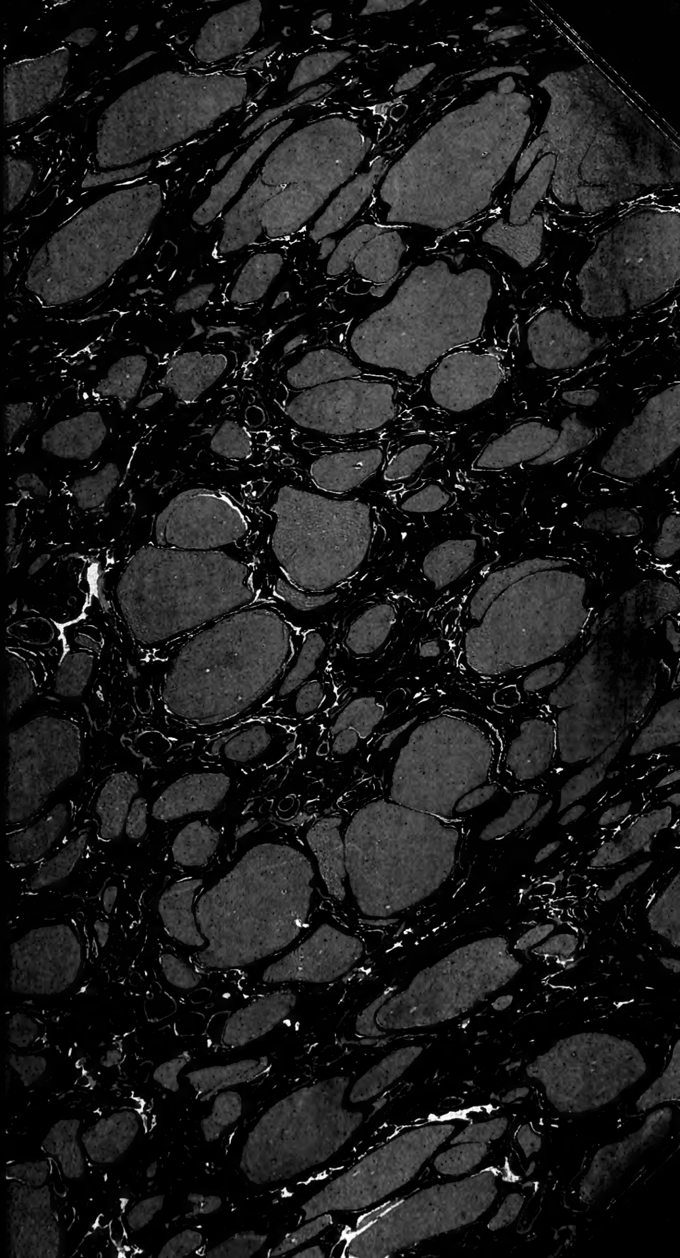

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Monit. Polit.* lib. i. cap. 1.

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ERRATA.

In the Supplement, January 1842, vol. xix. page 586, line 4 from the bottom, *for* 1·8 *read* 21.

Present volume, page 382, line 8, *for* "parallel to the ecliptic" *read* "in the plane of thermal refraction."

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

JANUARY 1842.

I. *Abstract of Thermometrical Researches. By* Professor HESS *of St. Petersburgh**.

[Continued from vol. xix. p. 183, and concluded.]

M. HESS having determined, as already described, the quantities of heat evolved by the combination of muriatic acid with potash, soda, and ammonia, proceeded to examine its relations to lime. The quantity of heat evolved by the combination of the acid $H\,Cl + 12\,Aq$ with slaked lime, he found to be represented in four experiments with the calorimeter by the numbers 435·2, 437·6, 436·3, 449·6, in mean result 439·7 units of heat for an atom of the acid. In order to control this number he made a series of experiments with unslaked lime, which were found, however, very difficult of accurate performance, as the lime became hard, and did not at all easily or rapidly combine with the acid. The numbers which he obtained were 580·7, 606·1, and 612. In determining previously the quantity of heat evolved by lime in slaking, he had deduced the number 167·2, but he repeated the experiment twice, and found 161·4 and 160·5, the mean of the three numbers being 163·03. Subtracting this from the mean of the numbers obtained with unslaked lime (599·6), the remainder, 436·57, fully confirms the number 439·7 obtained as the mean result of the trials with slaked lime.

M. Hess found that dry chloride of calcium gives out, when dissolving in water, 236·4 units of heat: this, however, is not all due to the fixation of the six atoms of water of crystallization, as the hydrated salt $Ca\,Cl + 6\,Aq$, when dissolved, gave out 33·65 units of heat.

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* From Poggendorff's *Annalen*, vol. lii. p. 97.

Experiments with nitric acid.—The experiments with nitric acid were carried on in precisely the same manner as those already described. The acid employed had the specific gravity 1·325. By combination with potash the quantity of heat evolved from an atom of this nitric acid was found to be expressed by the numbers 419·5 and 399·4, mean 409·45. With soda, the numbers 410·9 and 409·2 were obtained (mean 410·05). With ammonia, the experiments being carried on in the calorimeter, the results were 398·5, 404·5, and 400·8, the mean being 404·3. When combining with dry lime an atom of nitric acid was found to evolve heat, in proportions expressed for five trials, by the numbers 459·3, 459·3, 450·6, 435·6, and 451·7, the mean being 451·3.

The composition of the nitric acid used being expressed by the formula NO_5+8HO, the general results obtained by the combination of the bases and acids experimented on by M. Hess may be arranged as in the following table, in which the numbers exhibit the proportional quantities of heat, expressed by each base with an atom of the acid at the head of each column.

Base.	SO_3+HO.	NO_5+8HO.	$HCl+12HO$.
$KO+Aq$	601	409	361
$NaO+Aq$	605	410	368
NH_3+Aq	598	404	368
$CaO+Aq$	642	451	436

Although the mean numbers are here given without any correction, it is yet evident that the three first in each column are the same, differing only by quantities which fall within the limits of unavoidable experimental error.

The numbers given for lime are all much higher, but it is to be remarked, that with sulphate of lime this results evidently from the fixation of water (see Phil. Mag., S. 3., vol. xix. p. 181), and that the other lime salts also fix water. It hence follows that if the numbers in each vertical column are the same for all bases, a constant proportion holds between the numbers in each horizontal line, which are given by any one basis for the different acids, and *this proportion is the same for every base* (supposing the salt to be neutral and anhydrous). It is only necessary, therefore, to know the quantity of heat evolved by the union of an acid with any one base, to determine either the quantity of heat evolved by this base with any other acid, or by the same acid with any other base. It may be objected that this result may not be general, but hold only for a certain group of bases, but this problem it is to be hoped will soon be resolved.

Before passing to describe the other researches of M. Hess, it is proper to observe here, that the law just now announced is at variance with that which Dr. Andrews has deduced from his experiments, an abstract of which has been given in the Phil. Mag., S. 3., vol. xix. p. 183. Dr. Andrews considers that all acids evolve, when combining with the same base, the same quantity of heat, but that different bases evolve different quantities of heat. For sake of comparison, we here present in a table the numbers he obtained with the same acids and bases as M. Hess operated on.

Base.	Sulphuric acid.	Nitric acid.	Muriatic acid.
Potash	7°·32	6°·76	6°·56
Soda	7·44	6·45	6·74
Ammonia	6·34	5·58	5·58
Lime	......	7·20	7·08

It is not our purpose to discuss which of these conclusions is best supported by the evidence, as the respective authors will probably continue the investigation of the subject until the true law is obtained and recognized.

On Thermoneutrality.—If we take two solutions of neutral salts which have the same temperature, and by their mixture generate two new salts, there is in general no rise of temperature: these saline solutions are said by M. Hess to be *thermoneutral*, and he explains this phænomenon by the law and the numerical results that have been just now described. Thus, if the solutions contain sulphate of potash and nitrate of lime, the quantities of heat absorbed by the separation and evolved by the combination of the acids and bases respectively, are shown to be the same, for there are by the table,—

Before mixture.		After mixture.	
$CaO.NO_5$	= 451	$CaO.SO_3 + 2\,Aq$	= 642
$KO.SO_3$	= 601	$KO.NO_5$	= 409
	1052		1051

These numbers are not in any way corrected.

In some cases of double decomposition a slight change of temperature occurs; thus if chloride of calcium and sulphate of potash be taken, there is

Before mixture.		After mixture.	
$CaCl + 6\,Aq$	= 436	$CaO.SO_3 + 2\,Aq$	= 642
$KO.SO_3$	= 601	KCl	= 361
	1037		1003

The difference here evidently results (says M. Hess) from there having been more water in combination before the mixture than after it. Thermoneutrality is therefore perfect only when the circumstances of the salts before and after mixture are the same.

M. Hess remarks, that since all bases appear to evolve the same quantity of heat when combining with any given acid, we cannot assume the amount of heat evolved as a measure of affinity. He points out that the numbers obtained in his results are not really those indicating the heat evolved with the acid, but only the difference between the heat lost by separation from water, and gained by combination with the acid. This difference is with the same acid a constant quantity. It remains to know whether the quantities of heat evolved by different bases in combining with water is the same, and from experiments on the slaking of lime, and on the solution of hydrate of potash ($KO . HO$) in water, M. Hess deduces that it is not, but that, *the stronger a base is the more heat it evolves in combining with water*, the numbers with dry lime and hydrate of potash being 163 and 323.

Constitution of the Sulphates.—In relation to the nature of acid salts, M. Hess makes the following observations. Adverting to the views of the composition of common bisulphate of potash, which by Berzelius is looked upon as a double salt ($KO . SO_3 + HO . SO_3$), and by Graham as sulphate of water, in which the second atom of (saline) water is replaced by sulphate of potash, $HO . SO_3 (KO . SO_3)$ corresponding to $HO . SO_3 (HO)$, he considers the evidence obtained from his experiments as disproving both views, on the following grounds.

If $KO . SO_3$, when added to $HO . SO_3 (HO)$, did nothing more than replace (HO), no heat could be developed, but it is found by experiment that a considerable quantity is really evolved*. This heat cannot be attributed to the displaced water combining with the salt, for if water be directly added to ($KO . SO_3 + HO . SO_3$), no heat whatever is evolved. The true formula of the common bisulphate of potash he considers to be represented by $(KO + 2 SO_3) + HO$, from these facts. When the dry bisulphate of potash ($KO + 2 SO_3$) is placed in the calorimeter, with a vessel of water of ammonia, and the mixture effected, it is found that the quantity of heat evolved is the same (406 units) as the second atom of acid should have given when neutralized by an alkali after com-

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* Dr. Andrews, however, considers that no heat is evolved in the formation of such double salts.

plete dilution by water (see vol. xix. pp. 25 and 179). Hence $KO\,.\,SO_3$ in combining with SO_3 had disengaged the same quantity of heat as if the free acid had been fully diluted, and hence much more than an atom of water could. Hence $KO\,.\,SO_3$ and HO are not thermometrically equivalent, as Graham's theory should require.

The total quantity of heat evolved by the formation of $KO+2\,SO_3$, M. Hess represents as made up of the following quantities:—

K O, evolves by combining with water…………	x units.
SO_3 …………………………………………………	510
K O and SO_3, each fully diluted with water, evolve by combining with each other………	407
	$x+917$
$KO\,.\,SO_3$ combining with SO_3 ……………	510

If we place the numbers which represent the quantities of heat between the symbols of the bodies which evolve it, we have $KO\,.\,x + 917\,SO_3\,.\,510\,SO_3$.

To know with certainty the place of the water in the formula of the hydrated salt, we must estimate the quantity of heat evolved by its union, but it is so small that the exact determination is very difficult. If we term it $.y.$ then the expression becomes $KO\,.\,x + 917\,SO_3\,.\,510\,SO_3\,.\,y\,HO$. The dry salt is therefore $KO + 2\,SO_3$, and the ordinary salt $(KO + 2\,SO_3) + HO$.

M. Hess concludes with some observations on the necessity of a knowledge of the quantity of heat evolved by the combination of every essential element of a compound, to a correct knowledge of its theoretical nature, and suggests the determination of the relation between the quantities x and 917, and 510 and y as an object of additional researches.

II. *On the Cause of the Electricity of Effluent Steam.* By Wm. George Armstrong, *Esq.*[*]

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

After an interval of several months, I have lately resumed my experiments on this curious subject, and have at length ascertained, that the *place* where the excitation of electricity is effected, is that at which the steam is subjected to *friction*.

In reflecting upon many of the experiments of which I have

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[*] On the subject of this paper see Phil. Mag., 3rd series, vol. xviii. pp. 93, 95, 100.—Edit.]

already published an account, I felt convinced that the electricity manifested in an insulated boiler, during the emission of steam, could not be attributed to the accompanying evaporation of the water; and being wholly unable to conceive any other cause, operating in the boiler, to which the effect could with any probability be ascribed, I became persuaded that the source of the electricity could only be situated at the discharging orifice, or in the channel through which the steam was conveyed to it. Such of my previous experiments as appeared to militate against this supposition, I conceived might probably have been rendered fallacious by the omission of proper precautions to prevent a conduction of electricity, by moisture, between parts of the apparatus which I had assumed to be insulated, with respect to each other. I therefore determined to repeat the principal experiment of this kind, with the addition of such measures as I deemed requisite, to obviate the defect I have alluded to, and the following is the mode in which I proceeded.

Into an insulated boiler I inserted one end of the glass tube A, which was placed in a horizontal position, and to the other extremity of which a stop-cock C was affixed, having a passage through it considerably smaller than that through the tube; and in order to prevent any communication of electricity between the boiler and the cock by the deposition of moisture on the inner surface of the glass, I surrounded part of the tube with a red-hot iron cylinder, which is represented at B. I then attached to the cock a second glass tube D, from the extremity of which the steam was discharged.

Upon opening the cock the ejected steam proved, as usual, to be positively electrified; but the boiler, which in all former experiments had yielded the opposite electricity to that of the steam-cloud, *now remained neuter*; and the *cock*, instead of the boiler, became negatively electrified. When I say that the boiler remained neuter, I must be understood to mean, that it was as nearly so as could be expected from the difficulty of wholly intercepting the transmission of electricity from the cock to the boiler. Feeble electricity did appear in the boiler, but no question could exist with respect to its origin; for when I touched the cock with a wire, the electricity of the boiler vanished entirely; but when I touched the boiler, the electricity of the cock was scarcely diminished; and upon forming a communication between the boiler and the cock, the boiler became negatively electrified in the usual degree.

It was obvious, therefore, that the excitation took place at the cock, where, by reason of the narrowness of the channel, the force of the current was chiefly exerted.

I then removed the second glass tube, and discharged the steam direct from the cock, which continued to be negatively electrified precisely the same as before.

The heat of the iron cylinder proved exceedingly apt to rupture the tube which passed through it, and several annoying explosions were in consequence occasioned during my experiments; but I afterwards found that sufficiently decisive results could be obtained, without the aid of external heat, merely by inclosing a portion of the tube, next to the boiler, in a cylindrical case, about three inches in diameter, stuffed with wool or other similar material.

I next made a number of experiments to ascertain the effect of varying the form and material of the aperture at which the electricity was excited; and, contrary to what I had expected, I found that apertures which were calculated to produce an increase of friction did not in general cause an increase of electricity, and very often had an opposite effect. Notwithstanding this, however, the intensity of the development proved to be greatly dependent upon the nature of the orifice employed; and to such a degree have I increased the electricity by modifying the orifice, that I have produced sparks four inches long, with an expenditure of steam not greater than would be occasioned by a circular aperture 1-10th of an inch in diameter. But my experiments on this head are, as yet, far from complete; and it would therefore be premature at present to describe them, or to hazard any opinion respecting the immediate cause of the electrical excitation.

I have also recently obtained some very remarkable results, relative to the transmutation which, under certain conditions, takes place in the electrical states of the steam-cloud and boiler. When the gun-metal boiler, or generator, which I have described in a former communication, was new, and its interior surface was rough and oxidized, as the surface of castings usually are, the electricity of the steam was uniformly positive, provided no corrosive material was mixed with the water; but lately, upon having the generator bored out, so as to give it a smooth metallic surface within, the steam became negatively electrified, although nothing was contained in the water to produce that effect. I afterwards had the inside of the generator coated with tin; but the steam still continued to be negatively electrified.

The boiler which I am now using is made of wrought iron, and the electricity of the steam discharged from it has been invariably positive, except in the instance I am about to mention. Potash and soda, which in the gun-metal generator tended so strongly to the increase of positive electricity in the steam-cloud, appear to have little influence when introduced into the iron boiler; but I have not yet tried, in the iron boiler, the acids and other things which, in the gun-metal generator, caused the steam to give negative electricity.

In consequence, however, of the influence which the state of the inner surface of the generator had been proved to exert upon the electricity of the steam, I was induced to try whether the steam from the iron boiler could not be caused to evolve negative, instead of positive electricity, by being brought into contact with a considerable surface of polished brass on its way to the discharging aperture; and with this view, I conveyed the steam from the boiler through a brass tube, an inch and a half in diameter, made bright and smooth in the inside and terminating in a small hole, at which the steam was discharged. Under these circumstances the electricity of the steam continued to be positive, but was rendered exceedingly feeble. I then moistened the inside of the brass tube with dilute nitric acid, and, by this means, the steam from the iron boiler became for the first time *negatively* electrified, though not in a high degree.

Newcastle-upon-Tyne,
Dec. 9, 1841.

WM. GEO. ARMSTRONG.

III. *On* Mossotti's *Theory of Molecular Action. By the Rev.* P. KELLAND, *M.A., F.R.SS. L. & E., F.C.P.S., Professor of Mathematics in the University of Edinburgh, late Fellow and Tutor of Queen's College, Cambridge.*

To Richard Taylor, Esq.

DEAR SIR,

IT is with great pleasure that I find attention at length directed to the interesting subject of molecular equilibrium. Since the appearance of the translation of M. Mossotti's paper in the Scientific Memoirs[*], nothing has been written relative to it, except my own memoir in the seventh volume of the Transactions of the Cambridge Philosophical Society. The duty of noticing any objections which are brought against the theory, may, therefore, seem to devolve on me. It is with this impression that I give the following explanation of the points

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[*] Taylor's Scientific Memoirs, Vol. i. p. 448.

objected to, and by putting them in a proper light, I shall be considered, I trust, to have offered the best apology for them.

I proceed then to examine the difficulty raised relative to the second set of equations (p. 385. of vol. xix.). Let us suppose, for the sake of argument, that we are driven to the alternative of admitting "either that the molecular action expressed by the first terms of these equations is included in that expressed in the second, in which case the first terms are superfluous and the equations incorrect; or that besides the forces given in the hypothesis, we must take into account certain other forces whose nature is wholly unknown, viz. those by which the pressure on the molecules is produced."

If by the forces spoken of in the last clause, it is implied that the forces assumed to exist by M. Mossotti could not include pressure in addition to the effects previously taken account of, I have, for the present, no other reply to offer than this:—That M. Mossotti supposes the pressure to take place by *actual contact*. I say, this is my explanation *for the present*. The fact that finite sums are replaced by integrals introduces other considerations, on which we will not dwell now. The pressure which MM. Poisson and Cauchy trace to molecular action, is a pressure much of the same kind as this. These authors refer it to its *cause*, M. Mossotti estimates its *effect*. Now it appeared to M. Mossotti, and it does so to myself, that this effect cannot be treated as a *force*; for it does not depend on the relative positions of the active particles, or on the absolute amount of their proper action. It depends almost entirely on the action of other particles on the pressing one.

But if it be meant that M. Mossotti's expressions for the actions of his forces, inasmuch as they comprehend the cause, must give also the effect of pressure, we conceive that a simple illustration will clearly show the contrary. Imagine a single particle of matter to exist with a single particle of æther by its side; this will be a system of equilibrium; but if we estimate the force on the particle of matter, we shall find it to be one of attraction towards the particle of æther. How then is the equilibrium preserved? It appeared to M. Mossotti that the *pressure* of the particles sustained it; and this pressure (if it exists) is not in any legitimate sense of the word a *force*.

On the second set of equations, I shall say but little. The subject is one of great difficulty, and requires ample development. I am afraid, too, I do not understand rightly what your Correspondent's objection amounts to, in saying "the conclusion is obvious, and coincides with that which we deduced from the consideration of (2.)." Are we to understand that $\frac{d\varepsilon}{dx}$ and $\frac{dp}{dx}$ and $\frac{de}{dx}$ ought to be 0, according to M. Mossotti's views? If so, I am assuredly at issue with your Correspondent. But most probably something else is meant which does not strike one who thinks differently from the writer.

That I may not occupy much of your space in a matter not universally interesting, I will conclude with stating, that the real difficulty in the way of a molecular theory is our want of power to sum expressions in Finite Differences. To convert them into definite integrals is to omit that part of their value which is the most important. Should any of your readers desire to examine the subject, I would recommend them to consult, in addition to M. Mossotti's papers in the Scientific Memoirs, and my memoir in the Transactions of the Cambridge Philosophical Society, some of the following, which are amongst the most valuable contributions to this branch of theoretical physics:—Laplace, *Mécanique Céleste*, liv. 12. ch. 2., particularly art. 3; Poisson, *Journal de l'Ecole Polytech.*, cap. 20; and *Mém. de l'Institut*, vol. viii. and ix.; Cauchy, *Exercices*, vol. iii., particularly p. 226; *Avogadro Fisica de' Corpi ponderabili*, vol. i. p. 159.

I have the honour to be,
Dear Sir, yours very truly,
P. KELLAND.

IV. *On the Rotation of a rigid Body round a fixed Point.* *By* JAMES BOOTH, *Esq., M.A., Principal of, and Professor of Mathematics in Bristol College.*

[Continued from vol. xix. p. 441.]

XX. TO determine the position of the body in terms of the time.

It is clear that the indefinitely small portion $o\,s$, of the line m parallel to the tangent at Q[*] to the section of the ellipsoid, made by the plane of u and m, may be considered as the differential of the arc of the spherical conic, traced out by the vertex of u during the motion: calling the differential of the curve $\frac{ds}{dt}$, and recollecting that $o\,v : u :: \mathrm{G} : \mathrm{K}$; see (XI.)[†] and putting for $o\,v$ its value $\frac{ds}{dt}$, we find $\frac{ds}{dt} = \frac{u\,\mathrm{G}}{\mathrm{K}}$; then

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[*] See fig. in art. XI.

[†] The property of the Ellipsoid given in Number XI., first appeared in an essay by Prof. MacCullagh, on the Wave Theory of Light, published in in vol. xvi. of the Transactions of the Royal Irish Academy.

substituting for G and K their values given by (11.) and (7.), there results the equation

$$d\,t = \frac{1}{f}\,\frac{d\,s}{\tan\theta}, \text{ or } t = \frac{1}{f}\int \cot\theta\,.\,\frac{d\,s}{d\,z}\,d\,z, \ldots \quad (15.)$$

changing the independent variable from s to z.

We have now to express $\tan\theta$ and $\frac{d\,s}{d\,z}$, in terms of z the variable coordinate parallel to the real axe of the cone. For this purpose assuming the equations of the ellipsoid and sphere, by which the spherical conic the locus of the vertex of the axe of the impressed moment is determined,

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1, \text{ and } x^2 + y^2 + z^2 = u^2 \quad \ldots . \quad (16.)$$

Differentiating these equations, we find

$$\frac{d\,x}{d\,z} = \frac{z}{x}\,\frac{a^2}{c^2}\,\frac{(b^2-c^2)}{(a^2-b^2)}; \quad \frac{d\,y}{d\,z} = -\,\frac{z}{y}\,\frac{b^2}{c^2}\,\frac{(a^2-c^2)}{(a^2-b^2)} \quad \ldots \quad (17.)$$

Substituting these values of the differential coefficients $\frac{d\,x}{d\,z}$, $\frac{d\,y}{d\,z}$, in the known expression for the differential of an arc of double curvature, $\frac{d\,s^2}{d\,z^2} = 1 + \left(\frac{d\,x}{d\,z}\right)^2 + \left(\frac{d\,y}{d\,z}\right)^2$, and then eliminating from the resulting equation x^2 and y^2, by the help of equations (16.), we find

$$\left(\frac{d\,s}{d\,z}\right)^2 = \frac{u^2\,(a^2-c^2)(b^2-c^2)\,z^2 - c^4\,(a^2-u^2)(b^2-u^2)}{[(b^2-c^2)z^2-(b^2-u^2)c^2]\,[(a^2-u^2)\,c^2-(a^2-c^2)z^2]} \quad (18.)$$

Again, to determine $\tan\theta$ in terms of z.

As $\tan^2\theta = \frac{u^2-\mathrm{P}^2}{\mathrm{P}^2} = u^2\left\{\frac{x^2}{a^4} + \frac{y^2}{b^4} + \frac{z^2}{c^4}\right\} - 1$, we find, eliminating from this expression x^2 and y^2, by the aid of equations (16.),

$$\cot^2\theta = \frac{a^2\,b^2\,c^4}{(a^2-c^2)\,(b^2-c^2)\,u^2\,z^2 - c^4\,(a^2-u^2)\,(b^2-u^2)}. \quad \ldots \quad (19.)$$

Substituting these values of $\frac{d\,s}{d\,z}$ and of $\cot\theta$ in (15.), we obtain

$$d\,t = \frac{a\,b\,c^2}{f}\,\frac{\pm\,dz}{\sqrt{(b^2-c^2)\,z^2-(b^2-u^2)\,c^2}\,\sqrt{(a^2-u^2)c^2-(a^2-c^2)\,z^2}}\text{*}; \quad (20.)$$

* Eliminating from (20.) the quantities $a, b, c, u, z, f,$ by the help of equations (4.), (7.), (8.), and introducing the relation $h = n^3 f^2$, given in the preceding note, we obtain

$$d\,t = \frac{\pm\,\sqrt{\mathrm{A\,B}}\,.\,\mathrm{C}\,d\,r}{[(\mathrm{B}-\mathrm{C})\,\mathrm{C}\,r^2-(\mathrm{B}\,h-k^2)]^{\frac{1}{2}}\,[(\mathrm{A}\,h-k^2)-(\mathrm{A}-\mathrm{C})\,\mathrm{C}\,r^2]^{\frac{1}{2}}}$$

the equation given by Poisson, *Traité de Mécanique*, tom. ii. p. 140.

the sign + or − being taken according as z augments or diminishes.

Now this expression is an elliptic function of the first order, which may be reduced to the usual form by assuming

$$z^2 = \frac{(a^2-u^2)(b^2-u^2)\,c^2}{(a^2-u^2)\,(b^2-c^2)-(a^2-b^2)\,(u^2-c^2)\sin^2\phi} \dots (21.)$$

Substituting the value of z derived from this equation in (20.) we find

$$t = \frac{a\,b\,c}{f\sqrt{(a^2-u^2)\,(b^2-c^2)}}\int\frac{d\,\phi}{\sqrt{1-\dfrac{(a^2-b^2)(u^2-c^2)}{(a^2-u^2)(b^2-c^2)}\sin^2\phi}} \,.(22.)$$

Put $M^2 = \dfrac{(a^2-b^2)\,(u^2-c^2)}{(a^2-u^2)\,(b^2-c^2)}$; then the modulus M varies from 0 to 1, as u varies from c to b, the limits of u.

XXI. It is easy to show that the maximum and minimum values of z^2, are $c^2\,\dfrac{(a^2-u^2)}{a^2-c^2}$ and $c^2\,\dfrac{(b^2-u^2)}{(b^2-c^2)}$, which expressions are the squares of the vertical ordinates of the extremities of a quadrant of the spherical conic, and the corresponding values of ϕ are $\frac{\pi}{2}$ and 0; hence the time in which the vertex of the axis u of the impressed moment describes a quadrant of the spherical conic is given by a complete elliptic function of the first order. Calling this time T, we find

$$T = \frac{a\,b\,c}{f\sqrt{(a^2-u^2)\,(b^2-c^2)}}\int_0^{\frac{\pi}{2}}\frac{d\,\phi}{\sqrt{1-M^2\sin^2\phi}}; \dots (23.)$$

hence we may express z in terms of t, let $z = F\,(t)$. Substituting this value of z, in the simultaneous equations (16.) we obtain the equations

$$x = F'\,(t), \quad y = F''\,(t), \quad z = F\,(t). \quad \dots (24.)$$

By the help of the last of these three equations we can determine the angle which the plane of the principal axes x, y makes with the axis u of the impressed moment at the end of the time t; and from the two former we find the angle which the projection of u, on the plane of $x\,y$, then makes with the principal axis x; for the tangent of this angle $= \dfrac{F''\,(t)}{F'\,(t)}$.

XXII. We must now, in order to the complete determination of the position of the body at the end of the time t, determine the angle which the intersection of the plane containing the principal axes, (round which the moments of inertia are A and B) with the plane of the impressed moment, makes with a fixed line drawn in this plane at the end of the time t.

Let this angle be ψ. Now it is easily shown that this angle is made up of two distinct parts, one arising from the successive positions which the plane of $x\,y$, or of the moments of inertia A, B, assumes while enveloping a series of instantaneous right cones, whose semiangles γ, are given by the formula $\cos\gamma = \frac{z}{u} = \frac{F(t)}{u}$, the other arising from the uniform rotation of the body round the axis u, with the constant angular velocity ϖ (10.). Let the angle produced by the first cause be χ, then

$$\frac{d\,\psi}{d\,t} = \frac{d\,\chi}{d\,t} - \varpi; \quad \ldots\ldots\ldots\ldots \quad (25.)$$

a little consideration will show that χ and ϖ are always of opposite signs.

Now the differential of the angle on the plane of $x\,y$, of which $\frac{d\,\chi}{d\,t}$ is the projection, is $\frac{d\,\chi}{d\,t}$ sec γ; or putting for sec γ its value $\frac{u}{z}$, it becomes $\frac{d\,\chi}{d\,t}\cdot\frac{u}{z}$.

The differential of the area described by the projection of u on the plane of $x\,y$ in the time $d\,t$, is $(x^2+y^2)\left(\frac{d\,\chi}{d\,t}\right)\frac{u}{z}$; but it is also $\left(\frac{y\,d\,x}{d\,t} - \frac{x\,d\,y}{d\,t}\right)$: equating these values of the differential of the area, eliminating x, y, $\frac{d\,x}{d\,t}$, $\frac{d\,y}{d\,t}$ by the help of equations (16.) (17.), we obtain

$$\frac{d\,\chi}{d\,t} = \frac{f}{u}\;\frac{(u^2-c^2)}{c^2}\;\frac{z^2}{u^2-z^2}, \text{ as } u^2-z^2 = x^2+y^2.$$

Hence as $\varpi = \frac{f}{u}$, by (10.): we get

$$\frac{d\,\psi}{d\,t} = \varpi\left\{\frac{u^2-c^2}{c^2}\cdot\frac{z^2}{u^2-z^2} - 1\right\}.$$

Now it will be found that the part within the brackets is essentially negative; hence changing the sign, the formula becomes

$$-\frac{d\,\psi}{d\,t} = \varpi\left\{1-\left(\frac{u^2-c^2}{c^2}\right)\frac{z^2}{u^2-z^2}\right\} \quad \ldots \quad (26.)$$

Integrating this equation,

$$-\psi = \varpi t - \left(\frac{u^2-c^2}{c^2}\right)\int\frac{z^2\,dt}{u^2-z^2};$$

or putting for z and t their values given by (21.) (22.), we find the angle ψ expressed by two elliptic functions, one of the first, the other of the third order:

$$\left.\begin{aligned} -\psi = {} & \frac{a\,b\,c}{u\sqrt{(a^2-u^2)(b^2-c^2)}}\int\frac{d\phi}{\sqrt{1-\text{M}^2\sin^2\phi}} \\ & -\frac{a\,c}{b\,u}\,\frac{(b^2-u^2)}{\sqrt{(a^2-u^2)\,(b^2-c^2)}}\times \\ & \int\frac{1}{\left\{1-\dfrac{u^2}{b^2}\,\dfrac{(a^2-b^2)}{(a^2-u^2)}\sin^2\phi\right\}}\;\frac{d\phi}{\sqrt{1-\text{M}^2\sin^2\phi}} \end{aligned}\right\} (27.)$$

Put $\text{N} = -\dfrac{u^2}{b^2}\,\dfrac{(a^2-b^2)}{(a^2-u^2)}$.

N is the parameter of the elliptic function of the third order, and it lies between -1 and $-\text{M}^2$.

Let $\alpha = (1+\text{N})\left(1+\dfrac{\text{M}^2}{\text{N}}\right)$ . . . . (28.)

Substituting for N and M their values, we find

$$\alpha = \frac{a^2\,c^2\,(b^2-u^2)^2}{b^2\,u^2\,(a^2-u^2)\,(b^2-c^2)},$$ which is essentially positive.

Now the parameter of the elliptic function of the third order, whether it shall be *circular* or *logarithmic*, depends on the sign of α, being circular when α is positive, logarithmic when negative; hence in this case the parameter is circular. Introducing the conventional symbols for denoting elliptic functions, we obtain

$$-\psi = \frac{b^2\sqrt{\alpha}}{b^2-u^2}\,\text{F}_{\text{M}}(\phi) - \sqrt{\alpha}\,\Pi_{\text{M}}(\text{N},\phi). \quad . \; . \; . \; . \qquad (29.)$$

When the time is a multiple of that in which a quadrant of the spherical conic is described by the vertex of u, the elliptic functions in (29.) become complete, but a complete elliptic function of the third order may be represented by elliptic functions of the first and second orders; hence ψ in this case may be found by the help of elliptic functions of the first and second orders.

XXIII. In the particular case, where the semidiameter u is equal to b, the mean semiaxis of the ellipsoid, the functions by which the time t and the angle ψ are determined, become, the one logarithmic, the other circular; as may be thus shown:

In the general equation (20.) let $u = b$, then we find

$$dt = \frac{abc^2}{f} \frac{dz}{z\sqrt{b^2-c^2}\,.\,\sqrt{c^2(a^2-b^2)-(a^2-c^2)z^2}} \quad \ldots \quad (30.)$$

Now let $(a^2-c^2)z^2 = c^2(a^2-b^2)\sin^2\phi$, ... (31.) where ϕ, as may be shown, is the angle between u and the mean axe of the ellipsoid, measured on a circular section of the surface; by this transformation, equation (30.) is changed into

$$dt = \frac{abc}{f\sqrt{(a^2-b^2)(b^2-c^2)}} \frac{d\phi}{\sin\phi}, \quad \ldots\ldots \quad (32.)$$

$$\text{or, } t = \frac{abc}{f\sqrt{(a^2-b^2)(b^2-c)^2}} \log\ \tan\frac{\phi}{2} + \text{constant} \quad .\ . \quad (33.)$$

Hence if the axis u is found in one of the circular sections of the ellipsoid, at the commencement of the motion, the plane of the principal section of this surface containing the semi-axes a and c, will indefinitely approach to, yet never actually coincide with, the plane of the impressed moment.

XXIV. The angle ψ in this case may be determined thus: In the integral of (26.), namely,

$$-\psi = \varpi t - \left(\frac{u^2-c^2}{c^2}\right)\int \frac{z^2\,dt}{u^2-z^2};$$

putting b for u, and for dt and z their values given by (32.), (31.), the last equation is transformed into

$$-\psi = \varpi t + \frac{\varpi b}{f} \tan^{-1}\left\{\frac{c\sqrt{a^2-b^2}}{a\sqrt{b^2-c^2}}\cos\phi\right\} + \text{constant} \quad (34.)$$

XXV. When two of the principal moments of inertia are equal, as A = B, the ellipsoid becomes a spheroid of revolution, and the time with the angle ψ are determined by circular arcs.

XXVI. *The axis of the rotatory motion caused by the centrifugal forces, lies in the plane of the impressed moment.*

The cosines of the angles which the tangent at the vertex of u, to the conic section whose semiaxes are u and m, (see fig. in art. XI.) makes with the axes of coordinates are $\frac{dx}{ds}$, $\frac{dy}{ds}$, $\frac{dz}{ds}$; hence these differential coefficients represent the cosines of the angles, which m parallel to u makes with the same axes.

Let ω' be the angular velocity round this axe, p', q', r' its components round the axes of coordinates; then as the angular velocity round any principal axe is equal to the impressed moment resolved perpendicularly to this axe divided by the corresponding moment of inertia, we find

$$p' = \frac{\mathrm{G}\dfrac{dx}{ds}}{\mathrm{A}} = \frac{f^2 \tan\theta \,.\, \dfrac{dx}{ds}}{a^2} \text{ from (3.) and (4.).}$$

Now by (15.), $\dfrac{dx}{dt} = \dfrac{dx}{ds} f \tan\theta$, hence $p' = f\dfrac{\dfrac{dx}{dt}}{a^2}$.

$$\text{In like manner, } q' = f\frac{\dfrac{dy}{dt}}{b^2}, \quad r' = f\frac{\dfrac{dz}{dt}}{c^2}. \quad . \quad . \quad . \qquad (35.)$$

But the cosines of the angles which the instantaneous axis of rotation due to the centrifugal force makes with the axes of coordinates are $\dfrac{p'}{\omega'}$, $\dfrac{q'}{\omega'}$, $\dfrac{r'}{\omega'}$; and the cosines of the angles which u makes with the same axes being $\dfrac{x}{u}$, $\dfrac{y}{u}$, $\dfrac{z}{u}$, we shall have for the cosine of θ' the angle between the axis of the impressed moment, and the axis of rotation due to the centrifugal moment, the formula

$$\cos\theta' = \frac{f}{u}\left\{\frac{x}{a^2}\,\frac{dx}{dt} + \frac{y}{b^2}\,\frac{dy}{dt} + \frac{z}{c^2}\,\frac{dz}{dt}\right\}.$$

Now the part within the brackets is the differential of the equation of the ellipsoid, and therefore $= 0$; hence

$$\cos\theta' = 0, \quad \text{or } \theta' = 90.$$

XXVII. To find the component of the angular velocity due to the centrifugal force, resolved along the axis of instantaneous rotation.

Let δ be the angle between the axes of the rotation caused by the impressed moment and the centrifugal force, then

$$\cos\delta = \frac{p'}{\omega'}\,.\,\frac{\mathrm{P}x}{a^2} + \frac{q'}{\omega'}\,\frac{\mathrm{P}y}{b^2} + \frac{r'}{\omega'}\,\frac{\mathrm{P}z}{c^2} \text{ (see art. X.);}$$

or putting for p', q', r' their values given by (35.),

$$\omega' \cos\delta = \mathrm{P} f\left\{\frac{x}{a^4}\,\frac{dx}{dt} + \frac{y}{b^4}\,\frac{dy}{dt} + \frac{z}{c^4}\,\frac{dz}{dt}\right\}.$$

Now the part within the brackets is equivalent to

$\frac{1}{2}\frac{d}{d\,t}\cdot\frac{1}{\mathrm{P}^2}$. Hence $\omega' \cos\delta = -f\frac{d\,\mathrm{P}}{\mathrm{P}^2}$; but as $\omega = \frac{f}{\mathrm{P}}$

$\frac{d\,\omega}{d\,t} = -f\frac{d\,\mathrm{P}}{\mathrm{P}^2}$.

We shall have $\omega' \cos\delta = \frac{d\,\omega}{d\,t}$. . . . . (36.)

There are many curious properties of rotatory motion, a few of which only are subjoined, our paper having already much exceeded its limits.

XXVIII. Let segments equal to l, measured from the centre, be assumed on the three principal axes of rotation, *the sum of the areas described by the projections of these lines on the plane of the impressed moment, varies as the time.*

Let S_c be the area described by the projection of a portion of the axis of c, equal to l, on the plane of the impressed moment; then the projection of l on this plane is $l \sin\gamma$, and the differential of the area

$$\frac{d\,\mathrm{S}_c}{d\,t} = l^2 \sin^2\gamma\,\frac{d\,\psi}{d\,t}; \quad . \; . \; . \; . \; . \; . \quad (37.)$$

or substituting for $\sin\gamma$ and $\frac{d\,\psi}{d\,t}$, their values given by (8.) and (26.), we find

$$\frac{d\,\mathrm{S}_c}{d\,t} = l^2\,\varpi\left\{1-\frac{z^2}{c^2}\right\}. \quad . \; . \; . \; . \; . \; . \quad (38.)$$

In like manner,

$$\frac{d\,\mathrm{S}_a}{d\,t} = l^2\,\varpi\left\{1-\frac{x^2}{a^2}\right\}, \quad \frac{d\,\mathrm{S}_b}{d\,t} = l^2\,\varpi\left\{1-\frac{y^2}{b^2}\right\}.$$

Adding these equations together and integrating,

$$\mathrm{S}_a + \mathrm{S}_b + \mathrm{S}_c = 2\,l^2\,\varpi\,t + \text{constant}. \quad . \; . \; . \quad (39.)$$

XXIX. Should the portions l, instead of being equal, be proportional to the square roots of the moments of inertia round the corresponding axes, the sum of the areas described by the projections of these lines on the plane of the impressed moment still varies as the time.

In (38.) let $l^2 = \frac{\mathrm{C}}{m^3} = \frac{n^3}{m^3}c^2$, where m is a constant right line, and equation (38.) is changed into

$$\frac{d\,\mathrm{S}_c}{d\,t} = \frac{\varpi\,n^3}{m^3}\left(c^2-z^2\right) \quad . \; . \; . \; . \; . \; . \; . \; . \quad (40.)$$

Similarly, the two following equations become

$$\frac{d\,S_a}{d\,t} = \varpi\,\frac{n^3}{m^3}\,(a^2-x^2),\quad \frac{d\,S_b}{d\,t} = \frac{\varpi\,n^3}{m^3}\,(b^2-y^2);$$

hence adding and integrating,

$$S_a+S_b+S_c = \frac{\varpi\,n^3}{m^3}\left\{a^2+b^2+c^2-u^2\right\} t + \text{constant.} \quad (41.)$$

XXX. Let tangent planes be drawn to the three vertices of a, b, c, the semiaxes of the ellipsoid, cutting off from the axis of the impressed moment, three segments whose reciprocals are ξ, υ, ζ, we shall have $\xi^2+\upsilon^2+\zeta^2 =$ constant, during the motion,

for $\xi^2 a^2 = \cos^2\alpha$; but $\cos^2\alpha = \frac{x^2}{u^2}$, hence $\xi^2 u^2 = \frac{x^2}{a^2}$.

Similarly, $\upsilon^2 u^2 = \frac{y^2}{b^2}$, $\zeta^2 u^2 = \frac{z^2}{c^2}$; hence

$$\xi^2+\upsilon^2+\zeta^2 = \frac{1}{u^2}.$$

XXXI. *The sum of the squares of the distances of the vertices of the three semiaxes of the ellipsoid from the plane of the impressed moment divided by the corresponding moments of inertia is constant during the motion.*

Let x_1 be the distance of the vertex of a from the plane of the impressed moment, then $x_1{}^2 = a^2\cos^2\alpha$, and $A = n^3 a^2$;

hence $\frac{x_1{}^2}{A} = \frac{\cos^2\alpha}{n^3}$.

In like manner,

$$\frac{y_1{}^2}{B} = \frac{\cos^2\beta}{n^3},\quad \frac{z_1{}^2}{C} = \frac{\cos^2\gamma}{n^3};$$

hence

$$\frac{x_1{}^2}{A} + \frac{y_1{}^2}{B} + \frac{z_1{}^2}{C} = \frac{1}{n^3}.$$

[To be continued.]

V. *On a Voltaic Process for Etching Daguerreotype Plates. By* W. R. Grove, *Esq., M.A., F.R.S., Professor of Experimental Philosophy in the London Institution**.

DR. BERRES of Vienna was the first, I believe, who published a process for etching Daguerreotypes: his method was to cover the plates with a solution of gum-arabic, and then to immerse them in nitric acid of a certain strength.

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* From the Proceedings of the London Electrical Society, Part II.; having been read before the Society on the 17th of August, 1841. Revised by the Author.

I have not seen any plates thus prepared, but the few experiments which I have made with nitric acid, have given me a burred and imperfect outline; and I have experienced extreme difficulty of manipulation from the circumstance of the acid never attacking the plate uniformly and simultaneously. My object, however, in this communication, is not to find fault with a process which I have never perhaps fairly tried or seen tried by experienced hands, and the inventor of which deserves the gratitude of all interested in physical science; but to make public another, which possesses the advantage of extreme simplicity, which any one, however unskilled in chemical manipulation, may practise with success, and which produces a perfect etching of the original image; so much so, that a plate thus etched can scarcely be distinguished from an actual Daguerreotype, preserving all the microscopic delicacy of the finest parts of the impression.

One sentence will convey the secret of this process; it is to make the Daguerreotype the *anode** of a voltaic combination, in a solution which will not of itself attack either silver or mercury, but of which, when electrolyzed, the anion will attack these metals unequally. This idea occurred to me soon after the publication of Daguerre's process; but, being then in the country, and unable to procure any plates, I allowed the matter to sleep; and other occupations prevented for some time any recurrence to it. Recently having heard much conversation as to the practicability or impracticability of Daguerreotype engraving, I became anxious to try a few experiments in pursuance of my original notion; and for this purpose applied in several quarters for Daguerreotypes; but, thanks to the exclusiveness of M. Daguerre's patent, I found that to procure a sufficient number of plates for any reasonable chance of success was quite out of the question.

On mentioning the subject to Mr. Gassiot, he with his usual energy and liberality, offered to procure me a sufficiency of Daguerreotypes; and it is owing to his zealous and valuable cooperation that I have been able to get such definite results, as appear worth publication.

Five points naturally present themselves to the consideration of the experimenter on this subject: first, the quantity, of the voltaic current; secondly, its intensity; thirdly, the distance between the anode and cathode; fourthly, the time

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* Strictly speaking this is a misapplication of Faraday's term: he applied it to the surface of the electrolyte; as, however, all continental and many English writers (among whom I may name Whewell) have applied it to the positive electrode, and as an expression is most needed for that, I have not hesitated so to apply it.

during which the process should be continued; and fifthly, the solution to be employed.

1st. With regard to the first element, or quantity, many previous experiments had convinced me, that, to give the maximum and most uniform quantitative* action of any voltaic combination, the electrodes should be of the same size as the generating plates; in other words, that the sectional area of the electrolyte should be the same throughout the whole voltaic circuit. It seems strange that this point should have been so generally overlooked as it has been: an electrician would never form a battery, one pair of plates of which were smaller than the rest; and yet the electrodes, which offering of themselves a resistance to the current, from the inoxidability of the anode, are, *à fortiori*, a restriction when of small size, have generally been formed indefinitely smaller than the generating plates; I therefore, without further experiment, applied this principle to the process about to be detailed.

2nd. *The intensity of the voltaic current.*—Here it appeared to me, that, as in the electrotype, where the visible action is at the cathode, a certain degree of intensity throws down metal as a crystal, an increased intensity as a metallic plate, and a further intensity as a pulverulent mass; that degree of intensity which would show on the negative deposit the finest impressions from the cathode, would also produce on the anode the most delicate excavations, and consequently, an intensity which would just fall short of the point of evolving oxygen from the plate to be etched, would be the most likely to succeed; this point was not, however, adopted without careful experiment, the more so, as in one instance Mr. Gassiot succeeded in procuring a very fair etching with a series of ten pairs of the nitric acid battery; however, the results of repeated experiments, in which the intensity has been varied from a series of sixteen pairs to one of the nitric acid battery, were strongly in favour of the above idea, and consequently, went to prove that one pair gives the most efficient degree of intensity for the purpose required.

3rd. *The distance between the plates.*—As it was proved by De la Rive, that in an electrolytic solution, when the electrodes are at a distance, the action extends a little beyond the parallel lines which would join the bounds of the electrodes, and thus, that the current as it were diverges and converges, it appeared advisable to approximate the electrodes as nearly

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* I say quantitative action; for, where great intensity is required, as in decomposing alkalis, &c., it may be advisable to narrow the electrodes, so as to present a smaller surface for the reaction of the liberated elements.

as possible, so as to produce uniformity of action over the whole plate. Provided a solution be used which does not evolve gas at the cathode, I am inclined to think that the plates may be with advantage indefinitely approximated; but as this was not the case with the solution I selected for the greater number of experiments, 0·2 of an inch was fixed on as the distance, in order that the gas evolved from the cathode should not adhere to the anode, and thus interfere with the action.

4th. *Time of continuing the operation.*—This was a matter only to be decided by experiment, and must vary for the voltaic combination and solution employed. With a single pair of the nitric acid battery, from twenty-five to thirty seconds was, after a great number of experiments, fixed on as the proper time; and as the plate may at any period be removed from the solution and examined, the first experiment should never exceed twenty-five seconds, when, if not complete, the plate may be again subjected to electrolysis.

5th. *The solution to be employed.*—Here a vast field was open, and still is open to future experimentalists. Admitting the usual explanation of the Daguerreotype, which supposes the light parts to be mercury, and the dark silver, the object was to procure a solution which would attack one of these, and leave the other untouched. If one could be found to attack the silver and not the mercury, so much the better; as this would give a positive engraving, or one with the lights and shadows, as in nature; while the converse would give a negative one. Unfortunately, silver and mercury are nearly allied in their electrical relations. I made several experiments with pure silver and mercury, used as the anode of a voltaic combination; but found, that any solution which would act on one, acted also on the other. All then that could be expected, was a difference of action. With the Daguerreotype plates I have used the following:—

Dilute sulphuric acid, dilute hydrochloric acid, solution of sulphate of copper, of potash, and of acetate of lead. The object of using acetate of lead, was the following:—With this solution, peroxide of lead is precipitated upon the anode; and, this substance being insoluble in nitric acid, it was hoped that the pure silver parts of the plate, being more closely invested with a stratum of peroxide than the mercurialized portions, these latter would, when immersed in this menstruum, be attacked, and thus furnish a negative etching. I was also not altogether without hopes of some curious effects, from the colour of the thin films thus thrown down; here, however, I was disappointed: the colours succeeded each other much as in the steel plate used for the metallochrome; but with inferior lustre. On immersion in nitric acid of different degrees of dilution, the plates were unequally attacked, and the etching burred and imperfect. Of the other solutions, hydrochloric acid was, after many experiments, fixed on as decidedly the best: indeed, this I expected, from the strong affinity of chlorine for silver.

I will now describe the manipulation which has been employed by Mr. Gassiot and myself, in the laboratory of the London Institution, with very uniform success. A wooden frame is prepared, having two grooves at 0·2 of an inch distance, into which can be slid the plate to be etched, and a plate of platinum of the same size. To ensure a ready and equable evolution of hydrogen, this latter is platinized after Mr. Smee's method; for, if the hydrogen adhere to any part of the cathode, the opposite portions of the anode are proportionably less acted on. The back and edges of the Daguerreotype are varnished with a solution of shell-lac, which is scraped off one edge to admit of metallic connexion being established. The wooden frame with its two plates, is now fitted into a vessel of glass or porcelain, filled with a solution of two measures hydrochloric acid, and one distilled water (sp. gr. 1·1); and two stout platinum wires, proceeding from a single pair of the nitric acid battery, are made to touch the edges of the plates, while the assistant counts the time; this, as before stated, should not exceed thirty seconds. When the plate is removed from the acid, it should be well rinsed with distilled water; and will now (if the metal be homogeneous) present a beautiful sienna-coloured drawing of the original design, produced by a film of the oxychloride formed;—it is then placed in an open dish containing a very weak solution of ammonia, and the surface gently rubbed with very soft cotton, until all the deposit is dissolved; as soon as this is effected, it should be instantly removed, plunged into distilled water, and carefully dried. The process is now complete, and a perfect etching of the original design will be observed; this, when printed from, gives a positive picture, or one which has its lights and shadows as in nature; and which is, in this respect, more correct than the original Daguerreotype as the sides are not inverted; printing can therefore be directly read, and in portraits thus taken, the right and left sides of the face are in their proper position. There is, however, *ex necessitate rei*, this difficulty, with respect to prints from Daguerreotypes,—if the plates be etched to a depth sufficient to produce a very distinct impression, some of the finer lines of the original must inevitably run into each other, and thus the chief beauty of these exquisite images be destroyed. If, on the other hand, the process be only continued long enough to leave an exact etching of the original design, which can be done to the minutest perfection, the very cleaning of the plate by the printer destroys its beauty; and, the molecules of the printing ink being larger than the depth of the etchings, an imperfect impression is produced. For this reason it appeared to me, that at present, the most important part of this process is the means it offers of multiplying indefinitely Daguerreotypes, by means of the electrotype. An ordinary Daguerreotype, it is known, will, when electrotyped, leave a faint impression; but in so doing it is entirely destroyed; and this impression cannot be perpetuated; but one thus etched at the voltaic anode, will admit of any number of copies being taken from it. To give an idea of the perfect accuracy of these, I may mention, that in one I have taken, on which is a sign-board measuring on the electrotype plate 0·1 by 0·06 of an inch, five lines of inscription can, with the microscope, be distinctly read. The great advantages of the voltaic over the chemical process of etching, appear to me to be the following:—

1st. By the former, an indefinite variety of menstrua may be used; thus, solutions of acids, alkalies, salts, more especially the haloid class, sulphurets, cyanurets, in fact, any element which may be evolved by electrolysis, may be made to act upon the plate.

2nd. The action is generalized; and local voltaic currents are avoided.

3rd. The time of operation can be accurately determined; and any required depth of etching produced.

4th. The process can be stopped at any period, and again renewed if desirable.

The time I have given is calculated for experiments made with one pair of the nitric acid battery; it is, however, by no means necessary that this be employed, as probably any other form of voltaic combination may be efficient. It would seem more advisable to employ a diaphragm battery, or one which produces a constant current, as otherwise the time cannot be accurately determined. It is very necessary that the silver of plates subjected to this process be homogeneous. Striæ, imperceptible in the original Daguerreotype, are instantly brought out by the action of the nascent anion; probably silver, formed by voltaic precipitation, would be found the most advantageous. I transmit with this paper some specimens of the prints of the etched plates, and of electrotypes taken from them; and in conclusion, would call attention to the remarkable instance which these offer, of the effects of the imponderable upon the ponderable: thus, instead of a plate being inscribed, as "drawn by Landseer, and engraved by Cousins," it would be "drawn by Light, and engraved by Electricity!"

London Institution, Saturday, Aug. 14, 1841.

[With this communication were sent plates etched by the process detailed in the text; electrotype copies from the same; and a considerable number of prints obtained from the former.—SEC. ELECT. SOC.]

Postscript by the Author, Nov. 1.—Few of the readers of the Philosophical Magazine will have an opportunity of seeing any specimens of the process, and as the etching is not deep enough to produce impressions sufficient to accompany the paper, I may give an idea of them by saying that in the print of a portrait which I have now before me the whole expression of the features is distinct, the pupil of the eye and the speck of light upon it clearly defined, the gloss of the hair and of the satin stock very accurate. The microscopic details alone appear incapable of transference to *paper*, but these, as stated above, being *absolutely perfect* upon the etched plate, I had intended to have directed some experiments to the substitution of more delicate materials than paper and printing-ink for receiving the impressions; incessant occupations have prevented me, and will I fear for some time.

I would suggest the employment of hyposulphite of soda instead of ammonia to remove the oxychloride.—W. R. G.

VI. *Additional Remarks on the alleged Conversion of Carbon into Silicon. By* J. DENHAM SMITH, *Esq. and* R. H. BRETT, *Ph. D.*

To Richard Phillips, Esq., F.R.S.

DEAR SIR,

IN Dr. Brown's observations, p. 389, pres. vol., he states, "For the next experiments, on the ferrocyanide of potassium, let such as are interested in the subject judge whether they be sufficient repetitions of mine. I may mention, in passing, that the two apparatus described in this part of your Correspondents' letter, viz. an iron tube closed with an iron plug, and a gun-barrel protected by luting and well secured at the orifice, were the very instruments that foiled me last November and December. There must be free enough exit for nitrogen," &c. Now we may observe, that the iron tube with an iron plug was the instrument chiefly employed by us for the purpose of heating ferrocyanide of potassium and carbonate of potash together; nor did we employ this instrument on one occasion only, but at least half-a-dozen times. The plug only loosely went into the orifice, and the luting was a thin layer of china clay, more for the purpose of preventing dirt, &c. getting into the tube than for anything else; it appears, therefore, to us out of the question to talk about there not being free enough exit for nitrogen; besides, we find in the original memoir, p. 231 of the Transactions, the following statement:—"A little crucible of Berlin porcelain was filled with paracyanogen, and after the lid had been *tightly luted on, it* was imbedded in stucco paste within a larger crucible. The gypsum having set and dried, the apparatus was kept at a white heat for an hour and a half; the residue of this process resembled the last, only that it was denser and darker." Now in this experiment Dr. Brown asserts, that the transformation was effected; but will any candid observer maintain, that greater opportunity for *free exit of nitrogen* obtained in this experiment than in our experiments with the iron tube and plug? Again, at p. 286 of the Edinburgh Transactions, we are informed that a siliciuret of copper was obtained when bicyanide of mercury was heated for more than an hour to whiteness in a *double copper tube*: if reference be made to p. 174 of the Transactions, a description of this double copper tube will be found, and it is there stated that the open ends of these tubes terminate, the one in a male *screw*, the other in a female *screw*: and the section in which this tubular apparatus is described closes with the following remark, of considerable importance, we imagine, as regards the subject immediately under discussion:—"The latter," speaking of one of the tubes, "thrice as long as the former, is charged, and the two are firmly screwed together, so as to give *very great but not fixed pressure, for the screws always permit gaseous passage in such circumstances.*"

We must therefore be allowed to think, that the objection raised by Dr. Brown to our employment of an iron tube and plug, and gun-barrel, is answered by himself, seeing that in his own recorded experiments the *alleged* transformation of carbon into silicon was brought about in tubes, and in a crucible so contrived, that no greater, and we very much doubt if the same amount of freedom for *gaseous passage*, was allowed as in the instruments employed by us.

It is therefore incumbent upon Dr. Brown to look for other reasons to account for our failure: he will not say we did not hit upon the right temperature, for in certain experiments the temperature at which we failed was the same as that at which he succeeded. Will Dr. Brown still continue to maintain that the alleged transformation, when the materials operated upon are ferrocyanide of potassium and carbonate of potash, and the instrument an iron tube and plug, is so difficult of performance that a person may work for six weeks in continual failure? if so, we are totally unable to reconcile such an opinion with one or two statements, in very decided language, to be met with near the close of the original memoir, in p. 243 of the Transactions: the author there speaks of the production of crystallized *siliciuret of iron* from the action of heat upon a mixture of ferrocyanide of potassium and cyanide of potassium. "In one operation," says he, "I procured half an ounce of these little eight-sided crystals, and have worked with glass, porcelain, black-lead, iron and platinum vessels with equal success." He continues, "This is a difficult process, however, and one must be content to make several trials before a fine product be obtained; but although it is not easy to produce a perfect specimen, *it is the simplest thing in the world to satisfy oneself of the change which is effected*; *nay, it is impossible to make cyanide of potassium by the common process without performing this transformation*." And further on we have the following somewhat remarkable statement:—"I have at length come to the conclusion, that the production of *silicon from paracyanogen* has been performed by *every one who has decomposed the ferrocyanide of potassium by heat*, in order to procure the cyanide of the same metal." How comes it, we would ask, if it be the simplest thing in the world to satisfy oneself of the change which is effected, that in all our experiments with ferrocyanide of potassium and carbonate of potash we failed to obtain indications of the presence of silica as the result of a transformative action, but nevertheless obtained abundance of *cyanide of potassium*? nay, in one of our experiments, p. 803 of Phil. Mag., where ferrocyanide of potassium was heated alone, we altogether failed in effecting the alleged transformation; yet Dr. Brown has come to the conclusion, "that the production of silicon from paracyanogen has been performed by every one who has decomposed the ferrocyanide of potassium by heat."

But although we have entirely failed in bringing about this transformation, under circumstances, when, according to Dr. Brown, the thing has been done over and over again not only in this but in every country, where *cyanide of potassium has been obtained by decomposing the ferrocyanide of potassium by heat*; still he not only effects this alleged conversion, but even, as it appeared to us, proves too much, viz. he gets more silica in one experiment than the carbon, supposing it all to be transformed into silicon and the latter silicified, could yield. In p. 244 of the Transactions we find the following statement:

" 3·04 grs. of silicic acid were extracted from 5 grs. of paracyanide of iron." The term extraction here, we presume, has reference to the conversion of the carbon of the ferruginous compound into silicon, and subsequently into silicic acid by the action of carbonate of potash. Dr. Brown states, at p. 240 of the Transactions, " That the substance in question, viz. paracyanide of iron, contains nitrogen, carbon and iron, in the ratios of 1, 2 and 1." We suppose these numbers represent the atoms of the respective substances; 5 grs. of paracyanide of iron would therefore contain 1·111 of carbon, for 54 : 12 : : 5 : 1·111. Now taking the atom of silicon at one-third of Berzelius's atom, in order to make silicic acid an equi-atomic compound of silicon and oxygen, we shall have 22·22 ÷ 3 = 7·406, and 7·406 : 15·406 : : 1·111 : 2·3; so that all the silica which could have been procured by the transformation of the *whole* of the carbon in 5 grs. of paracyanide of iron, would have amounted to 2·3 grs. instead of 3·04 grs., as stated in the original memoir. We do not know whether the excess of silica in this case was obtained by a transformative force exercised upon iron or nitrogen, or both; but we cannot conceive that the carbon could have furnished it; if so, Dr. Brown has even surpassed the transformative power of nature. In another experiment, however, of Dr. Brown's, the quantity of silicic acid falls amazingly short of the calculated proportions, supposing the alleged conversion to have been full and perfect. Thus it is stated at p. 245 of the Transactions, that 5·4 grs. of silicic acid were obtained from 30 grs. of ferrocyanide of potassium: now supposing the ferrocyanide to have been anhydrous and the conversion of all its carbon effected, there ought to have been obtained 12·06 grs. of silicic acid; for the quantity of carbon in 30 grs. of anhydrous ferrocyanide of potassium is 5·8 grs., therefore 7·406 : 15·406 : : 5·8 : 12·06. Doubtless Dr. Brown considers the transformation in the experiment last referred to as being very far from full and perfect; and we confess we are quite astonished that he should have obtained as much silicic acid as stated in his memoir. If the alleged conversion were admitted, of course, *quoad* carbon alone, for sake of argument,as possible, it appears somewhat remarkable that two experiments similar in *principle* should have been so discordant in result; and we cannot but think, that asserting that 3·04 grs of one substance can be procured from a given weight of a second substance only capable of yielding 2·3 grs., is very like a " *plenary mistake.*"

The experiment on ferrocyanide of potassium, in p. 301 of the Phil. Mag., October, is alleged to be " original on the part of Mr. Smith." This ill agrees with the statement in pp. 240–1 of the Transactions, where, after directing the paracyanide of iron to be inclosed in a luted crucible and stucco powder, and then "the fiercest heat of a powerful wind furnace to be applied for two hours," Dr. Brown states, that "it is evident that the ferrocyanide may be substituted for the paracyanide in this formula, the cyanide being sublimed away by the heat of the furnace." Ferrocyanide of potassium is obviously meant here, as he speaks of the sublimation of the cyanide. In the same paragraph he continues, "In one crucible, of the capacity of an ounce and weighing 500 grs., one operation produced 165 grs. of the semicrystalline siliciuret of iron, and the third repetition raised the weight above that of the crucible itself:" yet forsooth the experiment just alluded to "is entirely original" on our part, whereas it really is an exact repetition of his own process, even to the capacity of the Berlin crucible employed.

The tangible objections made by Dr. Brown to our experiments apply, first, to the paracyanogen employed by us, and, secondly, to the apparatus. Armed with the first objection, he endeavours to destroy the force of our most decisive experiments, by declaring, that it "renders almost all their experiments of no value," and this because our paracyanogen was obtained from the decomposition of an aqueous solution of hydrocyanic acid, instead of the ignition of bicyanide of mercury; and from this argues, that if our paracyanogen "were not prepared in exactly the same way as my own," our results cannot challenge the correctness of his statements, and therefore conveniently avoids making any observations on the majority, and the most important of, our experiments. Now seriously, it appears to us that this author would have been equally justified in objecting to the nitric or hydrochloric acids employed by us, supposing that we had chosen to obtain the former from nitrate of barytes, and the latter from chloride of calcium, instead of using these acids obtained from the same sources in all probability as those employed by Dr. Brown,—nitrate of potash and chloride of sodium. What can it signify from what source we obtained paracyanogen, if, as we state, it possessed all the known properties of paracyanogen? In fact, Dr. Brown himself admits that we employed paracyanogen, though he terms it a "low hydrate" of that substance, since it was obtained from an aqueous solution of hydrocyanic acid. Now supposing that it is this low hydrate of paracyanogen so vastly insisted upon by Dr. Brown in the "Observations,"—which is likely to be the purer preparation? paracyanogen combined with a very small quantity of water, or this substance prepared by heat from bicyanide of mercury? which, according to Dr. Brown, contains "a third of its own weight of condensed cyanogen," is "hygrometric," which, "when thus prepared, may have suffered the loss of some of its nitrogen," may contain metallic mercury, "half charred cyanide of mercury, paracyanide of mercury," and for aught we know, a variety of other compounds; besides, at the temperature to which this so-called hydrate of paracyanogen was exposed by us in our various experiments, the water must have been driven off before any action could have taken place between it and the carbonate of potash, &c.; and this we find to be the case, for when the paracyanogen we principally employed is exposed to heat, water is first given off in small quantities, and then cyanogen. To put this beyond dispute, 3 grs. of this paracyanogen were heated over a spirit-lamp until no aqueous vapour was evolved, but always kept considerably below redness; it was then transferred to a small tube apparatus of German glass, well cleaned and warmed, and containing ·27 cub. inch of atmospheric air, in which 6 grs. of potassium were placed; on the application of heat, vivid ignition ensued in the tube, and air was driven over, which, collected over mercury, when examined for hydrogen, afforded no indications of the presence of that gas; nor was the apparatus itself in the least perceptible degree clouded by aqueous vapour; cyanide of potassium was formed.—(J. D. S.)

Now as 1 gr. of water would yield about 5 cubic inches of hydrogen, and as there was no increase in the volume of air contained in the tube, nor moisture deposited, we think we are justified in contending, that at the temperatures at which the transformative action is alleged to take place, the paracyanogen used by us was at least as pure paracyanogen as that employed by Dr. Brown. In its general properties it fully agrees with the description of the properties of paracyanogen in pp. 166–7 of the Transactions. We therefore deny the right of Dr. Brown to avoid all notice of our numerous satisfactory and decisive experiments made with this substance, because it was "not prepared in exactly the same way" as that which he used, asserting that our paracyanogen was as truly paracyanogen as any employed by him, even according to his own showing.

He then proceeds to consider the experiment made with paracyanogen obtained from bicyanide of mercury, remarking, that he has no "sufficient evidence" to show that the preliminary process was rightly performed; yet the sole difference in this process from those described by him, was the substitution of a well-luted and covered Berlin crucible, inclosed in another covered and luted crucible, for the glass, iron and copper tubes employed by him,—a variation surely not of a nature to affect the product: then comes the remark, "that this process is so difficult of performance," which, as we have before shown, was not even distantly alluded to in the original memoirs, and which we have therefore a right to consider an *ex post facto* objection, and as such inadmissible by us: this is followed by the volunteer of the opinion, that "I do not think Mr. Smith employed the same material as I used in this experiment." Perhaps not, but at any rate it was a substance prepared, as before noticed, with a slight, and, apparently to us, most trivial variation of apparatus, in the mode prescribed by Dr. Brown, and possessing the properties assigned by him to paracyanogen.

To gratify Dr. Brown, and "to make assurance doubly sure," we prepared some paracyanogen by inclosing bicyanide of mercury in a tube of German glass, drawing it out to a capillary, placing this in an iron tube with a plug, and exposing the apparatus to a heat visible only in the dark, for a considerable time over a coal fire. Dr. Brown directs a "coke fire" to be employed; we trust this accidental departure from "the very letter of the text" will not entirely vitiate the result and "render the experiment of no value," for we did not notice it until the experiment was terminated: the paracyanogen thus obtained resembled that described by Dr. Brown as employed by him, and possessed the acknowledged properties of paracyanogen; 5 grs. of it mixed with 20 grs. of carbonate of potash and ignited, in a closed platinum crucible placed in an exterior covered and luted one, and all the precautions taken directed in p. 236 of the Transactions, at a yellow heat, which appears to be Dr. Brown's white heat, when withdrawn from the fire afforded a white saline residue, which examined in the ordinary and well-known way for silica, afforded *none*. —(J. D. S.)

This experiment was exactly repeated, only substituting what Dr. Brown terms *pure* paracyanogen (precipitated from the sulphuric acid solution of the common product), *i. e.* that obtained from the bicyanide of mercury, for the paracyanogen previously employed, and in this case also *no trace of silicic acid was obtained.*—(J. D. S.)

The objections raised against our experiments on the alleged formation of metallic siliciurets, are the paracyanogen employed, and that they "were not performed according to my direction." The first has already been disposed of, the second is wholly uncalled for; the difference being, that we substituted a tube made by rolling up platinum foil for the crucibles or shreds of platinum foil mentioned in pp. 237-8 of the Transactions; and that a copper tube and paracyanogen, and copper in a minute state of division and paracyanogen were employed, instead of "a copper tube packed with bicyanide of mercury," which salt, according to Dr. Brown's own showing, must be converted into mercury and paracyanogen before any reaction could take place between the latter substance and the copper; so that this objection must also fall to the ground, unless this discoverer chooses to contend that our non-success was owing to our employing a tube made by rolling up thin sheet copper, whilst he used a cast copper one.

In the "Observations," Dr. Brown remarks, "Even so late as last November and December I tried to effect the alleged transformation before a celebrated physician and chemist six weeks in vain; but at last succeeded, or, as I must now say, appeared to succeed." As we before observed, we do not find in the original memoir a single recorded case of failure, or even a distant hint of the slightest difficulty being experienced in effecting the alleged transformation, which we are now informed was tried in vain for six weeks before a celebrated physician and chemist: may we in our turn be allowed to express our "respectful conviction" that such repeated unsuccessful attempts, say an average of one experiment per diem, should have rendered both the experimenter and the celebrated physician and chemist extremely cautious in recognizing that experiment, attended with success, as the true and proper result; and might have recalled to their recollection the unfortunate result of the laborious experiments, sometimes accompanied with apparent successful results, respecting the identity of palladium and an amalgam of mercury and platina? This unsuccessful attempt to effect the alleged conversion is not only not alluded to in the original papers, but the very reverse obtains; there "simplicity," "freedom from any intelligible source of fallacy," "infallible and easy of execution," are the terms applied to the various experiments. As to the six weeks' trial, we scarce know which to sympathize with most, the experimenter doomed for that period to constant failure and repeated disappointment, or the celebrated personage, whose patience, which is evidently great, was put to so severe a test.

We have now stated all that we conceive to be necessary in reply to Dr. Brown's observations in the last Number of the Philosophical Magazine; we trust that our remarks, both at present and in our former communication, have never violated the rules of courteous disputation, for, in common with Dr. Brown, we feel that truth is most likely to be arrived at by calmness in discussion. Time, which always does so much both in fixing scientific facts on a firm and solid basis, as well as in dissipating errors which will inevitably creep into experimental research, will, it is hoped, ere long settle this at present, perhaps, *quæstio vexata*. If Dr. Brown be right, then chemistry must undergo a revolution; our present notions must suffer a complete change in respect to all we understand concerning elementary bodies; and we feel assured, that we shall not be severely condemned by chemical philosophers, for expressing strong doubts as to the correctness of experiments which are more startling in their results than any which are recorded in the annals of chemical science, especially as those doubts are borne out by our own experience.

We shall refrain, Sir, from again addressing you on this subject, at least for the present; we feel that we have not judged hastily, nor without good grounds for forming an opinion. For our own parts, we are satisfied of the erroneous nature of Dr. Brown's views; but should the results of other experimenters serve to confirm them, should further experiments be made on the subject, we will reconsider our judgement, and should we have reason to believe ourselves in error, we will without hesitation acknowledge the truth of that which we at present firmly believe to be "a plenary mistake."

J. DENHAM SMITH.
Liverpool, Nov. 16, 1841. R. H. BRETT.

P.S. Through the kindness of Professor Johnston our attention has been directed to a paragraph in his paper on paracyanogen in the Edinburgh Philosophical Transactions, which completely confirms our views as to the constitution of the black powder produced by the decomposition of aqueous hydrocyanic acid, after it has been exposed to an elevated temperature, which proves that the composition of the substance is then identical with cyanogen, in short, that it is *paracyanogen*. We subjoin the paragraph, vol. xiv. p.33:—"When strong prussic acid is set aside it speedily decomposes and deposits a black powder in considerable quantity. Dried *in vacuo* over sulphuric acid, or at 212° Fahr., this substance still gives off, when heated in close vessels over a lamp, water, carbonic and hydrocyanic acids, and ammonia. The black matter that remains, burned with bichromate of potash in large excess, gave a mixture of carbonic acid and nitrogen in the proportion of 2 to 1."

VII. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. Francis *and* H. Croft.

[Continued from vol. xix. p. 452.]

On Citric Acid.

WACKENRODER has attempted to show that there is only one hydrate of citric acid, the formula of which is $C^{12} H^{16} O^{14}$, and which is the common acid, whose crystals have been measured by Brooke. Marchand has made some experiments on this subject; he found that the common acid of commerce contains 34·45 per cent. carbon, and 4·86 per cent. hydrogen. This acid was then crystallized at a temperature not exceeding 60° C. The crystals had the same form as described by Brooke, and, after having been pressed and dried between bibulous paper, were found to contain 34·72 per cent. carbon, and 4·69 hydrogen. The formula for this acid is exactly $C^{12} H^{20} O^{16}$. When dried *in vacuo* over sulphuric acid the crystals lose two atoms, or 8·5 per cent. of water, which is exactly the quantity that it loses when heated, according to Berzelius; the acid so obtained agrees with Wackenroder's formula, viz. $C^{12} H^{16} O^{14}$.

If a concentrated solution of citric acid be evaporated at a temperature of 100° C. until a crystalline pellicle is formed on the surface and then allowed to cool, the first crop of crystals consists of small, scarcely determinable individuals, which, according to Gustav Rose, are quite different in form from those described by Brooke. Afterwards the first compound crystallizes out of the mother liquor. This hydrate has the formula $C^{12} H^{16} O^{14}$. Berzelius heated it with oxide of lead, and found its formula to be $C^{12} H^{18} O^{5}$, for it lost 14 per cent. water; but it is evident that he had converted the acid $C^{12} H^{16} O^{14}$ into $C^{12} H^{10} O^{11}$.—*Annalen der Chemie und Pharmacie*, vol. xxxviii. p. 346.

On the Compounds of Bichloride of Tin with the Alkaline Chlorides.

Dr. Bolley has examined these salts. The bichloride was prepared by passing chlorine into a solution of the protochloride of tin. By mixing together dilute solutions of bichloride of tin and sal-ammoniac and slowly evaporating, a salt is obtained which crystallizes in small regular octahedrons, sometimes combined with the hexahedron. They are not changed by exposure to the air, and contain no water. Formula $Sn\,Cl^4 + N^2 H^8 Cl^2$. By boiling a dilute solution the whole of the oxide of tin is precipitated: this fact explains its value as a mordant; it is known to the calico-printer under the name of pink salt. The potassium salt is similar in form to the preceding; when heated, it leaves chloride of potassium and oxide of tin. Its formula is $K\,Cl^2,\ Sn\,Cl^4$. The sodium compound is very soluble in water, effloresces in warm air; when heated to 100° C. smells of the bichloride of tin, and loses from 12 to 13 per cent. water.—*Annalen der Chemie und Pharmacie*, vol. xxxix. p. 100.

On the Compounds of the Chloride, Iodide and Cyanide of Palladium with Ammonia.

Fehling has examined these salts; the palladium was determined by carefully heating them. The nitrogen and hydrogen were determined by combustion with oxide of copper, &c. On adding excess of ammonia to a cold, not too dilute, solution of chloride of palladium, a red precipitate is formed; this salt was known to Vauquelin. Its formula is $Pd\,Cl^2 + N^2\,H^6$. By boiling water it is dissolved and decomposed; there is a small brown residue which contains the same elements as the red compound, but less chlorine; the other product is a yellow salt, identical in composition with the red one, which turns yellow when heated to 200° C. A similar yellow salt is precipitated by acids from the solution which has been filtered from the red salt, and also from a solution of the red salt in aqueous ammonia. Both the red and the yellow salts dissolve in nitric acid with a brown colour, and give on evaporation a brownish red, very soluble mass, from whose solution ammonia always throws down the red compound. They both dissolve in ammonia, but with different degrees of rapidity. By evaporating the ammoniacal solution and continually adding ammonia, a white salt is obtained. Formula $Pd\,Cl^2 + 2\,N^2\,H^6 + H^2\,O$. Both the yellow and white salts absorb ammonia, the yellow more quickly; the above white salt is thereby produced: this compound is soluble in water, gives with acids the yellow salt, when heated loses one atom of ammonia and one of water.

Iodide of palladium dissolves easily in ammonia; and on the addition of hydrochloric acid a reddish-yellow precipitate is formed, which, by boiling with water, is converted into a red crystalline mass, having just the same formula as the yellow salt, viz. $Pd\,I^2 + N^2\,H^6$. A white compound, $Pd\,I^2 + 2\,N^2\,H^6$, may be obtained in exactly the same manner as the analogous chlorine compound.

Cyanide of palladium dissolves in ammonia when heat is applied; on cooling, the compound, $Pd\,Cy^2 + N^2\,H^6$, crystallizes out in the form of needles. This body does not absorb moist ammonia, &c.—*Annalen der Chemie und Pharmacie*, vol. xxxix.

Anthranilic Acid.

Liebig has given a short notice on the method of preparing this acid, and on some of its properties; he found Fritzsche's statement of its decomposition perfectly correct. It is best to mix the acid with twice its weight of coarsely powdered glass. He found the composition the same as Fritzsche.—*Annalen der Chem. und Pharmacie*, vol. xxxix.

Indigo.

Dumas has published a paper on this interesting body, and some of its compounds: when the indigo-blue is prepared by means of sulphate of iron and lime, and precipitation by an acid, it frequently contains sulphur, from which it may be freed by treatment with sulphuret of carbon. Dumas gives the formula as $C^{16}\ H^{10}\ N^2\ O^2$; he does not agree with Erdmann. Very carefully prepared reduced indigo gave, on analysis, the formula $C^{16}\ H^{12}\ N^2\ O^2$. Dumas therefore compares it to oil of bitter almonds.

