, are very rare: but small zoophytes and microscopic foraminifera are very abundant; and, in some instances, animals of the sponge tribe, replaced by flint or chalcedony, but retaining their form, constitute complete beds. This portion of the chalk series forms, very generally, the lower part of the strata in Seeland and Jutland, and the whole of the cliffs of Moen. But in Moen masses of gravel and sand have, in consequence of great disturbances, become entangled with portions of disrupted chalk, in the manner explained by Mr. Lyell in a paper lately read before the Society*.

This white chalk is immediately overlaid, in Seeland and elsewhere, by the Faxoe beds, consisting almost entirely of hard, yellowish limestone, susceptible of a polish. They contain some of the characteristic fossils of the white chalk and some which are peculiar, belonging to the genera Arca, Modiola, Venus, Trochus, Fusus, Voluta, Oliva, Cypræa, Nautilus, &c.; while in the quarries at Faxoe (Seeland) they are composed so largely of zoophytes that they may justly be regarded as a coral reef. This division of the cretaceous series attains at Faxoe a thickness of more than 40 feet, but it is only between 2 and 4 feet thick at Stevensklint, where it may be traced for 3 or 4 miles resting upon the white chalk and covered by other strata of this series. The Faxoe beds appear also in some places in Jutland as in the Island of Mors, the cliffs near Greuaa, &c.

These beds have been imagined to be perfectly parallel to those of Maestricht, but the organic remains differ considerably; and are more analogous to those found at Künruth near Liege. Among the fossils common to the last locality and the Faxoe beds, are *Baculites Faujasii*, *Nautilus fricator* (Beck), *Fusus elongatus* (Beck), and *Terebratula subgigantea* (Schlotheim). Dr. Beck also states that the *Nautilus Danicus* is not identical with the *Nautilus aganiticus* of the lias, though Von Buch considers that it is: he likewise states that he has not been able to identify any of the Faxoe fossils with those of the oolitic series, or with the shells of Gosau, or with any of the tertiary fossils hitherto described.

The cretaceous beds which immediately cover the Faxoe deposit in Stevensklint, consist of a whitish and hardish chalk, including so great a number of broken and almost pulverised zoophytes that the rock is sometimes entirely composed of them. The bivalves and echinodermata are chiefly the same as those of the white chalk, but the univalves, so common in the Faxoe beds, are wanting, while many of the smaller corals which occur in those beds occur also in this upper limestone. The flint of these superior strata is sometimes in continuous layers as in Stevensklint, sometimes in nodules, and differs from the flint in the white chalk in being more opake, and having a less conchoidal fracture. Sometimes it is replaced by a bluish grey stone, composed of silex and lime, and called in Danish "bleger."

Dr. Beck infers from the organic remains that the chalk of Saltholm; of the cliffs in Jutland, ranging from Rugaard by Daugbjerg and Mönsted, and terminating in the neighbourhood of Hjerm; as well as the chalk of the south of Thyholm, that resting upon the white chalk in part of Mors and in the north of Thÿ; and the chalk of the cliffs of Bulbjerg and the islet Skarreklit belongs to this uppermost bed.

* Proceedings, No. 41. Vol. II. p. 191., or Lond. and Edinb. Phil. Mag., vol. vii, p. 412.

Upon the chalk in various districts in Denmark is a breccia of angular fragments of chalk and flint cemented with carbonate of lime. The chalk hills of Denmark present generally the same rounded, smooth outline as in many parts of England, with this distinction, that in Denmark they are crowned very commonly with small mammilliform hillocks of gravel, sand, and erratic blocks. As the sandy beds sometimes contain shells identical with those now living in the German Ocean, it is evident that the chalk in Denmark has been submerged since the existence of the living species of Testacea.

In Bornholm, Moen, and Seeland, the strike of the cretaceous strata is dependent on the strike of the most ancient granitic rocks in Scania; but in Jutland it is not parallel to them, and evidently was not caused by the same system of movements.

In the central parts of Jutland is an extensive formation several hundred feet thick, referred by Dr. Beck to tertiary strata probably older than the erratic blocks. It consists in some localities of white micaceous sand, in which occasionally occur traces of brown coal, and near Skanderberg is a considerable layer of it. In other districts the formation is composed of clay, which also contains mica, flat masses of hydraulic limestone, like the septaria of the London clay, and occasionally a few organic remains, consisting of scales of fishes apparently belonging to the Cyprinidæ; the elytra of beetles, the cases of the larvæ of Phryganæa, and an hymenopterous insect which the author has called *Cleptis Stenstrupii*. In the neighbourhood of Thisted at Thye, the north of Mors and in the island Füür, Dr. Beck observed, in 1831, dislocations which affect equally these tertiary strata and the chalk.

To the tertiary period belong also the beds discovered by Professor Forchhammer in the island of Sylt, on the western shores of Holstein. Some of the few shells hitherto detected in them Dr. Beck has ascertained to agree with characteristic fossils of the London clay, and others, as *Voluta Lambertii*, with shells of the crag.

To the same older tertiary period the author is inclined to refer the strata containing Valvata, Gyrogonites, &c., detected at Segeberg, and the deposit between Altona and Geuchstad in which Mr. Lyell discovered a valve of a Cardita.

Newer than any of the above-mentioned formations are the deposits of gravel, sand, and loam, often several hundred feet thick, which generally cover the older strata, and constitute almost the whole surface of Denmark. In and upon these beds, the erratic blocks so common in that kingdom first appear. They consist principally of the commoner varieties of the gneiss and granitic schists of Scandinavia; but in the neighbourhood of Copenhagen Dr. Beck has observed blocks of transition limestone, basalt with olivine, and the well-known secondary sandstone of Hör. In the northern part of Jutland he has also noticed blocks of Elfadal porphyry, and the blue zircon-syenite of Fredericksvaern in Norway. The gravel beds with erratic blocks rarely contain any fossils, but when shells do occur, they are often absolutely identical with living species. Dr. Beck has, however, found at Moen a specimen of Pleurotoma, which he believes to be tertiary, and these and at Himlingoie several specimens of Turritella not hitherto known as living.

From the difference of the fossils, together with the manner in which the gravel beds are disposed upon the chalk, he infers that the older strata have been elevated and submerged more than once.

Dr. Beck says that space does not permit him to give his views respecting the erratic blocks, and he merely states that their deposition took place after the beginning of the tertiary period, and went on during the accumulation of blue marl and sand, from which he has obtained more than 70 species of shells now living in the German Ocean; and that he has proofs, of which he intends to give a more detailed account hereafter, that the transportation of these blocks continues on the coast of Jutland.

In conclusion the author mentions the existence of several, small, lacustrine formations in the interior of Jutland and of Moen, containing remains of Lymnæa, Physa, Helix, &c.; and an extensive formation of sand cemented by oxide of iron.

An extract from a letter addressed to the President by H. Edwin Strickland, Esq., F.G.S., dated Athens, 26th Oct., 1835, was then read.

Mr. Strickland noticed first at Trieste the vast formation of secondary limestone which appears to extend thence uninterruptedly into Greece; and of which the Ionian Islands are almost wholly composed. In Corfu, however, are several obscure and complicated patches of tertiary deposits, and in Cephalonia is a Pliocene formation of vast thickness, containing abundance of fossils. Mr. Strickland then describes the currents of sea-water which constantly flow into the land near Argostoli in the island of Cephalonia. This extraordinary phenomenon occurs about a mile north of Argostoli at the very extremity of the rocky promontory which separates that town from the large bay on the west. The promontory is composed of the hard, white, secondary limestone, the strata dipping about 30° to the east; and at this spot it contains several species of shells which in general are rather rare. The streams of water have been noticed for many years rushing in between the rugged masses of rock of which the coast consists, but it was only about two years since that they excited the attention of the English. Mr. Stevens of Argostoli, desirous to turn them to advantage, was induced to stop up three of these holes, and by excavating a channel at the principal one, has been enabled to obtain a sufficient supply of water to turn a mill. The channel which has been made is about three feet wide, and the average depth of the current is six inches. In the mean state of the tide the fall is about 3 feet, the usual rise of the tide being 6 inches, but during southerly winds it is considerably more. After passing the wheel the current flows for 6 or 7 yards, and is then partly absorbed in swallow holes and partly disappears under the rocks. The water at the bottom of the excavation at greatest at high tides, the quantity of water then flowing in being greatest. A small freshwater spring enters the excavation on the land side, and when the sea is effectually stopped out, renders the water at the bottom of the excavation quite fresh in the course of a day; raising it at the same time several inches to a certain point, where it rests.

This circumstance, Mr. Strickland thinks, may be explained by the less specific gravity of the fresh water requiring a higher column to overcome the obstacles met with in its subterranean course. In order to ascertain the direction of the current, Col. Brown has had an excavation made, by which it appears that the stream does not pass unddr the sea at the opposite side of the promontory. Mr. Strickland, in explanation of the constant flowing into the land of these streams, objects to the proposition that the subterranean current may be absorbed by the incumbent soil and evaporated at the surface, as it occurs in an island of small extent: but agrees to the supposition that an earthquake has at some period opened a communication between the sea and the region of volcanic fire; that the water being there converted into steam, is afterwards condensed in its upward course, and forms those hot-springs which exist in various parts of Greece.

A paper on the occurrence of fossil vertebræ of fish of the shark family in the Loess of the Rhine, near Basle, by Charles Lyell, Esq., F.G.S., was afterwards read.

Mr. Lyell described in a memoir communicated to the Society in May, 1834,* the geographical extent of the Loess or ancient silt of the Rhine, as far as he had then examined it. In tracing its southern limits during last summer, he found it in considerable force at Basle, and still higher on the Rhine at Waldshutt, where it contains the usual land and freshwater shells. Beyond this point he did not trace the deposit; but from the information he received, he believes that it terminates between Waldshutt and Schaffhausen. He here alludes only to the loamy portion, which can be identified by its fossils; for the gravel beds with which the loess sometimes alternates in its lower part, are probably of much greater extent, and are not easily to be separated from any other ancient gravel in which bones or shells have not been discovered.

The loess at Basle crowns the summit, and is found on the sloping sides of several low hills which bound the valley of the Rhine; but it is best seen one or two miles to the south of the town, in the hills called Bruder Holz, where it rests upon nearly horizontal beds of molasse. The loess has here an elevation of more than 1100 feet above the sea; for it is found in places which are more than 300 feet above the Rhine at Basle, according to the measurement of Prof. Merian, who has also determined that the Rhine at Basle is about 760 French feet (809 English) above the level of the sea.

The principal section examined by Mr. Lyell is near the northern extremity of the Bruder Holz below the church of the village of Binningen. The loess in this place is of its usual yellowish grey colour, and is filled with terrestrial and freshwater shells. The lower beds alternate with strata of sand and gravel, and in one of the loamy strata of this part of the series, he found the vertebræ of fish, together with the following loess shells: *Succinea oblonga, Pupa muscorum, Clau-*

* See Proceedings of the Geological Society, No. 41. Vol. II. p. 83; Jameson's Journal, Vol. 19.; and Lond. and Edinb. Phil. Mag., vol. v., p. 223.

silia parvula, Helix cellaria, H. plebeium, H. arbustorum, H. rotundata, Bulimus lubricus, and a small Planorbis, all recent shells.

The vertebræ, M. Agassiz says, belong decidedly to the Squalidæ or shark family, perhaps to the genus Lamna. The one is a caudal and the other an abdominal vertebra, each about a quarter of an inch in diameter. They are in such a state of preservation, and of such a colour as might be expected in bones preserved in loess, and as they were in a bed of fine loam in which there were no extraneous fossils, nor any fragments of rock washed out of other formations, there is no reason to suspect that they could have been derived from the tertiary molasse; and M. Agassiz also states that he has seen nothing like them in the molasse of Switzerland. It may seem very extraordinary that the first remains of fossil fish obtained from this freshwater silt should belong to a marine genus, but M. Agassiz has informed Mr. Lyell that both in the Senegal and the Amazon certain species of the shark and skate families (Squalus and Raia, Linn.) have been known to ascend to the distance of several hundred miles from the ocean, and analogous facts are referred to in Marcgrave and Piso's Natural History of India.

A notice on the occurrence of selenite in the sands of the plastic clay at Bishopstone near Herne Bay, by William Richardson, Esq., F.G.S., was lastly read.

The perpendicular cliff in which the selenite occurs is about a hundred feet in height, and consists of the following strata:

Vegetable mould.	
Reddish marl or brick earth........	5 feet.
London clay..............	20 to 30 —
Sand and sandstone..............	60 —

The selenite is found in the sand, which, as far as the author could determine, contains no iron pyrites or lime except in a few well-defined lines of testaceous remains. The superjacent clay abounds in pyrites, and is thickly studded with transparent crystals of sulphate of lime, but no connexion could be traced between the two deposits, and the sands for five or six feet underlying the clay contain no selenite.

January 6, 1836.—A notice on the transportation of rocks by ice, extracted from a letter of Capt. Bayfield, R.N., addressed to Charles Lyell, Esq., P.G.S., was first read.