Indigo-sulphuric Acid.—If we wish intirely to prevent the formation of the purple acid, we must use not eight but fifteen parts of concentrated sulphuric acid to one of pure indigo-blue, and allow the whole to digest in a bottle for several days at a temperature between 50° and 60° C. The formula of the potash salt is $C^{16}\ H^8\ O,\ 2\ S\ O^3 + K\ O$. The baryta salt has a precisely similar formula.

Purpuro-sulphuric Acid.—This is formed when eight or ten parts of sulphuric acid are used; it is put on a filter and washed with dilute hydrochloric acid. Its formula is $C^{32}\ H^{20}\ N^4\ O^4,\ 2\ S\ O^3$.

Indigotic Acid.—Its formula was found to be $C^{14}\ H^{10}\ N^2\ O^{10}$; the silver salt, $C^{14}\ H^8\ N^2\ O^{10} + Ag\ O$, &c.

Picrinnitric Acid.—Formula $C^{18}\ H^6\ N^6\ O^{14}$; the silver salt is $C^{12}\ H^4\ N^6\ O^{13} + Ag\ O$: the potash salt contains one atom of water.—*Ann. de Chim. et de Phys.*, trois. ser. ii. p. 204.

Marchand has published researches on the above acid and its salts; he finds for the free acid the formula $C^{12}\ H^6\ N^6\ O^{14}$, both when prepared from indigo and from salicin. He finds the potash salt to be anhydrous. The baryta, strontia and lime salts contain five atoms of water of crystallization; there are basic salts of baryta and strontia which contain two atoms of base. The magnesia salt seems also to contain five atoms of water. [An abstract of the conclusion of this paper in our next.]

Chlorisatinic Acid, Chlorisatyd, &c.

Erdmann has published a third treatise on Indigo, in which he corrects some of his previous formulæ. Laurent had commenced researches upon indigo, mention of which is made in the *Comptes Rendus*, tom. xii. p. 539, wherein he corrects some of Erdmann's formulæ. Erdmann found the correct formula for chlorisatin to be $C^{16} H^{8} N^{2} Cl^{2} O^{4}$. Chlorisatinic acid must therefore also be different. Chlorisatyd is $C^{16} H^{10} N^{2} Cl^{2} O^{4}$. By the action of hydrosulphuric acid on chlorisatin a white insoluble body is formed, sulpho-chlorisatin; its formula is $C^{16} H^{10} N^{2} Cl^{2} S^{5}$. Erdmann formerly stated that bichlorisatin contained the same number of atoms of hydrogen as chlorisatin; it appears, however, from his later experiments, which he made on account of Laurent's statements, that this is not the case; the formula is, according to Laurent, $C^{16} H^{6} N^{2} Cl^{4} O^{4}$. [This is the reason why in this abstract I have not called these bodies chloride and bichloride of isatin.—H. C.]. The formula of bichlorisatyd is $C^{16} H^{8} N^{2} Cl^{4} O^{4}$.

By the action of dilute chromic acid on indigo-blue, a very curious body, isatin, has been discovered both by Laurent and Erdmann; it crystallizes in large dark red crystals. Isatin is but little soluble in cold water, but more so in hot, with which it forms dark reddish yellow solution. When heated a small part sublimes, but the greater portion is decomposed. Formula $C^{16} H^{10} N^{2} O^{4}$, that is, indigo plus two atoms of hydrogen. When treated with chlorine it is converted into a mixture of chlorisatin and bichlorisatin: it dissolves in caustic potassa with a dark purple colour. Hydrochloric acid precipitates from the solution a reddish yellow crystalline powder, which appears to be unchanged isatin; if however the solution be heated or allowed to stand for some time, it becomes bright yellow, and gives on evaporation a crystallized potash salt soluble in alcohol: this salt contains isatinic acid. Isatin takes up an atom of water to form this acid. The silver salt is partly soluble; the solution of the salt cannot be heated without decomposition. Formula $C^{16} H^{12} N^{2} O^{5}, Ag O$. When a solution of isatinate of potassa is treated in the cold with hydrochloric acid no change ensues, but when warmed pure isatin is precipitated.

Isatin dissolves completely in hydrosulphuret of ammonium when heat is applied: on cooling, a white or yellowish somewhat crystalline powder is precipitated, which does not contain any sulphur: this Erdmann calls isatyd. Formula $C^{16} H^{12} N^{2} O^{3}$. Isatin reacts therefore quite differently from chlorisatin. Isatyd is scarcely soluble in water, gives a crystalline salt when dissolved in potash by the aid of heat.

By the action of hydrosulphuric acid on an alcoholic solution of isatin, a new body, sulphisatin, is formed; its formula is not yet fully determined.—Erdmann, *Journal für Prakt. Chem.*, vol. xxiv. p. 1; Laurent, *Comptes Rendus*, vol. xii. p. 539.

Phenyl Compounds.

Laurent has prepared a new volatile crystallizable body from coal-tar, whose formula is $C^{12} H^{10} O + 2$ aq, and which he names hydrate of phenyl. With potassium it gives a crystalline compound, $C^{12} H^{10} O + K O$, and combines directly with baryta without giving off water. With sulphuric acid it gives the phenosulphuric acid, the baryta salt of which is $C^{12} H^{10} O, H^2 O, S O^3 + Ba O, S O^3$. This salt, when distilled, gives hydrate of phenyl. The phenosulphate of ammonia, when treated with nitric acid, yields sulphuric acid, water ammonia and picrinnitric acid. Chlorine gives with hydrate of phenyl the well-known chlorophenesic acid, $C^{12} H^6 Cl^4 O +$aq, and afterwards the chlorophenisic acid, $C^{12} H^4 Cl^6 O +$ aq. With bromine an analogous bromophenisic acid is obtained. Nitric acid gives at first nitrophenesic (or pheneso-nitric) acid, which is yellow, crystallizable, forms red and yellow salts, and has the formula $C^{12} H^6 N^4 O^9$, 3 aq; and then it produces nitrophenisic (phenisonitric) acid, by which name the author now represents picrinnitric acid, $= C^{12} H^4 N^6 O^{13}$, aq. When phenesonitric acid is treated with bromine a yellow crystalline acid is obtained, which forms beautiful salts. Formula $C^{12} H^4 Br^2 N^4 O^9$, aq, and which is converted by the continued action of nitric acid into picrinnitric acid. By treating chlorophenisic acid with nitric acid, chloride of phenyl is obtained in golden yellow scales, $C^8 H^4 Cl^4 O$; the bromide cannot be obtained in this manner. If the chlorophenisic acid is prepared by acting on coal-tar with chlorine, chloride of albin is formed at the same time; it is a volatile body, crystallizes in white needles, is not altered by potash. Formula $C^{12} H^{12} Cl^4$. Phenesonitric, picrinnitric, chlorophenisic and bromophenisic acids are isomorphous, and moreover the potash and ammonia salts of phenisonitric (picrinnitric) acid.—*Comptes Rendus*, xii., *and Pharmac. Centralblatt.*, No. 38.

Oil of Esdragon.

It has been already stated in our reports, that Laurent had examined this oil; the impure oil is $C^{32} H^{40} O^3$. It is converted by nitric acid, into three new acids, which are either wholly or partially volatile and crystallizable:—Draconic acid, $C^{32} H^{26} O^{10} + 2$ aq.; nitro-draconnassic acid (draconassonitric), $C^{32} H^{24} N^2 O^{14}$, 2 aq; and nitrodraconessic (draconessonitric) acid, $C^{32} H^{22} N^4 O^{18}$ 2 aq. The atoms of water in each can be replaced by bases. The oil forms with chlorine a body, the formula of which is $C^{32} H^{30} O^3 Cl^{14}$, which, when treated with potassa, forms chloride of potassium and chloride of draconyl $= C^{32} H^{28} Cl^{12} O^3$.—*Comptes Rendus*, xii. p. 764.

Oil of Caraway (Oleum Carvi.)

Völckel has published some analyses of this oil (*Annal. der Pharm.*, vol. xxxv. p. 308); he found that by distillation it may be separated into several oils of different boiling points and different composition, of which the most volatile is probably a hydrocarbon. Schweitzer has instituted a fuller examination of the oil, and confirms Völckel's statements; he finds that it is partially decomposed by distillation. The hydrocarbon which first passes over has the composition $C^5 H^8$; he calls it carven. When oil of caraway is mixed with hydrate of potassa it becomes brown-coloured, and yields on distillation an oil which, by repeated cohobation, is entirely different from the original oil of caraway. This oil is the true carven; the above-mentioned is probably carven mixed with some other body. It boils at 173° C., smells something like oil of anise; it probably pre-exists in the oil of caraway. The residue from the above operation consists of a resin and the brown alkaline solution; this gives, on saturation with sulphuric acid, a thick brown oil which may be purified by distillation with water; this oil, which is of a very sharp biting character, he calls carvacrol; its formula is $C^{40} H^{56} O^3$. Two resins are formed at the same time, which are however of minor importance. The best method of obtaining both carven and carvacrol is to distil oil of caraway with hydrated phosphoric acid in a long-necked flask; the carven distils over, and the carvacrol flows back.

Carven is colourless, lighter than water, difficultly soluble in water, easily in alcohol and æther, oxidizes in the air, and forms a resin. It is also converted into resins by sulphuric and nitric acids. It forms with hydrochloric acid gas a camphor-like substance, soluble in alcohol, crystallizes in white scales, easily soluble in water; the solution is decomposed by warming; melts at 50·5°. Heated with lime it forms chloride of calcium, water and carven. Formula $C^{10} H^{18} Cl^2$, or $C^{10} H^{16} + H^2 Cl^2$. Carven combines with chlorine; the compounds have not yet been fully examined.

Carvacrol is colourless, similar in consistence to olive oil, has a biting taste, boils at 232°, does not possess acid properties.

Oil of caraway heated with potassium forms a brown mass, out of which water separates carvacrol. [These researches are to be continued.]—*Journal für Praktische Chemie*, vol. xxiv. p. 257.

Cedar Oil.

Walter has shown that the crystallized oil of peppermint belongs to a class of bodies analogous to camphor; he has found the same to be the case with cedar oil. The impure solid oil begins to boil between 100° and 150°, water passes over. The solid oil may then be distilled at 275°, the greater portion passes over at 282° C. It may be still further purified by crystallization from alcohol. It smells aromatic; melts at 74° C.; little soluble in water; formula $C^{32} H^{52} O^{2}$. Density of vapour 8·4; calculated 8·1; its composition may therefore be represented by $C^{32} H^{48} + H^{4} O^{2}$. By repeated distillations with anhydrous phosphoric acid a yellowish oily fluid is obtained, which smells like the solid oil; Walter calls it cedren. Formula is $C^{32} H^{48}$. Density of vapour 7·9, calculated 7·5. Cedren boils at 248° C. By pressing the common cedar oil a liquid oil may be obtained, which boils between 264° and 268° C. It has the same composition as cedren. Specific gravity 0·98.—*Annalen de Chim. et de Phys.*, Avril 1841, p. 498.

[To be continued.]

VIII. *On the Magnetic Influence of the Lunar Spectrum, in relation to a new Theory of Terrestrial Magnetism.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE to request the favour of your insertion of the accompanying paper "On the Magnetic Influence of the Lunar Spectrum." I conceive it of sufficient importance to attract the attention of natural philosophers at home and abroad, and I trust that its perusal will, in accordance with the author's wishes, induce further investigation of the peculiar qualities of *indirect*, or *reflected* light, in relation to the moon-beam, as distinct from the *calorific* qualities which accompany the direct light of the sun, as well as from the gravitating power of the moon.

It is clear that the establishment by multiplied experiments of the *assumed* fact, that the light of the sun reflected from another heavenly body, while unaccompanied by those heating, and possibly, gravitating properties by which it is attended in the solar beam, is capable of causing peculiar magnetic phænomena on the earth's surface, must lead to a vast change in the opinions at present entertained on the laws which control the development of magnetic and electrical influences.

So far as the author's experiments have been carried, they appear, not only from his memoir, but also from his personal explanations to myself, to have been conducted with care, and to bear out to a great extent, his preconceived opinions; but in a question of such importance it becomes matter of much anxiety to the originator of new views, to find that the results of experiments, from which in part those views have been deduced, should be corroborated by some of those eminent men who have devoted their lives to scientific pursuits, —after rigid, and, I would hope, *impartial* investigation.

I am, Gentlemen, yours faithfully,
F. W. De Moleyns, M.A.

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1. Having lately made some interesting observations upon the *magnetic influence* of the *lunar* spectrum, which may tend to the establishment of a new theory of magnetism, I venture to submit them to the many natural philosophers who read the Philosophical Magazine, in the hope that they may lead to other inquiries upon the same subject.

2. The hypothesis of Dr. Morichini, that the *blue* and *violet* rays of the solar spectrum were capable of imparting *magnetic* qualities to needles which were exposed to their influence, has been contested by several practical physicists, who have failed in obtaining the desired result when they adopted his method of making the experiment. Being desirous of satisfying myself by personal observation upon this question, I placed a sewing needle with care upon the surface of a cup of water, and found that it developed a polarity without any preliminary exposure to the prismatic rays of the sun, by arranging itself in the plane of the *magnetic* meridian *with its thinnest part or point towards the north, and its head or thickest part towards the south.*

3. The experiment was frequently repeated with different needles, and the result was always the same, unless the head of the needle was more than usually thin; the inference being, that the polarity of the needle was governed by a law which made the end having the least bulk a north pole. It is probable that Morichini was not aware of the fact, that a polarity existed in a needle independently of any such magnetizing influence, and that he supposed its ordinary polarity to have been produced by his exposing it to the refracted rays of the sun.

4. But such an exposure occasioned an important change in its magnetic properties; for having removed it from the surface of the water, and placed it on a table in the blue rays, where it remained for about five minutes, I found that when it was again made to float upon the water, it assumed a polarity opposite to that above mentioned, with *its head instead of its point* directed towards the *north magnetic* pole of the earth.

5. This experiment, repeated in various ways with different needles, always offered the same result:—1. The point was

placed in the blue rays; the head in the red rays. 2. The point was placed in the extreme violet rays; the head in the dark space beyond the violet rays. 3. The needle was placed longitudinally in the blue or violet rays. 4. It was also placed longitudinally in the red rays. The change however was effected more rapidly when the influence of the blue or violet rays acted on the needle, than when it was exposed to the red rays, but in all cases its head pointed to the north, although it had previously pointed to the south.

6. Several subsequent experiments proved that the needles so acted upon, retained their newly acquired magnetic properties, for twenty-four hours at least, after they had been submitted to the power of the refracted solar rays. But it is evident that this change of the poles was a phænomenon indicating some alteration in the relations of reciprocity between the previously existing magnetic properties of the needle and those of the earth.

7. As a series of inferential arguments had already induced the conclusion, that the *magnetizing* influence in all cases cannot be a gravitating or attractive force, but one which only interferes with, or controls that force; and that it is in some way connected with the production of light—although it may prevail where there is no illuminating process—I imagined that by making experiments upon the *lunar* instead of the solar beam, it would be possible to separate the cause of the illuminating property, from all those calorific and gravitating influences which are the accompaniments of light in the sunbeam.

8. The reflected moon-beam is the manifestation of an *occasional* and *variable* influence, acting within that region of space between the earth and the moon, which is the medium of a direct gravitating force: this force has nearly the same intensity at all times, due allowances being made for the elliptical character of the lunar orbit. But as regards the reflection of the solar light from the moon's surface, it is an indirect and borrowed influence, which is at its climax in relation to the earth when the moon is full, but altogether null when she is new.

9. It must also be remembered, that when the moon is full the most delicate experiments have not detected heat in her rays; therefore it may be demonstrated, that the astronomically *reflected* light of the sun's rays is separated from those rays in his direct beam, which evolve heat to a greater or less extent, in the portion of the earth's surface on which it shines; and this method of considering *indirect* or reflected light in relation to the moon-beam, enables us to treat it, *or its cause,*

as an influence wholly distinct from the gravitating lunar force which mainly occasions the rise and fall of the tides, as well as from those calorific qualities which accompany the *direct* light of the sun.

10. It is evident, that the phænomenon which we term light, can only be *a relation* to an animal sense of perception, or to a seeing organ in an animal body, which is affected in a peculiar manner by some reciprocal influence acting between the organ and the object perceived. But the influence of solar or lunar light, thus made manifest to one of our senses, must be attributed to some real property direct or reflected of the sun or moon; for illumination in the case of the moon-beam is evidence in favour of the existence of some force derived from the sun, which is separated from the heating and gravitating forces by the process of reflexion. Hence, supposing a magnetic influence to be the force which produces light when it acts upon our organs of vision, we are justified in the speculation that this result is *not* the *only* consequence of its prevailing in space to such an extent, as to enable us to perceive the immensely distant fixed stars.

11. It becomes, therefore, an experiment of the first importance, to examine whether the reflexion of the sun's light from another heavenly body, when it is divested of those heating and gravitating influences which always accompany it in the solar beam, is capable of occasioning peculiar magnetic phænomena on the earth's surface:—not only was I prepared for some such development, but for the manifestation of a much higher and more sublimed influence than that discovered in the above-mentioned experiments on the solar spectrum.

12. When the first prismatic observation was made with the lunar instead of the solar rays, the moon was nearly full, and the sun had been for some time below the horizon; but on a subsequent occasion it was repeated an hour before sunset, four days before full moon, with great care and accuracy; and as the interposition of glass had already been found to offer no impediment to the magnetizing influence of the solar beam, I was enabled to make my observations in a well-closed room, and to prevent any trifling currents of air from acting upon the surface of the water or from moving the floating needle.

13. The presence of day-light, as I expected, did not diminish, or in any way interfere with, the lunar influence, although it rendered the experiment more difficult, because it hindered me from seeing the prismatic image of the moon upon the table, and from determining the suitable position in which the

needle ought to be placed, in order to expose it to the most refrangible rays of the spectrum. The difficulty was only obviated by bringing the prismatic action to bear upon it, when it was floating upon water; this had been done previously in regard to the solar spectrum without occasioning any change in the polarity of the needle: its point continued to be its north pole, which might have arisen from the prevalence of the reciprocal influence between it and the magnetic meridian of the earth, when it was in that position; while there was no parallelism between its direction and that of the terrestrial magnetic currents, when it was lying on a table. The great superiority of the magnetic power of the moon, when compared with that of the sun, appeared immediately in reference to this fact, because the experiments about to be detailed were commenced *when the needle was parallel to the terrestrial magnetic currents.*

14. In order to prove that the substance of the prism itself had no magnetic properties or disturbing influence, it was brought close to the floating needle in various positions without causing the least motion, when it had adjusted itself in the plane of the magnetic meridian of the earth.

15. The following observations were then made:—

The needle, with its point directed towards the north magnetic pole of the earth, almost immediately exhibited the effects of the lunar influence; *at first its point deflected from the magnetic pole to the true north pole of the earth,* and it arranged itself in a direction parallel to that of the true meridian. Then the whole needle began to move *laterally,* but slowly, towards that part of the spectrum in which the blue and violet rays predominated. Its point, however, gradually deflected from the true meridian very soon after the commencement of this lateral movement, and the needle at last placed itself at right angles to the terrestrial meridian. When it was parallel to the earth's equator, it suddenly acquired a new and contrary impulse, and a comparatively rapid darting motion from east to west in the direction of its length, with its head foremost; as soon as its head was beyond the spectrum, it turned towards the north: this produced a new curvilinear movement of the whole needle, which brought it past its original position, when it was parallel to the terrestrial *magnetic* meridian; and there was a cessation of motion when it again became parallel to the true meridian of the earth, where it remained stationary for a short time although the prism was not moved. It must be borne in mind, that the moon was itself to the east of the meridian during the experiment, and that the prism was so held, that the blue rays of the spectrum were nearest the eastern horizon.

16. After this cessation of motion, the needle with its head directed towards the true north pole of the earth, soon began to move laterally as before; but although it went through the same varied and peculiar changes of position as it did previously, but more slowly, it exhibited a short cessation of motion when it was parallel to the terrestrial equator, which was not the case when its point deflected from the meridian in the first instance, and its darting equatorial motion was less rapid than before; but it underwent the same extent of bodily movement on the surface of the water, as to distance, before it was again parallel to the true meridian of the earth. When it attained that position, and *its point* was its north pole, *there was no pause*, as was the case when its head was so situated, for the subsequent lateral movement immediately followed.

17. After it had gone through a third series of these varied changes of position, and *its head* was again its north pole, the corresponding cessation of motion occurred. The prism was removed for a few seconds. Upon its being replaced, the head of the needle deflected from the meridian *towards the west*, and the direction of the rotation was reversed.

18. Finally, when the prism was removed permanently, the needle soon arranged itself in the magnetic meridian with that end pointing towards the north, which was on that side of the equator, when the prismatic influence ceased to operate. But the change of poles was not permanent, as had been found to be the case when the needle was acted upon by the solar spectrum; for its point gradually came round to the north if its head had been at first attracted to that position, or been left there when the prism was removed.

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19. These observations appear to corroborate a new physical theory, which has been gradually suggested by meditation upon the leading phænomena of *electrical induction*, but as the outlines of my general hypothesis do not coincide with the published opinions of several natural philosophers, for whom I entertain sincere respect, I abstain from offering them to the public until the magnetic influence of the moon has been made the subject of their investigation. My present object is to direct their attention to the above observations, and to invite them both to repeat my experiments (which are undoubtedly liable to error) and to attempt the solution of the great magnetic problem involved in this manifestation of a lunar influence upon the weak but constant polarity of a

needle which is *unmagnetized* in the *common* acceptation of the word.

20. It may be necessary to add, that the prism with which I operated was one of crown glass about four inches in length, and that it was held with both hands at the distance of about half an inch from the surface of the water, in such a position that the red rays of the spectrum were nearest the observer. R. V.

IX. *On the Connexion of Electricity with Evaporation.* By Mr. G. A. Rowell.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In a paper read before the British Association at Glasgow, I endeavoured to explain the various phænomena of rain, the aurora, and magnetism, by the hypothesis, that each particle of vapour takes with it from the earth its proportion of electricity, according to its expanded surface; that if condensed within the electrical attraction of the earth, the surcharge of electricity is withdrawn and the vapour is deposited as dew; but if it rise out of the electrical attraction of the earth, and is then condensed, the electricity, being insulated, forms an atmosphere around each particle of vapour; which surcharge of electricity not only suspends the vapour by its lightness, but also repels the neighbouring particles of vapour, and prevents the formation of rain; and on the removal (by any cause) of the electricity inclosing the vaporous particles, the repulsion is removed, and the particles of vapour then attract each other and form rain.

The discovery of the electricity of steam has strengthened my opinion, and the following experiment was made to prove that evaporation would not go on so freely from an insulated vessel as from an uninsulated one: believing the experiment is new, I respectfully submit it to the attention of the readers of your valuable Journal.

In a warm room over an oven in daily use, I suspended by silk threads two shallow vessels, eight inches and a half in diameter, containing eight ounces of water each; a small copper wire was hung from one vessel to the earth to take off the insulation, both vessels being similarly suspended in every other respect; after being suspended twenty-five hours the insulated vessel had lost two ounces eleven dwts. and fifteen grains; and the other vessel three ounces six dwts., showing an excess of evaporation from the non-insulated one of fourteen dwts. nine grains.

I have tried similar experiments with water placed in the rays of the sun, and on all occasions the evaporation has been greatest from the non-insulated vessel. There is a difficulty in obtaining correct calculations from the above experiments, as it is scarcely possible to keep up complete insulation from electricity; and the vessel of water must have its proportion of electricity when placed in an insulating situation, which will assist the evaporation for some time; but I believe if complete insulation could be obtained, and a vessel left without any electricity, that no evaporation would go on at moderate temperatures, and that evaporation at low temperature is owing to the extreme lightness, or rather no weight of electricity, buoying up the particles of water when expanded by heat.

I am, Gentlemen, yours, &c.

Oxford, Oct. 8, 1841. G. A. ROWELL.

X. *Remarks on Sir* J. W. Lubbock's "*Theory of Heat and Vapours.*" *By* JAMES IVORY, *K.H., M.A., Hon. M.R.I.A., Instit. Reg. Sc. Paris, et Reg. Sc. Götting. Corresp., and late a Fellow of the Royal Society of London*[*].

THE Theory of Heat and Vapours, published some time ago by Sir J. W. Lubbock, and reprinted in this Journal, [S. 3. vols. xvi. xvii.], requires some observations from me. It is not intended to examine minutely the whole tract, but only to make some brief remarks on the two last sections which treat of the atmosphere and the atmospheric refractions. The temperature, the density, and the pressure of a mass of air being represented by θ, ρ, p, the author assumes this equation,

$$V = C + D(1 + \alpha\theta),$$

in which V stands for the absolute heat, that is, for the sum of the sensible and latent heats, and C and D are constants. The author next adopts another equation, first given by Laplace, viz.

$$V = A + \beta \frac{p^{\frac{1}{\gamma}}}{\rho},$$

A, β and γ being constants. By equating the two values of V, we obtain

$$C + D(1 + \alpha\theta) = A + \beta \frac{p^{\frac{1}{\gamma}}}{\rho},$$

which is the fundamental equation of the Theory of Heat

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[*] Communicated by the Author.

and Vapours*. But in the case of atmospheric air, we likewise have this well-known equation,

$$p = k\rho(1 + \alpha\theta);$$

and it will easily be seen that this latter equation is inconsistent with the former one; for from it we obtain $\frac{1}{\rho} = \frac{k(1 + \alpha\theta)}{p}$, and this value being substituted in the other equation, we get

$$\frac{C - A}{kB} = (1 + \alpha\theta).\left(p^{\frac{1-\gamma}{\gamma}} - \frac{D}{kB}\right),$$

which would prove that the pressure is determined when the temperature is given, a property that certainly is not verified in the atmosphere. It is most evident, that, in atmospheric air, there can be no general relation between θ, ρ, p, except the law of Mariotte.

As the fundamental equation of the Theory of Vapours is inadmissible in the case of atmospheric air, we might at once reject all the deductions from it: but it may be worth while to add a few words in order to throw some light on the calculations which follow. Let θ', ρ', p', and θ'', ρ'', p'' be other values of θ, ρ, p; and put $\beta = \frac{1-\gamma}{\gamma}$, $E = \frac{D}{kB}$; then we get from the last equation,

$$\frac{C - A}{kB} = (1 + \alpha\theta) . (p^{\beta} - E)$$

$$\frac{C - A}{kB} = (1 + \alpha\theta') . (p'^{\beta} - E)$$

$$\frac{C - A}{kB} = (1 + \alpha\theta'') . (p''^{\beta} - E):$$

and hence

$$p'^{\beta} = \frac{1 + \alpha\theta}{1 + \alpha\theta'} \times (p^{\beta} - E) + E$$

$$p''^{\beta} = \frac{1 + \alpha\theta}{1 + \alpha\theta''} \times (p^{\beta} - E) + E,$$

equations which cannot obtain generally in the atmosphere; because, as has been said, the three equations from which they are derived are not properties of atmospheric air. But if we set aside the theoretical views of Sir J. W. Lubbock, and understand each of the two equations as merely expressing a relation between the quantities it contains, the two constants, β and E, will be determined when θ, p, θ', p', θ'', p'' are found by means of three separate observations. Sir J. W.

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* Theor., p. 2.

Lubbock makes choice of three elevations observed by Gay-Lussac in his aëronautic ascent from Paris; and he finds

$$\gamma = 1{\cdot}4910,\ \mathrm{E} = -\ 1{\cdot}1618;$$

which he changes to

$$\gamma = 1{\cdot}5,\ \mathrm{E} = -\ 1{\cdot}192.$$

With these constants, the two equations will, no doubt, verify the observed values of p', p''; but, as γ and E have been deduced from three particular observations, there is no evidence to prove that, with any other three assumed observations, the constants will, in every case, retain the same values. Thus if θ''' and p''' be new observed values, the equation

$$p'''^{\beta} = \frac{1 + \alpha\,\theta}{1 + \alpha\,\theta'} \cdot (p^{\beta} - \mathrm{E}) + \mathrm{E}$$

will not hold with the same values of γ and E, as before.

Such calculations retard knowledge by erroneous views. The author applies his equations to barometrical measurements, and to the refractions; but his processes have no foundation whatever in the constitution of the atmosphere.

We can hardly help noticing how fruitless it must be to attempt solving physical problems by means of algebraic equations containing many arbitrary constants. In such problems the constants are improperly called arbitrary: they are indeterminate, and must be found by observation or experiment; but as they are mixed with the other quantities in the equations of the problem, they are independent on the will of the calculator. In the Theory of the Moon, the coefficient of the cosine of the mean anomaly in the reciprocal of the radius vector is one of the constants, which must be found by comparing the theoretical with the observed place of the moon. Now this constant varies with the disturbing force; and if any one, not attending to this circumstance, should be rash enough to fix its value by an arbitrary definition, many errors would thereby be introduced in the expression of the moon's motion.

In Sir Humphry Davy's Chemical Philosophy this passage occurs at p. 233:—"Air analysed in different quarters of the globe, in cities and in the country, on sea and land, has been found not perceptibly different in composition: the accurate proportions of oxygen and azote are 21 and 79." Here it must be understood that air is unmixed with aqueous vapour or carbonic acid gas. The experiments of MM. Dumas and Boussingault, recorded in the *Comptes Rendus*, June 7, 1841, confirm the words of the illustrious chemist. It thus appears that the atmosphere, at different places, consists, at all elevations, of the same two gases mixed in the same proportion. This property is equivalent to the law of Mariotte, which is a consequence of it; for the elastic force in all gases being proportional to the product of the temperature, density, and a constant which varies with the nature of the gas, the law of Mariotte will hold good in a mixture of two gases, when the volumes of the constituent parts preserve constantly the same proportion. One general law of the constitution of the atmosphere is thus obtained; to which must be added the usual equation of equilibrium. By combining these two properties we deduce a relation between the temperature and pressure at the earth's surface, and the temperature, pressure, and height, at any elevation. But another property is still wanting to complete the development of the constitution of the atmosphere. We must know the law of the decrease of heat in ascending; of which we are at present entirely ignorant, except the rate of decrease at the earth's surface expressed by the height required to depress the thermometer one degree. Now, from this imperfect element I have already deduced the barometrical formula in its most improved form; and have computed a table of refractions agreeing with the observed quantities as far as they have been ascertained. My object in the above detail of the chief steps of my investigation, is to enable the unprejudiced inquirer to judge of its soundness and originality.

XI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xix. p. 547.]

June 2, 1841. "ON the Faluns of the Loire, and a comparison of their Fossils with those of the newer Tertiary Strata in the Cotentin, and on the relative age of the Faluns and Crag of Suffolk," by Charles Lyell, Esq., V.P.G.S.

In a paper "On the Crag of Norfolk and Suffolk," read in 1839*, Mr. Lyell stated that when M. Desnoyers assigned in 1825 a contemporaneous origin to the Crag and the Faluns of Touraine, he dissented from the conclusion; first, because the per-centage of recent species then assigned to the crag, including the Norwich beds, was greater than that ascribed by M. Deshayes to the shells of Touraine; 2ndly, because almost all the fossils in each locality were of distinct species, though only 300 miles apart; and 3rdly, because the fauna of the Suffolk crag had a northern, and that of Touraine an almost tropical aspect, notwithstanding the geographical proximity of the

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* Proceedings, vol. iii. p. 171. 1839 [Phil. Mag., Third Series, vol. xv. p. 407, 411.—Ed.].

two districts. In 1839, however, when he compared, with the assistance of Mr. G. Sowerby, a large collection of Touraine shells, and ascertained that the recent species amounted to 26 per cent., a nearly similar result to the one at which he had previously arrived respecting the red and coralline crag, he was induced to adopt M. Desnoyers' views. As some doubts nevertheless remained in his mind respecting the localities and true geological position of certain shells assigned to the Faluns, and as he was desirous of determining the range southwards of the organic remains of the English crag, as well as northwards of those of the Faluns, and ascertaining whether the fossils of the most northern of the Falun deposits approached nearest in character to the shells and corals of the English crag, Mr. Lyell examined in the summer of 1840, first, certain of the newer tertiary deposits in La Manche, particularly those near Valognes, and between Carentan and Coutances; then the tertiary strata in the neighbourhood of Dinan and Rennes; and afterwards those along the course of the Loire from Nantes to Tours and Blois, extending his researches northwards of that river as far as Savigné, and southwards to Bossée and Pontlevoy. The following notices contain summaries of the observations made at each locality.

CRAG.

Tertiary strata near Valognes.—The first geologist who explored the Cotentin was M. De Gerville. M. Desnoyers, in his memoir on that part of Normandy (published in 1825), shows that the newest secondary rock near Valognes is Baculite limestone*, and that it is overlaid by patches of tertiary strata, of the age of the Paris basin; but he does not allude to any deposit of more recent date. By the advice of M. De Gerville, Mr. Lyell visited a marl-pit at the farm of Cadet, near Ranville la Place, eight miles south-west of Valognes, and he found it to abound with Suffolk crag shells. He obtained twenty-nine species of Testacea, fifteen of which Mr. Searles Wood has identified distinctly with crag species, and seven doubtfully, the most abundant shell being *Lucina radula*. In M. De Gerville's collection from this locality, Mr. Lyell saw a specimen of the Falun variety of the *Voluta Lamberti*, or of what he considers to be a distinct species of Voluta. It is stated to have been found under an oyster-bed, and beneath the stratum containing the above shells.

Carentan.—At St. George de Bohon, five miles south-west of Carentan, is another deposit of Suffolk crag fossils. In travelling south from Carentan this formation is first met with at the hamlet of La Flaget. It consists of an iron-stained calcareous tufa, or an aggregate of fragments of organic remains, and is in some places thirty feet thick. The shells are difficult to extract, but Mr. Lyell obtained fourteen species; also three species of corals, and a caudal tubercle of a Raia, all of which have been identified with Suffolk crag

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* Mr. Lyell examined this limestone, and recognised its resemblance to the uppermost chalk at Faxoe in Seeland. See 'Proceedings,' vol. ii. p. 191 [Phil. Mag. Third Series, vol. vii. p. 412.], and 'Geol. Trans.,' 2nd Series, vol. v. p. 248, for an account of the Faxoe deposit.

fossils. Among the shells are numerous fragments of the large *Terebratula variabilis*. The corals and some of the Testacea are common to the Faluns of Touraine, but none of the distinguishing fossils of the latter have been discovered in the Carentan deposits.

Sainteny.—In sinking a well at this place, more than sixty feet of a white calcareous aggregate of comminuted shells were passed through. At Longueville, one and a half mile from Sainteny, is a soft calcareous stone, consisting of innumerable casts of fragments of shells, among which Mr. Lyell detected the *Pecten striatus* of the Suffolk crag; and a similar rock occurs at the farms of Blehou and Raffanville, several miles distant. The fossils obtained at these localities could not be satisfactorily determined, but Mr. Lyell is of opinion that they agree with those which are found near St. George de Bohon, except that he observed no fragments of the *Terebratula variabilis*. As far as they can be identified, they consist of Suffolk crag species, and they do not appear to possess a character intermediate between the Suffolk fauna and that of the Faluns.

Mr. Lyell saw no recurrence of this crag further south, and the most northern point at which he noticed a deposit of the age of the Faluns of Touraine was near Dinan, sixty geographical miles to the south-east of Sainteny, the intermediate country consisting of ancient strata and crystalline rocks.

FALUNS.

Dinan.—M. Desnoyers does not describe the Falun near Dinan, although he alludes to it. The neighbourhood of Dinan is entirely composed of granitic rocks; but at the village of Evran, situated near a stream which flows into the Rance, seven miles south of Dinan, is a small tertiary deposit, consisting of ten or twelve feet of white coralline and shelly sand, overlaid by a bed of stiff, reddish-brown clay, of very variable thickness. The great irregularities presented at the junction of the two strata, and the occasional projection of continuous layers of the sand into the clay, Mr. Lyell explains by supposing that the former at the time of its denudation, and previously to the deposition of the clay, possessed a certain amount of hardness, which allowed of its being undermined. At the bottom of the sand occur large oysters, different from the common Touraine species *O. virginica*; and in the same quarries Mr. Lyell found many corals, fragments of Echinodermata, sharks' teeth, ribs of the Lamantin, vertebræ of a Delphinus, and a tooth of a Mastodon. Some of the bones were buried in a solid semi-crystalline limestone, in which casts of shells are common. The formation occasionally assumes a concretionary or travertine structure: at Le Quiou it is micaceous, and splits into flags; and at the village of Pas de Hac some pinnacles of soft, white, calcareous aggregate present in the lower part fine examples of cross-stratification. At St. Juvat the variety of building-stone called *La jauge*, and composed of comminuted organic remains, resembles the deposit near Sainteny, but the occurrence of casts of cones and large Cypræ convinced Mr. Lyell that it must be assigned to the Faluns. It is overlaid by a bed of clay of variable thickness, having been very irregularly denudated; and it is penetrated by cylindrical hollows, similar to the sand-pipes in the English chalk. From these localities Mr. Lyell obtained twenty-six species of shells, one Cirriped, five species of Echinodermata, five of corals, and seven of fishes, besides the remains of Cetacea and Mammalia before mentioned. The shells are for the most part identical with species found in the Faluns of Touraine; the whole of the corals are well-known Touraine fossils; and the fishes, according to M. Agassiz, have been all found in the molasse of Switzerland, with the exception of one species, *Carcharias megalodon*. In the solid limestones of the localities above enumerated, many of the shells, which in several places in Touraine are beautifully preserved, occur only as casts.

Rennes.—The country between Dinan and Rennes consists of ancient rocks. M. Desnoyers states, in the memoir before alluded to, that tertiary beds of the age of the Paris basin and of the Faluns occur near Rennes, but Mr. Lyell is not aware of any published account of the fossils. In the ancient quarries of St. Gregoire, to which he was conducted by M. Pontallier, he found corals and casts of shells of Touraine species; also a large Spatangus, a claw of a crab, and teeth of sharks, imbedded in soft and hard limestones similar to those near Dinan. At La Chaussairie, five miles south of Rennes, occurs a perfectly distinct limestone, containing Milliolites and casts of marine shells, resembling those of the Paris basin; and associated with it are green and blue marls, enclosing freshwater Testacea. The deposit is of small extent, and rests upon transition strata; but Mr. Lyell suspects that it is in places overlaid by the ruins of the true Faluns, and that from these were derived the remains of a Lamantin and a tooth of *Carcharias megalodon*, found in the debris of a shaft sunk at La Chaussairie.

Nantes.—The district between Rennes and Nantes consists of transition and granitic rocks, but there are many detached patches of Miocene strata around Nantes. At Les Cleons is a soft coralline limestone, containing pebbles of quartz and spangles of mica, the fundamental rock of the country being mica-schist. Mr. Lyell obtained from the limestone six species of corals and five of Testacea, the whole of which, capable of determination, belong to Touraine fossils. In the museum at Nantes he saw specimens which indicate the existence of Falun strata at Le Loroux, Vieilleville and Limousinière, places within thirty miles of Nantes; also other organic remains which prove that Eocene strata occur at Cambon.

Angers.—Mr. Lyell was prevented from examining the pits north of this place, but he was presented by M. Millet with an extensive suite of shells and corals, collected by that gentleman. Of fifty-seven species of Testacea, all but thirteen occur in the Faluns near Tours, Savigné and Pontlevoy; but the fact of there being thirteen peculiar to the Angers district induces Mr. Lyell to suspect that the fossils depart more than those of other localities from the common type. The collection contains also only nine species which can be positively identified with known recent shells, and one which is doubtful, giving about seventeen per cent. of existing species, a much smaller proportion than was obtained by the author in other localities.

Doué.—At this town are extensive quarries of a calcareous building-stone, composed of comminuted shells and corals, and exposed to the depth of forty feet. The beds are horizontal, but exhibit highly inclined cross-stratification. From the marl-beds at La Grézille, and the calcareous sand and limestone of Renaudan and Illet, villages situated six or seven miles north of Doué, Mr. Lyell procured twenty-four species of corals, four of Echinodermata and three of fishes; also a few species of shells, the most conspicuous being the large *Pecten solarium.* In the great abundance of corals and Echinoderms, and the small number of Mollusks, Mr. Lyell states that this deposit presents a perfect analogy to the white or coralline crag of Suffolk; but that its fauna is as distinct, with respect to species, from the fauna of the coralline crag, as the other localities of the Faluns of the Loire generally.

Savigné.—Between Doué and Savigné the country consists partly of the Eocene freshwater formation, which extends thence almost continuously to Paris, and partly of *Craie tufeau.* Near Savigné the Falun is composed of limestone, containing most of the Doué fossils. The result of Mr. Lyell's labours in this neighbourhood gave the following amount of organic remains, obtained chiefly from a pit which he had made near the point where the road from Savigné to Channay divides from that leading to Courcelles. The total number of species of corals which have been determined amounts to eighteen, of Echinodermata to two, of Testacea to seventy-six, and of fishes to four. Mr. Lyell also obtained an upper molar of a deer, and a molar of the *Chæropotamus Cuvieri.* Of the shells, only ten species were not found by the author at other Falun localities near the Loire; and twenty-three species, or about thirty per cent., have been identified with recent shells. Among the fishes is *Lamna contortidens*, a species which occurs in the Suffolk crag. The tooth ascertained by Mr. Owen to belong to the *C. Cuvieri*, affords, Mr. Lyell states, another instance of a mammifer common to Eocene and Miocene periods.

District south of Tours.—The immediate neighbourhood of Tours consists of cretaceous valleys, with intervening platforms of Eocene freshwater strata. The Faluns occur from twelve to sixteen miles to the south, at Louans, Manthilan and Bossée. At Louans the deposit is exposed in pits from four to five yards deep, and consists of white and yellow marl, formed, to a great extent, of comminuted shells and corals. From this bed Mr. Lyell obtained 180 species of shells, many very small, and generally overlooked by collectors; the corals hitherto determined amount to only six species. Of the Testacea he procured all the species, except thirty-three, at other localities; and the recent species have been ascertained to be about forty-nine, or in the proportion of twenty-six per cent. At Bossée he obtained 129 species of Testacea, forty of which, or thirty-two per cent., have been identified with living shells; and of the entire number Mr. Lyell found all except thirteen in some of the other Faluns.

Six species of corals, and remains of Lamna and Myliobates, have been also ascertained to occur at Bossée; and a posterior molar tooth which Mr. Lyell procured there, Mr. Owen has proved to belong to the Dichobunes, a genus of Pachyderms, found likewise in the Eocene strata of France and the Isle of Wight.

Pontlevoy.—At this town, thirty miles south-east of Tours, a patch of white Falun marl rests on the Eocene freshwater formation. In the pits east of the town Mr. Lyell procured perfectly preserved shells; and fragments of the Eocene freshwater limestone are found in the Falun bored by Petricolæ, and full of their shells. The marl is usually covered by three feet of red clay, sand and mould. Mr. Lyell found here the first specimens of the shell, generally considered to be the *Voluta Lamberti* of the English crag, but which he believes to differ from it. During his researches at Pontlevoy he procured 163 species of shells, forty-five of which, or twenty-five per cent., have been identified with existing Testacea; and on comparing the whole number with a collection of 180 from Louans, 106 were found to be common to the two localities. Only thirty-four of the Pontlevoy shells were not procured by Mr. Lyell, at some other Falun locality. Not more than six species of corals have yet been ascertained to occur in this district. The other localities near Pontlevoy examined by Mr. Lyell are Sambin and Contres. At the former the white Falun, containing hard flags, is covered by a great deposit of red, ferruginous, stratified gravel, with grains of quartz and flint derived from the Eocene freshwater formation; and it bears a striking resemblance to the gravel-beds which overlie the red crag in Suffolk. Immediately east of Sambin, as well as between Contres and Soing, Mr. Lyell found specimens of the *Ostrea virginica* associated with fragments of other Testacea, which identified the deposits from which they were obtained with those of Touraine. These detached Faluns imply, he says, that a large part of France, now drained by the Loire and its tributaries, was submerged during the Miocene period, although it is only at a few isolated points that the evidence can be detected of the long time this submergence must have lasted, and of the distinctness of the fauna which then lived, both from that now existing, and still more from that of the antecedent Eocene epoch.

General Remarks.—Previously to his tour, Mr. Lyell considered that the collections which he had seen from the Loire might be divided into two groups, the larger resembling a Mediterranean or even a more northern fauna, and the smaller a tropical one; and that some of the shells composing the latter came from inferior beds of the deposit, or from patches of Falun of more ancient date than others: he also suspected, that where the tropical forms abounded, there would be found a smaller proportion of recent shells. He is, however, now convinced that all the shells belong to one group, or that the forty-four crag species were really contemporaneous in Touraine with the large cones, Cypræas, Fasciolarias, and other tropical forms of Testacea. At Bossée, where he found these large univalves, as well as the Astræa, Lunulites, and Dendrophyllia, most fully developed, he obtained the greatest proportion of recent shells, or thirty-two per cent., the average being twenty-five. In making the examinations upon which these results depend, Mr. Lyell states that he always had recourse to the assistance of Mr. G. Sowerby, and in doubtful cases to that of Mr. E. Forbes, or some other conchologist; and that he excluded from his calculations a great many species of which he did not possess perfect specimens, or a sufficient number to enable the specific identification to be confidently proved. Of the corals collected by the author, forty-three species have been determined by Mr. Lonsdale, only seven of which, or fifteen per cent., agree specifically with those found in the Suffolk crag. This per-centage in the Polyparia is almost exactly the same as that which has been obtained from a comparison of the Testacea. Some of the genera of corals, fossil in Touraine, as the Astræa, Lunulites, and Dendrophyllia, have not been found in European seas north of the Mediterranean; nevertheless the Polyparia of the Faluns do not indicate a climate warmer than that which now prevails on the southern coasts of Europe.

The next general question considered by Mr. Lyell is, whether the Faluns of the Loire and the English crag can be referred to the same geological period, eighty-five per cent. both of the corals and the shells being of distinct species. "Can," he says, "such a conclusion be embraced on the ground of the corresponding degree of analogy which both deposits bear to the existing fauna, and to the extremely wide departure which both the crag and the Faluns make from the fossils of the Eocene period?"

When Mr. Lyell compared in 1839, with the assistance of Mr. Searles Wood and Mr. G. Sowerby, the Suffolk crag shells in Mr. Wood's cabinet, the proportion of recent species in the red crag was found to be about thirty per cent., and in the older or coralline about twenty, or, including both, twenty-five per cent., the same amount as in the Faluns of Touraine; the analogy of the recent crag-shells being almost entirely to shells of the British seas, and that of those of the Faluns mostly to Mediterranean species. The argument which might be derived in favour of the more modern origin of the crag, from the recent species being precisely those of the neighbouring seas, while the existing species of the Faluns are not to the same extent, Mr. Lyell combats by stating that the whole assemblage of English crag genera and species departs very widely from that of the adjacent seas, consisting of northern and southern forms. Thus the Glycimeris, Cyprina and Astarte are northern genera, and of the Astarte there are about fourteen species; and of genera now known as existing only in equatorial latitudes, are Pyrula, Lingula, and some others. The fact, that four-fifths of coralline crag Testacea are extinct, implies high antiquity; as well as the sixteen species of Echinoderms found in the crag being unknown as recent species. The author therefore refers both the crag and Faluns to the Miocene epoch, notwithstanding the specific discordance of their fossils, and he is of opinion that this disagreement may be diminished when the two faunas are better known. The difference between the Testacea of the British coasts and of the Mediterranean is pointed out; and if the greater distance of these seas from each other than of the eastern shores of England from the Faluns should be urged as an objection to the inference that the crag and Faluns belong to one epoch, Mr. Lyell calls attention to the difference in the Testacea on the opposite sides of the isthmuses of Suez and Panama, though these tracts are very inconsiderable, both in height and breadth. That land existed in the immediate neighbourhood of the Faluns, Mr. Lyell says, is proved, from the occurrence of the remains of terrestrial Mammalia, and of land and freshwater shells, though they are of rare occurrence, compared with the marine reliquiæ; and if it formed a barrier between the district occupied by the crag and that by the Faluns, the more northern character of the crag fauna might be due to the sea in which it lived opening to the north; and in support of this opinion he alludes to the rapid transition in the southern hemisphere from a district possessing a mild and equable climate, in which tropical forms of Testacea exist with others common to high latitudes, to one of extreme cold. Lastly, Mr. Lyell says, whatever speculations may be indulged, it is clear that the fossils of the crag and Faluns are almost entirely different from those of the London clay and Paris basin; that at least one-fifth of the fossil shells, both in the crag and Faluns, are identical with recent species; that fifteen per cent. of the shells and corals of the Faluns are specifically identical with those of the Suffolk crag; and that the supposed difference of climate indicated to the Testacea and Polyparia is by no means so great as some observers have supposed. Mr. Lyell nevertheless does not attach such importance to the per-centage of recent shells in the present state of knowledge of all the recent species, as to deduce from this source alone a positive inference regarding the precise agreement in age of the Faluns and the crag, merely stating that both deposits are referable to the Miocene epoch; and as the red and coralline divisions of the Suffolk crag were not formed at the same time, so he conceives there may have been shades of difference in the relative age of the Faluns and the crag.

June 16. "Description of a Newer Pliocene Deposit at Stevenston, and of Post-Tertiary Deposits at Stevenston and Largs, in the County of Ayr," by the Rev. David Landsborough, and communicated by James Smith, Esq., F.G.S.

The Newer Pliocene Deposit.—This stratum was discovered in 1839 in opening two coal-pits in the parish of Stevenston. After penetrating from thirty to thirty-five feet of sand, a bed of blue clay, nine feet thick, was passed through, and found to contain marine fossils of the newer Pliocene epoch. All the species have been obtained in other deposits of the same age in the basin of the Clyde, except two, —*Astarte borealis*, which occurs in a fossil state in the crag and living in the Arctic seas, and *Astarte propinqua*, a new shell. Mr. Landsborough gives a list of the twenty-seven species collected by him, nineteen of which are common in the adjoining seas, six are known to exist in the Arctic seas, and two, *Natica glaucinoides*, a crag fossil, and *Astarte propinqua*, are believed to be extinct.

Post-Tertiary Deposits.—The author prefixes to his account of these beds a notice of the older formations in that part of Ayrshire.

The prevailing rock is red sandstone, which, at almost every point on the coast, has been worn, by the former action of the sea, into cliffs, which indicate a change of level of about forty feet. The terrace at the base of the cliff, Mr. Landsborough states, may be considered a marine raised beach, and the shells contained in it are, with two exceptions, one of which is doubtful, of existing species.

At Ardrossan, a deposit twenty feet above the level of the sea, and at Kelly, the soil which covers the base of the inland cliff to the height of thirty feet, are full of common marine shells. Similar beds are stated to occur in the islands of Arran, Cumbra, and Inch Marnock. In the parish of Stevenston, immediately under the vegetable soil, is a bed of shingle, in which forty-seven species of shells common on the adjacent shores have been found. It rests upon shale perforated in many places by the *Pholas crispata*, of which the shell, in a very friable state, is generally found within the cavity.

At Largs the shore rises to the height of twenty feet above high water. Under a bed of loam, from five to ten feet above the sea-level, is a sandy stratum one foot thick, from which Mr. Landsborough has obtained specimens of *Millepora polymorpha*, and seventy species of marine shells, the whole of which are well-known inhabitants of the British seas, except two species of Rissoa, one of which had been previously found only in the crag, and the other is referred with doubts to the *Rissoa Harveyii* of Mr. Forbes.

Respecting the age of this deposit, Mr. Landsborough states, that 160 species having been found in it by Mr. Smith and other geologists, it would be rash to infer from the above two exceptions, "that there is a difference in the faunas of the existing period of sea-level and of that which preceded it;" but he thinks it is not improbable that some change may have taken place during the very long period in which the inland cliff was formed by the slow wasting of the sea; and he adds, the position of the bed at Largs, being ten feet under the surface, indicates a considerable antiquity, although its age must be much newer than that of the Pliocene strata, in which there is a decided proportion of extinct Testacea.

Lists of the shells found by the author at each locality accompany the paper.

A letter from Capt. Alexander, F.G.S., "On the Annual Destruction of Land at Easton Bavent Cliff, near Southwold."

From careful observations, made during the last five years, Capt. Alexander is of opinion, that the local statements, of 350 yards in breadth having been destroyed at Easton Cliff in about thirty-five years, are not much overrated, as, during that period, a nearly square field, containing twelve and a half acres, has been entirely removed by the sea, and as only three acres remain of another which consisted of eighteen and a half. This ratio of loss, he says, has extended along the whole range of the cliff except at the extreme south end. During the five years that Capt. Alexander has personally watched the action of the sea upon this coast, the annual loss in breadth has been at least seven yards.

About 200 yards in rear of the lowest part of the cliff is a tract, consisting principally of marsh land, which is beneath the level of the sea, and extends in different directions to Bungay, Beccles and Halesworth, and was undoubtedly occupied, Capt. Alexander states, at no very remote period, by extensive rivers flowing into an estuary connected with the German Ocean.

The letter was accompanied by a section constructed by Mr. R. Alexander, and which gives the following bearings of two churches at each end of the cliff:—

South End.—Southwold church, 22° 30′ S. of W.; Blyborough church, 7° 30′ N. of W.

North End.—Roydon church, 14° 3′ 45″ N. of W.; Covehithe church, 30° E. of N.

The bearing of the edge of the cliff, at the two extremities, is stated to be 43° 7′ 30″ E. of N.

A paper entitled "Description of Cuttings across the Ridge of Bromsgrove Lickey, on the line of the Birmingham and Gloucester Railway," by Hugh Edwin Strickland, Esq., F.G.S.

When Mr. Strickland laid before the Society, in June 1840*, a description of a series of coloured sections on the Birmingham and Gloucester Railway, the cuttings on the Lickey not having been completed, he was prevented from detailing the phænomena exhibited on this part of the line. The present communication is therefore supplementary to the former memoir.

Where the cutting crosses the ridge, it has been excavated to the depth of fifty-six feet, and exhibits, Mr. Strickland states, clear proofs of the disturbance which attended the elevation of the Lickey. The lowest rock which has been exposed is a mass of hard, grey, brownish or reddish, compact or coarse-grained sandstone, occupying a horizontal distance of about seventy yards, and rising gradually to the north-east to the height of twenty feet. At the point where it attains this visible thickness, it is suddenly cut off by a nearly vertical fault. The strata dip about 60° to the east-south-east, or from the trap composing the Upper Lickey. No organic remains having been noticed, it is difficult, Mr. Strickland states, to fix the precise geological position of the deposit; but he is inclined to assign it, on mineral characters, to the lower new red sandstone of Mr. Murchison†.

These highly inclined strata are overlaid unconformably by a vast mantle-shaped mass of conglomerate, belonging to the "upper new red" or Bunter sandstein. The bedding of this deposit is so irregular that great accuracy of dip is not attainable; but to the east of the fault the inclination is about 5° to the east-south-east, and to the west about 5° to the south-south-west or south; and the slight departure from horizontality is strongly contrasted with the high inclination of the lower sandstone. The resemblance of this deposit, consisting of rounded pebbles in soft red sandstone, to ordinary

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* See Proceedings, vol. iii. p. 113 [Phil. Mag., Third Series, vol. xviii. p. 523].

† Silur. Syst., p. 54.

gravel is so perfect, that Mr. Strickland was at first induced to consider it as superficial detritus; but its true nature is proved by its containing numerous wedge-shaped masses of red sandstone and red marl, and by its being overlaid at each extremity of the cutting by the regular thick-bedded sandstone, which again is surmounted by red or Keuper marls. At least nine-tenths of the pebbles consist of white and crystalline, or brown and granular quartz, the latter doubtlessly derived, Mr. Strickland states, from such altered sandstones as exist *in situ* in the Lickey. The remainder of the pebbles are composed of various traps, chiefly porphyritic, and often decomposed into clay. Boulders also occur of a hard quartzose conglomerate, derived, the author believes, from the old red system; likewise pebbles of chert, containing casts of Spirifers and Crinoidea.

Patches of gravel overlie the red sandstone on the flanks of the Lickey, sometimes filling up considerable irregularities in its surface, but none were exposed on the summit of the ridge. The gravel resembles the conglomerate of the new red sandstone, as it consists chiefly of the same materials, but it may be distinguished by containing many fragments of slaty rocks, and by the whiter colour of the pebbles. It attains on the line of the railway a height of 544 feet*; and as the gravelly soil which has been stated to occur on the Lickey Beacon at an elevation of 900 feet may, Mr. Strickland says, belong to the new red conglomerate, the gravel on the line of the railway occupies the highest position which can with certainty be assigned to the northern drift of that part of England.

The point of greatest interest exposed in this cutting is the unconformability of the lowest rock to the overlying conglomerate. Assuming that the former is correctly identified with the "lower new red," it follows, Mr. Strickland observes, that a tolerably exact geological date is obtained for the principal protrusion of the volcanic rocks of the Lickey†; and that they must have been erupted after the deposition of the lower new red, and before that of the upper new red. It is also probable, he states, that the pebbles of the conglomerate were in great part derived from the shattered upheaved strata in the immediate vicinity. The author further infers, from the fault in the upper conglomerate beds, that additional elevations of the Lickey region took place at a later date, and threw the superior strata into an anticlinal position. He also suggests that some of the dislocations connected with the Lickey may have occurred subsequently to the deposition of the lias, as the faults which have affected that formation and the new red sandstone in Worcestershire and Warwickshire appear, he says, to have radiated from the Lickey‡.

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* The height of 587 feet, given in the corrigenda at p. 316, has been ascertained to be incorrect.

† The age here assigned to the trap rocks of the Lickey coincides, Mr. Strickland says, with that attributed to them, as well as to the trap rocks of Abberley and Malvern, by Mr. Murchison, though the want of unconformability between the upper and lower new red strata was apparently unknown to that gentleman. (Silur. Syst., p. 67.)

‡ See Geol. Trans., 2nd Series, vol. v. pp. 333, 335.

"Description of a Model of Arthur's Seat and the King's Park, Edinburgh," by J. Robison Wright, Esq., F.G.S.

This model was constructed on a horizontal scale of ten inches to a mile, but for the vertical scale double that dimension was employed, to render the crags perceptible to the eye. The included area is between two and three square miles, extending from Edinburgh on the west to Duddington on the east, and from Holyrood Palace on the north to Prestonfield on the south. The author notices in his description the structure and phænomena successively exposed in proceeding from Edinburgh eastward; but as the details have been chiefly extracted, with acknowledgment, from Mr. Maclaren's published work on Fife and the Lothians, it is not considered necessary to give an abstract of them.

"Notes by Mr. Maclauchlan, F.G.S., to accompany some Fossils collected by himself and Mr. Still, F.G.S., during their employment on the Ordnance Survey in Pembrokeshire."

Taking for a base-line the northern boundary of the Llandeilo flags laid down by Mr. Murchison, the author proceeds to describe a section extending from near Llanhuadain on the south, to Dinas Head on the north. At Potter's Slade, a little north-west of Llanhuadain, a conglomerate dips to the northward, and is traceable westward to Ford, and eastward towards Llangan, where a sandstone conglomerate occurs containing Trilobites and shells. Proceeding on the line of section, the conglomerate is succeeded first by sandstone and sandstone shales, and then at Clarbeston by limestone with carboniferous shales, dipping northward, and containing Graptolites and casts of shells. Similar carbonaceous shales exist on the west of Clarbeston, at St. Catharine's Bridge, near Camrose; also at Rudbaxton, and on the east at Long Ford, near Llandysilio. They have in some localities been unsuccessfully worked for coal. Graptolites have likewise been found in calcareous shales at Robleston, about a mile north-west of Camrose. At Llys-y-fran, north of Clarbeston, the carbonaceous shales are succeeded by roofing-slates, which at Mynydd Castell-bythe (Castell-y-furoch, Ord. Map) and Morfel alternate with trap. On the summit of Mynydd Pontfaen, sandstone with coarse slates occurs, and between the summit and Pontfaen, trap again alternates with slates. The summit of Mynydd Llanllawer consists of coarse-grained, rudely columnar greenstone, flanked on the northern declivity of the mountain by coarse sandstone of trappean aspect. This rock is overlaid by roofing-slates, which extend nearly to Dinas Head, where a hard conglomerate sandstone, containing crinoidal remains, is exhibited. All these strata are represented in a section as dipping towards the north.

In Aberreiddy Bay, about twelve and a half miles to the south-west of Dinas, slaty beds with a northwardly dip, and apparently prolongations of the schists on the line of section, contain the *Graptolithus Murchisonii* and *G. foliaceus* of the Llandeilo flags, also numerous casts of an Euomphalus, resembling the *E. perturbatus* of that formation, and a species of Lingula. Although these slates differ in lithological characters from the Llandeilo flags, yet Mr.

Maclauchlan is of opinion that this difference may have been produced by the masses of trap which are associated with the slates.

The conglomerate sandstone of Dinas Head, which occurs also in Newport Bay and at Trewyddel, near Cardigan, is stated to resemble one of the conglomerates of the Caradoc sandstone described by Mr. Murchison; and the crinoidal stems which it contains, to agree with analogous remains found by Mr. Murchison in the Caradoc sandstone at Little London, May Hill*.

In addition to the phænomena which occur in the immediate vicinity of the section, Mr. Maclauchlan alludes to indications of anticlinal lines near Narberth and at Camrose, in Southern Pembrokeshire; also at Solfach, south-east of St. David's, and at Porthllisky, to the south-west of that city. At the latter village the dip changes to the westward, and continuing to alter, assumes in Whitesand Bay a northwardly direction. This dip also prevails at St. Laurence, thirteen miles east of St. David's Head; at Leweston, three miles south of St. Laurence, and at Long Ford, about two and a half miles south of Llandysilio. Trappean ash also is stated to occur near greenstone at Penbury (Penberry, Ord. Map) Hill, two and a half miles north-north-east of St. David's; at Llanllawer, two miles south-east of Fishguard; and at Carningley, one mile south of Newport.

Though the summit of the principal Pembrokeshire chain is roofing-slate, yet trap-rocks occur near the top, and are described by the author as continuous through the district, extending to Plumbstone Mountain (five miles north-west of Haverfordwest) and to St. David's Head, re-appearing at the Bishop and Clerks and the Hat and Barrels rocks, and at the Smalls light-house. At Fishguard and Strumble Head, three miles west of Fishguard Bay, the trap is columnar. On the north-east of the chain at Whitechurch (Eglwyswen), six miles south-east from Newport, on the east at Llanfirnach (Llanfrynach, Ord. Map), and on the south at Llanglwydwen, are beds of dark carbonaceous shale, which have been fruitlessly worked for culm; they appear to mantle round the trap, but preserve a northerly dip; they are accompanied by lead-veins, one of which, at Llanfernach, has been worked successfully. At Llanglwydwen Bridge are indications of copper in a lode in contact with a considerable bed of limestone. Mineral veins also exist along the coast, from Newgate, in St. Bride's Bay, to St. David's Head.

"Description of some remains of a gigantic Crocodilian Saurian, probably marine, from the Lower Greensand at Hythe; and of Teeth from the same formation at Maidstone, referable to the genus Polyptychodon," by Richard Owen, Esq., F.G.S.

The fossil saurian remains from the lower greensand discovered by Mr. H. B. Mackeson include portions of the iliac, ischial and pubic bones, a large proportion of the shaft of a femur, parts of a tibia and fibula, and several metatarsal bones. In consequence of the absence of vertebræ and teeth, the determination of the specific characters of this Saurian is, the author states, a subject of great

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* Silur. Syst., pl. xx. fig. 19.

difficulty, and he therefore confines his remarks, in the present paper, to indications of the characters by which it differs from previously known extinct genera of Saurians. In the first place, Mr. Owen shows, from the femur and other long bones having no medullary cavities, but a central structure composed of coarse cancelli, that the animal of which they formed part was of marine habits; he, however, adds, that the principal bone being a femur, independently of the size and shape of the metatarsals, at once negatives the idea that these remains belonged to the cetacean order; and that the form and proportions of the metatarsals equally forbid their reference to any other mammalian genus.

Femur.—The portions of this bone secured by Mr. Mackeson include about the two distal thirds, excepting the articular extremity. Its length is two feet four inches, its circumference in the middle or smallest part of the shaft is fifteen inches six lines, and at the broken distal end, two feet five inches. These dimensions prove that the animal was equal to the most gigantic described Iguanodon*. If the supposition of the proportion of the femur which has been preserved be right, this bone, Mr. Owen says, differs from that of the Iguanodon, not only in the want of a medullary cavity, but also in the absence of the compressed second trochanterian process which projects from the outer side of the middle of the shaft, and which forms one of the several curious analogical relations between the Iguanodon and Rhinoceros. The bone also expands more gradually than in the femur of the Iguanodon, and the posterior part of the condyles must have been wider apart in consequence of the posterior inter-condyloid longitudinal excavation being longer and wider. Various other minor points of difference are noticed by the author.

Tibia and Fibula.—The portion of a tibia which has been preserved is compressed near its head, and the side next to the fibula is slightly concave. The longest transverse diameter is eight inches nine lines, and the two other transverse diameters at right angles to the preceding give respectively three inches three lines and two inches six lines. The bone soon assumes a thicker form, its circumference at about one-third from its proximal end being sixteen inches six lines. The cancelli occupying the central portion of the bone are arranged in a succession of layers around a point nearest the narrower end of the transverse section. Lower down the tibia again becomes compressed, and towards the distal end the transverse section exhibits a plate bent towards the fibula, and its narrowest transverse diameter is two and a half inches.

The portion of the fibula is eleven and a half inches long. In the middle it is flat on one side, slightly concave on another, and convex on the two remaining sides. It presents the same cancellous structure as the tibia, but the concentric arrangement of the layers of cells is more exact. Towards the opposite end of the bone the concave side becomes first flat and is then produced into a convex wall,

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* Femur of the Iguanodon,—length, 4 feet 6 inches; smallest circumference, 1 foot 10 inches.

terminating one end of a transverse section of a compressed and bent thick plate of bone.

Metatarsals.—These bones, Mr. Owen says, exhibit the characteristic irregularity of length of the crocodilian metatarsals. Of two imbedded in the rock, and considered by the author to be the innermost and second, the former or smaller measured one foot in length, and the latter two feet, having a diameter of eight inches at its greater and of four inches five lines at its narrowest or middle part, and of six inches at its other extremity, which was imperfect. The whole of the bone within the compact outer crust consisted of cells varying from a half to two-thirds of a line in diameter. Portions of four other detached metatarsals are described.

Ilia, Ischia, Pubis, and Coracoid Bone.—These bones, the author states, also conform to the crocodilian type. The remains of the ilia are flat and nearly straight, and they gradually but slightly widen towards one end. Of one ilium, a portion, twenty-five inches long and ten inches across at the broadest end, is preserved, and of the other a fragment twenty inches in length.

The mesial extremities of the pubis and ischium are preserved in the same block of stone. The pubis, Mr. Owen states, differs from the crocodilian type in its greater breadth. The portion exposed in this block is principally convex, but it becomes concave towards the opposite or median margin. At its broadest part it is thirteen inches across, and its length is seventeen inches. This expanded extremity is rounded, and the diameter of the corresponding expanded extremity of the ischium, which is obliquely truncated, is nine inches. In another block of stone the expanded extremity of the opposite pubis is preserved, and measures fourteen inches across and twenty-two inches in length.

The bone, considered by Mr. Owen to be a coracoid, is two feet in length and seventeen inches in its greatest breadth, and it varies in thickness from three to five inches. The breadth of this bone indicates, the author states, the great development of the muscles destined for the movement of the fore-leg, whence he infers that the anterior extremities were more powerfully and habitually used in progressive motion than in the Crocodiles, and that they were consequently provided with a webbed modification of the hand.

Mr. Owen then enters upon the question of the identity or affinities of the Hythe remains with any of the known marine genera of the saurian order, the texture of the long bones being conclusive against their having belonged to the terrestrial genera, the Iguanodon and Megalosaurus.

The length, thickness, and indications of condyles in the femur, and the length, thickness, and angular form of the metatarsal bones, place, he says, the Plesiosaurus and the Ichthyosaurus out of the pale of comparison; as well as the Mosasaurus, the locomotive extremities of which are considered to have been flattened paddles.

The superior expanse of the pubis and the broad coracoid (?), with the form of the femur and the gigantic proportions of the bones, forbid a reference to any subgenera, recent or extinct, of the crocodilian reptiles; and he shows that it is distinct from the Poikilopleuron of M. Deslongchamp by the long bones of that Saurian having medullary cavities.

Saurian Teeth from the Lower Greensand.—These teeth, described by Mr. Owen in his 'Odontography' under the name of Polyptychodon, are characterized by the crown presenting numerous closely set longitudinal ridges, which are continued, of nearly equal length, to near the apex of the crown. In their size and simple conical form the teeth of the Polyptychodon resemble those of the great sauroid fish, *Hypsodon*, Ag., but may be distinguished by the solid compact structure of the dentine, which is resolved by decomposition into successive cones; and also by the ridges on the exterior of the crown of the Hypsodon's teeth being alternately long and short, and terminating abruptly at different distances from the base, the interspaces between the longer ridges widening as they approach the apex. The tooth of the Polyptychodon is slightly and regularly curved, and invested with a layer of enamel of a clear, amber-brown colour, and of which the ridges are composed, the surface of the outermost layer of dentine being smooth. A tooth from the lower greensand near Maidstone has a crown three inches long, and one inch four lines across the base. It consists of a body of compact dentine composed of successive lamelliform cones, and has a short and wide conical cavity at the base.

From the teeth supposed to have belonged to the Poikilopleuron, the specimens above described differ in the ridges on the crown being greater in number and more closely set, as well as in the form of the teeth being nearly circular instead of elliptical; from the teeth of the Pliosaurus they differ also in being round and not three-sided, and in having longitudinal ridges over the whole surface of the crown; and from the teeth of the Mosasaurus they differ in being ridged and not smooth.

In conclusion, Mr. Owen states, that as the Hythe Saurian is distinct from all other described Saurians, and as these teeth belonged to a great Saurian also undescribed, and further, as the Maidstone tooth was found in the same formation as the Hythe fossil, so it may be convenient to consider all these remains for the present to have belonged to the genus Polyptychodon, originally proposed for the animal which was provided with the teeth.

LONDON ELECTRICAL SOCIETY.

[Continued from vol. xix. p. 405.]

Nov. 16, 1841.—The Society met in the Council room of the Adelaide Gallery. The papers before them were,—1st, "An Experimental Inquiry into the Nature of Ozone." By Mr. J. W. Gann.

The first experiments were made by breaking and making contact between various electrodes (from a series of ten Grove's) in a jar of atmospheric air. Copper, iron, silver, and platinum anodes developed the odour; zinc did not; neither did plumbago nor carbon: the same results occurred when the experiments were varied by using oxygen, nitrogen, hydrogen, carbonic acid, and nitrous oxide.

When acid water was electrolyzed, the odour was not produced, but with a platinum anode: this was also the case with muriate of soda; but the odour was not detected until the gas was first washed in ammonia to absorb the chlorine, which appeared to disguise the ozone. When the gases obtained by electrolysis were exploded in proper proportions in the eudiometer, the odour disappeared with the gas; it also disappeared from oxygen after the Leyden spark had been sent through it. The author here gives certain conclusions to which he is led, the chief being his opinion, that under certain conditions all metals develope it.

2nd. "On the tendency of Electricity to promote the Growth of Plants." By Mr. Pine.

The general conditions chiefly dwelt upon in this the writer's fourth communication, are the positive state of the air, and the negative state of the soil. He brings many cases in proof that the luxuriancy of vegetation is in proportion to these states. A drooping narcissus being removed into a room, the atmosphere of which was constantly surcharged with electricity from a machine often used for medical purposes, revived and attained the gigantic height of thirty-six inches. Mustard-seed, in a pot whose soil was negatively electrized, vegetated with greater vigour than seed in a positive soil, and far greater than seed in its natural condition.

3rd. "On the Powers of a Water Battery." By H. M. Noad, M. E. S.

A fact worthy of notice in this communication is, that two copper discs are connected with the terminals of a series of 500, and a pith ball insulated and suspended between the two vibrates without cessation: it had been in motion for a fortnight, with the exception of a few intervals, when it was removed to give place to other experiments. Mr. Noad mentions that he has erected, but on a smaller scale, an apparatus for atmospheric observations similar to that of Mr. Weekes.

4th. "Note upon a Phænomenon presented by solution of Nitrate of Silver, decomposed by the Current." By M. Ch. Matteucci. (Translated by the Secretary from *Les Archives de L'Electricité*.)

The author of this paper has observed that the *black* deposit obtained on the cathode, in the electrolysis of this salt, instantaneously becomes *white*, when the current ceases; that it does not occur except when the solution is weak, because from strong solutions crystalline silver is at once deposited; that if a portion of the deposit, after it has become white, be suspended between the electrodes while the current passes, those portions *towards* the anode again become *black*, if the experiment is made in a solution of the nitrate, but not if in mere acid water; it will, however, occur if a mere drop of nitrate is added to this latter solution. If a plate of glass is interposed between the electrodes, the phænomenon does not occur; this the author attributes to the great reduction of intensity. In conclusion the author suggests, "that the black deposit is formed of oxide of silver, which is preserved by the passage of the current, and which, when the current ceases, passes immediately to the metallic condition."

5th. The Secretary then read extracts from Mr. Weekes's Monthly Register for October:—

"The most remarkable features of the month have been the frequent recurrence of its heavy gales, and long-continued torrents of rain; and the major part of these were, in the language of an electrician, decidedly of a neutral character. The quantity of rain fallen, and, moreover, the immense deposit of dew during several of the nights,—albeit that the amount of daily evaporation proved comparatively very small,—were unprecedented within memory, as regards the locality of our observations."—"The neighbouring marshes and low lands are inundated. Many cellars in the town (Sandwich), known to have been dry upwards of sixty years, are now full from a rising in the springs."

Dec. 21.—The following papers were read:—1st, "Description of an Hydrostatic Galvanometer," by R. J. Iremonger, Esq., M.E.S. A small bar-magnet is inserted in a cork float, and so arranged as to float perpendicularly in water contained in a glass tube. Around the latter is a moveable shelf, supporting a De la Rive's ring, through which the current to be examined is sent. The effect is, to sink the floating apparatus in *direct* proportion to the power developed. The action of this instrument depends upon the law of the pressure of liquids, which is simply as their heights. Several rough models have been constructed, and have been found to act well. If, upon further experiment with well-constructed instruments, the opinions advanced in this paper should be borne out, the experimental philosopher will obtain an important and much needed instrument. 2nd, "General Explanation of the noise of Thunder," by M. Tessan (from the *Comptes Rendus*). The author argues that when a cloud is electrized, the outward pressure of the electricity relieves it from the inward pressure of the air, in a degree proportionate to its electrical state, and hence it expands, till the two antagonist powers are in equilibrio. But if any of the electricity escapes, as in a flash of lightning, the cloud instantly contracts, the vapour conglomerates into drops, the air rushes with a thundering noise, and the well-known deluge of rain falls. In these fundamental principles the author traces several analogous phænomena. 3rd, "Notice on a New Galvanic Battery," by J. A. Van Melsen of Maestricht (from the *Bullet. de l'Acad. Bruxelles*). This battery is a great improvement on the Wollaston, made by placing the plates as close as $\frac{1}{13}$th of an inch by amalgamating the zinc, and by immersing the whole series in one undivided trough. The great increase of power obtained by these means was given in the lengths of wire fused by different arrangements. A note was appended by Mr. Walker, speaking favourably of the arrangement, and recommending the introduction of the recent improvement of Mr. Smee and Prof. Grove. 4th, "Note upon a Modification in the Construction of Galvanic Piles," by M. Crahay. This note referred to certain variations of the above arrangement, the principal being the soldering together of the respective plates, back to back. 5th, "An Electro-Magnetic Steel-yard," by Prof. Jacobi (from the author), translated from the

German. This was an instrument to determine the lifting power of electro-magnets, and differs from the ordinary steel-yard in having the power applied *between* the fulcrum and the weight. The latter is fixed on wheels; the lever is counterpoised by a weight passing over a pulley. It would be difficult to describe the minutiæ of arrangement without a plate for reference. 6th, "On a New Electrometer," by J. C. Oersted. Coulomb's rod is in this instrument a brass wire, hanging from a silk fibre by a small iron stirrup *slightly* magnetized. The ends of the rod rest against a semicircle of brass, by means of which the electricity is communicated. It is stated as being extremely delicate, but nothing is said in explanation of the magnetized stirrup. 7th, Mr. Weekes's Register for November. The Secretary stated that he had received from Prof. Jacobi the description of other apparatus, which would be submitted to the Society at the next meeting, January 18, 1842.

AMERICAN PHILOSOPHICAL SOCIETY.

June 18, 1841.—Mr. Walker read a letter from Professor Forshey, of Natchez, giving an account of several interesting displays of meteors.

Mr. Walker observed, that the display of the 20th of April, which was noticed in Virginia in 1803, and which has been referred to by MM. Arago, Quetelet, Herrick and others, was watched for by Mr. Herrick in the last three years, without any remarkable result. Corresponding observations were made in the present year at Cambridge, New Haven, Philadelphia and Washington, on the 19th, the 20th and 21st being cloudy, from 11 o'clock till midnight; but the number of meteors seen was not greater than usual. In the morning of the 19th, however, a gentleman of Philadelphia, Mr. William F. Kintzing, counted eight in the course of ten minutes, shortly after midnight.

At about 8 o'clock on the same night, the 18th, at Vidalia, in Louisiana, Prof. Forshey noticed an unusual number of meteors in different parts of the heavens, and on tracing their paths backwards, found that they traversed the constellation Virgo. Having commenced precise observations at half-past eight, and continued them for three hours, he saw in two hours and a quarter, forty-five minutes being lost in recording, sixty meteors, of which all but five passed within 10° from the common radiant point. These meteors were very unlike those of the August shower, being chiefly without trains, and of a reddish colour, few of them of the first magnitude, and the greater number of the third and inferior magnitudes. Their velocities were remarkably equal and gentle; their paths short; and their light first increasing, then waning, as if they were moving on a chord to the circle of visibility. Professor Forshey determined their radiant point to be in a line drawn from Spica to θ Virginis, somewhat nearer to Spica, say in R. A. 198°, S. Decl. 8°. The convergent point was, therefore, in lon. $19^{\circ}\cdot6$, and lat. N. $0^{\circ}\cdot3$, while the observer's motion was towards a point of the ecliptic in long. 299°. This gives a deflection of the path of the meteors, relatively to the true path of the observer, of 80°·6; and hence their true velocity cannot have been much less than that of the observer, or about sixteen geographical miles per second. This observation of the convergent point of these meteors, Mr. Walker regards as strongly confirmatory of the cosmical theory of shooting-stars; inasmuch as it seems to demonstrate the existence in this group of a planetary velocity, like that of the December group observed by Mr. Herrick in 1838, in a direction normal to the observer's motion, and incapable of resulting from it.

Professor Forshey also observed the meteor-shower of the 12th of November, 1833: he was then a cadet at West Point. While engaged, long before dawn, in preparing his morning recitation, his attention was caught by flashes of light at his window as if from lightning. The spectacle which met him on opening it, he describes as one of singular and fearful sublimity, the whole sky streaming with fire-balls, throwing a bright light upon the plain, and reflecting luridly against the mountains which enclose West Point. After a few minutes, finding no intermission in the display, he roused his associates to witness it; and the first sense of personal hazard yielding to the remark, that none of the meteors (meteorites, as he then supposed) were actually descending into the plain, but that they became invisible before reaching the level of the mountains, he crossed the plain to awaken Professor Courtenay. While in company with this gentleman, he witnessed the magnificent meteor with a serpentine train, described by Professor Olmsted and others, and which has been called Twining's meteor, after the Professor who calculated its relative path and velocity. Both Professor Courtenay and himself noticed the white nebula which it left on exploding, and the beautiful silvery cloud that remained for some ten minutes after. He listened carefully, during the meteoric display, for the noises which are said sometimes to attend such phænomena, but could hear none; the explosion of the Twining meteor, he is confident, was not accompanied by an audible report.

Professor Forshey does not believe that the meteors of the 12th of November have the anniversary character. He has watched for them every year, except 1834 and 1836, since their appearance in 1833. He saw the great auroral arch of 17th Nov. 1835, from a point near the junction of the Ohio and Mississippi; and on the 14th November, 1837, he witnessed at Jefferson College a brilliant crimson arch, a rare phænomenon in that latitude, 31° 36′. He noticed also occasional brilliant meteors on the 13th and 14th of November, 1837, but they did not appear to come from the well-known radiant point of 1833, in Leo. The times for observation in 1838 and 1839 were too cloudy to allow of satisfactory results. The subsequent anniversaries were clear, and well watched, but without any observation of interest.

Professor Forshey mentions that he had seen the zodiacal light in the west from December to May, but that he first witnessed it in the east on the 4th of October of last year, when it continued in great brilliancy from 3 A.M. till daylight.

PROCEEDINGS OF THE SECTION OF PHYSICS, ETC., OF THE SCIENTIFIC CONGRESS HELD AT FLORENCE IN SEPTEMBER 1841*.

We owe to the kindness of M. Matteucci an extract from the minutes of the meetings of the physical, mathematical and chemical sections, of which he is the Secretary.

M. Pacinotti read a memoir on the induced currents which are developed in magnets put in rotation. After having studied the principal circumstances of this phænomenon, he showed how it may be made use of in order to study the distribution of magnetism in a magnet, and in order to have a current of constant power; and how also suitably, in bringing together several magnets in rotation, we may have a very powerful electrometer.

M. Cagnozzi read a memoir on tonography. He presented an instrument which he calls a *tonographe*, and with which he proposes to write down the music of declamation.

M. Vegui showed some *wire ropes* (*cordes en fil de fer*), in the centre of which there is a cord of hemp: it appears that this modification destroys the rigidity of ordinary wire ropes.

Professor Cassiani of Parma read a memoir, the object of which is the study of the oscillatory motion observed long since in an astatic system of magnetic needles. He finds that this motion is connected with the electric state of the atmosphere.

Prince Louis Bonaparte exhibited some platina gilt by De la Rive's process, and observes that this metal takes the gilding better than silver. He explains this, on the one hand, by the greater density of the platina, and, on the other, by the insoluble layer of chloride of silver, which must withstand the perfect gilding of silver.

M. Matteucci spoke of the experiments lately made in England, and of those of M. Peltier in France, according to which the vapour of water appears to him to manifest a state of electricity when it is formed at a high degree of tension and at very elevated temperatures. He afterwards noticed the observations which he has made for the purpose of studying the electric state of the atmosphere near the columns of vapour which rise in the Lagoons of Tuscany, where boracic acid is produced. He says that he has not observed any difference, with respect to electricity, between the air near the Lagoons and that at a great distance from them. From this he concludes, that we cannot consider the slow evaporation of water from salts, or from the sea, as the cause of atmospheric electricity.

M. Vincent Amici extends the principle of virtual velocities, and of living forces, to liquids which are acted upon by any forces whatever in their elements, and compressed at their surface.

M. Majocchi communicated a series of experiments, the result of which is that heat, whether conducted or radiating, is endowed with the same property which M. Becquerel, jun., found existing in those rays of the spectrum which he has named *continuateurs*. It is thus that he has discovered that photogenic papers submitted to the action of light during an exceedingly short time, and without their

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* From the *Bibliothèque Universelle*, No. 69, October 1841, p. 205.

undergoing any action, change if they are heated in any way whatever.

M. Perego related his experiments on the development of electricity by the immersion of bodies in mercury. One of the substances which succeed the best is the *felt* used in making hats.

M. Mossotti read a memoir on the explanation of the dispersion of light in the undulatory theory.

M. Dini communicated some experiments relative to the influence of heat on capillary attraction.

M. Orioli communicated the observation of a kind of light seen by an individual on his toes. M. Boyer noticed a similar observation. These two physicists explain these phænomena by supposing a morbid secretion of electricity.—M. Matteucci cannot admit this explanation, not seeing how a luminous electric charge can be preserved without discharging itself upon bodies which are in contact; he thinks that these phænomena should rather be referred to those which relate to the phosphorescence of rotten wood, fish, &c.

M. Matteucci exhibited an apparatus for electrostatic inductions. He describes an apparatus which he calls a differential inductometer, and which is composed of a spiral plane placed between two similar spirals; these may approach more or less to the intermediary spiral, and their extremities may be united together so as to produce two currents which circulate in contrary directions. He mentioned some contrary results which he found in studying these induced currents by means of magnetizing, by the galvanometer, and by the hole made in paper by the spark of induction.

M. Pacinotti described two experiments which appeared to him contrary to the theory of Ampère. He has a cylinder of soft iron, which is hollow; he introduces into the internal central part of this cylinder a spiral, and places a similar spiral at the exterior. M. Pacinotti has found that the current of the external spiral does not give an induced current in the internal spiral, and that a current in the internal spiral does not magnetize the cylinder of soft iron.

M. Mahlmann of Berlin read an extract from a work on the distribution of heat on the surface of the earth.

M. Giazi read a description of the effects produced by lightning. He exhibited some pretty vitrification of stones, some black and yellow traces left on a wall, and a nail magnetized by means of the thunder stroke.

The Count Scopoli mentioned the causes of the inundations of Lombardy, and added to the known causes the slow destruction of certain artificial lakes which are found on the declivities of the mountains.

MM. Stefani and Jordani presented models of *electric telegraphs*, and of *electro-magnetic telo-typographs*.

M. Cini exhibited a singular reproduction of a drawing, which is produced at the end of a certain time on the plate of glass which covers this same drawing.

M. Marianini read a summary of the experiments made by him on induction. He thinks he can establish as a fact, that the current of induction is developed in the metallic thread, the circuit of which is closed an instant after the voltaic circuit has been shut or opened. He believes, that by following up these experiments he shall be able to connect the principle of electro-dynamic induction with that of the influence of the electricity of tension.

M. Matteucci mentioned his experiments on the current yielded by the frog, and described some new experiments on the torpedo.

M. Carlini spoke of the use of the barometer in the measurement of heights, and described an apparatus for the purpose of having the level of the mercury constant.

M. Marianini read the history of two cases of paralysis which were completely cured by the use of dynamic electricity, applied in an intermittent manner.

M. Zantedeschi confirmed the experiments of M. Matteucci on the torpedo; he thinks that the nerves come from the dilatation of the *medulla oblongata,* which forms the fourth lobe, with the gray matter which covers it.—M. Savi has also made the observations.

M. Morren has found phosphorus in glow-worms, as well as a system of prisms or transparent lenses above the luminous matter.

Professor Gonnella* presented some printed memoirs, in one of which appears the theory and the description of a machine for measuring plane surfaces, which he had invented and constructed in 1824, and an abridged memoir of which he had published in the *Antologia* for 1825. In another of these memoirs he gives a theory of new systems of eye-pieces of arbitrary lengths for the Newtonian telescope, and for spherical as well as for parabolic object-glasses, according as is best suited for artists. In reducing for each telescope the length of the tubes of the eye-pieces greater than the semi-diameter of the object-glass, we may substitute for the small plane metallic mirror a rectangular prism of glass, very small, whatever may be the opening of the object-glass, since the prism may be placed very near the focus without the eye of the observer being obliged to be within the great tube; for the length of the eye-piece determines the place of the eye on the outside of this same tube. We thus obtain the power of constructing the best telescopes, with the prism, as pointed out by Newton, of large dimensions; for, the prism being very small, we may,—1st, easily find pieces of perfect glass for them; 2nd, we may have the total reflexion of the rays, since the quantity of light absorbed by the thickness of the prism is insensible; 3rd, we may obtain the advantage that the loss of the central rays of the object-glass, intercepted by the prism, shall be very small in relation to the diameter of the object-glass, and always smaller relative to the greatest diameters; for the size of the prism is always constant for all the object-glasses, viz. about an inch for each side of the right angle of the rectangular triangle which forms its base. In short, it only remains for us to observe, that the telescopes with the prism have double the clearness of those which have the small plane metallic mirror.

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* [This notice is extracted from a letter addressed by Prof. Gonnella to Mr. Babbage, and kindly communicated to us by that gentleman.—Edit.]

XII. *Intelligence and Miscellaneous Articles.*

ON THE THEORY OF ELECTROLYSIS: SUGGESTION OF A NEW EXPERIMENT.

THE highly interesting experiments of Mr. Knox open new views on the subject of dynamical electricity. It would however be desirable to extend the trial to all the known metals possible; since probably the power of conducting electricity through the mass will be found to be different for each metal, and to be in any given case inversely as the density of the metal. The case of a current of electricity passing through an electrolyte is very analogous; take water as an example: so long as the particles of the fluid are extremely near one another, the internal forces, or forces produced by the action of the molecules on each other, are sufficiently powerful to prevent the passage of the greater part of the current and consequent decomposition of the fluid; when, however, we mix sulphuric acid with the water, the only effect of which is to increase the distance between the molecules of the water and consequently diminish the value of the internal forces, the current is able to overcome those forces, and, by causing the molecules of the fluid to revolve round their axis, to effect its electrolyzation. This reasoning suggests an experiment, which it might perhaps be worth while to try. Thénard many years ago discovered that phosphorus when suddenly cooled, after being heated to a certain temperature, becomes black, owing to some change in the arrangement of its molecules. The internal forces at some instant, whilst the molecules are passing from one state of equilibrium to the other, must have passed through zero; if then this substance be decomposable by electricity, it will most probably be so whilst this change in the arrangement of its particles is taking place. It is not unworthy of remark, that the periodide of mercury, which is an exception to Faraday's law, that none but bodies consisting of a single equivalent of each of their ions are electrolytes, undergoes a change somewhat similar to that which takes place in phosphorus.

V.

ON THE FUSION OF SILICA AND CARBON. BY H. PRATER, ESQ.

To the Editors of the Philosophical Magazine.

GENTLEMEN,

Now that the discussions respecting the nature of carbon and silicon are going on, I beg to communicate, through the medium of your Journal, a result I obtained in April 1840, viz. *a fused compound* of silica and carbon. All that is necessary for the success of this experiment is, that the silica should be *in bulk* five or six times that of the carbon—that the powders should be intimately mixed, and exposed to a heat nearly white under sand or chalk for two or three hours.

The silica used being prepared (by Messrs. Dymond and Co.) from the silicate of potass, contained, as is nearly always the case, a very slight proportion of the latter, and this may have assisted the fusion;

but that the carbon itself had thoroughly fused was obvious. The mass formed a perfectly black *glass*, which, broken in pieces, showed no traces of black powder in any of the interstices.

I have many specimens of the mass in my possession; but the result (as far as I know is new) is easily obtained *by observing that the silica is in the excess mentioned.*

Fluoric acid, or strong hot caustic potass, dissolve out the silica, and the charcoal then falls in powder no way altered. With powdered plumbago the result was the same. Your's obediently,

Euston Hotel, October 1841. H. PRATER.

COMPOSITION OF WOLFRAM.

M. Schaffgotsch, in analysing wolfram obtained from different places, found that the sum of the ponderable quantities of the bases was always greater than it should be in a neutral tungstate of iron and manganese, and that the quantity of tungstic acid yielded by analysis was constantly larger than that calculated according to the composition generally admitted. This circumstance, supported by direct analysis, led M. Schaffgotsch to the conclusion that wolfram contains oxide of tungsten, and not tungstic acid.

The analyses were performed in the following manner:—The finely powdered mineral was fused with three times its weight of carbonate of soda. After treating the fused mass with water, the two oxides of iron and manganese which were left, were dissolved by hydrochloric acid, and separated by means of ammonia and its succinate. The tungsten exists in the solution as tungstic acid; but as it is almost impossible to separate this acid perfectly from soda, or from its sulphur salt, by acids, another method was sought. The wolfram was boiled in hydrochloric acid; the acid is to be repeatedly added, and the ore cannot be considered as perfectly acted upon until the deposit is of a very pure canary-yellow colour: this deposit is tungstic acid; it is to be collected on a filter, and the liquor, precipitated by excess of ammonia and of hydrosulphate, is evaporated after filtration. The residue roasted in the air yielded a small quantity of tungstic acid, which had been dissolved by the hydrochloric acid. This process succeeded without difficulty.

Taking the mean of such analyses as were made more than once, the following are the results of the mineral analysed from the places named:—

	Monte Video.	Ehrenfriedersdorf.	Chanteloupe.	Zinnwald.
Protoxide of iron......	19·24	19·16	17·95	9·54
Protoxide of manganese.	4·97	4·74	6·05	14·84
Tungsten and oxygen..	75·89	76·10	76·00	75·62
	100·10	100·	100·	100·

The author remarks that these analyses warrant the following conclusions:—

1st. That wolfram is formed by two compounds: one of 1 atom of protoxide of iron, FeO, and of 1 atom of oxide of tungsten, WO^2;

the other of 1 atom of oxide of manganese, Mn O, and 1 atom of oxide of tungsten, $W O^3$.

2nd. That these two compounds occur in different proportions in specimens coming from different places, but that these proportions are always simple atomic relations.—*Ann. de Chim. et de Phys., Août* 1841.

ON THE GASES DISENGAGED BY MARINE PLANTS. BY M. AIMÉ, PROFESSOR OF PHYSICS AT ALGIERS.

Having frequently had occasion to observe the plants which grow in the sea, M. Aimé remarked that they are commonly covered with a great quantity of bubbles; and this phænomenon is more readily visible as the water is more tranquil. In certain localities a kind of froth is formed on the surface of the liquid similar to that which is seen in freshwater marshes; and M. Aimé thought at first that they were similar in composition, but analysis showed that this was not the case.

The formation of bubbles takes place at all times of the year, but always depends on the action of light. At sunrise it is difficult to collect sufficient gas for analysis, whilst on the evening of a clear day the quantity is considerable: that light was the sole cause of its production was proved by the fact, that plants, with their roots exposed to vivid light for a few minutes in fresh portions of sea-water, immediately produced bubbles on the surface of the leaves, while, if kept in the dark for a sufficient time, they disappeared. These plants were kept alive for two months and retained their powers: to render this observation more conclusive, some plants on the sea-shore were examined during several successive days; after sunset all the bubbles were removed by agitation, and the next morning they were again perceptible. No appreciable quantity of gas was produced during the night, or that which was formed was immediately dissolved by the water; whereas, as soon as the rays of light fell rather vividly on the plant, the formation of bubbles occurred.

Every variety of plant which M. Aimé examined was covered with bubbles on the surface, but some have the additional property of forming them internally; they are in general plants with soft leaves, such as the Ulvæ, Confervæ, &c.

Light increased the bulk of these bubbles, often sufficiently so to tear the leaf containing them; darkness diminished them much, but never completely destroyed them.

The gases of the external and interior bulbs, and that obtained morning and evening, were analysed, with the annexed results:—

	Oxygen.	Azote.
From the interior bubbles collected before sunrise	17	83
... ... after sunset	36	64
... exterior ... before sunrise	21	79
... ... in the sun at 10 A.M.	55	45

It is the last gas which forms a scum on the surface of the water when it is not disturbed; it is produced in so great quantity, that a litre [about two pints] has often been collected by M. Aimé by agitating the leaves of plants spread through five or six square feet of horizontal surface.

The author remarks, that it is evident, on inspecting the quantities of gases by analysis, that they depend on the hour, the weather, the season, and probably also on the latitude of the place. His analyses were made at Algiers in July and August, which are there the hottest months in the year.

Sea-water having, like fresh, the power of dissolving carbonic acid, the author thought that this gas must act its part in the aspiration and expiration of plants, but that it escaped detection by analysis on account of the solvent power of the water.

To determine this point, fresh plants with their roots were placed in sea-water contained in a well-stopped bottle; after an exposure in the shade of twelve hours, it was found that the air of the bottle contained a notable quantity of carbonic acid: the inverse experiment was also made, the apparatus containing the carbonic acid was placed in the sun; the disengagement of bubbles greatly increased, and a portion of carbonic acid was converted into oxygen. To discover whether the interior and exterior bubbles were derived from gases contained in the water or from those of the plant, a leaf of an ulva, containing an interior bubble, was put into a bottle of boiled sea-water, the leaf having been previously washed in cold water which had been boiled. The bottle was carefully stopped and exposed to diffused light for several hours; no bubbles were formed on the exterior of the leaf, but the interior bubble increased to two and a half times its size before the experiment.

The same experiment was repeated with other leaves, and exterior bubbles were collected; but in order to succeed, the direct rays of the sun must be present; for if the disengagement be not rapid, the water, deprived of air, absorbs the gas as it is liberated, and the bubbles are invisible.

It is to be observed, that in the preceding experiments the temperature was noted, and they were so managed as that the water possessed the same temperature in the shade and in the sunshine.—*Ann. de Chim. et de Phys., Août* 1841.

ON SOME NITROUS COMPOUNDS.

M. Kuhlman finds that anhydrous sulphuric acid enters into direct combination with nitric oxide, hyponitrous, nitrous and nitric acid, $NO5 + HO$, but he could not obtain a compound of anhydrous sulphuric with anhydrous nitric acid; but the affinity of anhydrous sulphuric acid for nitric acid with an atom of water is so great, that on placing nitric acid in a bottle surrounded with a cooling mixture, and passing the vapour of the sulphuric acid into it, the vapours of nitric acid are absorbed, so that sulphuric acid is found in the retort and its neck contains white crystals.

The several anhydrous compounds appear to form also corresponding hydrates.

The compound of SO^3 et $NO^5 + HO$ subjected to distillation gave at first NO^4 and much oxygen; on continuing the distillation there passes a substance which solidifies and forms white crystals in the neck of the retort, and the disengagement of hyponitric [nitrous] acid and oxygen ceases.

The liquid in the retort is of a deep yellow colour, but becomes colourless by cooling; when put in contact with water a violent disengagement of nitric oxide takes place, and sulphate of nitric oxide remains. The reaction appears to be explained by the decomposition of the nitric compound at a high temperature into the much more stable compound nitric oxide, with the disengagement of oxygen.

The fluoride of boron and the fluoride of silicon, when put into contact with nitric oxide, hyponitrous, nitrous and nitric acid, give rise to corresponding compounds. The fluoride of boron especially has great affinity for nitrous compounds; no one of these compounds forms combinations with nitrous oxide.

The fluoride of boron and the fluoride of silicon are absorbed in large quantity by concentrated nitric acid; their solutions emit white fumes: water separates boracic acid from the solution of fluoride of boron, but does not separate silicon from that of fluoride of silicon.

When the last-mentioned fluoride is saturated with the alcalies, the silica is not precipitated, and the fluoride of silicon dissolved appears to enter into the composition of the saline matter obtained. The compound of fluoride of boron and nitric oxide appears to be the most stable, for on heating the compounds of hyponitrous acid, oxygen is disengaged. The perchloride of tin gives with nitric oxide a crystalline compound which readily distils, and is decomposed by water. Analogous compounds are obtained with the perchloride of tin and hyponitrous, nitrous and nitric acid; but the reaction of perchloride of tin on the acids is accompanied with an abundant disengagement of chlorine and nitrous acid, and after the distillation oxide of tin remains.

The most oxigenated nitrous compounds appear to be reduced by heat to the state of the compounds of chlorides with nitric oxide, which is the most stable of all the compounds in question.—*Ann. de Chim. et de Phys., Mai* 1841.

ON ARSENIURETTED HYDROGEN. BY M. HENRY ROSE.

A solution of bichloride of mercury is employed to detect the presence of arseniuretted hydrogen and to remove all traces of it; this gas occasions on the solution a yellow precipitate with a brown tint, and this character distinguishes it from that formed by phosphuretted hydrogen in the same solution.

The composition of this precipitate is quite unknown; Stromeyer appears to be the only chemist who has examined it. According to him, arseniuretted hydrogen forms with the bichloride of mercury, first arsenious acid and protochloride of mercury, and then an amalgam of arsenic and mercury. The precipitate, when treated with much water, decomposes after a certain time; it becomes black, and eventually consists entirely of finely divided mercury, the supernatant liquor containing arsenious and hydrochloric acids.

This decomposition is perfectly analogous to that which water effects in the precipitate formed by phosphuretted hydrogen in the solutions of bichloride of mercury, this precipitate changing into mercury, phosphorous and hydrochloric acids. This decomposition occurs more rapidly than that with the arsenical compound.

Both precipitates undergo similar changes by the action of nitric acid; by the aid of a gentle heat they are converted into protochloride of mercury, and the arsenic and phosphorus are at the same time oxidized.

This similar reaction seems to indicate also a similarity of composition, and this is confirmed by analysis; the arsenical precipitate, according to M. Rose, is represented by $As^2 Hg^3 + 3 Hg Cl^2$.

The phosphuretted precipitate differs from the above only in containing three atoms of water, whereas the arsenical one is anhydrous. These two precipitates also differ in the mode in which they are acted upon by heat; the chloro-phosphuret of mercury contains exactly so much water that its hydrogen converts all the chlorine into hydrochloric acid, and its oxygen all the phosphorus into phosphorous acid. The chloro-arseniuret of mercury, on the contrary, does not yield any gaseous product under the same circumstances; it sublimes without any residue, decomposing into protochloride of mercury and metallic arsenic. There sublimes, at the same time, a small quantity of a reddish substance which consists of mercury, chlorine and arsenic, and which is probably merely the substance sublimed without decomposition. Sometimes the sublimate contains a little metallic mercury. The analysis of the arsenical precipitate and its action on water fully confirm the composition of arseniuretted hydrogen as stated by MM. Dumas and Soubeiran.

The precipitate formed by antimoniuretted hydrogen gas, in the solution of bichloride of mercury, differs from both the preceding in composition.—*Ann. de Chim. et de Phys., Juillet* 1841.

NATIVE BROMIDE OF SILVER, AND ANALYSES.

M. Berthier says, that in the district of Plateros, seventeen leagues from Zacatecas in Mexico, silver ore is found in two different states: first, native silver; and secondly and principally in a state of combination in small olive-green or yellowish crystals, supposed to be chloride, but which he found to be bromide of silver. According to M. Duport, from whom M. Berthier received these specimens, this substance is not rare in Mexico, but occurs frequently in fine cubic and octahedral crystals.

The specimen examined by M. Berthier was from San Onofe. It was compact, of a slightly reddish gray colour; fracture uneven; splendent; penetrated with small cavities, some of which were partially filled with a substance of a dull pale yellow colour, and which the miners call oxide of lead; other cavities contain very small imperfect crystals, which are brilliant, and of a pale olive-green colour, and have the appearance of chloride of silver. This specimen was very rich, for it yielded 0·0688 of silver, and contained 0·45 of carbonate of lead, which, intimately mixed with quartz and a little oxide of iron, formed the principal portion of the mass.

M. Berthier has also found this mineral among the silver ores of Huelgoeth, department of Finistère in France. Two specimens were obtained by him: the first of these is described as being porous or scoriform, containing white quartz imbedded in foliated hydrate of iron. On the edges of the foliated iron ore the naked eye could distinguish small cubic grains of a pearl-white colour, which had all the characters of chloride of silver.

The second specimen had the appearance of compact oxide of iron, containing here and there milk-white quartz; it was throughout impregnated with chloride of silver, which occasionally appeared in the form of very small brilliant crystals. To analyse this mineral, 10 grammes were first treated with ammonia and heat to dissolve the chloride of silver, and afterwards by boiling hydrochloric acid to dissolve the oxide of iron; this acid also dissolved a certain portion of lead, which probably was in the state of phosphate. The quartzose residue weighed 3·26 grammes; it contained 0·17 gramme of silver, which must have been in the metallic state: the ammoniacal solution gave by boiling and saturation with nitric acid, 1·84 gramme of chloride of silver, which, supposing it to be pure, contained 1·40 gramme of silver, which, added to 0·17 gramme remaining in the quartz, gives a total of 1·57 gramme; a result which differed so very little from that obtained by assaying, as to prove the absence of bromide of silver, and that this was the case was confirmed by additional experiments.

After this a third specimen was received from Huelgoeth; it was very small, but as rich as the foregoing, and in it there were distinguishable, besides granular cubic crystals of chloride of silver, other grains of an olive-green colour, which had exactly the same appearance as the bromide of Plateros, and the following experiments proved the presence of this substance.

Five grammes of the pulverized mineral were boiled in a solution of oxalic acid, until the oxide of iron was perfectly dissolved: the residue weighed about a gramme, and it evidently contained a mixture of canary-yellow and white grains. It was digested in hot solution of ammonia until all the yellow powder disappeared; it required a large quantity of the alkali for this purpose, which would not have been the case to dissolve pure chloride. The solution was gradually saturated with nitric acid, and it was observed that the successive deposits formed had an evident yellow tint, but gradually diminishing in intensity, except the last, which were white. The yellow deposits were collected and examined in the following manner:—A portion was treated with chlorine and æther; the æther became of a yellow colour. Another portion was dissolved in ammonia, hydrosulphate of ammonia was added to the solution, and the black precipitate formed was separated, and was found to be pure sulphuret of silver. The liquor was concentrated by exposure to the air, and filtered to separate the sulphur which was deposited; a little potash was then added, and it was evaporated to dryness; acetic acid was added to saturate the excess of potash, and it was again dried.

To determine whether the saline residue contained a bromide, a small portion of it was treated in a tube with pure nitric acid, and a yellow liquid was immediately obtained. Another portion was mixed with peroxide of manganese, and the mixture was placed in a glass tube; a few drops of concentrated sulphuric acid were added, and when gently heated, red vapours were immediately disengaged, and after some time there were deposited on the sides of the tube small drops of a red liquid. The existence of bromine was therefore evident, and it was proved that the bromide was unmixed with iodide. Bromide of silver appears to be rare at Huelgoeth; but it may be readily distinguished from the chloride by its greenish or canary-yellow colour, which is characteristic of it. It is remarkable that it occurs with the chloride in the same specimens, but without there being an intimate mixture of the two substances.—*Ann. de Chim. et de Phys., Août* 1841.

METEOROLOGICAL OBSERVATIONS FOR NOVEMBER 1841.

Chiswick.—November 1. Rain. 2. Hazy: fine: foggy. 3, 4. Foggy: hazy. 5. Hazy. 6—8. Hazy: very fine. 9. Overcast: windy at night. 10. Overcast and fine. 11. Very fine. 12. Showery. 13. Cloudy: clear and fine: rain. 14. Rain: stormy: clear. 15. Frosty: hazy: sleet. 16. Frosty: clear. 17. Sharp frost: clear. 18. Stormy with sleet: cloudy and cold. 19. Densely overcast: rain: clear. 20. Foggy: overcast. 21. Stormy with rain. 22. Rain: clear at night. 23. Clear: overcast. 24, 25. Clear and fine. 26. Frosty and foggy. 27. Foggy: rain at night. 28. Hazy and damp: heavy rain at night. 29. Heavy rain. 30. Boisterous: barometer very low.

Boston.—Nov. 1. Cloudy: rain P.M. 2. Cloudy. 3, 4. Foggy. 5, 6. Cloudy. 7—9. Fine. 10. Cloudy. 11. Fine: rain early A.M. 12. Cloudy: rain early A.M. 13. Fine. 14. Stormy. 15—17. Fine. 18. Cloudy. 19. Snow and stormy: rain A.M. and P.M. 20. Foggy: rain P.M. 21. Rain. 22. Fine: rain P.M. 23—26. Fine. 27. Cloudy: rain early A.M.: rain P.M. 28. Cloudy. 29. Cloudy: rain early A.M. 30. Rain and stormy: rain early A.M.

Sandwick Manse, Orkney.—Nov. 1. Cloudy: clear. 2. Frost A.M.: clear. 3, 4. Clear. 5. Clear: aurora borealis. 6. Rain. 7, 8. Showers. 9. Rain. 10. Clear shower: aurora borealis. 11. Cloudy. 12. Cloudy: rain. 13. Snow showers. 14—17. Snow lying: showers. 18, 19. Snow lying. 20. Snow lying: rain. 21. Showers. 22. Rain. 23, 24. Fine. 25. Frost: clear. 26. Showers. 27, 28. Frost. 29, 30. Rain.

Applegarth Manse, Dumfries-shire.—Nov. 1. Slight rain. 2. Hard frost. 3, 4. Slight frost. 5. Moist after frost. 6. Cloudy A.M.: rain P.M. 7, 8. Dull: showery P.M. 9, 10. Dark and squally: rain P.M. 11. Showery but mild. 12. Showery but boisterous. 13. Showery: snow on hills. 14. Snow shower: frost all day. 15. Hard frost. 16. Hard frost: clear and fine. 17. Hard frost: cloudy. 18. Milder: slight snow. 19. Thaw: fine and mild. 20, 21. Wet. 22. Fair but dull. 23, 24. Showery and squally. 25. Frosty and clear. 26. Frosty but cloudy. 27. Rain. 28. Drizzling. 29, 30. Very wet and squally.

Sun shone out 20 days. Rain fell 17 days. Frost 9 days. Snow 2 days.

Wind North 1 day. North-north-east 1 day. North east 5 days. East-north-east 2 days. East 6 days. South-east 1½ day. South 2 days. South-south-west 1 day. South-west 3½ days. West-south-west 1 day. West 3 days. West-north-west 1 day. North-west 1 day. North-north-west 1 day.

Calm 9 days. Moderate 9 days. Brisk 6 days. Strong breeze 4 days. Boisterous 2 days.

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary*, Mr. Roberton; *by* Mr. Thompson, *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *by* Mr. Dunbar, *at Applegarth Manse*, DUMFRIES-SHIRE, *and at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.								Thermometer.											Wind.					Rain.					Dew-point.
1841. Nov.	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8¾ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R.S. 9 a.m.
		Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mm.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.											
1.	29·948	30·063	29·901	29·42	29·80	29·99	29·92	30·13	46·5	53·2	46·3	52	45	47·5	49½	42	47	41	w.	w.	calm	nnw.	w.	·175	·10	......	......	·9	46
2.	30·238	30·226	30·187	29·73	30·11	30·11	30·22	30·22	47·2	50·3	46·2	53	37	45	45	28	39	41	n.	ne.	calm	nne.	se.	·072	......	·13	......	......	45
3.	30·368	30·369	30·309	29·84	30·15	30·21	30·23	30·28	48·3	51·4	46·6	55	36	43·5	49	33	45	45½	n.	e.	calm	e.	s.	......	......	......	......	......	46
4.	30·424	30·372	30·340	29·94	30·29	30·26	30·30	30·31	47·3	53·5	46·2	48	40	46	47	33	47	45	e.	e.	calm	ene.	se.	......	......	......	......	......	45
5.	30·400	30·379	30·341	29·87	30·20	30·19	30·27	30·10	47·0	50·0	46·0	50	44	47	45½	32	42	43	se.	se.	calm	e.	se.	......	......	......	......	......	45
☾ 6.	30·450	30·391	30·366	29·92	30·14	30·04	30·00	29·92	48·8	50·9	45·4	58	35	47	52	42	50	50	s.	s.	sw.	s.	s.	......	......	......	0·10	·12	46
7.	30·436	30·391	30·351	29·90	30·14	30·13	29·98	29·94	46·5	55·6	44·4	53	37	43	53	48	48½	47	s.	w.	calm	ssw.	w.	......	......	......	......	·14	46
8.	30·370	30·333	30·306	29·75	30·05	30·08	29·94	29·96	46·3	52·8	43·3	51	39	44	51	46	47	46	s.	sw.	calm	sw.	w.	......	·01	......	......	·55	46
9.	30·328	30·291	30·198	29·73	29·99	29·79	29·64	29·44	44·3	51·7	42·6	50	45	40	49	46½	51	48½	s.	sw.	calm	se&sw	sw.	......	......	......	......	·56	44
10.	30·180	30·127	30·061	29·60	29·78	29·72	29·68	29·64	50·3	51·7	44·6	53	44	48·5	50	48½	44	43½	w.	w.	calm	sw.	nw.	......	......	......	......	......	45
11.	30·018	29·976	29·934	29·45	29·80	29·77	29·76	29·84	50·3	53·8	48·7	55	35	49	49	40	37	44	s.	w.	calm	wnw.	nw.	......	·40	·14	......	·10	46
12.	29·584	29·561	29·515	29·20	29·31	29·18	29·42	29·33	50·8	55·5	45·2	63	32	42	44	34½	38	38	s.	w.	calm	w.	e.	·416	·06	·06	......	......	47
● 13.	29·468	29·431	29·102	28·94	29·19	29·14	29·44	29·54	42·2	52·7	40·0	48	34	39	40½	29½	35	34	sw.	w.	w.	w.	e.	......	·27	·07	......	·20	41
14.	29·044	29·480	29·050	28·71	29·54	29·44	29·56	29·51	38·3	48·6	36·4	41	23	42	40	32½	35	36	wnw.	nw.	ne.	n.	nw.	·258	·04	·05	......	·06	36
15.	29·486	29·479	29·307	29·10	29·35	29·34	29·50	29·55	34·3	42·5	32·6	37	25	30	42	25½	31	36	sw.	se.	calm	ne.	n.	......	·04	......	0·41	·08	32
16.	29·454	29·588	29·431	29·11	29·37	29·43	29·50	29·51	33·4	39·3	32·8	35	15	27·5	37½	22	36½	32	w.	w.	calm	ne.	n.	......	......	......	......	·07	32
17.	29·706	29·700	29·414	29·38	29·49	29·36	29·52	29·49	29·7	35·9	28·2	39	32	27·5	32½	18	34	30	s.	se.	calm	ne.	nw.	......	·12	......	......	·06	29
18.	29·280	29·666	29·316	29·05	29·29	29·38	29·33	29·46	34·3	38·3	30·3	42	25	38	38	27	35	36½	n.	ne.	calm	e.	s.	·122	·12	......	......	·09	33
19.	29·462	29·474	29·266	29·21	29·28	29·20	29·40	29·25	43·7	44·8	34·8	47	27	34	39½	33	36	34	se.	se.	se.	e.	s.	·063	·24	......	......	......	40
20.	29·366	29·375	29·195	29·07	29·13	28·88	29·16	29·10	39·5	48·4	36·8	52	40	33	39	29	36	37½	s.	s.	calm	e.	se.	·183	·18	·15	......	......	37
☽ 21.	29·376	29·382	29·291	28·99	29·08	29·00	28·97	29·10	45·7	50·8	40·0	55	51	40	40	34	42	37½	sse.	s.	calm	e.	w.	·183	·30	·06	......	·26	41
22.	29·204	29·455	29·170	28·69	28·82	29·01	29·06	29·09	54·4	56·2	46·7	58	31	54	43½	34	35	39	s.	sw.	sw.	w.	se.	·166	·21	·40	0·70	......	49
23.	29·694	29·736	29·656	29·16	29·23	29·42	29·19	29·38	41·3	57·4	39·8	47	26	37	43½	33½	37	39	s.	s.	calm	wsw.	s.	·188	......	·02	......	·30	43
24.	29·916	29·957	29·881	29·48	29·50	29·59	29·46	29·56	38·0	48·2	37·6	45	27	36	43	35½	35	38	s.	nw.	calm	sw.	w.	......	......	......	......	......	38
25.	29·800	29·913	29·783	29·42	29·65	29·74	29·71	29·90	38·3	45·6	37·3	44	23	35	43½	29	38	37½	nw.	nw.	calm	nw.	nw.	......	......	......	......	......	38
26.	29·904	29·894	29·798	29·56	29·73	29·65	29·85	29·83	33·2	43·6	32·7	47	37	28	34	23	35	33	nw.	e.	calm	ne.	n.	......	·02	......	......	·09	35
27.	29·704	29·697	29·583	29·41	29·54	29·47	29·82	29·77	44·3	45·5	32·6	51	42	38	41	32½	31	32½	se.	sw.	s.	ene.	ne.	......	·11	·05	......	·03	39
○ 28.	29·666	29·634	29·464	29·28	29·40	29·20	29·63	29·42	45·3	52·2	44·6	54	43	41·5	45	38½	38	40½	s.	sw.	calm	ne.	e.	·111	·60	·15	......	......	43
29.	29·236	29·176	948	28·70	28·82	28·65	28·98	28·72	50·4	54·2	45·2	58	49	51	47½	39	43	40	s.	sw.	calm	s.	e.	·575	·54	·25	1·16	·30	48
30.	28·952	29·250	28·845	28·38	28·44	28·70	28·57	28·79	53·8	58·6	51·2	54	40	50	47	41	44	42	sse.	sw.	sw.	se.	e.	·338	·05	·58	......	·41	52
Mean.	29·782	29·825	29·710	29·33	29·866	29·535	29·600	29·600	43·9	49·8	40·8	49·90	35·30	40·8	44	34·3	39·73	39·58						Sum. 2·850	3·41	2·11	2·37	3·51	Mean. 42

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XIII. *On Mixed Gases. By* JAMES IVORY, *K.H., M.A., Hon. M.R.I.A., Instit. Reg. Sc. Paris, et Reg. Soc. Götting. Corresp.*[*]

THE subject of mixed gases is of great importance in many physical researches; and, in particular, the atmosphere being a mixture of gases, the inquiry into its constitution essentially depends upon the knowledge of the laws according to which elastic fluids combine in the same volume. But although the subject has been often discussed, it is still embarrassed by several difficulties which it is very desirable to clear up.

Suppose a gas contained in an envelop, the pressure, density, and volume being p, ρ, v, and the temperature θ: conceive that the envelop is extended on all sides, the enlarged volume being V; in consequence the gas will expand, and after a little time all motion will cease, and the elastic fluid will assume a state of rest and equilibrium, exerting the same pressure at every point of the envelop. Now, when the volume of the gas was v, the pressure was p; wherefore when the volume is enlarged to V, the pressure will be lessened to $p \cdot \frac{v}{\mathrm{V}}$, which is the exact measure of the elastic force of the expanded fluid, and the pressure at every point of the envelop.

Let us now take a different gas, which has no chemical action on the other, the pressure and density being p', v', the temperature θ as before, and the volume $v' = \mathrm{V} - v$, so that the sum of the two volumes v and v' is equal to the volume of the envelop. If this new gas be introduced into

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[*] Communicated by the Author.

the same envelop as the first, it will expand by its elasticity, and will force its way by continued agitations through the obstruction of the quiescent gas, in like manner as this expanded through a vacuum; the only difference being that the agitations will continue longer in one case than in the other. After a time, as there is no mutual action of the particles of the two fluids, all the agitations will cease; the first gas will resume its state of rest; and the new gas will be diffused with a uniform density and pressure through the envelop.

We have next to inquire, What is the elastic force of the combined gases, or which is the same thing, what is the external pressure that must be applied at the common surface, in order to confine them both without change of volume. When the first gas alone was contained in the envelop, we have found that the elastic force of the expanded gas was $p \cdot \frac{v}{V}$: but when both gases are present in the envelop, the pressure $p \cdot \frac{v}{V}$ upon the space of the envelop equal to the unit of surface, which space is equally in contact with both fluids, will cause a compression of the second gas as well as of the first. As the two fluids are separately in equilibrium and have no mutual action upon another, it is easy to discover that the same compressive force applied on the same space of the common surface will transmit pressures equal to $p \cdot \frac{v}{V}$ to the particles of both fluids. But the elastic force of the particles of the second gas being p' when the volume is v', it will be $p' \cdot \frac{v'}{V}$, when the volume is V; to which adding $p \cdot \frac{v}{V}$, the increase of elasticity caused by the action of the first gas on the envelop, the sum $p' \cdot \frac{v'}{V} + p \cdot \frac{v}{V}$ will be the whole elastic force of the particles of the second gas. As the two fluids are similar and interchangeable in their conditions, the like reasoning will show that the whole elastic force of the particles of the first gas, caused by the action of both gases on the envelop, is $p \cdot \frac{v}{V} + p' \cdot \frac{v'}{V}$. Wherefore, if P represent the elastic force of the mixture, we shall have

$$P = p \cdot \frac{v}{V} + p' \cdot \frac{v'}{V}.$$

The foregoing view is in accordance with that part of the theory of Dalton, now so far universally adopted, which regards the separate condition of different gases, not acting chemically upon one another, when they are confined in the same envelop: every gas is in equilibrium by the elastic force of its own particles, as would be the case if it occupied by itself the whole space of the envelop. It also agrees with the fact that no change of temperature is produced by the expansion of the gases in mixing; for the agitations caused by the second gas in forcing its way through the first one, which is quiescent and in equilibrium, are attended by alternate condensations and dilatations that produce neither gain nor loss of heat.

In what regards the elasticity of the mixture, the result obtained above likewise agrees with the usual theory, but not with the opinion of Dalton. According to the usual theory the elastic force of the mixture is equal to the sum of the elasticities of the constituent gases, supposing that these elasticities are reduced to the volume of the mixture; which is equivalent to what P stands for.

When several elastic fluids are mixed in the same envelop, Dalton is of opinion that the elastic force which every one impresses on the envelop is independent of the action of all the rest, and the same it would be if these were not present. But in forming this opinion the illustrious philosopher has not remarked that, as every space of the envelop is in contact with all the fluids, a pressure on any such space, produced by whatever cause, will necessarily be transmitted with equal energy to all the particles of the several fluids. Thus, when all the conditions of the question are taken into account, it appears that every particle within the envelop has the same elastic energy impressed upon it as in the case of a simple gas: if it were not so there could not possibly be an equilibrium of the whole mass. In corroboration of this it may be observed that only one sound is propagated through a mixture of gases such as we have been considering; whereas if every gas were separately in equilibrium, without any mutual action of the several fluids, there would be as many different sounds moving with different velocities as there are gases in the mixture.

XIV. *Notice of the Discovery of some remains of the Ichthyosaurus in Ireland. By* James Bryce, *Jun., M.A., F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In May 1831 I published in your Magazine a short notice of the discovery of remains of the Plesiosaurus in this neighbourhood*. Since that time detached saurian vertebræ

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* They were discovered by Mr. John H. Smythe of Carnmoney.

have been found in various places, in beds between the chalk and new red sandstone. These were generally so small and badly characterized, that I found it difficult to determine the genus to which they belonged. Within the last few months, however, some well-marked remains have been discovered. They were found in the lower greensand at Woodburn near Carrickfergus, by Mr. Wm. Young of this town; and consisted of a large dorsal vertebra and some smaller ones from the extremities, but in such a position as rendered it doubtful whether they belonged to the same individual. They were easily recognized to be vertebræ of an Ichthyosaurus, by the compressed form of the body, the double concavity of the articulating surfaces, by the small pits on each side of the spinal canal, and by the articulating tubercles for the attachment of the bifurcate rib. The length of the largest vertebra is an inch and a half, and the diameter of its end four inches.

So far as I am aware, this is the only instance yet recorded of the discovery of Ichthyosaurian remains in this country. In this case, and in that referred to above, the discoveries were due to the zeal and the knowledge of two young gentlemen, who were inspired by myself with a love for this delightful study; and this circumstance, while gratifying to me, is one of the many arguments which might be adduced in favour of turning the attention, at an early period, to the cultivation of the "Science of Observation."

Belfast, Jan. 20, 1842.

XV. *On the Method of investigating generally the partial Differential Equations applicable to the Motion of Fluids. By the Rev.* JAMES CHALLIS, *M.A., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge*[*].

I PROPOSE in this communication to generalize the method I employed in the Number of this Journal for last September (S. 3, vol. xix. p. 229), in the solution of a problem of hydrodynamics. Although that solution has not received the assent of a distinguished mathematician, I do not despair of eventually establishing the correctness of my views, and showing the necessity of their being adopted before any considerable advance can be made in the mathematical theory of fluid motion. It is very possible that in entering upon a new train of reasoning I may have failed to place it in the clearest point of view; and, in fact, I am now prepared to point out the source of some portion of the obscurity. I have argued

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[*] Communicated by the Author.

that the partial differential equations of fluid motion (namely, those in which the principal variable is usually designated by ϕ) cannot be generally applied, unless the variation of the coordinates from one point to another of space at a given time, be restricted to take place in the direction of the motion. The necessity for this limitation, which might reasonably be objected to if the equations were general, arises from the limited nature of those equations. Were we in possession of the most general equations no such limitation would be required. In support of this view I proceed to point out the method of finding the general partial differential equations of fluid motion.

Let the pressure (p) and density (ρ) of the fluid be related to each other by the equation $p = k\rho$. Let u, v, w be respectively the resolved parts of the velocity, in the direction of the axes of coordinates, of a particle whose coordinates at the time t are x, y, z. Putting for shortness sake P for k. Nap. log ρ, and supposing no extraneous force to act, we have the known equations

$$\frac{d\rho}{dt} + \frac{d.\rho u}{dx} + \frac{d.\rho v}{dy} + \frac{d.\rho w}{dz} = 0, \quad \ldots\ldots\ldots \quad (1.)$$

$$d\mathrm{P} + \left(\frac{du}{dt}\right) dx + \left(\frac{dv}{dt}\right) dy + \left(\frac{dw}{dt}\right) dz = 0. \quad (2.)$$

To render these equations applicable to instances of fluid motion, it is requisite to transform them into others containing partial differential coefficients of a single variable. This has been done in the case in which $u\,dx + v\,dy + w\,dz$ is an exact differential, with respect to space, of a function of x, y, z, and t, by substituting $d\phi$ for this quantity. The views I have detailed in the Number of this Magazine for last June (S. 3, vol. xviii. p. 477), led me to infer that this is only a limited case, and that in every instance of fluid motion essentially different from that of a solid, $u\,dx + v\,dy + w\,dz$ may be made integrable by a factor. Independently of those views, it is clear that the reasoning is not conducted in the most general manner by supposing that quantity integrable *per se*, and it is remarkable that no one hitherto has traced the consequences of introducing a factor.

Let us therefore suppose $\frac{1}{\mathrm{N}}$ to be the factor, and let

$$\frac{u}{\mathrm{N}}\,dx + \frac{v}{\mathrm{N}}\,dy + \frac{w}{\mathrm{N}}\,dz = d.\,\phi\,(x, y, z, t).$$

I have already proved (Philosophical Magazine for September, p. 230) that $\frac{u}{\mathrm{N}}\,dx + \frac{v}{\mathrm{N}}\,dy + \frac{w}{\mathrm{N}}\,dz = 0$ is the differential equation of a surface of *displacement*, that is, a surface which cuts at right angles the directions of motion of the particles through which it passes. By integrating, the equation of every such surface becomes

$$\phi\ (x, y, z, t) = 0.$$

It is unnecessary to add an arbitrary function of the time, as it may be supposed included in ϕ. Now the coordinates of a given fluid particle, which were x, y, z at the time t, become $x+u\,dt$, $y+v\,dt$, $z+w\,dt$, at the time $t+dt$, dt being indefinitely small, and these must ultimately be coordinates of a surface of displacement. Hence

$$\phi\,(x+u\,dt,\ y+v\,dt,\ z+w\,dt,\ t+dt) = 0.$$

Consequently, putting ϕ for $\phi\,(x, y, z, t)$, we have

$$\phi + \frac{d\phi}{dx}u\,dt + \frac{d\phi}{dy}v\,dt + \frac{d\phi}{dz}w\,dt + \frac{d\phi}{dt}dt = 0,$$

which, since $\phi = 0$, gives

$$\frac{d\phi}{dt} + u\,.\,\frac{d\phi}{dx} + v\,.\,\frac{d\phi}{dy} + w\,.\,\frac{d\phi}{dz} = 0.\ .\ .\ .\ .\ \ (3.)$$

Again, from the equation $d\phi = \frac{u}{\mathrm{N}}\,dx + \frac{v}{\mathrm{N}}\,dy + \frac{w}{\mathrm{N}}\,dz$, it follows that

$$u = \mathrm{N}\ \frac{d\phi}{dx};\ v = \mathrm{N}\,\frac{d\phi}{dy};\ \ w = \mathrm{N}\,\frac{d\phi}{dz}.$$

Hence, substituting in (3.), we obtain

$$\frac{d\phi}{dt} + \mathrm{N}\left(\frac{d\phi^2}{dx^2} + \frac{d\phi^2}{dy^2} + \frac{d\phi^2}{dz^2}\right) = 0.\ .\ .\ .\ .\ \ (4.)$$

This equation serves to find N when ϕ is known as a function of x, y, z, and t. In general, when the preceding values of u, v and w are substituted in the equations (1.) and (2.), by means of these and the equation (4.) N and ρ may be eliminated, and the resulting equation in ϕ, x, y, z, and t is the general partial differential equation which it was required to find. The eliminations are far too complicated to be introduced here, and I must therefore content myself with thus indicating the process. For incompressible fluids the equation (1.) is simply

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0;$$

and by substituting the values of u, v, and w, it becomes

$$\mathrm{N}\left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2}\right) + \frac{d\mathrm{N}}{dx}\cdot\frac{d\phi}{dx} + \frac{d\mathrm{N}}{dy}\cdot\frac{d\phi}{dy}$$
$$+ \frac{d\mathrm{N}}{dz}\cdot\frac{d\phi}{dz} = 0.$$

The elimination of N between this equation and equation (4.) presents no difficulty.

I proceed now to draw some conclusions from the equation (2.), after substituting in it the expressions for u, v, and w. By the substitution the equation becomes

$$-d\mathrm{P} = \left(\frac{d\,.\,\mathrm{N}\frac{d\,\phi}{d\,x}}{d\,t}\right)d\,x + \left(\frac{d\,.\,\mathrm{N}\frac{d\,\phi}{d\,y}}{d\,t}\right)d\,y + \left(\frac{d\,.\,\mathrm{N}\frac{d\,\phi}{d\,z}}{d\,t}\right)dz$$

$$= \mathrm{N}d\,.\,\frac{d\,\phi}{d\,t} + \left(\frac{d\,\mathrm{N}}{d\,t}\right)d\,\phi + \frac{\mathrm{N}^2}{2}\,d\,.\left(\frac{d\,\phi^2}{d\,x^2} + \frac{d\,\phi^2}{d\,y^2} + \frac{d\,\phi^2}{d\,z^2}\right).$$

Hence by integrating,

$$f(t) - \mathrm{P} = \mathrm{N}\frac{d\,\phi}{d\,t} + \int\left(\frac{d\,\mathrm{N}}{d\,t}\right)d\,\phi + \frac{\mathrm{N}^2}{2}\left(\frac{d\,\phi^2}{d\,x^2} + \frac{d\,\phi^2}{d\,y^2} + \frac{d\,\phi^2}{d\,z^2}\right)$$

$$-\int\left\{\frac{d\,\phi}{d\,t} + \mathrm{N}\left(\frac{d\,\phi^2}{d\,x^3} + \frac{d\,\phi^2}{d\,y^2} + \frac{d\,\phi^2}{d\,z^2}\right)\right\}d\,\mathrm{N} \;\ldots\; (5.)$$

The equation (4.) makes the last term of this equation disappear.

First, it may be remarked that if N be a function of t, the equation becomes

$$f(t) - \mathrm{P} = \frac{d\,.\,\mathrm{N}\,\phi}{d\,t} + \frac{\mathrm{N}^2}{2}\,.\left(\frac{d\,\phi^2}{d\,x^2} + \frac{d\,\phi^2}{d\,y^2} + \frac{d\,\phi^2}{d\,z^2}\right);$$

and if $\mathrm{N}\,\phi = \psi$, by differentiating with respect to space, $\mathrm{N}\,d\,\phi = d\,\psi$; so that we have

$$f(t) - \mathrm{P} = \frac{d\,\psi}{d\,t} + \frac{1}{2}\,.\left(\frac{d\,\psi^2}{d\,x^2} + \frac{d\,\psi^2}{d\,y^2} + \frac{d\,\psi^2}{d\,z^2}\right),$$

which is the equation that is obtained when $u\,d\,x + v\,d\,y + w\,d\,z$ is an exact differential.

Next let the motion be supposed very small. We may then substitute $\frac{d\,\mathrm{N}}{d\,t}$ for $\left(\frac{d\,\mathrm{N}}{d\,t}\right)$ in the expression for $-d\,\mathrm{P}$, and neglecting the last term as being of the second order, we have

$$-d\,\mathrm{P} = \mathrm{N}\,d\,.\frac{d\,\phi}{d\,t} + \frac{d\,\mathrm{N}}{d\,t}\,d\,\phi = \mathrm{N}\,.\,\frac{d\,.\,d\,\phi}{d\,t} + \frac{d\,\mathrm{N}}{d\,t}d\,\phi = \frac{d\,.\,\mathrm{N}\,d\,\phi}{d\,t}.$$

Hence by integrating,

$$f(t) - \mathrm{P} = \frac{d\,.\int\mathrm{N}\,d\,\phi}{d\,t}. \;\ldots\ldots\; (6.)$$

At the same time equation (1.) to the same degree of approxition becomes,

$$\frac{1}{k}\,.\,\frac{d\,\mathrm{P}}{dt} + \frac{d\,u}{d\,x} + \frac{d\,v}{d\,y} + \frac{d\,w}{d\,z} = 0. \;\ldots\; (7.)$$

It will be convenient in the subsequent reasoning to adopt some notation to designate when the differentiation of a quantity is with respect to space, the time being given. I shall continue to indicate a complete differential coefficient with respect to time and space by putting it in brackets, but a *differential* in brackets will show that the differentiation is with respect to *space* only. Thus $\frac{(d\phi)}{dt}$ is a fraction the numerator of which is the increment of ϕ with respect to space, and the denominator is the increment of the time. This being premised, from equation (6.) we obtain, by differentiating with respect to time,

$$f'(t) - \frac{dP}{dt} = \frac{d^2.\int N(d\phi)}{dt^2},$$

and by differentiating this equation with respect to space, and dividing by a given increment of the time,

$$-\frac{\left(d.\frac{dP}{dt}\right)}{dt} = \frac{d^2.N\frac{(d\phi)}{dt}}{dt^2}.$$

$$\text{But }\ N\frac{(d\phi)}{dt} = N\left(\frac{d\phi}{dx}.\frac{dx}{dt} + \frac{d\phi}{dy}.\frac{dy}{dt} + \frac{d\phi}{dz}.\frac{dz}{dt}\right)$$

$$= N^2\left(\frac{d\phi^2}{dx^2} + \frac{d\phi^2}{dy^2} + \frac{d\phi^2}{dz^2}\right) = -N\frac{d\phi}{dt} \text{ by (4.).}$$

Therefore,

$$\frac{\left(d.\frac{dP}{dt}\right)}{dt} = \frac{d^2.N\frac{d\phi}{dt}}{dt^2} = N.\frac{d^3\phi}{dt^3} + \frac{d^2N}{dt^2}.\frac{d\phi}{dt} + 2\frac{dN}{dt}.\frac{d^2\phi}{dt^2}.$$

Again, from (7.) a value of $\frac{\left(d.\frac{dP}{dt}\right)}{dt}$, expressed in partial differential coefficients of N and ϕ, may be obtained. Equating this to the value above, and eliminating N by means of (4.), the result is the required equation in partial differential coefficients of ϕ. This equation is of the third order, and is very complicated, but, I believe, admits of much simplification. My present limits do not allow of pursuing this inquiry further, and I am desirous of making one more deduction from equation (2.)

It is shown above that $\frac{d\phi}{dt} + \frac{(d\phi)}{dt} = 0$. But with reference to the application I am about to make of this equation, it is particularly to be remarked that the variation of the coordinates in $(d\,\phi)$ is from one point to another *in the direction of the motion.* This is evident from equation (3.), by substituting $\frac{d\,x}{d\,t}, \frac{d\,y}{d\,t}, \frac{d\,z}{d\,t}$, respectively for u, v, and w. Now by the adopted notation,

$$\left(\frac{d\,\mathrm{N}}{d\,t}\right) = \frac{d\,\mathrm{N}}{d\,t} + \frac{(d\,\mathrm{N})}{d\,t}.$$

Hence
$$\left(\frac{d\,\mathrm{N}}{d\,t}\right)(d\,\phi) = \frac{d\,\mathrm{N}}{d\,t}(d\,\phi) + \frac{(d\,\mathrm{N})}{d\,t}(d\,\phi)$$
$$= \frac{d\,\mathrm{N}}{d\,t}(d\,\phi) + (d\,\mathrm{N})\frac{(d\,\phi)}{d\,t}$$
$$= \frac{d\,\mathrm{N}}{d\,t}(d\,\phi) - (d\,\mathrm{N})\frac{d\,\phi}{d\,t}.$$

By the last substitution the limitation as to the direction of the variation of coordinates is introduced. From this equation it follows that

$$\int\left(\frac{d\,\mathrm{N}}{d\,t}\right)(d\,\phi) = \int\frac{d\,\mathrm{N}}{d\,t}(d\,\phi) - \mathrm{N}\frac{d\,\phi}{d\,t} + \int \mathrm{N}\cdot\frac{d(d\,\phi)}{d\,t}$$
$$= -\,\mathrm{N}\frac{d\,\phi}{d\,t} + \frac{d\,.\int\mathrm{N}\,(d\,\phi)}{d\,t}.$$

Hence substituting in equation (5.), we have

$$f\,(t) - \mathrm{P} = \frac{d\,.\int\mathrm{N}\,(d\,\phi)}{d\,t} + \frac{\mathrm{N}^2}{2}\cdot\left(\frac{d\,\phi^2}{d\,x^2} + \frac{d\,\phi^2}{d\,y^2} + \frac{d\,\phi^2}{d\,z^2}\right) \quad (8.)$$

But if V be the velocity of the particle whose coordinates are x, y, z at the time t,

$$\mathrm{V}^2 = \mathrm{N}^2\left(\frac{d\,\phi^2}{d\,x^2} + \frac{d\,\varphi^2}{d\,y^2} + \frac{d\,\phi^2}{d\,z^2}\right)$$
$$= \mathrm{N}\left(\frac{d\,\phi}{d\,x}\cdot\frac{d\,x}{d\,t} + \frac{d\,\phi}{d\,y}\cdot\frac{d\,y}{d\,t} + \frac{d\,\phi}{d\,z}\cdot\frac{d\,z}{d\,t}\right).$$

Hence $\mathrm{V}^2\,d\,t = \mathrm{N}\,(d\,\phi)$. Or, supposing $\mathrm{V} = \frac{d\,s}{d\,t}$, s being a line drawn in the direction of the motion, $\mathrm{V}\,d\,s = \mathrm{N}\,(d\,\phi)$. Consequently, by substitution in equation (8.), we have

$$f\,(t) - \mathrm{P} = \int\frac{d\,\mathrm{V}}{d\,t}\,d\,s + \frac{\mathrm{V}^2}{2}.$$

This equation is known to be true by very different reasoning from that employed above.

The investigation I have now gone through opens a very extensive field of inquiry, which I hope to be able to prosecute at some future opportunity. In the mean time I consider myself entitled to say, that the solution I have given of the problem of the resistance of the air to an oscillating sphere is correct, since it takes account of the condition expressed by equation (4.); and that Poisson's is at fault by neglecting this condition, and consequently depending on an equation of insufficient generality. It would not be difficult to show that the equation in ϕ which Poisson makes use of, applies only to instances in which a surface of displacement coincides with a surface of equal pressure, which is by no means the case in every instance of fluid motion.

Cambridge Observatory, Dec. 17, 1841.

XVI. *Remarks on* Fernel's *Measure of a Degree.* *By* Thomas Galloway, *A.M., F.R.S.*

To the Editors of the Philosophical Magazine.

Gentlemen,

I BEG to offer a few remarks on Fernel's measure of the terrestrial degree, partly in reply to Professor De Morgan's letter which appeared in the Number of your valuable Magazine for December last, and partly for the purpose of pointing out some mistakes into which Lalande has fallen in treating of the same subject.

The most important point in Mr. De Morgan's letter is that which concerns Montucla and Delambre. Montucla reports the result of Fernel's measure of the degree to be 56,746 French toises; Delambre makes it 57,070 toises; and Mr. De Morgan says, that " in looking over the work of Fernel he was surprised to see that he himself states a *very different* result," namely, 68 Italian miles and 96 paces; and calls the attention of mathematicians to the circumstance as showing how the history of science is sometimes written. Coming from so high an authority, an implied charge of inaccuracy against two authors whose works are in the hands of every student, and whose statements are so generally trustworthy, demands some investigation.

Mr. De Morgan has cited from the *Cosmotheoria* three different passages in which the result is expressed in Italian miles, but there is still another passage in the same work (forming, indeed, part of the same sentence from which the third citation is made), in which the result is otherwise expressed, and accompanied with details which appear to me to be equally important to the right understanding of Fernel's operation, and to the discussion of the point at issue. The passage is as follows:—After describing the manner in which he ascertained, by observations of the sun's altitude, that he was exactly one degree to the north of Paris, and stating that, according to the report of the country people, the distance of the place from Paris was twenty-five leagues, Fernel adds (I quote from Snellius, *Eratosthenes Batavus*, p. 115),

"*Nec tamen vulgi supputatione satiatus, vehiculum quod Parrhisios recta via petebat conscendi; in eoque residens tota via* 17,024 *fere rotæ circumvolutiones collegi, vallibus et montibus ad æqualitatem quoad facultas nostra ferebat, redactis. Erat autem rotæ illius diameter* 6 *pedum, sexque paulo magis digitorum geometricorum; ob idque ejus ambitus pedum erat* 20, *seu passuum* 4; *his ergo revolutionibus per* 4 *ductis, repperi passus* 68,096; *qui miliaria sunt Italica* 68, *cum passibus* 96, *malui tamen hos passus in passus* 95 *cum quarto convertere, ne quæpiam fractio foret in terræ diametro præfigenda.*"

Here, then, is a perfectly clear and explicit account of the whole of the geodetical part of the operation. After making a discretionary allowance for the inequalities of the road, the wheel of the vehicle in which he travelled was found to have made 17,024 revolutions. The diameter of the wheel was measured and found to be six *geometrical* feet and six digits (the digit is one-sixteenth of a foot), and a little over; whence the circumference of the wheel was twenty feet, or four paces, and the whole distance 17,024 × 4 = 68,096 paces of five feet each, that is to say, 340,480 feet. The primary measure is therefore in *feet*, and the question to be decided resolves itself into this,—What is the equivalent of the measure which Fernel calls a geometrical foot?

Now the most obvious and probable supposition is, that the foot used by Fernel in measuring the diameter of his wheel was the ordinary Paris foot in use at the time. This is the supposition of Picard, Cassini, Montucla, Lalande, and Delambre; and although, perhaps, it cannot be formally proved to be correct, there is nothing in the statement of Fernel (notwithstanding the use of the word *geometricorum*) to furnish the slightest reason for suspecting that in measuring the diameter of the wheel he used a rule or scale different from what would have been applied to any common purpose of mensuration; or that, having used an ordinary scale, he reduced the actual measure to any other standard. But before discussing this point it will be proper to explain the origin of the numbers given by Montucla and Delambre.

Fernel reduces his 68,096 paces to 68,095$\frac{1}{4}$ in order to have the diameter of the earth expressed by a whole number. From the numbers which he gives it is easy to see that he proceeded in this manner: observing that his value of the degree would give the diameter *nearly* equal to 7800 miles of 1000 paces each, he assumes it to be 7800 miles *exactly*; whence, taking the ratio of the circumference to the diameter to be as 22 to 7, he finds the circumference = 24,514 miles and $285\frac{5}{7}$ paces, and consequently one degree = $68{,}095\frac{1}{4}$ paces *nearly*, that is, equal to 340,476 feet, omitting a fraction.

The value assigned to Fernel's degree by Montucla, namely, 56,746 toises, was first given by Picard in his *Mesure de la Terre*, published in 1671. Picard merely observes that 68,096 paces (of five feet) make 56,746 toises and four feet (the toise being six feet), "selon nostre façon de mesurer" (p. 2). Taking Fernel's reduced value, or 340,476 feet, we get 56,746 toises. Ozanam (*Dictionnaire Mathématique*, 1691) and Cassini (*Grandeur et Fig. de la Terre*, 1720) give this value; and Montucla, writing in 1758, quotes it without suggesting any doubt of its accuracy.

Delambre's value, namely 57,070 toises, is due to the ingenuity of Lalande, by whom it was given, in the *Mém. de l'Acad. des Sciences*, 1787, as a correction of Picard's reduction. After remarking that Fernel's result as stated by Picard, namely, 56,746 toises, differs only by 323 toises from 57,069 toises, the length assigned to the degree by a then recent determination, he adds, "But this exactness, already so singular, becomes much more astonishing when we take into account the change made in the Paris foot since the time of Fernel. We know positively, from the testimony of Picard, of Azout, and De la Hire (*Mém.*, 1714), that the toise of Paris was shortened by *five lines* in 1668; therefore, on computing according to the toise now in use, it is necessary to add 323 toises to the measure of Fernel, which thus becomes 57,070 toises, that is to say, the same within a single toise as the value found at the present time," p. 219.

In this passage there is manifestly some inaccuracy, for on adding 323 to 56,746, we find not 57,070 but 57,069, as Lalande himself had remarked in a previous sentence. From the whole statement we might be inclined to suspect 323 to be a misprint for 324; but it is curious enough that neither of those numbers is obtained from the data. If the toise was shortened by five lines *exactly*, it follows that Picard's toise contained 869 lines of the corrected toise, or was to the corrected toise in the ratio of $1\frac{5}{864}$ to 1. Now $\frac{5}{864} \times 56{,}746 = 328$; which being added to 56,746, gives 57,074, and not 57,070, as stated by Lalande in the passage above quoted, and also in his *Astronomie* (2638). In adopting the correction of Lalande, Delambre probably did not think it worth while to verify the numbers.

Lalande refers to the *Mémoires* for 1714 as the authority for his correction. The volume indicated contains a paper by La Hire, in which the reduction of the toise is simply mentioned with a reference to a former volume, but a circumstantial account of the matter is given by Paucton (*Métrologie*, p. 18).

But to return to the subject in hand. The case as respects Montucla and Delambre stands thus:—(I quote the dates, as Mr. De Morgan says the confusion is older than Delambre and Lalande.)

1. Fernel, in 1528, declares the length of a degree of any great circle of the earth to be 68,095$\frac{1}{4}$ paces of five feet each.
2. Picard, in 1671, assumes that the foot used by Fernel was the ordinary Paris foot of six to the toise, and accordingly states the result to be 56,746 toises, which is the number given by Montucla.
3. Lalande, in 1787, shows that in consequence of the alteration of the toise in 1668, which Picard had overlooked or not thought it worth while to notice, the result obtained by Fernel was 57,070 toises, which is the number given by Delambre in his *Astronomie**.

Thus it appears that Montucla and Delambre, instead of stating "a very different result" from Fernel, state precisely the same result but in different terms; on the assumption, however, that Fernel gives the diameter of his wheel in *Paris* feet.

It is now necessary to advert to the passage in which Fernel states the result of his measure to be 68 *Italian miles* and 96 paces. This point has been discussed by Lalande (*Mém.*, 1787), who remarks that Fernel's Italian mile of 68 to the degree is in fact a measure used in some parts of Italy, but that the mile differs widely in different parts of the country, varying from 48 to 81 in the degree; and from this circumstance and the manner in which Fernel compares his result with that of the Arabians, he concludes that what he says about Italian miles must be abandoned; in other words, has no definite meaning apart from the expression of the measure in feet. Riccioli, who lived nearer the time of Fernel, and will be admitted to be an unexceptionable authority, confirms this description of the vagueness of the Italian mile. "Italica milliaria incredibilem habent varietatem, tum in modo ea compo-

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* Delambre has given a detailed account of Fernel's measure in the *Histoire de l'Astronomie du Moyen Age*.

nendi ac metandi, tum in quantitate." (*Geographiæ et Hydrographiæ Reformatæ*, lib. xii. 1661, p. 48.) It is obvious, therefore, that the expression *Italian mile*, unless accompanied with some explanation, or reference to some other standard, conveys no precise information. Fernel's statement, taken altogether, seems to suggest the idea that he used the expression, not for the purpose of defining the foot which was the original measure, but as a popular term, familiar to the writers of that age, and therefore likely to give a clearer notion of a large magnitude than would be conveyed by expressing it in feet.

The *geometrical pace* participates in all the vagueness of the Italian mile. Riccioli makes frequent mention of the *Roman* geometrical pace as distinguished from that of the moderns. The true solution of the greater part of the difficulties attending this subject will probably be found in the supposition that the terms *geometrical pace* and *geometrical foot* were used more frequently to denote a particular mode of division than absolute measures. Five geometrical feet make a geometrical pace, and 1000 paces a mile; while the writers of each country tacitly assume the foot to be taken according to their own standards. It would be easy to accumulate quotations in support of this view, but the following may suffice.

Norwood, in the "Epistle Dedicatory" prefixed to his *Seaman's Practice* (1678), says "the way of finding distances at sea is rather opinionative and conjectural than certain, being founded upon this supposition, that the compass of the world in any great circle is 21,600 *Italian* miles (as they call them), and that such an *Italian* mile contains 1000 paces, and every of those paces five *English* feet." Thus the common practice in England, according to Norwood, was to call 5000 *English* feet an Italian mile.

Paucton (*Métrologie*, p. 179) cites the following passage from an old treatise on itinerary measures by the Sieur Samson of Abbeville:—"Le pas commun de l'homme est de deux pieds et demi. Le pas des Allemands, qu'on appelle *pas géométrique* est de cinq *pieds de Roi.* Ainsi quand on dit: le mille d'Italie a mille pas, la lieue de France a trois milles pas, on entend des pas géométriques." Here the geometrical pace is expressly stated to be five *French* feet, and consequently the Italian mile to contain 5000 *French* feet.

I may observe, in passing, that the sentence last quoted furnishes, at least, one evidence that the "curious fiction which the geometers of the 16th century called a geometrical pace" was sometimes regarded as *longer* than five English feet.

In order to make good the assertion that Fernel "certainly used the Italian mile of his day," it would be necessary to show,—1st, that there was only one Italian mile in use in the days of Fernel; and 2nd, that Fernel knew its precise value—not merely knew that so many repetitions of it made a degree (which was the usual definition), but had a material representative of some aliquot part of it which he could apply to the measurement of his wheel. Neither of these points will be easily established. The only thing certain in the relation of Fernel, is 17,024 revolutions of a wheel 20 feet in circumference; and the whole question, as has been already remarked, is whether he meant Paris feet (*Pieds de Roi*), or feet of some other denomination. Picard and all the French authors assume that he meant Paris feet. Norwood evidently makes the same assumption when he represents Fernel's pace as being "more than five English feet," the French foot exceeding the English foot, according to Snellius (from whom, probably, Norwood took his idea of the French foot), in the ratio of 1055 to 968.

In stating his reasons for assigning a different value to Fernel's measure from that of Montucla and Delambre, Mr. De Morgan quotes Riccioli as giving "a true account." Lalande, on the other hand, asserts that Riccioli seized upon the expression *Italian miles* as a means of making Fernel's result, as well as that of the Arabians, agree with his own, which was much less accurate; and he says, "it would be unjust to admit the valuation of Riccioli, who makes Fernel's degree equal to 64,421 Bologna paces, which are equal to 62,726 toises" (*Mém.*, 1787, p. 220). Now it will probably be considered not the least remarkable circumstance connected with this history, that Riccioli *gives no reduction of Fernel's result whatever*, nor states its value in any other terms than those used by Fernel himself. He gives, it is true, the value of the *Arabian* measure as being 64,421 Bologna paces, and as Fernel states his result to differ from that of the Arabians only by 96 geometrical paces, it may be presumed that Riccioli considered the above as a sufficiently near expression of its value. But however this may be, the measure reduced by Riccioli is the Arabian measure and not Fernel's, and the 64,421 Bologna paces are found by a process wholly and absolutely independent of Fernel's operation. The reduction which is given in Riccioli's work above cited is as follows:—

The Arabian degree is stated by Campanus and others to be $56\frac{2}{3}$ miles. In order to define the length of the mile, the Arabian authors say it contained 4000 cubits; that a cubit contained 24 digits; and that a digit was equal in length to six grains of barley. Hence the Arabian mile is equivalent to $4000 \times 24 \times 6 = 576{,}000$ barleycorns. Now Riccioli and Grimaldi found by trials repeated frequently (*iterum ac sæ-*

pius), that 80 grains of barley are contained exactly in the ancient Roman foot of the time of Vespasian, and consequently that a Roman mile, or 5000 Roman feet, must contain 400,000 such grains. Supposing, therefore, grains of barley grown in Lombardy to be of the same size as those of barley grown in Arabia, the ancient Roman mile is to the Arabian mile in the ratio of 400,000 to 576,000, or of 1000 to 1440, so that an Arabian mile contains 1440 Roman paces; whence the Arabian degree is $56\frac{2}{3} \times 1440 = 81,600$ Roman paces (p. 44). Riccioli also found the Bologna foot to be to the ancient Roman foot in the ratio of 1520 to 1200 (p. 46), that is, of 19 to 15. Multiplying, therefore, 81,600 by $\frac{15}{19}$, the length of the Arabian degree is found expressed in Bologna paces, namely, 64,421 paces. The numbers 81,600 and 64,421 are given by Riccioli in the table at the end of his fifth book (p. 176) as the length of the degree in Roman paces and Bologna paces respectively, with the marginal indication, "Ex Campano et Fernelio," from which indeed an incautious reader might think Fernel's *French* degree was meant.

The method by which Fernel connects his measure with that of the Arabians is not quite of so refined a nature as that of Riccioli, but probably about as accurate. He conjectures, from the statements of Campanus, that the Arabian mile contained 1000 *common paces* (meaning double paces, or two steps) of a man of the average size, and that 1000 common paces are exactly equal to 1200 geometrical paces of five feet, which he found, upon trial, to be the case with respect to his own paces. Hence the Arabian degree must contain $56\frac{2}{3} \times 1200 = 68,000$ geometrical paces, or 68 Italian miles, the foot, which is not defined, being always the unit of the measure.

Now, even if all the other assumptions were admitted, it would still be necessary, in order to connect Fernel's result with Riccioli's Bologna paces, to suppose that 576,000 grains of barley are exactly of the same length as 6000 feet of the scale in terms of which Fernel has given the diameter of his wheel. Riccioli makes no such supposition; in fact he sets aside altogether Fernel's measure of the French degree, and gives his conjectural value of the Arabian degree in Roman feet and Bologna paces. Lalande says Riccioli was deceived by Fernel's mention of the Italian mile. The party deceived appears to have been M. Lalande himself.

There is still a consideration which may be suggested as favouring the presumption of the correctness of Picard's supposition. Is it at all probable that Fernel was aware of the difference between the Paris foot and the foot used in different parts of Italy, or even the ancient Roman foot? The treatise of Budæus, *De Asse*, &c., had been recently published; and according to Snellius (*Eratos. Bat.*, p. 132), Budæus expressly makes the Roman foot the same as the *Pied de Roi.* Fernel was not likely to be better informed, and in such case could have no reason for stating the dimensions of his wheel in terms of a foreign standard.

With respect to the statement of Weidler, on which Mr. De Morgan has raised some questions, namely, that Fernel's measure was finished in 1550, Lalande's remark may be regarded as a satisfactory reply: *c'est encore une erreur.*

Although I have already trespassed too far on your space, it would scarcely be right to close these remarks without observing, that neither Picard, Montucla, Lalande, nor Delambre attach the slightest value to Fernel's measure, but all concur in representing the accuracy of the result as a matter of mere accident. A ruder and more inadequate operation it would, in fact, be difficult to imagine. He begins by observing the sun's meridian altitude *somewhere* in Paris on the 25th of August (the year is not mentioned); he then sets out to travel northwards (it is *supposed* on the road to Amiens); on the 28th he observes again, and finds that he has not yet reached a degree, but the observation enables him to see how far it would be necessary to travel on the following day. Having gone this distance, he finds himself on the 29th exactly a degree to the north of Paris. His observations of altitude are only given to minutes, and Delambre shows that in allowing for the sun's motion in declination he made an error of $2'$, which alone would vitiate the result to the amount of 1900 toises. He gives no indication whatever of the place at which he stopped, so that no means exist of estimating the amount of his error with respect to the celestial arc. Picard says it was $3' 24''$, but this supposes that he proceeded all the way to Amiens, which is very doubtful. He then mounts a waggon going direct to Paris, and counts as he proceeds the revolutions made by the wheel during the journey. By what means he thought himself enabled to make any probable estimate of the reduction necessary on account of the turnings and acclivities of the road, is beyond comprehension. However, he fixes upon 17,024 revolutions. He then compares his result with that of the Arabians; and from some conjectures (the grounds of which have been stated) concludes the Arabian degree to be 340,000 feet. His own determination was 340,480 feet; an agreement which Snellius roundly asserts he never would have obtained from his rude geodesy. For these reasons, most of those who have described his operation have expressed a suspicion that he took his result from the

Arabians as they took theirs from Ptolemy. The 7800 miles which he assumed for the diameter of the earth might indeed be taken as well from the reputed result of the Arabians as from his own measure; and having assumed this, he could dispense with all the rest. But in whatever way he came by his result, its near approximation to the true value of the degree must be considered as a very remarkable circumstance. It is singular that the historian of his life (Plantius), who was his intimate friend, does not once allude to the measure of the earth, although it was a feat in which, as Lalande remarks, Fernel might reasonably be expected to have prided himself[*].