Capt. Bayfield says that both on the lakes of Canada and in the St. Lawrence he has seen fragmentary rocks carried by ice. The St. Lawrence is low in winter, and the loose ice accumulating on the extensive shoals which line each side of the river is frozen into a solid mass, being exposed to a temperature sometimes 30° below zero. The shoals are thickly strewed with boulders, which become entangled in the ice; and in the spring, when the river rises from the melting of the snow, the packs are floated off, frequently conveying the boulders for great distances. It is also well known that stones are carried by the ice. Anchors laid down within high-water mark to secure vessels hauled on shore for the winter, are cut out of the ice on the approach of spring, or they would be carried away. In 1834 the Gulnare's bower-anchor, weighing half a ton, was transported some yards by the ice, and so firmly was it fixed, that the force of the moving ice broke a chain cable as large as that of a 10-gun brig, and which had rode the Gulnare during the heaviest gales in the Gulf. The anchor was cut out of the ice or it would have been carried into deep water and lost.

With respect to rocks being transported by icebergs, Capt. Bayfield's testimony is equally conclusive, as he passed three seasons in the vicinity of the Strait of Belleisle. In an iceberg which he examined, boulders, gravel, and stones were thickly imbedded; and he saw others which owed their dirty colour to the same cause. Some of these immense ice-islands, Capt. Bayfield thinks, had been detached from the coast very far to the northward, perhaps from Baffin's Bay. The northern current brings similar masses in great numbers down the coast of Labrador every year, and they are very frequently carried through the straits, and for several hundred miles to the S.W. up the Gulf of St. Lawrence.

A paper "On the syenite veins which traverse mica slate at Goodland cliff and chalk at Torr Eskert, to the south of Fair Head in the county of Antrim," by Richard Griffith, Esq., F.G.S., and P.G.S. of Dublin, was afterwards read.

The part of Antrim to which this paper refers is situated between Fair Head on the north, and Cushleake mountain on the south. The base, or oldest formation of the district, consists of inclined strata of mica slate passing into gneiss, and containing subordinate beds of hornblende slate and schistose limestone. Upon the mica slate repose nearly horizontal and unconformable strata of coal measures, new red sandstone, and chalk; and the whole of these secondary deposits are surmounted by an overlying mass of rudely columnar trap, the northern extremity of which forms the magnificent promontory of Fair Head.

Besides the hornblende schist, which is interstratified with the mica slate and dips conformably with it, there are other rocks containing hornblende, which appear to be imbedded in the slate, but which are really intruded veins. On the sea-shore at Torr Point are two of these veins, consisting of syenite and syenitic green-stone; and they may be traced passing obliquely along the face of the stupendous and, for the greater part, perpendicular cliff of Goodland. On the sea-shore they appear so regular and conformable, both in strike and dip, to the strata of mica slate, that they might be considered as integral portions of it; but on minute inspection the syenite is found to mould into the rough and saw-like edges of the strata of mica-slate; and on tracing the veins as they gradually ascend the cliff, they are found to pursue undulating courses, neither parallel to each other nor to the laminæ of the slate, in some places approaching within four feet, and in others being more than 20 feet apart. To the south of the fault which traverses the cliff about 150 yards from Torr Point, the veins reappear at a higher level than on the north of the line of dislocations; and between the two previously noticed is a third and smaller one. Where first seen, this small vein is in contact with the upper surfaces of the lower vein, from which it gradually diverges and approaches the upper, but afterwards again descends towards the lower vein.

The mass of the two larger veins consists of dark green, crystallized hornblende, brownish red felspar, and occasionally quartz; and regular transitions may be traced from syenite to greenstone. When viewed at a distance they present a rudely columnar structure. The centre vein contains much black hornblende, some black quartz, and presents a concretionary structure, the oval-shaped masses being enveloped in a congeries of pinchbeck brown mica. A tendency to this structure is observable also in the upper vein.

Owing to the covered nature of the ground, the syenite veins of the coast cannot be traced continuously to Torr Eskert, but by laying down the line of the veins of Goodland cliff on the Ordnance Map, and making due allowance for their average inclination and the elevation of the hill, Mr. Griffith entertains no doubt that the syenite in the chalk of Torr Eskert is a prolongation of one of the syenite veins in the slate of the cliff.

The syenite which traverses the chalk cannot be distinguished from that of the mica slate, and passes also in syenitic greenstone. At one point the author had a portion of the surface soil removed, and obtained the following section:

Top.	Compact chalk	5 feet
	Syenite	5 —
	Chalk, irregular bed from 9 inches to 1 foot	
	Mica slate....................	

The lower bed of chalk contains quartz pebbles, green sand, and numerous, red, siliceous grains, some of which resemble garnets. The syenite presents large masses separated by chalk containing quartz-pebbles, green sand, and numerous fragments of fossils. These remains have nearly a vertical position when *in situ*, and Mr. Griffith consequently infers that they are not in the position in which they were deposited.

The irregularities on the surface of the chalk are accurately filled with the syenite: the chalk in immediate contact with the vein is usually compact, sometimes crystallized; and pebbles of quartz similar to those in the green sand and chalk are found occasionally in the syenite. The author noticed a small reniform mass of syenite imbedded in the chalk—the grain of the included portion being finer than that of the syenite in general. Small particles of chalk were likewise noticed in the syenite, and the union of the two rocks is so perfect that the chalk appears to be an integral portion of a compound deposit. Among other peculiarities exhibited at the junction of the two formations, the author mentions spheroidal masses of syenite included in the chalk; and, in conclusion, he says, that if the views which he has put forward have been substantiated, a new and important fact is added to those already described, which may ultimately lead us to attribute a comparatively recent origin not only to syenite veins and primary greenstone, but also to crystalline rocks generally when associated with schistose strata.

A letter from H. T. De la Beche, Esq., addressed to the President, and dated Truro, the 18th of December, 1835, was then read.

This letter was accompanied by a collection of fossils from the schistose rocks of the North of Cornwall, and presented to the Society on the part of the Ordnance Geological Survey.

Mr. De la Beche says that in the grauwacke of Western Somerset, Devon, and Cornwall natural divisions may be made, founded on marked characters. How far these divisions may coincide with those in Prof. Sedgwick's Cambrian system he has no means at present of judging; but he is of opinion that the whole of the district is older than the Silurian formations of Mr. Murchison.

Some of the organic remains obtained at Dinas Cove, in Padstow Harbour, belong to a system of beds consisting of slates, sandstones, and conglomerates, which encircles the northern flank of Dartmoor, then makes a great curve south of Launceston, bends afterwards northward round the Rough Tor and Brown Willy granite, and lastly, again inclines southward, crossing the Padstow river to the sea on the western coast. In various parts of this line the system is fossiliferous, particularly where limestone occurs or calcareous matter abounds. The Tintagel slate, long since shown by Dr. Buckland and the Rev. John Conybeare to contain organic remains, belongs to this system. Part of the fossils which accompanied the letter were procured from Trevelga Island (Lower St. Columb Porth), and Towan Head near New Quay, from the same series of beds, which, in consequence of an east and west anticlinal line ranging by St. Eval, St. Issey, and St. Breocks Downs, is folded over to the south, and constitutes the schistose system of St. Columb Major, St. Columb Minor, New Quay, &c.

The remainder of the fossils was obtained by Dr. Potts at the western entrance of Bodmin, and by Mr. De la Beche from the vicinity of Liskeard, on a prolongation of the same strata.

Altered or metamorphic rocks, having frequently the appearance of gneiss, mica slate, hornblende rock, &c., occur in the neighbourhood of Tintagel and Camelford; and Mr. De la Beche says, that a little care in tracing these rocks shows that they are altered portions of strata which possess the usual and varied characters of grauwacke. In conclusion he observes, that there is every reason to believe that two movements have taken place of the land in Somerset, Devon, and Cornwall, one to a height of 30 or 40 feet above the present sea-level, and another to an uncertain depth beneath it, since the vegetation of the land and the molluscous inhabitants of the neighbouring sea were the same as they now are.

January 20.—A paper was read "On the geological structure of Pembrokeshire, more particularly on the extension of the Silurian system of rocks into the coast cliffs of that county." By Roderick Impey Murchison, Esq., V.P.G.S.

This memoir was prefaced by an account of the origin of the terms Silurian and Cambrian Systems as applied to the older sedimentary deposits. Having occupied several years in establishing a fixed order of succession amid the strata of age anterior to the old red sandstone, and having finally named the formations in descending order, the Ludlow rock, Wenlock limestone, Caradoc sandstones, and Llandeilo flags, the author was urged by many leading geologists to propose a new, comprehensive name for this group, and thereby to prevent the confusion which had so long prevailed by the use of the words "Transition" and "Grauwacke." He adopted the term Silurian System, because the territory in which the successive formations above mentioned are exhibited, was formerly occupied by the ancient, British people the Silures. The Silurian rocks are underlaid by vast masses which rise up into the mountains of North, and the western part of South Wales, and to these Professor Sedgwick, connecting his labours with those of Mr. Murchison, has assigned the name of "Cambrian System." A portion of last summer was employed in tracing these rocks from Caermarthenshire into Pembroke, and in doing this the author was led to attempt a general survey of the county, examining the strata from the youngest to the oldest, dwelling, however, specially on the deposits of the "Silurian System."

Owing to its peninsulated form and the transverse fissures proceeding from Milford Haven into the heart of the county, Pembrokeshire affords great facilities for the comprehension of its mineral structure, and as the chief masses range from E. to W., sections from S. to N. expose the formations of which it is composed in descending order from the coal-measures to the Cambrian System. The points of novelty in the descriptions of the author apply to the persistence of the carboniferous deposits along the coast of St. Bride's Bay, where they are not separated by any mass of greywacke as indicated in former maps, the parts producing culm*, lying simply to the N. and S. of a highly dislocated promontory of carboniferous grit. The contortions and innumerable faults of these coal-measures being pointed out, attention is then called to some of the probable results of such movements in the singular accumulations of finely fractured stone coal in small basins called "slashes," and to other vertical downcasts of the mineral termed "sloughs." The shale of these culm deposits resembling in some respects certain strata of the upper Silurian rocks, might to an unpractised eye appear undistinguishable; but even where the order of superposition is not to be detected, essential differences are invariably to be observed, in the coal shales never containing those organic animal remains which are so abundant in the Silurian system, whilst the latter never contains a single plant similar to those which abound in the former. Instances are cited where by dislocations the coal measures are thrown into positions apparently conformable to old greywacke rocks of the Cambrian system, and hence the author surmises, that if the millstone grit and carboniferous limestone were not present in many adjoining parts to test the true age of these coal measures, mistakes might easily result from such juxta-positions. Cherty and siliceous sandstones (the millstone grit) rise in dome shapes to the west of Haverford, and occupy large portions of the coal tract underlying the productive culm measures and capping the mountain limestone.

Carboniferous Limestone.—In this formation, besides the very ac-

* All the coal of Pembroke is stone coal, and it is usually in the laminated condition of culm.

curate outline expressed in Mr. De la Beche's map*, the author remarks the existence of a double trough of the lower limestone shale overlying the old red sandstone in East Angle Bay; and he particularly adverts to the peculiar mineral character of these beds in Pembrokeshire in containing yellow and light coloured sandstones alternating with shale. The fossils of this lowest member of the carboniferous system are numerous, many having been furnished by the Earl of Cawdor; and as far as they have been yet examined they appear to differ specifically from all the fossils of the inferior systems. The coal measures and mountain limestone of Pembroke are singularly subject to great faults; one of the most remarkable of which occurs between Johnston and Haverfordwest, where the carboniferous limestone is thrown into a position by which it appears to overlay the coal.

Old Red Sandstone.—The upper strata of this great formation pass upwards in many places into the shale and sandstone of the carboniferous limestone, and the lowest members graduate into the Silurian system. The great mass consists of sandy shale, here termed the "red rab," associated with red sandstones and grits; but lithological variations from the usual types in Herefordshire have led to the belief that large districts (Cosheston, Williamston, Benton, &c.) consisted of greywacke. These are yellow, grey, and greenish micaceous sandstones which the author proves to be interlaced with the "red rab," and to occupy the same position as similar varieties of the rock previously described in Hereford, Radnor, &c. Some of the coarse grits (Canaston wood) are undistinguishable from the "greywacke" grit of the oldest rocks of the Cambrian system. Calcareous matter is very sparingly exhibited, imperfect concretions or very impure "cornstones" appearing only at wide intervals. The fishes so profusely detected by the author in the range of the formation through Salop, Hereford, and Monmouth, have not been observed. Amid the many faults affecting this formation, those by which the strata ranging from Caermarthen into Pembrokeshire have been powerfully bent and broken, and thrown into a westerly direction (Tavern Spite, &c.), are perhaps the most striking.

Silurian System.—Though the order of superposition and the organic remains clearly attest the age of the rocks of the Silurian system, the masses differ so much in mineral aspect from those selected as types that it is rarely possible to subdivide them into the Ludlow, Wenlock, Caradoc, and Llandeilo formations; but adopting the classification proposed, the author has laid down their course upon the map as two sub-groups consisting of "upper and lower Silurian rocks†."

The former parting with their mudstone characters are for the most part hard and siliceous, containing little calcareous matter, and are never subdivided by zones of limestone as at Aymestry and Wenlock. The lower Silurian rocks, on the contrary, are amply displayed in all

* The survey of the county was much facilitated by the possession of Mr. De la Beche's map of South Pembroke, which, though differing in some points from that completed by the author, is mentioned by him as a work of great merit for the period of its publication.