Fernel was court physician to Henry II. of France. In his early years he cultivated mathematical studies, but abandoned them upon his marriage and devoted himself exclusively to the practice of his profession, by which he acquired a large fortune. He died in 1558, at the age of seventy-two. A gossiping account of him may be seen in Bayle's Dictionary.

I am, Gentlemen, yours faithfully,

Serjeants' Inn, Jan. 6, 1842. T. GALLOWAY.

XVII. *On the Electric Origin of the Heat of Combustion.* *By* J. P. JOULE, *Esq*[†].

[Illustrated by Plate III.]

1. IN the papers which I had some time ago the honour of communicating to the Royal Society, I related an investigation concerning the calorific effects of voltaic electricity, and stated my opinion with regard to the heat evolved by combustion and certain other chemical phænomena. In the present paper I intend to bring forward some experiments in confirmation of my theory, and to prove that the heat of combustion, terminating in the formation of an electrolyte, is the consequence of resistance to electric conduction.

2. We have seen that when those chemical actions which are not the sources of transmitted electricity are allowed for, the heat evolved from any part of the voltaic apparatus is the effect of the resistance which is presented by that part to the electric current; and that hence it necessarily follows, that the total voltaic heat generated by the action of any closed galvanic pair is proportional to the number of chemical equivalents which have been consumed in the act of propelling the current, and the intensity of the galvanic arrangement. Now, if it can be shown that the quantity of heat which is evolved by ordinary

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[* On the subject of this paper see an additional communication from Professor de Morgan, at p. 116.—EDIT.]

[†] Read before the Literary and Philosophical Society of Manchester, November 2, 1841; and now communicated by the Author.

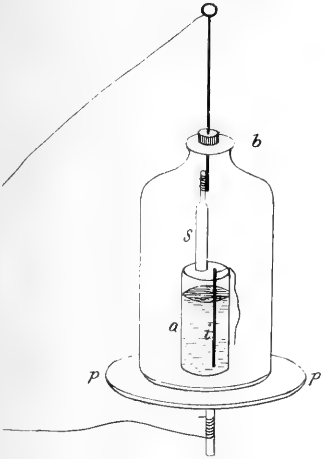


Fig: 1.

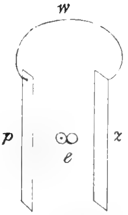
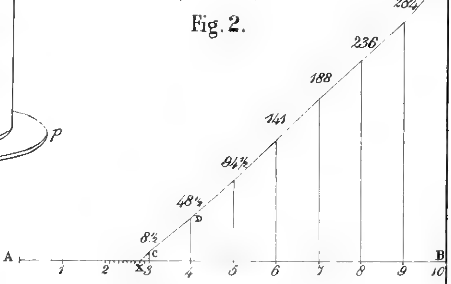


Fig. 2.



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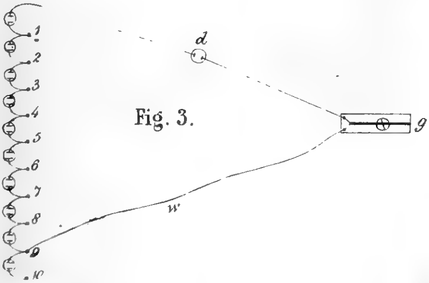

Fig. 3.

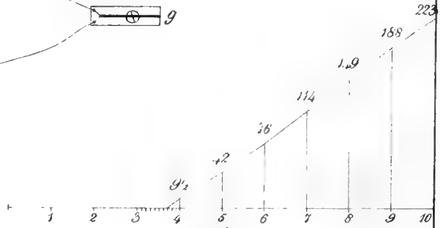

Fig: 5.

Fig: 7.

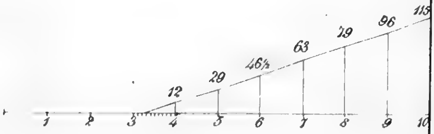

Fig: 6.

J. Basire, Lithogr.

Mr. Joule on the Electric Origin of the Heat of Combustion

chemical combination is the same as calculation founded on these facts would lead us to expect, no reasonable doubt can be entertained that it also is the product of resistance to electric conduction.

3. In studying the character of the heat of combustion, the first point was to determine the intensities of the affinities of different combustibles for oxygen. For this purpose I have, in accordance with the views which were first stated by Davy, and have since been adopted by the most eminent electricians, made use of the measure of these intensities which is afforded by the electric current.

4. I had not proceeded far before some curious phænomena were observed, which, though not very well understood, have long been known* to electricians. I shall notice these first, because of their important bearing upon subsequent reasonings and conclusions.

5. I was working with an arrangement consisting of iron, platinized silver, and dilute sulphuric acid. The circuit was closed by a galvanometer, the coil of which consisted of 119 turns of thin-silked copper wire, forming a rectangle, measuring 1 foot by 6 inches. The needle indicated a pretty constant deviation of 20°, but on moving the platinized silver backwards and forwards the needle advanced gradually to 40°, where it was kept for some time by continuing the agitation. As soon as the motion of the platinized silver was discontinued, the needle resumed its former position. Similar effects were produced by stirring the liquid, and thus causing it to impinge against the platinized silver.

6. I repeated the above experiment many times with similar results, but I found that whenever a large quantity of hydrogen had been evolved from the liquid by the action of the pair, or otherwise, the phænomena were not well produced. This circumstance convinced me that the effects were due to atmospheric air held in solution by the liquid, and that the displacement of a part of it by the hydrogen had occasioned their partial prevention. My opinion was confirmed by the following experiment.

7. I filled three quarters of the contents of a glass flask with dilute sulphuric acid, and then placed it over the flame of a spirit-lamp until I judged that all the atmospheric air had been

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* In 1830, Mr. Sturgeon remarked that when two pieces of iron are placed in dilute muriatic acid, the *agitation* of one of them will make it operate as copper in the copper-zinc battery: also, that if two pieces of iron are immersed in succession in a solution of nitrous acid, the iron *last* immersed will act as copper in the copper-zinc battery.—Recent Experimental Researches, p. 46—49. We shall hereafter see the true cause of these phænomena.

boiled out. I then removed the lamp, and immediately placed in the mouth of the flask a cork, through which a small piece of platinized silver and a stout iron wire had been passed. On connecting the metals with the galvomometer (5.) its needle was deflected to 32½°, and on shaking the flask very briskly it could not be made to advance further than to 34°. This advance, slight as it is, was probably entirely occasioned by air, which, notwithstanding my precautions, had found its way into the upper part of the flask.

8. The phænomena originated entirely from the platinized silver; and although a slight advance of the needle was sometimes produced by agitating the *iron*, it was not difficult to see that the real cause was the propulsion thereby occasioned of the aërated liquid against the negative* element, for when this was avoided no advance of the needle could be produced by agitating the positive metal.

9. I thought it probable that an increase of the intensity of the current would be produced by directing a stream of oxygen gas against the negative element. On making the experiment, I found that the needle advanced a few degrees, and that the same effect could be produced by a stream of hydrogen. There could be no doubt that the increase of intensity arose rather from the agitation of the liquid than from any specific action of the gases, and that this experiment was essentially the same as that described in (5.).

10. I impregnated some dilute sulphuric acid with a very small quantity of oxygen, according to Thenard's process, and then immersed into it a plate of platinized silver and a rod of iron, both properly communicated with the galvanometer. The needle stood for the first few seconds at 68°; in three minutes it declined to 50°; in five minutes more to 49°, and in another five minutes to 48½°. On agitating the platinized silver so as to bring it repeatedly in contact with the yet undecomposed deutoxide of hydrogen, the needle advanced to above 60°. The same pair, immersed in common acid, would have deflected the needle no further than 29° or 30°.

11. The effect of the presence of oxygen at the negative element is well observed by making it, in water, the positive electrode of a voltaic battery. By this means oxygen is deposited on its surface, and is there ready to produce an extraordinary intensity. This deposit of oxygen is in fact the cause of the action of Ritter's secondary piles.

12. The following was also a very convenient method of

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* To avoid misconception, it is perhaps as well to observe that I call those elements of the voltaic battery *negative*, which attract or combine with those bodies which are called "positively electrical," or "cations."

showing the increase of intensity arising from the presence of oxygen. Some dilute sulphuric acid was agitated with chlorine until the former had taken up as much of the gas as it could. By pouring a solution of sulphate of oxide of silver into the liquid, I now precipitated chloride of silver, leaving sulphuric acid and oxygen in solution. When a pair, consisting of platinized silver and iron, was placed in the acid thus prepared, the galvanometer was permanently deflected 50°, and by agitating the platinized silver the needle advanced as far as 60°. When a piece of amalgamated zinc was substituted for the iron the permanent deflection of the needle was 65°, and by agitating the negative element as before, the needle advanced to 70°. Had the same pairs been immersed in a simple solution of sulphuric acid, the permanent deflections would have been no greater than 30° and 63°.

13. Similar results were obtained with the solution of chlorine, as might have been anticipated from its strong affinity for hydrogen.

14. From the above experiments, we see that the agitation of the negative element is productive of an increase of intensity, simply because it is thereby brought into contact with bodies capable of combining with the hydrogen, which would otherwise have been evolved from it. When those bodies are present in considerable quantities, as in (10.), (12.) and (13.), the intensity of the current is great, even though the pair be left quiet, because then the negative plate can collect them readily upon its surface. Again, by causing the current to encounter great resistance, the effects of agitation which we have noticed are proportionally increased, because then the number of particles required for neutralizing the hydrogen is less. Hence it is that when I have used a resistance of 500 or 600 yards of thin wire, I have frequently found the deviation of the needle (even when the pair was left quiet in a common solution of sulphuric acid) considerably greater than was due to that resistance. This is also the probable reason why De la Rive in one instance* found the intensity of the copper-zinc pile the same, whether charged with water or nitric acid.

15. In the course of the preceding experiments I was forcibly struck with the very great intensity of the pairs at the moment of their immersion, compared with that which they were able to maintain permanently. It appeared to me that the theories which had been put forth to explain the first effect of immersion, though seemingly plausible with regard to the zinc battery, were not at all equal to account for the same phæno-

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* *Ann. de Chimie*, 1836, part i. p. 179.

menon as existing to a far greater extent when iron is used as a positive element.

16. A rod of iron and a small plate of platinized silver were immersed in a dilute solution of sulphuric acid. On connecting them with the galvanometer (5.), the needle was permanently deflected 29½°. After a few preliminary trials to ascertain the proper point, I caused the needle to be maintained by a glass weight at 55°, *beyond* which it was free to travel. I then exposed the platinized silver to the air during one minute of time. On re-immersing it the needle sprang as far as 60°, and then immediately recoiled to its resting place at 55°, thus indicating a *transitory* current of about 57½°.

17. On exposing the platinized silver for 5″ only, the transitory current, ascertained in a manner similar to that just mentioned, was 41°.

18. Greater effects were obtained by washing and drying the platinized silver before it was immersed. In this way the needle, adjusted at 62°, would spring as far as 66°, indicating a transitory current of about 64°. Having now removed the glass weight, the needle took up a permanent position at 29½°, as at the beginning of the experiments.

19. When, instead of the platinized silver, the positive element (iron) was exposed to the air, whether simply or in conjunction with washing and drying, no appreciable increase of intensity was occasioned by its immersion. And although, on the repetition of the experiment, I sometimes observed slight effects, I conceive that they were owing to the power which the negative element seems to possess of collecting upon its surface the air held in solution by the circumambient liquid.

20. With an arrangement of platinized silver and amalgamated zinc, I obtained results of a similar though less striking character. The galvanometer indicated a permanent deflection of 62°, and after washing and drying the platinized silver, I had a transitory deviation of 72°. The immersion of the amalgamated zinc, after washing and drying, produced no effect.

21. The *maximum* effects of immersion were produced in the following manner. A plate of silver was rubbed with a little nitric acid, and then exposed to a red heat, by means of which the film of nitrate of oxide of silver was decomposed and metallic silver reduced*. When the plate prepared in

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* By this process all the oxygen is not driven off, but a considerable quantity remains adhering to the silver so tenaciously that it is not entirely removed by making the plate quite bright with glass paper. The oxygen thus deposited (it can hardly, I think, be considered as chemically combined with the silver) is the cause of the great intensity of the current immediately after immersion. By simply heating the silver to redness the same general effects can be produced, though not to the same extent

this way was associated in dilute sulphuric acid with a piece of iron, the needle would deviate 63½° for some time, and then gradually decline until it took up a permanent situation at 29½°. By experimenting in the same way with amalgamated zinc as a positive element, I had a transitory deflection of 76°, and a permanent deflection of 63°.

22. Very trifling transitory effects were obtained by the immersion of iron, when that metal was associated with amalgamated zinc. But this might have been anticipated, because the transitory current is owing to the presence of oxygen on the negative plate, and it is obvious that the hydrogen evolved by the local action of the iron, would, whilst in a nascent state, combine with that oxygen, and thus prevent a great part of it from exercising any influence upon the intensity of the current.

23. An experiment was also made with an arrangement of copper, amalgamated zinc, and dilute sulphuric acid. It was able to deflect the needle 51° pretty permanently. On washing and drying the copper, and experimenting as in (16.), I observed a transitory deflection of 72°. This experiment deserves attention, because it shows that the transitory current occasioned by the copper is the same as that exhibited by platinized silver when experimented with in the same way (20.). I take it as an argument, that when copper is in its best state it forms with amalgamated zinc a battery as intense as the platinized silver.

24. That the transitory currents which we are discussing are not occasioned by the diffusion of the salt formed about the positive element during the cessation of voltaic action, is obvious from the fact that (when the proper precautions are observed) they are not produced by the agitation (8.), or by the immersion (19. and 20.) of the metal about which the salt is formed. And if anything can render this more evident, it is the fact that the immersion of the copper plate of a Daniell's battery causes the needle to advance little or no higher than its permanent situation, as might have been anticipated from the theory which refers the transitory effects to chemical combination at the negative plate, on account of the slight affinity of copper for oxygen. The following experiments are also decisive of this question.

25. A glass jar, *a*, fig. 1, containing some dilute sulphuric acid, was placed upon the plate *p p* of an air-pump. A small rod of iron, *i*, was immersed in the liquid, and connected by means of the pump-plate to the galvanometer (5.). An open receiver, *r*, was now placed over the jar, and the ground brass plate *b*, with its stuffing-box and sliding rod (the latter having the small piece of platinized silver, *s*, affixed to its extremity),

was placed on the top of the receiver. A copper wire, fastened to the ring of the sliding rod, connected the platinized silver with the galvanometer.

26. The sliding rod was now moved until the platinized silver in connexion with it was immersed in the acidulated water. Then the pump was worked until a very excellent vacuum was obtained, and so tight was every part of the apparatus that it could be left alone for half an hour without the admission of any appreciable quantity of air. The galvanometer indicated a permanent deflection* of 27°. I now placed a piece of glass so as to prevent the needle from going lower than 27°, and by means of the sliding rod I removed the platinized silver entirely out of the acid. After it had been exposed during a quarter of an hour I re-immersed it, when the needle sprang from 27° to 30° and back, indicating a transitory deflection of about 28½°. Although the effect of immersion exhibited by this experiment is extremely small, it appeared to be almost entirely occasioned by the repose of the electric condition of the iron, for when, instead of entirely withdrawing the platinized silver, its extremity was just allowed to touch the liquid, the transitory deflection was only 27½° after an exposure during a quarter of an hour.

27. On admitting a quantity of air into the receiver sufficient to counterbalance the pressure of one inch of mercury, the effects of immersion were considerable after a very short exposure of the platinized silver. In a quarter of an hour it collected upon its surface sufficient oxygen to cause the needle to spring from 27° to 78°, whether it had or had not remained in contact with the liquid during its exposure.

28. When, instead of the vacuum, I used an atmosphere of hydrogen, the exposure of the platinized silver for any length of time did not render the current more intense at the moment of immersion than it remained permanently. And even when the hydrogen was diluted with one quarter of its bulk of atmospheric air the transitory effects did not appear, on account, no doubt, of the union† of the oxygen with the hydrogen as fast as the former, or both, collected upon the plate. On using a mixture of equal bulks of hydrogen and air, the transitory effects were very small, even after the platinized silver had been exposed for ten minutes.

29. I made several experiments with carbonic acid, but the

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* No change in the permanent deflection of the needle was occasioned by the removal of atmospheric pressure.

† The phænomenon of Dœbereiner, so fully investigated by Faraday, to whose paper, published in the Phil. Trans. for 1834, I refer the reader for some valuable observations on the power possessed by metals of condensing gases upon their surfaces.

transitory currents did not entirely disappear as was anticipated. The gas, though prepared carefully and in different ways, could not be obtained perfectly pure, and when exposed to an alkaline solution, $\frac{1}{200}$th of it would remain uncondensed. In order therefore to remove any free oxygen which the gas might contain, I exposed it during two days and two nights to the action of a stick of phosphorus. After this, immersion caused no, or at most, very trivial transitory effects; but on admitting only one per cent. of oxygen they became very considerable,—a striking example of the power possessed by metals of collecting and condensing oxygen upon their surfaces. I do not bring forward this experiment as a proof of the *entire* non-action of carbonic acid, because the phosphorus was found to have decomposed it partially.

30. All these phænomena are easily understood, if, with the great body of philosophers, we keep in view the intimate relation which subsists between chemical affinity and the electric current. For let p, fig. 2, represent a plate of platinum; z, a plate of zinc, or other electro-positive metal; and c, one of a series of atoms of water extending from p to z. The intensity of the current along the wire w, is proportional to the affinity of oxygen for the positive metal, minus the affinity of oxygen for hydrogen. But if p be covered with a film of oxygen, the current will be entirely proportional to the affinity of the positive metal for oxygen. In the former case, $c = z - h$; in the latter, $c' = z$.

31. Considering these equations, it is obvious why, as I have observed (15.), the transitory currents are better exhibited with iron than with zinc as a positive element; for in proportion to the smallness of z, provided it remain greater than h, will the difference between c and c' be more manifest. If $c' - c$ be the same for both iron and zinc, we shall have a proof of the accuracy of these principles.

32. Thus from (21.), turning the deflections of the needle into quantities of electricity, we have $63\frac{1}{2}^\circ = 0^\circ{\cdot}034$ Q, and $29\frac{1}{2}^\circ = 0^\circ{\cdot}0072$ Q, of which the difference is $0^\circ{\cdot}0268$ Q, when *iron* is the positive element. We have also $76^\circ = 0^\circ{\cdot}056$ Q, and $63^\circ = 0^\circ{\cdot}027$ Q, of which the difference is $0^\circ{\cdot}029$ Q, when *zinc* is the positive element. I consider these differences as nearly equal as could have been expected from the nature of the experiments.

33. I might now proceed to consider in detail several phænomena (such as the very rapid corrosion of metals when they are exposed to the joint action of air and moisture, &c.) which are occasioned by the great intensity of galvanic action in consequence of the mixture of oxygen with the liquid But I hasten to fulfil my principal design.

Intensities of the Affinities which unite bodies with Oxygen.

34. In order to ascertain the intensities of galvanic arrangements, we may either use a galvanometer furnished with a short and thick wire, or with a long and thin wire (within certain limits (14.)). In the former case the calculations must be conducted on the principles of Ohm; in the latter it is only necessary to take care that the resistances of the pairs under comparison are pretty nearly equal, in order that the deviations of the needle may be depended upon in calculating the intensity of the current. I have adopted the latter plan on account of the superior facilities which it presents.

35. *Affinity of zinc for oxygen.*—From (32.) we have the intensity of the action of zinc = $0^\circ{\cdot}056$ Q; and the intensity required for the electrolysis of water = $0^\circ{\cdot}029$ Q. Hence 29 : 56 : : 1, the affinity of hydrogen, : 1·93, that of zinc for oxygen.

36. *Affinity of iron for oxygen*, likewise obtained from (32.), is 1·27, for 268 : 340 : : 1 : 1·27.

37. *Affinity of potassium for oxygen.*—Twenty grains of potassium were combined with about ten ounces of mercury. The amalgam was poured into a wooden cup, into the bottom of which a copper wire connected with the galvanometer (5.) had been let. At about half an inch above the surface of the amalgam I secured a piece of platinum, also in connexion with the galvanometer. On pouring dilute sulphuric acid into the cup the needle was deflected 74° (= $0^\circ{\cdot}05$ Q) during three successive minutes, but the local action of the amalgam was so vigorous that at the end of this interval of time most of the potassium was dissolved, and the needle declined very fast. On treating 20 grains of zinc in precisely the same manner, I had a deviation of 49° (= $0^\circ{\cdot}0152$ Q). Hence

$$k - h = 0{\cdot}05,$$

and

$$z - h = 0{\cdot}0152,$$

whence

$$152\,k = 500\,z - 348\,h.$$

But from (35.), $z = 1{\cdot}93$, and $h = 1$; therefore k, the affinity of potassium, = 4·06.

38. It is necessary, however, to pay attention to the circumstances under which the experiments were made, in order to obtain correct ideas concerning the above intensities of affinity. The increase of the intensity of the voltaic apparatus by heat is by no means great; and as all the experiments were conducted at common temperatures, no regard need be paid to *it*. But then the intensities of affinity were obtained by comparing currents which had been produced under peculiar circumstances with regard to the *condition* of the elements of the galvanic arrangements: in one case the hydrogen was evolved in a

gaseous state; whilst in the other, the hydrogen, by combining with free and condensed oxygen, did not escape. Now we shall see from the following experiments that electric intensity is *expended* in the act of converting a body into the gaseous state.

39. I took ten glass jars (see fig. 3), made them perfectly clean and dry*, and placed them in series on a non-conducting substance. Into these I poured a quantity of dilute sulphuric acid, taking care not to wet the glass within an inch of the top of each. Pairs of platinized silver and amalgamated zinc were placed in the jars, and connexions, furnished with the mercury cups 1, 2, 3, &c., were established between them *seriatim*. A decomposing cell, *d*, furnished with platinum wires, was connected on one hand with the battery, and on the other with the galvanometer (5.). Lastly, I provided a copper wire, *w*, by means of which connexion could be conveniently made between the galvanometer *g*, and any of the mercury cups, 1, 2, 3, &c.

40 Into *d* I poured a small quantity of dilute sulphuric acid. Then, by placing the wire *w* in each of the mercury cups, beginning at 10 and ending at 10, I observed the deviations of the galvanometer contained in the following table.

No. of pairs in action.	Deflections of Galvanometer.				Comparative quantities of Electricity.
	Down.	Up.	Mean.		
	°	°	°	′	
10	72½	71	71	45	341
9	69	68	68	30	284
8	65¼	65	65	7	236
7	60	60	60	0	188
6	53	53⅔	53	20	141
5	42	43	42	30	94½
4	25½	27¾	26	37	48½
3	5½	5½	5	30	8½
2	0	0	0	0	0
1	0	0	0	0	0

41. Now if we divide the straight line, A B, fig. 4, into ten equal parts, representing pairs on Mr. Smee's plan, and if at each division we erect straight lines, perpendicular to A B and proportional to the comparative quantities of electricity just given, the principles of electric action demand that the line drawn through the extremities of those perpendiculars

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* It is necessary to be very careful in insulating the apparatus, in order to obtain the *maximum* intensity of a battery. The divided porcelain trough has frequently great conducting powers (particularly when the glaze has been partially destroyed), which render it unfit for accurate experiments.

should be straight. It is in fact so nearly a straight line, that its slight discrepancies therefrom may be properly referred to unavoidable errors of experiment. Produce the straight line C D so as to meet A B in X, and the straight line A X, equal to 2·8, will indicate the number of pairs necessary to decompose water.

42. Fig 5 represents an experiment of the same kind, with a solution of sulphate of oxide of zinc in the decomposing cell. Oxide of zinc was decomposed, the oxygen being evolved at the positive, and the zinc being reduced at the negative electrode. The intensity necessary to decompose oxide of zinc is equal to that of 3·7 of Mr. Smee's pairs.

43. With sulphate of protoxide of iron I did not at first succeed, on account of the formation of peroxide at the positive electrode. However, by placing the negative electrode among some crystals of the salt, pouring water thereon, and suspending the positive electrode in the water, I obviated that difficulty, and obtained the results which are projected in fig. 6, and which indicate 3·3 pairs as the intensity necessary to decompose protoxide of iron.

44. Now from (41.) and (42.) we have (using the same letters as before) 2·8 pairs $= h$, and 3·7 pairs $= z$; whence $2{\cdot}8\,z = 3{\cdot}7\,h$, and $z = 1{\cdot}32\,h$; or, in other words, the intensity required to separate oxide of zinc into metal and *gas* is to the intensity required to separate water into its *gaseous* elements as 1·32 : 1. But from (35.), the intensity produced by the union of *non-gaseous* oxygen with zinc is to the intensity necessary to separate water into *non-gaseous* oxygen and *gaseous* hydrogen as 1·93 : 1; and 1·32 : 1 :: 1·93 + 1·9 : 1 + 1·9. Wherefore, the intensity necessary to give oxygen the gaseous form is to the intensity necessary to separate water into non-gaseous oxygen and gaseous hydrogen as 1·9 : 1.

45. Thus we see that a very great intensity of current is employed in changing the condition of bodies, as well as in separating them from their combinations. The field of investigation here opened is very extensive, but I may not at present enter further upon it. I will only remark, that if the intensity necessary to convert a body into a different state, compared with the heat or cold due to the mechanical or other production of that different state, be such as accords with the relations of intensity and heat which we observe in the voltaic apparatus, we have a proof that some of the effects which are usually referred to "latent heat," are in fact nothing more than the recondite operations of resistance to electric current*.

46. In our investigation into the cause of the heat of com-

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* Some experiments, which I have not time to refer to at present, render this hypothesis more than probable.

bustion, it will be necessary to deduce our calculations from the electric intensity which is required in order to reduce the product of combustion to the state in which its elements were prior to combustion. The following is a list of these intensities, reckoning the decomposition of water into its gaseous elements as unity.

47. *Intensity necessary to decompose oxide of zinc into gaseous oxygen and metal,* from (42.) and (44.), is 3·7 pairs of Smee's battery, or 1·32 h.

48. *Intensity necessary to decompose protoxide of iron into gaseous oxygen and metal.*—From (43.), 3·3 of Smee's pairs $= i$; and from (41.), 2·8 pairs $= h$; whence $2{\cdot}8\, i = 3{\cdot}3\, h$, or $i = 1{\cdot}18$.

49. *Intensity necessary to decompose potassa into potassium and gaseous oxygen.*—From (44.) and (37.) we have $1{\cdot}93 + 1{\cdot}9 : 4{\cdot}06 + 1{\cdot}9 :: 1{\cdot}32\, h : 2{\cdot}05\, h$, the intensity required, which may be otherwise expressed by 5·74 of Smee's pairs.

Heat evolved by Combustion, when it terminates in the formation of an Electrolyte.

50. Finding that our information on the quantity of heat evolved by the combustion of metals was not very satisfactory, I have, without wishing to depreciate the labours of Dulong, Despretz and others, thought it right to bring forward such of my own experiments as are necessary in order to make my investigation complete.

51. I provided two glass jars. The smaller had an internal capacity of 90 cubic inches; and when placed within the other jar, as represented by fig. 7, the space left between the two was sufficient to contain three pounds of water. By means of a scale, *s*, suspended by wire from a thick fold of moistened paper, I was able to introduce a combustible within an atmosphere of oxygen, and by means of a heavy weight I could keep the paper sufficiently close to the top of the jar to prevent the escape of any considerable quantity of heated air, while at the same time it was not so tight as to prevent the admission of air as the oxygen was consumed. The increase of the temperature of the water was measured by a thermometer of great sensibility.

52. *The heat evolved by the combustion of zinc* was ascertained in the following manner. The smaller jar was filled with oxygen, placed in the other jar, and surrounded by three pounds of water, the heat of which was contrived to be as much below the temperature of the surrounding air as it was expected to exceed it at the close of the experiment. A piece

of phosphorus, weighing 0·4 grain, was then put into the scale, and over it I placed a heap of fine zinc turnings, weighing 50 grains. I now ignited the phosphorus, and plunged the scale into the inner jar. After the combustion had terminated, and the heat thereby evolved had been evenly distributed throughout the water by stirring, the increase of temperature was noted. The contents of the scale were then thrown into dilute sulphuric acid, and the volume of hydrogen thereby evolved indicated the quantity (generally about 15 grains) which had not been burnt. Two-tenths of a degree of heat were deducted from the observed heat, on account of the phosphorus, and an allowance having been made, on account of the capacity of glass for heat, the results were reduced to the standard of one pound of water.

53. The mean of several experiments conducted in the above manner, showed that the heat evolved by the combustion of 32·3 grains of zinc is able to increase the temperature of a pound of water by 10°·8.

54. *The heat evolved by the combustion of iron* was ascertained in a similar way. The iron was in the state of fine wire, and that portion of it that was not burnt was carefully collected, weighed, and deducted from the original quantity. The mean of several trials indicated that 28 grains could increase the temperature of a pound of water by 9°·48.

55. *Heat evolved by the combustion of potassium.*—This metal, in pretty large lumps, was introduced into an atmosphere composed of equal bulks of oxygen and air. I then introduced a stout iron wire, sharpened at the end, into the jar, and with it I cut the potassium into small pieces. Under this treatment it soon became so soft, that every time the rod was lifted it would draw out a string of metal. In this state it often ignited, and the experiment was spoiled on account of the partial formation of peroxide. However, by careful management, I succeeded in making some good experiments, in which nearly all the potassium was converted into *potassa*; and the exact quantity of unoxidized metal was ascertained by observing the volume of hydrogen evolved when the contents of the scale were exposed to the action of water. The mean of these showed that the heat evolved by the conversion of 40 grains of potassium into potassa is able to increase the temperature of a pound of water by 17°·6.

56. *Heat evolved by the combustion of hydrogen.*—The gas was burned in an atmosphere of oxygen, diluted with common air, by means of a jet furnished with a very narrow bore. A grain of hydrogen evolved as much heat as is able to increase the temperature of a pound of water by 8°·36.

57. We shall now proceed to examine how far the theory of resistance to electric conduction agrees with the above experimental results.

58. We have seen (47.), (48.) and (49.) that the intensities of the affinities which unite gaseous oxygen with zinc, iron, potassium and gaseous hydrogen, are as 1·32, 1·18, 2·05 and 1; and the proportional quantities of heat which were generated by the combustion of the equivalents of these bodies are 10°·8, 9°·48, 17°·6, and 8°·36, or 1·29, 1·13, 2·105 and 1, a ratio which is very nearly the same as that of the intensities just given. Hence we see that *the quantities of heat which are evolved by the combustion of the equivalents of bodies are proportional to the intensities of their affinities for oxygen.* Now I proved in my former paper[*] that a similar law obtains in the voltaic apparatus, in consequence of its heat being produced by resistance to conduction. And hence we have an argument that the heat of combustion has the same origin.

59. But our proof of the real character of the heat of combustion is rendered more complete by regarding *quantities* as well as ratios of heat. From the quantity of heat generated by the motion of a given current along a wire of known resistance, we can deduce the quantities of heat which, according to the theory of resistance to electric conduction, ought to be produced by the combustion of bodies; and then these theoretical deductions may be compared with the results of experiment.

60. The mean of three careful experiments detailed in my former paper[†], shows that if a wire, the resistance of which is an unit, be traversed by an electric current of 1°·88 Q[‡] for one hour, the heat evolved by that wire will be able to increase the temperature of a pound of water by 15°·12. Now I have ascertained experimentally that a pair consisting of amalgamated zinc and platinized silver, excited by dilute sulphuric acid, is able to propel a current of 0°·168 Q against the whole resistance of the circuit, when that resistance is 5·2; consequently, a similar pair can propel a current of 0°·168 Q × 5·2 = 0°·874 Q against the resistance which I have called an unit. But from (42.), the intensity necessary to separate oxide of zinc into zinc and gaseous oxygen is to the intensity of one of Smee's pairs as 3·7 : 1. Consequently, the electricity produced

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[*] Philosophical Magazine, October 1841, S. 3. vol. xix. p. 275. (70.)

[†] Ibid. p. 266.

[‡] I beg to remind the reader that my *degree*, expressed thus (1°Q), indicates that quantity of current electricity which, after passing constantly during one hour, is found to have electrolized a chemical equivalent expressed in grains; as, 9 grains of water, 36 grains of protoxide of iron, &c.

by the union of zinc and gaseous oxygen must be sufficiently intense to propel a current of $0^\circ{\cdot}874\,Q \times 3{\cdot}7 = 3^\circ{\cdot}234\,Q$ against an unit of resistance. Now 1°·88 Q, when urged against an unit of resistance, was able in one hour of time to increase the temperature of a pound of water by 15°·12; therefore 3°·234 Q could, in the same circumstances, produce $\left(\frac{3{\cdot}234}{1{\cdot}88}\right)^2 \times 15^\circ{\cdot}12 = 44^\circ{\cdot}74$ of heat. But in (70.) of my former paper, I proved that the same quantity of heat should always (according to the theory which refers the whole of the heating power of the voltaic apparatus to resistance to the electric current) be produced by a given quantity and intensity of electrolysis, whether the resistance opposed to the current be small or great. Wherefore, the heat, which on these principles ought to be generated by the combustion of 3·234 equivalents of zinc, is 44°·74; or, in other words, *one* equivalent, or 32·3 grains of zinc, should generate heat sufficient to increase the temperature of a pound of water by 13°·83.

61. Now, as I have before stated, the quantities of heat evolved by the combustion of the equivalents of bodies, ought, according to the theory of resistance to electric conduction, to be proportional to the intensities of their affinities for gaseous oxygen. These, in the cases of zinc, iron, potassium, and hydrogen, are 1·32, 1·18, 2·05 and 1. Hence 13°·83, 12°·36, 21°·47, and 10°·47, are the quantities of heat which ought, according to our theory, to be produced by the combustion of 32·3 grains of zinc, 28 grains of iron, 40 grains of potassium, and 1 grain of hydrogen.

62. By comparing these results of theory with the quantities of heat, 10°·8, 9°·48, 17°·6, and 8°·36*, which were (53—56.) obtained from experiment, it will be seen that the former exceed the latter by about one quarter. Considering the difficulty of preventing some loss of heat, in consequence of the escape of air from the mouth of the inner jar (51.) during the first moments of combustion, &c., it will, I think, be admitted that experiment agrees with the theory as well as could have been expected.

63. I conceive, therefore, that I have proved in a satisfactory manner that the heat of combustion (at least when it terminates in the formation of an electrolyte) is occasioned by resistance to the electricity which passes between oxygen and the combustible at the moment of their union. The amount of this resistance, as well as the manner of its opposition, is immaterial both in theory and in experiment; and if the resist-

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* Crawford, whose method was well adapted to prevent loss of heat, obtained 9°·6. More recently, Dalton observed about 8°·5.

ance to conduction be great, as it most probably is when potassium is slowly converted into potassa by the action of a mixture of oxygen and common air; or little, as it probably is when a mixture of oxygen and hydrogen is exploded; still the quantity of heat evolved remains proportional to the number of equivalents which have been consumed, and the intensity of their affinity for gaseous oxygen.

64. That the heat evolved by other chemical actions, besides that which is called *combustion*, is caused by resistance to electric conduction, I have no doubt. I cannot, however, enter in the present paper upon the experimental proof of the fact.

Broom Hill, Pendlebury, near Manchester,
October 5, 1841.

XVIII. *On the Development of the Cosines and Sines of Multiple Arcs. By* HENRY MALDEN, *M.A., late Fellow of Trinity College, Cambridge, and Professor of Greek in University College, London*[*].

IN the Philosophical Magazine for August 1841, [S. 3. vol. xix.] Mr. Booth has developed the cosine of a multiple arc in descending powers of the cosine of the simple arc, by the application of a new theorem in the calculus of finite differences, and has demonstrated that cos $n\,\theta$ cannot be so expanded when n is either negative or fractional.

As this is the case, it seems sufficient to present the expansion of the integral multiple in an easier way.

$$\cos (n+1)\,\theta = 2 \cos \theta \,.\, \cos n\,\theta - \cos (n-1)\,\theta :$$

hence
$$\frac{\cos n\,\theta}{\cos (n+1)\,\theta} = \frac{\cos n\,\theta}{2 \cos \theta .\ \cos n\,\theta - \cos (n-1)\,\theta},$$

i. e.
$$= \frac{1}{2 \cos \theta - \dfrac{\cos (n-1)\,\theta}{\cos n\,\theta}};$$

and as the fractional term in the denominator is of the same form as the fraction on the first side of the equation, this may be developed in the form of a continued fraction, thus:

$$\frac{\cos n\,\theta}{\cos (n+1)\,\theta} = \cfrac{1}{2 \cos \theta - \cfrac{1}{2 \cos \theta - \cfrac{1}{2 \cos \theta - \&c.}}},$$

in which $2 \cos \theta$ recurs n times as the integral part of the partial denominators, and the last fractional part is

$$\frac{\cos 0\,\theta}{\cos \theta}, \text{ or } \frac{1}{\cos \theta}.$$

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[*] Communicated by the Author.

ex. gr. $$\frac{\cos 3\,\theta}{\cos 4\,\theta} = \cfrac{1}{2\cos\theta - \cfrac{1}{2\cos\theta - \cfrac{1}{2\cos\theta - \cfrac{1}{\cos\theta}}}},$$

$$= \frac{(4\cos\theta)^3 - 3\cos\theta}{8\,(\cos\theta)^4 - 8\,(\cos\theta)^2 + 1}:$$

and similarly,

$$\frac{\cos n\,\theta}{\cos\,(n+1)\,\theta} = \frac{\cos\theta.\,\{(2\cos\theta)^{n-1} - n\,(2\cos\theta)^{n-3} + \&\text{c.}\}}{\cos\theta\;\{(2\cos\theta)^{n} - (n+1).(2\cos\theta)^{n-2} + \&\text{c.}\}}.$$

And as the fraction on the right-hand side of the equation is formed from the fraction on the left-hand side by taking it to pieces, and then reconstructing it in the reverse order, without the omission or cancelling of any part, not only are the fractions equal, but the numerators are equal, and the denominators are equal; and

$$\cos n\,\theta = \cos\theta\,.\Big\{(2\cos\theta)^{n-1} - n\,.\,(2\cos\theta)^{n-3}$$
$$+ \frac{n\,.\,n-3}{1\,.\,2}\,(2\cos\theta)^{n-5} - \frac{n\,.\,n-4\,.\,n-5}{1\,.\,2\,.\,3}\,(2\cos\theta)^{n-7}$$
$$+ \frac{n\,.\,n-5\,.\,n-6\,.\,n-7}{1\,.\,2\,.\,3\,.\,4}\,(2\cos\theta)^{n-9} - \&\text{c.}\Big\}.$$

which is the usual series in a form slightly varied.

It will not be difficult to make the induction strict, by showing, that, if the form of series be true for $\cos\,(n-1)\,\theta$ and $\cos n\,\theta$, so that $\frac{\cos\,(n-1)\,\theta}{\cos n\,\theta}$ may be expressed as the ratio of two series of the proposed form, since

$$\frac{\cos n\,\theta}{\cos\,(n+1)\,\theta} = \cfrac{1}{2\cos\theta - \cfrac{\cos\,(n-1)\,\theta}{\text{con}\; n\,\theta}},$$

$\cos\,(n+1)\,\theta$ will come out to be of the proposed form likewise.

The general form of the coefficient, after the second term, or the coefficient of the mth term, is

$$\frac{n\,.\,(n-m)\,.\,(n-m-1)\,.\,.\,.\,.\,.\,(n-2\,m+3)}{1\,.\,2\,.\,3\qquad.\,.\,.\,.\,.\qquad(m-1)},$$

as Mr. Booth has shown in another shape.

Sin θ may be expanded in a similar manner, and I am not aware that this series has ever been given.

$$\sin(n+1)\theta = 2\sin n\theta\,.\cos\theta - \sin(n-1)\theta:$$

hence $$\frac{\sin n\theta}{\sin(n+1)\theta} = \frac{\sin n\theta}{2\sin n\theta\,.\cos\theta - \sin(n-1)\theta},$$

$$= \frac{1}{2\cos\theta - \dfrac{\sin(n-1)\theta}{\sin n\theta}} = \cfrac{1}{2\cos\theta - \cfrac{1}{2\cos\theta - \cfrac{1}{2\cos\theta - \&c.}}},$$

as before; but in this series, while $2\cos\theta$ recurs n times as the integral part of the partial denominators, the last fractional part is $\dfrac{\sin 0\theta}{\sin\theta}$, or 0.

ex. gr. $$\frac{\sin 3\theta}{\sin 4\theta} = \cfrac{1}{2\cos\theta - \cfrac{1}{2\cos\theta - \cfrac{1}{2\cos\theta}}}, = \frac{4\cos^2\theta - 1}{8\cos^3\theta - 4\cos\theta}:$$

and similarly, $$\frac{\sin n\theta}{\sin(n+1)\theta}$$

$$= \frac{(2\cos\theta)^{n-1} - (n-2).(2\cos\theta)^{n-3} + \dfrac{(n-3).(n-4)}{1\,.\,2}(2\cos\theta)^{n-5} - \&c.}{(2\cos\theta)^{n} - (n-1).(2\cos\theta)^{n-2} + \dfrac{(n-2)(n-3)}{1\,.\,2}(2\cos\theta)^{n-4} - \&c.}.$$

And it may be shown, as before, that if $\dfrac{\sin(n-1)\theta}{\sin n\theta}$ be of the proposed form, $\dfrac{\sin n\theta}{\sin(n+1)\theta}$ will also be of the proposed form.

But it is not true that, as in the case of the cosines, $\sin n\theta$ is equal to the series in the numerator, and $\sin(n+1)\theta$ equal to the series in the denominator. This arises from the neglect of the last member of the continued fraction, viz. $\dfrac{\sin 0\theta}{\sin\theta}$, or $\dfrac{0}{\sin\theta}$.

To reconstruct series which shall be equal to $\sin n\theta$, and $\sin(n+1)\theta$, this fraction must be taken into account. Of course, the numerator will make no difference, but the denominator will be a constant multiplier; and

$$\sin n\theta = \sin\theta\,.\Big\{(2\cos\theta)^{n-1} - (n-2)\,.\,(2\cos\theta)^{n-3}$$
$$+ \frac{(n-3).(n-4)}{1\,.\,2}(2\cos\theta)^{n-5} - \frac{(n-4).(n-5).(n-6)}{1\,.\,2\,.\,3}(2\cos\theta)^{n-7}$$
$$+ \&c.\Big\}.$$

The reason will now be seen of my keeping $\cos\theta$ as a multiplier outside the brackets in the series for $\cos n\theta$.

In the series for the sine, the coefficient of the mth term is

$$\frac{(n-m)\,.\,(n-m-1)\,\ldots\ldots\,(n-2m+2)}{1\,.\,2\,.\,3\quad\ldots\ldots\ldots\quad(m-1)}.$$

A difficulty has been raised with regard to these series, that, if $\theta+\frac{2\pi}{n}$ be substituted for θ, one side of the equation will become $\cos\{n\theta+2\pi\}$, which is the same as $\cos n\theta$, and consequently that side of the equation will not be changed, while on the other side the powers of $2\cos\left(\theta+\frac{2\pi}{n}\right)$ will be very different from the powers of $2\cos\theta$. The difficulty however is only apparent. The *sum* of the series is the same in either case, whether it involve powers of $2\cos\theta$, or powers of $2\cos\left(\theta+\frac{2\pi}{n}\right)$. This may be easily verified in simple arithmetical examples.

It is curious, that if the continued fraction

$$\cfrac{1}{2\cos\theta-\cfrac{1}{2\cos\theta-\&c.}}$$

be supposed to be continued infinitely, it is equal to $\cos\theta \pm \sqrt{-1}\,.\sin\theta$.

University College, London,
December 22, 1841.

XIX. *Additional Note on the History of* Fernel's *Measure of a Degree. By Professor* De Morgan.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

TO my last communication on this subject I add the following, which I cannot help thinking is completely decisive on the subject. You will remember that the assertion made by me is, that Fernel's 68·096 Italian miles are not (as Delambre and others make them) more than 69 English miles, but really less than $64\frac{1}{2}$ English miles. In my last I left upon any one who would dispute this assertion the onus of proving that Fernel's pace was longer than five English feet: I will now disprove this alternative.

Fernel himself has a table of his own measures, in which, as was usual, he makes the *geometrical* pace to be five feet, and the Italian mile 1000 paces; and he always styles his degree as 68·096 *Italian* miles. He mentions besides a *common* mile which he says is of 1000 *common* paces. But of these common paces, which are his own and those of all men of ordinary stature, he says that five make six geometrical paces. His words are, "Horum autem passuum (qui mei et cujusque hominis staturæ mediocris sunt) quinque sex geometricos passus efficiunt." Hence, according to him, the common pace is *six* of his feet, or the simple step (half of the pace) is three feet. Now if this foot were as long as the English foot, this gives too much, for it is well known that the yard is a long and forced step for a man of moderate stature, whose walking step is rather under than over 30 inches; and Fernel says he walked from the palace to St. Denis and found it 5950 *common* paces, and also the whole length of Paris, which he found 2110 paces. But to make 68,096 geometrical paces exceed 69 English miles, his step must have exceeded an English yard and 2½ inches. There is then this alternative:—if his walking step, described by himself, was that of other men, his degree is demonstrably four or five miles wrong; but if he would have us believe that he walked, or rather strided, 5950 paces, at the rate of 38½ inches each step, or half-pace, he must have been a deceiver, either of himself or others, to an extent which makes his evidence incapable of being received.

I remain, Gentlemen, yours faithfully,

University College, Jan. 1, 1842. A. De Morgan.

XX. *Plausible Reasons and positive Proofs, showing that no Portion of the Devonian System can be of the age of the Old Red Sandstone. By the Rev.* D. Williams, *A.M., F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

PROFESSOR Sedgwick and Mr. Murchison having recently contended for the identity of the Old Red Sandstone with the many varied rocks of the Devonian system, the former in a late supplemental memoir to his "Synopsis of stratified Rocks inferior to the Old Red Sandstone," and the latter by using them as convertible terms in your Journal for December last, I feel it a duty I owe to truth, to caution geologists and your readers generally, that it *is not the fact*, and never can reasonably be assumed to be so, without a violation of the only safe laws which govern observation and legitimate induction.

Professor Sedgwick draws his conclusion from the following syllogism:—"Any formation with fossils intermediate between the Carboniferous and Silurian systems, must have an intermediate position, must therefore be on a parallel of some part of the Old Red Sandstone which fills that whole intermediate position." Athenæum, No. 736, page 937.

Admitting the validity of the organic test for the sake of argument, it has not yet been shown that the Devon and Cornish organic fossils are of the requisite intermediate type or proportion, taking the mountain limestone as a standard of reference, to justify the identification contended for; nay more, I contend that when the elements of the premises are fairly distributed, the result will tend rather to impeach the Old Red hypothesis than to confirm it.

It is remarkable that the *proportions* given in Lonsdale's Table *, from the limited area of the South Devon limestones, should agree so nearly with Phillips's Tables, drawn from the entire region: it tends at least to warrant an inference that future discoveries in the Devonian and Carboniferous systems generally may not cause those proportions to vary materially one way or the other.

Up to the period of the publication of the Palæozoic Fossils, the aggregate numbers stand thus:—

Mountain and magnesian lime species	420
Devonian species	275†

Of the latter, fifty-one are determined as mountain lime species. We appear thus to attain good evidence that the earth in by-gone ages has *not* been visited by periodical universal suspensions of animal and vegetable life, followed by perfectly new creations, while a great moral caution is tacitly but emphatically conveyed as to the application of testimony, drawn from organic remains, to establish remote equivalents.

Omitting the probability that many mountain lime species will yet be found in Silurian strata or their coeval deposits, the revolting and, to me, monstrous conclusion implied by the Old Red hypothesis is, that during the period of the Old Red Sandstone (a formation which has been repeatedly stated to pass by well-defined transitions into the mountain limestone) a ratio of 224 species of corals, encrinites and hardy molluscs out of every 275 must have been finally and universally obliterated from the creation, and replaced by 369 new species. An analysis of the Devonian species by Phillips, gives

Polypiaria.—34 species, of which two only are mountain lime.

Crinoidea.—16 species, of which four only are carboniferous.

Conchifera Plagimyona ...	26	Mountain Lime	2
... Mesomyona ...	20	... ...	2

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* Geol. Trans. 2nd series, vol. v. part iii. p. 737.

† Phillips's Palæozoic Fossils, p. 165.

Conchifera Brachiopoda	83	Mountain Lime	21
Gasteropoda	36	... ...	8
Cephalopoda Monothalamacea	7	... ...	3
... Polythalamacea	44	... ...	8
Crustacea	9	... ...	1

Here we have presented to us the strange anomaly of the *Polypiaria,* a class of creatures which have the widest distribution of any, and are apparently constituted better than any to resist physical mutations and local accidents, being nearly annihilated by conditions under which 25 per cent. of the higher orders of *Brachiopoda* and *Gasteropoda* flourished, while the *Crinoidea* could only exist in a like proportion with the latter.

If we refer to the class *Crustacea,* the Devonian species bear the same proportion to the Silurian as to the mountain limestone, one and one. So that according to the Old Red supposition, these creatures were universally effaced in the ratio of 36 for every one which survived; effaced too during the short intermediate interval implied in the upper Ludlow rocks. If, however, we regard the aggregate amount of species, the Silurian exceeds the Devonian by something more than four times, viz. 37 to 9, and exceeds the Carboniferous as 37 to 10. So that looking at the Trilobites as a family, they appear rather to have been introduced in an advanced period of the Devonian epoch, to have flourished by myriads in the meridian of the Silurian, and to have died out in the Carboniferous.

Mr. Lonsdale's table gives 11 species as mountain lime, 13 as Silurian, and *not one as Old Red Sandstone* out of 40 figured by Mr. Murchison. The Table comprises 62 species, collected from the upper strata of the Devonian rocks, and therefore, according to the imaginary Old Red equivalence, verging on the confines of the mountain limestone; for I repeat it as a fact which can never be disproved, that some of the South Devon limestones are included in the floriferous series, while others with the killas overlie it. Mr. Lonsdale's list does not include one mountain lime coral, while 8 out of 19 are Silurian; so that, according to that enumeration, the *upper* Devonian approximates rather to the Silurian than to the Carboniferous system, and, in truth, rather shows the Silurian to be the intermediate formation than as Mr. Lonsdale puts it. Mr. Lonsdale however relieves himself from this untoward result, by pretty broadly stating that " Professor Sedgwick and Mr. Murchison have proved for the first time that the culm-measures of the central part of Devon are newer than any of the stratified groups associated with them, and of the age of the carboniferous series*." I earnestly invite and entreat Mr. Lonsdale to adduce the proofs in opposition to an array of structural facts which I purpose to detail in the course of this communication. I can well anticipate that the results of six years' patient and cautious and rigid investigation of the structure of Devon and Cornwall by myself will go for nothing, or that the great facts I have already announced, or am now about to advert to, will be considered at present as not worth refuting. The indifferent arbitration between Mr. Lonsdale's proofs and mine, I confidently leave to free and just and generous minds hereafter, when geology, under happier auspices and in whiter days, shall be something more than a mere conventional thing of arbitrary and false hypotheses of facts perverted to *à priori* views, or forced and tortured to artificial adaptations; till then I patiently bide my time; and having discharged a duty I owe to truth, it will be indifferent to me whatever pleasantries of identification may be indulged in or received. The twenty-mile diameter of the Devon and Cornwall beds may be designated as Old Red or New Red, or Oolite or Chalk, for aught I care, for either would be equally true.

The meaning of that remarkable hypothesis is sufficiently obvious. Professor Sedgwick and Mr. Murchison assume the Silurian and Cambrian deposits to be the base-line of geology as a system, that therefore all the formations of the earth must conform, somehow or other, to this arrangement, and that the several circumferential zones or systems are capable of being determined over the different regions of the earth by their organic remains. We thus attain, by another process of calculation, by another road, as it were, to the doctrine of universal formations of Werner. It comes recommended to us by so much convenience, so much simplicity; it offers such a facility of comprehending what would otherwise appear the most abstruse problems, that it seems a pity nature should ever have been permitted to cast stumbling-blocks or other occasions of falling along such a royal road to knowledge. The argument carried out to its necessary consequences, and fairly represented in all its collateral relations, involves, however, many startling propositions, which I must not now dwell upon; but it is right we should know whether we are required to believe that there have been as many final and universal suspensions of animal life succeeded by as many new creations as there are systems, because if such be not implied, we may reasonably suppose that when or wherever the bed of a sea or ocean became dry land, the creatures

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* Geol. Trans. 2nd series, vol. v. part iii. p. 723.

which lived on it would previously shift their habitat to other submarine abodes, where they may have existed into higher epochs, and thereby mislead the geologist of after days to confidently identify, as mountain limestone for instance, rocks which in reality are of the age of the New Red Sandstone,—a perplexing occurrence, which can only be effectually cured by supposing an universal annihilation of animal and (it would be very convenient to include) vegetable life, and a creation of entirely new genera and species, of both; otherwise the oolite fossils of India, again, which have been so cautiously reported by Col. Sykes, and so triumphantly appealed to by Mr. Lonsdale, may be of the age of the European tertiaries. Devonian and Silurian fossils, like those discovered by Mr. Lyell, may be found above, below, or in the American coal-measures; and many other antipodal equivalents might be very seriously deranged.

The assumption that the Cambrian rocks are the true base line of geology as a system, is, however, altogether gratuitous; it appears to have been founded on the supposed paucity of their organic contents, their mineral aspect and character, and because in the region where they occur, they are the nethermost in a descending section from the New Red Sandstone: they *do* however contain organic remains, which Professor Sedgwick states to be Silurian species, so that, by the way, one *System* would have served instead of two. The number of species, or the greater or less abundance of individuals, I take to be altogether a matter of accident quite independent of age or time. A large area of Cornwall, bounded by about the meridian of St. Austell and the parallel of Padstow, is equally if not more destitute of them. Chemical analysis confirms the evidence afforded by the presence of organic remains, that they are of derivative origin, regenerated from the destruction (not by abrasion) of pre-existing rocks; they have no pretensions to be classed with those primitive or primary slates,

> " ...... which rose from out the trackless sea,
> Those slates for boys to scrawl, when boys should be."

The Cambrian slates are not simple minerals, or compounds of simple minerals, which have been governed by the laws of definite proportions, but plain secondary aggregates of varying per-centages, of commonly eight or ten constituents; they are parted from the Old Red Sandstone by only 7500 feet of strata which graduate into them, and together with the Silurian prefer far better pretensions to be admitted into the Old Red Sandstone system, than any of the vast group of Devon and Cornwall.

In that very hasty classification (from a very meagre list of fossils only) of Devon and Cornwall with the English Old Red Sandstone, Mountain limestone and Coal-measures, many important facts were overlooked or disregarded, which I am bound to advert to among other plausible reasons against it.

The Old Red Sandstone, in its ordinary and familiar types, extends from the Orkneys to Monmouthshire, Bristol and the Mendips, and with its Harlequin and other beds, and fishes, is widely expanded in Russia, &c.; yet we are required to believe that in a distance of only twenty miles which part the Mendips from the Quantocks, it reciprocally assumes an opposite mineral and organic contrast; that in the Quantocks, its conglomerates, cornstones, Harlequin beds and tilestones, have been miraculously metamorphosed into a red quartz rock with hard shales, into several varieties of excellent limestone, and a four-mile thick mass of crystalline glossy chlorite and talc slate; that its thirty or more species of Fishes, which are entombed in the Old Red in such myriads as are described in the vivid and graphic work of Mr. Miller of Cromartie, and its 25 genera and species of shells figured by Mr. Murchison, all not only most unaccountably disappear, but more unaccountably are represented by seven or eight species of *Silurian* corals. It surely violates all analogy and probability to suppose that in so short a geological span as exists between the Quantocks and the Mendips, or between Exmoor and Monmouthshire, there should not be something like mutual interpolations of some of their strongly characterized mineral divisions. Why, the Morte slates alone (No. 5.) of Exmoor, if they are an inch, I verily believe are at least four miles in true diametrical thickness, thicker than the Old Red Sandstone and Silurian together, with 550 feet to spare towards the Cambrian. They rival Dunkerry Beacon in elevation within 58 feet*, and extend from Lundy Island to near Bridgewater, a range of upwards of sixty miles; and surely some portion of this vast sediment of such lightly suspended materials would be drifted less than half the distance, to the Mendips or Monmouthshire, if they had been deposited in the same sea and at the same time with the Old Red Sandstone.

I must omit the other subdivisions of Exmoor as open to the same objections I have just stated, or to those previously suggested under organic fossils. Major Harding exhibited some fossils at Plymouth from No. 4, which I did not see, but which I understood were considered to be Silurian. No. 6

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* Geol. Survey, De la Beche, page 14 *et seq.*

sometimes assumes an aspect and character as must probably perplex the advocates of the Old Red hypothesis to suggest a reason why its fish and fossils are all wanting. No. 7 is the first, hitherto, which has afforded the Trilobite, while its disproportion to mountain lime fossils is as 153 to 1000.—Phillips's Palæozoic Fossils, p. 177*.

We are thus introduced in order to the "Carboniferous or Great Mountain Limestone" of Devonshire; it is here divested of its mountain character, of its lithological types, of upwards of four hundred of its animal remains, and of all its usual mineral associations. Everywhere on the north and south, and from east to west, it occurs in small elliptical bunches in that most un-English formation the Coddon Hill grit, a formation perfectly *sui generis* so far as my experience has gone. This "Great Carboniferous Limestone" has a maximum thickness of about thirty-five feet, and, instead of its bold elevation in the Mendips near at hand, is posited in holes and contracted hollows, a result which can be shown in some localities to be due to its incapability of resisting atmospheric decomposition in an equal degree with its jaspery and cherty matrix. The coral reef analogy, which has been ascribed to the mountain limestone elsewhere, will by no means obtain here, as corals and encrinites are rarely if ever met with. Altogether we must admire the ingenuity and great powers of imagination, which could detect it under its multifarious disguises. The same or greater difficulties must have beset the discoverers of the English coal-measures in the floriferous group, when we consider their respective subdivisions, and that, with the exception of a few plants, a little impure culm and carbonaceous mud, they correspond in none. Where is the Millstone grit, where the Pennant, where is the Clay-ironstone, where is the Coal? It is surely strange, and passing strange, that in an assumed identification of *three* great English *formations*, in such close parallels, they should not correspond in one; that it should be all repulsion and no affinity; that the discordance should be everywhere and the resemblance nowhere! We have no occasion to draw so profusely on imaginary possibilities to determine the New Red Sandstone, the Lias, the Oolites, the Greensand, or the Chalk in Devonshire!

Meeting thus as we do with such an accumulation, such mutual implications of contradiction and denial of this hypo-

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* For the several numbers here referred to, viz. 4, 5, 6, 7, &c., relating to the author's subdivisions of the Devonian System, the reader is requested to see the section in the Phil. Mag. for Jan. 1840, page 60.

thesis, in lithological characters and common associations, we are somewhat prepared to expect that superposition, the only law we possess, may add the force and authority of its positive proofs to the abundant plausible reasons we have met with, and dissipate this cabinet hypothesis into its original elements, by confirming the respective independence and individuality of the several formations which it has been attempted to class in common.

I presume if I show by abundant evidences afforded in nearly all parts along the confines of the killas and culm rocks (from one extremity to the other an irregular line of about seventy miles), that the *killas overlies the culm-field*—a great truth, which is proved everywhere and contradicted *nowhere*—it will sufficiently demonstrate that neither the killas slates of Devon and Cornwall, nor anything below them can possibly be the equivalent of the Old Red Sandstone; that the floriferous series, or so called culm-measures, can in no sense represent the great English coal-field; and that the Coddon Hill grit with its Posidonia limestones, which underlie them both, never can be raised to the parallel of the mountain limestone.

I was occupied for as much time as I could spare of two years, in taking observations along those confines, and I state it without affectation, that at times I was in an agony of embarrassment at the facts I met with, till I discovered at Ashton and Doddiscomb Leigh, on the north of Chudleigh, the Coddon Hill grit with its Posidonia limestone brought up in a broad anticlinal line, entailing a permanent northern dip on the floriferous beds to the north of it, and an equally permanent southern dip on the floriferous rocks, with their included coral limestones and killas on the south of it.

I may here observe, that beyond an *à priori* view which had commonly been entertained of the paramount high antiquity of the killas (with which at one time I was as much impressed as any one), founded on its supposed inorganic character, and its metalliferous and mineral peculiarities, no reason could be assigned for it, drawn from undisputed evidences of its infraposition; it was a sort of problem which had been solved by common consent, although no one had troubled himself to discuss the elements.

It is necessary here that I refer your readers to a section I gave of this region from the Bristol to the British Channel, in your Journal for January 1840, page 60; if they could refer to the Ordnance or any good map, also, I should be better understood. In that section it will be seen that No. 8, the Coddon Hill grits, which constitute the lower division or base line of the culm-measures in the north of Devon, are brought up in the south through the upper culm-measures, throwing them, and at times irregular portions of the overlying killas, off on either shoulder with a counter or reversed dip north and south, as I have before explained at Ashton and Doddiscomb Leigh. This great dynamical axis extends from Doddiscomb Leigh, near Exeter on the east, to the western extremity of South Tregear Down, at Tor-Park Water on the north-west of Launceston, where, by a moderate declination, it becomes depressed below the upper culm-measures; and between this and the sea, a distance of ten miles, is only met with again on this range in one instance, which I shall shortly notice. Now if I had nothing more to advance, the evidences connected with this great southern axis never could be shaken, but wherever nature proclaims any great truth she is sure to be lavish of her proofs. The Coddon Hill grits of the north of Devon, with their Posidonia limestones, *are* brought up on the south, and in the range I have mentioned. No one disputes the fact; but they *are protruded with a reversed dip through the upper culm-measures, and do not trough up from below them*, in the way they ascend in the north of Devon. The southern parallel, or fractured counterpart of the upper culm-measures, ranges from the south of South Tregear Down, branching off there from the floriferous in mass, and extends by Launceston and Heathfield to Dartmoor. Now that southern range, *repeatedly alternating with*, and finally *underlying*, the killas, altogether negatives the supposition, that the killas may be a member of the Exmoor group below the Coddon Hill grit. The fact is an insuperable bar to any such hypothesis. In consequence of the depression of No. 8 at South Tregear Down, the upper culm-measures only are exposed from thence to Boscastle. All parties fortunately are agreed that the black culmy slates of Boscastle and Forrabury Cliffs are either carboniferous, carbonaceous, or floriferous,—Dr. Boase, Mr. De la Beche, Professor Sedgwick, Mr. Murchison, Mr. Phillips, and myself. In truth, if we follow the coast line from Clovelly and Hartland Point on the north, we bring them with us in unbroken continuation to Boscastle, where, and within any radius of five miles, we meet with no intrusive igneous rocks. The beds have been affected about here in only a trifling degree by the great anticlinal axis which is so amply exposed on to the west extremity of South Tregear Down, ten miles to the eastward; they *have*, however, been affected by it, for they all dip to the north at a low angle, commonly 10°, rarely I think exceeding 15°, so that a line drawn due south from Valency bridge and Forrabury church, to the parallel of Camelford, along the surface of the beds, would describe a long low undulation; if, however, we follow the coast, we observe, about three furlongs north of Bossinny, the Coddon Hill grit exposed under all the circumstances of a valley of elevation, underlying the killas on either flank of the valley, both of them dipping to the north on the north side, and south 15° W. on the south side. The place is marked "Mill" on the Ordnance map, and bears W. 10° N. from South Tregear Down, from hence they undulate to Boscastle, as in the section No. 1.

But let us return to Boscastle, to the bottom of the town near the harbour. We stand upon beds of the culm-measures, and observe distinctly the same beds before us and behind us inclining at a low angle to the north; from hence we take our departure up the steep road which leads to Camelford; happily several cuttings have been made to facilitate the ascent. Examining the slates as we ascend, we at length observe the black or dark slates behind us to have lost much of their carbonaceous character, and to have assumed a deep blue, a bluish and then a grayer aspect; and these again (at about where we leave the houses) to pass into a pale green subcrystalline killas, which continues for about 200 yards up the cutting to the top of the hill, and thence accompanies us the entire way to the slate quarries of Delabole and Tintagel. Ascending the hill just mentioned, we do not observe any variation in the angle or direction of the dip 10° N., or any evidence whatever of a fault or shift of the beds, and are constrained to conclude that the killas and culm-measures, which appear as if tranquilly piled one upon the other, have been quietly elevated by the same movements and at the same times.

Again, before we leave the houses at the top of the town, a road branches off to the eastward, leading to Minster and Lesnewth. It is hardly necessary to premise that the northern margin of the killas hereabout describes an irregular line, sometimes considerably waved, at others indented, or overlying the culm rocks in rather long spurs; following this road to Minster, at about a quarter of a mile, we meet with a small cutting about five feet deep. I collected and numbered the specimens on the spot, and ascertained pretty accurately the thickness of their beds. They were as follows:—

Pale green killas	2 feet.
Slate, glossy blue, and dark blue	1 foot.
Black culmy schist, sometimes soiling the fingers	2 feet.

So that here we have a fine feather edge of the killas, resting nearly flat on the floriferous or culm rocks.

Advancing further to the eastward, the road is on the culm rocks to a little beyond some houses marked "Treworrel" on the Ordnance sheet; here we observe a thick narrow spur of killas descending from the main body to near St. Juliott, on the north. Here also we see the culm slates dipping below it, and the overlying killas to dip in the same angle and direction, north five points east; but we do not here trace them foot by foot, nor is the passage of the one into the other, in consequence, so clearly exposed as in the former instances.

There are some good sections to the same effect about Alternoǹ and Five Lanes, but the floriferous rocks there belong to another parallel range of hills, and it would involve me in too long details to particularize them at present. I therefore request your readers will accompany me to South Petherwin and Landlake on the south-west and south of Launceston. It is prudent here that I fortify myself by observations recorded by two distinguished geologists, in every word of which (except "overlying") I perfectly concur. In your Journal for April 1839, page 246, Professor Sedgwick and Mr. Murchison write, "last summer one of us found near South Petherwin, what appeared an *unequivocal passage* between the fossiliferous slates and the overlying (*under*lying) culm series." The "*unequivocal passage*" is what I am most concerned in, as bearing remarkably on the same occurrence, and the clear indisputable sections at Boscastle; for so far as the ridge of South Petherwin and Landlake is concerned, the superiority of either killas or culm rocks might be argued, although the balance of evidence certainly inclines to the former. If we take a north and south traverse from Tresmarrow on the south-west of Launceston, in the direction of Petherwin church, we observe the culm slates to be bowed over in a low broad arch of about a mile across,—ascending from the north at Tresmarrow, being flat at "Quarry" on the south of it, and dipping south 12° at "Does House" beyond, down to the brook below, crossing which we come upon killas, at the foot of the hill, which, being cut into for the road, shows it also to dip easily to the south 10° to 15°. Before we leave this locality, I request your readers will bear in mind, that about half a mile west by north of South Petherwin church, and on the north-east of Bolathan, the Coddon Hill grit is exposed, and ranges distinctly from thence by Congdon, to nearly north of Trevasper.

Mr. Phillips, in his extremely valuable work on the "Palæozoic Fossils of Devon and Cornwall," has detailed with such remarkable accuracy and fidelity all the structural phænomena he met with, that I have great reason to lament his

Survey was necessarily so limited. That accomplished geologist, however, departed somewhat from his characteristic caution, when (pp. 195 and 196) he states that "the Petherwin slates rise from under the black carbonaceous shales;" and I venture to hope, on re-perusing the section on the "Petherwin Group," he will correct the only blemish I have noticed in his admirable work. The evidences about Landlake are of themselves, I take it, altogether too equivocal to justify our forming a judgement one way or the other; my original inference that the Trilobite slates of Exmoor, No. 7, were brought up in the south, in the same order of succession in which they descended on the north, was however deduced from the same premises and localities (according to his description) as that drawn by Mr. Phillips, viz. a traverse I made from Launceston due south by "Bad Ash" to the Clymenien limestones at Landlake. I was betrayed into error by the tempting order of succession, by a supposed higher antiquity of the killas, and the doubtful nature of the evidences: the following is a brief detail of what I since observed.

The barren ridge of St. Stephen's Down, a mile north of Launceston, is the Coddon Hill grit, No. 8, and a portion of the great south anticlinal axis before noticed. South of this the overlying culm grits and slates are carried to near the Landlake lime quarries by an easy undulation, as from Tresmarrow to Petherwin, before noticed. In a large abandoned quarry immediately north of the brook, the carbonaceous slates dip south 10°, and in an old low cutting in the road below, have an apparent tendency to flatness. Twenty paces further conduct us to the little brook, and fifty or sixty more to the killas and Landlake limestones; if, however, having crossed the brook, we turn into a gate behind the cottage, and follow an old road-way through the wood, till we attain the first open field, we shall see a small stone quarry for the roads: this is undoubted Coddon grit; it is situated in the rising ground a little south of the brook and valley; dips at a low angle somewhat north of N.E., and must be very near the northern foot of the Landlake limestone and killas*.

Having returned to the road by the cottage, and ascended a short distance south, we observe a spoil heap of the killas and lime rocks close on our right hand, and immediately on our left, over the fence, near the old lime-kiln, a deep old slate quarry; but this is a very different slate from that on our right-hand above; it underlies it, and is almost destitute of

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* It is right to observe that isolated dips like this, in the Coddon Hill grit, are never to be relied on, its beds, in this respect, in their descent from the plane to the base of a hill being oftentimes full of contradictions.

fossils, and intimately resembles some of the culm-beds and building slates quarried at St. Thomas, below Launceston. This was first pointed out to me by Mr. Pattison, who considers the slate, I believe, to be the same as the Yeolm Bridge flags. This eastern extremity of the Petherwin ridge is, however, distinctly arched over, and undulates on to Bottonnel, and just behind Trebollet Shop, about two miles to the south, where it is abrupted by a protruded axis of Coddon grit and floriferous (see section, No. 3), which ranges continuously in hills and downs, from Inny Foot and Mount Pleasant on the Tamar, to the west of Alternon, between which and Camelford it is concealed beneath the wild moors under Brown Willy, &c.

The abundant alternations seen about Linkinghorne and South Hill are very satisfactorily accounted for by their being in the actual confines of the killas in mass, and the great spur of No. 9, which, as hereafter stated, extends uninterruptedly from the culm-field to Stoke Climsland. About a furlong or less to the N.E., the E. and the S.E. of the Landlake lime-quarries, the Coddon grit in its full force is exposed and extends to the Tamar and many miles beyond, so that remembering its re-appearance at Congdon and near Trevasper, on the west, the evidences *per se*, are here at least quite as good for supposing it to be the mineral axis of the arch of killas, as for supposing the Petherwin slates to rise from below it. If, however, we examine the cutting at Landue, on the new road from Launceston to Callington, about two miles S.E. of the Landlake quarries, and not more than half a mile due east of the killas in mass, we find it to consist of the lower culm-beds; there is no true killas there, but the carbonaceous slates appear sometimes to have been neutralized by an admixture of killas matter (composite beds), and they contain thin calcareous plates with several of the *Landlake fossils*. The unconformity once imagined here, has been nearly all cut out; in truth, when I first saw it, I recognised it as nothing more than one of the many thousand complicated involutions which everywhere almost, in a greater or less degree, characterize the culm rocks,—multiplied unconformities among themselves.

If, again, we take a north and south traverse from a mile east of Werrington on the north of Launceston, we observe the culm rocks, either No. 8 or 9, to be exposed continuously from the great body of the culm-field to the south of Stoke Climsland, and at Linkinghorne and South Hill, within three miles of Callington, where we observe them *repeatedly alternating* with killas, till finally they are altogether overlaid by it. The alternations here are commonly between a glossy gray or delicate pale-green killas, and the dull olive *sandstones* with shales and plants, or with Coddon Hill grits with manganese; it is, in truth, these repeated alternations of the killas and culm *slates* or schists (the olive grits being to a considerable extent absent), oftentimes reciprocally adulterated by common intermixture, or by a series of mutual interlockings, of either rudely or well-defined wedge-shaped masses, along the confines of the killas and floriferous groups, which furnish such abundant sources of difficulty and perplexity along that parallel, independent of the unequal contortions of the beds and contradictions of dip noticed by Mr. Phillips. It is a circumstance worthy of attention, that while these fantastic inflexions implicate the beds of the Coddon grit and floriferous almost everywhere, they very rarely affect the killas; the former, however, abound in cotemporaneous traps and greenstones along that confine line; and if we regard the killas as ejected volcanic ash or mud occurring at intervals in this region during the deposition of the culm rocks, and eventually superseding them during a long succeeding period, one source of difficulty at least will be removed, and it will perhaps materially assist us in solving many embarrassing problems both here and elsewhere.

Again, on the south of Callington, little more than a mile, we fall in with a considerable area of the Coddon Hill grit and floriferous, extending about six miles from east to west, and two miles from north to south*; it is entirely insulated in the killas; but to object to its identity on that account would be altogether captious or invidious, for it is critically the same dull olive sandstones with black shales and plants underlaid by the same distinctly defined Coddon Hill grits with manganese, that we observe on the north and south of the great culm-field. At a deep cutting in the turnpike road to Plymouth, at Penter's Cross, the upper culm-beds are seen, beyond any reasonable doubt, first to intercept the killas in a trough of a quarter of a mile wide, then to alternate with it, and finally, to be surmounted by it, both of them being first inflected and then fractured through by the protrusion of the Coddon Hill grit about a hundred yards west of the cutting. About Pillaton, round the confines of the killas and floriferous, we observe repeated alternations and interlockings of the two, as seen at Stoke Climsland, South Hill, and Linkinghorne.

It is here convenient that we adjourn to Tavistock: the floriferous rocks are brought into the greater portion of this town, from the west flank of Dartmoor, in uninterrupted con-

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* See Section, No. 2.

tinuation from their great central area; at the west extremity of the town, by the turnpike gate on the Callington road, it is killas, delicate pale-green killas. A few years since a shaft was sunk in it there on a copper lode, which was sufficiently productive for about sixteen fathoms deep; below this they came to a very black carbonaceous slate and grit beds, where they lost the lode; they sunk the shaft into the same black greasy shales and grits to the depth of forty fathoms, but could neither sink through them or recover the lode. I saw the black stuff as it came up in buckets, and could not well doubt its identity with the same schist and shale I observed both in and about the town.

If we cross the river Tavy by the bridge almost behind the Bedford Arms, and take the eastward road, we shall shortly come to a quarry, and a little beyond it to a cutting a few feet only above the river bed. The quarry is chiefly greenstone trap, but it rests on and is parted by culmy schists, with glance surfaces, which blacken the fingers; the cutting beyond it is the same slate and shale, which, like the beds in the quarry, dip into the hill which rises steeply above us; that hill is all killas; a part of Whitchurch Down delicate gray, or pale-green killas with quartz veins.

From hence we must return to the broad range of Coddon Hill grit, which constitutes the great south anticlinal line; near Greeston bridge, on the road from Launceston to Tavistock, it bifurcates, as it were, into two ridges, in consequence of their having intercepted a valley of killas about a mile in width between them *, shown to be so by the fact alone of several shafts at Kelly having been sunk through the killas into the subordinate Coddon Hill grit after its manganese. The southern range of the grit, oftentimes apparent, sometimes concealed by the floriferous or killas, extends in about an east and west direction to Brentor, whence it is carried by an easy curve to Lidford, where it corresponds with the anticlinal axis on the other side of Dartmoor, at Ashton and Doddiscomb Leigh. The northern range is continued from South Tregear and St. Stephen's Down to Lew Trenchard, east and west, whence it curves parallel to the southern range, striking off to the north-east, and conforming to the outline of Dartmoor, which from Sourton and Sourton Tor, on the north-west of Amicombe Hill, is closely invested by it in one broad uninterrupted zone, round to Meavy on the south-east of Tavistock. I have identified it in its true type, out of the metamorphic influence which the granite has exercised on its

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* See Section, No. 2.

inner zone, during the whole of its circuit. At Bovey Tracey it is concealed for a short distance, the floriferous beds there abutting against the granite; in many other instances, on the east and south-east of Dartmoor, the floriferous beds are seen in advance of it, and resting on it, and together underlying the killas and coral limestones; in truth, the entire distance from the west limit of Bovey Heath near Chudleigh to Ivy Bridge and Cornwood, is one continuous series of highly instructive sections for the advocates of the Old Red hypothesis. Thus a traverse from Haytor Rocks to Bickington, gives in ascending succession, granite, Coddon grit, floriferous, coral limestone, killas.

From Rippon Tor by Owlecombe and Ashburton Down to Ashburton, north and south, the section is granite, Coddon grit, killas, coral limestone with anthracite and culm-shales, killas.

North of Ashburton, from Summer House by Waterleat and Rewmill, the section is granite, Coddon grit, killas, greenstone, coral limestone, killas.

From Uppercot by Holne chase to Ashburton, the ascending sequence is granite, Coddon grit, killas.

From Holne Lee by Holne to Buckfastleigh, the order is granite, Coddon grit, floriferous, killas, coral limestones, killas.

From Buntingdon Cross by Walliford Down to Dean Prior, the section is granite, Coddon grit, floriferous, killas, coral limestone, killas.

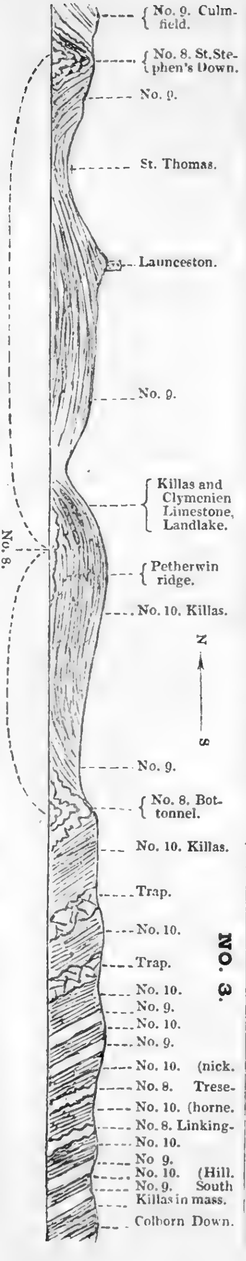
South of this I have met with no well-exposed section till we come to Bittaford Bridge, two miles east of Ivy Bridge. The Coddon grit here and at Ivy Bridge, appears to contain a somewhat higher per-centage of silica and less of alumina; its ordinary aspect presents a coarse jaspery character; at Bittaford Bridge it differs but little from this; at Ivy Bridge it is a finer variety, but still a coarse one. If at either Bittaford Bridge or Ivy Bridge it be an altered rock, it is the Coddon grit altered, and *not* killas, for it strikes with and runs into undoubted Coddon grit on either side of Bittaford Bridge and Ivy Bridge. Weathered specimens show it to possess the same striped, plated and layered character, weathering irregularly, the thin and more jaspery plates standing considerably in relief of the layers which contain more alumine, and which have been as deeply eroded as the other projects in relief. I do not believe, however, that at either of these places it has been at all altered by the granite, for near a by-lane which leads from Ivy Bridge to Peak-mill, a little south of east, I observed the same rock interpolated among perfectly unaltered killas; and the same rock is included in true Coddon grit at Camelford, Milton Abbot, Lidford, and on the east of Oakhampton. I mention this because Professor Sedgwick and Mr. Murchison, in their memoir on South Devon, have coloured it in the section from Dartmoor to Plymouth as "altered slate."

At Bittaford Bridge the Coddon grit dips S.S.E., the killas beyond dipping the same.

At Ivy Bridge it dips south 60°, but I have ascertained no precise dip of the killas ridge south of it, but in several places to the west it dips south.

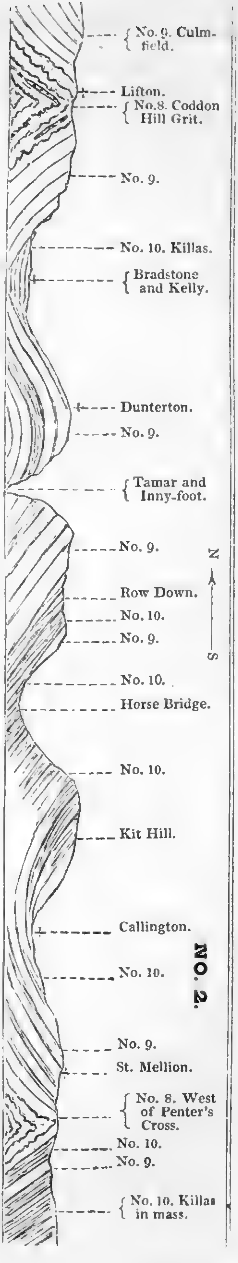
At Shaugh Prior on the N.W. it assumes its true Coddon Hill type, and underlies the killas, both of them dipping S. by W. The section adopted by Professor Sedgwick and Mr. Murchison, fig. 8, from Dartmoor to Abbots Kerswell, is to me a marvel and a mystery, as I state emphatically and in unreserved sincerity, that I have never met with one instance which has negatived the superposition of the killas in its relation to the floriferous.

I have stated that the hypothesis of the Cambrian slates being the base of geology as a system, is altogether gratuitous; it may be prudent therefore that I guard myself from misconstruction. Geology, like every other science, has its prescribed limit,—the extreme shore, on which the inevitable decree is written, "So far shalt thou go and no farther." The great Creator has enabled every thinking and well-constituted mind to apprehend and feel that it necessarily must be so, from the operation of diurnal natural causes only; the progress of inquiry in all the other kindred sciences, quickens our perception of, and strengthens our faith in, a great and good Creator; if it do not, the sublimest conclusions we arrive at are more heartless and worthless than the rattle of childhood or the toys of dotage. On what analogy, then, shall the geologist assume that his science becomes deficient in moral induction, and retrogrades as he advances in the comprehension of its profoundest truths; that it will only conduct him to an unbrooded abyss of darkness and chaotic masses of matter acted on by crystalline forces alone, but otherwise destitute of the higher evidences of His eternal superintending wisdom, power and providence? No. The nethermost apparent foundations of the earth, when or wherever they shall be attained, if possibly they may not teem with the most exquisite organization of plant and animal, will doubtless testify of life and light, and all the secondary agents by which the physical government of this world of ours has been and is now conducted. At that terminus the natural records end and the moral



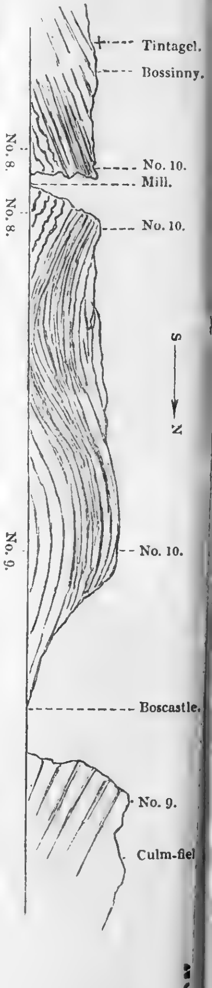
NO. 3.

No. 3.—Section from the Culm-field, north of Launceston, to Colborn Down, near Callington.



NO. 2.

Section from the Culm-field, north of Lifton, to the Killas in mass south of Callington.



scheme begins, and the interpreter having (so far as may be in his works,) "found out the Almighty to perfection," will be left to the Word and his own free-will to form his judgement of the past, present and future, and "He who reads the secrets of the heart," will know to what end he applies his knowledge, and whether his faith be or be not without virtue.

I have now, Sirs, to apologize to yourselves and readers for this lengthy and I fear tedious detail, but I felt I owed a duty to what I believed to be the truth to make it. I have an utter aversion to polemical discussions of any kind; circumstances of a local nature have unfortunately involved me in the present controversy with gentlemen to whom I owe the deep obligation of a great addition to my amount of knowledge; it was their valuable labours elsewhere which stirred my ambition to elucidate the structure of a region, some of whose headlands and hills and valleys bound my daily landscape. I unaffectedly feel gratitude, and something more, for what they have taught me. I accord my full confidence in their observations when they do not write and travel too fast; but for this, I am well assured, I should not now feel constrained to adopt the mortifying alternative of proclaiming how essentially I differ from the results of their Devon and Cornwall labours, and in this spirit I venture to conclude,

"Amicus Sedgwick, amicus Murchison, sed magis amica veritas."

I have the honour to remain, Gentlemen,
Your obliged and obedient Servant,
Bleadon, near Cross, Jan. 1, 1842. D. WILLIAMS.

XXI. *On the Invention of the Signs + and −; and on the sense in which the former was used by* Leonardo da Vinci.

By A. DE MORGAN, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN M. Libri's History of Mathematics, vol. ii. p. 46, he attributes the invention of the signs + and − to Leonardo da Vinci, for which he cites, without quotation, a manuscript of that extraordinary man in the library at Paris. I think it most likely that M. Libri, when called upon, will be able to substantiate his own assertion by an express quotation which shall put the matter beyond doubt: but it will be necessary that he should do so, on account of the occurrence of + and − in one manuscript at least of Da Vinci's, in a sense altogether different from that of algebraic addition and subtraction.

Being lately at the British Museum, I was informed by my friend Mr. Panizzi that they were in possession of a large number of notes by L. da Vinci, consisting of manuscript scraps on various subjects. These are bound up in a book, which was shown to me. It consists of loose memoranda principally on mechanics, and is written in Italian, from right to left, after the Oriental fashion, as if it had been written with the left-hand: any Christian who wishes to make it out, must use a looking-glass, and examine the reflexion of the page, instead of the page itself. At a glance I was struck with the frequent occurrence of the sign + in arithmetical processes, with an occasional use of —: but on looking more closely, I found that + always stood for the numeral 4, which at the beginning of the book is a cross with two adjacent points joined, and gradually degenerates into nothing but the cross, sometimes with one of the corners a little crooked. For example (p. 221 of the manuscript), we see (putting Da Vinci's written numerals into common type, except in the case of the 4)

$$\frac{3}{+}\ \genfrac{}{}{0pt}{}{-}{-}\ \frac{1}{2}\times\frac{2}{3}, \quad \text{or} \quad \frac{1}{2}\div\frac{2}{3}=\frac{3}{4}.$$

This sign of division × (a very significant one, evidently imagined from the process) is used throughout: but the accidental mark = by which equality is here denoted in effect, does not occur again, or it might have been supposed that Da Vinci was the first who used the sign =. Other instances in the same page are—

$$\frac{8}{9}\Big(\frac{2}{3}\times\frac{3}{+}, \quad \text{or} \quad \frac{8}{9}=\frac{2}{3}\div\frac{3}{+},$$

$$\frac{35}{36}\Big|\,\frac{5}{6}\times\frac{6}{7}, \quad \text{or} \quad \frac{35}{36}=\frac{5}{6}\div\frac{6}{7},$$

$$\frac{+}{5}\times\frac{5}{6}\Big)\frac{2+}{25}, \quad \text{or} \quad \frac{4}{5}\div\frac{5}{6}=\frac{24}{25}.$$

On turning to other pages, I found myself able to interpret every operation, + invariably (as far as I examined) meaning 4, and × being the symbol of division.

The occurrence of such a use of the sign + makes it desirable that M. Libri should quote those manuscripts of Leonardo da Vinci in which it takes a totally different signification. As I said before, I should think he would turn out to be right in his assertion, it being a point in which an historian who has shown himself both acute and accurate, and versed moreover in the manuscripts of this very writer, could hardly be deceived; nevertheless it would be a strange thing if Da Vinci, having got into the habit of using + for 4, should afterwards fix upon this very + as the sign of addition; and not less strange if after having established the latter in his own mind, he should have gradually dropped into the former.

I remain, Gentlemen, yours faithfully,

University College, Jan. 12, 1842. A. DE MORGAN.

XXII. *Ephemeris of the Periodical* [Encke's] *Comet*, 1842; *and on the Mass of the Planet Mercury. By* Professor ENCKE, *in an Extract from a Letter to* Mr. AIRY*.

THE comet of short period comes to perihelion on the 12th of April next; and judging from its present course and from former experience during Mr. Henderson's residence at the Cape of Good Hope, it may be well observed there during the end of April and the whole of May, and probably also in June. May I then trouble you with the request to get the accompanying ephemeris conveyed there, or to the southern hemisphere generally; and also to provide for its circulation in England? I should think that, with the present active communication between England and all parts of the world, there is yet time enough to send the ephemeris to the astronomer at the Cape of Good Hope by the end of March; and it would have the greater interest for him, because to this time *no* return of the comet since 1819 has been missed. The ephemeris is not strictly founded on all the earlier observations, because it was impossible for me, notwithstanding all my endeavours, to reduce completely the observations of the comet made here in 1838. The compared stars still required some more observations for their determination. Meanwhile, I have provisionally determined a correction of the last observations of 1838, or rather a correction of the calculations relating to that time, which will not be far from the truth. Upon this provisional reduction, and the calculation of the perturbations produced by Jupiter *alone*, the elements now given for 1842 are founded. Judging, however, from earlier experience, I believe that even with this incomplete calculation, the predicted place will be wrong by only about a few minutes. The error certainly cannot amount to half a degree, consequently the comet must be found, if it is really visible, and if the search be made with care.

The difference between the observation and the calculation in the year 1838, is with great probability to be attributed to a very important error in the hitherto received mass of Mercury. This mass is the same that Lagrange, in the Berlin Mem. 1782, has derived from a hypothesis on the density of the planets, according to which the density ought to increase

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* As circulated by the Astronomer Royal.

very much with their proximity to the sun. Laplace has adopted the number without alteration. In order to obtain approximately the mass of Mercury, which has *never* yet been determined, I have first substituted the most accurate values of the other masses. Thanks to your excellent observations, which are completely confirmed by Bessel (Bessel finds the mass of Jupiter $= \frac{1}{1047871}$), we are quite clear about the most important of them, that of Jupiter. The others have a smaller influence even if they are erroneous. I have then used a double calculation. In the first I have assumed that, till 1835 inclusive, the earlier determined elements were quite accurate. In fact the errors of 1832 and 1835 are small. With this assumption I have sought to remove the apparent error in 1838, by an alteration of the mass of Mercury, and an alteration of the constant of the resistance. This succeeded perfectly, and I obtained a slightly changed constant of the resistance, and a mass of Mercury $= \frac{1}{3091947}$, about $\frac{2}{3}$rds of the former mass. In the second calculation I have endeavoured to remove the errors of 1832 and 1835, as well as that of 1838 (each being previously treated as if it had arisen from an erroneous time of perihelion-passage), by an alteration of the same elements, namely, the mass of Mercury, and the constant of the resistance. Here remain likewise only small differences, if the constant of the resistance is left quite unchanged, and the mass of Mercury assumed at $\frac{1}{4865751}$, or about $\frac{5}{12}$ths of that of Lagrange. This last determination I intend to adopt as a basis for accurate comparison, and correct it more closely. It has the truly remarkable peculiarity that with it the *densities* are

☿ = 1·12 ♀ = 0·92 ♁ = 1·00 ♂ = 0·95
☉ = 0·25 ♃ = 0·24 ♄ = 0·14 ♅ = 0·24;

consequently the solar system appears to be composed of two sets of bodies, whose densities are either nearly = 1, or = $\frac{1}{4}$, and which are separated by the interval between Mars and Jupiter, where there exists no large planet, but only the four small ones.

Dec. 20, 1841.

Ephemeris of the Periodical [Encke's] Comet, 1842.

Elements.

Epoch, 1842, April 12, 0^h, Berlin Mean Time.

Mean Anomaly	359° 58′ 34″·3	
Sidereal Daily Motion	1070″·61433	
Angle of eccentricity .... $\phi =$	57° 39′ 13″·8	($e = \sin\phi$)
Longitude of Perihelion	157 30 4·7	Mean Equinox of April 12, 1842.
Longitude of ascending node ..	334 39 1·8	
Inclination	13 20 24·8	

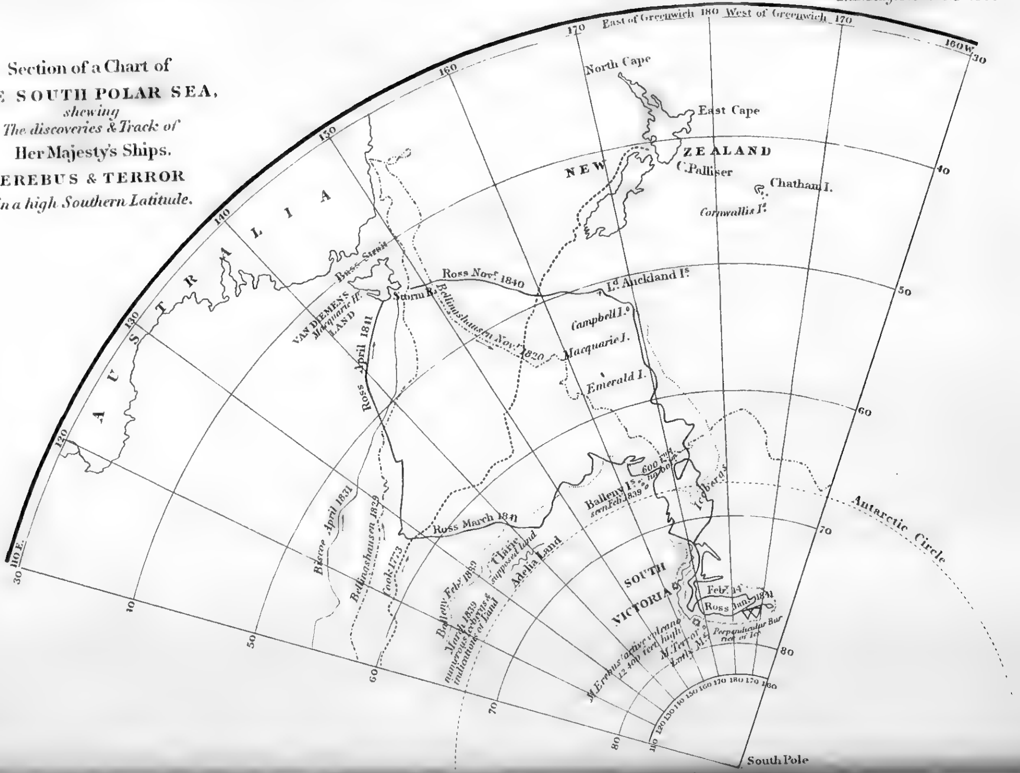
The Right Ascensions and Declinations are referred to the Mean Equinox of April 12, 1842.

1842. 0h Berlin Mean Time.	Right Ascension of Comet.		Declination of Comet.	Comet's Log. Distance.	
	In Arc.	In Time.		From the Earth.	From the Sun.
	° ′ ″	h m s	° ′ ″		
Jan. 26	351 30 5	23 26 0·3	+ 4 46 57	0·29172	
28	352 13 3	23 28 52·2	+ 5 3 20	0·29020	
30	352 57 19	23 31 49·3	+ 5 20 21	0·28847	0·15938
Feb. 1	353 42 55	23 34 51·7	+ 5 38 0	0·28652	
3	354 29 53	23 37 59·5	+ 5 56 17	0·28433	
5	355 18 14	23 41 12·9	+ 6 15 13	0·28192	
7	356 8 1	23 44 32·1	+ 6 34 47	0·27925	0·12431
9	356 59 15	23 47 57·0	+ 6 54 59	0·27634	
11	357 51 59	23 51 27·9	+ 7 15 49	0·27316	
13	358 46 15	23 55 5·0	+ 7 37 16	0·26971	
15	359 42 6	23 58 48·4	+ 7 59 21	0·26598	0·08421
17	0 39 36	0 2 38·4	+ 8 22 3	0·26196	
19	1 38 47	0 6 35·1	+ 8 45 22	0·25762	
21	2 39 44	0 10 38·9	+ 9 9 17	0·25297	
23	3 42 32	0 14 50·1	+ 9 33 47	0·24799	0·03759
25	4 47 17	0 19 9·1	+ 9 58 51	0·24265	
27	5 54 3	0 23 36·2	+10 24 29	0·23694	
Mar. 1	7 2 57	0 28 11·8	+10 50 39	0·23084	
3	8 14 6	0 32 56·4	+11 17 20	0·22433	9·98237
5	9 27 37	0 37 50·5	+11 44 29	0·21736	
7	10 43 39	0 42 54·6	+12 12 4	0·20992	
9	12 2 18	0 48 9·2	+12 39 59	0·20197	
11	13 23 44	0 53 34·9	+13 8 11	0·19345	9·91550
13	14 48 4	0 59 12·3	+13 36 32	0·18433	
15	16 15 28	1 5 1·9	+14 4 54	0·17454	
17	17 46 2	1 11 4·1	+14 33 5	0·16401	
19	19 19 51	1 17 19·4	+15 0 50	0·15267	9·83250
21	20 56 58	1 23 47·9	+15 27 51	0·14041	
23	22 37 20	1 30 29·3	+15 53 40	0·12713	
25	24 20 48	1 37 23·2	+16 17 46	0·11268	
27	26 7 0	1 44 28·0	+16 39 22	0·09691	9·72841
29	27 55 11	1 51 40·7	+16 57 28	0·07962	
31	29 44 9	1 58 56·6	+17 10 46	0·06061	
April 2	31 31 54	2 6 7·6	+17 17 28	0·03964	
4	33 15 24	2 13 1·6	+17 15 17	0·01650	9·60828
6	34 50 12	2 19 20·8	+17 1 20	9·99102	
8	36 10 22	2 24 41·5	+16 32 19	9·96325	
10	37 8 46	2 28 35·1	+15 44 48	9·93347	
12	37 38 22	2 30 33·5	+14 36 1	9·90241	9·53775

(*Ephemeris Continued.*)

1842. 0h Berlin Mean Time.	Right Ascension of Comet.		Declination of Comet.	Comet's Log. Distance.	
	In Arc.	In Time.		From the Earth.	From the Sun.
	° ′ ″	h m s	° ′ ″		
April 14	37 33 57	2 30 15·8	+13 4 45	9·87118	
16	36 53 55	2 27 35·7	+11 12 0	9·84115	
18	35 40 44	2 22 42·9	+ 9 1 3	9·81365	
20	34 0 5	2 16 0·3	+ 6 36 53	9·78968	9·60596
22	31 59 10	2 7 56·7	+ 4 5 15	9·76985	
24	29 45 28	1 59 1·9	+ 1 31 50	9·75428	
26	27 25 26	1 49 41·7	− 0 58 32	9·74276	
28	25 4 15	1 40 17·0	− 3 22 15	9·73482	9·72610
30	22 45 37	1 31 2·5	− 5 37 0	9·72987	
May 2	20 31 57	1 22 7·8	− 7 41 41	9·72732	
4	18 24 36	1 13 38·4	− 9 36 5	9·72658	
6	16 24 5	1 5 36·3	−11 20 40	9·72716	9·83064
8	14 30 21	0 58 1·4	−12 56 16	9·72866	
10	12 43 0	0 50 52·0	−14 23 53	9·73074	
12	11 1 20	0 44 5·3	−15 44 38	9·73316	
14	9 24 36	0 37 38·4	−16 59 32	9·73574	9·91400
16	7 51 55	0 31 27·7	−18 9 34	9·73834	
18	6 22 28	0 25 29·9	−19 15 36	9·74088	
20	4 55 23	0 19 41·5	−20 18 25	9·74329	
22	3 29 53	0 13 59·6	−21 18 39	9·74554	9·98115
24	2 5 12	0 8 20·8	−22 16 53	9·74763	
26	0 40 35	0 2 42·3	−23 13 34	9·74954	
28	359 15 21	23 57 1·4	−24 9 6	9·75130	
30	357 48 51	23 51 15·4	−25 3 48	9·75292	0·03658
June 1	356 20 27	23 45 21·8	−25 57 53	9·75446	
3	354 49 34	23 39 18·3	−26 51 30	9·75595	
5	353 15 40	23 33 2·7	−27 44 42	9·75743	
7	351 38 16	23 26 33·1	−28 37 31	9·75897	0·08338
9	349 56 57	23 19 47·8	−29 29 52	9·76064	
11	348 11 24	23 12 45·6	−30 21 36	9·76248	
13	346 21 21	23 5 25·4	−31 12 29	9·76458	
15	344 26 39	22 57 46·6	−32 2 15	9·76698	0·12357
17	342 27 16	22 49 49·1	−32 50 36	9·76976	
19	340 23 17	22 41 33·1	−33 37 10	9·77298	
21	338 14 54	22 32 59·6	−34 21 35	9·77668	
23	336 2 26	22 24 9·7	−35 3 29	9·78092	0·15871
25	333 46 18	22 15 5·2	−35 42 28	9·78574	
27	331 27 4	22 5 48·3	−36 18 12	9·79117	
29	329 5 24	21 56 21·6	−36 50 22	9·79724	
July 1	326 42 7	21 46 48·5	−37 18 43	9·80397	0·18978

Section of a Chart of
THE SOUTH POLAR SEA,
shewing
The discoveries & Track of
Her Majesty's Ships.
EREBUS & TERROR
in a high Southern Latitude.



(*Ephemeris Continued.*)

1842. 0h Berlin Mean Time.	Right Ascension of Comet.		Declination of Comet.	Comet's Log. Distance.	
	In Arc.	In Time.		From the Earth.	From the Sun.
	° ′ ″	h m s	° ′ ″		
July 3	324 18 5	21 37 12·3	−37 43 1	9·81136	
5	321 54 12	21 27 36·8	−38 3 9	9·81942	
7	319 31 28	21 18 5·9	−38 19 3	9·82812	
9	317 10 50	21 8 43·3	−38 30 46	9·83745	0·21756
11	314 53 15	20 59 33·0	−38 38 24	9·84738	
13	312 39 31	20 50 38·1	−38 42 9	9·85786	
15	310 30 26	20 42 1·7	−38 42 14	9·86886	
17	308 26 37	20 33 46·5	−38 38 59	9·88032	0·24259
19	306 28 33	20 25 54·2	−38 32 43	9·89220	
21	304 36 35	20 18 26·3	−38 23 47	9·90443	
23	302 51 0	20 11 24·0	−38 12 32	9·91697	
25	301 11 54	20 4 47·6	−37 59 18	9·92977	0·26541
27	299 39 21	19 58 37·4	−37 44 24	9·94278	
29	298 13 17	19 52 53·1	−37 28 9	9·95596	

XXIII. *Notice of the Magnetometric, Geographical, Hydrographical and Geological Discoveries or Observations made by the Expedition under the command of* Capt. James C. Ross, R.N., F.R.S.; *being extracts from a Despatch addressed to the Secretary of the Admiralty**.

[With a Map, Plate II.]

I HAVE the honour to acquaint you with the arrival of Her Majesty's ship under my command, and the Terror, under my orders, this afternoon at this port.

I have further to report to you, for the information of my Lords Commissioners of the Admiralty, that in accordance with the intentions expressed in my despatch to you, dated from Hobart Town on the 11th of November last, I proceeded to Auckland Islands, and satisfactorily accomplished a complete series of magnetometric observations on the important term-day of November last.

Under all the circumstances, it appeared to me, that it would conduce more to the advancement of that branch of science for which the expedition has been more expressly sent forth, as well as for the extension of our geographical knowledge of the Antarctic regions, to endeavour to penetrate to the southward, or about the 170th degree of east longitude, by which the isodynamic oval, and the point exactly between the two foci

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* From a Return to the House of Commons, ordered to be printed eptember 6, 1841.

of greater magnetic intensity, might be passed over and determined, and directly between the tracks of the Russian navigator Bellinghausen, and our own Captain James Cook; and after entering the Antarctic circle to steer S.W. towards the Pole, rather than attempt to approach it directly from the north, on the unsuccessful footsteps of my predecessors.

Accordingly, on leaving Auckland Islands on the 12th December, we proceeded to the southward, touching for a few days at Campbell Island for magnetic purposes; and, after passing among many icebergs to the southward of 63° latitude, we made the Pack Edge, and entered the Antarctic circle on the 1st day of January 1841.

This pack presented none of those formidable characters which I had been led to expect from the accounts of the Americans and French; but the circumstances were sufficiently unfavourable to deter me from entering it at this time, and a gale from the northward interrupted our operations for three or four days. On the 5th January we again made the pack, about 100 miles to the eastward, in latitude 66° 45′ south, and longitude 174° 16′ east, and although the wind was blowing directly on it, with a high sea running, we succeeded in entering it without either of the ships sustaining any injury, and, after penetrating a few miles, we were enabled to make our way to the southward with comparative ease and safety.

On the following three or four days our progress was rendered more difficult and tedious by thick fogs, light winds, a heavy swell, and almost constant snow showers; but a strong water-sky to the S.E. which was seen at every interval of clear weather, encouraged us to persevere in that direction, and on the morning of the 9th, after sailing more than 200 miles through this pack, we gained a perfectly clear sea, and bore away S.W. towards the magnetic Pole.

On the morning of the 11th January, when in latitude 70° 41′ south, and longitude 172° 36′, land was discovered at the distance, as it afterwards proved, of nearly 100 miles, directly in the course we were steering, and therefore directly between us and the pole.

Although this circumstance was viewed at the time with considerable regret, as being likely to defeat one of the more important objects of the expedition, yet it restored to England the honour of the discovery of the southernmost known land, which had been nobly won, and for more than twenty years possessed, by Russia.

Continuing our course towards this land for many hours, we seemed scarcely to approach it. It rose in lofty mountain peaks of from 9000 to 12,000 feet in height, perfectly covered with eternal snow; the glaciers that descended from near the mountain summits, projected many miles into the ocean, and presented a perpendicular face of lofty cliffs. As we neared the land, some exposed patches of rock appeared, and steering towards a small bay, for the purpose of effecting a landing, we found the shore so thickly lined for some miles with bergs and pack-ice, and with a heavy swell dashing against it, that we were obliged to abandon our purpose and steer towards a more promising looking point to the S.E., off which we observed several small islands; and on the morning of the 12th I landed, accompanied by Commander Crozier and a number of the officers of each ship, and took possession of the country in the name of Her Most Gracious Majesty Queen Victoria.

The island on which we landed is composed wholly of igneous rocks, numerous specimens of which, with other imbedded minerals, were procured. It is in latitude 71° 56′ S., and longitude 171° 7′ E.

Observing that the east coast of the mainland tended to the southward, whilst the north shore took a N.W. direction, I was led to hope that, by penetrating to the south as far as practicable, it might be possible to pass beyond the magnetic pole, which our combined observations placed in 76° S. nearly, and thence, by steering westward, complete its circumnavigation. We accordingly pursued our course along this magnificent land, and on the 23d January we reached 74° 15′ S., the highest southern latitude that had ever been attained by any preceding navigators, and that by our own countryman, Captain James Weddell.

Although greatly impeded by strong southerly gales, thick fogs, and constant snow-storms, we continued the examination of the coast to the southward, and on the 27th we again landed on an island in latitude 76° 8′ S., 168° 12′ E., composed, as on the former occasion, entirely of igneous rocks.

Still steering to the southward, early the next morning, the 28th, a mountain of 12,400 feet above the level of the sea was seen, emitting flame and smoke in splendid profusion. This magnificent volcano received the name of Mount Erebus. It is in latitude 77° 32′ S., and longitude 167° E.; an extinct crater to the eastward of Mount Erebus, of a somewhat less elevation, was called Mount Terror. The mainland preserved its southerly tending, and we continued to follow it, until, in the afternoon, when close in with the land, our further progress in that direction was prevented by a barrier of ice stretching away from a projecting cape of the coast directly to the E.S.E.

This extraordinary barrier presented a perpendicular face of at least 150 feet, rising, of course, far above the mast-heads of our ships, and completely concealing from our view everything beyond it, except only the tops of a range of very lofty mountains in a S.S.E. direction, and in latitude 79° S.

Pursuing the examination of this splendid barrier to the eastward, we reached the latitude of 78° 4′ south, the highest we were at any time able to attain, on the 2nd February, and on the 9th, having traced its continuity to the longitude of 191° 23′ in latitude 78° S., a distance of more than 300 miles, our further progress was prevented by a heavy pack, pressed closely against the barrier; and the narrow lane of water by means of which we had penetracted thus far, became so completely covered by rapidly forming ice, that nothing but the strong breeze with which we were favoured enabled us to retrace our steps.

When at a distance of less than half a mile from its lofty icy cliffs, we had soundings with 318 fathoms, on a bed of soft blue mud.

With a temperature of 20° below the freezing point, we found the ice to form so rapidly on the surface, that any further examination of the barrier in so extremely severe a period of the season being impracticable, we stood away to the westward, for the purpose of making another attempt to approach the magnetic pole, and again reached its lat. (76° S.) on the 15th of February; and although we found that much of the heavy ice had drifted away since our former attempt, and its place in a great measure supplied by recently formed ice, yet we made some way through it, and got a few miles nearer the Pole than we had before been able to accomplish, when the heavy pack again frustrated all our efforts, completely filling the space of 15 or 16 miles between us and the shore. We were this time in latitude 76° 12′ S., and longitude 164°, the dip being 88·40, and variation 109·24 E. We were, of course, only 160 miles from the Pole.

Had it been possible to approach any part of this coast, and have found any place of security for the ships, we might have travelled this short distance over the land; but this proved to be utterly impracticable; and although our hopes of complete attainment have not been realized, it is some satisfaction to feel assured that we have approached the Pole more nearly, by some hundred miles, than any of our predecessors; and from the multitude of observations that have been made in both ships, and in so many different directions from it, its position can be determined with nearly as much accuracy as if we had actually reached the spot itself.

It had ever been an object of anxious desire with us to find a harbour for the ships, so as to enable us to make simultaneous observations with the numerous observatories that would be at work on the important term-day of the 28th of February, as well as for other scientific purposes; but every part of the coast where indentations appeared, and where harbours on other shores usually occur, we found so perfectly filled with perennial ice, of many hundred feet in thickness, that all our endeavours to find a place of shelter for our vessels were quite unavailing.

Having now completed all that it appeared to me possible to accomplish in so high a latitude, and at so advanced a period of the season, and desirous to obtain as much information as possible of the extent and form of the coast we had discovered, as also to guide in some measure our future operations, I bore away, on the 18th of February, for the north part of this land, and which, by favour of a strong southerly gale, we reached on the morning of the 21st.

We again endeavoured to effect a landing on this part of the coast, and were again defeated in our attempt by the heavy pack, which extended for many miles from the shore, and rendered it impossible.

For several days we continued to examine the coast to the westward, tracing the pack-edge along, until, on the 25th of February, we found the land abruptly to terminate in latitude 70° 40′ S., and longitude 165° E., tending considerably to the southward of west, and presenting to our view an immense space occupied by a dense pack, now so firmly cemented together by the newly-formed ice, and so covered by recent snow, as to present the appearance of one unbroken mass, and defying every attempt to penetrate it.

The great southern land we have discovered, whose continuity we have traced from nearly the 70th to the 79th degree of latitude, I am desirous to distinguish by the name of our Most Gracious Majesty Queen Victoria.

Following the edge of the pack to the N.W. as weather permitted, we found it to occupy the whole space between the N.W. shore of the great southern land and the chain of islands lying near the Antarctic circle, first discovered by Balleny in 1839, and more extensively explored by the American and French expeditions in the following year.

Continuing our course to the westward, we approached the place where Professor Gauss supposed the magnetic pole to be, and having obtained all the observations that were necessary to prove the inaccuracy of that supposition, we devoted some days to the investigation of the line of no variation; and having completed a series of observations, by which the isodynamic lines and point of greater magnetic intensity may be determined, and which I had left incomplete last year, I bore away on the 4th of April for this port.

A chart, showing more plainly the discoveries and track of the expedition, is herewith transmitted; and a more detailed plan, containing all magnetic determinations, shall be sent as soon as they are reduced.

I have much satisfaction in being able to add that the service has been accomplished without the occurrence of any casualty, calamity, or disease of any kind, and there is not a single individual in either of the ships on the sick-list.

It affords me the highest gratification to acquaint you, that I have received the most cordial and efficient co-operation from my well-tried friend and colleague Commander Crozier, of the Terror, and no terms of admiration that I can employ can do justice to his great merit; nor have the zeal and persevering devotion of the officers of both ships been less conspicuous, under circumstances of no ordinary trial and difficulty; and whilst the conduct of our crews has been such as to reflect the highest honour on their characters as British sailors, it has given to myself, Commander Crozier, and the officers of the expedition, the most confident assurance of more extended success in pursuing the important duties we have yet to fulfil.

H.M.S. Erebus, Van Diemen's Land, April 7, 1841.

XXIV. *Observations made at the Magnetic Observatory at Toronto, during a remarkable Magnetic Disturbance on the 25th and 26th of September*, 1841; *with Postscripts, containing the Observations of the same Disturbance made at the Magnetic Observatories of Trevandrum, St. Helena, and the Cape of Good Hope*[*].

(Illustrated by Plate I.)

THE interest which Mr. Airy's Circular Letter has excited on the subject of the magnetical disturbance, which was observed at Greenwich on the 25th of September last, makes it probable that considerable advantage may be derived, by immediate publicity being given to the observations which were made on the same day at the Magnetical Observatory at Toronto in Canada, showing the effects of the same disturbance in America.

In the regular course of the publication, proceeding under the direction and at the expense of Government, of the observations made at the Magnetical Observatories conducted by officers of the Royal Artillery, several months would necessarily elapse before the observations of September 25, 1841, would pass through the press. Under these circumstances, the MASTER-GENERAL of the ORDNANCE has approved of their immediate publication in a separate form, which will enable them to be communicated at once to the Directors of similar establishments in all parts of the globe; and the Committee of the BRITISH ASSOCIATION, appointed to conduct the co-operation of that body in the system of simultaneous magnetical and meteorological observations, have deemed this a fitting occasion for the employment of a portion of the grant placed at their disposal.

The abstracts received from the Observatory contain the observations ex-

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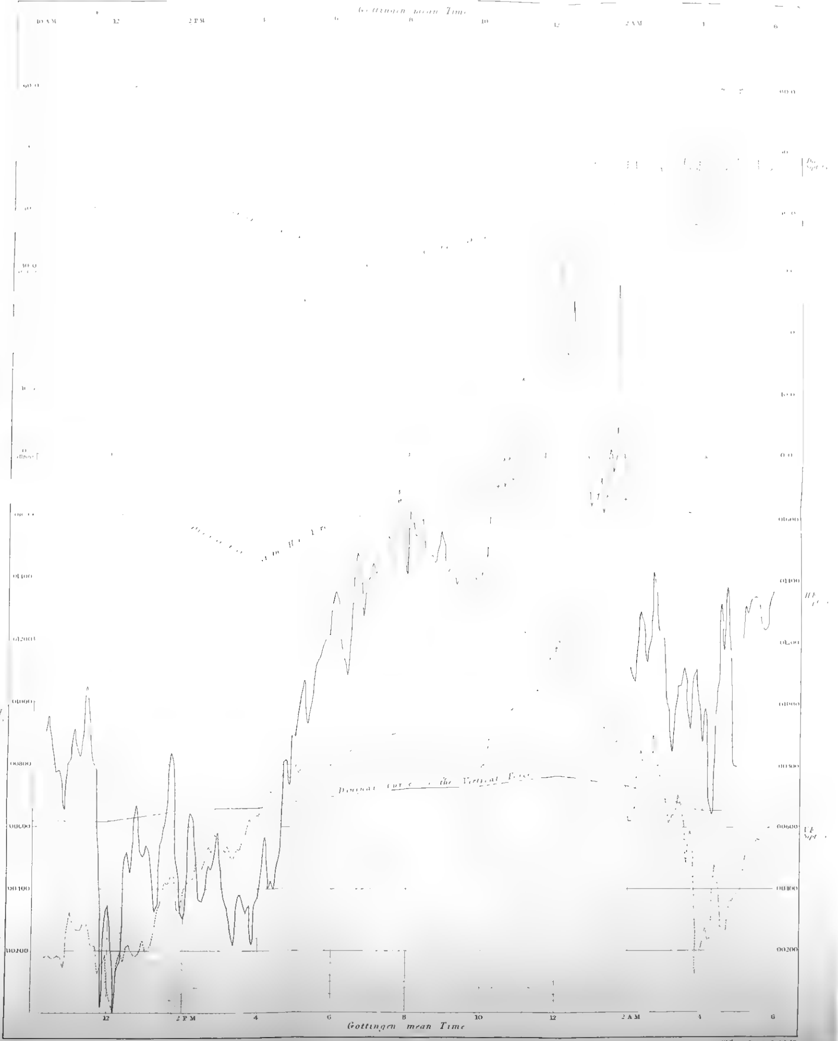
[*] Reprinted, by the kind permission of Lieut.-Col. Sabine, from the original publication, recently printed and circulated at the expense of the British Association (to whose forthcoming Report for 1841 it will be appended), with the approbation of the Master-General of the Ordnance. Mr. Airy's Circular Letter has already appeared, in our last Supplement, vol. xix. p. 505. We are enabled to add, on the authority of Prof. Dove, in a letter addressed to Mr. H. Croft, that at Berlin, "on the 25th of September, the magnetic needle was observed both in the morning and at noon, and that no considerable perturbation was visible."

sed in the scale divisions in which they are made; these have be

CURVES REPRESENTING THE OBSERVATIONS OF THE DECLINATION & HORIZONTAL AND VERTICAL FORCE MAGNETOMETERS
at TORONTO in CANADA during a MAGNETIC DISTURBANCE on the 25th of September 1841



Ascending Numbers denote Increasing Easterly Declination and Increasing Force.

pressed in the scale-divisions in which they are read: these have been converted by Lieut. Riddell, R.A., both in the tables and plate, into the more convenient forms,—of angular value for the declination,—and for the horizontal and vertical force, of the proportion which the changes of those forces bear to their whole amount. The plate also shows the mean daily curve of each element during the month of September, the comparison of which with the curve on the 25th affords a measure of the magnetic disturbance on that day, both being reduced to the same Zeros.

The perseverance with which the magnetometers were followed during twenty hours, by observations taken at intervals of a minute and a half, is highly creditable to Lieut. Younghusband and his detachment.

EDWARD SABINE, Lieut.-Col. R.A.

Woolwich, Dec. 10, 1841.

OBSERVATORY AT TORONTO, CANADA.

Abstract of Observations taken during a remarkable disturbance on the 25th and 26th September, 1841.

Declination-Magnetometer.

	Gött. Mean Time.	m s	m s	m s	m s	m s	m s	m s	m s	m s	m s	m s	m s
		0 0	5 0	10 0	15 0	20 0	25 0	30 0	35 0	40 0	45 0	50 0	55 0
25	10 A.M.	60′·3	′...	′...	′...	65′·0	65′·0	61′·9	57′·8	59′·2	59′·9	60′·5	50′·9
	11 ,,	50·8	31·2	32·6	34·0	35·9	37·7	43·3	42·3	40·6	39·2	45·9	44·0
	12 ,,	41·3	38·7	37·6	32·7	26·5	30·4	30·0	30·5	28·8	28·6	29·6	26·1
	1 P.M.	18·8	18·1	23·0	29·4	30·2	26·5	18·1	17·2	12·5	11·7	8·9	12·8
	2 ,,	21·5	25·7	23·5	22·5	22·8	21·5	18·9	22·5	22·7	22·8	25·9	24·8
	3 ,,	24·8	9·2	10·4	9·7	4·7	7·8	17·7	18·9	21·2	12·7	15·5	14·3
	4 ,,	17·6	19·2	19·2	18·6	17·1	16·1	14·0	12·3	16·2	17·0	17·9	16·8
	5 ,,	21·2	17·3	26·7	31·0	31·2	27·4	28·8	32·6	37·0	39·8	41·3	42·6
	6 ,,	39·2	38·8	39·9	39·8	40·3	40·8	34·0	39·2	38·4	40·8	30·6	32·3
	7 ,,	32·6	32·3	30·2	32·0	29·5	28·0	26·6	23·8	20·3	22·1	18·7	20·0
	8 ,,	17·1	18·2	15·0	16·1	17·9	18·1	20·9	21·5	22·1	21·8	22·7	23·5
	9 ,,	23·3	23·3	24·1	24·5	26·0	28·1	28·8	28·9	29·2	30·3	30·9	31·9
	10 ,,	31·9	30·8	28·7	27·2	29·2	28·7	26·4	27·5	28·3	31·8	31·5	29·8
	11 ,,	30·7	22·6	29·4	...	...	31·0	30·1	35·6	29·3	34·8	32·7	29·3
26	12 ,,	26·9	27·3	29·1	29·3	29·3	35·9	31·0	31·2	35·9	36·5	37·7	41·6
	1 A.M.	45·9	49·5	51·1	49·2	51·0	52·0	48·9	32·8	36·0	37·1	40·5	46·3
	2 ,,	52·1	55·9	47·1	39·8	40·8	47·7	51·7	50·8	49·4	48·6	46·2	45·1
	3 ,,	45·4	46·5	45·2	45·6	45·7	45·1	49·2	58·1	38·5	35·9	52·3	31·0
	4 ,,	11·4	0·00	4·0	9·6	13·7	49·4	62·8	46·8	45·4	29·4	34·8	53·5
	5 ,,	62·3	56·0	55·5	53·7	50·8	49·5	44·9	43·4	43·8	46·0	47·7	47·9

Increasing numbers denote an increase of easterly declination; the lowest reading (at 4^h 5^m A.M. 26th) has been taken as the zero.

OBSERVATORY AT TORONTO, CANADA.

Abstract of Observations taken during a remarkable disturbance on the 25th and 26th September, 1841.

Horizontal-Force Magnetometer.

	Gött. Mean Time.	m s 2 0	m s 7 0	m s 12 0	m s 17 0	m s 22 0	m s 27 0	m s 32 0	m s 37 0	m s 42 0	m s 47 0	m s 52 0	m s 57 0	H.F. Ther.
25	10 A.M.	·00972	......	......	......	·00906	·00953	·00869	·00777	·00780	·00739	·00661	·00805	66°·3
	11 „	·00812	·00917	·00850	·00827	·00896	·01050	·00923	·00812	·00789	·00506	·00020	·00286	66·5
	12 „	·00345	·00266	0·00	·00138	·00170	·00440	·00510	·00456	·00607	·00668	·00570	·00528	66·2
	1 P.M.	·00558	·00547	·00428	·00332	·00355	·00547	·00609	·00671	·00833	·00759	·00461	·00320	66·0
	2 „	·00301	·00409	·00642	·00629	·00493	·00364	·00362	·00397	·00471	·00461	·00514	·00579	65·8
	3 „	·00447	·00367	·00336	·00276	·00219	·00317	·00375	·00358	·00323	·00346	·00223	·00350	65·8
	4 „	·00371	·00438	·00565	·00400	·00428	·00400	·00482	·00604	·00809	·00811	·00738	·00885	65·6
	5 „	·00871	·00951	·01026	·01074	·00913	·00975	·01048	·01117	·01142	·01185	·01210	·01216	65·8
	6 „	·01295	·01352	·01325	·01173	·01126	·01071	·01202	·01394	·01480	·01419	·01282	·01362	66·0
	7 „	·01402	·01449	·01412	·01474	·01494	·01497	·01531	·01434	·01428	·01689	·01611	·01598	66·2
	8 „	·01419	·01618	·01588	·01537	·01571	·01591	·01475	·01464	·01475	·01452	·01496	·01553	66·3
	9 „	·01517	·01424	·01449	·01453	·01385	·01423	·01479	·01491	·01479	·01458	·01410	·01384	67·5
	10 „	·01392	·01441	·01474	·01598	·01640	·01676	·01733	·01790	·01702	·01797	·01728	·01740	67·4
	11 „	·02014	·02053	·01856	......	......	·01767	·01709	·01660	·01659	·01870	·01914	·01936	68·1
26	12 „	·02001	·02218	·02437	·02372	·02131	·02154	·02316	·02099	·01973	·01859	·01833	·01797	67·3
	1 A.M.	·01640	·01672	·01667	·01735	·01618	·01686	·01825	·01752	·01807	·02337	·01948	·01654	67·
	2 „	......	·01119	·01074	·01191	·01292	·01263	·01117	·01191	·01423	·01374	·01236	·01221	66·
	3 „	·01064	·00940	·00848	·00966	·01058	·01060	·01114	·00985	·00920	·01078	·01110	·00992	66·
	4 „	·00885	·00981	·00680	·00643	·00850	·01067	·01325	·01173	·01372	·01144	·00804	·00799	66·
	5 „	......	·01197	·01316	·01292	·01342	·01347	·01346	·01279	·01243	·01247	·01328	·01366	66·

Changes of Total Intensity.

26	12 P.M.	·01236	·01237	·01361	·01257	·01185	·01181	·01213	·01169	·01144	·01149	·01151	·01119
	1 A.M.	·01113	·01083	·01082	·01052	·00953	·00960	·00903	·00953	·00933	·01026	·00961	·00786
	2 „	......	·00653	·00691	·00812	·00875	·00814	·00770	·00779	·00927	·00857	·00801	·00770
	3 „	·00709	·00633	·00646	·00669	·00724	·00698	·00647	·00516	·00606	·00508	·00191	·00206
	4 „	·00255	·00316	·00264	·00271	·00501	·00467	·00346	·00336	·00335	·00409	·00394	·00411
	5 „	......	·00505	·00552	·00569	·00606	·00597	·00614	·00607	·00630	·00633	·00652	......

The changes of horizontal and vertical force, and of the total intensity, are expressed in terms of the whole forces, and are uncorrected for variations of temperature, the precise corrections for which have not been yet determined, but the extreme corrections on this account would probably fall short of ·0003 in the horizontal, and ·0005 in the vertical force.

Increasing numbers denote an increase of force.

OBSERVATORY AT TORONTO, CANADA.

Abstract of Observations taken during a remarkable disturbance on the 25th and 26th September, 1841.

Vertical-Force Magnetometer.

	Gött. Mean Time.	m s 3 30	m s 8 30	m s 13 30	m s 18 30	m s 23 30	m s 28 30	m s 33 30	m s 38 30	m s 43 30	m s 48 30	m s 53 30	m s 58 30	V.F. Ther.
25	10 A.M.	·00234	……	……	……	·00178	·00191	·00177	·00177	·00184	·00173	·00146	·00267	66°·3
	11 „	·00330	·00277	·00263	·00263	·00286	·00291	·00260	·00216	·00216	·00103	·00142	·00182	67·5
	12 „	·00086	·00044	0·00	·00157	·00191	·00168	·00213	·00217	·00188	·00184	·00192	·00237	67·0
	1 P.M.	·00199	·00200	·00224	·00276	·00336	·00379	·00420	·00416	·00446	·00438	·00331	·00325	66·5
	2 „	·00394	·00418	·00434	·00431	·00487	·00484	·00465	·00498	·00537	·00514	·00571	·00559	65·8
	3 „	·00549	·00517	·00538	·00496	·00491	·00527	·00523	·00550	·00582	·00623	·00607	·00632	66·5
	4 „	·00637	·00666	·00693	·00692	·00712	·00691	·00719	·00752	·00776	·00769	·00755	·00769	65·7
	5 „	·00799	·00772	·00795	·00786	·00767	·00766	·00773	·00786	·00779	·00803	·00802	·00780	65·5
	6 „	·00821	·00823	·00812	·00806	·00819	·00821	·00850	·00851	·00857	·00846	·00846	·00875	65·7
	7 „	·00883	·00882	·00887	·00893	·00894	·00900	·00900	·00896	·00899	·00900	·00888	·00872	65·7
	8 „	·00862	·00857	·00857	·00842	·00835	·00826	·00799	·00791	·00780	·00768	·00754	·00763	66·0
	9 „	·00758	·00755	·00755	·00763	·00773	·00787	·00804	·00814	·00814	·00806	·00793	·00787	65·0
	10 „	·00792	·00805	·00850	·00897	·00905	·00914	·00943	·00963	·00969	·00973	·00977	·01004	66·0
	11 „	·01004	·01054	……	……	……	·01002	·01046	·00997	·01081	·01088	·01147	·01127	66·5
26	12 „	·01183	·01169	·01287	·01180	·01120	·01114	·01137	·01104	·01087	·01100	·01104	·01073	66·8
	1 A.M.	·01066	·01042	·01042	·01005	·00907	·00910	·00840	·00898	·00872	·00935	·00893	·00726	67·0
	2 „	·00622	·00621	·00664	·00786	·00846	·00783	·00746	·00750	·00893	·00821	·00771	·00739	67·0
	3 „	·00685	·00612	·00632	·00649	·00701	·00673	·00615	·00483	·00584	·00469	·00127	·00152	66·8
	4 „	·00211	·00270	·00235	·00245	·00477	·00425	·00278	·00278	·00263	·00358	·00366	·00384	67·5
	5 „	·00384	·00457	·00499	·00519	·00555	·00545	·00563	·00561	·00588	·00590	·00605	……	67·0

Changes of Inclination.

26	12 P.M.	6′·92	8′·87	9′·72	10′·08	8′·55	8′·79	9′·97	8′·41	7′·49	6′·42	6′·16	6′·12
	1 A.M.	4·85	5·33	5·28	6·17	6·01	6·56	8·33	7·22	7·91	11·85	8·92	7·85
	2 „	……	4·21	3·47	3·42	3·77	4·06	3·14	3·73	4·48	4·67	3·93	4·08
	3 „	3·20	2·77	1·83	2·68	3·02	3·27	4·22	4·24	2·84	5·15	8·31	7·10
	4 „	5·70	449	3·76	3·37	3·15	5·43	8·85	7·57	9·38	6·65	3·70	3·51
	5 „	……	6·26	6·91	6·54	6·65	6·11	5·56	5·54	6·07	6·62	6·78	……

Increasing numbers denote an increase of the vertical force, and a decrease of the inclination.

OBSERVATORY AT TORONTO, CANADA.

Abstract of Observations taken during a remarkable disturbance on the 25th and 26th September, 1841.

Gött. M. T.	Barometer Corrected.		Therm.		Wind.		Remarks.
Hour.	Height.	Th.	Dry.	Wet.	Direction.	Force.	
10 A.M.	29·289	66°·0	57°·8	56°·2	....	Calm	8·30 A.M. Rain slackened. Very densely clouded.
11 „	29·291	66·0	57·2	55·9	....	Calm	Densely clouded. Drizzling rain.
12 „	29·301	66·0	57·0	55·6	....	Calm	Densely clouded. Cirro-strati and haze.
1 P.M.	29·320	66·5	57·1	55·2	N.	Light	Densely clouded. Cirro-strati and haze.
2 „	29·331	66·0	57·4	55·6	North^{y}	Almost Calm	Densely clouded. Cirro-strati, dense haze, raining very slightly.
3 „	29·319	66·0	58·2	56·2	N.N.W.	Light	Densely clouded. Cirro-strati and dense haze.
4 „	29·319	66·0	59·6	56·4	N.	Nearly Calm	Densely clouded. Cir.-cum. and cir.-strati.
5 „	29·309	65·5	61·0	57·2	N.	Light	Clouded cirro-strati and cumulous haze.
6 „	29·309	66·0	61·5	57·1	N. by W.	Light	Densely clouded. Cir-cum. and cum.-strati.
7 „	29·313	66·0	62·7	57·9	N.W.	Mod^{e}	Densely clouded. Cir.-strati and cum.-strati.
8 „	29·323	66·0	62·0	56·5	N.W.	Mod^{e}	Densely clouded. Cir.-strati, a few breaks to the S. and S.W.
9 „	29·321	66·0	63·0	56·8	N.W.	Light	$\frac{5}{8}$ Clouded, cum.-strati and cir.-cum. Zenith clear.
10 „	29·308	67·0	65·0	56·8	N.W.	Light	$\frac{6}{8}$ Clouded. Light masses of cir.-cum.
11 „	29·306	67·0	65·6	56·4	N.W.	Light	$\frac{3}{8}$ Clouded. Lightmasses of cir.-cum., chiefly round horizon.
12 „	29·328	67·0	60·0	54·1	....	Calm	Fair. A few light cirri dispersed about.
1 A.M.	29·326	67·0	56·6	52·4	....	Calm	$\frac{1}{8}$ to the Sd. clouded with strati, remainder clear.
2 „	29·342	67·0	57·8	53·7	N.W.	Mod.	1h 10m. Faint auroral light to the northward. $\frac{7}{8}$ densely clouded.
3 „	29·332	66·5	57·2	53·7	N.W.	Light	$\frac{6}{8}$ overcast, cirri and cir.-strati. Zenith partially clear.
4 „	29·327	66·0	56·6	53·2	West^{ly}	Very light	Overcast. Strati and cir.-strati.
5 „	29·329	66·0	56·3	53·1	West^{ly}	Nearly calm	Densely clouded. Strati.
6 „	29·305	66·0	56·0	52·6	West^{ly}	Nearly calm	Densely clouded. Strati.

Additional Remarks.

Tor. M. T. 25th. Gott. M. T. 26th.

7^{h} 15^{m} P.M. 1^{h} 12^{m} A.M. Sky almost entirely clear. Bright bank of auroral light in the N., moving across the sky as light cirri.

7^{h} 20^{m} P.M. 1^{h} 17^{m} A.M. Several bright streamers and patches of light appearing and disappearing rapidly.

7^{h} 35^{m} P.M. 1^{h} 32^{m} A.M. Light brighter. Clouds rising rapidly.

7^{h} 38^{m} P.M. 1^{h} 35^{m} A.M. Sky overcast.

The sky continuing overcast, nothing further was seen of the aurora until 9^{h} 30^{m} P.M. (3^{h} 27^{m} A.M. Gött.), when the clouds being bright in the N.E. several splendid streamers, and bright pulsations were observed in that quarter; they remained visible until 9^{h} 55^{m}, becoming gradually more faint.

9^{h} 55^{m} P.M. 3^{h} 52^{m} A.M. Partially clouded round N. horizon, low bank of light in N. and N.E.

10^{h} 40^{m} P.M. 4^{h} 37^{m} A.M. Clearing slightly in N. Light larger and brighter.

11^{h} 40^{m} P.M. 5^{h} 37^{m} A.M. Densely clouded; nothing of aurora visible.

Midnight. 5^{h} 57^{m} A.M. Densely clouded; nothing of aurora visible.

Remarks relative to the Magnetic Disturbance at Toronto on the 25th and 26th of September, 1841.

The disturbances appear to have commenced at Toronto nearly at the same absolute time as at Greenwich, and to have been generally simultaneous at both stations. Additional observations were taken at Greenwich early in the morning of the 25th, the needles being in an agitated state, and an aurora being visible. Though additional observations were not commenced at Toronto at this time, the difference between the regular observations at 2 and 4 A.M., Gött. mean time, show a considerable disturbance; the change of declination between the hours of 2 and 4 amounting to 15′·3; the change of horizontal force to ·00444 of its whole value; and of vertical force to ·00202 of its whole value. The additional observations at Greenwich were soon after discontinued, the disturbance appearing to be over.

The readings at 10 A.M., Gött. mean time, exhibited so great a change at both stations, that extra observations were resumed at Greenwich, and commenced at Toronto.

The additional observations at Toronto were continued without intermission for 20 hours, from 10 A.M., Gött. mean time on the 25th, until 6 A.M., Gött. mean time on the 26th, each instrument being observed at intervals of 5 minutes, in the following order, viz.: Declin. $00^{m}\ 0^{s}$—H. F. $2^{m}\ 00^{s}$—V. F. $3^{m}\ 30^{s}$—Declin. $5^{m}\ 00^{s}$, &c.

The disturbance in the first hour (10 to 11) was not very great, compared with those that followed; but in the next 5 minutes, viz. from $11^{h}\ 00^{m}$ to $11^{h}\ 05^{m}$, the change of declination amounted to 19′·6; and between $11^{h}\ 27^{m}$ and $11^{h}\ 52^{m}$ the horizontal force had decreased by ·01030 of its whole value. These changes are specially noted, in consequence of there apparently having been no corresponding disturbance at Greenwich.

The disturbance at Toronto was at its height from 11 A.M. to 4 P.M., Gött. mean time: its general effect appears to have been that of causing a decrease of easterly variation and of *total* intensity.

The agreement in direction of the changes of horizontal and vertical force deserves remark: the minimum and maximum of each occurred simultaneously, or as nearly so as can be learned from the observations, the minimum of both being observed at the second reading after noon, and the maximum of both at precisely the same interval after the following midnight.

The observations at Toronto, being continued until midnight of Saturday at that station, which was 6 A.M., Gött. mean time of the 26th, lasted about five hours longer than those at Greenwich; and during these five hours a second disturbance was observed even greater than the preceding.

Between 4 and 10 P.M., Gött. mean time, the disturbance had been much lessened; it then increased rapidly, and the changes between 12 P.M. and 6 A.M. were very remarkable: in 15 minutes the declination magnetometer moved through an angle of 52′·3, and in 25 minutes more had returned through an angle of 62′·8 in the opposite direction. The horizontal and vertical-force magnetometers were also greatly disturbed, the change in the horizontal force amounting in 20 minutes to ·0122 of the whole force.

The changes of horizontal and vertical force in the last six hours have been reduced to the equivalent changes of inclination and total intensity, and are printed, for the convenience of comparison at these observatories, where the variations of the inclination and total intensity are directly observed. As the two magnetometers were not observed precisely simultaneously, the changes of inclination and total intensity can only be regarded as approximate, though the difference in the times of observation having been only 1½ minute, the errors occasioned thereby are probably seldom very great.

The observations of the second disturbance show a striking connexion between the changes of declination and intensity, an increase of force corresponding to an increase of easterly declination, and *vice versâ*: the same connexion was observed in the Toronto observations of the 29th of May, 1840.

Auroras.—An aurora was visible at Greenwich both in the morning and evening of the 25th. At Toronto the morning was heavily clouded, with rain, consequently no aurora could be seen: in the evening the aurora was visible at intervals from 7 to 10 P.M., Toronto time, or 1 to 5 A.M., Gött. time, the period of the second great disturbance; the remainder of the night was heavily clouded. A gale of wind occurred on the following day (26th), and in the evening another aurora was seen.

The *extreme* changes of the declination, horizontal, and vertical force, during the two disturbances, were as follows:—of the declination, 1° 05′; of the horizontal force, ·02438 of its whole value; and of the vertical force, ·01288 of its whole value. The days of occurrence, and the extreme ranges of the principal disturbances observed at Toronto in 1840 are subjoined, in order that the relative extent of the present disturbance may be estimated.

DATES.	EXTREME RANGES.		
1840.	Declination.	Horizontal Force.	Vertical Force.
	° ′		
May 29	1 59	Off scale, exceeding ·044	Off scale, exceeding ·024
August 28	1 44	0·03521	0·00846
September 22..	1 01	Not observed, being out of adjustment.	
„ 25..	0 30	0·00184	0·00112
December 21..	1 22	0·01522	0·01074
1841.			
Sept. 25 and 26	1 05	0·02438	0·01288

An aurora was observed on each of the above days in 1840; that of the 29th of May was the most brilliant of any seen since the establishment of the Observatory. Very few additional observations were taken either on the 22nd or 25th of September, 1840; on the last day especially, the few that were taken were not commenced until the greater part of the disturbance was apparently over; consequently the actual changes were, in all probability, much greater than those observed; the recurrence of so great a disturbance on the same day in the following year is remarkable. Additional observations have occasionally been taken in the course of every month in 1841, in consequence of unusual disturbances, but the changes have never equalled those above-mentioned.

The months of September and October appear to be those of greatest disturbance.

Gauss's arrangement of the scale and mirror has been adopted for the horizontal-force magnetometer at Toronto, in consequence of the disturbance of the 29th of May, 1840, having driven the magnet beyond the range of the collimator scale. The range of the declination scale (on Professor Lloyd's plan) being about 6°,—or three times greater than the extreme range hitherto observed,—there is little probability of its ever being exceeded.

POSTSCRIPT, Dec. 14th.—Whilst these pages were in the press, I received, through the kindness of Mr. Caldecott, the abstract of the observations made in September at the Magnetic Observatory instituted by the Rajah of Travancore, at Trevandrum, in lat. 8° 30′ 35·2″ N., and long. $5^h\ 7^m\ 59^s$ E.; by which I am enabled to state, that an unusual magnetic disturbance took place in India simultaneously with those observed in England and America: although, in consequence of the easterly position of Trevandrum, the commencement only of the disturbance was observed there. Each observatory discontinued its observations when its own Saturday night arrived; thus the observations at Greenwich continued five hours later than at Trevandrum, and at Toronto five hours later than at Greenwich; the latest observations, in each case, showing the continuance of the disturbance.

The observations at Trevandrum consist of the regular two-hourly readings of the three magnetometers, day and night. By comparing the positions of the magnetometers at each of the magnetic hours of the 25th of September, with the mean position at the same hour in the previous twenty-four days, we obtain what we may consider a measure of the magnetic disturbance of that day. As the disturbance was indicated principally by the horizontal-force magnetometer, we may commence with the comparison of that instrument, premising that as the inclination at Trevandrum is only — 2° 50′, the horizontal intensity at that station comprises nearly the whole magnetic intensity, the vertical component being extremely small.

Horizontal-Force Magnetometer, Trevandrum, September 25, 1841.

Mean Time.		Mean Position. 1st to 24th Sept.	Sept. 25.	Difference and Remarks.	
Trev.	Gött.				
h m	h	Scale Div.	Scale Div.		
0 28 A.M.	8 P.M.	120·3	131·4	11·1	Scale-divisions in excess on the 25th of September, corresponding to diminished intensity; one scale-division = ·000133 nearly of the whole horizontal force at Trevandrum. The approximate *absolute* horizontal force, resulting from two experiments in the month of September, expressed in reference to the units of the British system, is 7·77. (Instructions of the Royal Society, p. 21.)
2 28	10	118·7	126·8	8·1	
4 28	Mid.	117·9	125·4	7·5	
6 28	2 A.M.	116·3	127·3	11·0	
8 28	4	111·0	124·2	13·2	
10 28	6	103·0	118·5	15·5	
0 28 P.M.	8	113·8	122·6	8·8	
2 28	10	125·1	140·2	15·1	
4 28	Noon.	128·0	160·1	32·1	
6 28	2 P.M.	124·7	167·1	42·4	
8 28	4	124·0	157·9	33·9	
10 28	6	122·3	171·6	49·3	

The positions in the Table are uncorrected for the variations of temperature of the bar, but the corrections on that account may be safely neglected for the present purpose: the thermometer of the horizontal-force magnetometer on the mean of the 12 magnetic hours of the 25th, was one degree of Fahrenheit *less* than the average of the month.

During the 24th of September the horizontal intensity differed little from its mean position, at the same hour, since the commencement of the month, until $10^h\ 28^m$ P.M., Trevandrum time, being the last observation of the day, when it was weaker than the average at that hour by an amount equal to about 7 scale-divisions. During the whole day of the 25th it was weaker than the average of the preceding twenty-four days of the month, by the quantities shown in the above table; and at the Göttingen hours of noon, 2, 4, and 6 P.M., when great disturbances were taking place at Greenwich and Toronto, the observations at Trevandrum show a decrease of intensity much exceeding the usual fluctuations. Mr. Caldecott has annexed the following remark to the readings at these hours:—"These irregular readings "examined into at the hours they were made, and found not to arise from "any instrumental irregularity.—J. C."

On the 26th, being Sunday, no observations were made; but the following table, exhibiting the *mean position* of the magnetometer *in each day* in September, shows that during the remaining days of the month the horizontal intensity did not return to its previous average value; corresponding with the remark deduced by Professor Kreil from ten perturbations observed at Prague, namely, that "the horizontal intensity remains weaker for some time after the "great oscillations have ceased, and only gradually resumes its ordinary "force*."

Mean Position of the Horizontal-Force Magnetometer in each day in September.

	Scale Div.		Scale Div.
1	123·8	18	124·0
2	118·3	20	123·7
3	113·5	21	124·6
4	114·1	22	123·5
6	110·9	23	119·8
7	110·4	24	117·4
8	116·9	25	139·4
9	114·5	27	133·0
10	117·0	28	129·9
11	117·3	29	129·5
13	122·7	30	130·0
14	127·5		
15	122·1	Mean position during the month	121·8
16	118·4		
17	125·1		

Declination Magnetometer.—The effect of the disturbance on the declination magnetometer at Trevandrum appears to have been comparatively small. The north end of the magnet was, however, during the whole day to the eastward of its average position at the same hours in the preceding part of the month, as is shown in the subjoined table. The second part of the table exemplifies the small amount of the fluctuations which take place from day to day in the mean position of this magnetometer at Trevandrum. The mean position *for the month*, corrected for torsion of the thread, was 253·48 scale-divisions, or the mean declination for the month = 0° 43′ 45·7″ East. A scale-division in this instrument = 39·85″ nearly.

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* Letter to Lieut.-Col. Sabine, translated in Phil. Mag., Third Series, vol. xvii. p. 429.

Declination Magnetometer, Trevandrum, September 1841.

Mean Time.		Mean Position 1st to 24th Sept.	Sept. 25.	Diffs.	Mean Position in each day.			
Trev.	Gött.				Day.	Scale Div.	Day.	Scale Div.
h m	h	Scale Div.	Scale Div.					
0 28 A.M.	8 P.M.	253·5	254·2	0·7	1	253·4	16	253·3
2 28	10	253·6	255·3	1·7	2	253·6	17	252·7
4 28	Mid.	253·6	256·4	2·8	3	253·9	18	253·1
6 28	2 A.M.	255·4	257·1	1·7	4	253·1	20	253·9
8 28	4	254·3	256·0	1·7	6	253·3	21	253·0
10 28	6	251·9	253·6	1·7	7	253·3	22	253·3
0 28 P.M.	8	251·3	252·6	1·3	8	253·1	23	253·6
2 28	10	252·8	253·1	0·3	9	252·9	24	253·0
4 28	Noon.	253·3	253·4	0·1	10	253·3	25	254·7
6 28	2 P.M.	253·1	254·5	1·4	11	253·5	27	253·7
8 28	4	253·0	255·5	2·5	13	252·8	28	253·6
10 28	6	253·2	255·0	1·8	14	253·6	29	253·6
					15	253·6	30	253·6

Mr. Caldecott having arrived at Trevandrum in May 1841, and established his magnetic instruments in a temporary building until they were removed into the permanent observatory early in October, had not at that time determined the value of the readings of the Vertical-Force Magnetometer. For this reason a complete account of the disturbance of that component of the force cannot be given; and considering the very small amount of the whole vertical force at Trevandrum, it may perhaps be sufficient to state, that the readings of the instrument indicate an unusual difference in the force at the Göttingen hours of noon, 2, 4, and 6 P.M., from its value at the other magnetic hours of the same day.

SECOND POSTSCRIPT, Dec. 20th.—Whilst the Plate (I.) accompanying this notice was still in the engraver's hands, the arrival of the abstracts for the month of September from the magnetic observatory at St. Helena has furnished the means of adding to the present account the observations of this remarkable disturbance made at that station.

An unusual movement of the magnetometers appears to have been noticed by Lieut. Lefroy at an earlier period than at any observatory from which accounts have yet been received; extra observations having been commenced at St. Helena between 11 and 12 A.M., Gött. mean time, on Friday the 24th. At 2 P.M., the disturbance appearing to have subsided, they were discontinued, but were resumed at 8 P.M., and continued thenceforward without intermission for twenty-six hours, until midnight of Saturday the 25th. During these twenty-six hours of consecutive observation, the declination-magnetometer was observed at intervals of 5 minutes, and the horizontal and vertical-force instruments each at intervals of 10 minutes. The observations are given in the subjoined tables.

The element principally affected at St. Helena was the horizontal force, which underwent frequent fluctuations of unusual amount, and sustained, on the whole, a considerable diminution of intensity. Between 2 P.M. and 8 P.M. of the 24th, the loss of force amounted to about ·0048 of its whole value:

this weakened state remained, with little change, until 2 A.M. on the 25th, when the force again augmented, and had regained about half the original loss at 10 A.M., the period when the disturbances at Greenwich and Toronto occasioned extra observations to be commenced at those observatories. At this epoch another great diminution of the horizontal force took place at St. Helena, and the intensity continued to weaken until $7^h\ 42^m\ 30^s$ P.M., when the loss since 10 A.M. amounted to ·0118, and since noon of the preceding day, to ·015 of the whole force. The magnetic inclination at St. Helena being — 21°, the horizontal force forms by much the larger component of the total magnetic force: the approximate *absolute* horizontal value in English units may be taken at 5·9.

In the account of the Toronto observations it has been noticed that the greatest disturbance of the instruments took place after the observations at Greenwich had been discontinued; namely, after the midnight of Saturday at Greenwich. St. Helena being nearly in the same meridian as Greenwich, the observations ceased at nearly the same time and for the same reason: St. Helena does not therefore afford any observations corresponding to those of the second disturbance at Toronto.

On Sunday 26th, the instruments were observed at 9 and 11 A.M., 3 and 8 P.M.; and on Monday 27th, extra observations were commenced at $1^h\ 30^m$ A.M., at intervals of 30^m, and continued until 11 A.M., when the intervals were changed to 5^m for the declination-magnetometer, and to 10^m for each of the force-magnetometers, the extra observations being finally discontinued at midnight of that day.

A table is subjoined of the mean positions of the magnetometers at the several magnetic hours during the month of September, showing the mean diurnal curve for the month of each element; also a table of the mean positions of the instruments on each day of the month, showing the monthly curve. It is seen by the latter table that the horizontal force was considerably below its average intensity on the 25th, and that it did not wholly recover the loss before the end of the month.

The fluctuations of the declination at St. Helena, as at Trevandrum, were far less striking or remarkable than those of the horizontal intensity, or than those of the declination at Toronto and Greenwich. The magnetic disturbances at the tropical stations have not however always this character. During a disturbance observed at St. Helena on the 26th of June, 1840, the extreme range of the declination-magnetometer amounted to 1° 20′; and during several of the most extensive movements of the declination bar, Lieut. Lefroy states that the horizontal-force bar remained perfectly at rest. The following passages are extracted from Lieut. Lefroy's report of the disturbance here referred to (26th June, 1840), on account of their striking resemblance to some of the remarks in Mr. Airy's account of the recent disturbance at Greenwich:—

"On taking the reading for noon, my attention was called to the disturbed
" state of the magnet; I found it making rapid and irregular movements, with
" sudden jerks and momentary pauses, too rapid to allow readings......Two suc-
" cessive movements frequently occurred in the *same direction*; and another
" unusual circumstance was, that the magnet, after a violent movement, came
" *instantly* to rest; it is for this reason that but two readings instead of three
" are sometimes given."

With a view to a more complete examination of these singular phenomena, it is deserving of consideration, whether some slight modification might not be introduced in the mode of observing on such extraordinary occasions. For example, peculiarities of movement might be more advantageously studied and described by an observer, whose attention was not distracted by the necessity of recording the exact position of the bar at stated intervals of quick recurrence; and possibly a very light needle, read by a mirror, or by reflection from its own highly-polished side, and suited to follow more instantaneously than the declination bar the effects of rapidly-succeeding impulses, might be found useful in exhibiting the effect of each impulse more distinctly. An observer so circumstanced would note the time of those movements only which should appear to him deserving of special notice. Such observations might be made in addition to the readings of the magnetometers, which might continue to be made at intervals as short as the strength of each observatory will permit; and, where it can conveniently be done, the readings of the horizontal and vertical-force magnetometers should be simultaneous.

In a letter just received from Toronto, dated November 19, Lieut. Younghusband states, that though the curve of the 25th of September shows indeed extraordinary fluctuations, they are not to be compared to those observed on the night of the 18th November (1841), when the declination magnetometer ranged through above 86′, and the horizontal-force magnetometer went beyond the scale in Gauss's method of observation; a light held in the prolongation of the scale, about one foot from its extremity (equal to the length of about 200 divisions), was reflected from the mirror into the field of the telescope: whence the force must have been *diminished below the average more than $\frac{1}{20}$th of its whole value*: the greatest rapid change of force was equivalent to 0·03 of the total value, which was shown by a progressive movement of the magnet during five minutes. A very brilliant aurora accompanied this disturbance.

Third Postscript, Dec. 29th.—Whilst finally revising the last page of this notice, the September returns from the magnetic observatory at the Cape of Good Hope have arrived. Although the reduction of these observations, for the purpose of subjoining them, would occasion an inexpedient delay in this publication, it is satisfactory to be able to state, that the remarkable disturbance under consideration manifested itself in that southern latitude; that it arrested the attention of Lieut. Eardley Wilmot at an early hour of the 24th; and that it was followed by that officer and his detachment with the utmost promptitude and assiduity. Extra observations, at the same intervals as on term days, were commenced at $6^h\ 12^m\ 30^s$ A.M. on the 24th, and continued to 10 A.M.; resumed at $2^h\ 35^m$, and continued to 4 P.M.; resumed again at $6^h\ 5^m$ P.M., and continued without intermission for the succeeding thirty hours. The epochs here spoken of are Göttingen mean time. All the magnetometers were greatly affected. The greatest disturbance of the horizontal force commenced about 10 A.M. on the 25th, and attained its extreme limit at $7^h\ 45^m$ P.M. on the same day. The vertical-force magnetometer was deflected out of the field of view at $6^h\ 30^m$ P.M. on the 24th, and remained so: the instrument being adjusted afresh to the needle, the latter was again deflected out of the field at $2^h\ 27^m\ 30^s$ P.M. on the 25th.

The observations at the Cape may form a supplement to this notice, accompanied by observations of the same disturbance, expected to arrive by the next overland mail, from the magnetic observatory at Simla in the Himalaya, where, presuming it to have occurred, it is not likely to have escaped the indefatigable vigilance of Captain Boileau, of the Bengal Engineers, director of that observatory. The September returns from the Van Diemen's Land Observatory, conducted by Lieut. Kay, R.N., may be expected in February; about which time we may also hope to receive accounts of the same date from Captain Ross, R.N., who intended to pass the last fortnight of September at the Chatham Islands, where he would establish his magnetometers on shore.

OBSERVATORY AT ST. HELENA, SEPTEMBER 24TH and 25TH, 1841.

Declination-Magnetometer.

	Gött. Mean Time.	m s 0 0	m s 5 0	m s 10 0	m s 15 0	m s 20 0	m s 25 0	m s 30 0	m s 35 0	m s 40 0	m s 45 0	m s 50 0	m s 55 0
24	11 A.M.	′ …	′ …	′ …	′ …	′ …	′ …	8′·39	′ …	′ …	8′·46	8′·53	8′·46
	12 „	8·46	8·53	8·53	8·96	9·10	9·17	9·24	9·24	9·74	9·74	9·81	9·81
	1 P.M.	9·81	9·81	9·74	9·39	9·32	9·53	9·46	9·24	9·17	9·24	9·10	9·10
	8 P.M.	3·63	3·70	3·70	3·77	3·77	3·56	3·48	3·48	3·34	3·41	3·56	3·48
	9 „	3·20	3·13	3·13	3·13	2·99	2·92	2·84	3·13	3·06	2·84	2·84	2·84
	10 „	2·84	2·84	2·84	2·84	2·84	2·78	2·84	2·84	2·84	3·13	3·06	2·84
	11 „	2·74	2·70	1·78	1·86	1·71	1·53	1·49	2·06	2·13	2·13	2·13	2·13
25	0 A.M.	2·56	2·91	3·03	2·91	2·84	2·75	2·70	2·56	2·06	1·92	1·92	1·99
	1 „	1·81	1·85	1·99	2·35	2·56	2·70	2·70	2·77	2·91	3·06	2·99	2·84
	2 „	2·78	2·84	2·84	2·99	3·20	2·64	2·82	3·13	3·34	3·41	3·41	3·48
	3 „	3·55	3·69	4·05	3·98	3·98	4·09	4·12	4·12	4·12	4·19	4·27	4·34
	4 „	4·34	4·90	5·19	5·26	…	5·69	5·69	5·76	5·97	6·18	6·33	6·47
	5 „	6·90	6·97	6·54	6·40	6·40	7·04	7·11	7·11	7·11	7·18	7·11	7·11
	6 „	7·18	7·11	7·04	7·04	7·11	7·39	7·39	7·53	7·61	7·75	7·75	7·82
	7 „	7·82	7·82	7·82	7·82	7·75	7·68	7·54	7·18	7·04	7·04	7·11	7·11
	8 „	7·11	7·04	6·86	6·79	6·65	6·61	6·55	6·55	6·54	6·66	6·86	6·97
	9 „	7·11	7·11	7·39	7·04	7·05	7·04	7·04	7·04	6·97	7·07	7·00	7·00
	10 „	6·90	6·90	6·43	6·71	6·97	6·83	6·40	6·12	5·76	5·76	5·76	5·69
	11 „	5·69	5·76	5·55	5·55	5·55	5·55	5·55	5·34	5·20	5·20	5·41	4·95
	12 „	4·62	4·23	4·41	4·41	4·27	4·27	4·77	4·84	4·48	4·13	3·49	3·60
	1 P.M.	3·77	3·39	3·34	3·41	3·48	3·55	3·41	3·41	3·85	3·99	3·94	4·27
	2 „	4·70	4·34	4·34	4·18	3·38	3·34	3·45	2·91	2·77	2·39	2·55	2·67
	3 „	2·84	2·84	2·84	2·77	2·42	2·67	3·36	4·03	4·55	3·82	4·12	3·55
	4 „	3·13	2·52	2·56	2·49	2·77	2·13	2·11	1·53	1·28	0·68	0·64	0·57
	5 „	0·37	0·30	0·01	0·07	0·0	0·53	0·57	0·58	0·64	1·21	1·33	1·21
	6 „	1·24	1·21	1·01	1·28	1·88	2·06	2·06	1·79	1·99	2·13	2·27	2·53
	7 „	2·84	2·91	3·05	3·05	3·05	3·33	3·19	3·40	2·91	3·05	2·91	3·05
	8 „	3·19	3·47	3·26	2·77	2·70	2·42	2·42	2·13	2·06	2·06	1·85	1·50
	9 „	1·42	1·42	1·42	1·42	2·06	2·06	2·10	2·70	2·77	2·91	3·05	2·91
	10 „	2·77	2·63	2·41	2·56	2·57	2·63	2·63	2·63	2·56	2·49	2·35	2·20
	11 „	2·27	2·34	2·20	2·13	2·13	1·92	1·64	1·50	1·50	1·42	1·42	1·49
	12 „	1·70	1·91	1·84	1·84	1·77	1·49	1·42	1·42	1·42	1·42	1·42	1·42

Increasing numbers denote a decrease of westerly declination.