† See Lond. & Edinb. Phil. Mag., vol. vii. p. 46, Silurian System.

their characteristic forms, the limestones of the Llandeilo formation expanding to greater thicknesses (Llanpeter-Felfry, Llandewi, &c.) than in any other part of their course, and containing many beautiful fossils, including two unpublished species of Trilobites, common to Caermarthenshire. The chief mass of the Silurian system ranges from E. to W. across the county, passing by Haverfordwest, till its western extremity subsides beneath the coal measures of Druson Haven and St. Bride's Bay. Other bands of it rise from beneath the old red sandstone at Orlanton, Hoten, and Johnston; whilst a most remarkable zone is heaved up in an anticlinal line extending across the most southern promontory of the county (Castle Martin Hundred), from Fresh Water East to Fresh Water West. The most perfect succession of the rocks of which the system is composed, is exhibited in the bold coast cliffs of Marloes Bay, extending for a distance of two miles, in which space the uppermost strata, rising at angles of 35° to 40° from beneath the old red sandstone of Hook Point, are succeeded by conformable, underlying masses, until the whole graduates down and passes into the rocks of the Cambrian system in Wooltack Park and Skomer Island. The Ludlow and Wenlock formations can be here defined; the latter containing many well-known fossils. The lower Silurian rocks are still more largely developed; a vast thickness of fossiliferous sandy strata being quite identical with the "Caradoc sandstones," whilst the Llandeilo flags with *Asaphus Buchii* and *A. Bigsbii* (a new species of the author) occur in the haven called Moseley-wick Mouth. This coast section is 150 miles distant from the N. eastern extremity of the Silurian system.

The Cambrian System.—If divided by a line passing from E. to W., the northern half of Pembroke is exclusively composed of the older rocks of the Cambrian system, consisting, in descending order, of

a. Dark-coloured incoherent schists, with few stone bands, no calcareous matter, and scarcely any traces of organic remains. These occupy a great breadth, and, as in Caermarthenshire, they form the beds of passage between the Silurian and the Cambrian systems (sometimes without any break).

b. Hard grits and flagstones, coming strictly within the definition of greywacke of German mineralogists.

c. Hard purple sandstones and schists, identical with the slaty greywacke of the Longmynd, Salop, (the Lammermuir hills, Scotland, may be cited as a good and well-known type of these rocks).

d. Slates coarse and fine, with quartz veins and concretions.

At St. David's, Pantiphilip, and Scillyham, where the roofing-slates are quarried, the author has detected what he believes to be a coincidence between the laminæ of deposit as indicated by differently coloured layers of sediment, and the lines of slaty cleavage; though in the great majority of cases in Pembroke the rocks of this system, whether consisting of sandstone, schist, or hard slate, exhibit the divergences between the lines of true bedding and slaty cleavage, so clearly and ingeniously explained by Professor Sedgwick. The author therefore thinks it right to point to these exceptions to the observations of Prof. Sedgwick, because if strictly scrutinized the phænomena are not placed in opposition to them, since it was his belief upon the spot, that the crystallizing action which gave to these masses their hard slaty properties produced the flaglike laminæ of the beds.

Trap Rocks.—Of these there are distinctly two classes: 1, Bedded, and synchronous with the formation of the older rocks; 2, Posterior and intrusive. Of the former there are no examples like those cited in West Salop, Montgomery, and Radnor, (see former memoirs,) of alternation with the strata of the Silurian system, being all confined to the Cambrian rocks. The tract extending from Fishguard to St. David's and the Isle of Skomer offer illustrative examples of both these classes of trap. In addition to the varieties of sienite, compact felspar rock (corneen of De la Beche), greenstone, &c., of which these rocks are composed, the author has detected crystallized chromate of iron and albite in St. David's Head,—small veins of copper ore also occur between Solfach and St. David's. Among the more remarkable changes effected by the intrusive trap, he adverts to jaspidified schists inclosed between a large bifurcated mass of trap proceeding from Trafgarn. Having traced the Silurian system in a course of 120 miles from the Wrekin to Caermarthen in ridges more or less parallel running from *N.E.* to *S.W.*, the author has shown in former memoirs that this strike of the strata uniformly coincides with the direction of linear outbursts of volcanic matter. In Caermarthenshire vast dislocations and transverse breaks are exhibited by which the strata are for short distances thrown into E. and W. directions, but on the whole the south-westerly course is maintained. A ridge of intrusive rocks recently discovered, ranging between the rivers Towey and Taf (Castel, Cogan, &c.), having the same course, serves to explain how that dominant direction has been there preserved. In entering Southern Pembroke, however, the whole of the strata from the coal measures to the Cambrian rocks are thrown into an E. and W. direction, accompanied by violent contortions and powerful faults; whilst in northern parts of the county the old N.E. and S.W. direction prevails. As these converging lines are accompanied by linear, parallel ridges of trap rock, the author is confirmed in his belief that the forces which evolved the latter have been the proximate cause of such directions; and he further refers the extraordinary convulsions and dismemberments to which the strata in Pembroke have been subjected, to the interference of two great lines of elevation dependent upon volcanic activity. In accordance with phænomena observed in other parts of S. Wales, it is remarked that all the superficial detritus is of local origin, the southern or lower part of the county being partially strewed over with the debris of the rocks which rise into mountains on the north coast. After some observations on the blown sands, and the period of their formation, the author recapitulates the value of the Pembrokeshire coast sections in exhibiting the "Silurian System" precisely in the same geological position assigned to it from examinations in the interior; and concludes by stating it as his opinion, that as this one county is shown to contain rocks in the true coal measures and in the old red sandstone, as well as in the Silurian and Cambrian systems, which from their lithological characters have been mistaken for "greywacke," the use of that word as expressing the age of rocks is no longer consistent with the advanced state of geological science, and that if used, the name should either be rigidly restricted to some of the very oldest sedimentary deposits, or simply employed as a mineralogical definition of peculiar grits which actually reoccur in strata formed in many succesive epochs.

Feb. 3.—A paper on "The Gravel and Alluvia of S. Wales and Siluria as distinguished from a northern drift covering Lancashire, Cheshire, N. Salop, and parts of Worcester and Gloucester," by R. I. Murchison, Esq., V.P.G.S., was read.

The first part of this memoir describes the detritus in the Welsh and Silurian territories. The surface of this region is completely exempt from the debris of any of those far-transported rocks which constitute what has been called "diluvium" in other parts of England; all the loose materials in S. Salop, Herefordshire, and the adjoining Welsh counties having been derived from the Silurian and trap rocks of the adjacent mountains. These mountains range from N.E. to S.W., presenting inclined planes to the S.E., on the surfaces of which the broken materials are distributed. Four of the rivers which descend from the higher parts of Wales flow to the S.E. in accordance with the prevailing lines of drift, traversing the ridges of Silurian rocks through fissures which have resulted from dislocations of the strata. These are the Teme, the Onny, the Lug, and the Wye, all tributaries of the Severn. That great river, on the contrary, does not follow the "line of drift" to the S.E., but escapes from the mountains to the north by a lateral gorge under the Breidden Hills; and after a circuit in the Vale of Shrewsbury passes eastward through a narrow transverse rent in the upper Silurian rocks and coal measures of Coalbrook Dale; and taking its final course southward, from Bridgnorth to the Bristol Channel, forms the eastern limit of the country covered by the Welsh or Silurian detritus. The drainage of the Teme, Onny, Lug, and Wye, is described in detail, with a view of showing, that in the valleys in which these rivers descend from the mountains, the materials change with each successive ridge, the larger fragments being transported only short distances; and that as the gravel advances into the plains, it becomes more finely comminuted; Herefordshire and the low countries being chiefly covered with local debris of the old red sandstone. The author specially distinguishes this drift, which is extensively spread over valleys and slopes, and sometimes found in high situations, from the detritus which has been carried down by rivers under the atmosphere, conceiving that the former accumulations have been washed down the surfaces of the inclined strata; because wherever the latter dip to the S.E., so are the materials invariably found to have been propelled in that direction. In no instance has any fragment been found on the west which can have been derived from rocks on the east. He therefore believes that at those periods when the Silurian and older rocks were raised from beneath the waters, great quantities of coarse and fine detritus were drifted down these slopes; and that as the rocks on which the loose materials have been deposited are replete with dislocations, and penetrated at many points by ridges of trap rock, it is to be inferred, that during and after the evolution of this volcanic matter, great and successive elevations of the bottom of the sea took place, throwing up the drifts to the various heights at which we now find them. As soon as the land was raised from beneath the sea, the present rivers, it is conceived, began to flow; passing through the ridges by gorges produced by great lateral cracks the result of elevation; and that these streams have since merely transported to short distances those broken materials which were previously gathered together by subaqueous drift. To prove that the drifted matter of each district within this region may be referred to *disturbances purely local*, it is shown that although wherever the hills have been elevated from N.E. to S.W. the lines of drift are from N.W. to S.E., yet in those contiguous tracts which have been elevated in other directions the course of the drift *changes* immediately with the variation of the *strike*. Thus on the exterior margin of the great coal-field of S. Wales vast quantities of materials resulting from the breaking up of the carboniferous series have been dispersed to *the N.E., N.*, and *N. West*, directions *excentric* from the broken margin of that elevated tract.

In Pembrokeshire, again, where the prevalent lines of strike are from E. to W., the drift has been carried southwards. Conceiving that the great masses of these drifts have been formed at various periods *under the sea*, either in gulfs, estuaries, or straits, and have been raised up at different periods when the solid strata were elevated, the author then proceeds to consider the probable conditions of the surface of this portion of the country for some time after such emersion, and yet at a period comparatively remote. He instances many flat embayed tracts which, from the equable surface of the sand and gravel, are supposed to have been for some time under water, occupying lakes which have been drained by the deepening of gorges issuing from these bays; since it is shown that a very slight difference of level in the beds of rivers at several gorges would effectually bar up the present streams, and pond them back into lacustrine expanses. Hence he infers that slight additional movements of the land, aided by the excavating process of the rivers themselves, may have operated in draining these flat tracts. A large part of Herefordshire watered by the Wye is supposed to have been under such waters, which have since escaped by the picturesque gorges of Ross and Chepstow. The Vale of Radnor is a similar case; now drained only by a feeble rivulet. But the clearest examples of successive lacustrine expanses are exhibited in the descent of the Teme; first in the tract still called "Wigmore Lake;" from whence the superabundant waters have escaped through the upper Silurian rocks in the gorge of Downton on the rock; and next in various expansions and contractions between Ludlow and the Abberley Hills, where they have been again barred up by that ridge until the gorge at Knightwick Bridge was deepened, opening out a channel for their escape into the great Valley of the Severn. The finely levigated sand, marl, and mud, at small heights above the present stream point to this anterior lacustrine condition.

The period of the final desiccation of these river-lakes, and the reduction of the rivers to their present channel, is supposed to have been contemporaneous with that recent elevation which in raising the land to greater heights brought up large adjacent portions of the bottom of the sea, and to the consideration of which the second part of the memoir is devoted under the head of "*northern drift.*"

In the region of Welsh and local drift attention is specially called to the length of time during which existing causes have been in undisturbed action, as proved by the magnificent mass of Travertino formed and still forming at the Southstone rock; whilst he also points to the discovery of shell marl in a bog near Montgomery, containing several species of *Lymnæa*, which has evidently been formed in the manner described by Mr. Lyell in his memoir on the marl loch of Forfarshire.

Northern Drift.—Detritus differing entirely from that which covers Wales and Siluria, is spread over large parts of Lancashire, Cheshire, and N. Shropshire, ranging up to the edges of the region above mentioned. The materials of this drift consist of granites, porphyries, and other hard rocks, which have been derived from the mountains of Cumberland, a few perhaps from those of Scotland. The drift further contains much sand and clay, with many pebbles of smaller size, which varies exceedingly in different districts. Thus, in N. Salop, near the great outlier of lias, described by the author*, fragments of that formation are added to the mass, and as it advances to the south the materials become still more varied; the fragments, however, of the northern granite and porphyries always existing to identify the drift. Its distinguishing feature is the reoccurrence at intervals of large blocks or boulders, of northern origin, a large proportion of which lie at various heights on the slopes of the mountains skirting the N. Welsh coal-field, and encumbering the northern flanks of the Wrekin and of Haughmond Hill; while a few have been propelled to the edge of the Silurian rocks south of Shrewsbury. They prevail in vast quantities in the high inland district between Wolverhampton and Bridgnorth, from which latitude they begin to diminish in size; but coarse gravel, composed of the same materials, is prolonged southwards like the tail of a delta through Worcestershire, until it dies away in the fine silt and gravel of the Vale of Gloucester. *Not a fragment of any such detritus enters into the region of Welsh and Silurian drift*; but in the environs of Shrewsbury certain mounds of the latter are capped by clay and boulders of the northern drift, which is thereby shown to be of subsequent formation. The best proof of the recency of the epoch during which this northern drift was accumulated is, that it contains sea shells of *existing species*. These were formerly noticed at Preston, in Lancashire, by Mr. Gilbertson; and by the author at the height of 350 feet above the sea. In Cheshire they have been observed by Sir P. Egerton at heights of about 70 feet†. Mr. Trimmer has cited similar shells on Moel Tryfane‡, now ascertained to be 1392 feet above the sea, and has recently detected them near

* Proceedings of the Geological Society, Vol. II. p. 114.

† *Ibid.*, Vol. II. p. 189. ‡ *Ibid.*, Vol. I. p. 331.

Shrewsbury. Mr. Murchison has collected evidence of their diffusion over a wide area in Shropshire, tracing them at intervals from Marington Green, N.W. of Shrewsbury, by the Wrekin and Wellington, to the high grounds between Bridgnorth and Wolverhampton, at least 60 *miles inland*, and at heights varying from 300 to 600 feet. He has also been enabled to add several species to those mentioned in any former list. These shells having been examined by good conchologists (including Dr. Beck, of Copenhagen,) prove to be identical with species now inhabiting adjacent seas, viz. *Buccinum reticulatum*, *B. undatum*, *Dentalium entalis* (Linn.), *Littorina littorea*, *Tellina soldula?* *Venus* ——, *Astarte* ——, *Cardium tuberculatum*, *C. edule*, *Cyprina islandica*, *Turritella ungulina* (Beck), (*Turbo ungulinus*, Linn.,) *Donax* or *Mactra*.

It was a prevalent belief that large boulders were usually lodged upon the surface of the gravel and sand; but cuts which have been made through mounds of these materials at Norton, near Shrewsbury, have proved that the larger blocks occur at considerable depths below the surface mixed up with shells, sand, gravel, and clay. This is the locality described by Mr. Trimmer* as indicating the existence of dry land anterior to the deposit of the shells and gravel, by the occurrence of a peat bog, which he supposed to have been formed out of the remains of a submerged forest; the stumps of the trees of which were said to be still rooted in their parent soil, and standing in their growing posture. Having examined the spot (accompanied by Dr. Du Gard), Mr. Murchison has obtained clear proofs that the supposed trees were stakes with sharpened points which had been driven down into a patch of subjacent clay; the other remains consisting of a plank and smaller stakes which had been laid horizontally. This woodwork formed the support of the old road, which in making the new one had been cut down beneath the ancient foundations. The patch of clay into which the piles were driven, lying in a depression between two hillocks of gravel, must have given rise to a wet and boggy spot, which having been rendered passable by piling and damming, the dry materials of the contiguous hillocks were doubtless shovelled in to complete the road, thus giving rise to the deceptive appearances of marine drift overlying the supposed forest.

Though the collocation of the boulders, sand, gravel, loam, clay, and shells is in parts very irregular, yet the materials are sometimes finely laminated: the whole, it is presumed, may have been thus brought together at the bottom of a sea, as the mass is not unlike many raised sea-beaches, with one of which, at the mouth of Carlingford Bay, Ireland, recently visited by Professor Sedgwick and himself, the author compares it.

From the evidences afforded by these recent shells it is inferred, that the tracts covered by them must have lain under the sea during the modern period; whilst from the continuation of the granitic drift from the high grounds east of Bridgnorth into the Vale of Worcester, Mr. Murchison conceives that the sea must at the same time have covered the Valley of the Severn from Bridgnorth to the Bristol

* Proceedings of the Geological Society, Vol. II. p. 200.

Channel, thus separating Wales and Siluria on one side, from England on the other. Having shown that the Welsh and Silurian mountains were partly raised at an earlier period, he points out the Abberley and Malvern Hills, as constituting the western side of a strait of the sea, the eastern shore of which was the Cotteswold Hills. He deduces the principal proof of the preexistence of this eastern coast from the observations of Mr. H. Strickland, which show the transport from the east and north-east of fluviatile and land shells mixed with the remains of extinct quadrupeds in banks of coarse gravel, following the drainage of the Avon near to where that river empties itself into the Severn; and he asserts, that the terrace-like deposits of Cropthorne are exactly those which would have been accumulated at the mouth of a river, if the materials had been carried onwards beneath the waters of the adjoining strait of the sea, illustrating his views by the analogies of other rivers and estuaries. He therefore presumes that the deposit of Cropthorne may have been coeval with that of the northern drift. After an explanation of the theories hitherto proposed to account for the transport of large boulders to distant points, the author states that the evidences in question seem to him to be subversive of the diluvial hypothesis which imagines that the blocks were carried over the land, it being proved that here, at least, they were accumulated *under* the sea. He does not think we have yet been furnished with a full explanation of any method by which such blocks can have been transported to distances of 100 miles: for supposing them to have been derived from the shores of Cumberland, and that they extended in a delta from thence, it would appear that assuming the slightest degree of inclination, viz. 3°,—which could give adequate momentum to the ordinary power of running water acting upon these loose materials,—the southern part of the delta (even at a distance of 50 miles from Cumberland,) must, as suggested by Mr. Lonsdale, have lain at the vast depth of 13,000 *feet* beneath the sea, in which case all Wales would have been equally submerged; though we have proof that the mountains of that country *had* risen to a certain height previous to the accumulation of the northern drift. It is further submitted that under the physical features of the region when this drift was formed, i. e. when a great arm and strait of the sea separated England from Wales, submarine currents alone could not have been powerful enough to propel these large blocks, though the question is one which ought to be more completely disposed of by those versed in the laws of dynamics. Mr. Murchison next takes into consideration the theory of the transport by ice. After allusion to the views of Esmarck, De l'Arrivière, Haussman, &c., it is shown that Mr. Lyell has thrown great additional light on this subject by his observations on Sweden and the Alps, by which it really appears that under certain limitations "ice floes" may have been "veræ causæ" in the transport of large blocks, depositing them under seas and lakes at great distances from the source of their origin. In the Salopian case, however, *though it is possible* such means may also have been employed, there are many arguments which weaken the application of the hypothesis, such as the *rounded* and *worn* exterior of the boulders, and their diminution in size and quantity from north to south. It might also be contended that we have no right to infer the existence of a colder climate in our latitudes in those days; but this objection does not appear unanswerable, since it might be replied, that if at the period of the *northern drift* England, Ireland, and the continent of Europe were united by a lofty chain of mountains, there might have been a temperature sufficient to have formed annually large bodies of ice on the shores of Cumberland. Passing however from this difficult question of the method of transport, Mr. Murchison states that the greatest of the anomalies hitherto presented by these boulders is obviated, when we dispel from our minds the idea of their having been carried over preexisting lands. Having once ascertained that large distributions of them took place *under the sea*, the different heights at which we now find them may, he supposes, be satisfactorily accounted for, by *movements of elevation and depression acting upon the bed of the sea with unequal measures of intensity, raising up shells, gravel, and boulders which were accumulated at the same period, to the respective levels which they now occupy*, doubtless producing many of the cracks and fissures with which the solid strata are replete, and leaving denuded valleys between the points so elevated.

Feb. 24.*—A paper was first read, entitled "Observations on a Patch of red and variegated Marls, containing Fossil Shells, at Collyhurst, near Manchester," by J. Leigh, Esq., and E. W. Binney, Esq., and communicated by Roderick Impey Murchison, Esq., F.G.S.

Manchester stands on a slightly elevated platform of upper new red sandstone; but the country to the north-west, north, and east of the town rises to a considerable height, and is traversed by the valleys of the Irwell, the Irk, and the Medlock, which furnish the only natural sections of the district. The formations exhibited in these valleys, and supposed to extend under Manchester, are, first and lowest, the carboniferous group; secondly, the lower red sandstone and marls; thirdly, the magnesian limestone; fourthly, the lower red marl; fifthly, the upper red sandstone; sixthly, the upper red marl; and seventhly, the superficial detritus.

The principal object of the authors being to describe the upper red marl, they notice briefly the characters of the other deposits.

The accumulations of superficial detritus are sometimes thirty feet thick, capping nearly all the high ground, and extending over the valleys. In the lower part they consist of water-worn fragments of granite, greenstone, porphyry, claystone, mountain limestone, and coal measures, imbedded in sand; and in the upper, of stiff blue clay, containing partially rounded fragments of the same rocks but of greater size. Portions of the lower red sandstone and marl are sometimes found, but none of the magnesian limestone. Blocks of granite, weighing two or three tons, occur on the summit of some of the hills which surround the Irwell and the Irk.

The lower red sandstone, the magnesian limestone, and lower red marl are exposed at Worsley Mills and at Stockport, dipping conform-

* The Anniversary Proceedings of Feb. 19, will be found at p. 310, in our Number for April.

ably with the coal measures; but at the latter locality they are stated to be overlaid unconformably by the upper new red sandstone.

The immediate vicinity of Manchester consists of upper new red sandstone, occupying the cavity formed by a flexure in the underlying deposits, and is generally supposed to be unconformable to them. It is very soft when first exposed, but hardens by exposure to the atmosphere; and is occasionally marked by belts and nodules of white sand, and in the lower part contains rounded fragments of granite, quartz, and other older rocks. No organic remains have been noticed in it.

The upper red marl is exposed only at Collyhurst, about a mile to the north-east of the Manchester Exchange, in the old road to Blakeley; but it has there yielded a greater number of fossils than has been found in any other bed of the superior divisions of the new red sandstone group in England.

The deposit extends about a hundred yards, and at one of the points examined presented the following details:

Top	*a.* Variegated marls, no organic remains	6 inches.
	b. Strong, red marl, traversed near its centre by a thin layer of fragile bivalve shells ..	5 ——
	c. Light-coloured, calcareous marl, marked with lines and spots of a beautiful red ..	3 ——
	d. Light-coloured, calcareous, strong marl, containing an immense number of imperfect casts of bivalves and perfect univalves	5 ——
	When the marl is first excavated it crumbles under the touch, but after exposure for a short time, it is fractured with difficulty.	
	e. Clay, striped red and white, and containing casts of bivalves	4 ——
	f. Light-coloured marl, similar to No. 4, and inclosing numerous casts of bivalves and univalves	3 ——
	g. Variegated marl, with an immense number of univalves and bivalves, 2 inches	2 ——
	h. Indurated red marl, mottled with streaks of a greenish colour. The upper part contains numerous casts of large bivalves, and the light-coloured streaks also inclose casts of bivalves and univalves. Few shells are found below the depth of one foot, though the author had the bed penetrated to the depth of 29 feet, when an influx of water prevented them from boring any further. The rhomboidal fracture, so characteristic of the red marl, was very observable in this bed	29 feet.

With respect to the geological position of these fossiliferous marls, the authors are fully satisfied that the deposit reposes on the upper new red sandstone by which the marls are surrounded, though, from the covered nature of the ground, the connexion of the two formations cannot be ascertained. In mineral aspect the lowest beds at Collyhurst are said to agree with the upper red marl of Lincolnshire and Cheshire, and to be distinguished from it only by the presence of fossils and the absence of salt; while the Collyhurst strata differ from the lower red marl, in colour, fracture, and the organic remains.

In accounting for the presence of these fossiliferous marls in the situation described, and their absence from the top of the new red sandstone in the immediate neighbourhood, the authors suppose that the marls were deposited in a hollow of the new red sandstone, and to have been, therefore, protected from denudation.

A notice, by Francis Offley Martin, Esq., inclosing communications from Col. Brown and Lieut. Laurence, of the Rifle Brigade, and Mr. Stevens, on the streams of sea water which flow into the land in the island of Cephalonia, was next read.

These communications were procured by Mr. Martin at the request of Mr. Lyell.

Lieut. Laurence's letter is dated 31st of May, 1835, and contains an extract from an account sent to him by Mr. Stevens of the nature, excavation, and the operation of the stream. The length of the channel made for conducting the water was 20 yards and its width 3 feet; and at the end of the channel a pit was made nearly 100 square yards in extent, and to the depth of about 4 feet below the level of the sea. On opening the sluice a stream of 150 square inches rushes into the pit with a velocity of 20 feet a second, and down a channel in the form of a segment of $\frac{1}{4}$th of a circle of 18 feet diameter. A constant discharge of this stream raises the water in the pit to within 2 feet of the top of the arched channel. The stream escapes through the fissures in the pit, but the direction which it afterwards takes has not been well ascertained, though shafts have been sunk for that purpose. In these shafts water of the same description with that in the pit is found, rising and falling in the same manner. Mr. Laurence also states that when the sluice-gate is shut down after a very considerable discharge of sea water into the pit, the water in the pit falls a few inches lower than it was previously to the discharge; but is afterwards raised to the usual level by the freshwater springs.

Mr. Stevens's letter, dated the 28th August, 1835, gave an account of the making of the excavation, and states that the experience of a year and a half had proved that the stream is not liable to any periodical change.

Col. Brown's communication bears date the 27th of August, 1835, and gives an account of the physical features of the island, the nature of the excavation, and the probable manner by which the subterranean current is disposed of.

On the eastern side of the harbour of Argostoli the country rises abruptly from the shore to a considerable elevation, and then more gradually until it is lost in one of the great ridges which intersect the island; but on the western side the narrow peninsular ridge at the foot of which Argostoli is built, nowhere exceeds 400 feet in height, sloping gradually towards the sea, and is surrounded by comparatively shallow water. The whole of the ridge consists apparently of coarse limestone, presenting on the surface large, detached blocks. Col. Brown's account of the excavation agrees with those already given. He notices the springs of fresh water, and the fact, that when the sluice is first shut the pool is drained to a much lower level than that at which it afterwards stands, and this phænomenon he conceives may be explained on the principle of natural siphons.

He says that there are three other openings on opposite sides of the promontory, through which sea-water flows into the land, and he is of opinion that there may be many more.

With respect to the question what becomes of the water, Col. Brown has always believed that the streams are conducted to subterranean fires, and that the earthquakes so common in the island are caused by the expansion of the gases generated by the action of those fires on the sea water.