Declination at 5h 20m P.M.; 23° 06′·42 W.

Gött. Mean Time.	Horizontal Force.						Vertical Force.					
	m s 2 30	m s 12 30	m s 22 30	m s 32 30	m s 42 30	m s 52 30	m s 7 30	m s 17 30	m s 27 30	m s 37 30	m s 47 30	m s 57 30
11 A.M.	...	...	...	171·6	...	172·0	...	...	...	...	7·2	6·3
Noon.	172·0	172·1	173·0	173·0	173·0	173·0	6·0	5·8	5·7	5·6	5·5	5·4
1 P.M.	173·0	172·8	172·85	172·6	172·1	171·1	5·1	5·0	4·7	4·9	4·6	4·0
8 P.M.	143·95	143·9	144·0	144·9	145·9	144·2	5·9	5·9	5·9	5·9	6·3	...
9 „	144·9	145·0	145·4	144·8	144·5	143·1	7·9	8·2	8·1	8·5	8·4	8·7
10 „	142·0	141·5	142·5	143·9	143·9	143·9	8·9	9·0	8·8	8·9	9·0	9·0
11 „	144·0	143·95	143·0	142·55	142·3	142·8	...	8·9	8·8	8·9	9·1	9·3
Midn.	144·57	144·9	144·9	145·35	145·9	146·2	9·8	9·9	9·9	9·9	9·8	9·8
1 A.M.	144·9	144·1	144·1	144·1	144·9	145·0	9·9	10·4	10·6	11·3	11·3	11·7
2 „	145·6	146·8	148·0	150·9	151·8	149·0	11·9	11·9	11·9	11·9	12·0	11·9
3 „	148·2	148·7	150·2	152·0	152·6	153·0	12·0	12·0	12·1	12·1	12·1	12·1
4 „	152·9	152·4	...	152·3	153·0	153·0	12·2	...	12·2	12·6	12·6	12·9
5 „	151·8	151·0	152·0	152·9	153·0	153·8	12·7	12·7	13·0	12·8	12·5	12·6
6 „	153·9	154·1	154·2	154·0	153·0	152·2	13·5	13·5	12·3	12·3	12·0	12·0
7 „	152·0	151·7	151·5	151·5	151·5	151·5	12·0	11·9	11·9	11·8	11·6	11·6
8 „	151·5	151·9	151·5	150·5	150·8	151·1	11·6	11·8	11·8	11·8	11·9	12·1
9 „	151·5	152·0	152·0	153·1	154·0	155·0	12·1	12·0	12·0	12·0	11·9	11·9
10 „	155·4	155·0	154·9	152·7	150·0	149·2	11·9	12·1	12·1	10·9	10·8	10·4
11 „	148·0	147·4	148·0	147·8	148·9	148·7	10·4	10·5	10·5	10·7	10·7	10·7
Noon.	144·2	142·9	140·95	140·05	140·5	138·05	10·7	10·6	10·6	10·5	10·5	10·2
1 P.M.	136·8	135·2	135·0	134·1	134·4	134·0	9·9	9·9	9·9	9·9	10·2	10·2
2 „	132·9	130·47	127·9	127·95	125·1	123·9	10·6	10·5	10·8	10·8	10·6	10·5
3 „	123·1	122·05	119·0	121·0	114·3	113·0	10·5	10·3	10·1	10·8	10·9	10·7
4 „	110·7	111·1	111·9	109·27	110·3	110·0	10·7	10·6	10·6	10·2	10·1	9·9
5 „	109·0	108·0	108·1	107·9	107·6	104·8	9·8	9·5	9·5	9·5	9·7	9·7
6 „	103·5	102·5	100·8	97·0	95·1	91·9	9·6	10·0	10·1	10·2	10·2	10·3
7 „	90·9	90·8	90·2	89·9	88·8	89·7	10·0	11·2	13·2	12·5	12·7	13·1
8 „	90·0	94·8	98·2	100·8	102·1	106·0	13·9	13·9	14·0	13·9	13·5	13·3
9 „	107·4	109·1	111·1	114·0	115·1	116·0	14·0	14·7	14·8	14·9	15·2	15·2
10 „	116·9	117·5	118·5	118·6	119·4	120·0	16·0	15·7	15·5	15·8	15·8	16·1
11 „	121·8	121·8	122·1	122·4	123·0	123·2	16·5	16·5	14·3	9·0	10·1	11·7
Midn.	123·2	123·3	123·3	123·1	123·0	123·0	14·3	14·5	15·3	15·5	16·2	16·5

The numbers are scale-divisions: coefficients for reduction into changes of force { Hor. Force 0·000177.
Vert. Force 0·000225.

Observations taken on Sunday, 26th September, 1841.				
Magnetometers.	Date.			
	9 A.M.	11.	3 P.M.	8.
Declination	4′·05	3′·63	4′·05	3′·63
Horizontal Force	Div. 148·2	Div. 151·6	Div. 148·2	Div. 143·8
Vertical Force ...	16·0	...	16·0	13·2

Half-hourly observations of the Magnetometers from $1^h\ 30^m$ to $10^h\ 30^m$ A.M., 27th September.						
Gött. Mean Time.	Declination.		Horizontal Force.		Vertical Force.	
Hour.	m s 00 00	m s 30 00	m s 2 30	m s 32 30	m s 57 30	m s 27 30
			Scale Div.	Scale Div.	Scale Div.	Scale Div.
1 A.M.	′ ...	4′·48	...	149·2	...	14·4
2 ,,	4·46	4·48	147·9	147·5	14·1	14·1
3 ,,	4·12	4·05	147·0	147·2	14·0	13·8
4 ,,	3·77	...	147·9	...	13·8	...
5 ,,	...	...	...	...	...	...
6 ,,	3·84	4·12	149·6	149·6	14·5	14·5
7 ,,	4·87	4·91	149·0	148·3	14·6	14·4
8 ,,	3·48	3·13	150·0	148·5	14·2	14·5
9 ,,	1·96	1·14	146·0	144·6	14·1	13·6
10 ,,	1·71	1·78	147·0	148·1	13·2	11·6

Observations of Magnetometers continued, September 27.

Declination.														
Gött. Mean Time.	11 A.M.	12.	1 P.M.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	1 A.M.
m s 0 00	1′·35	4′·12	6′·32	5′·69	5′·62	4′·84	2′·84	3′·48	4′·34	4′·83	4′·12	4′·41	4′·91	5′·33	5′·12
5 00	1·56	4·83	6·67	5·73	6·05	4·91	2·63	3·40	4·27	4·69	4·19	4·41	4·91	5·12	5·12
10 00	1·78	5·01	6·60	5·90	6·19	5·19	2·49	3·40	4·27	4·83	4·19	4·34	4·98	5·55	4·98
15 0	1·42	5·40	6·39	5·83	5·76	5·40	2·63	3·47	4·55	4·83	4·34	4·34	4·98	5·55	...
20 0	1·42	5·61	6·39	6·26	5·55	4·98	2·70	3·47	4·55	4·83	4·34	4·41	5·05	5·55	...
25 0	1·99	5·61	6·30	6·44	5·41	4·41	2·84	3·47	4·27	4·83	4·41	4·55	5·05	5·62	...
30 0	2·20	5·61	6·34	6·40	5·48	4·27	2·91	4·04	4·27	4·83	4·27	4·76	5·05	5·58	5·12
35 0	2·70	5·57	6·34	...	5·48	4·06	3·05	4·04	4·33	4·27	4·27	4·83	5·19	5·58	...
40 0	2·84	5·57	6·34	6·19	5·20	3·99	3·19	4·18	4·83	4·27	4·27	4·83	5·26	5·58	...
45 0	2·91	5·57	6·34	6·03	5·06	3·56	3·33	4·18	4·83	4·19	4·34	4·83	5·33	5·55	...
50 0	3·41	5·61	6·06	5·73	4·84	3·35	3·40	4·26	4·83	4·19	4·41	4·83	5·33	5·48	...
55 0	3·48	5·75	5·69	5·62	4·77	3·07	3·40	4·69	4·83	4·19	4·31	4·83	5·33	5·26	...

Observations of Magnetometers continued, September 27.

Horizontal Force.

Gött. Mean Time.	11 A.M.	12.	1 P.M.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	1 A.M.
	Sc. Di.	Sc. Di.	Sc. Di.	Sc. Di.	Sc. Di.	Sc. Di.	Sc. Di.	Sc. Di.	Sc. Di.	Sc. Di.	Sc. Di.	Sc. Di.	Sc. Di.	Sc. Di.	Sc. Di.
2 30	145·1	152·0	149·0	145·8	146·1	145·0	139·4	141·1	139·0	144·0	140·0	142·0	144·1	147·1	149·2
12 30	145·1	152·0	147·27	144·0	146·0	142·3	140·0	141·2	139·7	144·0	140·1	142·0	144·8	149·0	...
22 30	146·0	151·0	145·75	144·77	145·8	142·6	141·0	141·1	140·9	143·0	140·7	142·5	145·1	150·0	...
32 30	148·0	149·8	144·7	145·0	145·1	141·1	140·9	141·1	142·0	142·0	141·2	142·9	145·5	150·0	149·9
42 30	149·2	149·0	144·6	145·9	143·8	140·8	140·6	140·0	142·8	140·1	142·0	143·0	146·0	149·9	...
52 30	150·9	148·6	144·9	146·0	142·0	140·2	140·9	139·0	143·0	139·1	142·4	143·8	146·2	149·2	...

Vertical Force.

Gött. Mean Time.	11 A.M.	12.	1 P.M.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	1 A.M.
7 30	9·9	9·6	7·9	7·0	6·9	7·3	7·5	7·7	8·4	9·0	10·2	10·7	11·2	11·7	12·0
17 30	9·8	9·2	7·5	6·9	6·4	7·3	7·5	7·7	8·3	9·1	10·2	10·7	11·2	11·9	...
27 30	9·8	8·8	7·3	7·1	7·0	7·3	7·7	8·2	8·3	9·5	10·1	10·9	11·4	11·9	12·4
37 30	9·7	8·3	7·2	7·1	7·0	7·5	7·7	8·2	8·6	9·4	10·3	11·0	11·5	12·0	...
47 30	9·7	8·1	7·1	6·9	7·0	7·5	7·7	8·2	8·8	9·5	10·2	11·1	11·6	12·1	...
57 30	9·7	8·0	6·8	6·5	7·0	7·5	7·7	8·5	9·2	9·5	10·7	11·1	11·7	12·1	...

Half-hourly Observations from 2 A.M. to 9 A.M. of the 28th of September.

Gött. Mean Time.	Declination.		Horizontal Force.		Vertical Force.	
	m s 00 00	m s 30 00	m s 2 30	m s 32 30	m s 27 30	m s 57 30
	′	′	Scale Div.	Scale Div.	Scale Div.	Scale Div.
1 A.M.	......	......	......	......	......	12·7
2 "	5·40	4·98	150·0	150·6	13·1	13·4
3 "	4·91	4·91	150·0	150·0	13·7	13·8
4 "	4·91	......	150·0	......	......	......
5 "	......	......	......	......	......	13·7
6 "	4·46	5·09	151·2	151·9	13·8	13·8
7 "	6·27	5·69	150·9	150·0	13·9	14·0
8 "	4·83	4·76	151·2	151·9	14·2	14·4
9 "	4·27	......	153·0	......	......	......

Tables exhibiting the mean diurnal change of the Declination, Horizontal and Vertical Intensity, and the mean daily position of the several Instruments during the month of September 1841.

Gött. Mean Time.	Mean Values.		
	Declination.	Horizontal Intensity.	Vertical Intensity.
		Scale Div.	Scale Div.
0 A.M.	23° 01′·36	152·61	3·61
2 „	1·38	154·25	3·59
4 „	1·27	155·28	4·69
6 „	0·96	156·11	5·23
8 „	0·30	155·93	5·67
10 „	1·99	158·98	6·36
12 „	2·24	161·89	5·39
2 P.M.	1·58	159·65	4·32
4 „	2·07	153·91	3·43
6 „	2·02	151·11	2·55
8 „	1·46	150·02	3·07
10 „	1·48	150·98	3·49
12 „	1·36	152·61	3·61

Day.	Mean Values.	
	Declination.	Horiz. Intensity.
		Scale Divisions.
1	23° 2′·37	....
2	3·38	....
3	3·25	154·18
4	2·52	157·95
6	3·16	158·29
7	2·35	157·80
8	2·48	157·15
9	2·07	158·06
10	1·21	160·04
11	0·80	160·05
13	0·75	155·19
14	0·83	152·22
15	22 59·77	155·52
16	59·09	156·36
17	59·41	155·73
18	58·82	158·49
20	23 1·71	156·05
21	1·53	155·72
22	0·93	158·16
23	0·09	160·54
24	0·65	158·77
25	2·24	131·72
27	2·28	146·62
28	1·63	151·37
29	3·06	151·42
30	3·28	153·99

Dates and Extreme Ranges of the principal Magnetical Disturbances observed at St. Helena.

Date.	Declination.	Horizontal Intensity.	Vertical Intensity.	Remarks.
1840.				
June 26 and 27......	1° 25′·4	0·00127	....	Declination-Magnetometer observed at intervals of 5 minutes from 1 P.M. on the 26th to 9 A.M. on the 27th. Horizontal-force from 8 P.M. inclusive.
July 25	0 02·63	0·00339		
September 21 & 22	7·24	0·00579		
October 19	7·67	0·00500		
1841.				
March 22 and 23 ...	6·96	0·00598		
April 3	3·48	0·00554	0·00253	
September 25	9·80	0·01490	0·00281	

XXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xix. p. 522.]

Nov. 18, 1841. THE following papers were read:—

1. "Variations de la déclinaison et intensité magnétique horizontale observées à Milan le 28 et 29 Mai, le 23 et 24 Juin, le 21 et 22 Juillet, le 27 et 28 Août, et le 22 et 23 Septembre 1841." Par Sig^r. Carlini, For. Memb. R.S.

2. "Variations de la déclinaison magnétique et de l'intensité magnétique horizontale observées à Bruxelles le 23 et 24 Juin, et le 21 et 22 Juillet 1841." Par M. A. Quetelet, For. Memb. R.S.

3. "Meteorological Register kept on board the Earl of Hardwicke, during a voyage from London to Calcutta and back to London, by Captain Alexander Henning." Communicated by Sir John F. W. Herschel, Bart., F.R.S., &c.

4. "Meteorological Register kept at Port Arthur, Van Diemen's Land, by Deputy-Assistant-Commissary-General Lempriere, from Feb. 1, 1840, to Feb. 1, 1841." Communicated by Captain Beaufort, R.N., F.R.S., Hydrographer to the Admiralty.

5. "Term Observations of the Variation, Magnetic Declination, Horizontal Intensity, and Inclination at Prague, for June, July, August and September 1841." By Professor Kreil. Communicated by S. Hunter Christie, Esq., Sec. R.S.

November 25.—The following papers were read, viz.—

1. "Explanation of the construction, positions, comparisons, and times of observation, of the Meteorological Instruments at the Royal Observatory, Greenwich, with which the Observations have been made that are contained in the sheets of Meteorological Observations, forms 1 and 2, for each month from 1840 November to 1841 July, both inclusive, sent to the Royal Society in 1841, October 26." By George Biddell Airy, Esq., M.A., F.R.S., Astronomer Royal.

2. "On the Laws of the rise and fall of the Tides in the River Thames." By George Biddell Airy, Esq., M.A., F.R.S., Astronomer Royal.

The conclusions arrived at by the author, and stated in this paper, were derived from an extensive series of observations of the tides, made, on his suggestion, at the Royal Victualling Yard at Deptford, under the superintendence of Captain Shireff, R.N. The object of the first series of observations was simply to ascertain the times of high and low water, for the purpose of ascertaining the duration of the rise and fall of the tide: the height of the water was observed at every quarter of an hour, night and day, during half a lunation. The curves representing the law of rise and fall of the water were found to be different for high tides and for low tides; and both are sensibly different from the line of sines. The author then investigates mathematically the motion of a very long wave, such as a tide-wave, in a rectangular canal, whose section is everywhere the same, on the supposition that the extent of vertical oscillation bears a sensible proportion to the mean depth of the water; and deduces an expression for the vertical elevation of a particle at the surface. This expression supposes the canal unlimited at the end farthest from the sea. If the canal be stopped by a barrier, the expression changes its form. The formulæ obtained by the author enable him to explain a circumstance, hitherto perplexing, namely, that the age of the tide is different as inferred from the height of the high water, or from the time of high water; being always greater in the former mode of estimation.

3. "Register of Tides, observed at Coringa, from January 1st to June 30th, 1841."

4. "Meteorological Journal, from the 20th April 1840 to the 29th April 1841. Kept at the Falkland Islands on board H.M. Ketch, Arrow."

5. "Daily Thermometrical Observations at Cape Palmas, for May 1841."

These last three papers were communicated to the Society by the direction of the Lords Commissioners of the Admiralty.

November 30, 1841.—Anniversary Meeting. The following are extracts from the Report of the Council.

Awards of Medals.

The Council has awarded the Copley Medal for the present year to Dr. G. S. OHM, of Nuremberg, for his researches into the laws of Electric Currents, contained in various memoirs published in Schweigger's Journal *, Poggendorff's Annalen, and also in a separate work, entitled *Die Galvanische Kette Mathematisch Bearbitet*†, published at Berlin in the year 1827. In these works, Dr. Ohm has established, for the first time, the laws of the electric circuit; a subject of vast importance, and hitherto involved in the greatest uncertainty. He has shown that the usual vague distinctions of intensity and quantity have no foundation, and that all the explanations derived from these considerations are utterly erroneous. He has demonstrated, both theoretically and experimentally, that the action of a circuit is equal to the sum of the electro-motive forces divided by the sum of the resistances; and that whatever be the nature of the current, whether voltaic or thermo-electric, if this quotient be equal, the effect is the same. He has also shown the means of determining with accuracy the values of the separate re-

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* 1. On the electric conductibility of the metals. (Schweigger's Journal, second series, vol. xiv.)

2. Experiments to discover the power of electro-magnetic multipliers. (Ibid. vol. xxv.)

3. Researches to ascertain the nature of unipolar conductors. (Ibid. vol. xxix.)

4. On hydro-electric currents. (Ibid. third series, vol. iii.)

5. Statement of facts destroying the relations which have been confusedly established between several galvanic properties, and particularly hydro-electric conductors. (Ibid. vol. v.)

6. Theory of galvanic currents. (Ibid. vol. vii.)

[† A translation of this work will be found in Taylor's Scientific Memoirs, vol. ii.—EDIT.]

sistances and electro-motive forces in the circuit. The light which these investigations has thrown on the theory of current electricity is very considerable; and although the labours of Ohm were, for more than ten years, neglected, (Fechner being the only author who, within that time, admitted and confirmed his views,) within the last five years, Gauss, Lenz, Jacobi, Poggendorff, Henry, and many other eminent philosophers, have acknowledged the great value of his researches, and their obligations to him in conducting their own investigations. Had the works of Ohm been earlier known, and their value recognised, the industry of experimentalists would have been better rewarded. In this country those who have had most experience in researches in which voltaic agency is concerned, have borne the strongest testimony to the assistance they have derived from this source, and to the invariable accuracy with which the observed phænomena have corresponded with the theory of Ohm. This accordance, it may be observed, is altogether independent of the particular hypothesis which may be adopted as to the origin of electro-motive force; and obtains equally, whether that force is regarded as being derived from the contact of dissimilar metals, or as referable to chemical agency.

The Council have awarded one of the Royal Medals for this year, which had been proposed for the subject of Chemistry, to ROBERT KANE, M.D., M.R.I.A., Professor to the Royal Dublin Society, for his paper "On the Chemical History of Archil and Litmus," published in the Philosophical Transactions for 1840.

It has been found that various lichens, which communicate no colour to pure water, strike a fine blue with solution of ammonia. The valuable colouring matters archil, litmus and cudbear, are commercial preparations of these lichens. Some progress had already been made in the investigation of their colouring principles by the labours of Robiquet, Heeren, and Dumas; of which the most important step was the discovery of *Orcine*, and also of *Orceïne*, into which the former is converted by ammonia; but the observations were isolated, and the whole subject was in the greatest obscurity. The present memoir by Dr. Kane records the first attempt to sketch a general history of the class: and, considering the great and peculiar difficulties attending inquiries into organic colouring matters, the attempt may be esteemed eminently successful. It proved an investigation of considerable intricacy and great extent, involving several hundred organic analyses; and it has been conducted in a manner highly creditable to the author's skill as an analyst. The paper contains an account of the discovery of a large number of new compounds, not less than twelve, derived from archil and litmus, together with the more exact discrimination of several others, already known, but imperfectly described. The distinction made of two *Orceïnes*, which have hitherto been confounded as one, is a striking result contained in the paper: while the observations on the action of chlorine and of nascent hydrogen upon several of the bodies described, open new branches of inquiry.

The objects which the author had in view in these inquiries were the three following: namely, first, to ascertain the primitive form of the colour-making substance in a given species of lichen, and trace the stages through which it passes before the coloured substance is developed; secondly, to determine the nature of the various colouring substances which exist in the archil of commerce; and thirdly, to examine the colouring materials of ordinary litmus. He finds in the lichen *Roccella tinctoria* the following bodies, either pre-existing in the plant, or formed during the processes employed for its analysis: 1. Erythryline; 2. Erythrine (the Pseudo-erythrine of Heeren); 3. Erythrine bitter; 4. Telerythrine; and 5. Roccelline (the Roccellic acid of Heeren). The properties and constitution of these substances are then described, and the chemical formulæ given, which are deducible from their respective analyses. The author finds the archil of commerce to consist essentially of three ingredients, namely, orceïne, erythroleic acid, and azoerythrine; of each of the two former there exist two modifications, and there is, in addition, a yellow matter. After comparing his results with those obtained by Heeren, by an examination of the products evolved by his erythrine in contact with air and with ammonia, and stating reasons for some changes in nomenclature, the author gives the chemical formulæ resulting from his own analysis of these different substances.

His inquiries into the constitution of ordinary litmus, which form the last division of his subject, lead him to the conclusion that that substance contains the principles designated by him as Erythrolein, Erythrolitmine, Azolitmine, and Spaniolitmine; and that the colouring constituents of litmus are, in their natural condition, red; the blue substances being produced by combination with a base, the bases in that of commerce being lime, potass, and ammonia; and there is mixed up in the mass a considerable quantity of chalk and sand. The details of the analyses of these several substances, and the resulting chemical formulæ representing their constitution, are then given.

The concluding section of the paper is occupied by an inquiry into the decoloration of the bodies which exist in archil and in litmus. The latter of these, the author concludes, is reddened by acids, in consequence of their removing the loosely combined ammonia by which the blue colour is produced; and the so-called hydrogen-acids liberate the colouring matter by their combining with the alkali to form bodies (either chlorides or iodides), with which the colouring matter has no tendency to unite. Hence it appears that the reddening of litmus is no proof that chloride of hydrogen is an acid, and that the double decomposition which occurs is the same in principle, whether hydrogen or a fixed metal come into play. After detailing the blanching effects of other deoxydizing agents on the colouring matter of litmus, and the action of chlorine on orceïne and azolitmine, the author remarks, that in these actions chlorine is subjected to conditions different from those which determine the nature of the results with the generality of organic bodies, and that the displacement of hydrogen, so marked in other cases, does not exist in the class of substances under consideration; but that, in reality, the products of the bleaching energy of chlorine resemble in constitution the compounds of chlorine which possess bleaching powers.

This paper may be viewed as a very important contribution to organic chemistry, and as highly deserving of the Royal Medal; an award which will, doubtless, be hailed by chemists as a just encouragement to perseverance in skilful analytical research.

There being no paper on Mathematics coming within the stipulations regulating the awards of the Royal Medals, which has been deemed worthy of that for Mathematics in the present year, the Council have, in virtue of the power given to them, under these circumstances, by the regulations prescribed by Her Majesty, awarded the other Royal Medal to EATON HODGKINSON, Esq., for his paper, which was published in the Philosophical Transactions for 1840, and is entitled "Experimental Researches into the Strength of Pillars of Cast Iron, and other materials."

This paper has been esteemed by the Council to be peculiarly valuable in a practical as well as theoretical point of view, and therefore to deserve, in an eminent degree, the honour of a Royal Medal. It contains the results of an immense series of experiments, conducted with great patience and admirable skill, and at a very considerable cost. Mr. Hodgkinson's position among the manufactories of Manchester, together with the unlimited command over the resources of one of the largest engineering establishments, which he obtained through the liberality of its proprietor Mr. Fairbairn, enabled him to direct his inquiries to the forms of pillars which are found most useful in practice. The results of his labours he has reduced to empirical formulæ, peculiarly adapted for application to the purposes of mechanical art.

Among the most useful of the practical conclusions to which he has arrived, the following are more particularly deserving of notice.

Mr. Hodgkinson has found, that in all long pillars of the same dimensions, the resistance to crushing by flexure is about three times greater when the ends of the pillars are flat, than when they are rounded. A long uniform cast-iron pillar, with its ends firmly fixed, whether by means of discs or otherwise, has the same power to resist breaking as a pillar of the same diameter, and half the length, with the ends rounded, or turned so that the force would pass through the axis. The strength of a pillar with one end round and the other flat, is the arithmetical mean between that of a pillar of the same dimensions with both ends round, and one with both ends flat. Some additional strength is given to a pillar by enlarging its diameter in the middle part.

The strength of long cast-iron pillars with relation to their diameter and length is also made the subject of Mr. Hodgkinson's investigations; and the result he deduces from them is, that the index of the power of the diameter, to which the strength is proportional, is 3·736. He has also determined, by a comparison of experimental results, the inverse power of the length to which the strength of the pillar is proportional. The highest value of this power he finds to be 1·914, the lowest 1·537, and the mean of all the comparisons 1·7117. He thus deduces, first, approximate empirical formulæ for the breaking weight of solid pillars, and afterwards, more correct methods of determining their strength. From experiments on hollow pillars of cast-iron, formulæ representing the strength of such pillars are, in like manner, deduced.

The strength of pillars of wrought iron and of timber, in relation to their dimensions, is made the subject of another series of experiments. The result for wrought iron is, that the strength varies inversely as the square of the length of the pillar, and directly as the power 3·75 of its diameter, the latter being nearly identical with the result obtained for cast iron; while in timber, the strength varies nearly as the fourth power of the side of the square forming the section of the pillar. In like manner, the power of cast-iron pillars to resist long-continued pressure, and the relative strengths of long pillars of cast iron, wrought iron, steel and timber, are determined.

The inquiry which constitutes the subject of this paper is not, however, the first of the kind in which Mr. Hodgkinson has been engaged; several series of experiments and papers on the strength of iron, in various forms, have been published by him at different times; and their accuracy has established his claim to our confidence on the present occasion.

[The remainder of the Anniversary Proceedings will be given next month.]

XXVI. *Intelligence and Miscellaneous Articles.*

PREPARATION AND COMPOSITION OF SEBACIC ACID. BY J. REDTENBACHER.

M. THÉNARD prepared this acid by treating the products of the distillation of most fatty bodies with boiling water; as the solution cools, foliated crystals of the acid separate; these are purified by adding acetate of lead to the aqueous solution, and by decomposing the precipitate with hydrosulphuric acid.

Berzelius prepared this acid by boiling the product of the distillation of fatty bodies with carbonate of lime diffused through a large quantity of water, and decomposing the sebate of lime formed by nitric acid. Berzelius supposed the acid thus produced to be benzoic acid modified by some empyreumatic matter.

MM. Dumas and Peligot gave the following as the composition of hydrated sebacic acid: $C^{10} H^{16} O^3 + H^2 O$, the water being separable by an equivalent of a metallic base; with this the analysis of M. Redtenbacher agrees.

After relating various experiments on the preparation of this acid, M. Redtenbacher observes, that the greater part of fatty bodies, both of animal and vegetable origin, yield sebacic acid when they are distilled, as ox-fat, hog's-lard, olive-oil, &c. Neither stearin nor margaric acid, nor glycerin, when they are pure, yield the smallest trace of sebacic acid by distillation: as fats and oils contain only oleic acid besides the three compounds above named, it follows as a necessary conclusion, that sebacic acid is produced from it by distillation.

When oleic acid is distilled alone, the product, among other substances, contains a large portion of sebacic acid, the quantity of which is not increased when the oleic acid employed contains other fixed fatty acids: wax yields no sebacic acid by distillation, therefore it contains no oleic acid, nor does spermaceti. M. Thénard has indeed indicated the formation of sebacic acid during the distillation of wax, as a means of determining whether it is adulterated with fat.

As sebacic acid is but very sparingly soluble in cold water, it is easily recognised, both by its appearance and its reaction with the salts of lead, mercury, and silver; and as it is sufficient to distil less than an ounce of any fatty body, the sebacic acid from the product of which is easily separated by boiling water, it must be admitted that sebacic acid is the best reagent for detecting the presence of oleic acid in a fatty matter. This fact is especially important in the preparation of margarin and stearin, the only method which has been hitherto employed consisting in the saponification of the fatty matter, and ascertaining the fusing point of the fatty acid set free.

The best process which can be employed for the preparation of sebacic acid is that of distilling the rough oleic acid produced in the manufacture of stearin candles.—*Ann. de Chim. et de Phys., Sept.* 1841.

EUCHROIC ACID.

M. Wœhler finds, that when mellitate of ammonia is heated to 302° Fahr. much ammonia is evolved and water evaporated, and the salt is converted into two new azotized bodies, one of which is an acid and remains combined with ammonia, in the form of an ammoniacal salt, while the other is quite insoluble in water. The disengagement of ammonia commences at 212°, and when the temperature is raised to above 320° Fahr., secondary products are formed which interfere with the results.

When the decomposition is complete, the salt is changed into a pale yellow powder which is separated by water into two substances, namely, an ammoniacal salt which is dissolved, and a white substance which remains undissolved; the former is *euchroate of ammonia*, and the latter *paramide.*

If the decomposition has not been complete, the euchroate of ammonia contains excess of acid, or rather supereuchroate of ammonia; and if the heat be too great, the yellow bitter substance already mentioned is produced. The ammoniacal salt with excess of acid is deposited in imperfectly crystalline scales, which are but sparingly soluble in cold water, and have a strongly acid reaction.

To obtain the acid from the ammoniacal salt, it is to be dissolved in the smallest possible quantity of boiling water, and nitric or hydrochloric acid is to be added to the hot solution. As soon as the temperature is lowered a few degrees, the euchroic acid begins to separate in the form of a white crystalline powder, and after cooling but little of it remains in solution. It is to be purified by repeated solution in hot water and crystallization; by long and well managed cooling, small, but very distinct crystals are obtained.

This acid crystallizes in very flat four-sided prisms, which are usually aggregated in a very peculiar manner; it is very sparingly soluble, has a very distinct acid taste, resembling that of bitartrate of potash. When exposed to heat the crystals lose water, and become opake without efflorescing. At different temperatures it appears to lose portions of the water which it contains, and under certain circumstances, which have not been altogether determined, it appears to crystallize with a different quantity of water than that alluded to; in all cases, however, it loses all its water at 212°, without decomposing.

Euchroic acid appears to be composed of

2 equiv.	Hydrogen..	12·479
12	Carbon....	917·022
4	Oxygen ..	400·000
4	Azote	354·008
	Equivalent	1683·509

In its crystallized state it contains 10·67 per cent. of water.

Euchroic acid may be heated to 536° Fahr. without alteration; if the temperature be higher than this, it fuses, boils up and decomposes into cyanide of ammonia, and a deep green and extremely bitter sublimate. The aqueous solution undergoes no change by boiling, even when hydrochloric or nitric acid is added; but if the acid be heated to 212° in a close tube with less water than is sufficient to dissolve it, it is then decomposed and converted into mellitate of ammonia, and dissolves without precipitating on cooling.

Euchroic acid is distinguished from all known organic acids by its action on metallic zinc; it is converted into a deep blue substance which precipitates upon the zinc undissolved by the acid. The colour is so deep, that the smallest traces of the acid are discoverable by it, when a drop is put on bright zinc: this blue substance does not quit the zinc when boiled in the solution of the acid, but the blue deposit becomes almost black, with a reddish tint like indigo. If the zinc be immersed for a second in dilute hydrochloric acid, the blue body dissolves and may be separated by the filter; when washed and dried, it is a black substance containing no zinc; when slightly heated, it gradually becomes white, and is reconverted into euchroic acid; it acts exactly like the colourless indigo of Berzelius, except conversely with respect to colouration. This property of euchroic acid resembles a reduction, and the blue substance may be considered either as a lower degree of oxidation or as the radical of the acid; if the latter opinion be adopted, M. Wœhler proposes to call it *euchrone*.

When this substance is dissolved in ammonia or potash, the solutions have a beautiful purple colour, exceeding in this respect and also in intensity those of oximanganate of potash and murexide; but no sooner do the solutions come into contact with the air, than they begin to decolorize at the surface, and by agitation they become perfectly colourless.

Euchrone is deposited upon platina when it is galvanically combined with zinc and immersed in a solution of the acid; the protosalts of iron also precipitate it; and when a solution of chloride of iron is mixed with one of euchroic acid, no change is produced till an alkali is added, and then a bulky precipitate of a beautiful blue colour is formed.—*L'Institut*, No. 383.

SEPARATION OF GOLD AND PLATINA.

Mr. Kemp has found that oxalic acid reduces the solutions of gold, and has no action on those of platina. To separate these two metals, therefore, and to determine their quantities, a solution may be made in aqua regia and the gold precipitated by oxalic acid in the metallic state, and the platina by formic acid. The difficulty of separating these two metals which has hitherto existed, renders this process of considerable importance.—*Journal de Pharmacie, Jan.* 1842.

LITHOFELLIC ACID.

M. Göbel has discovered in a biliary calculus, deposited in the Zoological Cabinet of Dorpat, a new body, to which he has given the above name. In order to obtain it, he dissolved the calculus in boiling alcohol of 99 per cent., and he slowly evaporated the filtered liquor, which was of a greenish-brown colour; solid crystalline deposits of the same colour were formed, which were powdered and treated repeatedly with cold alcohol: by this process he succeeded in extracting the more soluble colouring matter of the bile, and obtained a yellowish powder; this being re-dissolved in boiling alcohol, the lithofellic acid was separated by slow evaporation in the form of crystalline crusts; the crystals are rhombic prisms, terminated by an oblique surface; when these are examined with a glass some of them are entirely colourless.

Lithofellic acid dissolves in 294 parts of alcohol of 99 per cent. at 68° F., and in $6\frac{1}{2}$ parts of boiling alcohol; it requires 444 parts of absolute æther at 68° F., and 47 parts at a boiling heat. It fuses at 400° F. into a liquid of a slight yellow colour, and forms after cooling a solid mass, which is colourless and crystalline. When heated in a small retort this acid emits white vapours, which condense into a yellowish liquid, and gives a mixture of empyreumatic oil and a little acidulous water. The distilled product has a penetrating odour resembling that of oil of amber; a very small quantity of carbon remains in the retort. This empyreumatic product appears to contain a new acid; it forms a soap with potash, which is decomposed by hydrochloric acid.

When the lithofellic acid is treated with a solution of potash or soda, saponification soon takes place if the solutions are strong and the soap separates; but if the solution be dilute, the soap dissolves in and separates only on concentration.

The soap floats on the surface in the state of a fluid mass, of a light yellow colour, as long as the liquor is hot. On cooling it forms a solid mass resembling white colophony; it dissolves in æther, alcohol, and water, and acids decompose it completely; the lithofellic acid then separates entirely from the aqueous solution, and yields by desiccation a white powder; one part of soda saturates ten parts of lithofellic acid.

Ammonia dissolves lithofellic acid, and hydrochloric acid separates it from solution, without any alteration, in the state of a white powder: if the ammoniacal solution be evaporated in a salt-water bath, decomposition also occurs, and the acid separates in white scales. The soda soap of this acid gives with the salts of silver, mercury, iron, lead, platina, lime, and barytes, compounds which are but slightly soluble, or insoluble in water.

Nitric acid, by its action on lithofellic acid, gives rise to a new acid which is of a fine lemon-yellow colour.

The calculus appeared to be almost entirely composed of lithofellic acid, which seems to be of a peculiar and hitherto unknown nature.

It was analysed by MM. Ettling and Will, and found by one analysis, which did not differ much from two others, to consist of

Carbon.................	71·19
Hydrogen	10·85
Oxygen	17·96
	100·

They found the atomic weight of this acid combined with oxide of silver to be = 4213 and 4276 in two experiments, and that of the hydrate with one atom of water = 4327 and 4388.

The formula which best accords with these results is $C^{42} H^{76} O^8 = C^{42} H^{74} O\ 7 + Aq$, which gives in 100 parts,

42 atoms	Carbon	3185·86	71·43
76 ...	Hydrogen	474·22	10·63
8 ...	Oxygen	800·00	17·94
		4460·08	100·

The results of M. Göbel's analysis would give as the formula of this acid, $C^{36} H^{66} O\ 7$; the composition of this acid therefore cannot be considered as definitely fixed; but its solubility in alcohol, its facility of saponifying, the hardness of its crystals, its fusing point, and the large quantity of oxygen which it contains, seem to the author to afford sufficient essential characters to distinguish it from other fatty acids.—*Journal de Pharmacie, Nov.* 1841.

ON A NEW ARRANGEMENT OF INSTRUMENTS FOR OBSERVING TEMPERATURES AND THE DEW-POINT. BY MR. R. ADIE.

To the Editors of the Philosophical Magazine and Journal.

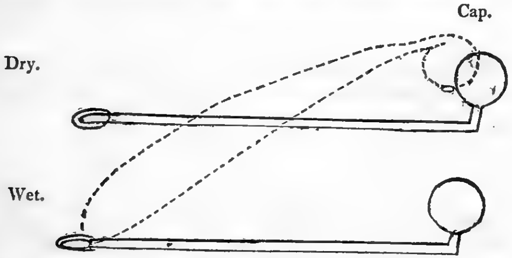
Gentlemen,

I respectfully submit for your examination the following account of a new arrangement of meteorological instruments, which may be said to be simply a compound of the two well-known instruments, Dr. Rutherford's registering thermometer, and Dr. Mason's wet and dry bulb hygrometer.

The complete instrument consists of four thermometers, two maxima and two minima, which can be fitted on one or two separate frames; with separate frames I take for the first.

Two spirit thermometers, with indexes as commonly made for the minima thermometers, graduated on their tubes to degrees and half-degrees; the reservoirs at the tops of the tubes I cover with small metallic caps, to prevent condensation of the spirit therein*; one of these tubes has the bulb covered with silk, the other left bare, but the colour of the two bulbs made to approximate, as far as practicable, like Leslie's hygrometers.

The two tubes are then fastened on a frame, the wet bulb about three inches below the dry, and the tubes parallel, thus:



The dotted line shows the position of the bird's water-fountain, which is concealed at the back of the frame: the extreme dimensions of the frame are twelve inches long by four and a half deep.

The second frame is arranged in precisely the same way with the two maxima tubes: if they are intended for the sun's rays, they are vacuum-filled†.

It will be evident these frames supply us with the common thermometer, maxima and minima thermometers, Mason's hygrometer; and, in addition, the wet and dry minima tubes give in most cases the dew-point at the time of minimum temperature, while the max-

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[*] I have found small caps of tinfoil sufficient to prevent, in a spirit thermometer exposed to two months' summer sun, the condensation of the spirit in the upper part of the tube. I need scarcely mention, that under similar circumstances, without a metallic cap, spirit would condense in a few hours after a hot sun.

[†] To fill mercury-registering thermometers with the upper part of the tube, a vacuum is the readiest mode I know to get them to work correctly in the sun; at all events, it is certain oxygen must not be present, for the sun's rays are an irresistible source of chemical action betwixt mercury and oxygen. I have a tube that has worked unimpaired for eight months in the sun.

I should mention, that it was Prof. Lloyd of Trinity College, Dublin, who noticed to me in March last the liability there would be to error in the registers of maximum and minimum temperatures, and their respective dew-points.

imum tubes furnish the dew-point at the time of maximum temperatures; I say in most cases, for there are times in unsettled weather when the result is fallacious. I watched the instrument through May and June last in the sun; the dew-point at time of maximum temperature reached 17° in town, while the dew-point at minimum temperature never exceeded 3°; this last occurred when the earth was screened by high thin clouds.

I believe this arrangement originated with me in the spring of last year; and I now submit it for the consideration of your readers, to say whether or not the weak point I have mentioned be sufficient to condemn it for practical use.

I remain, Gentlemen,
Your most obedient humble Servant,
Liverpool, November 23, 1841. R. Adie.

INFLUENCE OF COMETS.

Dr. Forster has just published a short Essay on the Influence of Comets, in which, alluding to a controversy long going on between him and M. Arago on this subject, he says, in conclusion, "I do not pretend that there is any calculable amount of influence exercised by comets sufficient to account for their apparent influence on our atmosphere. I assert merely, that on comparing catalogues of these bodies with terrestrial and atmospheric commotions, the coincidences are too numerous to belong to the chapter of accidents. The doctrine of chances forbids such a supposition."

VEGETABLE AND ANIMAL FIBRIN, ALBUMEN, AND CASEIN.

MM. Scherer and Jones, operating in the laboratory of M. Liebig, have found that vegetables contain three azotized bodies, which they have called vegetable fibrin, vegetable albumen, and vegetable casein. These substances possess precisely the same elementary composition.

Vegetable fibrin is the matter which does not dissolve when gluten is treated with alcohol; the portion soluble in this liquid they have named vegetable gluten. Vegetable albumen is found in the juice of vegetables, and casein is extracted from peas, beans, or lentils; when these are treated with water, the portion dissolved is the casein.

On comparing the properties of these three bodies with those of the corresponding bodies of animal origin, that is to say, albumen, fibrin, and casein, it is found that the vegetable substances possess all the properties which belong to those of animal organization.

On submitting these different matters to analysis, MM. Scherer and Jones found that they are isomeric. These unexpected results throw great light on physiological phænomena; they explain the reciprocal transformations which fibrin, casein, and albumen undergo in the animal organization; they also lead to this very remarkable physiological consequence, that herbivorous animals find in vegetables, substances which are analogous to their blood and muscular flesh. The following are the results of the different analyses:—

	Vegetable Fibrin.	Animal Fibrin.
C	= 53·83	= 54·454
N	= 15·59	= 15·762
H	= 7·02	= 7·069
O S Ph	= 23·56	= 22·715
	100·	100·

	Vegetable Albumen.	Animal Albumen.
C	= 54·74	= 55·097
N	= 15·85	= 15·920
H	= 7·77	= 7·073
O S N	= 21·64	= 22·007
	100·	100·097

	Vegetable Casein.	Animal Casein.
C	= 54·138	= 54·507
N	= 15·672	= 15·670
H	= 7·156	= 6·900
O S Ph	= 23·034	= 22·923
	100·	100·

Journal de Pharmacie, Jan. 1842.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1841.

Chiswick.—Dec. 1. Overcast. 2. Cloudy: rain. 3. Fine: rain: clear at night. 4. Clear: heavy rain: densely clouded. 5. Cloudy: clear and fine: cloudy. 6. Overcast: heavy rain: clear. 7. Clear: overcast at night. 8. Rain: cloudy. 9. Very fine: rain. 10. Overcast: rain: clear. 11. Slightly overcast: clear: rain at night. 12. Rain: stormy. 13. Rain: clear at night. 14. Cloudy and cold. 15. Densely overcast. 16. Very fine. 17. Clear and frosty. 18. Frosty haze. 19. Sharp frost: slight snow. 20. Frosty: fine. 21. Clear. 22. Slight frost: drizzly. 23. Hazy: drizzly. 24. Overcast: rain. 25. Rain: clear. 26. Overcast: clear: cloudy at night. 27. Hazy. 28. Foggy: cloudy and fine. 29. Dense fog. 30. Hazy. 31. Very fine: rain at night.

Boston.—Dec. 1. Fine: rain yesterday P.M. 2. Fine. 3. Rain: rain early A.M. 4. Stormy: rain early A.M.: rain A.M. and P.M. 5. Stormy. 6. Rain: rain early A.M. 7. Fine. 8. Cloudy: rain early A.M. 9. Fine: rain P.M. 10. Cloudy: rain A.M. 11. Fine. 12. Cloudy: rain early A.M.: rain A.M. 13. Cloudy. 14. Stormy. 15. Cloudy. 16. Fine: rain early A.M. 17—19. Fine. 20. Misty. 21, 22. Fine. 23. Cloudy. 24. Fine. 25. Fine: rain P.M. 26—28. Fine. 29. Rain: rain early A.M. 30, 31. Fine.

Applegarth Manse, Dumfries-shire.—Dec. 1. Slight showers. 2. Rain A.M. 3. Wet and stormy. 4. Fine A.M.: rain P.M. 5. Fine. 6, 7. Rain morning and evening. 8. Rain morning and evening: Aurora. 9. Frost A.M.: rain P.M. 10—12. Heavy showers. 13. One slight shower. 14. Frost A.M.: Aurora. 15. Heavy rain. 16. Rain and squalls. 17. Clear and frost. 18. Clear and frost: cloudy P.M. 19. Slight fall of snow: frost. 20. Thick fog: frost. 21. Frost. 22. Frost: fine. 23. Fog and thaw: rain. 24. Rain. 25. Frost A.M.: rain P.M. 26. Frost, fair and clear. 27. Frost A.M.: thaw and rain P.M. 28. Fine. 29. Dull and moist: rain P.M. 30. Thick fog: rain P.M. 31. Fog and rain.

Sun shone out 21 days. Rain fell 21 days. Frost 10 days. Snow 1 day. Fog 4 days. Aurora 2 days.

Wind north ½ day. North-north-east ½ day. North-east 1 day. East 2 days. South-east 1½ day. South-south-west 4 days. South-west 11½ days. West-south-west 1½ day. North-west 5½ days. North-north-west 3 days.

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary,* Mr. Roberton; *by* Mr. Thompson, *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *by* Mr. Dunbar, *at Applegarth Manse,* DUMFRIES-SHIRE, *and at Sandwick Manse,* ORKNEY.

Days of Month.	Barometer.								Thermometer.										Wind.					Rain.					Dew-point.
1841. Dec.	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8½ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R.S. 9 a.m.
		Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mm.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.											
1.	29·396	29·536	29·301	28·97	29·06	29·14	29·10	29·33	48·7	55·8	47·6	54	43	43·5	47	42½	43	38	se.	se.	calm	e.	calm	·055	·08	·26	......	·50	48
2.	29·486	29·437	29·207	29·02	29·18	29·17	29·40	29·42	49·3	52·4	47·3	53	46	44	48	38	43	44	s.	s.	calm	ne.	e.	·033	·15	......	......	......	47
3.	28·954	29·246	28·948	28·62	28·78	28·78	29·38	29·25	51·2	53·3	49·0	54	43	48	49	41½	45	45	e.	sw.	calm	se.	e.	·039	·17	·05	......	·10	48
4.	29·272	29·922	29·297	28·72	28·85	29·19	29·21	29·48	48·0	53·4	45·4	49	40	46	49½	42	46	45	s.	sw.	w.	sw.	e.	·172	·13	·10	......	·43	45
☾ 5.	29·960	30·038	29·938	29·39	29·74	29·77	29·85	29·83	44·0	50·6	43·4	50	37	48	47½	40½	42	45	w.	w.	w.	sw.	se.	·161	·14	·21	......	......	40
6.	29·776	29·761	29·582	29·29	29·34	29·43	29·25	29·13	50·2	51·4	43·5	52	35	48	49½	41	46	46½	s.	sw.	sw.	wsw.	ses.	·091	·30	·21	......	·35	45
7.	29·916	29·868	29·793	29·42	29·55	29·40	29·40	29·21	43·0	53·0	40·6	50	47	42	50	39	46	46	s.	w.	sw.	sw.	w by s.	·222	·15	·25	1·20	·30	43
8.	29·488	29·478	29·391	29·00	29·14	29·40	29·03	29·51	50·7	51·8	43·2	54	33	50	51	42½	45	36	s.	w.	calm	sw.	w by n	·119	·01	·12	......	·23	47
9.	29·916	29·865	29·708	29·44	29·70	29·10	29·64	29·11	41·3	54·4	40·8	44	38	35·5	47	30	35	37	w.	w.	calm	sw.	s.	......	·15	......	......	·05	40
10.	29·386	29·659	29·252	28·91	29·01	29·36	28·91	29·21	50·7	52·2	41·6	55	36	47	43½	40	40	39	s.	sw.	calm	nw.	nw.	·116	·07	·08	......	·51	46
11.	29·916	29·913	29·879	29·40	29·56	29·54	29·38	29·38	40·8	54·8	40·0	47	37	40	44	35	42	40	w.	w.	w.	ssw.	w.	·083	·09	·11	......	·07	40
● 12.	29·660	29·660	29·515	29·15	29·15	29·08	29·25	29·10	47·7	49·3	41·0	52	47	46	50	40½	38½	40½	s.	sw.	calm	ssw.	e.	·077	·17	·04	......	·24	43
13.	29·366	29·350	29·238	28·79	28·92	29·94	28·80	29·02	51·0	52·3	48·4	52	40	50	50	41	39	39	s.	sw.	calm	ssw.	calm	·213	·08	·03	......	·12	46
14.	29·540	30·033	29·490	29·06	29·50	29·68	29·42	29·50	42·2	53·2	42·2	42	27	38	41½	33	35	37	w.	n.	n.	sw.	w.	·061	......	......	......	·17	40
15.	29·908	29·907	29·510	29·42	29·19	29·12	28·91	28·86	42·8	43·7	35·2	50	37	40	48	35½	42	39	s.	s.	se.	sw.	sw.	......	·10	......	1·38	·17	39
16.	29·400	29·419	29·266	28·96	28·90	29·02	28·88	29·17	42·3	50·3	41·6	49	30	37·5	42	36	38	34½	s.	w.	calm	ssw.	n.	·063	......	·06	......	·26	42
17.	29·494	29·609	29·461	29·06	29·33	29·40	29·27	29·33	35·3	45·7	35·4	39	17	35	39½	27½	34	37½	w.	w.	calm	nnw.	nw.	......	......	......	......	·45	38
18.	29·586	29·585	29·402	29·26	29·39	29·30	29·33	29·36	31·3	38·8	31·2	35	16	29	35	22	35	31	s.	ne.	calm	nnw.	calm	......	......	......	......	·15	34
19.	29·252	29·270	29·188	29·05	29·24	29·16	29·50	29·40	31·4	35·7	29·9	37	27	24	34	22½	31	37	n.	ne.	calm	nw.	e.	......	......	......	......	......	28
20.	29·226	29·444	29·218	30·00	29·15	29·33	29·37	29·45	32·7	35·5	31·6	39	20	28·5	32	22	39	37½	n.	ne.	calm	nnw.	e.	......	......	......	......	·44	30
☽ 21.	29·626	29·676	29·599	29·32	29·42	29·50	29·40	29·43	32·3	36·2	32·8	35	26	28	38	22½	31	35	nw.	w.	calm	nw.	nw.	......	......	......	0·43	......	32
22.	29·790	29·851	29·780	29·42	29·59	29·61	29·65	29·64	32·8	37·0	32·6	37	33	27	33	24	33	32	w.	w.	calm	e.	calm	......	·02	......	......	·14	33
23.	29·860	29·854	29·793	29·46	29·44	29·50	29·40	29·42	39·7	40·8	33·3	46	27	36	40½	27	35	42	s.	sw.	calm	se.	se.	·014	·06	......	......	......	38
24.	30·050	30·016	29·902	29·64	29·56	29·47	29·35	29·10	39·3	47·4	36·9	50	42	37	47	32½	37	41	s.	sw.	calm	sw.	se.	·060	·12	......	......	·13	40
25.	29·768	29·769	29·657	28·30	29·40	29·40	29·12	29·30	44·3	50·4	39·6	49	22	40	41	36	41	42	s.	sw.	calm	sw.	w.	·183	·06	......	......	·16	42
26.	29·794	29·944	29·755	29·46	29·65	29·83	29·65	29·90	35·8	46·0	33·4	39	24	32	41	30½	41	37½	n.	nw.	calm	n.	nw.	·044	·01	·03	......	·06	35
27.	30·132	30·121	30·084	29·77	29·90	29·80	29·71	29·70	31·7	40·2	31·4	36	25	29	41½	26½	41	42	sw.	w.	calm	nw.	sw.	......	·02	......	......	·14	33
○ 28.	30·088	30·063	30·054	29·72	29·92	29·95	29·94	30·00	38·8	40·0	31·5	44	37	35	43½	37	40	40	w.	nw.	calm	nw.	nw.	·025	·02	......	0·40	·12	35
29.	30·106	30·078	30·035	29·78	29·95	29·92	29·94	30·03	43·4	45·2	39·3	45	39	38	45	39	44	42	w.	nw.	calm	sw.	sw.	......	·05	·08	......	......	39
30.	30·126	30·166	29·927	29·77	29·90	29·91	30·06	30·02	38·8	46·8	39·0	42	31	36·5	46	39½	40½	37½	ne.	se.	calm	sw.	s.	·038	......	·04	......	·14	38
31.	30·262	30·211	30·187	29·89	29·93	29·92	29·92	30·01	35·0	43·2	35·0	40	33	37	45	41	41	43½	e.	se.	s.	sw.	s.	......	......	......	......	......	35
Mean.	29·694	29·766	29·592	29·27	29·140	29·422	29·400	29·438	41·5	47·1	38·8	45·80	33·38	38·5	43·8	34·4	39·64	39·64						Sum. 1·869	3·15	1·67	3·41	5·43	Mean. 39

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[THIRD SERIES.]

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XXVII. *Abstract of* Dr. Hermann Kopp's *Researches on the Specific Weight of Chemical Compounds*[*]. *By* HENRY CROFT, *Esq.*, *Teacher of Chemistry*[†].

CHEMISTS have lately commenced directing considerable attention to the "atomic volumes" of bodies, that is to say, the space occupied by an atom of any body, or, in other words, the quotient of the density into the atomic weight. In one of the preceding volumes of this Journal, Dr. Kopp has shown that *perfectly* isomorphous bodies possess the *same* atomic volume, and that in the cases of those bodies which are not perfectly isomorphous, every difference in the crystalline form is expressed by a difference in the atomic volume. The same author has lately published a work, entitled "Uber das specifische Gewicht der Chemischen Verbindungen," in which he discusses the composition of chemical bodies according to the volumes of their elements. The following is a short abstract of this work, which it seems very desirable to make known to English chemists. Dr. Kopp has struck out an entirely new branch in molecular chemistry, which promises to be hereafter of very great importance. The first attempt to determine the composition of bodies according to the volumes of their elements was made by Schröder (Poggendorff's Annals, 50, p. 553), but many objections may be raised to the manner in which this attempt was made, and to the conclusions therein deduced; a critique, by Dr. Kopp, of this work appeared in the 52nd volume of the same Journal.

In his researches on the same subject, Dr. Kopp has en-

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[*] Uber das Specifische Gewicht der Chemischen Verbindungen. Frankfurt, 1841.

[†] Communicated by the Author. Dr. Kopp's paper on the atomic volume and crystalline condition of bodies will be found in Phil. Mag. Third Series, vol. xviii. p. 255.

deavoured to avoid those faults which he blamed in Schröder's work, and which consist principally therein, that Schröder made too great a number of arbitrary assumptions and suppositions, and, notwithstanding, considered the results of his theory as absolutely certain, forgetting that the probability of a theory becomes the less in proportion as the number of assumptions on which it is founded becomes greater. Dr. Kopp, on the contrary, believes that on this subject no absolutely true theory can at present be made, but still a very probable one may be proposed, whose probability is proved by its explaining, by means of the fewest possible assumptions, the greatest possible number of experimental facts. The views of Dr. Kopp, which are explained in the following paper, differ considerably from those of Schröder; but they embrace a much greater number of compounds, which they fully explain with very few assumptions.

It will be better first of all to give a tabular view of the atomic volumes of the simple bodies as obtained by dividing the atomic weights by the densities. These atomic volumes of simple bodies in an isolated state are called by Dr. Kopp the "Primitive atomic volume," to distinguish them from those which elements possess when they enter into combination; for in compounds they may be contained with atomic volumes quite different from their primitive ones. We will here give the atomic volumes as adopted by Dr. Kopp; the atomic weights (which are always those of Berzelius, with the exception of that of bismuth, which is taken as 1330 according to the latest researches) may be left out, as well as the observed densities, which agree very well with those calculated from the adopted atomic volumes. Cyanogen has been added.

TABLE I.—Primitive atomic volumes of the simple bodies, and of their calculated densities.

	Prim. At. Vol.	Calcul. Density.		Prim. At. Vol.	Calcul. Density.
Antimony........	120	6·72	Iodine	160	4·93
Arsenic..........	80	5·87	Iridium..........	57	21·6
Bismuth	135	9·85	Lead	114	11·35
Bromine	160	3·06	Manganese	44	7·86
Cadmium........	81	8·60	Mercury	93	13·6
Charcoal	36	2·11	Molybdenum	69	8·68
Chlorine	160	1·38	Nickel	44	8·41
Chromium	69	5·10	Osmium	57	21·8
Cobalt	44	8·39	Palladium	57	11·7
Copper..........	44	9·00	Phosphorus	111	1·77
Cyanogen........	160	1·03	Platinum	57	21·6
Gold	65	19·1	Potassium	583	0·84

Table (*continued*).

	Prim. At. Vol.	Calcul. Density.		Prim. At. Vol.	Calcul. Density.
Rhodium	57	11·4	Tin	101	7·28
Selenium	115	4·30	Titanium	57	5·33
Silver	130	10·4	Wolframium	69	17·1
Sodium	292	0·99	Zinc	58	6·95
Sulphur	101	1·99			

From this table we see that the bodies contained in the several groups following possess equal atomic volumes.

Bromine, chlorine, iodine, cyanogen, chromium, molybdenum, wolframium, iron, cobalt, copper, manganese, nickel, iridium, osmium, palladium, platinum, rhodium.

The atomic volume of silver is double that of gold.
... ... potassium ... sodium.

The atomic volume of a chemical compound is hardly ever equal to the sum of the primitive atomic volumes of its elements. For instance, the atomic volume of chlorine $= 160$, that of sulphur $= 101$; but the atomic volume of the compound $S\,Cl$ is not $= 160 + 101 = 261$, but according to observation it is $= 225$. Consequently, in this compound either both or one of the elements have different atomic volumes from their primitive ones.

Only such compounds will be examined as we can consider as binary.

If the atomic volume of a compound is greater than the primitive atomic volumes of either of its components, we cannot tell whether one or both of the elements are contained in it with an atomic volume different from their primitive one; but we know for certain that one element in a compound does not possess its primitive atomic volume if the atomic volume of the compound is smaller than the primitive atomic volume of that element. It is not as yet possible to state for every compound, which element enters into it with its primitive atomic volume, or whether both acquire different ones. It is only possible to state this with a great degree of probability for such classes of compounds as have an analogous composition, so that in all the compounds there is *one* common element.

Schröder has found, namely, that if we have a series of analogous bodies, A O, B O, C O, whose atomic volumes we know, and if we know also the primitive atomic volumes of A, B, C, we shall always obtain the same remainder if from the atomic volume of A O we subtract the primitive atomic volume of A, from the atomic volume of B O, the primitive volume of B, and from the atomic volume of C O that of C. For instance, in the case of the oxides:—

$$\begin{array}{lrlrlr} \text{The atomic volume of } \mathrm{Pb\,O} & = 146, & \mathrm{Cd\,O} & = 113, & \mathrm{Zn\,O} & = 90 \\ \ldots \quad \ldots \quad \ldots \quad \mathrm{Pb} & = \underline{114}, & \mathrm{Cd} & = \underline{81}, & \mathrm{Zn} & = \underline{58} \\ & 32 & & 32 & & 32 \end{array}$$

Or for the nitrates:—

$$\begin{array}{lrlr} \text{Atomic volume of } \mathrm{Pb\,N^2\,O^6} & = 472, & \mathrm{Ag\,N^2\,O^6} & = 488 \\ \ldots \quad \ldots \quad \ldots \quad \mathrm{Pb} & = \underline{114}, & \mathrm{Ag} & = \underline{130} \\ & 358 & & 358 \end{array}$$

For the explanation of the atomic volumes of these bodies, we see that one assumption as to the atomic volume of one element is quite sufficient. We assume that the metal is contained therein with its primitive atomic volume. In the nitrates we say that the radical $N^2\,O^6$ has the atomic volume 358.

Dr. Kopp examines first the salts, and divides them into two groups,—salts of heavy metals and salts of light metals. He considers them according to the hydracid theory.

In the salts of heavy metals he assumes that the metal possesses its primitive atomic volume; but with the salts of the light metals this is not possible, for the atomic volumes of the salts are often smaller than the primitive atomic volumes of the component metals.

He therefore assumes for the light metals a peculiar atomic volume, which remains the same in all their salts; we will give these atomic volumes in the following table:—

Ammonium	218	Magnesium	40
Barium............	143	Sodium............	130
Calcium	60	Strontium.........	108
Potassium	234		

He determines these numbers in the following manner:—Suppose $M + R$ to be a compound of a heavy metal, $m + R$ the analogous compound of a light one. Suppose A to be the known atomic volume of $M + R$ and a that of $m + R$, B the primitive atomic volume of M, and b of m.

$$\begin{array}{rl} \text{Then, atomic volume of } M + R & = A \\ \text{Prim. atom. volume of } M & = B \\ \hline A - B & = x, \end{array}$$

the atomic volume with which R is contained in the compound. It is assumed that R retains its value in $M + R$; and therefore

$$\begin{array}{rl} \text{Atomic volume of } m + R & = a \\ \ldots \quad \ldots \quad R & = x \\ \hline \end{array}$$

therefore peculiar atomic volume of $m = a - x$, which are both known.

For the carbonates the following rule may be given. To the primitive atomic volumes of the heavy metals, or the peculiar ones of the light metals, the number 151 must be added, which is the assumed atomic volume of CO^3. In this manner the following atomic volumes and densities of the carbonates are obtained:—

Salts.	Atom. Vol.	Calcu. Sp.Gr.	Obser. Sp.Gr.	Salts.	Atom. Vol.	Calcu. Sp.Gr.	Obser. Sp.Gr.
$Pb + CO^3$ ..	265	6·30	6·43	$BaCO^3$	294	4·19	4·24
$Cd + CO^3$ ..	232	4·63	4·49	$CaCO^3$	211	3·00	3·00
$Fe + CO^3$ ..	195	3·67	3·83	KCO^3......	385	2·25	2·26
$Mn + CO^3$...	195	3·70	3·59	$MgCO^3$	191	2·80	2·98
$Ag + CO^3$ ..	281	6·15	6·08	$NaCO^3$	281	2·37	2·47
$Zn + CO^3$ ..	209	3·73	4·44	$SrCO^3$	259	3·56	3·60

And for the double salts,

$$\text{Bitterspar}\left\{\begin{matrix} MgCO^3 \\ CaCO^3 \end{matrix}\right\} 402 - 2{\cdot}90 - 2{\cdot}88$$

$$\text{Mesitin}\left\{\begin{matrix} MgCO^3 \\ FeCO^3 \end{matrix}\right\} 386 - 3{\cdot}24 - 3{\cdot}35.$$

All these agree very well, except the zinc salt.

In the nitrates the atomic volume of N^2O^6 is assumed to be = 358.

PbN^2O^6....	472	4·40	4·40
AgN^2O^6....	488	4·36	4·36
AmN^2O^6 ..	576	1·74	1·74
BaN^2O^6....	501	3·20	3·19
KN^2O^6	592	2·14	2·10
NaN^2O^6....	488	2·19	2·19
SrN^2O^6	466	2·84	2·89

In several of the sulphates the atomic volume of SO^4 may be assumed to be 236. According to this the following salts were calculated:—

$CuSO^4$	280	3·56	3·57
$AgSO^4$	366	5·34	5·34
$ZnSO^4$	294	3·42	3·40
$CaSO^4$	296	2·90	2·93
$MgSO^4$	276	2·75	2·61
$NaSO^4$	366	2·44	2·46

But the assumption of 236 as the atomic volume of SO^4 does not hold good in some of the other sulphates, so that we are obliged to assume that there are two groups of sulphates, in one of which SO^4 has the atomic volume 236, and in the other 186. According to this latter number the following salts have been calculated:—

$Pb\,S\,O^4$	300	6·32	6·30
$Ba\,S\,O^4$	329	4·43	4·45
$K\,S\,O^4$	420	2·60	2·62
$Sr\,S\,O^4$	294	3·90	3·95

In the chromates we may assume $Cr\,O^4 = 228$.
In the wolframates ... ... $WO^4 = 244$.

$Pb\,Cr\,O^4$	342	5·98	5·95
$K\,Cr\,O^4$	462	2·69	2·64
$Pb\,W\,O^4$	358	8·04	8·00
$Fe\,W\,O^4$	288	6·67	7·10
$Ca\,W\,O^4$	304	6·05	6·04

We must assume that there are two groups of chlorides, in one of which the atomic volume of $Cl^2 = 196$, and in the other 245.

First Group.				Second Group.			
$Pb\,Cl^2$	310	5·60	5·68	$Am\,Cl^2$	463	1·44	1·45
$Ag\,Cl^2$	326	5·50	5·50	$Ca\,Cl^2$	305	2·29	2·27
$Ba\,Cl^2$	339	3·83	3·86	$K\,Cl^2$	479	1·94	1·94
$Na\,Cl^2$	326	2·25	2·26	$Cu\,Cl^2$	333	3·70	3·68
				$Hg^2\,Cl^2$	431	6·90	6·99
				$Hg\,Cl^2$	338	5·05	5·14
				$Sr\,Cl^2$	353	2·80	2·80

Neither of the above assumed numbers will explain the specific gravity of bichloride of tin; it seems, however, unnecessary to assume another value for the sake of only one substance.

The density of several iodides may be approximately explained by assuming the atomic volume of iodine = 361. In the potassium and silver-salts it is probably 463.

The atomic volume of bromine in the salts of lead and silver is = 233; in that of mercury = 290; and in that of potassium = 375. These assumptions must be confirmed by more observations. Dr. Kopp then proceeds to a criticism of Schröder's work: he compares the results therein obtained with those given above: he shows that Schröder has made a great many assumptions to explain but few facts, and has not mentioned those salts which do not agree with his theoretical formulæ, and he therefore considers his own theory as the simpler.

The density of many oxides of the heavy metals may be explained by assuming the atomic volume of oxygen to be = 32.

Oxides.	Calculated atomic volume.	Calculated atomic weight.	Observed atomic weight.
$Pb\,O$	146	9·55	9·50
$Cd\,O$	113	7·05	6·95
$Cu\,O$	76	6·53	6·43
$Mn\,O$	76	5·87	4·73
$Hg\,O$	125	10·9	11·00
$Zn\,O$	90	5·48	5·43
$Sn\,O$	133	6·28	6·67
$Mo\,O^2$	133	6·01	5·67
$Ti\,O^2$	121	4·16	4·18
$Pb\,O^2$	178	8·40	8·90
$Sb^2\,O^3$	336	5·69	5·78
$Pb^2\,O^3$	324	8·91	8·94
$Fe^2\,O^3$	184	5·31	5·25
$Co^2\,O^3$	184	5·64	5·60
Ilmenite $\left.\begin{matrix}Fe\\Ti\end{matrix}\right\}O^3$	197	4·78	4·78
$Bi^2\,O^3$	366	8·09	8·17

Some few of these oxides do not agree, viz. $Mn\,O$, $Sn\,O$, $Mo\,O^2$, and $Pb\,O^2$. For some oxides the atomic volume must be assumed = 16, for others = 64.

Oxides.	Calculated atomic volume.	Calculated specific weight.	Observed specific weight.
$Sb\,O^2$	154	6·53	6·53
$Sn\,O^2$	133	7·03	6·96
$Cr^2\,O^3$	186	5·39	5·21
Atomic volume = 64.			
$Cu^2\,O$	152	5·87	5·75
$Hg^2\,O$	250	10·5	10·69
$Ag\,O$	194	7·48	7·25
$Mo\,O^3$	261	3·44	3·46
$W\,O^3$	261	5·68	5·27

Dr. Kopp then shows the untenability of Schröder's theory with respect to the oxides, inasmuch as he has made an assumption for almost every fact to be explained, and a theory which requires as many or more assumptions than there are facts to be explained is quite useless.

The statements, with regard to the densities of the sulphurets, are very insecure and variable, on account of the great difficulty in obtaining them in a sufficiently pure state. The number of assumptions required is therefore necessarily increased.

Atomic volume of S = 53.				
Sulphuret of nickel	NiS	97	5·86	5·76
Iron pyrites	FeS^2	150	4·94	4·90
Atomic volume = 78.				
Galena	PbS	192	7·78	7·76
Copper glance	Cu^2S	166	5·97	5·74
Molybdenum glance	MoS^2	225	4·45	4·44
Bisulphuret of tin	SnS^2	257	4·43	4·42
Bismuth glance	Bi^2S^3	504	6·47	6·40
Antimony glance	Sb^2S^3	474	4·67	4·63
Atomic volume = 94.				
Sulphuret of copper	CuS	138	4·33	4·16
Manganese glance	MnS	138	3·96	3·95
Sulphuret of zinc	ZnS	152	3·91	3·92
... tin	SnS	195	4·80	4·85
... silver	AgS	224	6·93	6·9
Cinnabar	HgS	187	7·84	8·0
Silver copper glance*	$\left.\begin{matrix}Cu^2\\Ag\end{matrix}\right\}S^2$	406	6·26	6·26
Variegated copper	$\left.\begin{matrix}Fe\\Cu^4\end{matrix}\right\}S^3$	502	5·03	5·00
Copper pyrites	$\left.\begin{matrix}Fe\\Cu\end{matrix}\right\}S^2$	276	4·12	4·16

In this table might also be placed some of the more complex sulphurets, whose calculated specific gravities agree very well with those observed.

For the sulphurets of cadmium and platinum, it appears necessary to adopt 110 as the atomic volume of sulphur.

Arsenic is much more closely allied to the metalloids than to the metals, and it seems that in its acids and sulphurets it has the atomic volume = 119, and not its primitive one; in its compounds with metals and their sulphurets, it seems to require the numbers 74.

In water and in peroxide of hydrogen, the hydrogen seems to possess the atomic volume 80, and oxygen 32, which it has in most metallic oxides.

Dr. Kopp then proceeds to a comparison of the relative merits of the oxyacid and hydracid theories of the constitution of salts, as far, at least, as regards their densities. To enter fully into the reasonings adduced in favour of the hydracid theory would lead us beyond the limits of this abstract: he finds that if the oxyacid theory were retained, a greater num-

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* Isometric copper glance.—*Mohs.*

ber of assumptions would be necessary in the formulæ for the densities of salts than if we adopted the hydracid theory, as the improbability of a theory increases with the number of assumptions which it is obliged to make, and as that theory is the most probable which makes fewest assumptions, Dr. Kopp decides in favour of that of the hydracids. In a future communication we may perhaps enter more fully into this subject.

The densities of several hydrated oxides may be calculated if we assume the water to have the atomic volume 78, and the oxide to retain its primitive volume.

Hydrate of magnesia...	$MgO + H^2O$	159	2·33	2·35
... oxide of tin	$SnO + H^2O$	211	4·96	4·93
Diaspore..................	$Al^2O^3 + H^2O$	242	3·11	3·36
Gibbsite	$Al^2O^3 + 3H^2O$	398	2·46	2·40

In some hydrates of salts the volume of water must be 84.

Chloride of barium ...	$BaCl^2 + 2H^2O$	507	3·00	3·05
Sulphate of lime	$2CaSO^4 + H^2O$	676	2·70	2·76
Gypsum	$CaSO^4 + 2H^2O$	464	2·33	2·33
Sulphate of copper ...	$CuSO^4 + 5H^2O$	700	2·24	2·23
... manganese	$MnSO^4 + 5H^2O$	700	2·15	2·10
... nickel......	$NiSO^4 + 7H^2O$	868	2·03	2·04
... zinc	$ZnSO^4 + 7H^2O$	882	2·03	2·04
... magnesia .	$MgSO^4 + 7H^2O$	864	1·79	1·75

Other salts require the atomic volume 96.

Carb. soda	$NaCO^3 + 10H^2O$	1241	1·44	1·42
	$NaCO^3 + 8H^2O$	1049	1·49	1·51
... lime	$CaCO^3 + 5H^2O$	691	1·73	1·75
Nitrate of copper	$CuN^2O^6 + 3H^2O$	690	2·19	2·17
... magnesia ...	$MgN^2O^6 + 4H^2O$	782	1·78	1·74
Sulphate of iron	$FeSO^4 + 6H^2O$	856	1·89	1·88
... soda.........	$NaSO^4 + 10H^2O$	1426	1·41	1·45

Of the combinations of the metalloids with one another, we may mention those of chlorine. In this group the atomic volume of chlorine = 298. The volume of the metalloid combined with it remains primitive.

Chloride of sulphur ...	$2S + Cl^2$	500	1·69	1·69
	$S + Cl^2$	399	1·61	1·62
... carbon ...	$C + Cl^2$	334	1·56	1·55
	$C + 2Cl^2$	632	1·52	1·59
... cyanogen .	$Cy + Cl^2$	458	1·32	1·32
... phosphorus	$2P + 3Cl^2$	1116	1·54	1·45

Dr. Kopp then goes on to disprove the assumption of Schröder, viz. that the atomic volume with which an element is contained in a compound, bears a simple relation ($\frac{1}{2}$, $\frac{1}{3}$—$\frac{1}{9}$; $\frac{2}{1}$, $\frac{2}{3}$—$\frac{2}{9}$; $\frac{8}{1}$, $\frac{8}{2}$, $\frac{8}{3}$—$\frac{8}{9}$) to its primitive atomic volume.

Sesquioxide of iron, sesquioxide of cobalt, ilmenite, and oxide of chromium are all nearly isomorphous. Their atomic volumes are nearly equal.

$$\left.\begin{array}{lll} Fe^2O^3 \;.\;. & 160 \;.\;. & 182 \\ Co^2O^3 \;.\;. & 185 \;.\;. & 195 \\ \text{Ilmenite} \;. & 197 \;.\;. & 199 \\ Cr^2O^3 \;.\;. & 193 & \end{array}\right\}\text{Calculated from the different densities as given by different observers.}$$

The atomic volumes obtained from the formulæ given above, are—

$$\left.\begin{array}{l} Fe^2O^3 = 184 \\ Co^2O^3 = 184 \\ \text{Ilmenite} = 197 \\ Cr^2O^3 = 186 \end{array}\right\}\text{which agree tolerably well with the preceding values.}$$

According to the atomic theory, in regard to the composition in equivalents, the four above-mentioned oxides are perfectly similar in their composition; but according to the theory of atomic volumes, as regards the manner in which the atomic volumes of the compounds are composed, there is a total dissimilarity.

In the oxides of iron and cobalt, and in ilmenite, the atomic volume of oxygen is 32; in that of chromium it is 16. Oxide of iron and oxide of chromium are isomorphous; they have however not the same constitution in atomic volumes. The sums of the atomic volumes of the elements in both compounds are equal; the atomic volume of oxide of iron is equal to that of oxide of chromium. In the oxide of iron each atom of iron occupies a space = 44; each atom of oxygen a space = 32; in oxide of chromium an atom of metal occupies a space = 69, and an atom of oxygen a space = 16. How can this be explained if these compounds are formed by simple juxtaposition of their elements? The same form can never be obtained by laying together two balls, each of which contains 44 cubic unities (inches, feet, &c.), and three balls of 32 cubic unities contents, as by putting together two balls, containing each 69 cubic unities, with three balls of 16 cubic unities, although $2 \cdot 44 + 3 \cdot 32$ is nearly equal to $2 \cdot 69 + 3 \cdot 16$. Other similar cases might be adduced, and they are altogether contrary to the idea of juxtaposition. Dr. Kopp is almost inclined to assume a penetrability of matter.

It appears that there are cases in which an element, combining with another in several proportions, does not always retain the same atomic volume, although these cases appear to be of less frequent occurrence than those in which it always retains one and the same atomic volume.

The above is a short abstract of Dr. Kopp's most important researches on the specific weight of chemical compounds; at the same time he published another work on the specific gravities of arbitrary mixtures, in which there are some important considerations with regard to the hydracid theory. Should the above abstract meet with the favour of British chemists, I may perhaps be induced to attempt something similar with the last-mentioned work.

January 11th, 1842. HENRY CROFT.
35 Upper Gower Street, London.

XXVIII. *On a great Regularity in the Physical Properties of analogous Organic Compounds.* *By* Dr. HERMANN KOPP, *Lecturer at the University of Giessen*[*].

IN a recent publication on the Specific Weights of Chemical Compounds[†], I endeavoured to show how the atomic volumes of analogous groups of such combinations might be very simply explained by means of certain general admissions, and how the specific weight of any compound belonging to such a group might be determined *à priori* with considerable accuracy. This work was confined to inorganic compounds: I have since then extended my researches to the organic combinations, and have likewise arrived at very simple results.

In organic chemistry there are proportionally more liquid compounds than in the inorganic, and the existence of dimorphism, which frequently renders the discovery of the laws for the specific weight difficult in this latter branch, is of rarer occurrence. The determinations of the densities of fluids are in general easier, and the statements respecting them more accurate than is the case with solid bodies. On the other hand, fluids have a greater expansibility by heat than solids, and as long as it remains unascertained at what temperature a comparison of the specific volumes of various bodies can properly be made, this stronger expansibility, which varies in different compounds, appears to lay great difficulties in the way of our arriving at correct results from a comparison of specific volumes. I have not hitherto been able to devote any special attention to this subject, on account of the several

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[*] Communicated by the Author.

[†] Vide preceding abstract by Mr. H. Croft.

statements respecting the specific weight of one and the same compound frequently differing very considerably, and the uncertainty which this gives rise to is greater than that resulting from our ignorance of the temperature which should be taken as basis when determining the density of any body.

Let us imagine a great number of organic compounds, which may be considered as analogous, arranged under the following scheme:—

$A+\alpha$	$B+\alpha$	$C+\alpha$	$D+\alpha$
$A+\beta$	$B+\beta$	$C+\beta$	$D+\beta$
$A+\gamma$	$B+\gamma$	$C+\gamma$	$D+\gamma$
$A+\delta$	$B+\delta$	$C+\delta$	$D+\delta$

where A, B, C, D, $\alpha, \beta, \gamma, \delta$ express certain bodies or constant combinations of elements.

A, for instance, may represent hypothetical anhydrous acetic acid, B formic acid, C benzoic acid, &c.; α water, β oxide of æthyl, γ oxide of methyl, &c. Or we may conceive by A chlorine, by B iodine, by C sulphur, &c.; and by α hydrogen, by β æthyl, by γ methyl, &c.

It is only necessary to become acquainted with one *horizontal and* one *vertical series, to know the most important physical properties of* all *the combinations contained in such a table. If the properties of the compounds contained in one horizontal or in one vertical series are known, the mere knowledge of one of these compounds is sufficient in order to ascertain the properties of all the compounds arranged in any other horizontal or vertical series.*

The specific weight is given by the specific volume, since the atomic weights* of the compounds are taken as known. If the specific volumes and the boiling points of the compounds $A+\alpha$, $A+\beta$, $A+\gamma$, &c. are known, and we are also acquainted with the specific gravity and boiling point of $B+\alpha$, we ascertain directly the specific gravities and boiling points of the bodies $B+\beta$, $B+\gamma$, $B+\delta$. For between the specific volumes and the boiling points of

$A+\alpha$ and $B+\alpha$	$B+\alpha$ and $D+\alpha$	$A+\alpha$ and $A+\delta$
$A+\beta$ and $B+\beta$	$B+\beta$ and $D+\beta$	$B+\alpha$ and $B+\delta$
$A+\gamma$ and $B+\gamma$	$B+\gamma$ and $D+\gamma$	$C+\alpha$ and $C+\delta$
. . . .	. . . .	. . . .
. . . .	. . . .	. . . .

the differences are always the same.

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* The atomic weight of carbon is taken at 75·854 as found by Liebig and Redtenbacher. [See our last volume, p. 210. Edit.]

This law is confirmed by all known observations. The following specific volumes of the combinations of acetic acid (A.), formic acid (B.), and benzoic acid (C.) with water (α.), oxide of æthyl (β.), and oxide of methyl (γ.), result from the observations which follow:—

	A.		B.		C.	
α....	709	Mollerat.	467	Liebig.	?	
β....	1243	Liebig.	1020	...	1794	Dumas.
γ....	1012	Dumas.	?	...	1558	...

and the observations give the boiling points in centesimal degrees for the same compounds:—

	A.		B.		C.	
α....	120°	Liebig.	99°	Liebig.	239°	Liebig.
β....	74	...	53	...	209	Dumas.
γ....	58	Dumas.	?		198	...

If we designate the specific volumes or the boiling points of the compounds $A+\alpha$, $A+\beta$, $A+\gamma$ .... by $(A+\alpha)$ $(A+\beta)$ $(A+\gamma)$ ..., we find that within the limits of the errors of experiment,

$$(A+\alpha)-(A+\beta) = (B+\alpha)-(B+\beta)$$
$$(A+\beta)-(A+\gamma) = (C+\beta)-(C+\gamma)$$
$$(A+\alpha)-(B+\alpha) = (A+\beta)-(B+\beta)$$
$$(A+\beta)-(C+\beta) = (A+\gamma)-(C+\gamma), \text{ \&c.}$$

This law may be specially applied to single cases and rules advanced for certain compounds which could be proved by numerous observations. I will here enumerate some few cases which frequently occur.

I. "The specific volume of a hydrated acid $(\overline{A}+H^2O)$ is 534 smaller than that of the corresponding æthyl compound $(\overline{A}+Ae\,O)$." To prove this and the following laws, I will give a table of the compounds, containing the formulæ, the atomic weights, the observed specific volumes and densities, and those calculated after the above laws.

In the following table therefore the observations of the hydrated acid have always been placed first, and from the specific volume of the hydrated acid resulting from the observed specific weight, the specific volume of the æthyl compound is obtained by the addition of 534. If the atomic weight of the æthyl compound be divided by this calculated specific volume, we obtain the calculated specific weight, and the agreement between the latter and that observed shows the correctness of the law.

Formula.	Atomic weight.	Specific volume.		Density.		
		Observed.	Calculated.	Calculated.	Observed.	
Hydrate and Æther of Acetic Acid.						
$\overline{A}+H^2O$	753	709	...	...	1·062	Mollerat.
$\overline{A}+Ae\,O$	1107	...	1243	0·8906	0·89	Liebig 15° C.
Hydrate and Æther of Formic Acid.						
$\overline{F}+H^2O$	577	467	...	...	1·2353	Liebig 12°.
$\overline{F}+Ae\,O$	930	...	1001	0·9293	0·912	...
Hydrate and Æther of Succinic Acid.						
$\overline{Su}+H^2O$	741	478	...	...	1·55	Richter.
$\overline{Su}+Ae\,O$	1094	...	1012	1·0812	1·036	D'Arcet.

II. "The specific volume of a hydrated acid ($\overline{A}+H^2O$) is 300 smaller than that of the corresponding methyl compound ($\overline{A}+Me\,O$)."

To test the correctness of this law, let us, as above, calculate the density of the methyl compound from the observed density of the hydrated acid, by adding 300 to the specific volume of the hydrated acid as resulting from observation, and dividing the atomic weight of the methyl compound by this sum, which is the calculated specific volume. The quotient which is the calculated density of the methyl compound may then be compared with that obtained by experiment.

Formula.	Atomic weight.	Specific volume.		Density.		
		Observed.	Calculated.	Calculated.	Observed.	
Sulphate of Water and of Oxide of Methyl.						
SO^3+H^2O	614	332	...	...	1·85	Dalton.
$SO^3+Me\,O$	790	...	632	1·2511	1·324	Dumas 22°.
Nitrate of Water and of Oxide of Methyl.						
$N^2O^5+H^2O$	790	519	...	...	1·522	Mitscherlich.
N^2O^5+MeO	966	...	819	1·1800	1·182	Dumas 22°.
Acetate of Water and of Oxide of Methyl.						
$\overline{A}+H^2O$ ...	753	709	...	...	1·063	Mollerat.
$A+Me\,O$...	930	...	1009	0·9220	0·919	Dumas 22°.

III. "The specific volume of each æthyl compound is 234 greater than that of the corresponding methyl compound."

Formula.	Atomic weight.	Specific volume.		Density.		
		Observed.	Calculated.	Calculated.	Observed.	
Alcohol and Pyroligneous Spirit (Wood-spirit).						
$Ae+H^2O$	578	730	...	...	0·792	Gay-Lus. 18°.
$Me+H^2O$	402	...	496	0·8098	0·798	Dumas 20°.
Sulphurets of Æthyl and Methyl.						
$Ae+S$......	567	687	...	...	0·825	Regnault 20°.
$Me+S$......	390	...	453	0·8610	0·845	... 21°.
Iodides of Æthyl and Methyl.						
$Ae+I^2$ ...	1944	1012	...	...	1·9206	Gay-Lus. 22°.
$Me+I^2$ ...	1767	...	778	2·2712	2·237	Dumas 22°.
Acetates of the Oxides of Æthyl and Methyl.						
$\bar{A}+Ae\,O$...	1107	1244	...	...	0·89	Liebig 15°.
$\bar{A}+Me\,O$...	930	...	1010	0·9213	0·919	Dumas 22°.
Benzoates of the Oxides of Æthyl and Methyl.						
$\overline{Bz}+Ae\,O$	1890	1793	...	...	1·0539	Dumas 10°.
$\overline{Bz}+Me\,O$	1713	...	1559	1·0987	1·1	... 17°.
Suberates of the Oxides of Æthyl and Methyl.						
$\overline{Su}+Ae\,O$	1348	1329	...	...	1·014	Laurent 18°C.
$\overline{Su}+Me\,O$	1171	...	1095	1·0694	1·003	... 18 C.
Mucates of the Oxides of Æthyl and Methyl.						
$\overline{Mu}+Ae\,O$	1671	1266	...	...	1·32	Malaguti.
$\overline{Mu}+Me\,O$	1494	...	1032	1·4480	1·48	...
Hydrates of Acetic and Formic Acids.						
$Ac+O^4-H^2$	753	709	...	...	1·063	Mollerat.
$Me+O^4-H^2$	577	...	475	1·2148	1·2353	Liebig 12°.

In this manner a number of laws which are confirmed by experience, may be advanced for numerous compounds, indeed for all analogous combinations. Thus the specific volumes of all acetates are 234 greater than those of the corresponding formates; the specific volumes of the benzoates are 548 greater than those of the corresponding acetates; the specific volumes of all the succinates are 34 greater than those of the corresponding formates; the specific volumes of all the mucates are 538 greater than those of the corresponding benzoates, &c.

Let us compare the compounds of valerianic acid with the corresponding ones of acetic acid. The specific volume of the hydrated acetic acid is 709; the specific volume of the hydrated valerianic acid (spec. weight = 0·944 Trommsdorff, atomic weight = 1738) is 1842, that is, 1133 greater than that of the hydrated acetic acid. If we have studied the compounds of acetic acid we may then deduce the specific volumes and densities of the corresponding valerianates; the specific volume of any compound of valerianic acid is 1133 greater than that of acetic acid. The specific volume of acetic æther is 1243, that of valerianic æther must therefore be 1243 + 1133 = 2376. The density therefore is equal to the atomic weight 2098 divided by 2376 = 0·833. Otto found it to be 0·894.

Similar laws might be adduced with reference to the boiling points, *e. g.* if we neglect the small differences resulting from the varying states of the barometer, the boiling points of all the compounds of æthyl are 18° C. higher than those of the corresponding methyl compounds within the limits of the errors of experiment.

Formulæ.	Observed boiling point C.		Differences.
$\mathrm{Ae\,O+H^2\,O}$	78°·4	Gay-Lussac.	18°·4
$\mathrm{Me\,O+H^2\,O}$	60	Kane.	
$\mathrm{Ae+I^2}$	64 ·8	Mitscherlich.	24 ·8—14·8
$\mathrm{Me+I^2}$	40—50	Dumas.	
$\mathrm{Ae\,S+H^2\,S}$	36 ·2	Liebig.	15 ·2
$\mathrm{Me\,S+H^2\,S}$	21	Gregory.	
$\underline{\dddot{\mathrm{C}}}+\mathrm{Ae\,O}$	184	Dumas.	23
$\underline{\dddot{\mathrm{C}}}+\mathrm{Me\,O}$	161	Dumas.	
$\overline{\mathrm{A}}+\mathrm{Ae\,O}$	74	Liebig.	16
$\overline{\mathrm{A}}+\mathrm{Me\,O}$	58	Dumas.	
$\overline{\mathrm{Bz}}+\mathrm{Ae\,O}$	209	Dumas.	11
$\overline{\mathrm{Bz}}+\mathrm{Me\,O}$	198	Dumas.	
$\mathrm{Ae+O^4-H^2}$	120	Liebig.	21 ·5
$\mathrm{Me+O^4-H^2}$	98·5	Liebig.	

Moreover, the boiling points of all the hydrated acids are 45° C. higher than those of the corresponding æthers, and 63° C. higher than the corresponding compounds of methyl; the boiling points of all the oxalates are 106° higher than those of the acetates; the boiling points of the benzoates 148° C. higher than those of the formates; the boiling points of the succinates

128° C. higher than those of the acetates; the boiling points of the acetates 18° higher than those of the corresponding formates, &c.

The accurate study of the physical properties of all the compounds of one body and of any single compound of another, enables us to form a conclusion with respect to all other compounds of the latter, and affords a control for the observations in general.

A similar regularity occurs in the physical properties in such cases where substitutions occur. Many organic compounds may be so arranged in a series that the amount of hydrogen decreases successively, while that of chlorine increases, which is explained by the supposition of the substitution of chlorine in the place of hydrogen. Let A represent, for instance, a combination of oxygen and carbon which remains unaltered, α hydrogen, β chlorine, m, n, x, y numbers, we then have the following scheme:—

$$\begin{array}{ll} A + m\,\alpha & + \; n\,\beta \\ A + (m-x)\,\alpha & + \; (n+x)\,\beta \\ A + (m-y)\,\alpha & + \; (m+y)\,\beta. \end{array}$$

The number of atoms α and β taken together remains the same in each compound.

Compounds belonging to such a group or scheme may be arranged in the table given at the commencement of this paper, and the laws which have there been communicated may also be applied to them. But we may also consider independently a whole series of such compounds where chlorine and hydrogen replace each other, and prove specially for them a peculiar regularity in their physical properties.

With reference to the specific volumes of such compounds, the following law holds good:—

"If in any compound x atoms of hydrogen are replaced by (x atoms of chlorine), the specific volume of the new compound is greater than that of the former by the number $x.\,80$."

[The number 80 is merely approximate, inasmuch as no attention has been paid to the temperature at which the specific volumes should be considered.]

In proof of this law I will arrange together a large number of substitutions, and always calculate by the above law from the observed density of the preceding compound the density of that which has been formed from it by substitution, and compare the calculations with the observations.

Formula.	Atomic weight.	Specific volume.		Density.		
		Obs.	Calculated.	Observed.	Calcul.	
Substitution of Chlorine in Hydruret of Benzoyl.						
$C^{14} H^{12} O^{2}$	1337	1282	... ...	...	1·043	Liebig & Wöhler.
$C^{14} H^{10} O^{2} Cl^{2}$	1767	...	1282+2·80=1442	1·225	1·196	... 18° C.
Substitution of Chlorine in Oxide of Methyl.						
$C^{2} H^{4} Cl^{2} O$	719	547	... ...	...	1·315	Regnault 20°.
$C^{2} H^{2} Cl^{4} O$	1149	...	547+2·80=707	1·626	1·606	... ...
Substitution of Chlorine in Acetate of the Oxide of Methyl.						
$C^{6} H^{12} O^{4}$ ...	930	1012	... ...	...	0·919	Dumas & Peligot 22°.
$C^{6} H^{8} Cl^{4} O^{4}$	1790	...	1012+4·80=1332	1·344	1·261	Malaguti 16°.
Substitution of Chlorine in Oxide of Æthyl.						
$C^{4} H^{10} O$......	466	643	... ...	...	0·724	Gay-Lussac 12°.
$C^{4} H^{6} Cl^{4} O$	1326	...	643+4·80=963	1·377	1·501	Malaguti.
Substitution of Chlorine in Acetic Æther.						
$C^{8} H^{16} O^{4}$ ...	1107	1244	... ...	...	0·89	Liebig 15°.
$C^{8} H^{12} Cl^{4} O^{4}$	1967	...	1244+4·80=1564	1·258	1·301	Malaguti 12°.
Substitution of Chlorine in Formic Æther.						
$C^{6} H^{12} O^{4}$ ...	930	1020	... ...	...	0·912	Liebig.
$C^{6} H^{8} Cl^{4} O^{4}$	1790	...	1020+4·80=1340	1·336	1·261	Malaguti 16°.
Substitution of Chlorine in Camphoric Æther.						
$C^{14} H^{24} O^{4}$ ..	1612	1566	... ...	...	1·029	Malaguti 16°.
$C^{14} H^{20} Cl^{4} O^{4}$	2472	...	1566+4·80=1886	1·311	1·386	... 14°.
Substitution of Chlorine in Benzol (Benzin).						
$C^{12} H^{12}$	985	1159	... ...	...	0·85	Mitscherlich.
$C^{12} H^{6} Cl^{6}$...	2276	...	1159+6·80=1639	1·389	1·457	... 7°.
Substitution of Chlorine in Aldehyd.						
$C^{4} H^{8} O^{2}$......	553	700	... ...	...	0·79	Liebig 18°.
$C^{4} H^{2} Cl^{6} O^{2}$	1844	...	700+6·80=1180	1·563	1·502	... ...
Substitution of Chlorine in Hydrated Acetic Acid.						
$C^{4} H^{8} O^{4}$......	753	709	... ...	...	1·063	Mollerat.
$C^{4} H^{2} Cl^{6} O^{4}$	2044	...	709+6·80=1189	1·719	1·617	Dumas 64°.
Substitution of Chlorine in Œnanthic Æther.						
$C^{18} H^{36} O^{3}$ ...	1890	2193	... ...	...	0·862	Liebig & Pelouze.
$C^{18} H^{28} Cl^{8} O^{3}$	3612	...	2193+8·80=2833	1·275	1·291	Malaguti 16°.
Substitution of Chlorine in Sulphuret of Æthyl.						
$C^{4} H^{10} S$......	567	687	... ...	...	0·825	Regnault 20°.
$C^{4} H^{2} Cl^{8} S$...	2288	...	687+8·80=1327	1·724	1·673	... 24°.
Substitution of Chlorine in Chloride of Methyl.						
$C^{2} H^{4} Cl^{4}$ ...	1062	790	... ...	...	1·344	Regnault 18°.
$C^{2} H^{2} Cl^{6}$......	1492	...	790+2·80=950	1·571	1·480	Liebig 18°.
$C^{2} Cl^{8}$	1922	...	790+4·80=1110	1·731	1·599	Regnault 1°.

Where several substitutions occur successively, it is requisite to calculate from each of the compounds examined the density of the other, so that any accidental error of experiment may not be allowed to have too great an influence. To be as precise as possible, I will here take for calculation two of the best-examined compounds, one containing the greatest number of atoms of hydrogen, and the other the greatest number of atoms of chlorine.

Substitution of Chlorine in Chloride of Æthyl.

If we calculate from the combination $C^4 H^{10} Cl^2$,

Formula.	Atomic weight.	Specific volume.		Density.		
		Obs.	Calculated.	Observed.	Calcul.	
$C^4 H^{10} Cl^2$	808	925	... ...	...	0·874	Thenard 5°.
$C^4 H^8 Cl^4$	1239	...	925+ 2·80=1085	1·142	1·174	Regnault 17°.
$C^4 H^6 Cl^6$	1669	...	925+ 4·80=1245	1·341	1·372	... 16°.
$C^4 H^4 Cl^8$	2099	...	925+ 6·80=1405	1·494	1·530	... 17°.
$C^4 H^2 Cl^{10}$	2529	...	925+ 8·80=1565	1·616	1.644	... 17°.
$C^4 Cl^{12}$	2959	...	925+10·80=1725	1·715	nearly 2	Faraday.

If we calculate from the compound $C^4 H^2 Cl^{10}$,

Formula.	Atomic weight.	Obs.	Calculated.	Observed.	Calcul.	
$C^4 H^2 Cl^{10}$	2529	1538	... ...	...	1·644	Regnault.
$C^4 H^{10} Cl^2$	808	...	1538− 8·80= 898	0·900	0·874	Thenard 5°.
$C^4 H^8 Cl^4$	1239	...	1538− 6·80=1058	1·171	1·174	Regnault 17°.
$C^4 H^6 Cl^6$	1669	...	1538− 4·80=1218	1·370	1·372	... 16°.
$C^4 H^4 Cl^8$	2099	...	1538− 2·80=1378	1·523	1·530	... 17°.
$C^4 Cl^{12}$	2959	...	1538+ 2·80=1698	1·743	nearly 2	Faraday.

From this table it becomes evident that the law above advanced is generally confirmed by experiment. However, the calculated densities frequently differ more from those observed than was the case for the other laws. This arises from various circumstances.

It is very difficult to obtain by substitution a pure compound, one which is not rendered impure by the presence of another preceding or following degree of substitution. This uncertainty in the preparation of the compounds, to which Berzelius has drawn particular attention, is the cause that the statement of various observers are often in contradiction. In the 41st volume of the *Annalen der Chemie und Pharmacie*, I have treated at length of the uncertainty and contradiction of the observations on such compounds, and have shown that the differences between the observations are not smaller than those between the calculated and the observed magnitudes.

Another reason why the calculation does not always agree accurately with the experiment, is that the temperatures at which the comparison between the specific volumes should properly be made, have always been neglected. Without doubt such a comparison is, strictly speaking, only then admissible when the temperatures are equidistant from the boiling points; but all observations have been made at mean temperature, and this accounts for the differences between the results of calculation and those of observation. On this account all the numbers in the above laws must be regarded as being merely approximative.

The less the boiling points of the corresponding compounds differ from each other, the greater will be the agreement of the calculated results with those of observation. Since the corresponding compounds of æthyl and methyl differ only by 18° C. in their boiling points, the neglecting the circumstance of at what temperature the specific weights should be compared, has very slight influence in the consideration of these compounds; but where it is a question of replacement of hydrogen by chlorine it is totally different, as the boiling point rises rapidly with the increase of chlorine.

Unfortunately, the observations on the boiling points of substitution compounds are far more contradictory than those on their specific weights, and the uncertainty is too great to allow us at present to state with accuracy how many degrees the boiling point rises when x atoms of chlorine take the place of x atoms of hydrogen.

The best observations seem however to indicate, that for substitution compounds, which can be compared with regard to their condensation or specific weight in the gaseous form, the boiling point is raised $x \,.\, 12^\circ$ C. by the substitution of x atoms of chlorine for x atoms of hydrogen. Let D be the number of degrees the boiling point is raised by the substitution of one atom of chlorine for one of hydrogen, we have from the best observations,—

Hydrated acetic acid and chloro-acetic acid,

$$\left.\begin{array}{lll} C^4\,H^8\,O^4 & 120 & \text{Liebig} \\ C^4\,H^2\,O^4Cl^6 & 195 & \text{Dumas} \end{array}\right\} 6\,D = 75^\circ;\; D = 12^\circ{\cdot}5.$$

Aldehyd and chloral,

$$\left.\begin{array}{lll} C^4\,H^8\,O^2 & 21{\cdot}8 & \text{Liebig} \\ C^4\,H^2\,Cl^6\,O^2 & 94 & \ldots \end{array}\right\} 6\,D = 72{\cdot}2;\; D = 12^\circ{\cdot}0.$$

Several other observations, which however I do not regard as perfectly admissible, and do not consider as fit for the establishment of the law, agree in this respect. If we calculate for several substitution compounds the boiling points of all the other compounds from that which has the lowest boiling point, and compare them with those afforded by observation, we obtain the following table:—

	Boiling point C. Calculated.	Observed.	
$C^4 H^{10} S$	....	73	Regnault.
$C^4 H^2 Cl^8 S$	$73+8\cdot12=169$	160	...
$C^2 H^4 Cl^4$	....	30·5	...
$C^2 H^2 Cl^6$	$30\cdot5+2\cdot12=54\cdot5$	60·8	...
$C^2 Cl^8$	$30\cdot5+4\cdot12=78\cdot5$	78·0	...

But, as I have already stated, too much uncertainty prevails respecting the boiling points to allow of our determining the numbers in a law with absolute accuracy: the existence of a law of the form which has been proposed is beyond doubt.

When the specific volumes of a body are known for all temperatures, those of an analogous compound always differ by a constant magnitude, and the knowledge of the specific weight, and of the expansion of the first and of the specific weight of the latter for any temperature, consequently gives the expansion of the latter.

It is scarcely necessary to call attention to the importance of these laws; they are for the determination of the physical properties of chemical compounds what the law of definite proportions is for the knowledge of their constitution.

XXIX. *On the Constitution of the Atmosphere.* By James Ivory, *K.H., M.A., Hon. M.R.I.A., Instit. Reg. Sc. Paris, et Reg. Soc. Götting. Corresp.**

IT is known that atmospheric air is a mixture of several gases and of aqueous vapour. The quantity of aqueous vapour amounts, at a maximum, only to an inconsiderable proportion of the whole volume, and often to so small a proportion as to be insensible in its effect. We may therefore, for the sake of simplicity, suppose, in the first place, that the air of the atmosphere contains no vapour of water, or is in a dry state. The constituent gases of air, as far as they have been estimated, are three, oxygen, azote, and carbonic acid, but as this last never exceeds $\frac{1}{1000}$th of the mixture, we may neglect it and consider air as containing only oxygen and azote.

Let us now take a portion of oxygen, one of azote, and one of atmospheric air; p and θ representing the pressure and temperature common to all the three fluids; and their densities and volumes being respectively ϱ, v; ϱ', v'; and R, V. We shall have, by the law of Mariotte,

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* Communicated by the Author.

$$\left.\begin{aligned} p &= k\varrho\ (1+\alpha\theta), \\ p &= k'\varrho'\ (1+\alpha\theta), \\ p &= \mathrm{K}\,\mathrm{R}\,(1+\alpha\theta): \end{aligned}\right\} \ldots\ldots\ldots\ldots (1.)$$

whence we deduce

$$\mathrm{K}\,\mathrm{R} = k\varrho = k'\varrho';\ \varrho = \frac{\mathrm{K}}{k}\,\mathrm{R},\ \varrho' = \frac{\mathrm{K}}{k'}\,\mathrm{R}.$$

The equations (1.) show that R, ϱ, ϱ' stand for the respective densities of air, oxygen, and azote, under the same pressure p and the same temperature θ: but it has been determined experimentally that the three densities in such circumstances are proportional to the numbers 1, 1·1057, 0·972: wherefore, putting $\mathrm{A} = 1{\cdot}1057$, $\mathrm{A}' = 0{\cdot}972$*, we shall have

$$\varrho = \mathrm{A}\,\mathrm{R}, \qquad \frac{\mathrm{K}}{k} = \mathrm{A}$$

$$\varrho' = \mathrm{A}'\,\mathrm{R}, \qquad \frac{\mathrm{K}}{k'} = \mathrm{A}'.$$

Multiply the densities by the respective volumes, and take the sum of the products; the result will be

$$\varrho\,v + \varrho' v' = (\mathrm{A}\,v + \mathrm{A}'\,v')\,\mathrm{R}.$$

Now $\varrho\,v + \varrho'\,v'$ is the sum of the masses of the oxygen and azote; and we may assume that this sum is equal to R V the mass of the atmospheric air; in consequence, by substituting and leaving out the common factor R, we shall obtain

$$\mathrm{V} = \mathrm{A}\,v + \mathrm{A}'v'.$$

This equation is not sufficient for determining v and v' when a given value is assigned to V: we may therefore assume this other condition, that $v+v'$ is equal to V, viz.

$$\mathrm{V} = v + v'.$$

By means of the two equations, the values of v and v' will be found, when V is given: thus $\mathrm{V} = 100$, $v = 20{\cdot}9$, $v' = 79{\cdot}0$. The conclusion is, that a volume of 21 parts of oxygen added to a volume of 79 parts of azote, the pressure and temperature of both gases being the same, make 100 parts of atmospheric air under the same pressure and temperature.

The subject of mixed gases has been greatly perplexed by conjectures about the question, whether particles of the different fluids in a mixture do, or do not, act mutually upon one another. The difference of opinion on this point has been carried so far, that some eminent chemists have had recourse to a chemical combination of the gases, supposing that air consists of atoms formed by the union of two atoms of

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* These numbers are taken from the *Comptes Rendus*, June 7, 1841.

azote with one atom of oxygen. According to the view of the matter we have here taken, there is no such thing as an atom of air: for a volume of that fluid contains the atoms in 21 parts of oxygen and 79 parts of azote, the atoms of each of the two gases being separately arranged in symmetrical order through the whole volume of air. The difficulty alluded to is completely solved when it is observed that the action of any one of the gases at a point of the common surface is transmitted to all the other gases; by which means the equilibrium of the mixture is established, not by the action of the particles, but by the mutual action of the elasticities of the fluids.

The method of investigation followed above, has the advantage of proving that the resolution of air into its elementary gases is a necessary consequence of the law of Mariotte: for the three equations (1.) are applications of that law to the different gases. But the elements of which air is composed might have been deduced from the theory of mixed gases, as explained in the last Number of this Journal: and it may be no improper addition to show that two trains of reasoning which appear so little connected, lead nevertheless to the same result. The common temperature being θ, let p, ϱ, υ denote the pressure, density and volume of a portion of oxygen, and p', ϱ', υ' the like quantities of a portion of azote: if the two fluids be introduced into an envelop, the volume of which is $V = \upsilon + \upsilon'$, it is shown that the elasticity of the mixture, that is, the pressure at every point of the envelop and the elastic force which keeps in its place every particle within the envelop, is equal to

$$p \cdot \frac{\upsilon}{V} + p' \cdot \frac{\upsilon'}{V}:$$

and if we suppose $p = p'$, the same elastic force will be simply p. Let R denote the density of atmospheric air under the pressure p and temperature θ: then, as observed before, R, ϱ, ϱ' will be proportional to the numbers 1, A, A'; so that

$$\varrho = A\,R,$$
$$\varrho' = A'\,R,$$
$$\varrho\,\upsilon + \varrho'\,\upsilon' = (A\,\upsilon + A'\,\upsilon') \,.\, R.$$

Now $\varrho\,\upsilon + \varrho'\,\upsilon'$ is the sum of the masses of the constituent parts of the envelop: wherefore, V being the volume of the envelop, if D represent the density of the mixture, we shall have

$$\varrho\,\upsilon + \varrho'\,\upsilon' = V\,D$$
$$V\,D = (A\,\upsilon + A'\,\upsilon')\,R.$$

Further, we may assume

$$V = A\,\upsilon + A'\,\upsilon';$$

because this assumption conjoined with the condition,

$$V = \upsilon + \upsilon'$$

will determine υ and υ', when V is given: thus $V = 100$; $\upsilon = 20{\cdot}9$, $\upsilon' = 79{\cdot}0$. From the foregoing formulas we likewise obtain $D = R$; that is, the density of the mixture under the pressure p and temperature θ is equal to the density of atmospheric air under the same pressure and density; which proves that the mixture can be no other than atmospheric air.

What has been demonstrated as necessary consequences of the physical properties of the gases agrees with the results of many laborious experiments undertaken for the purpose of resolving air into its constituent elements. Whatever be the local circumstances in which air is collected, it is found, on being analysed, to consist in volume of 21 parts of oxygen and 79 parts of azote. This is true not only at the earth's surface, but on the tops of elevated mountains situated at great distances from one another, and even at the greatest height to which man has been able to ascend in the atmosphere, as is proved by the analysis of the air brought down by Gay-Lussac in his aëronautic ascent from a height of about 6400 yards.

Another observation it is important to make. The law of Mariotte has been verified in the case of atmospheric air, between very wide limits of pressure and temperature: and as it follows from what has been shown, that this law, and the formation of air by the combining of two gases in a constant proportion, are reciprocally consequences of one another, we have two independent experimental researches confirmatory of those co-existing properties of air. It appears, therefore, that there is good evidence to conclude that every portion of the atmosphere from the earth's surface to the top, is composed of two volumes of oxygen and azote in the proportion of 21 to 79, and that the law of Mariotte is applicable at all heights.

The atmosphere at any two points on the earth's surface, being composed of the same elements combined in the same proportion, can differ from one another only on account of the heat derived from the earth or from other sources. The effect of such heats is not to alter the aërial fluid in its constitution, but merely to dilate it, and to vary the height at which a given temperature will prevail in the column of air. It is therefore necessary to investigate the law according to which the heat decreases in ascending, in order to complete our knowledge of the atmosphere. Little progress has been made in this research. The measurement of small heights leads to results that cannot be depended on; because a minute error of the thermometer is accompanied with a great variation of the elevation. Even in great heights, the discrepancies are so great as to warrant no more than this approximate conclusion, that the decrease of heat near the earth's surface is proportional to the increase of elevation. By careful experiments the rate of the decrease of heat at any place, that is, the height required for depressing the thermometer 1°, may be ascertained with considerable accuracy; and it has been found that this imperfect element is sufficient for solving the two chief problems relating to the atmosphere, namely, the measuring of heights by the barometer, and the astronomical refractions.

XXX. *On the Propagation of Luminous Waves in the Interior of Transparent Bodies. By the Rev.* MATTHEW O'BRIEN, *late Fellow and Mathematical Lecturer of Caius College, Cambridge*[*].

ONE of the chief difficulties we meet with in applying the general equations of motion to determine the circumstances of propagation of the waves of light in the interior of transparent bodies, arises from our ignorance of the manner in which the particles of the luminous æther are arranged in the interstices between the particles of matter; for, supposing that each particle of matter is surrounded by many particles of æther, it is evident that the attractions or repulsions of the material particles on any æthereal particle must in general be different according to the different positions which that particle may occupy with reference to the material particles; now the consequence of this variation of force must evidently be an unequal arrangement of the particles of the æther; so that if we consider any one particle, the rest will in general be disposed unsymmetrically with respect to it, according to some law varying with the position it occupies with respect to the particles of matter; and this law of course we cannot determine, as it must depend on the nature of the molecular forces, both material and æthereal, of which we are quite ignorant.

Now when we come to form the approximate differential equations which determine the vibratory motion of the particles, we shall find that the coefficients of the partial differential coefficients with respect to $x\,y\,z$, which occur in these equations, become variable in consequence of the variable arrangement of the particles just described; and hence, instead

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[*] Communicated by the Author.

of linear differential equations with constant coefficients, we shall have linear differential equations with variable coefficients, of which we do not even know the law of variation. Thus it appears almost hopeless to attempt making any use of the general equations of motion as applied to determine the circumstances of propagation of luminous waves in the interior of transparent bodies, supposing each material particle surrounded by many particles of æther.

But if we adopt a different hypothesis and suppose that there are not so many, or *at most* as many, particles of æther as there are particles of matter in the transparent body, then it is evident that the particles of æther will be arranged with respect to each other in a perfectly symmetrical manner (supposing of course that the same is true with respect to the particles of the transparent body, *i. e.* supposing it to be perfectly homogeneous and uncrystallized); this will appear immediately, without any further explanation, from the following figures.

Fig. 1.

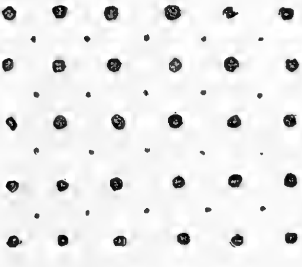

Fig. 2.

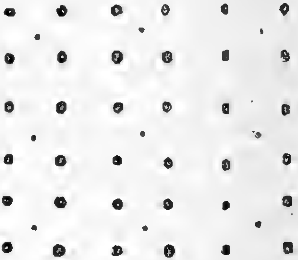

Fig. 3.

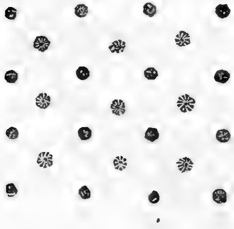

Fig. 4.

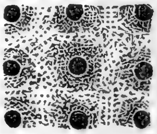

Fig. 1 represents what we may conceive to be the arrangement of the particles of æther and matter*, supposing that there are fewer of the former than of the latter, the larger dots representing the particles of matter and the smaller those of æther.

Fig. 2 represents the case where there are just as many particles of æther as of matter.

Fig. 3 represents a case (and, as we shall hereafter show, a very probable case) in which there are more æthereal than material particles, and yet the circumstances are exactly the same as in fig. 2: it is that case in which the particles of matter exercise so strong a repulsive force on the æthereal particles as to gather them into globules, which may be regarded as each one particle.

Fig. 4 represents the case first described, in which each particle of matter is surrounded by many particles of æther, which are not gathered into globules as in fig. 3, but spread over the whole space between the particles of matter.

It is evident from these figures, that we have two very different cases to consider in applying the equations of motion to the vibrations of æther as it exists in the interior of transparent bodies: 1st, that case which is represented by figures 1, 2, and 3, in which (as will be shown) the equations of motion will be linear equations with *constant* coefficients; and 2ndly, that case which is represented by fig. 4, in which the equations of motion will be linear equations with *variable* coefficients. My object in the present investigation is to examine the circumstances of propagation of waves in both these cases, confining myself entirely to the consideration of plane waves propagated with a uniform velocity, both for the sake of simplicity and because similar results to those deduced in the case of plane waves may be obtained in a similar manner in the case of any other species of waves.

The following is a brief outline of the course I have pursued and the results I have arrived at.

In the first place, I have investigated the general equations of vibratory motion of a system of material particles surrounded by æthereal particles.

I have then simplified these equations by assuming that the arrangement of the æthereal particles is such as is represented in figures 1, 2, or 3, and applied the equations thus simplified to the case of plane waves propagated with a uniform velocity.

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* We here suppose the particles of matter to be placed at the corners of cubes; but our reasoning will be equally true for any other arrangement of the particles, provided it be perfectly symmetrical and homogeneous.

I have then deduced the following results:—

1st. That the velocity of propagation is in general *different for transversal and for direct vibrations*, and that consequently any arbitrary disturbance will give rise to two waves, propagated with different velocities, one consisting of transversal and the other of direct vibrations.

2ndly. That plane waves *cannot* be propagated with a uniform velocity, *unless the particles vibrate according to the cycloidal law.*

3rdly. That if υ be the velocity of propagation, and λ the length of the wave, then there is the following relation between υ and λ (*quite independently of the hypothesis of finite intervals*), viz.

$$\frac{4\pi^2}{\lambda^2} = \frac{C}{\upsilon^2 - B},$$

supposing the particles of matter absolutely fixed in space; and

$$\frac{4\pi^2}{\lambda^2} = \frac{C}{\upsilon^2 - B} + \frac{C_{\prime}}{\upsilon^2 - B_{\prime}},$$

supposing the particles of matter capable of motion (as they must be).

In these formulæ B is a certain constant depending on the law of force of one particle of æther on another; $B_{\prime}$ a similar constant with reference to particles of matter; and C $C_{\prime}$ two constants depending on the mutual action of matter and æther on each other. *B and $B_{\prime}$ are not the same for transversal and direct vibrations.*

These results show that the dispersion of light may be completely accounted for without having recourse to the hypothesis of finite intervals. Indeed a more general value of λ than that just given may be obtained in the following form, viz.

$$\frac{4\pi^2}{\lambda^2} = \frac{C}{\upsilon^2 - B} + \frac{C_{\prime}}{\upsilon^2 - B_{\prime}} + \frac{C_2}{\upsilon^2 - B_2} + \frac{C_3}{\upsilon^2 - B_3} + \&c.$$

by supposing that the particles of transparent bodies are compound (as they must be in many cases), consisting of several essentially different atoms, there being a term in the above equation for each different atom. Of course with such a relation as this we may make the different corresponding values of μ and λ agree with their actual values obtained by observation as nearly as we please.

Lastly. I have shown that, though it appears at first sight almost impossible to solve the differential equations in the case represented by fig. 4, yet there is a peculiar circumstance in the case of luminous waves which enables us to get over this difficulty and arrive at exactly the same results as those just mentioned; *so that these results are true in general whatever be the arrangement of the luminous particles in the interstices between the particles of matter.*

Formation of the Equations of Vibratory Motion.

Let us adopt the following notation, viz. let

$$\left.\begin{array}{l}x\ y\ z \text{ be the coordinates of any particle of æther,}\\ x_{,}\,y_{,}\,z_{,} \text{ be the coordinates of any particle of matter,}\end{array}\right\}\text{when in a state of equilibrium;}$$

$$\left.\begin{array}{l}x+\delta x\ \ y+\delta y\ \ z+\delta z \text{ be the coordinates of any particle of æther,}\\ x_{,}+\delta x_{,}\ \ y_{,}+\delta y_{,}\ \ z_{,}+\delta z_{,} \text{ be the coordinates of any particle of matter,}\end{array}\right\}\text{near the former in a state of equilibrium;}$$

$$\left.\begin{array}{l}x+\alpha\ \ y+\beta\ \ z+\gamma\ \ \ldots\ldots\ldots\\ x_{,}+\alpha_{,}\ \ y_{,}+\beta_{,}\ \ z_{,}+\gamma_{,}\ \ \ldots\ldots\ldots\\ x+\alpha+\delta x+\delta\alpha\ \ y+\beta+\delta y+\delta\beta\ \ z+\gamma+\delta z+\delta\gamma\\ x_{,}+\alpha_{,}+\delta x_{,}+\delta\alpha_{,}\ y_{,}+\beta_{,}+\delta y_{,}+\delta\beta_{,}\ z_{,}+\gamma_{,}+\delta z_{,}+\delta\gamma_{,}\end{array}\right\}\text{the coordinates of the same particles respectively at any time } t \text{ during the motion.}$$

Let us for brevity put

$$\delta x^2+\delta y^2+\delta z^2=r^2$$

$$\delta x_{,}^2+\delta y_{,}^2+\delta z_{,}^2=r_{,}^2$$

$$(\delta x+\delta\alpha)^2+(\delta y+\delta\beta)^2+(\delta z+\delta\gamma)^2=(r+\rho)^2$$

$$(\delta x_{,}+\delta\alpha_{,})^2+(\delta y_{,}+\delta\beta_{,})^2+(\delta z_{,}+\delta\gamma_{,})^2=(r_{,}+\rho_{,})^2$$

$$(x-x_{,})^2+(y-y_{,})^2+(z-z_{,})^2=r'^2$$

$$(x+\alpha-x_{,}-\alpha_{,})^2+(y+\beta-y_{,}-\beta_{,})^2+(z+\gamma-z_{,}-\gamma_{,})^2=(r'+\rho')^2.$$

Also let the attraction at the time t of

A particle of æther on a particle of æther be $m(r+\rho)f(r+\rho)$
... matter ... æther $m_{,}(r'+\rho')\phi(r'+\rho')$
... æther ... matter $m(r'+\rho')\phi(r'+\rho')$
... matter ... matter $m_{,}(r_{,}+\rho_{,})\psi(r_{,}+\rho_{,})$.

Hence we evidently have the following equation for the motion parallel to the axis of x of any particle of æther, viz.

$$\frac{d^2\alpha}{dt^2}=\Sigma\left\{mf(r+\rho)(\delta x+\delta\alpha)+m_{,}\phi(r'+\rho')(x_{,}-x+\alpha_{,}-\alpha)\right\}.$$

We shall suppose the relative motion of any two particles within the sphere of mutual attraction or repulsion to be very small compared with the distance between them; hence α $\delta\alpha$ $\alpha_{,}$ ρ ρ' are very small compared with δx $(x_{,}-x)$ r r': we have therefore approximately,

$$\frac{d^2 \alpha}{d t^2} = \Sigma \Big\{ m \left(f(r) + f'(r)\rho\right)(\delta x + \delta \alpha) + m_{,}\left(\phi(r') \right.$$
$$\left. + \phi'(r')\rho'\right)(x_{,} - x + \alpha_{,} - \alpha) \Big\}$$
$$= \Sigma \left\{ m\left(f(r)\,\delta \alpha + f'(r)\,\rho\,\delta x\right) + m_{,}\left(\phi(r)(\alpha_{,} - \alpha) \right.\right.$$
$$\left.\left. + \phi'(r')\,\rho'\,(x_{,} - x)\right)\right\},$$

observing that by the condition of previous equilibrium we have

$$\Sigma \left\{ m f(r)\,\delta x + m_{,}\,\phi(r')\,(x_{,} - x)\right\} = 0.$$

Now we evidently have approximately,

$$\rho = \frac{1}{r}\left(\delta x\, \delta\alpha + \delta y\, \delta \beta + \delta z\, \delta \gamma\right),$$

and $\rho' = \frac{1}{r'}\left((x_{,} - x)(\alpha_{,} - \alpha) + (y_{,} - y)(\beta_{,} - \beta) + (z_{,} - z)(\gamma_{,} - \gamma)\right).$

Hence our equation becomes

$$\frac{d^2 \alpha}{d t^2} = \Sigma m \Big\{ f(r)\,\delta \alpha + \frac{1}{r'} f'(r)\left(\delta x^2 . \delta \alpha + \delta x\, \delta y . \delta \beta \right.$$
$$\left. + \delta x\, \delta z . \delta \gamma\right) \Big\}$$
$$+ \Sigma m_{,} \Big\{ \phi(r')(\alpha_{,} - \alpha) + \frac{1}{r'}\,\phi'(r')\left((x_{,} - x)^2 (\alpha_{,} - \alpha) \right.$$
$$\left. + (x_{,} - x)(y_{,} - y)(\beta_{,} - \beta) + (x_{,} - x)(z_{,} - z)(\gamma_{,} - \gamma)\right) \Big\};$$

and similarly we shall obtain equations for

$$\frac{d^2 \beta}{d t^2}\ \frac{d^2 \gamma}{d t^2}\ \frac{d^2 \alpha_{,}}{d t^2}\ \frac{d^2 \beta_{,}}{d t^2}\ \frac{d^2 \gamma_{,}}{d t^2}.$$

We shall now substitute in these equations for $\delta \alpha$ its value

$$\frac{d \alpha}{d x}\delta x + \frac{d \alpha}{d y}\delta y + \frac{d \alpha}{d z} d z + \frac{d^2 \alpha}{d x^2}\frac{\delta x^2}{2} + \frac{d^2 \alpha}{d x d y}\delta x\, \delta y$$
$$+ \frac{d^2 \alpha}{d x\, d z}\delta x\, \delta z + \frac{d^2 \alpha}{d y^2}\frac{\delta y^2}{2} + \text{\&c. \&c.}$$

and similar values for $\delta \beta\ \delta \gamma$.

When we have made these substitutions it is evident that the several differential coefficients of $\alpha\ \beta\ \gamma\ \alpha_{,}\ \beta_{,}\ \gamma_{,}$ may be brought outside the sign of summation. The result of all this will be six linear differential equations composed of the partial differential coefficients of $\alpha\, \beta\, \gamma$, $\alpha_{,}\, \beta_{,}\, \gamma_{,}$ multiplied by quantities such as $\Sigma\, m f(r)\, \delta x$, $\Sigma\, m f(r)\, \delta x^2$, &c. Now it is evident that these quantities must be different for different values of $x\, y\, z$, $x_{,}\, y_{,}\, z_{,}$ except in the cases represented by figures 1, 2, and 3;

except therefore in these cases our equations will have variable coefficients, and it will be impossible to solve them in general.

But if we suppose that the particles of æther are arranged as in figures 1, 2, or 3, then we may evidently take the axes of coordinates in such positions that all the particles both of matter and æther shall be similarly circumstanced with respect to them : for instance, supposing the particles of matter to be arranged at the corners of cubes, and therefore the particles of æther at the centres of these cubes, it is evident that if the coordinate axes be assumed parallel to the edges of these cubes, then the particles will be similarly circumstanced with respect to each axis.

Proceeding then upon the supposition that the particles are arranged as in figures 1, 2, or 3, we may assume that the axes of coordinates are so chosen that the particles are similarly circumstanced with respect to each of them.

Now, this being the case, it is evident that the quantity $\Sigma\,\{m \times$ any function of $r \times$ the product of any powers of δx, $\delta y, \delta z,\}$ will be zero, unless each of the powers of δx, δy, δz be even; and we may interchange $\delta x\, \delta y\, \delta z$ in this sum without altering its value. The same is evidently true with respect to the sums

$\Sigma\,\{m_{\prime} \times$ function of $r' \times$ powers of $x_{\prime} - x, y_{\prime} - y, z_{\prime} - z\}$

$\Sigma\,\{m\ \times$ function of $r' \times$ powers of $x - x_{\prime}\, y - y_{\prime}\, z - z_{\prime}\}$

and $\Sigma\,\{m_{\prime} \times$ function of $r_{\prime} \times$ powers of $\delta x_{\prime}\, \delta y_{\prime}\, \delta z_{\prime}\}$.

Moreover, all these sums are constants, independent of $x\, y\, z$, or $x_{\prime}\, y_{\prime}\, z_{\prime}$. Hence if we put

$$\mathrm{M} = \Sigma\, m f(r)\, \delta x^2 \qquad \mathrm{N} = \Sigma\, m \frac{1}{r} f'(r)\, \delta x^2\, \delta y^2$$

$$\mathrm{P} = \Sigma\, m \frac{1}{r} f'(r)\, \delta x^4 \;\; \mathrm{C} = \Sigma m_{\prime} \left\{ \phi(r') + \frac{1}{r'} \phi'(r') (x_{\prime} - x)^2 \right\},$$

our differential equation becomes

$$\frac{d^2 \alpha}{d t^2} = \frac{\mathrm{M}}{2} \left(\frac{d^2 \alpha}{d x^2} + \frac{d^2 \alpha}{d y^2} + \frac{d^2 \alpha}{d z^2} \right) + \frac{\mathrm{P}}{2} \frac{d^2 \alpha}{d x^2}$$

$$+ \frac{\mathrm{N}}{2} \left(\frac{d^2 \alpha}{d y^2} + \frac{d^2 \alpha}{d z^2} + 2 \frac{d^2 \beta}{d x\, d y} + 2 \frac{d^2 \gamma}{d x\, d z} \right)$$

+ differential coefficients of the fourth and higher orders,

$$- \mathrm{C} \alpha + \Sigma\, m_{\prime} \left\{ \phi(r')\, \alpha_{\prime} + \frac{1}{r'} \phi'(r') \Big((x_{\prime} - x)^2 \alpha_{\prime} \right.$$

$$\left. + (x_{\prime} - x)(y_{\prime} - y)\, \beta_{\prime} + (x_{\prime} - x)(z_{\prime} - z)\, \gamma_{\prime} \Big) \right\}.$$

Let us now compare the relative magnitudes of the terms which compose this equation, in order to determine what terms may be neglected in approximating: let us take, for instance, the terms $\left(\Sigma m f(r) \delta x^2\right) \frac{d^2 \alpha}{d t^2}$ and $\left(\Sigma m f(r) \delta x^4\right) \frac{d^4 \alpha}{d x^4}$. If the motion we are investigating consist of a wave whose length is λ, it is evident that α will be in some such form as $a \sin \frac{2 \pi}{\lambda}(v t-x)$, and therefore $\frac{d^2 \alpha}{d x^2}$ will be of the same order of magnitude as $\frac{\alpha}{\lambda^2}$, and $\frac{d^4 \alpha}{d x^4}$ as $\frac{\alpha}{\lambda^4}$; also δx is of the same order of magnitude as r; hence $\Sigma m f(r) \delta x^2 \frac{d^2 \alpha}{d x^2}$ and $\Sigma m f(r) \delta x^4 \frac{d^4 \alpha}{d x^4}$ are respectively of the same order of magnitude as $\Sigma m f(r) \frac{r^2}{\lambda^2}$ and $\Sigma m f(r) \frac{r^4}{\lambda^4}$; if we take $\bar{r}$ to be the value of r, for which $f(r)$ has its mean value, these quantities are of somewhat the same order of magnitude as $\Sigma m f(\bar{r}) \frac{\bar{r}^2}{\lambda^2}$ and $\Sigma m f(r) \frac{\bar{r}^2}{\lambda^4}$, which quantities are in the ratio $\frac{\bar{r}^2}{\lambda^2}$. Hence it evidently appears, that if the distance at which the molecular force has its mean value be very small compared with the length of the wave, all the terms in our equation involving differential coefficients of the fourth and higher orders will be very small compared with those involving the second differential coefficients, and we may neglect them. We shall assume this to be the case*, and therefore retain only the second differential coefficients in our equation. It is evident that the coefficients we have denoted by N and P are not by any means to be neglected as compared with M, for they are of exactly the same order of magnitude as M, as appears immediately, if we observe that δx, δy are of the same order as r, and $r f'(r)$ as $f(r)$.

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* The results arrived at in this paper are equally true, whether we retain or reject these terms; if the hypothesis of finite intervals be true we must retain them. But we know, from the fact of the dispersion of light being small compared with the whole deviation, that these terms (if not quite insensible) must be small compared with those retained. We therefore reject these terms, as it is not our object to investigate that part of the dispersion which may arise from them, but from other terms, namely, those introduced into our equations, in consequence of the forces exercised by the particles of matter on those of æther.

Now there is a very simple relation between P and N; for, putting $\delta\, x = u \cos \theta$, $\delta\, y = u \sin \theta$, and therefore $\delta\, x^4 = \frac{u^4}{8}(3 + 4 \cos 2\, \theta + \cos 4\, \theta)$ and $\delta\, x^2\, \delta\, y^2 = \frac{u^4}{8}(1 - \cos 4\, \theta)$, and observing that $\Sigma\, m \frac{1}{r} f'(r)\, u^4 \cos 2\, \theta$ and $\Sigma\, m\, \frac{1}{r} f'(r)\, u^4 \cos 4\, \theta$ are each zero, in consequence of the symmetry of the arrangement of the particles with respect to the coordinate axes, it is evident that $\Sigma\, m\, \frac{1}{r} f'(r)\, \delta\, x^4 = 3\, \Sigma\, m\, \frac{1}{r} f'(r)\, \delta\, x^2\, \delta\, y^2$, that is, $P = 3\, N$.

Hence, if for brevity we put $\frac{M + 3\, N}{2} = A$, $\frac{M + N}{2} = B$, and therefore $N = A - B$, our equation becomes

$$\left.\begin{aligned} \frac{d^2 \alpha}{d t^2} = A \frac{d^2 \alpha}{d x^2} + B \left(\frac{d^2 \alpha}{d y^2} + \frac{d^2 \alpha}{d z^2}\right) + (A - B) \left(\frac{d^2 \beta}{d x\, d y}\right. \\ \left. + \frac{d^2 \gamma}{d x\, d z}\right) - C\, \alpha + \Sigma\, m_{\prime} \left\{ \phi\, (r')\, \alpha_{\prime} + \frac{1}{r'} \phi'(r') \right. \\ \left. ((x_{\prime} - x)^2 \alpha_{\prime} + (x_{\prime} - x)\, (y_{\prime} - y)\, \beta_{\prime} + (x_{\prime} - x)\, (z_{\prime} - z)\, \gamma_{\prime}) \right\} \end{aligned}\right\} (1.)$$

and we may obtain, in the same manner exactly, similar equations for $\frac{d^2 \beta}{d t^2}$ and $\frac{d^2 \gamma}{d t^2}$; also in the same way we find the following, viz.—

$$\left.\begin{aligned} \frac{d^2 \alpha_{\prime}}{d t^2} = A_{\prime} \frac{d^2 \alpha_{\prime}}{d x_{\prime}^2} + B_{\prime} \left(\frac{d^2 \alpha_{\prime}}{d y_{\prime}^2} + \frac{d^2 \alpha_{\prime}}{d z_{\prime}^2}\right) \\ \left\{ + (A_{\prime} - B_{\prime}) \left(\frac{d^2 \beta_{\prime}}{d x_{\prime} d y_{\prime}} + \frac{d^2 \gamma_{\prime}}{d x_{\prime}\, d z_{\prime}}\right) - C_{\prime}\, \alpha_{\prime} \right\} \\ + \Sigma\, m \left\{ \phi\, (r')\, \alpha + \frac{1}{r'}\, \phi'\, (r')\, ((x - x_{\prime})^2\, \alpha \right. \\ \left. + (x - x_{\prime})\, (y - y_{\prime})\, \beta + (x - x_{\prime})\, (z - z_{\prime})\, \gamma) \right\} \end{aligned}\right\}. (2.)$$

and similar equations for $\frac{d^2 \beta_{\prime}}{d t^2}$ $\frac{d^2 \gamma_{\prime}}{d t^2}$

where $A_{\prime}$ $B_{\prime}$ $C_{\prime}$ are quantities analogous to A B C.

We shall now proceed to apply these equations to determine the circumstances of propagation plane waves in different cases.

The first case will be that in which the plane waves are propagated with a uniform velocity, the vibrations being supposed transversal, and the particles of matter absolutely fixed.

The particles of matter being supposed absolutely fixed, the equations (2.) will be identically zero, and the part under the sign Σ in (1.) will also be zero; hence we shall have

$$\left.\begin{aligned}\frac{d^2\alpha}{dt^2} &= \mathrm{A}\frac{d^2\alpha}{dx^2} + \mathrm{B}\left(\frac{d^2\alpha}{dy^2} + \frac{d^2\alpha}{dz^2}\right) \\ &\quad + (\mathrm{A}-\mathrm{B})\left(\frac{d^2\beta}{dx\,dy} + \frac{d^2\gamma}{dx\,dz}\right) - \mathrm{C}\alpha\end{aligned}\right\} \quad \ldots (1.)$$

$$\left.\begin{aligned}\frac{d^2\beta}{dt^2} &= \mathrm{A}\frac{d^2\beta}{dy^2} + \mathrm{B}\left(\frac{d^2\beta}{dx^2} + \frac{d^2\beta}{dz^2}\right) \\ &\quad + (\mathrm{A}-\mathrm{B})\left(\frac{d^2\alpha}{dx\,dy} + \frac{d^2\gamma}{dy\,dz}\right) - \mathrm{C}\beta\end{aligned}\right\} \quad \ldots (2.)$$

$$\left.\begin{aligned}\frac{d^2\gamma}{dt^2} &= \mathrm{A}\frac{d^2\gamma}{dz^2} + \mathrm{B}\left(\frac{d^2\gamma}{dx^2} + \frac{d^2\gamma}{dy^2}\right) \\ &\quad + (\mathrm{A}-\mathrm{B})\left(\frac{d^2\alpha}{dx\,dz} + \frac{d^2\beta}{dy\,dz}\right) - \mathrm{C}\gamma\end{aligned}\right\} \quad \ldots (3.)$$

Let the equation to any one of the planes of like phase be $px + qy + sz = u \ldots\ldots\ldots (4.)$

When $p\,q\,s$ are the cosines of the angles it makes with the coordinate planes respectively, and therefore u the perpendicular upon it from the origin. Now it is evident that $\alpha\,\beta\,\gamma$ must be functions of u alone, so far as $x\,y\,z$ are concerned, otherwise we should have particles in the same plane of like phase in different states of vibration, which is absurd. Again, if v be the constant velocity of propagation, we must have

$$\frac{d^2\alpha}{dt^2} = v^2\frac{d^2\alpha}{du^2}, \quad \frac{d^2\beta}{dt^2} = v^2\frac{d^2\beta}{du^2}, \quad \frac{d^2\gamma}{dt^2} = v^2\frac{d^2\gamma}{du^2} \quad \ldots (5.)$$

For if we suppose u and t always to vary in such a manner that $\alpha\,\beta\,\gamma$ remain invariable, then $\frac{du}{dt}$ obtained on such a supposition will be the velocity of propagation (v). Now, on this supposition we have $d\alpha = 0$, that is,

$$\frac{d\alpha}{du}du + \frac{d\alpha}{dt}dt = 0;$$

and therefore, since $du = v\,dt$, we have

$$v\frac{d\alpha}{du} + \frac{d\alpha}{dt} = 0;$$

and hence

$$\frac{d^2\alpha}{dt^2} = \frac{d}{dt}\left(-v\frac{d\alpha}{du}\right) = -v\frac{d}{du}\left(\frac{d\alpha}{dt}\right) = v^2\frac{d^2\alpha}{du^2};$$

and similarly we may show that

$$\frac{d^2\beta}{d\,t^2} = v^2\frac{d^2\beta}{d\,u^2} \quad \frac{d^2\gamma}{d\,t^2} = v^2\frac{d^2\gamma}{d\,u^2}.$$

Also, since the vibrations are transversal, we have

$$p\,\alpha + q\,\beta + s\,\gamma = 0 \quad \ldots\ldots\ldots\ldots \quad (6.)$$

We shall now simplify the equations (1.), (2.), (3.), by the conditions just obtained.

Since α, β, γ are functions of u alone, we have by (4.),

$$\frac{d^2\alpha}{a\,x^2} = \frac{d^2\alpha}{d\,u^2}\,p^2 \quad \frac{d^2\alpha}{d\,x\,d\,y} = \frac{d^2\alpha}{d\,u^2}\,p\,q, \text{ \&c.};$$

hence, and by (5.), the equation (1.) becomes

$$v^2\frac{d^2\alpha}{d\,u^2} = (\mathrm{A}\,p^2 + \mathrm{B}\,(q^2 + s^2))\frac{d^2\alpha}{d\,u^2}$$

$$+ (\mathrm{A} - \mathrm{B})\,p\left(q\frac{d^2\beta}{d\,u^2} + s\frac{d^2\gamma}{d\,u^2}\right) - \mathrm{C}\,\alpha;$$

or, since $q^2 + s^2 = 1 - p^2$,

$$\left.\begin{aligned}(v^2 - \mathrm{B})\frac{d^2\alpha}{d\,u^2} &= (\mathrm{A} - \mathrm{B})\,p \\ &\left(p\frac{d^2\alpha}{d\,u^2} + q\frac{d^2\beta}{d\,u^2} + s\frac{d^2\gamma}{d\,u^2}\right) - \mathrm{C}\,\alpha\end{aligned}\right\} \quad \ldots\ldots \quad (7.)$$

$$= -\,\mathrm{C}\,\alpha \text{ by (6.)},$$

and similar equations for β and γ.

We have therefore $\frac{d^2\alpha}{d\,u^2} + \frac{\mathrm{C}}{v^2 - \mathrm{B}}\,\alpha = 0$;

and by (5.) this gives

$$\frac{d^2\alpha}{d\,t^2} + \frac{\mathrm{C}\,v^2}{v^2 - \mathrm{B}}\,\alpha = 0.$$

Now the most general value of α which satisfies these equations, is

$$\alpha = a\cos k\,(v\,t - u + \varepsilon) + a'\cos k\,(v\,t + u + \varepsilon'),$$

where a ε a' ε' are arbitrary constants,

and $$k^2 = \frac{\mathrm{C}}{v^2 - \mathrm{B}}.$$

This value of α represents two waves, one transmitted forwards and the other backwards, with the same velocity v; confining our attention to the wave transmitted forward, we may put

$\alpha = a\cos k\,(v\,t - u)$, omitting ε also; and similarly we may show that

$$\beta = b \cos k\,(\upsilon t - u)$$
$$\gamma = c \cos k\,(\upsilon t - u).$$

The following two consequences follow from these results:

1st, That the particles must necessarily vibrate according to the cycloidal law; a plane wave cannot be transmitted with a uniform velocity unless this condition hold.

2ndly, That the velocity of transmission depends on the length of the wave; for if λ be the length of the wave,

$$k = \frac{2\pi}{\lambda}; \text{ and } \therefore \text{ since } k^2 = \frac{C}{\upsilon^2 - B}, \text{ we have}$$

$$\upsilon^2 = B - \frac{C}{4\pi^2}\lambda^2.$$

The second case we shall apply our equations to is that in which all the circumstances are the same as in the preceding case, except that the vibrations are normal. If this be the case, instead of the condition (6.) we have the conditions

$$\frac{\alpha}{p} = \frac{\beta}{q} = \frac{\gamma}{s},$$

and therefore (7.) becomes

$$(\upsilon^2 - B)\frac{d^2\alpha}{du^2} = (A - B)\,(p^2 + q^2 + s^2)\frac{d^2\alpha}{du^2} - C\alpha,$$

or

$$(\upsilon^2 - A)\frac{d^2\alpha}{du^2} + C\alpha = 0.$$

Hence the same consequences follow in this as in the former case; the only difference is, that instead of $k^2 = \frac{C}{\upsilon^2 - B}$ we have $k^2 = \frac{C}{\upsilon^2 - A}$, and therefore

$$\upsilon^2 = A - \frac{C}{4\pi^2}\lambda^2.$$

Hence it follows that transverse and normal vibrations are propagated with different velocities, viz.

$$\sqrt{B - \frac{C}{4\pi^2}\lambda^2} \text{ and } \sqrt{A - \frac{C}{4\pi^2}\lambda^2} \text{ respectively.}$$

It is evident that if C were zero equation (7.) would be satisfied quite independently of α by putting $\upsilon^2 = B$ if the vibrations be transverse, or A if normal. Hence it follows that if the particles of matter did not act on the particles of æther (i. e. if C was zero), a plane wave might be transmitted with a uniform velocity whether the law of vibration was cycloidal or not.

The third case we shall apply our equations of motion to is that in which the particles of matter are free to move, and do actually vibrate along with the particles of æther.

In this case we must not neglect the equations (2.), nor the part of our equations under the sign Σ. Now in the equation (1.) the sign Σ has reference to the particles of matter, and denotes the sum of a number of quantities in which $\alpha_{,}\,\beta_{,}\,\gamma_{,}$ have different values; but if the sphere of attraction or repulsion of the particles of matter on the particles of æther be very small, as we assume it to be, it is evident that $\alpha_{,}\,\beta_{,}\,\gamma_{,}$ will vary very little for all the different particles of matter to which the sign Σ has reference.

Hence for a first approximation we shall suppose $\alpha_{,}\,\beta_{,}\,\gamma_{,}$ the same for all the particles of matter to which the sign Σ has reference; and therefore $\alpha_{,}\,\beta_{,}\,\gamma_{,}$ may be brought outside the sign Σ, and then the part affected by the sign Σ becomes

$$\alpha_{,}\,\Sigma\, m_{,}\left\{\phi\,(r') + \frac{1}{r'}\,\phi\,(r')\,(x_{,}-x)^2\right\} + \text{a part which is zero}$$

in consequence of the symmetry.

Hence since $\Sigma\, m_{,}\left(\phi\,(r') + \frac{1}{r'}\,\phi'\,(r')\,(+x_{,}-x)^2\right) = \mathrm{C}$, we have

$$\frac{d^2\alpha}{d\,t^2} = \mathrm{A}\,\frac{d^2\alpha}{d\,x^2} + \mathrm{B}\left(\frac{d^2\alpha}{d\,y^2} + \frac{d^2\alpha}{d\,z^2}\right) + (\mathrm{A}-\mathrm{B})$$
$$\left(\frac{d^2\beta}{d\,x\,d\,y} + \frac{d^2\gamma}{d\,x\,d\,z}\right) - \mathrm{C}\,\alpha + \mathrm{C}\,\alpha_{,},$$

and similar equations with reference to $\frac{d^2\beta}{d\,t^2}\;\frac{d^2\gamma}{d\,t^2}$; and in exactly the same manner the equation (2.) becomes

$$\frac{d^2\alpha}{d\,t^2} = \mathrm{A}_{,}\frac{d^2\alpha_{,}}{d\,x_{,}^2} + \mathrm{B}_{,}\left(\frac{d^2\alpha_{,}}{d\,y_{,}^2} + \frac{d^2\alpha_{,}}{d\,z_{,}^2}\right) + (\mathrm{A}_{,}-\mathrm{B}_{,})$$
$$\left(\frac{d^2\beta_{,}}{d\,x_{,}d\,y_{,}} + \frac{d^2\gamma_{,}}{d\,x_{,}\,d\,z_{,}}\right) - \mathrm{C}_{,}\,\alpha_{,} + \mathrm{C}_{,}\,\alpha,$$

and similar equations for $\frac{d^2\beta_{,}}{d\,t^2}\quad\frac{d^2\gamma_{,}}{d\,t^2}$.

We shall now suppose, as in the previous case, that the vibrations of the æther constitute plane waves propagated with a uniform velocity; and it is evident that this being the case the vibrations of the particles of matter will also constitute plane waves propagated with a uniform velocity; hence making the same simplifications in our equation as in the preceding case, we have

$$(v^2-\mathrm{B})\,\frac{d^2\alpha}{d\,u^2} + \mathrm{C}\,\alpha - \mathrm{C}\,\alpha_{,} = 0 \quad \ldots\ldots \quad (1.)$$

supposing the vibrations of the æther transversal; and similarly,

$$(v^2-\mathrm{B})\frac{d^2\beta}{du^2}+\mathrm{C}\beta-\mathrm{C}\beta_{\prime}=0 \quad \ldots\ldots \quad (2.)$$

$$(v^2-\mathrm{B})\frac{d^2\gamma}{du^2}+\mathrm{C}\gamma-\mathrm{C}\gamma_{\prime}=0; \quad \ldots\ldots \quad (3.)$$

(1.) p + (2.) q + (3.) s gives, since $p\alpha+q\beta+s\gamma=0$,

$$\mathrm{C}(p\alpha_{\prime}+q\beta_{\prime}+s\gamma_{\prime})=0;$$

hence the vibrations of the particles of matter are transverse, and hence we have exactly, as in the case of the vibrations of the æther,

$$(v_{\prime}^2-\mathrm{B}_{\prime})\frac{d^2\alpha_{\prime}}{du^2}+\mathrm{C}_{\prime}\alpha_{\prime}-\mathrm{C}_{\prime}\alpha=0$$

$$(v_{\prime}^2-\mathrm{B}_{\prime})\frac{d^2\beta_{\prime}}{du^2}+\mathrm{C}_{\prime}\beta_{\prime}-\mathrm{C}_{\prime}\beta=0$$

$$(v_{\prime}^2-\mathrm{B}_{\prime})\frac{d^2\gamma_{\prime}}{du^2}+\mathrm{C}_{\prime}\gamma_{\prime}-\mathrm{C}_{\prime}\gamma=0,$$

where $v_{\prime}$ is the velocity with which the vibrations of the material particles are propagated. We here make no difference between u and $u_{\prime}$, for supposing $\alpha\,\beta\,\gamma$ and $\alpha_{\prime}\beta_{\prime}\gamma_{\prime}$ to belong to contiguous particles, it is evident that $u_{\prime}$ does not differ sensibly from u.

We have supposed $(v_{\prime})$ the velocity of propagation of the vibrations of the material particles not the same as (v) that of the æthereal particles; for these quantities are evidently quite independent of each other, so far as the above equations of motion are concerned; and whatever connexion subsists between them must evidently be determined from initial or other circumstances independent of the equations of motion.

Let us now find α from the equations

$$(v^2-\mathrm{B})\frac{d^2\alpha}{du^2}+\mathrm{C}(\alpha-\alpha_{\prime})=0 \quad \ldots\ldots\ldots \quad (1.)$$

$$(v_{\prime}^2-\mathrm{B}_{\prime})\frac{d^2\alpha_{\prime}}{du^2}+\mathrm{C}_{\prime}(\alpha-\alpha_{\prime})=0 \quad \ldots\ldots \quad (2.)$$

(1.) $\mathrm{C}_{\prime}$ + (2.) C gives

$$(v^2-\mathrm{B})\,\mathrm{C}_{\prime}\frac{d^2\alpha}{du^2}+(v_{\prime}^2-\mathrm{B}_{\prime})\,\mathrm{C}\frac{d^2\alpha_{\prime}}{du^2}=0;$$

and $\therefore\ \alpha_{\prime}=-\dfrac{v^2-\mathrm{B}}{v_{\prime}^2-\mathrm{B}_{\prime}}\dfrac{\mathrm{C}_{\prime}}{\mathrm{C}}\alpha,$

adding no constants, because the motion is oscillatory.

Hence (1.) becomes

$$(v^2-\mathrm{B})\,\frac{d^2\,\alpha}{d\,u^2}+\left(\mathrm{C}+\frac{v^2-\mathrm{B}}{v_{,}^2-\mathrm{B}_{,}}\,\mathrm{C}_{,}\right)\alpha=0,$$

or

$$\frac{d^2\,\alpha}{d\,u^2}+\left(\frac{\mathrm{C}}{v^2-\mathrm{B}}+\frac{\mathrm{C}_{,}}{v_{,}^2-\mathrm{B}_{,}}\right)\alpha=0;$$

or, if we denote $\frac{\mathrm{C}}{v^2-\mathrm{B}}+\frac{\mathrm{C}_{,}}{v_{,}^2-\mathrm{B}}$ by k^2,

$$\frac{d^2\alpha}{d\,u^2}+k^2\,\alpha=0;$$

and in exactly the same manner we have

$$\frac{d^2\,\alpha_{,}}{d\,u^2}+k^2\,\alpha_{,}=0;$$

and similar equations for determining $\beta\;\beta_{,}\;\gamma\;\gamma_{,}$.

From the equation $\frac{d^2\,\alpha}{d\,u^2}+k^2\,\alpha=0$, we conclude, as in the former case, first, that the particles must vibrate according to the cycloidal law; and secondly, that

$$\frac{4\,\pi^2}{\lambda^2}=k^2=\frac{\mathrm{C}}{v^2-\mathrm{B}}+\frac{\mathrm{C}_{,}}{v_{,}^2-\mathrm{B}_{,}}.$$

Now in the case where the particles of matter are put in motion solely by the vibrations of the æther, it is, I think, evident that the particles of matter will (except in peculiar cases) vibrate in the same time as the particles do which put them in motion *. Now $\frac{2\,\pi}{k\,v}$ is the time of vibration of the particles of æther, and since k is the same for $\alpha_{,}$ as for α, $\frac{2\,\pi}{k\,v_{,}}$ is that of the particles of matter, hence by what has been said we must have in general $v_{,}=v$, and hence it follows that

$$\frac{4\,\pi^2}{\lambda^2}\qquad\frac{\mathrm{C}}{v^2-\mathrm{B}}+\frac{\mathrm{C}_{,}}{v^2-\mathrm{B}_{,}}.$$

It now remains to show, that the results we have thus obtained are equally true when the particles are arranged as in figure 4, and are therefore true in all cases, whatever be the law of arrangement of the particles; the proof of this is simple, but as it leads to some interesting results which I have

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* We know this to be the case when aërial vibrations put the particles of any body in motion; provided those particles be capable of vibrating in *any time*.

not yet sufficiently considered, and as I have already trespassed much on your valuable space, I shall reserve it for a future communication.

It also remains to obtain the more general value of $\frac{4\,\pi^2}{\lambda^2}$ mentioned at the commencement of this paper, on the supposition that the particles of the transparent body are composed of atoms of different kinds.

I intend also to proceed to a second approximation in the solution of the equations (1.) and (2.), on the supposition that $\alpha_{\prime}\,\beta_{\prime}\,\gamma_{\prime}$ are *not* the same for all particles under the sign Σ; which I feel pretty sure will lead to the following result, viz. that the velocity of propagation of plane waves depends in some small degree on their position with respect to the axes of symmetry. As we know experimentally that this is not the case, we may hence conclude, that our first approximation was sufficiently near the truth. I believe also that the result of this second approximation may be made use of to prove that the hypothesis of finite intervals cannot be correct.

Brighton, Dec. 27, 1841.

XXXI. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. Francis *and* H. Croft.

[Continued from p. 39.]

New Method of determining Nitrogen in Organic Compounds. By Drs. Varrentrapp *and* Will.

THE nitrogen in organic substances, when not contained in them in the form of ammonia, is always determined quantitatively by burning them with the oxide of copper, absorbing by an alkali the carbonic acid formed, and measuring the volume of nitrogen gas set free. Even in the analysis of certain ammoniacal compounds, as the ammoniacal salts of organic acids, it has generally been preferred to determine the amount of ammonia by ascertaining the volume of nitrogen. The principal objection to be made to the methods generally in use, which, though different in their execution, do not differ in principle, is, that the amount of nitrogen is not determined by weight, but by measuring the volume of the gas. The doubts still existing as to the constitution of many organic bases and of numerous indifferent nitrogenous compounds, are not solely owing to a want of knowledge of their true atomic weight, but likewise of the actual quantity of nitrogen they contain. The determinations of the nitrogen in most of the organic substances which contain but a small quantity of that element, even when performed by able chemists, vary in their percentage results far more than we find to be the case in determinations of carbon and hydrogen. These differences resulting from the difficulties and uncertainties of the usual methods, made chemists feel the want of one more simple and accurate.

M. Dumas has already, in his examination of oxamide, determined the nitrogen by collecting it in the form of ammonia; and the experiments of H. Rose left no doubt that ammonia could be most accurately weighed in the form of the ammonio-chloride of platinum. These, and direct experiments of Wœhler, who had succeeded in determining correctly the nitrogen in uric acid by converting it into ammonia and weighing it as ammonio-chloride of platinum, led us to hope that this experiment might conduct to a sure method for all nitrogenous bodies, and we believe that our endeavours have been attended by complete success.

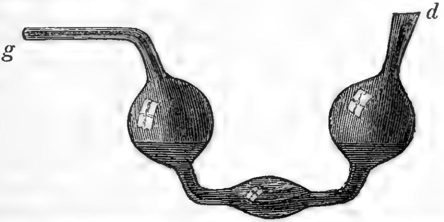
The method to be described is as simple and as certain as the determination of carbon and hydrogen according to Prof. Liebig's method. It is founded on the action of the hydrates of the alkalies on nitrogenous organic substances at high temperatures, and consists in determining the weight of the nitrogen in the form of ammonia, *i. e.* as ammonio-chloride of platinum, or as metallic platinum.

Gay-Lussac showed that if any organic substance free from nitrogen be melted with hydrate of potash, the water of that hydrate is decomposed, its oxygen combining with the carbon and hydrogen of the organic body, while its hydrogen is evolved in the gaseous state. The products which arise from this powerful process of oxidation vary according to the temperature to which the mixture is exposed, and according to the constitution of the organic substance. It is sufficient to know, that with substances free from nitrogen, *hydrogen is liberated*; this hydrogen combines when a nitrogenous substance is subjected to the same kind of decomposition, with the entire amount of nitrogen, and forms ammonia. Hitherto this action has been employed merely to ascertain whether a body contains nitrogen or not.

In substances which contain much nitrogen, for instance, uric acid, melamin, mellon, &c., the whole of the nitrogen is not converted into ammonia at the commencement of the decomposition; a portion of it combines with a portion of the carbon of the substance to form cyanogen, which, as such, and probably also in the shape of cyanic acid, combines with the metal of the alkali, in the latter case with the alkali itself. The permanent nature of these cyanides at high temperatures made us suspect that in such substances the whole amount of nitrogen could not be converted into ammonia. Direct experiments, however, have convinced us, that on employing a sufficient excess of the hydrated alkali, and not too low a temperature, every compound containing cyanogen or nitrogen, every substance, in fact, which does not contain its nitrogen in the form of nitric acid, is decomposed in such manner that all the nitrogen is obtained in the form of ammonia as the final product. If cyanide of potassium, cyanate of potash, or paracyanogen be melted with an excess of hydrate of potash at a red heat, or heated with a non-fusible mixture of the hydrate of potash or soda and caustic lime, a considerable evolution of ammonia takes place, and not a trace of cyanogen or of a compound of cyanogen can be detected in the residuum. It is necessary, on performing this experiment, to employ so much of the hydrated alkali that all the carbon of the substance may be oxidized by the oxygen of the water of the former. The mixture should become again perfectly white. According to the amount of carbon in the substance, and to the temperature, other permanent gases are evolved together with the ammonia; such as marsh gas, olefiant gas, hydrogen, or a mixture of these; and in many cases even liquid carburetted hydrogens, as Benzin; at least the oily drops sometimes formed have quite the smell of the latter substance.