A notice accompanying rock specimens from the caves of Ballybunian, on the coast of Kerry, by Lieut. Col. W. H. Sykes, F.G.S., was then read.

The author states that his principal object in bringing this communication before the Society is to induce geologists to examine a part of Ireland seldom visited, but which he conceives to be highly deserving of attention.

The coast of Kerry, in the neighbourhood of Ballybunion, presents a series of cliffs varying from 100 to 150 feet in height, and is indented by numerous bays. The stratification consists of several feet of debris, composed of angular fragments of silicious rocks and earth; a bed of alum shale follows, breaking into rhombs; then a stratum of lignite or carbonaceous schist, and another of iron shale. These strata are occasionally repeated, and said, on the authority of Mr. Ainsworth, to rest on limestone.

A principal feature in these beds is a disposition to separate into rhombs; and the cliffs in several places present the solid angles projecting beyond the vertical line of the cliffs, while the roof of some of the caves is groined like the intersection of Gothic arches. The general inclination of the strata is about 13° to the east, but it is frequently altered by faults, and sometimes presents anticlinal dips.

Of the exact age of the beds the author offers no opinion, but he thinks that it is not posterior to the carboniferous series.

Among the specimens which accompanied the memoir were some from the west end of the Isle of Innisfallen, in the Lake of Killarney. The strata at that point consist of narrow vertical and alternating ridges of a silicious rock and limestone: the former projecting beyond the surface of the latter.

A paper was last read, entitled, "An Account of some fossil vegetable Remains found in the sandstone which underlies the lowest bed of the carboniferous Limestone, near Ballisadiere, in the County of Sligo, Ireland," by Sir Alexander Crichton, M.D., F.G.S., &c.

In the county of Sligo there are no coal deposits, the nearest being the Arigna coal-field, in the county of Leitrim. The bed of sandstone containing the plants is well exhibited, resting upon gneiss, with which it is stated to dip conformably, and is covered by the mountain limestone. The state of the plants prevented the author from ascertaining their generic characters, but the specimens consist principally of flattened stems covered occasionally with a thin coating of carbonaceous matter. The lowest beds of limestone in this part of Ireland abound with corals, and contain nodules of chert; while the upper contain many shells, and are purer and better adapted for forming quicklime. The author then remarks on the great interval which must have taken place between the growth of the plants contained in the sandstone, which underlies the limestone, and of those which occur in the coal-measures resting upon it.

March 9.—A paper was read, "On the Remains of Mammalia found in the Sewalik Mountains, at the southern foot of the Himalayas between the Sutluj and the Ganges," by Capt. Cautley, F.G.S., and communicated by J. F. Royle, Esq., F.G.S.

The range of mountains from which the remains described in this paper were obtained, extends from the Sutlej to the Burhampooter and the district of Cooch Behar. Its general direction near the Sutluj is N.W. and S.E., but on approaching the Burhampooter it is many points nearer direct E. and W. It is either connected with the Himalayas by a succession of low mountains, or is separated from them by valleys varying in breadth from three to ten miles, the principal being the Deyra valley, between the Ganges and the Jumna, and the Kearda and the Pinjore, between the Jumna and the Sutluj. The breadth of the range is from six to eight miles; and the loftiest peaks do not exceed 3000 feet, the average height being from 2000 to 2500 above the level of the sea, or from 500 to 1000 above that of the adjacent plains. The only roads by which the range can be passed follow the line of the rivers which flow through gorges flanked by precipitous cliffs, sometimes crowned by inaccessible pinnacles, on the top of which is usually a solitary fir-tree. As the range is not known to the present inhabitants or to geographers by a distinct name, Capt. Cautley has been induced to call it the Sewalik, a term by which the portion between the Jumna and the Ganges was formerly known*; and he states that he is anxious to give to it a distinct appellation to avoid the use of the indefinite terms Lower Hills and Sub-Himalayas.

The formation of which the range is composed between the Sutluj and the Ganges, the portion personally examined by the author, consists of alternating beds of conglomerate, sandstone, marl, and clay, inclined at angles varying from 15° to 35°. The succession of the beds is irregular, the marl prevailing to the west and the conglomerate to the east of the Jumna.

* Smith's Exotic Botany, vol. i. p. 9. Dow's History of India. The name is also used in some writings in the possession of the high priest residing at Deyra. The word is a corruption of *Shibwalla*, from the district between the Ganges and the Jumna having been the residence of Shib.

The beds of conglomerate, or in the language of the author, of shingle, are of enormous thickness, and are composed of pebbles of granite, gneiss, mica-slate, hornblende-slate, and trap, derived apparently from the Himalayas, and are either loosely aggregated or cemented by clay and carbonate of lime.

The sandstone consists of grains of quartz and scales of mica, cemented by oxide of iron or carbonate of lime. The colour presents various shades of red and grey; and the state of induration differs in proportion to the quantity of the cementing matter, which sometimes gives the stone a crystalline appearance. It is occasionally used as a building material and in some instances has resisted for a long time the action of the atmosphere. Carbonaceous matter is of common occurrence in the sandstone, either in fragments exhibiting the structure of dicotyledonous plants, or as grains disseminated throughout the stone in nearly equal proportions with the sand. Carbonaceous matter exists also in the marl, and in one instance Capt. Cautley noticed it in the conglomerate. It has never yet been found in sufficient quantity to be of œconomical importance. At the Kalowala Pass, one of the entrances into the Deyra valley, the author discovered in a bed of yellow and red sand elliptical masses of sandstone coated by a thin layer of carbonaceous matter.

The marl or clay conglomerate is described as consisting of fragments of indurated clay cemented by clay, sand, and carbonate of lime. It is exceedingly tough, and is less easily acted upon by running water than the other strata.

The only point at which trap has been observed is in the neighbourhood of Nahun, where it has been noticed by Dr. Falconer.

Soda effloresces on the surface of the shingle and sandstone, and selenite occurs occasionally in the clay.

The distribution of the organic remains in the district between the Jumna and the Ganges, Capt. Cautley states to be as follows, the greater part of the fossils having been obtained at the Kalowala Pass.

Conglomerate or Shingle Beds.—Lignite, scarce.

Sandstone.—Trunks of dicotyledonous trees in great abundance, lignite, and remains of reptiles.

Marl.—*Pachydermata*: teeth, and remains of a species of Anthracotherium.

Carnivora: genera doubtful, but some of the teeth correspond with those of the Bear.

Rodentia: Rat, and a small variety of Castor.

Ruminantia: Deer, more species than one.

Solipede, teeth of a Horse.

Gavial and *Crocodile*, teeth and bones in abundance.

Emys and *Trionyx*, fragments of.

Pisces, vertebræ and perhaps scales.

Shells, freshwater genera.

The district between the Jumna and the Sutluj consists of the same series of shingle or conglomerate, sand, clay, and marl; but the shingle is less abundant, and differs in being composed of pebbles of various kinds of clayslate and quartz, and the marl is exposed only at Nahun, where it contains the same organic remains as in the Kalowala Pass. From Nahun to the plains there is a succession of sandstones and clays, dipping on an average about 20° to the north. In the neighbourhood of that town the sandstone is hard and used for building; but it becomes soft on approaching the plains. The clays are stated to be more or less rich in Testacea, and the sandstone in remains of Mammalia.

The large collections of bones obtained by Capt. Cautley were found partly lying on the slopes among the ruins of fallen cliffs, and partly in situ in the sandstone; and he is of opinion that the former have been, in a great measure, preserved by the sandstones in immediate contact with the bones, being much harder and more ferruginous than in the general mass.

The following is a list of the remains which had been determined at the time the memoir was written.

Mastodon, elephant, rhinoceros, hippopotamus, hog, horse, ox, elk, deer, several varieties; carnivora, canine and feline; crocodile, gavial, emys, trionyx, and fishes; and portions of undescribed mammalia.

The remains of all these animals are in great abundance, with the exception of the Horse and Carnivora; but the bones of the head are better preserved than those of the trunk or the extremities. Sometimes the fractured bones have admitted of being joined, though the surfaces were coated with calcareous spar.

In assigning an age to the formation composing the Sewalik mountains, Capt. Cautley adopts the views of his friend Dr. Falconer, who in a notice read before the Asiatic Society of Calcutta, considered the deposit to be synchronous with that from which Mr. Crawfurd obtained the remains near Prome, on the banks of the Irawadi, there being an agreement in the organic remains. The author then offers some remarks on the *Mastodon elephantoides* and *M. latidens*, and in consequence of his having found jaws in which the front teeth are not to be distinguished from the teeth of *M. latidens*, and the rear from the teeth of *M. elephantoides*, he conceives that the distinction established on detached teeth will be found to be erroneous.

March 23.—A paper was first read, entitled, " A Description of various Fossil Remains of three distinct Saurian animals discovered in the autumn of 1834, in the Magnesian Conglomerate on Durdham Down, near Bristol." By Henry Riley, M.D. and Mr. Samuel Stutchbury; and communicated by Charles Lyell, Esq., P.G.S.

The conglomerate in which these Saurian remains were discovered rests upon the edge of inclined strata of mountain limestone, filling up the irregularities of their surface, and consists of angular fragments of the limestone cemented by a dolomitic paste. The thickness of the deposit at the point where the remains were discovered does not exceed twenty feet.

Of the three animals described in the paper, two belong to a genus for which the author proposes the name of Palæosaurus, and the third to one which they have called Thecodontosaurus.

The characters of the genus Palæosaurus are derived from the teeth, which are described as being carinated laterally, and finely serrated at right angles to the axis. They are stated to differ from those of all the Saurians known to the authors: and as the teeth in their possession exhibit minor marked characters, they are induced to consider that they belonged to two species, which they have named *P. cylindricum* and *P. Platyodon.*

The genus Thecodontosaurus is likewise founded on the structure of the teeth, and their having been deposited in distinct alveoli. Among other remains in the Museum of the Bristol Institution is the right ramus of a lower jaw, $3\frac{1}{4}$ inches long and $1\frac{1}{4}$ in the greatest depth, from the summits of the teeth to the under rise, consisting of the dental bone, containing 21 teeth, with portions of the subangular and complementary bones, and perhaps traces of the opercula. The alveolar groove for the reception of the teeth is formed by two ridges of nearly equal height, the teeth being deposited in it, in distinct alveoli, to nearly half their length. The teeth somewhat resemble in shape a surgeon's abscess-lancet, being acutely pointed and flattened; while the anterior edge is also curved, but concave and strongly serrated, the serrature being directed towards the apex of the tooth. The middle teeth are the largest, rising not less than a quarter of an inch above the socket. They all possess a conical hollow, and in a specimen belonging to the Rev. D. Williams a young tooth is well exhibited in one of the alveolar cavities. From these characters the authors infer that the jaw belonged to a Saurian, but not to the great genus Lacerta of Linnæus as reformed by Cuvier by rejecting the Crocodiles and Salamanders. They further infer from the shape and serrated edge of the teeth that it did not belong to the Crocodiles; nor to the Lizards, whose alveolar inner edge is either wanting or much less elevated than the outer. They also show that it was not allied to the Monitors, because of the elevated inner alveolar edge, the distinct alveoli, the teeth remaining hollow and the formation of the new tooth in the same cell with the old one, as well as from the great number of the teeth. With respect to the Iguanas and Scinks they show that the fossil could not have belonged to them, in consequence of the distinct alveoli, the inner alveolar edge, and the form of the summit and serratures of the teeth: and that it differed from the Saurodon in having a ridge on the outside of the tooth with the edge crenated and of unequal length.

Numerous other bones have been discovered, but as none of them were found in connexion with teeth, the authors hesitate to assign them to either of the genera which they have established. Among these remains the following are described:

Vertebræ possessing the peculiar characters, of having the centre of the body diminished one half in its transverse and vertical diameters so as to resemble an hour-glass; of a suture connecting the annular part or body with the processes; and in the extremities of the vertebræ being deeply concave. These characters the author conceives distinguish the fossil vertebræ from those of all recent Saurians.

A nearly perfect chevron bone; ribs, one flat and imperfect, the other round with a double head and a deep intercostal groove; a clavicle; portions of coracoids; a humerus, the articulatory extremities of which expand to nearly three times the diameter of the centre of the bone; a humerus 7 inches long, 2 inches broad at the superior extremity, and 1¼ at its inferior; two femurs, one nearly perfect, being 10 inches in length; part of an ischium; a tibia; a fibula; metacarpal or metatarsal bones, with penultimate and ungueal phalanges.

In conclusion the authors state that these remains afford further proof of the truth that the more ancient the strata the more the animal remains differ from existing types.

A memoir was afterwards read, "On the Ossiferous Cavern of Yealm Bridge, 6 miles south-east from Plymouth." By Capt. Mudge, Royal Engineers, F.G.S., F.R.S., &c.

This cavern is situated in a mass of limestone adjoining the village of Yealmpton, near Yealm Bridge, and on the south side of the river. It has been long known, and though large quantities of the bones have been burnt in the limekiln, yet it was not till lately that its contents attracted the attention of the scientific observer. Mr. Bellamy, of Yealmpton, first detected their value, and Capt. Mudge in a visit to Devonshire in the autumn of last year collected the information detailed in the memoir. "There were originally three openings into the cave, each about 12 feet above the river Yealm, and a few yards distant from each other. Large portions of the rock being removed for economical purposes, a considerable part of the cavern has been destroyed, and at the time of Capt. Mudge's visit portions of only the eastern and western chambers remained. The former consisted of a descending shaft to the depth of 10 feet, which turned at right angles and again ascended to the surface, both the descent and the ascent being at an angle of 45°. Of the western cavern, a portion remained uninjured. From the present opening it takes a northerly direction for 43 feet, the height varying from 5 to 6 feet, and the breadth from 4 to 5. It then turns westerly for 25 feet, the height varying from 5 to 12 feet, and the breadth from 3½ to 5. The cave contained five, distinct, sedimentary deposits, and where they did not fill it to the roof the uppermost bed was covered by a layer of stalagmite. The order of the deposits was as follows:

Top.	Loam, containing bones and stones	3½ feet.
	Stiff whitish clay	2½
	Sand	6
	Red clay................................	3½
	Argillaceous sand	6 to 18.

Animal remains have been found only in the uppermost bed, and the author, on the authority of Mr. Clift and Mr. Owen, states that they belong to the elephant, rhinoceros, horse, ox, sheep, hyæna, dog, wolf, fox, bear, hare, water-rat, and a bird of considerable size. Coprolites also occur in the same bed. Many of the bones are splintered, chipped, and gnawed. Of the elephant only two teeth of a young animal have been preserved, and the remains of the rhinoceros are also rare, being confined to teeth and a doubtful bone, but those of the hyæna, particularly teeth, exceed in quantity all the bones of the other animals. Teeth and bones of the horse and ox are very abundant, but the remains of the bear are confined to teeth. It is however stated by the author that it is impossible now to determine what proportions the animals originally bore to each other. The pebbles found in the same bed with the bones are apparently derived from the confines of Dartmoor, and differ from those contained in the bed of the Yealm. In one part, where the roof is a little lower than usual, the limestone is beautifully polished, as if by the friction of the animals which inhabited the cave.

There are many other caverns in the neighbourhood, but the one next in importance to that at Yealm Bridge is in the grounds at Ketley. The floor of this cavern rises but little above the present level of the river, and consists of gravel and pebbles corresponding with those in the bed of the Yealm. It has been ascertained that it does not contain bones, and Capt. Mudge therefore concludes that the caverns of Yealm Bridge and Ketley were exposed to very different conditions when the elephant and the hyæna inhabited the southern part of Devonshire. As far as regards space the accommodation in Ketley Cavern was much superior to that of Yealm Bridge Cave, and consequently it may be inferred that at the time when the hyænas inhabited the latter, they were prevented from entering the former either from its having been frequently flooded or permanently under water.

LINNÆAN SOCIETY.

May 24, 1836.—At the Anniversary Meeting held this day, His Grace the Duke of Somerset in the chair, previous to the usual business the Secretary, Dr. Boott, stated that the Society had lost by death during the past year the following 11 Fellows and 2 Associates, viz.

Fellows.—G. T. Burnett, Prof. of Botany in King's College; A. Crouch, Esq.; George Harry, Baron Grey of Groby; D. Hosack, Esq., M.D., F.R.S.; E. Jennings, Esq.; J. Macculloch, Esq., M.D., F.R.S.; Mr. Wm. Malcolm; H. Phillips, Esq.; H. Sim, Esq.; W. Smith, Esq., F.R.S.; Rev. G. A. Thursby, M.A., F.R.S.

Associates.—Mr. T. Drummond; Rev. J. T. Thomson.

The Secretary then particularized, in the following terms, some of the deceased members:

"Dr. Hosack, without attaining to or claiming any eminence as a botanist, was one of the earlier promoters of that science in America, and formed a botanic garden in the neighbourhood of New York, which was eventually purchased by the State or by one of the colleges of that city; and he undoubtedly would have anticipated many of the discoveries of later observers, had not his attention been necessarily drawn to medicine, from the distinguished reputation which he acquired as a physician. He visited Europe at an early period of life, and made the acquaintance of the more distinguished men of science in this country and in France; and the grateful recollection he ever cherished of the reception he met with in Europe prompted that liberal hospitality to strangers for which he was always honourably distinguished. He was a generous patron of science and art; and one of those who stood prominently forward in giving that grateful reception to the distinguished leaders in one of the Arctic expeditions. It may be remembered that Sir J. Franklin and Dr. Richardson, and, I believe, Capt. Back, were hailed on their arrival at New York by the mayor and principal citizens of the city, and that they were conveyed to the confines of Canada free of expense, cheered by the sympathies of all around them,—one of those evidences which the heart affords of the cordiality and respect existing between America and England, whatever some writers may say of the contrary. Dr. Hosack published the life of his friend Gen. Clinton, and was the author of various papers on the medical and other branches of science.

"I feel that this is not the place for doing full justice to the memory of Mr. Smith, for I am not aware that he particularly directed his attention to any branch of natural history; but as the early, strenuous, and constant advocate of civil and religious liberty he is entitled to the respect and admiration of all those who believe in the capacities of human nature for a progressive advancement in intellectual and moral power. It is well known that Mr. Smith was returned M.P. for Sudbury and Norwich for nearly half a century, and that towards the close of the last century, and up to a comparatively late period of his life, he was one of what originally was comparatively a small band, who, when liberal sentiments were obnoxious to a degree which I believe it to be impossible for us at the present time to conceive of, pleaded for the rights of humanity, when those rights were denied not only to the enslaved African, but to a large portion of the people of this country, especially to the conscientious Dissenter, who was at one time looked upon as hardly a loyal subject of the realm. To those who, in watching the changes which human institutions undergo in the progress of time, are convinced that a chastened and enlightened spirit of liberty, as it has become more and more developed, has in all times given additional dignity to human nature, by enabling it to put forth those inherent energies, all-instructive of good, which, like the branches of the oak, acquire a hardy vigour and nobler growth as they are free to extend themselves in the liberal light and air of heaven,—to such an observer of the eventful period of the last half-century the name of Mr. Smith must often recur as one of the most unquestionable benefactors of mankind. If not distinguished by any of the splendour of genius, or as standing prominently forward in the first ranks of public life, he was more honourably distinguished by the inflexible integrity of his character, the uniform liberality and consistency of his opinions, and the unobtrusive virtues of his private life,—characteristics which, if they fail to give the widest and loudest development of fame, are yet sure of the truest and most lasting. Mr. Smith attained to an advanced period of life, and survived to see many of the great and benevolent objects which he advocated finally accomplished; and I should say, that of the larger award of charity and justice which has been rendered to the Dissenters of this country, of which Mr. Smith was one, not a little of it may be said to have been owing to the evidence which his own life afforded of the peaceable disposition, the unquestionable loyalty, the intelligence and high moral worth which belong to that large portion of the people of England.

"In the death of Mr. Drummond we have to lament the loss of a very useful man; one to whom we have been indebted for many years for many rare plants which he collected in the Rocky Mountains during his first expedition to North America, and in the valley of the Mississippi in his last visit to that continent. He was sent out principally through the instrumentality of Sir William J. Hooker, for the purpose of making collections in all the departments of natural history; and there are some present who know how well he accomplished the objects of his mission. His plants were purchased by several naturalists in this country, in France, and America; and I understand from Sir W. Hooker that independent of the sums he had successively paid towards the expenses of Mr. D. in America, and towards the support of his family in Scotland, there is a considerable balance due to his widow. Mr. Drummond died of fever last year at the Havannah; and I feel that his death is scarcely less disastrous than that of Mr. Douglas.

"It does not seem to be sufficiently known that the natives of the colder regions inevitably run great hazard of sickness and death in resorting to climes of a high mean temperature. When it is recollected what a remarkable exemption from mortality has uniformly characterized the arctic expeditions under Sir J. Ross, Sir E. Parry, Sir J. Franklin, and Captain Back, and what a frightful destruction of life followed the naval and military expeditions to the West Indies in the war of 1793, and how many excellent men have successively perished in the fatal attempt to explore Africa, it is an irresistible conclusion that a high mean temperature is most prejudicial to the health of those unaccustomed to its influence: and the fact, I believe, is satisfactorily explained by the more prolific sources of disease which may reasonably be supposed to exist in the teeming climes of the south, and the effect which a sudden change of temperature exerts on the human body. When we reflect, for instance, that in this country the mean temperature is about 52°, and that we consequently have a vital energy equivalent to the production of 46° of animal heat to enable us to maintain the blood at its standard heat of 96°; and that a native of Great Britain, by resorting to a clime where the mean temperature is 75° or 80°, cannot accommodate himself at once to this great change of circumstances,—that is, with a power to generate 46°, he cannot at once lose this power, and generate only 21° or 16°, to put himself on a level with the condition of the native of a tropical clime,—it will be evident that if he falls under the influence of the causes of fever, the disease must have in him a violence and a precipitancy highly dangerous to life. I believe this fully explains the nature and fatality of what is called yellow fever; a disease entirely unknown to the *natives* of the West Indies and the most southern states of North America. Dr. Ramsey of Charleston, South Carolina, shows for a long series of years that the deaths by yellow fever in that city have been confined exclusively to strangers, and that no native physician or nurse was ever known to contract the disease. This liability to a fatal form of fever should at least induce great caution in those who resort to hot climates; and had the principle on which the mortality in those climates depends been fully understood, we might for many years perhaps have reaped advantages from the labours of Mr. Drummond in the cause of science. We owe to him many interesting plants; and like his fellow-labourer Mr. Douglas, his name must ever be honourably associated with the botany of North America."

His Grace the Duke of Somerset was re-elected President; E. Forster, Esq., Treasurer; Francis Boott, M.D., Secretary; and Richard Taylor, Esq., Under-Secretary; and the following five gentlemen were elected into the Council, in the room of others going out, agreeably to the By-Laws: viz. William Borrer, Esq.; John Bostock, M.D.; John George Children, Esq.; Archibald Menzies, Esq.; Rev. Thomas Rackett, M.A.

XCIV. *Intelligence and Miscellaneous Articles.*

ON THE PROPERTIES OF LIQUID CARBONIC ACID.

According to M. Thilorier, this liquefied gas presents the strange and paradoxical fact of a liquid more expansible than the gases themselves: from 32° to 86° Fahr., its volume increases from 20 to 29, that is to say, that at 86° Fahr. the increase of volume is nearly equal to half the volume at 32° Fahr. Its expansion is four times greater than that of atmospheric air, which from 32° to 86° Fahr. only expands $\frac{30}{267}$, whilst the expansion of liquid carbonic acid on the same scale is $\frac{116}{267}$. If the temperature of a tube containing a portion of liquid carbonic acid is raised, this liquid boils, and the empty space above the liquid is saturated with a greater or less quantity of vapour according to the elevation of the temperature. At 86° Fahr., the quantity of liquid at 32° Fahr. sufficient to saturate the empty space is represented by a portion of liquid equal to one third of the space in which the vaporisation has been effected. At 32° Fahr. the portion of liquid of saturation is only $\frac{1}{17}$ of the space saturated.

The pressure of the vapour formed by the liquefied gas from 32° to 86° Fahr., amounts from 36 to 73 atmospheres, which is equivalent to an increase of one atmosphere for every centigrade degree. It is important to observe that the weight or the density of the vapour increases in a much greater proportion than the pressure, and that the law of Mariotte is no longer applicable within the limits of the liquefaction. If the density of the vapour is taken for the base of the pressure, the pressure at 86° Fahr. will be equal to 130 atmospheres, whilst the manoscope will only indicate 73 atmospheres. If a tube of glass containing a portion of liquid and a portion of gas be heated, two contrary effects will take place:

1st, the liquid will augment by expansion;

2nd, the liquid will diminish by vaporisation.

The thermoscopic effects are very different according as the portion of liquid is greater or smaller to the portion of gas; the liquid in the tube will either expand, contract, or remain stationary. These anomalies furnished the means of verifying the numbers which the preceding researches had given on the expansion and vaporisation. According to these numbers, the points of equilibrium above which the liquid increases and below which it diminishes on the addition of heat, result from such a proportion when empty or full that at zero the liquid occupies $\frac{3}{10}$ of the whole tube. If the liquid at 32° Fahr. occupies one third of the tube it seems as a retrograde thermometer, of which the liquid increases by cold, and diminishes by heat. If the liquid at 32° Fahr. occupies two thirds of the tube it acts as a regular thermometer; that is to say, the liquid increases and diminishes according to the laws of expansion. This thermometer is limited to 86° Fahr. as at this temperature the tube is entirely filled by the liquid.

The specific gravity of this liquefied gas at 32° is 0·83, water being 1. It presents the singular phænomenon of a liquid which from $-68°$ to $+86°$ Fahr., runs through the scale of densities from 0·90 to 0·60. It is insoluble in water, with which it does not mix; but is soluble in alcohol, æther, naphtha, oil of turpentine, and sulphuret of carbon, in every proportion; it is decomposed in the cold with effervescence by potassium; it does not act sensibly on lead, tin, iron, copper, &c.

When a jet of liquid carbonic acid is directed upon the bulb of an alcohol thermometer, it falls rapidly to $-194°$ Fahr.; but the frigorific effects do not correspond with this decrease of temperature, which is accounted for by the almost absolute want of conducting power, and the little capacity for heat of the gases; therefore the intensity of the cold is enormous, but the sphere of action is limited in some measure to the point of contact. If the gases have little effect in the production of cold, such is not the case with the vapours, of which the conducting power and the capacity for heat are much greater. If æther, for instance, could be placed in the same conditions of expansion as the liquefied gas, a much greater frigorific effect would be obtained than by liquefied carbonic acid. To accomplish this object it is necessary to render æther explosive, which is easily effected by mixing æther with liquid carbonic acid. In this intimate combination of two liquids which dissolve one another in every proportion, the æther ceases to be a liquid permanent under the pressure of the atmosphere; it becomes expansive similar to a liquefied gas, at the same time preserving its properties as a vapour, that is to say, its conductibility and capacity for caloric.

The effects produced by a tube filled with explosible æther are remarkable; a few seconds were sufficient to congeal 772 grains of mercury in a glass vessel. On exposing the finger to the jet which escapes, the sensation is intolerable, and seems to extend much further than the point of contact.

M. Thilorier intends to replace æther by sulphuret of carbon; and it is probable the effects obtained will be still more powerful.

Annales de Chimie, Dec. 1835.

AMALGAMATION OF ZINC PLATES.

In consequence of the great advantages pointed out by Mr. Faraday of employing amalgamated zinc plates in the voltaic pile, M. Masson recommends the following simple and rapid method. After having placed on the zinc a little mercury, dilute sulphuric acid is poured upon it; the mercury is then rubbed over the surface of the zinc by means of a piece of linen. The mercury spreads over the surface with great facility, and the amalgamation is very rapid: a small quantity of dilute acid should be added from time to time; this appears to act as a cleanser to the zinc, for in forming a voltaic circuit with only one element, the operation does not go on so well or so quickly.—*Annales de Chimie,* November 1835.

CRYSTALLIZED OXICHLORIDE OF ANTIMONY.

M. Malaguti observes that when a large quantity of water is added to a solution of protochloride of antimony, there is immediately formed a very white and bulky precipitate, and which, thrown upon a filter and washed, constitutes the powder of algaroth. This, according to Grouvelle, is composed of 2 atoms of protoxide and 1 atom of protochloride of antimony. If, instead of filtering the precipitate, it is suffered to remain in the fluid from which it is precipitated, for 30 or 40 hours, it contracts considerably, and is converted into a thick crystalline deposit. The supernatant liquor being poured off, the crystals are to be washed and dried by exposure to the air.

The crystals thus procured are small, white, brilliant prisms, decomposeable into oxide of antimony by boiling in water, by continued washing, or by an alkaline carbonate; they are entirely soluble in nitric acid; they fuse by the heat of a common lamp, and lose the greater part of their chlorine.

The method of analysis adopted was very simple: a given quantity of the salt was boiled in a solution of carbonate of potash; the solution, after saturation with nitric acid, was treated with nitrate of silver; the residue gave the proportion of oxide of antimony. Another less simple method was also employed: a quantity of the oxichloride was heated by a spirit lamp in a bent tube, one end of which was blown into a bulb. The greater part of the chlorine was condensed in the cold part of the tube, which was separated from the bulb; the chloride was dissolved in muriatic acid, and the antimony was precipitated by tin. What remained in the bulb was dissolved by tartaric acid, and afterwards treated with nitrate of silver. The metallic antimony on one hand, and the chloride of silver on the other, gave the quantity of protochloride contained in the oxichloride analysed; these agreed perfectly with that obtained by carbonate of potash.

The analysis by carbonate of potash gave

Protochloride	24·72	25·30	25·19
Protoxide	75·27	74·10	74·48

Two analyses by fusion gave,

Protochloride	25·50
————	25·38

The mean of three analyses by carbonate of potash gave,

		Calculated.
Protoxide of antimony ..	74·51	74·54
Protochloride of antimony	25·70	25·46
	100·21	100·00

After having prepared sulphuretted hydrogen by muriatic acid slightly diluted with water and sulphuret of antimony, it will be observed that the solution remaining with the undissolved sulphuret becomes red on cooling. If it be added to a great quantity of water, an abundant yellowish precipitate is obtained, which, after some days, becomes a thin stratum of small crystals of a fine red colour. These crystals are merely the oxichloride coloured with variable quantities of sulphuret of antimony.—*Ann. de Ch. et de Ph.*, lix. 220.

ON POTATOE STARCH. BY M. GUÉRIN-VARY.

The author divides his memoir into two parts. In the first he examines what substances accompany potatoe starch prepared with distilled water, and considers several problems, including those proposed by the commission of the Academy for examining the memoirs on starch fecula. The second part contains the proximate analysis of starch, as well as ultimate analysis of this substance, of amidine, of exterior amidin (de l'amidin tégumentaire), of soluble amidin, and of the exteriors (tégumens) insoluble in water and deprived of the property of becoming blue with iodine.

In a former memoir M. Guérin has stated that potatoe starch, when prepared with distilled water, contains, as foreign organic bodies, only chlorophylle, and a substance of a waxy appearance. M. Payen, on the contrary, recognises in starch, besides other inorganic substances, a volatile oil ready formed; and he also states that every specimen of potatoe starch that he has examined possessed the property of restoring reddened litmus paper to its original colour, and contained carbonate of lime. M. Guérin in this memoir shows that the alkaline property of starch noticed by M. Payen is owing to the water which is used for washing it; and that volatile oil does not exist ready formed in the exteriors of starch, MM. Dubrunfaut and Beudant stating that it is formed during the alcoholic fermentation of starch.

After noticing dextrine bread, M. Guérin gives the solutions of several problems, and arrives at the following conclusions:

1st. Iodine has the same action on starch in water deprived of air, as it has when the air is not expelled.

2nd. Starch heated only with either pure or saline water, in a close vessel, gives a distilled liquor, which does not become blue on the addition of iodine.

3rd. When starch is treated with diastase and water, in a retort containing or not containing air, the distilled liquid does not turn blue with iodine.

4th. Pure fecula exposed to air by itself, or moistened with water for 48 hours at 113° or 115°, does not give rise to any carbonic nor acetic acid, and does not appear altered under the microscope. Thus the property of germinating, which is lost by the grains of certain description of corn after remaining for some hours in a damp soil at the temperature of 113° Fahr., cannot be attributed to any alteration which pure and damp fecula undergoes, as MM. Colin and Edwards consider.

5th. By treating starch with sulphuric acid, according to the process of M. de Saussure, no crystallizable compound could be obtained.

Having treated potatoe starch with alcohol and water, to extract the chlorophylle and the matter of a waxy appearance which it contains, the author proceeded to its proximate analysis. By rubbing this substance with water at 32° Fahr. until nothing more could be extracted from it, and evaporating the liquors in vacuo it left a residue containing

Amidine	61·71
Amidin (soluble)	38·29—100·

The part exhausted by water at 32° having been treated with boiling water, gave a liquor which by evaporation to dryness in vacuo, afforded

Amidine	60·31
Amidin (soluble) ..	39·69—100·

The part insoluble in boiling water amounted to 2·12 per cent. of the starch employed. From these experiments it appears that water acts equally upon starch at a freezing or a boiling temperature. Now as there is no known substance which by the mere action of water at 32° is converted in several distinct products, except very dilute nitrate of bismuth and other analogous products, the author concludes that boiling water does not convert starch into amidine and soluble amidin, as might be thought by the modifications that heat and water cause in the constitution of many products of organization.

The above results give the following composition :

Exterior amidin ..	2·12
Soluble amidin ..	38·13
Amidine	59·75—100·

Several ultimate analyses of starch, of exterior and soluble amidins, dried in vacuo at 275° Fahr., and the analysis of amidine dried in vacuo at 239° Fahr., allow him to give the composition of these substances as follows:

Starch $= C^{17} H^{10} O^{10}$; amidine $C^{10} H^5 O^6$; exterior amidin $=$ soluble amidin $C^7 H^5 O^4$, or $C^{17} H^{10} O^{10} = C^{10} H^5 O^6 + C^7 H^5 O^4$.

Starch $=$ amidine $+$ amidins.

These atomic formularies show that starch is equal to carbon and water; amidine to carbon, water, and oxygen; and amidin to carbon, water, and hydrogen. M. Guérin states that diastase converts exterior and soluble amidin, when hydrated, into a substance like sugar, and a substance insoluble in water, not rendered blue by iodine. When these amidins are dried, diastase does not act upon them; 100 parts of starch contain 1·705 parts of insoluble matter, which does not become blue with iodine, and which gives by analysis,

	1st Analysis.	2nd Analysis.
Carbon	47·71	47·68
Hydrogen ..	7·09	7·11
Oxygne ..	45·20—100·	45·21—100·

These analyses compared with those of exterior amidin show the difference that exists between these substances; and the author is inclined to consider exterior amidin as an immediate principle, and not as a mixture of exteriors, not becoming blue with iodine and amidine.—*L'Institut*, 1re Fev. 1836.

ARTIFICIAL CAMPHOR OR CAMPHOGENE.

M. Opperman obtains artificial camphor by passing a current of dry hydrochloric acid gas into oil of turpentine. The absorption of the gas is very rapid, particularly if the vessel containing the oil of turpentine is surrounded by ice.

Two distinct substances are formed: the one solid, the other liquid. The solid substance, which is commonly called artificial camphor, is composed of,

Carbon................	70·015
Hydrogen	9·717
Chlorine	20·272—100·004

This substance, when purified by sublimation, crystallizes in large and lengthened crystals, its taste is aromatic but weak, it burns with a very brilliant light, and colours the flame green, is soluble in alcohol like common camphor; its solution does not form any precipitate with nitrate of silver; the alkalies, lime, &c. decompose it by combining with its hydrochloric acid. To obtain the base of this salt the camphor was distilled with hydrate of lime: a clear and transparent oil was also obtained of a particular but weak odour, and an aromatic taste, which became solid at — 44° Fahr.: its composition, ascertained by means of the apparatus of M. Liebig, is,

Carbon..................	88·48
Hydrogen	11·52—100·

This substance, which had been called by M. Dumas camphogene, and by MM. Blanchet and Sell dadyle, is not acted upon either by nitric or acetic acid; but hydrochloric acid immediately reproduces artificial camphor.—*L'Institut*, No. 149.

NEW ACID OF BROMINE.

M. Eugene Peligot is engaged in determining the action of chlorine, bromine, and iodine on salts formed by the organic acids with some of the metallic oxides, and has already arrived at results interesting both from their novelty and from the generalization they appear to present. When dry benzoate of silver is acted on by bromine it is decomposed, and bromine is absorbed in large quantity. There is produced, bromine of silver, and a new acid which resembles benzoic acid in some of its physical properties, but differs extremely in its composition. It contains, besides the elements of benzoic acid, all the oxygen of the oxide of silver and an atom of bromine. It may be obtained anhydrous by treating the products of the action by dry sulphuric æther, which dissolves the acid, and leaves the bromide of silver.

At ordinary temperatures this acid is solid, but melts a little below the boiling point of water; slightly soluble in cold, but extremely so in boiling water, which upon cooling deposits the greater part of it: it burns with a flame edged with green, indicating the presence of bromine, which could not be recognised by a solution of nitrate of silver, this not precipitating with it; it forms crystallizable salts with oxides, in which the oxygen of the acid to the oxygen of the base is as 4 to 1.

M. Peligot has endeavoured, without success, to form an analogous acid by means of chlorine; the action is very violent, and inflammation and complete destruction of the salt ensues. This happens with bromine if placed in contact with the salt in a fluid state; it must be acted on by passing the vapour of bromine slowly into it, which will be absorbed. The action of iodine differs from that of bromine, for it forms both iodide and iodate of silver; but the acid has not yet been sufficiently examined to determine its nature.

The action of bromine on benzoate of silver is moreover not a particular action caused by the nature of benzoic acid, for it acts in a similar manner on salts formed by acids which appear to be less disposed to superoxygenation, as the oxalic and acetic acids, and everything tends to the belief that the mode of action of this body will become general.—*L'Institut,* 15 Fev. 1836.

OBSERVATIONS ON THE SOLAR ECLIPSE OF MAY 15, 1836.

Photometrical Observations during the Eclipse, by Thos. Galloway, Esq.

To R. Taylor, Esq.

DEAR SIR,

The following observations, made during the solar eclipse on the 15th instant with two of Leslie's photometers, may be interesting to some of your readers. The observations were made at Mr. Bishop's observatory in the Regent's Park, and the photometers were placed on a table on the lawn, fully exposed to the sun's influence. With the exception of a few passing clouds the sky remained clear till towards the end of the eclipse, when the atmosphere became hazy.

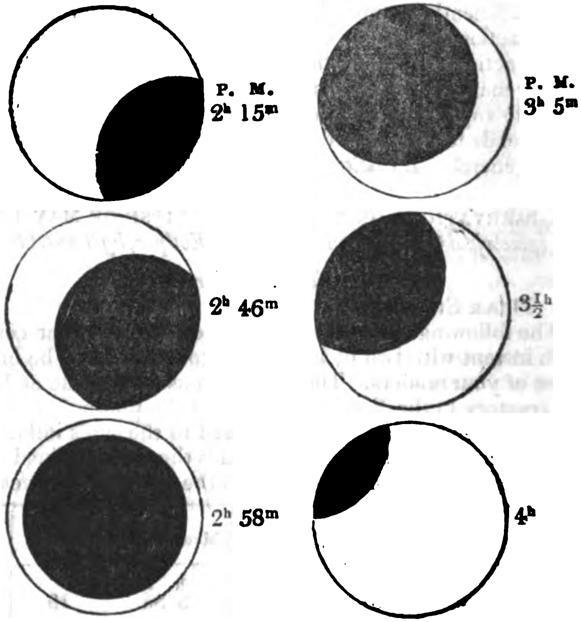
Mean Time.	Phot. A.	Phot. B.	Mean Time.	Phot. A.	Phot. B.
h m			h m		
1 5	87	74	3 35	19	18
40	83	73	40	24	22
2 0	87	78	45	30	27
15	76	70	50	34	30
25	70	65	55	40	35
30	65	58	4 0	48	41
35	56	53	5	53	45
40	52	44	10	53	45
45	46	43	15	47	43
50	35	34	20	60	49
55	30	29	25	53	48
3 0	18	19	30	51	43
5	21	19	35	40	31
10	19	19	40	40	32
15	14	17	45	20	23
20	14	17			
25	14	17			
30	15	17			

Yours truly,

Sergeant's Inn, May 24, 1836. THOS. GALLOWAY.

Extract of a Letter from R. V. Yates, Esq. of Toxteth Park, Liverpool, dated Edinburgh, May 15, 1836.

"We have just enjoyed a most glorious sight, an annular eclipse. The morning arose cloudy, and gave little promise; but about 10 the clouds cleared off, and during the whole period of the eclipse nothing interfered with our seeing it perfectly. It was curious to watch it when it was just going to become annular, the light broke in so rapidly. It remained annular only a very short time, perhaps between 5 and 10 minutes."



Much obscurity.

Annular.

Air considerably offuscated.

In a garden near Birmingham the Gentians partially closed their flowers during the eclipse, and then opened again.

The Rev. James Yates, by whom the above was communicated to us, has obligingly directed our attention to the description of an Annular Eclipse in the Norwegian Account of Haco's Expedition against Scotland, which we transcribe from p. 44 of that work.

Þá er Hacon kònongr lá í Rögnvalzvagi dró myrkr mikit á sólina, sva at lítill hringr var biartur um sólina utan*, ok hellt þrí nockora stund dags.

While King Haco lay in Ronaldsvo a great darkness drew over the sun, so that only a little ring was bright round about the sun, and it continued so for some time.

* Though relating to inquiries of a different class, I am tempted to note the Islandic expression "*um* solina *utan*," as a remarkable illustration of the real origin of our compound preposition ABOUT, ymbutan,—the noun being here interposed between the two prepositions. See my Note on the complicated mistakes in which the history of this word had been involved by Spelman, Skinner, and Tooke, subjoined to the 8vo. edition of the Diversions of Purley, 1829, vol. i. p. viii.—R. TAYLOR.

HYDRAULIC LIME.

M. Vicat communicated a paper to the Royal Academy of Sciences at Paris on the sole efficacy of magnesia in rendering certain limestones hydraulic. This paper has for its object the correction of an opinion given by M. Berthier in the *Journal des Mines* of 1822, that magnesia alone has no more efficacy than alumina to render lime hydraulic; from which it would follow that silex was the only essential principle in all cases.

M. Vicat was for a long time of the same opinion, which he now declares is incorrect; and says that magnesia alone, when in sufficient quantity, will render pure lime hydraulic. He does not explain the degree of energy of these new species of lime, but only affirms that they will solidify from the 6th to the 8th day, and continue to harden in the same manner as ordinary hydraulic lime.

Until his experiments are further advanced, he states that the proportions of magnesia taken and weighed after calcination should be from 30 to 40 of every 40 of pure anhydrous lime. The native limestones examined and cited by M. Berthier contained only from 20 to 26 of magnesia for every 78 to 60 of lime: it is probable that this want of proper proportions was the cause of his negative results. M. Vicat, in conclusion, points out the importance of these observations,—hydraulic lime never having been found in the calcareous formation below the lias is because the dolomites have never been examined, but it is now probable it may be found in this lower formation.—*L'Institut*, No. 153.

NOTE RESPECTING CERTAIN CONTROVERSIAL COMMUNICATIONS LATELY SENT FOR INSERTION IN THIS JOURNAL.

LIEUT. LECOUNT.

We have received a letter from Lieut. Lecount, claiming the insertion, in its entire form, of his previous letter in reply to Mr. P. Barlow, from which we gave an extract in our last Number, p. 439. Lieut. Lecount makes this claim on the ground "that it is the general practice to allow any person who is attacked in a periodical publication the right of replying." We have merely to observe, in answer, that our extract includes the real matter of Lieut. Lecount's reply, and that we omitted only irrelevant matter of a personal nature, at the same time referring our readers to the pamphlet which he has published. We must therefore decline all further allusion to the subject.

We may remark in reference to this, as well as to other cases of a similar kind which have lately occurred, that we cannot permit a scientific discussion to degenerate into a personal controversy.

MR. HENWOOD.

We take the present opportunity of noticing Mr. Henwood's letter in the *Records of General Science* for May, to which the remark just made is also applicable. There was a very sufficient reason for our not inserting Mr. Henwood's last reply to Mr. John Taylor on the subject of the steam-engines of Cornwall, namely, that the quotation it contains from the *Records of Mining* is made in so garbled a manner as to be a complete misrepresentation of Mr. Taylor's statement. To prove this we give the passage entire as we find it in the *Records of Mining*, including in brackets what Mr. Henwood has suppressed, and by the suppression of which he has perverted the sense of the whole.

"In the early part of the year (1813) the best duty was about 26 millions, by Captain Trevithick at Wheal Prosper, [Captain John Davey at Wheal Alfred, and Messrs Jeffree and Gribble at Stray Park. Towards the close of the year Captain Davey first attained 27 millions, then Jeffree and Gribble 28, and by the end of the year the latter had nearly arrived at 30 millions.]"

One half of a sentence of the foregoing paragraph is thus brought forward in contradiction of Mr. Taylor's statement that Captain Trevithick's "engine did only about 26 millions duty, and did not equal other engines then working in the common way." A reference to the work itself showed us that if the remainder of the paragraph had been given, it would at once have been seen that the imputation was groundless. Can it reasonably be required of us to lend our pages to charges thus supported?

We will only add to this that the title "On a new Rotative Steam-Engine," was prefixed to Mr. Taylor's first paper not by him, but by ourselves; and the only sense in which we used the term "new" was in that of "newly or lately erected." Mr. Henwood must have been aware that Mr. Taylor had himself wholly precluded the supposition that it could mean "newly invented," by mentioning an older engine of the same description erected by Mr. Godfrey.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1836.

Chiswick.—April 1. Dry haze: sleet: stormy with rain at night 2, 3. Cold and windy. 4. Clear and fine. 5. Slight haze: cloudy: rain. 6. Rain: cloudy. 7. Rain: clear. 8, 9. Rain: cloudy and fine. 10. Fine. 11. Cold haze: clear at night. 12. Overcast: rain. 13. Cloudy. 14. Overcast and cold. 15. Slight rain. 16. Foggy. 17. Rain: cloudy and cold. 18. Drizzly: fine. 19. Fine. 20. Cloudy: rain. 21. Very fine. 22. Rain: fine. 23. Rain. 24. Rain: stormy at night. 27. Cloudy and cold. 28. Overcast. 29, 30. Clear, cold and dry.

Boston.—April 1. Fine: rain and snow P.M. 2. Cloudy. 3. Stormy: rain and snow A.M. 4. Fine. 5. Cloudy. 6. Rain. 7. Rain. 8. Rain. 9. Cloudy. 10. Fine. 11. Cloudy. 12. Cloudy. 13. Cloudy: rain P.M. 14. Cloudy: rain P.M. 15. Cloudy. 16. Fine: rain P.M. 17. Cloudy. 18. Rain. 19. Fine: rain P.M. 20. Cloudy. 21. Fine. 22. Fine: rain early A.M. 23. Fine. 24. Cloudy: rain early A.M. 25. Cloudy. 26. Fine. 27. Rain. 28. Cloudy. 29. Fine: ice this morning. 30. Stormy.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary; by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; and by Mr. Veall at Boston.

Days of Month. 1836. April.	Barometer. London: Roy. Soc. 9 A.M.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ A.M.	Thermometer. London: Roy. Soc. Fahr. 9 A.M.	Thermometer. London: Roy. Soc. Self-registering. Min.	Thermometer. London: Roy. Soc. Self-registering. Max.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ A.M.	Wind. London: Roy. Soc. 9 A.M.	Wind. Chisw. 1 P.M.	Wind. Bost.	Rain. London: Roy. Soc. 9 A.M.	Rain. Chisw.	Rain. Boston.	Dew-point. Lond.: Roy. Soc. 9 A.M. in degrees of Fahr.
○ F. 1.	29·792	29·826	29·361	29·35	43·4	36·9	44·7	46	33	42	SW.	SE.	NW.	...	·76	·03	34
S. 2.	29·511	29·833	29·528	29·05	39·3	33·7	44·3	46	34	38	SW.	W.	W.	·591	·02	·27	37
⊙ 3.	29·913	30·335	30·003	29·50	41·6	33·8	45·0	48	30	42	WNW. var.	N.	N.	·047	·07	...	36
M. 4.	30·366	30·412	30·364	28·94	39·7	32·3	46·6	51	27	39	NW.	N.	N.	·133	...	·17	35
T. 5.	30·179	30·242	30·161	29·77	42·3	34·7	50·2	54	40	43	SW.	SW.	calm	...	·17	...	35
W. 6.	29·893	29·931	29·793	29·46	44·2	40·8	48·2	50	39	44·5	SE. var.	SW.	calm	·111	·11	·07	41
Th. 7.	29·348	29·431	29·134	28·92	43·6	40·3	50·3	55	35	43	S. var.	SW.	SW.	·061	·40	·09	41
☾ F. 8.	29·006	29·251	29·034	28·70	46·4	38·3	49·8	53	39	40	SE. var.	S.	calm	·355	·04	·34	41
S. 9.	29·283	29·484	29·334	29·03	42·4	40·2	46·7	48	39	45	E.	E.	calm	·036	·11	·13	42
⊙ 10.	29·550	29·743	29·569	29·20	45·4	40·2	53·2	56	42	47	N.	NE.	calm	·194	...	...	43
M. 11.	29·707	29·765	29·737	29·30	43·7	42·8	48·8	53	29	43·5	N.	W.	calm	·019	...	...	43
T. 12.	29·775	29·848	29·795	29·30	48·2	38·5	53·7	53	44	45	SW.	SW.	W.	...	·01	...	42
W. 13.	29·816	29·846	29·784	29·21	51·4	45·2	56·4	57	35	53	SW.	W.	W.	...	...	...	46
Th. 14.	29·992	30·089	30·005	29·45	48·2	42·9	54·5	58	47	47	W.	SW.	calm	...	·01	·04	43
● F. 15.	30·152	30·208	30·150	29·65	50·3	47·5	54·2	56	30	47·5	NNW.	NE.	calm	...	·01	·11	49
S. 16.	30·162	30·200	30·101	29·64	43·4	38·8	52·6	58	42	47	E.	E.	calm	·036	·10	...	45
⊙ 17.	30·115	30·146	30·102	29·66	46·6	42·3	47·8	51	36	47	E.	N.	calm	·027	·03	·07	43
M. 18.	30·095	30·133	30·119	29·60	48·2	42·0	54·7	59	35	47	S.	W.	calm	·061	...	...	44
T. 19.	30·117	30·142	30·095	29·58	51·2	44·3	56·2	57	47	51	S.	SW.	calm	...	...	·04	45
W. 20.	29·984	30·010	29·899	29·40	53·3	48·5	55·2	57	41	53	SW. var.	SW.	W.	...	·08	·06	47
Th. 21.	29·936	29·944	29·912	29·40	49·5	43·3	56·3	58	46	50	SW.	SW.	W.	·063	·07	...	44
F. 22.	29·814	29·971	29·806	29·22	53·6	47·2	61·3	63	42	55	SW.	W.	W.	·061	·09	·08	48
S. 23.	29·890	29·908	29·843	29·31	51·6	47·4	54·6	55	44	52·5	SW.	SW.	W.	·094	·13	...	48
☽ ⊙ 24.	29·699	29·973	29·707	29·25	46·9	45·3	47·9	52	35	48	E.	SE.	calm	·161	·55	·03	46
M. 25.	30·117	30·144	30·133	29·65	45·8	39·7	54·7	57	38	46	NW.	NE.	calm	·375	...	...	43
T. 26.	30·111	30·134	29·867	29·51	50·6	44·4	53·5	55	36	50	W.	NW.	NW.	...	·04	...	43
W. 27.	29·780	29·992	29·806	29·31	44·6	38·2	48·7	51	33	43	NW.	N.	NW.	...	·07	·07	38
Th. 28.	29·938	29·968	29·869	29·42	46·3	37·5	52·3	56	34	45	NNW.	NW.	calm	·036	...	...	38
F. 29.	29·903	29·926	29·844	29·41	39·4	35·5	46·7	55	27	38·5	NW. var.	NW.	N.	·027	...	...	32
S. 30.	29·774	29·807	29·766	29·25	42·3	32·2	49·4	51	32	43	WSW.	N.	N.	...	·01	...	32
	29·857			29·38	46·1	40·5	51·3			45·8				Sum 2·488	2·98	1·60	41·5

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END OF THE EIGHTH VOLUME.

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