Melamin, mellon, cyanogen and its compounds, belong to the class of bodies which abounds most in nitrogen; but these all contain as much carbon, or even more than is necessary, to set free by its oxidation hydrogen enough to form ammonia with all the nitrogen, without any of the latter remaining uncombined. In some of these compounds, as in mellon, which is represented by the formula $C^6 N^8$, in melamin $C^6 N^{12} H^{12}$, the decomposition is effected by means of a sufficient quantity of hydrated alkali without a trace of permanent gas being formed. All the carbon is converted into carbonic acid, which remains in combination with the alkali; all the nitrogen into ammonia, which is given off. Our method consists, as above stated, in condensing this ammonia by means of an acid, and in weighing it in a solid form as ammonio-chloride of platinum.

The apparatus employed is as simple as that which is in use for determining the carbon and hydrogen of organic substances according to Liebig's method, and we shall now proceed to a more particular description of it, and of the measures of precaution to be observed. The apparatus consists of a common tube of combustion, which may be smaller than those used for determining carbon and hydrogen, and glass bulbs containing muriatic acid for the absorption of the ammonia generated, fixed, air-tight, to the tube by means of a bored cork which need not be previously dried. The form of the bulb apparatus chosen will allow of its being easily and perfectly washed out, while there is not the least danger of any of the ammonia not being absorbed. It is represented in the annexed wood-cut,



and is easily constructed. It is filled by inserting the pointed end *g* into muriatic acid of the usual strength (specific gravity 1·13), and drawing it in with the mouth at *d* until the stand of the liquid attains the level indicated by the shading.

For decomposing the nitrogenous substances and oxidizing their carbon and hydrogen, we employ a mixture of the hydrate of soda or of potash with caustic lime, in such proportions, that, although acting energetically, it does not melt at a red heat, but merely frits slightly: this mixture has at the same time the advantage of being of an easy pulverizable nature, of not attracting moisture very rapidly, and is as easily managed as the oxide of copper or chromate of lead. The best mixture is that of 1 part by weight of hydrate of soda to 2 parts of caustic lime (one part of hydrate of potash requires three of caustic lime, but this latter mixture attracts moisture more rapidly, and has other disadvantages which will be subsequently mentioned), and is easily prepared by slacking the caustic lime with a solution of soda of known concentration; it is then heated in a crucible and powdered finely; or the melted and cooled hydrate of soda is pounded quickly in a warm mortar, and well mixed with the finely powdered caustic lime in the above proportions. The mixture is again exposed to a gentle heat to drive off all moisture, and preserved in a well-stopped bottle with a wide mouth.

When the substance to be examined has been reduced to a fine powder, dried and weighed, the previously cleansed and dried tube of combustion is filled one half with the mixture of alkali and lime, in order to have a fixed measure for the quantity with which the substance to be analysed is to be mixed. The quantity of the latter which is requisite for a combustion, can be taken according to the supposed amount of nitrogen it contains; it is, however, rarely requisite to employ more than 400 milligrammes with substances containing little nitrogen, and not less than 200 milligrammes of those which contain much. The weighed quantity of the substance is mixed with the measured quantity of soda-lime in a mortar previously somewhat *warmed* and *unglazed*, by stirring *gently* with the pestle. If these two measures of precaution are observed, there is no danger of any loss resulting from an adhesion of substance to the sides of the mortar and pestle. When the mixture has been introduced into the tube in the usual manner, the mortar is repeatedly rinsed with some soda-lime, the tube filled within an inch of the aperture, and a close layer of previously ignited asbestos introduced to prevent any of the fine powder being carried away by the gases evolved during the burning, which, especially on employing a mixture of the hydrate of potash and lime, would give rise to incorrect results, as the potash-chloride of platinum acts in the same way towards solvents as the ammonio-chloride. In this respect the mixture of soda and lime is preferable, for should any such accident happen, the soluble soda-chloride of platinum is easily removed by washing.

The tube is then connected with the apparatus containing the muriatic acid by a soft tight-fitting cork, placed in a common combustion furnace, and some air expelled by warming the bulb *a* with a glowing charcoal, to see whether the apparatus be air-tight; if so, the front part of the tube, which contains none of the organic substance, is heated to redness by surrounding it with glowing charcoal, that nothing may distil over undecomposed. The cork must be kept as warm as possible, that it may not retain or imbibe any moisture, which, by absorbing ammonia, would occasion an error in the result. As soon as this part of the tube is red-hot the burning is proceeded with; carbonic acid is formed from the oxygen of the hydrate and the whole or a part of the carbon of the substance; the hydrogen set free combines with the nitrogen in its nascent state as ammonia, which escapes in the form of gas. At the same time, according to the amount of carbon in the substance, pure hydrogen, or carburetted hydrogen gas, is given off, which are not absorbed by the acid, and indicate the progress of the combustion. It is particularly observed, that the burning must be carried on so quickly that a constant and uninterrupted evolution of gas takes place; there is no danger of any ammoniacal gas escaping; the absorption is so complete and rapid that there is rather reason to fear the acid rising into the tube, which would render the analysis useless. Only few substances contain so much nitrogen that the whole carbon requires to be oxidized and converted into carbonic acid, in order that sufficient hydrogen be set free to form ammonia with the whole of the nitrogen; and we are not acquainted with a single nitrogenous organic substance in which the nitrogen is not present in the form of nitric acid, that does not contain carbon at least sufficient for this purpose. To the last class belong mellon, mellamin, &c.; these also give, as well as the sulphuret of cyanogen, perfectly accurate results if the following precaution be taken. As in this case the greater part of the air is expelled from the tube by the first action of the heat, nearly pure ammonia is given off, which is absorbed with such violence by the acid, that let the bulb next the tube be ever so large, a returning back of the acid is scarcely avoidable. This evil is easily remedied by adding about an equal weight of some organic substance free from nitrogen (sugar for instance) to the substance to be analysed. This affords permanent gases by its decomposition, which dilute the ammonia and render its absorption by the acid less rapid. After the tube has been gradually brought to redness the whole of its length, and the evolution of gas has *entirely* ceased, which occurs when all the carbon has become oxidized and the mixture again appears white, the extreme erect point is broken off, and a current of atmospheric air drawn through the absorbing apparatus, that the ammonia still in the tube may be condensed. In doing this a tube containing potash is fixed on to the extremity of the absorbing apparatus by which the air enters; this perfectly secures it against acid vapours which may accompany the current of atmospheric air. The becoming white of the mixture in the tube is of importance, as ammonia in contact with alkali and carbon, at a high temperature, easily forms prussiate of ammonia, which would cause a loss of nitrogen in the analysis. All the carbon, however, is burnt when sufficient heat is employed, and not the least fear need be entertained of the formation of cyanogen.

Such is the course to be followed in the conversion of nitrogen into ammonia with solid organic bodies. The number of fluid nitrogenous substances is not very considerable, and their combustion offers no difficulties. Some of the mixture is first introduced into the tube, then a small bulb with a known quantity of substance, the point of which has been broken off previous to its introduction; the remainder of the tube is filled with the alkaline mixture and asbestos. When the first third portion of the tube has been heated to redness, it is best to drive a portion of the substance out of the bulb by means of a glowing coal; it is then diffused in the middle portion of the tube and is burnt gradually.

When the burning is finished, the contents of the absorbing apparatus are poured into a small porcelain evaporating dish, a mixture of alcohol and æther is introduced into the apparatus to remove any carburetted hydrogen from its sides, and it is then washed with water until the same shows no acid reaction. A pure solution of the chloride of platinum is then added in excess to the sal-ammoniac, and the whole evaporated to dryness in a drying apparatus guarded against dust. When the combustion has been well conducted, the dry ammonio-chloride of platinum is of a beautiful yellow colour: if the substance contain much carbon or is difficult to burn, it is darker, the muriatic acid becoming black on evaporation from contact with carburetted hydrogen. This colouring has no influence on the result if the precipitate is carefully washed.

The dried residue in the porcelain vessel is treated, when cooled, with a mixture of two volumes of strong alcohol with one of æther, in which the ammonio-chloride is insoluble, the chloride easily soluble. It is soon seen whether an excess of the chloride of platinum has been added, from the yellow colour of the liquid. The precipitate is thrown on a filter which has been dried at 212° F. and weighed, and then washed with the same mixture of alcohol and æther until it runs off perfectly clear, leaves no residue, and does not re-act acid. The precipitate perfectly washed is dried at 212° F., and the amount of nitrogen calculated from its weight. It is proper to control this weighing by exposing the ammonio-chloride to ignition, and calculating the nitrogen from the platinum obtained. The ammonio-chloride of platinum is pure when the two calculations do not sensibly differ.

In decomposing the ammonio-chloride, it is best, as observed by H. Rose in his memoir ‘On the Combinations of Ammonia with Carbonic Acid*,’ to place the salt with the filtering paper in the crucible, and to expose it for some time with the lid closely applied to a moderate heat. If this precaution is not taken, some undecomposed salt and metallic platinum may be mechanically carried away by the vapours of the muriate of ammonia, which would occasion a loss of platinum, and consequently of nitrogen.

It must be especially observed, that perfectly pure chloride of platinum must be employed in this kind of nitrogen analyses; it must contain previously no ammonio-chloride of platinum in solution. It is difficult to free the platina sponge obtained from the ammonio-chloride entirely from the muriate of ammonia. If the sponge be boiled with distilled water, the filtered liquid frequently gives a considerable precipitate

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* Taylor's Scientific Memoirs, vol. ii. (part 5.) p. 99, &c.

with nitrate of silver. It is therefore advisable to boil the sponge several times in distilled water previous to dissolving it in aqua regia. We consider it more simple, and as affording more accurate results, to weigh the nitrogen in the form of the ammonio-chloride of platinum, as in this case we have for every 177 parts of nitrogen 2788 parts of the salt on the balance, but with metallic platinum only 1233 parts for the same quantity of nitrogen. The error in weighing must consequently be double as great to exert the same influence on the result. The weight of the ammonio-chloride remains constant even when desiccation at 100° is continued for a considerable length of time, and the filter does not vary in the least if previously well washed; but when it contains a trace of free acid the paper becomes black and friable.

[The authors have tested the accuracy of this method on various nitrogenous compounds, and a comparison of the results with those obtained by former methods will convince every one of its superiority. We give at present but a few of MM. Will and Varrentrapp's analyses of well-known substances, reserving others, for which new formulæ have been advanced, for our next communication.]

PROPORTION OF NITROGEN.

		According to		Calculated.
Urea	46·79	Wœhler & Liebig	46·73	46·76
Uric acid	33·18	Liebig	33·36	33·37
Taurin	11·00	Demarçay	11·29	11·27
Oxamide	31·7	Dumas	31·85	31·8
Caffein	28·90	Liebig	28·78	28·83
Asparagin	21·27	Liebig	21·17	21·27
Melamin	66·22	Liebig	66·67	66·56
Hippuric acid	7·78	Liebig	7·82	7·82
Amygdalin	3·12–2·96	Wœhler & Liebig	3·07	3·07

(*Ann. der Chem. und Pharm.* xxxix. p. 257.)

Method of preparing pure Oxide of Antimony.

The following is, according to Adolph Rose, the best method of preparing pure oxide of antimony:—One part of finely powdered pure sulphuret of antimony is dissolved in $3\frac{1}{2}$ parts of fuming hydrochloric acid in a retort, and the clear solution subjected to distillation. The first portion which passes over is set aside, as it contains nearly pure hydrochloric acid, and as soon as the liquid which comes over becomes milky on the addition of water, it is received separately. The distillation is discontinued when no more passes over at a moderate heat, the contents of the receiver diluted with water, and the *pulvis Algarothi* decomposed with a solution of carbonate of soda.

The oxide is easily freed from the salts of soda by washing. According to the methods generally in practice, this preparation is obtained in an impure state, either mixed with metallic antimony, which is easily detected, or, what is more frequently the case, with the higher oxides. To detect these the author proposes the following method, founded on the well-known fact, that oxide of antimony melts with the sulphuret to form the glass of antimony without any disengagement of sulphurous acid, while the higher oxides are reduced to this protoxide under liberation of sulphurous acid. Fifteen parts of the supposed pure oxide are carefully mixed with thirty-five parts of the sulphuret, and the mixture placed in a glass tube, which is connected on the one side by a chloride of calcium tube with a flask for disengaging gas, on the other with a glass tube, which is bent at right angles, and terminates in a small vessel containing dilute water of ammonia. The mixture is then heated, during which a current of carbonic acid gas is passed over it to prevent any access of air. As soon as the oxide has melted with the sulphuret, the heating is discontinued, and the ammoniacal liquid subjected to examination. Hydrochloric acid is added to it, and then a clear solution of sulphuretted hydrogen; the opacity caused by the precipitated sulphur indicates sulphurous acid; another portion is treated with dilute sulphuric acid, which must be perfectly free from any oxide of nitrogen, and then a dilute solution of hypermanganic acid, which, when sulphurous acid is present, is instantaneously decolorated: this is the best test. Only pure oxide, and oxide containing metallic antimony, are easily fusible; a small addition of antimonious acid renders the fusion difficult; and with a larger quantity (about 30 per cent.) the mixture cannot be melted over an Argand lamp.—*Poggendorff's Annalen*, liii. p. 161.

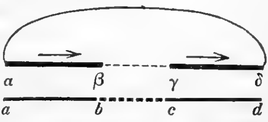
Method of detecting and distinguishing Gum, Dextrin, Grape Sugar, and Cane Sugar.

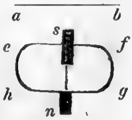
M. Trommer has made several experiments to find out a method of distinguishing the above substances in solution; the method advanced is founded on their different actions towards the sulphate of copper when a free alkali is present. Gum solution gives a blue precipitate, which is insoluble in an alkaline liquor, but is soluble in pure water, and can be boiled without becoming black; a proof that it is not the hydrate of the oxide of copper, but a combination of gum with the oxide of copper. Starch and gum tragacanth exhibit the same action; a solution of dextrin affords a deep blue-coloured liquid, without a trace of a precipitate, which, when heated to

85° C., deposits a red crystalline precipitate of red oxide of copper, which dissolves completely in muriatic acid. In this way he found, that on the conversion of starch into dextrin and so into grape sugar, not a trace of gum is formed as intermediate product; if a precipitate was obtained, it resulted from undecomposed starch, which might be easily recognized by the addition of an aqueous solution of iodine. If a solution of grape sugar and of potash be treated so long with a solution of the sulphate of copper as the separated hydrate of the oxide of copper is redissolved, in a very short time a precipitation of the red oxide of copper takes place at the common temperature; if the solution is warmed the red oxide is immediately separated, even when a small quantity of the sulphate of copper has been employed, and the liquid soon becomes colourless; a liquid containing $\frac{1}{100,000}$ of grape sugar gives, when boiled, a perceptible precipitate; and when it contains $\frac{1}{1,000,000}$, a distinct red colouring is seen when the light is let fall on it. A solution of cane sugar with potash, treated with the sulphate of copper, becomes of a deep blue; it can be boiled, when potash is used in excess, without any separation of the red oxide of copper, which results only after continued boiling, or when left to stand for several days; but even after several weeks the whole of the oxide has not been reduced. Milk sugar acts like grape sugar. This method is of importance in detecting small quantities of grāpe sugar in the chyme, chyle and blood; the author had not been able to detect any in the latter.—(*Ann. der Chem. und Pharm.* xxxix. p. 360.

XXXII. *Experiments in Electricity and Magnetism.* *By Professor* H. W. Dove *of Berlin*[*].

1. *On the Induced Currents excited by the Magnetization of Iron by means of Frictional Electricity.*





If in two wires, *a b* and *c d*, which are connected by the wire *b c*, we excite an electric current, this current when disappearing will induce in two neighbouring parallel wires an electric current having the same direction. If, however, we connect these wires crosswise, then the currents induced in *α β* and *γ δ* by the primary currents in *a b* and *c d* will act exactly contrary to each other, and there will be a perfect equilibrium; if, however, near to *a b* there is another

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[*] Extracted from a letter from Professor Dove, and communicated by H. Croft, Esq., Teacher of Chemistry.

closed wire, *efgh*, or perpendicular to its plane an iron bar *sn*, then this equilibrium of the currents will be destroyed. In the first case the current in $\alpha\beta$ will be retarded by the current induced in *efgh*, and consequently its galvanometric action remains unchanged, but its property of produced physiological effects and of magnetizing steel will be diminished. The magnetism of *sn*, which vanished on the cessation of the current in *abcd*, will increase the quantity of electricity circulating in $\alpha\beta$, and consequently the action of the current will be increased.

If we suppose this electro-magnetized iron, *sn*, surrounded by a conducting wire, on account of the magnetism disappearing in *sn*, a greater quantity of electricity will be set in motion in $\alpha\beta$ than in $\gamma\delta$, but on account of the simultaneously excited current in *efgh* this electricity will be moved with less rapidity than the smaller quantity in $\gamma\delta$.

Three different cases may here occur:—

1st. The increased quantity of electricity raises any one particular action of the current more than the simultaneous retarding of the current decreases it.

2nd. The action which is increased by the greater quantity of electricity is exactly compensated for by the retarding of the current.

3rd. The retarding of the current weakens any particular action more than it is strengthened by the increased quantity of electricity.

In all experiments which have as yet been made respecting induction, the first case alone has been observed. The following experiments show that the other two cases can occur, and indeed for one particular action of the current $\alpha\beta$, the one case, and for another the third.

A bundle of isolated iron wires does not admit of the formation of electrical streams circulating round the whole bundle; if, however, we inclose it in a conducting case or covering, as for instance in a brass tube, the bundle then represents the magnet *sn*, and the covering of the bundle the wire *efgh*: in the case of a massive bar of iron, its surface may be considered as the inclosing case *efgh*. *Sn* with its surrounding wire *efgh* is therefore such an electro-magnet.

In order to increase the action *ab* and *cd* are cylindrical spirals, $\alpha\beta$ and $\gamma\delta$ are larger spirals which fit over *ab* and *cd*; they are separated from each other by a tube of pasteboard, and all the spirals are covered with silk and carefully varnished. The bars of iron and bundles of wires are inserted into the spiral *ab*.

1. If the primary current *abcd* is that of a galvanic bat-

tery, and if there is in *a b* a bundle of iron wires or a massive bar of iron or nickel, when the current ceases *all the actions* of the current in *α β* are increased; the current which is observed after the equilibrium has been destroyed must therefore pass from *α* to *β*. [The thermic, physiological, and galvanometrical effects were examined, also the power of magnetizing steel and soft iron.] The separation of the iron bar into wires, or, in other words, the destruction of the current *e f g h*, has no visible influence on the strength of the current as measured by the galvanometer and by its power of magnetizing soft iron, but it increases the physiological effect and the power of magnetizing steel to an extraordinary degree, as I have shown at length in a former paper. (Reports of the Berlin Academy, 1839, p. 163, and Poggendorff's Annals, vol. xlix. p. 72.) If a bar of unmagnetic metal, or a simple tube (that is, *e f g h* without *s n*), be inserted into *a b*, then the galvanometric effect of the current *α β* is not altered, but physiological effect and the power of magnetizing steel are diminished.

2. If the primary current is that of a self-discharging Leyden jar, then

a. The thermic effect of the current *α β* is weakened both by the electro-magnet *s n* alone, and by the wire *e f g h*, and also by both together; for the heating effects are diminished by the insertion of bundles of wires or of bars of either magnetic or unmagnetic metals into *a b*.

b. The physiological effect of the current *α β* is weakened by the wire *e f g h* either with or without *s n*, but strengthened by this latter alone; for it is diminished by a massive bar of iron, and by a bar or tube of an unmagnetic metal, but considerably increased by a bundle of iron wires.

c. The electroscopic effect is just the same, for the current, which may be tested by means of the condenser and resinous figures, is found to proceed from *β* to *α* if there is in *a b* a bar of iron or of an unmagnetic metal, but from *α* to *β* when it is a bundle of iron wires or a bar of nickel. In this latter badly-conducting metal the excited current *e f g h* is too weak to overcome the action of *s n*.

d. The power of magnetizing steel is, on the contrary, increased by *s n* alone, and by *s n* surrounded with *e f g h*, but diminished by *e f g h* alone; for the polarity of a steel needle magnetized by the stream induced in *α β* shows that its direction is from *α* to *β* if a bar or bundle of iron or nickel wires has been employed, but from *β* to *α* if a bar of an unmagnetic metal has been used.

e. The induced current does not act on the galvanometer, does not decompose chemically, and cannot magnetize soft iron.

The explanation of these phænomena would be as follows:—

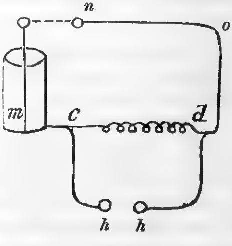
An electric current when it commences produces in iron near it an electric current of very short existence; during its whole continuance it causes magnetism, and at the moment of its cessation another electric current. The actions of this evanescent magnetism, and of the second electric current, are exactly opposed to each other.

If the current continued longer (*i. e.* if it were galvanic), that is, if the magnetism could be fully developed, it would overpower the opposing action of the electric current, but if momentary (or electrical) this latter action overcomes the former. The electrical currents in iron have therefore different properties to what the magnetism of the iron possesses, and as far as regards their actions they follow quite different laws; so that in one the electric current overpowers the magnetism, and in another the contrary is the case.

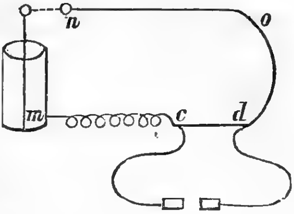
A bundle of isolated iron wires is the nearest possible realisation of an Ampère's *solenoid*, but it does not behave like a magnet, for it wants a conducting unbroken covering, which first causes it to become a magnet.

As, moreover, magnetism is excited in iron during the continuance of an electric current, where, as is well known, electric currents are never produced by induction, they appearing only at the beginning and the end, it is evident that those currents which have been hypothetically assumed by Ampère to exist in iron are altogether wanting in experimental proof.

2. *On the Induction which the Connecting Wire of the Leyden Jar exerts on itself.*



If *m o n* represent the connecting wire of a Leyden jar, and *c h h d* another connecting wire which ends in handles at *h h*, by the induction of the spiral part of the connecting wire upon itself a shock is obtained from *c h h d*. That this does not arise from a division of the current, is proved by the fact that the accessory shock is not produced when the following arrangement is employed. This action is greatly increased by the insertion into the spiral of a bundle of iron wires, but, on the contrary, diminished by a bar of iron. The thermic action is diminished in both cases, but that of magnetizing steel increased; the current so induced behaves therefore exactly like that induced in another wire.



3. *On the Magnetism of the so-called Unmagnetic Metals.*

We have as yet endeavoured to prove the magnetism of the so-called unmagnetic metals by attempting to direct or attract them by strong magnets, or else by trying their power of moving very light delicate magnetic needles; both methods however without success. In the above experiments we have demonstrated the hindering influence of the electrical currents induced in iron during its magnetization, and this excited a hope of being able to let the weak magnetism of these metals appear, by preventing the formation of those electric currents. This was crowned with complete success; for while massive bars of antimony, bismuth, lead, tin, zinc, copper and brass weakened the current $\alpha\beta$, so that when the equilibrium was destroyed, a magnetic needle indicated the direction of the current as being from β to α; on the contrary, the current was strengthened, or the direction became from α to β when those substances were introduced into the spiral *a b* in the form of well-isolated wires.

XXXIII. *Reply to the Inquiry in the Supplement Number of the Philosophical Magazine for January* 1842, *respecting a Manuscript at Oxford on the Rectification of the Circle. By* WILLIAM RUTHERFORD, *Esq.**

IN the Supplement Number of the Philosophical Magazine for January 1842, Mr. Halliwell inquires whether I can give him any information respecting a manuscript at Oxford, said to contain the computation of the ratio of the diameter of a circle to its circumference to 154 places of figures.

I have not seen the manuscript to which I have alluded in my paper on the Rectification of the Circle in the Philosophical Transactions; but I have no doubt whatever that such a manuscript does exist, and my belief is strengthened by the fact that the figures in the ratio are there correctly computed

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* Communicated by the Author.

as far as 152 places, the extent to which I have verified the approximation. This manuscript is also alluded to by the writer* of the article, "Quadrature of the Circle," in the Penny Cyclopædia, vol. xix. No. 1190, p. 187; and if Mr. Halliwell will look into that article he will find it there stated, that Baron Zach informed Montucla that he *had seen* a manuscript in the Radcliffe Library, at Oxford, in which the ratio of the diameter of a circle to its circumference was carried to 154 places. Professor Davies, of the Royal Military Academy, in his twelfth edition of Hutton's Course of Mathematics, vol. i. p. 476, also refers to the Oxford approximation, probably from the same source as myself, viz. that of the Penny Cyclopædia. Should Mr. Halliwell be successful in his search in the Radcliffe Library, it is to be hoped that he will publish an analysis of the contents of this curious manuscript, and give the method of procedure adopted for the accomplishment of such a laborious task as the computation of the ratio of the diameter to the circumference to 154 places of figures.

Royal Military Academy, Woolwich,
January 28, 1842.

XXXIV. *On* Fernel's *Measure of a Degree, in reply to* Mr. Galloway's *Remarks. By* Professor De Morgan.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In making some reply to the remarks which Mr. Galloway has made on my correction (as I am obliged still to maintain it to be) of Montucla and Delambre (or Lalande), I must first express my gratification at the subject having been taken up with so much research.

The question between Mr. Galloway and myself lies in little compass: he contends for the *geometrical* foot of Fernel being the French foot of the day: I maintain this to be unlikely in itself, as being contrary to usual practice, and wholly inadmissible, as by implication making Fernel declare that the step of an ordinary man was more than 38 English inches. As this last point however is a new one, raised by me in my second letter (of last month), I will here confine myself to the first point, namely, as to the question whether the geometrical foot is to be taken to be the French foot.

I will also omit all question as to whether the French foot

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* Probably Professor De Morgan.

of 1528 is to be assumed as agreeing with that of 1668, to within its twentieth part. In the tract of John Buteo of Dauphiny on Noah's Ark (1559), he has given a *semipes regius* printed from a line of metal. This agrees so nearly with the French measure of our own day, that it may be considered as probable that no important change took place from 1528 to 1559, seeing that none has taken place from that time to our own day. I have another *semipes regius* in the work of one Glareanus (1572) agreeing with the former to the thirtieth of an inch. Both should be lengthened to allow for the shrinking of the paper.

In my first communication, I mentioned the "famous Italian mile, the universal standard of the middle ages," taking it for granted that no dispute could arise on the subject. Mr. Galloway has taken me to mean by "Italian mile," a mile used by the Italians, of which he asserts, and correctly, that there were many. He requires it to be shown that there was only one Italian mile in use in the days of Fernel, and that Fernel knew its precise value. The first I shall endeavour to do, namely, to show that the *mathematicians* of the sixteenth century had a mile among themselves, or believed they had such a mile, which they called Italian; and also that there could have been no very great difference between the (so-called) Italian mile of one writer and another. As to the second point, namely, that Fernel knew its precise value, though I believe he did know it as well as others, yet it is only necessary for me to establish a common usage: I do not see how I can be required to show that Fernel was cognizant of notions prevailing in his day and country.

There was among the writers of the fifteenth and preceding centuries a system of measures derived most expressly from the human body, founded on the *finger-breadth*, which was assumed to represent four grains of barley placed side by side (not end to end as in the English inch). In this system mathematicians expressed lengths, always using the Italian mile, frequently the German mile, sometimes others. The Italian mile was believed to be the old Roman mile, the German mile was mostly used by writers of that country, and so on: but to avoid the diversity of measures which actually existed even in each country, and the inconveniences arising from want of compared standards, the mathematical writers of all countries settled their miles by convention founded on an appeal to the finger-breadth, palm and foot, which, if their own expressions are to be trusted, were actually considered as represented by the lengths bearing their names. Fernel, as well as others whom I shall cite, gives a complete table, as follows:—

4 barley-corns are	1 digit.
4 digits	1 palm[*].
4 palms	1 foot.
1½ foot	1 cubit.
10 palms (2½ feet)	1 step.
20 palms (5 feet)	1 pace.
10 feet are	1 perch.
125 paces	1 Italic stadium.
1000 paces	1 Italic mile.
4000 paces	1 German mile.
5000 paces	1 Swiss mile.

This system is found in manuscripts, but was particularly conspicuous from the invention of printing up to A.D. 1570. It sank gradually, as actual measurement began to prevail, and was hardly understood by the middle of the seventeenth century. This table appears also in the Cosmography of Gemma Frisius (1548); in that of Apian (printed at various times from 1529 up to 1570), in which the measures are illustrated by drawings from the human body; in Stofler's Commentary on Proclus (1534); and in many other works which I have seen. From its constant occurrence, always accompanied by reference to the human body, and never by division of the foot into twelve inches, it may be considered as highly probable that the geometrical writers found a reference to the parts of the human body more convenient and less liable to error than an attempt to write in the measures of one country for the readers of all. In our own day few authors think of more than their own countrymen: it was otherwise when a learned language was in use. The *vernacular* writers soon made the confusion of not distinguishing the feet of their own country from the geometrical foot. Their predecessors made the distinction; their only connexion with the actual measures of the world being their belief that the geometrical Italian mile was the old Roman mile, in which they were not far wrong; and that their geometrical German mile was a representative of some one mile used in Germany, nearly.

I had formed the preceding opinion from such circumstances as I have referred to, and many others; being certain of this much, that an "Italian mile," believed to be the ancient Roman mile, was universal. I lately found a strong confirmation of my opinion, in all its details, namely, that the same thing is asserted by Clavius (who died in 1612 at the age of 75) in his Commentary on Sacrobosco (I quote the edition of 1618). His words are, "... enumerandæ sunt mensuræ *quibus mathematici, maximè geometriæ, utuntur.* Mathematici enim, ne confusio oriretur ob diversitatem mensurarum in variis regionibus (quælibet namque regio proprias habet propemodum mensuras) utiliter *excogitârunt quasdam*

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[*] It appears from the drawings that the palm was measured across the middle joint of the fingers.

mensuras quæ certæ ac raræ apud omnes nationes haberentur." He then gives the preceding table. Clavius would be, I should think, a better authority than Riccioli on a habit of the sixteenth century.

It is not then a thing to be taken without further showing that Fernel, a mathematician, writing for mathematicians, used a French foot when he says he used a geometrical foot. Even without the assertion relative to his own pace, as compared with the geometrical pace of five foot-lengths, it seems to me that there is ample ground on which to contest such an assumption. If the writers of the sixteenth century had had anything to do with the usual measures, they would sometimes have mentioned their diversity; instead of which there is a uniform determination to avoid speaking of local measures, and a continual reference to a system which is adopted in the same words, with the same descriptions, and by writers of all countries, Italian, German, French, or Belgian. That this system was in practice a "curious fiction," is perfectly true, since, though the difference between the results of one careful appeal to the natural standard and another might be rather small, there was no certainty of any agreement of even what would then be called the closest kind. But, let their mode of measurement be ever so defective, it *was* their mode of measurement, and Fernel must be held to have had recourse to it until some reason is given for supposing that he chose to be an exception.

I remain, Gentlemen, yours faithfully,
University College, Feb. 12, 1842. A. De Morgan.

XXXV. *Considerations on the Insalubrity of the Air of the Maremma. By* M. Paul Savi, *Professor in the University of Pisa.*

[This memoir was read to the Geological Section of the Scientific Meeting held at Pisa in October 1839, and published in the *Nuovo Giornale dei Litterati*, Nos. 106 and 107. We have taken it from the *Annales de Chimie et de Physique* for November last, in which it follows a translation of Professor Daniell's paper 'On the Spontaneous Evolution of Sulphuretted Hydrogen,' &c. (Phil. Mag., Third Series, July 1841.) The French editor's observations we also give.—Edit. *Phil. Mag.*]

THE important memoir of M. Daniell having directed the attention of philosophers to the production of sulphuretted hydrogen by the mutual action of sulphates and organized bodies, we have deemed it proper to reprint M. Savi's memoir, which is but little known, but in which the same reaction is indicated as one of the most powerful causes of malaria.

It is generally known that several parts of Tuscany and various regions of the south of Italy are afflicted with the scourge described in the country by the names of *cattiv' aria* and *mal aria* (bad air), common language thus appearing to attribute the morbid effects to the constitution of the atmosphere in these localities. Examinations into the causes of the unhealthiness of various regions of the Tuscan soil have previously occupied several philosophers, and the author of this memoir has endeavoured to throw light on this important question at a period in which the generous efforts of the sovereign are directed towards improving the salubrity of these localities.

M. Savi readily admits that he is not prepared to propose a remedy for the scourge: the object of his memoir, which is principally geological, is to review the different unhealthy localities, to examine carefully the constitution of their soil, and particularly to describe those regions in which the unhealthiness appears not to result from the usually admitted causes, in order to discover in the phænomena peculiar to these places the origin of their insalubrity; the information acquired in these particular cases probably leading to the discovery of a common cause of the noxious effects in various localities, and which may have been attributed to very different causes.

We shall follow the author through the several parts of his memoir.—[Edit. *Annales de Chimie et de Physique.*]

Insalubrity of the Environs of Volterra.

M. Savi begins by examining the low valleys of the environs of Volterra, in which the non-existence of marshes precludes the most common hypothesis, as to the origin of their unhealthiness. This country is formed by very extensive marine deposits of the tertiary epoch, and which are chiefly composed of gray argillaceous marls (*mattajone*); these lands, raised in many places by igneous rocks, and altered in others by subterraneous emanations, frequently contain selenitic masses impregnated with sulphur, and often with common salt; the igneous rocks constitute the peaks of the mountains; its sides are formed of this *mattajone*, raised, altered and impregnated with gypsum and common salt; the valleys are also formed of *mattajone*.

In the bottom of these valleys, not only near water-courses, but also on the sides of the hills, and even at a certain height, malaria exists to such a degree, as not only annually to attack a great part of the inhabitants with obstinate intermittent fevers, but also with fevers of a more dangerous description.

The author is induced to disregard the hypothesis which attributes the unhealthiness to sudden changes of temperature, moisture, &c.; for other localities, very unfavourably circumstanced in these respects, are not subject to the same morbid influences.

He notices another opinion existing in Tuscany, which, though absurd at first sight, appears to him not to be destitute of some foundation. It is stated, that these lands, after having been dried by the heat of the summer, and then subjected to rain water, undergo a kind of fermentation; that the earth *boils* (*ribolle*), as they commonly say; and that, in consequence of this ebullition, deleterious miasmata are disengaged, which are the source of diseases, and particularly of intermittent fevers. It is quite certain that diseases do not commence, or at least do not become common, until after rain and inundations have occurred. The greater the alternations of heat and rain which occur in a year, the more are the ravages of fever felt. This is a fact ascertained by experience, and which no inhabitant of the *maremma* would deny. It is cited by many authors, especially by the celebrated Brocchi*.

It is also asserted, that these circumstances are productive of fevers, not only in marshy soils, but also in certain situations that contain no marshes, such as that of the environs of Volterra. Instead therefore of saying, as often happens, that the morbid effects are derived from a mixture of rain and stagnant water, it would be more rational to say that they are due to the action of water on certain parched lands.

Examination of the Substratum of unhealthy Marshes.

M. Savi begins by observing, that all marshes do not appear to be capable of evolving hurtful effluvia; hence the necessary distinction between unhealthy and indifferent marshes. It is, however, well known that the latter contain scarcely any salts in solution, and the substratum yields no marine mineral products. Such is the marsh of Bientina, and also that of Maciuccoli. The unhealthy marshes are those which hold in solution a notable proportion of salts, and they may be divided into three classes, according to the source of the salts:—1. those of mineral waters (lake of Rimigliano, &c.); 2. those of sea water; 3. those of land formerly occupied by sea water (marshes of Castiglion della Pescaja, of Scarlino, &c.). In the Tuscan *maremma* the unhealthy marshes belong to the two latter classes. They are for the most part small ancient gulfs, at first changed into low lands by the alluvium of rivers, and then more or less separated from the sea by an accumulation of sand heaped up by the action of the winds and waves.

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* *De l'Etat Physique du Sol Roman*, page 276.

The third class, though deprived of all communication with the sea, have a bottom of marine mud; this origin is sufficiently proved by the shells which it contains, and especially by the *Cardium edule* and the leaves of algæ which are found on digging. In summer these marshes become dry, and saline efflorescences are found on the surface.

Marshy Lands recently improved.

M. Savi confirms the fact previously known and described by Count Fossombroni, first in his dissertation on the Val di Chiana, and then in his report on the low lands (*maremmes*) of Tuscany, presented to the grand duke*, that marshy land, dried by draining and covered with artificial soil or *colmate*, continues to be for a long time a source of insalubrity; the improvement takes place slowly, and requires years for completion; it seems as if the healthy earth, which covers the former marshy bottom, must acquire a certain degree of thickness and compactness before it can replace the soil which was noxious under atmospheric influence.

The existence of common salt in these dangerous soils has not escaped the notice of Count Fossombroni, who observed its influence, and has described by the name of *salmastraje*, this sort of dried lands, producing efflorescence in dry seasons. These *salmastraje* are incapable of sustaining the greater number of plants which grow in good soils; when they spread, the vegetation of contiguous soils becomes languishing, and is eventually destroyed. Gradually, however, fresh vegetation takes place, and of the only kinds which suit these soils: among them occur *Atriplex*, *Salsola*, *Statice*, &c.

M. Savi compares the soil of Volterra to these *salmastraje*, as containing elements capable of undergoing modifications analogous to those which are produced in the *salmastraje*.

The soil consisting of *mattajone*, interspersed with eruptive rocks, and subject to subterranean emanations, contains gypsum, sulphur, and common salt, accompanied with sulphate and carbonate of soda, &c.; and to these must be added an oleo-bituminous substance, which in hot seasons yields evident exhalations, and whenever fresh surfaces of the *mattajone* are brought into contact with the air. In spite of its sterility this soil exhibits some traces of vegetation, so that to the substances already noticed as contained in it, must be added the remains of dry vegetables in a state of decomposition. These lands appear therefore to resemble in many respects marshy lands rendered unhealthy by rains. Water acts readily on

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* Inserted in the work of M. Ferdinand Tastini, 'On the Improvement of Tuscan Low Lands.'

these soils in consequence of their peculiar nature and the absence of vegetation, and being thereby furrowed and ravined in all directions, fresh surfaces are continually exposed to the action of the atmosphere.

Nor can it be questioned that this soil exhales gases which are unfit for respiration. The ventilation of wells and adits opened in *mattajone* is very difficult of execution, and frequently requires the use of furnaces to support respiration and combustion. Does this circumstance produce, or not, any noxious effect? This question is put, but not answered by the author.

Insalubrity produced by Mineral Waters.

It has been long known, and philosophers have testified the fact, that salt water, when mixed with marshy waters, occasions insalubrity: pestilential marshes have been rendered almost completely healthy when the access of salt water has been prevented. The most striking example of this fact occurs in the memoir of M. Giorgini, 'On the Marshes of Pietrasantino and Montignosino' (*Ann. de Chim.* xxix.). M. Savi has discovered examples of similar effects in the influence of mineral waters. Such was the lake of Rimigliano, situated between Torre San-Vincenzo and the promontory of Populonia. This marsh ceased to exist in 1832. Before this period it received, by the *fossa calda*, the mineral and thermal waters of the source of the Caldana, near Campiglia. These waters contain bicarbonate of lime and magnesia, chlorides of calcium and magnesium, sulphates of soda, lime and magnesia. The waters of the ocean had no access to this lake.

The bottom of the lake, lying on a black stratum of marine origin, was formed of a yellowish white substance; it was of a pasty, and sometimes of a gelatinous consistence, and filled with fragments of the stalks of the *Chara hispida* (the only plant vegetating in this marsh), in a state of decomposition. When this slime was stirred, it emitted an intolerable smell: this, after analysis, was attributed by the author to sulphuretted hydrogen, and to a peculiar organic substance (*putérine*); the solid portion of the deposit was a mixture of organic matter, carbonate and sulphate of lime, &c. The mineral waters conveyed by the *fossa calda* being diverted by another channel, and the lake running into the sea, the bottom of the marsh was soon covered with flourishing vegetation. Were the malignant emanations of lake Rimigliano derived from different causes from those which exist in common submarine marshes? It will be observed that this locality exhibits two circumstances which are peculiar to it, viz. the exclusive vegetation of the *Chara*, and the supply of the lake with thermal waters. The author performed some experiments which prove, that in a limited atmosphere, the emanations from the *Chara*, when decomposing, acted mischievously on the economy*. They might therefore in part produce the deleterious action of the waters of this lake. But as the *Chara* does not exist in many other unhealthy marshes, and does vegetate in healthy situations, the cause of the insalubrity of the air cannot be attributed to it in all cases.

An examination made by M. Savi showed that much gas emanated from the bottom of it; it contained carburetted and sulphuretted hydrogen, and these were in the largest proportion. The author, on well-known chemical grounds, attributes the existence of sulphuretted hydrogen to the reduction of the sulphates into sulphurets by the influence of decomposing organic matter: analysis showed that the proportion of sulphurets is smaller in the lake than in the mineral waters which flow into it. The author does not venture yet to decide on the question whether the insalubrity of the air is derived from the sulphuretted or carburetted hydrogen, or to putrid miasmata, the production of which would be proportional to that of these two gases; he is satisfied with remarking the prominent circumstances occurring in this locality—the presence of mineral waters in a soil containing decomposing vegetable matters, and the presence of sulphates in these waters.

Insalubrity caused by Putrefying Algæ.

The author states that this putrefaction occurs in those places in which collections of algæ are washed by fresh water mixed with sea water; the decomposing vegetables exhale a perceptible smell of putrid eggs, and these places become centres of insalubrity: intermittent and more dangerous fevers prevail there. He cites as an example the port of Vada, Porto Nuovo of Piombino, the ancient port of Talamone, &c. The presence of sulphuretted hydrogen gas has been demonstrated in the products of the putrefaction: some experiments have proved that the algæ do not putrefy in pure water so as to disengage sulphuretted hydrogen; the presence of sulphates being necessary. This putrefaction, at any rate, is not peculiar to the algæ alone, but to many vegetables carried into sea water.

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* *Recherches Physiques et Chimiques sur le Chara ou Putéra*, 1832.

Can Sulphuretted and Carburetted Hydrogen either directly or indirectly render the Air insalubrious?

The insalubrity of the air has been long attributed to these gases; this observation has been questioned with respect to sulphuretted hydrogen by many philosophers, who observed that the emanations of the *solfaterras* and *lagoni* of the *Siennois* and *Volterrano*, which contain a considerable proportion of this gas, do not occasion the marsh diseases in those who breathe the air of these localities. The same observation applies to the air of the lagoons of Venice. These incontestable facts prove that sulphuretted hydrogen is not always capable of producing fevers; but as in all the localities of marshes in which unwholesome air prevails, sulphuretted or carburetted hydrogen, and especially the former, are generated, their formation is naturally regarded as connected, at least, with the cause of insalubrity.

The author will not however affirm that insalubrity depends on one cause only: several circumstances may concur to develop noxious influences, or much to increase their malignity. Such is the effect already attributed by several philosophers to winds from the south, and the sirocco, which, stemmed by the chain of the Apennines, appear greatly to increase insalubrity, whilst the winds from the north produce favourable effects on unhealthy countries.

Conclusions.

It appears to be proved that places exposed to the effects of insalubrious air are—

1st. Lands containing collections of stagnant and salt water, or lands which are not submerged, but which contain saline matters and organic substances, when watered by the rains of summer.

2nd. Lands receiving mineral waters containing sulphates and chlorides, which rest upon decomposing organic matter.

3rdly. Shores on which masses of algæ accumulate, which are afterwards watered by fresh water, or by a mixture of fresh and salt water.

As an hypothesis resulting from the facts observed, the author particularizes sulphuretted and carburetted hydrogen gases, if not as the direct agents of deleterious influence, yet as contributing to the production of malaria. In a word, the origin of the insalubrious air is associated with the production of these hydroguretted gases.

XXXVI. *A Table of Shocks of Earthquake, from September* 1839 *to the end of* 1841, *observed at Comrie, near Crieff.* *By* JAMES DRUMMOND, *of Comrie*[*].

CONSIDERING that the theory of the Comrie earthquakes has not yet been satisfactorily established, I thought that an account, by a resident on the spot, of the shocks during the last two years might be of interest, and perhaps of value, to the readers of the Philosophical Magazine, and to those engaged in inquiring into these phænomena. It is not my intention to enter into any discussion on the question just now, but I think it right to say that these observations originated in the dissatisfaction I feel as to the theories heretofore propounded, and as to the seat of the disturbance. When I consider that the shocks are only of modern occurrence, that they date only from the opening of quarries into the basaltic dykes on each side of the river Lednoch, and that they are consequent on floods of that river, I cannot feel justified in giving my assent to the doctrine, that they are derived from the percolation of water. Neither can I admit, as is the popular belief here, that the seat of the disturbance is in any of the neighbouring mountains.

I flatter myself, also, that a register of shocks from a resident at Comrie will not be without its uses, as from greater habitude the people of this place are better able to distinguish the shocks.

Before coming to the register, I think it advisable to make a few preliminary observations as to some of the circumstances attendant on the shocks. With regard to 1839, in which the disturbances were most frequent, both the harvest and winter were, with the exception of very short periods, wet. About eleven on the morning of the 20th of October a drizzling rain came on and continued until seven in the evening of the 23rd, when it cleared up and the stars appeared; neither the sun, moon, nor stars having been seen for about fifty-six hours; at half-past seven, however, it began to rain heavily, and when the shock took place the rain increased. I do not remember that there was any strong wind at the time. On the morning of the 24th it was fair again. It is to be remarked that the rain was general, and the shock was felt over all the central parts of Scotland. It has been said that this shock was felt in all parts at one and the same instant, but I beg leave to demur to the possibility of this, as at Inverness, for instance, the sound was heard some time before the shock, showing that the sound had travelled faster.

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[*] Communicated by the Author.

In 1841, the end of June and beginning of July were wet nearly every day until the 13th of July, when we had good summer weather. After the shock the weather got cold and stormy, and on the 29th of July we had a regular hurricane from the north-east, doing much damage. After the great shock on the 30th the weather got mild: I observed that the shock was transmitted much further in the direction of the hurricane than in any other.

The greatest shocks happen in summer and harvest. The vertical shocks, I cannot find, are felt further south from the Lednoch quarries than Comrie, on the east further than Tamperran, or on the south-west than Leachkin, being all about half a mile distant. At Kinggart, the nearest house on the north and a mile off, the shocks, vertical at Comrie, &c., are felt laterally. The greatest of the lateral shocks are nothing in comparison with the greatest of our vertical shocks, and yet the people of Comrie are more afraid of the lateral shocks than of the others. It is considered here that the shocks originate in the mountains, some referring them to Dunira Hill, some to the Cluan, and some to Kinggart. Upon this I will observe, that, on the 12th of October 1839, the people living in those parts observed five shocks, while on the same day at Bruckhill and Saures eight were observed, and at Comrie ten; on the 23rd following, the people about Dunira and Cluan observed three, while at Comrie some observed fourteen, others eighteen, and some as many as twenty. On the 30th of July, 1840, the Dunira people observed six, and at Comrie fourteen were observed. In general, the shocks are not felt at equal distances in every direction: I think the greatest shocks are felt further north-east and south-west than in any other direction. The shock of the 30th of July, 1841, was not felt at Stirling, twenty-four miles south. Shocks are not felt in every place. Those who are placed on high ground do not observe them near so readily as those on low ground; and they have been felt in coal-mines at Stirling when they have not been felt on the surface. They are not felt alike in Comrie, one house built on a rock not feeling them at all so severely as the others. So slight are the shocks, that to a stranger they are often inappreciable, the ground moving very little with a shock that is felt over a diameter of twelve or twenty-four miles in diameter: it is only those that are felt forty miles and upwards in diameter which cause any alarm. With regard to any external appearance, the only thing I have heard of is from the shepherds watching on Dalengrass, who tell me that they have often observed a thin cloud stretched over the part of the Lednoch valley, which I have assigned as the seat of the disturbance.

To come to the register, I must observe that it gives the state of the river, then the number of what I call gas explosions, next the hour at which the vertical shocks occurred, the number of them, and the utmost distance at which felt; afterwards the lateral shocks, and then the tremours or waving shocks, and lastly the state of the weather. The noises, which I am inclined to consider as arising from gas explosions, are only heard at Comrie, Tamperran and Leachkin, the site of the vertical shocks.

1839.

Sept. 15. Extraordinary flood.
16. Extraordinary flood *.
Oct. 4. 1 gas explosion.
7. 1 gas explosion.
8. 1 gas explosion.
9. 1 gas explosion.
10. 1 gas explosion.—1 vertical shock at 4½ a.m., felt 20 miles.—Fair.
11. 1 gas explosion.
12. 5 gas explosions.—1 vertical shock at 1 p.m., felt 14 miles; 1 at 2½ p.m., felt 8 miles; 1 at 3 p.m., felt 24 miles; 1 at 3½ p.m., felt 12 miles.—1 tremour at 4½ p.m., felt 6 miles.—Fair, with clouds.
13. 2 gas explosions.
14. 3 gas explosions.—1 vertical shock at 2½ a.m., felt 24 miles.
15. 2 gas explosions.
16. 2 gas explosions.—1 vertical shock at 2½ a.m., felt 20 miles; 1 at 5¾ p.m., felt 12 miles.—Changeable.
17. 4 gas explosions.
18. 3 gas explosions.
19. 2 gas explosions.
20. 2 gas explosions.
21. 1 gas explosion.
22. 2 gas explosions.
23. High flood.—8 gas explosions.—1 vertical shock at 10¼ p.m., felt 100 miles.—1 lateral shock at 11 p.m., felt 30 miles; 1 at 12 midnight, felt 10 miles.—Heavy rain.
24. 7 gas explosions.—1 tremour at 5 a.m., felt 6 miles.
25. 2 gas explosions.—1 tremour at 7 p.m., felt 8 miles.—Changeable.
26. 3 gas explosions.—1 tremour at 7 p.m., felt 8 miles.
27. 2 gas explosions.
28. 3 gas explosions.
29. 2 gas explosions.

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* The floods throughout the harvest and winter were continual, and sometimes very high.

Oct. 30. 2 gas explosions.
31. 2 gas explosions.
Nov. 1. 1 gas explosion.
2. 2 gas explosions.
3. 1 gas explosion.
4. 1 gas explosion.
5. 2 gas explosions.
6. 1 gas explosion.
7. 1 gas explosion.—1 tremour at 4 a.m., felt 8 miles.—Frosty.
8. 2 gas explosions.
9. 1 gas explosion.
10. 1 gas explosion.
12. 1 gas explosion.
15. 2 gas explosions.
17. 1 gas explosion.
18. 1 gas explosion.
23. 1 gas explosion.
25. 1 gas explosion.
26. 1 gas explosion.
28. 1 gas explosion.
29. 2 gas explosions.
30. 3 gas explosions.
Dec. 1. 1 gas explosion.
2. 2 gas explosions.
3. 1 gas explosion.
4. 1 gas explosion.
5. 2 gas explosions.
6. 1 gas explosion.—1 tremour at 3 a.m., felt 8 miles.—Changeable.
7. 1 gas explosion.
8. 2 gas explosions.
9. 1 gas explosion.
11. 1 gas explosion.—1 tremour at $9\frac{1}{4}$ p.m., felt 6 miles.
12. 1 gas explosion.—1 tremour at 3 a.m., felt 8 miles.
13. 2 gas explosions.
15. 1 gas explosion.
17. 1 gas explosion.
18. 2 gas explosions.
19. 1 gas explosion.
20. 1 gas explosion.
21. 1 gas explosion.
23. 1 gas explosion.
24. 2 gas explosions.
26. 1 gas explosion.
27. 1 gas explosion.
28. 2 gas explosions.
30. 1 gas explosion.
31. 1 gas explosion.

1840.
Jan. 1. 1 gas explosion.
2. 1 gas explosion.
4. 1 gas explosion.
6. 1 gas explosion.
7. 1 gas explosion.
8. 2 gas explosions.
9. 1 gas explosion.
11. 1 gas explosion.
12. 1 gas explosion.
14. 1 gas explosion.
16. 1 gas explosion.
17. 1 gas explosion.
18. 1 gas explosion.—1 lateral shock at 9¼ p.m., felt 18 miles.—Wind and rain.
19. 2 gas explosions.
20. 1 gas explosion.
23. High floods.—1 gas explosion.
25. 1 gas explosion.
26. 1 gas explosion.
27. 2 gas explosions.—1 tremour at 6 a.m., felt 5 miles.
28. 2 gas explosions.
31. 1 gas explosion.
Feb. 4. 1 gas explosion.
6. 1 gas explosion.
9. 1 gas explosion.
10. 1 gas explosion.—1 tremour at 4 a.m., felt 6 miles.
12. 1 gas explosion.
14. 1 gas explosion.
16. 1 gas explosion.
18. 1 gas explosion.
20. 1 gas explosion.
22. 1 gas explosion.
24. 1 gas explosion.
25. 1 gas explosion.—1 tremour at 2 p.m., felt 6 miles.
26. 1 gas explosion.
28. 1 gas explosion.
March 7. 1 gas explosion.
8. 2 gas explosions.
9. 1 gas explosion.—1 tremour at 5½ p.m., felt 8 miles.—Fair.
10. 1 gas explosion.
11. 3 gas explosions.
12. 1 gas explosion.
13. 1 gas explosion.—1 tremour at 8 p.m., felt 6 miles.—Fair.
14. 1 gas explosion.—1 tremour at 9½ p.m., felt 6 miles.—Fair.
13, 14, 15, 16, Lednoch flooded by the melting of snow on the North hills.
21. 1 gas explosion.
23. 1 gas explosion.
24. 1 gas explosion.

March 25. 1 gas explosion.
26. 1 gas explosion.
27. 1 gas explosion.
April 1. 2 gas explosions.—Slight snow.
3. 1 gas explosion.
5. 1 gas explosion.
7. 2 gas explosions.—1 lateral shock at 4 p.m., felt 12 miles. —Dry and cloudy.
10. 1 gas explosion.
12. 1 gas explosion.
13. 1 gas explosion.
May 19. 2 gas explosions.—1 tremour at $1\frac{3}{4}$ p.m., felt 6 miles.—Dry and cloudy.
22. 1 gas explosion.
25.
29. 1 gas explosion.
16. Flood.—1 gas explosion.
[Apparently there is some error here, in the MS.—Edit.]
25. Flood.
July 2. 2 gas explosions.
15. 2 gas explosions.
17. 1 gas explosion.
19. Flood.
21. Flood.
25. 1 gas explosion.
August 6. 1 gas explosion.
17. Flood.
Sept. 1. 1 gas explosion.
19. 2 gas explosions.
23. Flood.
27. 1 gas explosion.
28. Flood.
Oct. 21. 1 gas explosion.
26. 1 gas explosion.—1 vertical shock at 7 p.m., felt 12 miles. —Mizzly rain.
27. 1 gas explosion.
30. Flood.
Nov. 4. Slight flood.
6. Slight flood.
12. 2 gas explosions.
13. 3 gas explosions.
14. 1 gas explosion.
16. Slight flood.—1 gas explosion.
17. 2 gas explosions.
Dec. 7. High flood.
13. 2 gas explosions.
15. Flood.—1 gas explosion.
24. 2 gas explosions.
25. 1 gas explosion.

1841.
Jan. 6. 1 gas explosion.—1 tremour at 12 midnight, felt 6 miles. —Frosty.

Jan. 10. 2 gas explosions.
15. 1 gas explosion.
17. 2 gas explosions.
22. Flood.
27. Flood.
Feb. 1. 1 gas explosion.
13. Flood.
14. Flood.—1 gas explosion.
15. Flood.
16. 1 gas explosion.
March 6. 1 gas explosion.
10. 1 lateral shock at 5 p.m., felt 15 miles.—Dry and cloudy.
19. Flood.
21 and 22. Floods.—2 gas explosions.—1 tremour at $6\frac{1}{2}$ a.m., felt 6 miles.—Fair.
23 and 30. Flood.
April 3. 1 gas explosion.
9. 1 vertical shock at 8 a.m., felt 10 miles.
12. 1 tremour at 5 a.m., felt 5 miles.
24. 1 vertical shock at $1\frac{3}{4}$ p.m., felt 10 miles.
26 and 27. Slight floods.
May 3, 4 and 10. Slight floods.
5. 1 gas explosion.
22. 1 tremour at 12 noon, felt 6 miles.
26. 1 gas explosion.
27. 1 gas explosion.
28. 1 gas explosion.
30. 1 tremour at 7 a.m., felt 5 miles.
June 21, 23 and 28. Floods.
July 2. 1 gas explosion.
10, 11 and 12. Slight floods.
23. 1 gas explosion.—1 lateral shock at $4\frac{1}{4}$ p.m., felt 10 miles.—Mild and warm.
25.
26. 1 gas explosion.
27. 1 gas explosion.
30. 12 gas explosions.—1 vertical shock at 8 a.m., felt 10 miles: 1 at 2 p.m., felt 60 miles.—Dry, cold and cloudy.
31. 1 gas explosion.—1 vertical shock at 8 a.m., felt 8 miles.—Dry, cold and cloudy.
August 1. 1 gas explosion.
2. 1 gas explosion.
3. 1 gas explosion.
5. Flood.
7. 1 gas explosion.
8. Flood.
10. 1 gas explosion.
12. 1 vertical shock at 10 a.m., felt 10 miles.—Fair.
14. Flood.
19. High flood.
22. 1 gas explosion.

Aug. 23. High flood.
30. Flood.
Sept. 1. 1 gas explosion.
2, 9 and 10. Slight flood.
10. 1 vertical shock at 11¼ p.m., felt 20 miles.—Changeable, dry and fair after the shock, and starlight at the time.
11. 1 vertical shock at 2¼ a.m., felt 15 miles.—Fair; rain immediately after the shock.
15. High flood.
16. 1 gas explosion
22. 1 gas explosion.
23. 1 gas explosion.—1 vertical shock at 1¼ a.m., felt 10 miles.—Changeable.
24. Flood.
25. Flood.—1 gas explosion.—1 lateral shock at 9 p.m., felt 8 miles.—Rain.
27, 28 and 29. Floods.
Oct. 10, 13, 14, 17 and 20. High floods.
13. 1 tremour at 12 midnight, felt 6 miles.—Fair.
23. Slight flood.—1 lateral shock at 1 a.m., felt 8 miles.—Rainy.
Nov. 3. Flood.—3 gas explosions.
5. 1 tremour at 1 a.m., felt 5 miles.—Fair.
6. 1 gas explosion.—1 tremour at 8 a.m., felt 6 miles.—Changeable.
8. 1 gas explosion.—1 tremour at 8 a.m., felt 5 miles.—Fair.
18. 1 gas explosion.—1 tremour at 8½ a.m., felt 5 miles.—Frosty.
29. Slight flood.
Dec. 1. Flood.
3. Slight flood.—1 gas explosion.
6. Slight flood.—1 tremour at 3 a.m., felt 6 miles.—Rain.
7. 1 gas explosion.
12. Flood.
15. High flood.
19. 1 vertical shock at 1½ a.m., felt 5 miles.—Frosty.

I am aware that some persons who have been keeping a list of shocks reject the very small sounds, and mark the louder ones as slight shocks of earthquakes, whether those sounds be occasioned by the explosions of hydrogen and oxygen gas or not: it is contrary to common sense to call a slight sound a shock of an earthquake. Again, it is to be observed that the people at Comrie were in constant terror during the latter part of 1839, and it is possible that people imagined they heard sounds of earthquakes when there were none: making due allowance for this, it is certain that the sounds were uncommonly frequent during that year (1839).

James Drummond.

XXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 168.]

[*Anniversary Proceedings, Nov.* 30, 1841, *concluded.*]

DR. ROGET begged leave to observe, that, for many years past, it has been customary for the President, in his anniversary addresses to the Society, to give narratives of the leading incidents in the lives, and an account of the scientific labours of the more distinguished associates of whom death had deprived us during the preceding year. The utility of such a retrospect, he remarked, is sufficiently obvious. Consolation may be afforded to the survivors by the just tribute thus publicly paid to the memory of those they mourn. In marking the several steps of their ascent to eminence, in retracing the services they have rendered to science and to mankind, and in establishing their respective claims to our respect, our admiration and our gratitude, fresh motives of emulation are presented to those who are following in the same arduous paths, and aspiring to the same honourable distinctions. The Society can never forget how well these objects have been fulfilled by the excellent biographical notices we have been accustomed to hear from our Presidents on each returning anniversary, and must feel how much reason they have to regret the omission of the usual discourse from the Chair on the present occasion. It is with a view to prevent this interruption of the series being drawn into a precedent, that Dr. Roget has now been induced, by the desire of the President, to attempt supplying, however imperfectly, the omission he has alluded to. Having but little leisure to perform this task, he wished to claim the indulgence of the Meeting for the many imperfections they will discover in the mode of its execution.

Of the deceased members on the home list, Dr. Roget has been able to notice only two, namely, Mr. Bauer and Sir Astley Cooper, not having received, with regard to the rest, any authentic information which was deserving of record in this place. It is impossible for him, however, to pass over in complete silence the honoured name of one, whose loss within these few days we all so deeply deplore—the late SIR FRANCIS CHANTREY*. But the calamity is too recent and too sudden to afford the opportunity, if indeed the effort could, under these painful circumstances, have been made, of collecting the materials for a narrative which might render adequate justice to his superior merits as an artist, and to his exemplary character as a man. This tribute to his memory must be reserved for a period when his biographer will be able to review the subject more extensively, and with more calm deliberation.

FRANCIS BAUER was born at Feldsberg, in Austria, on the 4th of October, 1758. While yet a boy he lost his father, who held an

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* He was born in 1782, and expired quite suddenly on the 25th of the present month (Nov. 1841); only five days before the present meeting.

appointment as painter to Prince Lichtenstein; so that the care of his education devolved upon his mother. He manifested very early a talent for botanical drawing; and the first published production of his pencil, at the age of thirteen, was a figure of the *Anemone pratensis* appended to a work of Stoerck. He came to England in the year 1788, and was about to proceed to Paris; when, on the eve of his intended departure, he was offered by Sir Joseph Banks the appointment of draughtsman at the Royal Gardens at Kew, a proposal which induced him to relinquish his intentions of leaving England. He took up his residence near those Gardens, and he continued to dwell, during the remainder of his life, in their neighbourhood. The salary of the new office which Mr. Bauer held was defrayed by Sir Joseph Banks during his own life, and its continuance after his decease was provided for by his will.

Mr. Bauer, in fulfilment of his engagement, made numerous drawings and sketches of the plants in the Garden; and these are now preserved in the British Museum. A selection from his drawings was published in 1796, under the title of "Delineations of Exotic Plants cultivated in the Royal Gardens at Kew," containing in all thirty plates of different kinds of Heaths. His drawings have also illustrated several papers published in the Linnæan Transactions, and particularly those of Mr. Brown. The 13th volume of that work contains a paper by Mr. Bauer on the Ergot of Rye, drawn up from materials collected between the years 1805 and 1809; and the plate which illustrates it is derived from drawings forming part of an extensive series in the British Museum, illustrating the structure of the grain, the germination, growth and developement of wheat, and the diseases of that and other Cerealia. This admirable series of drawings constitutes perhaps the most splendid and important monument of Mr. Bauer's extraordinary talents as an artist, and of his skill in microscopic investigation. The subject was suggested to him by Sir Joseph Banks, who was engaged in an inquiry into the disease of corn known by the name of *blight*; the part of Mr. Bauer's drawings which relates to that disease was published in illustration of Sir Joseph's memoir on the subject, and has been several times reprinted with it*. Mr. Bauer himself gave, in the volume of the Philosophical Transactions for 1823, an account of his observations on the *Vibrio tritici* of Gleichen, with the figures relating to them; and another small portion of his illustrations of the diseases of corn has since been published by him in the 'Penny Magazine' for 1833. His figures of a somewhat analogous subject, the apple-blight, and the insect producing it, accompany Sir Joseph Banks's memoir on the introduction of that disease into England, in the second volume of the Transactions of the Horticultural Society.

Mr. Bauer had commenced, before the close of the last century, a series of drawings of Orchideæ, and of the details of their remarkable structure, to which he made additions from time to time,

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* [See Phil. Mag., First Series, vol. xxi. p. 320.—Edit.]

as opportunities offered, nearly to the termination of his life. A selection from these, which form one of the most beautiful and extensive series of his botanical drawings, was lithographed and published by Professor Lindley, between the years 1830 and 1838, under the title of "Illustrations of Orchidaceous Plants."

A paper by Mr. Bauer, entitled "Some Experiments on the Fungi which constitute the colouring matter of the Red Snow discovered in Baffin's Bay," was published in the Philosophical Transactions for 1820. By mixing the snow containing these fungi with water, he found that they could be made to vegetate, but that they produced new fungi of a green instead of a red colour. By exposure to excessive cold the primitive fungi are killed, but their seed still retains vitality, and, if immersed in snow, which appears to be their native soil, they reproduce new fungi, which are generally of a red colour.

The Philosophical Transactions for 1823 contains the paper by Mr. Bauer already alluded to, entitled "Microscopical Observations on the Suspension of the Muscular Motions of the *Vibrio tritici*," which forms the Croonian Lecture for that year. This minute worm, which infests wheat, and is the immediate cause of that destructive disease called the *Ear Cockle* or *Purples*, congregates in immense numbers in the substance of the grains thus diseased, forming masses of a white and apparently glairy mucus, which, when immersed in water, separate and exhibit, under the microscope, the worms in lively motion. After they have become perfectly dry, and apparently lifeless, they may be readily revived by being moistened with a drop of water, when they become as lively as before. Mr. Bauer determined, by a series of experiments, that the ova of these worms are conveyed into the cavities of the germens by the circulating sap. On inserting some of the worms into sound grains of wheat, and allowing them to germinate, he found the worms, in different stages of their growth, in the stalk, and ultimately in the germens of the new plant.

In the year 1816 he commenced lending the assistance of his pencil to Sir Everard Home, in the various anatomical and physiological investigations in which the latter was engaged; and in the course of ten or twelve years furnished, in illustration of Sir Everard's numerous papers in the Philosophical Transactions, more than a hundred and twenty plates, which were afterwards reprinted in his 'Lectures on Comparative Anatomy.' These plates, which form together the most extensive series of Mr. Bauer's published works, embraced a great variety of important subjects, chiefly in microscopic anatomy, and afford abundant evidence of his powers of observation and skill in depicting the most difficult objects. It is this rare and previously almost unexampled union of the observer and the artist that has placed Mr. Bauer in the first rank of scientific draughtsmen. His paintings, as the more finished of his productions may well be termed, are no less perfect as models of artistic skill and effect, than as representations of natural objects.

He died at his residence on Kew Green, on the 11th of December last, in the 83rd year of his age*.

Sir Astley Paston Cooper, Bart., was the fourth son of the Rev. Dr. Samuel Cooper, of Yarmouth in Norfolk. His mother was a daughter of James Bransby, Esq., of Shottisham, and was known as the authoress of a novel entitled 'The Exemplary Mother.' Sir Astley was born at Brooke, in the same county, on the 23rd of August, 1768. Even in his boyhood he was noted for his bold and enterprising spirit, the sociability and kindness of his disposition, and for the animation with which he entered into all the sports of his juvenile companions. After receiving from the village schoolmaster, and from his father, who was a good scholar, some portion of classical instruction, he was placed, at the age of fifteen, with Mr. Turner, a surgeon and apothecary at Yarmouth. Here he remained but a few months, and was then sent to London, and bound apprentice to his uncle, Mr. William Cooper, one of the surgeons of Guy's Hospital, but was soon after transferred, by his own desire, to Mr. Cline, who had already attained great eminence, and was surgeon of St. Thomas's Hospital. This connexion afforded him ample opportunities of acquiring professional knowledge, under the guidance of a master distinguished by a truly philosophical mind, and for whom his pupil always felt the most profound regard and veneration. Young Cooper's labours in the wide field of observation thus open to him, both in the hospital and dissecting-room, were unremitting; and the practical information he there acquired formed the solid basis of his future fame. He made a short visit to Edinburgh in the year 1787, and, although only in his nineteenth year, was a distinguished member of the Royal Medical Society of that place. On his return to London, Mr. Cline, who was the teacher of anatomy, physiology and surgery at St. Thomas's Hospital, appointed him his demonstrator of anatomy, and soon after gave up to him a part of the anatomical lectures. Sir Astley also gained the consent of Mr. Cline and the other surgeons of the hospitals of Guy and St. Thomas, to give a course of lectures on the principles and practice of surgery, a subject which had previously only formed a part of the anatomical course. He had now full scope for the display of those talents which afterwards shone forth on the wider theatre of the world, in a profession of which he became the brightest ornament. At first he was attended only by fifty students; but his class soon increased to four hundred, which was by far the largest that had been known in London. His popularity as a teacher rapidly increased: he made no attempts at displays of oratory, but always studied to render the subject which he treated as plain and intelligible as possible to his hearers, wisely avoiding distracting their attention by entering on controversial topics connected with physiology.

On the close of 1791, the year he commenced as a lecturer, he married the daughter of Thomas Cock, Esq., of Tottenham, who was a distant relation of Mr. Cline: but as a proof of his constant soli-

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* The above account is chiefly an abridgement of that contained in the Proceedings of the Linnæan Society for 1841, p. 101.

citude never to neglect the performance of any public professional duty, it is remembered that on the evening of the day on which the marriage ceremony was performed he delivered as usual his lecture, without the slightest intimation to his class of what had happened in the morning; and even at the time when he was most fully engaged in this exceedingly laborious practice, he never omitted to deliver his regular lectures at the hospital.

In 1792, after spending some months at Paris and attending the lectures of Dessault at the Hotel Dieu, and also those of Chopart, he commenced practice in London, taking up his residence in the city, where he dwelt for many years before he removed to the west end of the town. The popularity he enjoyed as a surgeon, and the extent of his practice, have probably surpassed that of any of his predecessors: and the large fortune which he acquired was the just and honourable reward of distinguished merit and the most unremitting application.

Sir Astley Cooper was elected a Fellow of this Society on February the 18th, 1802. He had previously contributed to the Philosophical Transactions two papers: the first entitled "Observations on the Effects which take place from the Destruction of the Membrana Tympani of the Ear*," and the second containing "Further Observations on the same subject, together with an Account of an Operation for the removal of a particular kind of Deafness†." The operation of puncturing the membrana tympani for the relief of that species of deafness which arises from an obstruction of the Eustachian tube, suggested itself from observing that, in several cases, an aperture in the membrane did not essentially diminish the powers of the ear, and that even its total destruction by disease is not followed by total deafness. Several cases are described in which the operation proved successful; but of course, when deafness proceeds from any other cause, the operation is not likely to be of the least benefit.

The other professional publications of Sir Astley are exceedingly numerous; they all bear the stamp of the peculiar character of his mind: simple and unaffected in point of style, and without pretension to elegance, they contain a plain relation of facts, unbiassed by preconceived theories, the fruits of a long and extended experience, and leading to sound practical conclusions. He never sought pecuniary advantage by his publications; and while he spared no expense in the execution of such engravings as were best calculated to afford instruction, he invariably published them at a low price.

His publications relate chiefly to the following subjects, namely, the anatomy and treatment of the various kinds of hernia; of aneurism; of spina bifida; of dislocations and fractures; of exostoses; of encysted tumors; the extraction of calculi from the bladder; the structure and diseases of the breast and of the testis. Among the last subjects to which he had particularly turned his attention was the structure and functions of the thymus gland.

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* Phil. Trans. for 1800, Part I. p. 151. [Phil. Mag., First Series, vol. viii. p. 359.—Edit.]

† Phil. Trans. for 1801, Part II. p. 435. [*Ib.* vol. x. p. 86, xi. p. 268.—Ed.]

The splendid anatomical and pathological museum which he had collected and created entirely by his own industry and labour, and chiefly within the few last years of his life, at a period when the ardour of most men for scientific pursuits begins to flag, consists of nearly three thousand preparations, each most exquisitely worked out, and the whole admirably arranged. The injected preparations are of unrivalled beauty, and show that he had acquired a facility and perfection in the art of anatomical injection quite peculiar to himself.

He was latterly engaged in an experimental investigation on the functions of the different parts of the brains of the lower animals. His health had suddenly declined a short time before his death, which happened on the 12th of February, 1841.

Sir Astley was left a widower in June 1827; the year following, he married the daughter of John Jones, Esq., of Derry Ormond, in Cardiganshire. He has left no children, and has bequeathed by his will the whole of his museum to his nephew, Mr. Bransby Cooper, and he has also left some property in the funds (namely, £4000 three per cent. consols), of which the interest is to be given as a triennial prize for the best original Essay or Treatise on given subjects in Anatomy, Physiology or Surgery, to be awarded by the Physicians and Surgeons of Guy's Hospital*.

AUGUSTIN PYRAMUS DE CANDOLLE, one of the most distinguished botanists of the present age, was born at Geneva on the 4th of February, 1778. The same year is also memorable by the death of Linnæus, the father of modern botany, which took place about three weeks before the birth of one, who was destined to emulate his fame in the same department of natural history. When seven years of age, De Candolle sustained a serious attack of hydrocephalus, a disease generally so fatal in its tendency, that the present affords a remarkable instance of complete recovery, after life had been, for many days, despaired of.

Possessing a remarkable facility of writing verses both in French and Latin, and having at the same time a keen relish for the study of history, young De Candolle at first resolved to make literature his profession; aspiring, as the summit of his ambition, to the fame of being a great historian. But this dream of his youth was effaced by a new taste, imbibed during a residence in the country, where he amused himself with examining the plants of the neighbourhood, and with writing their descriptions, before he had even opened a single book on botany. The few pages he there read of the volume of nature were sufficient to captivate his affections for the pursuit which henceforth became the dominant passion of his life. The botanical lectures of Professor Vaucher, which he attended in 1794, increased his ardour, and confirmed him in the resolution he had formed, of devoting himself to the cultivation of botany as his primary object, to which all other sciences, as well as branches of lite-

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* The greater part of this memoir of Sir Astley Cooper, and especially the account of his early life, has been extracted from Pettigrew's 'Medical Portrait Gallery.'

rature, were hereafter to be deemed subordinate, and to be followed merely as recreations from severer study.

A visit to Paris, which he made in 1795, gave him the opportunity of attending the lectures of Cuvier, Fourcroy, Vauquelin, and other distinguished Professors of that period, and of forming friendships with Desfontaines and Lamarck. He always prided himself in having been the pupil of Desfontaines, in particular, towards whom he continued through life to feel the warmest gratitude and affection.

The establishment of the Society of Physics and Natural History at Geneva, which took place, after his return, under the auspices of the celebrated De Saussure, gave a fresh and powerful impulse to his exertions; as was evinced by the numerous memoirs which he presented to that Society.

The state of Geneva being, soon after this period, absorbed into the French empire, De Candolle was induced to quit that city and attend the medical lectures in Paris; a course of study which, tending to enlarge his views of the physiology of organized beings, contributed greatly to the success with which he afterwards cultivated the Philosophy of Botany. While at Paris, he founded, in conjunction with his friend M. Benjamin Delessert, the *Société Philantropique*. One of the first advantages resulting to the public from this institution was the distribution of economical soups throughout the different quarters of the city. Of this institution he was the active secretary for ten years; during which period another society was also formed under his direction and management for the *Encouragement of National Industry*.

In 1804 he gave lectures on Vegetable Physiology at the Collège de France, and published an outline of his course in 1805, in the *Principes de Botanique* prefixed to the *Flore Française*.

In 1806 he was commissioned by the French Government to collect information on Botany and the state of Agriculture through the whole of the French empire, the limits of which, at that time, extended beyond Hamburgh to the north, and beyond Rome to the south. Every year, during the following six years, he took a long journey in the fulfilment of the task assigned him, and drew up a report of his observations for the minister. In these annual reports, however, he did not confine himself to the special objects of his commission, but made known his views with regard to the internal administrations of the countries he visited, suggesting at the same time measures for their amelioration and for the correction of existing abuses. He had projected a great work on the agricultural state of the empire, and had even executed considerable portions of it, comprehending the French Flora arranged according to modern views of classification, when the political events of 1814 put an entire stop to the work.

In 1807 he was appointed Professor of Medicine at Montpellier; and in 1810, a chair of Botany was instituted in the same Academy, which he was invited to occupy. Under his superintendence, the Botanical Garden of that city was more than doubled in extent, and the study of Botany assumed a degree of importance it had never before possessed. De Candolle quitted Montpellier in 1816, very much to the regret of the students and of his colleagues, who employed every means in their power to induce him to remain among them: but his country had been restored to liberty, and he was firm in his determination to fix himself in his native city, and devote to its services the remainder of his days.

Soon after his return to Geneva he was appointed to the chair of Natural History, an office which had been created expressly that he might occupy it. Among the first of the public benefits which he conferred upon his countrymen was the establishment of a Botanic Garden. The government of Geneva willingly lent their aid in forming so laudable an institution, in which he was also assisted by a great number of voluntary subscribers. The enthusiasm which he inspired for his favourite science was remarkably displayed on one particular occasion, when, being desirous of procuring for Geneva a copy of a Flora of Mexico which had been deposited with him for a few days, an appeal which he made to the public was responded to with such alacrity, that in the course of eight days, one thousand drawings had been finished by amateurs, who volunteered their services on the occasion.

The activity and powers of De Candolle's mind were displayed in a multitude of objects of public utility, the furtherance of which ever called forth in him the most lively interest;—whether it was the improvement of agriculture, the cultivation of the fine arts, the advancement of public instruction, the diffusion of education, or the amelioration of the legislative code. Feeling deeply of what vast importance to the welfare of mankind it is that sound principles of political economy should be extensively promulgated and well understood by all ranks of men, De Candolle never failed to develope and enforce those principles in his lectures and popular discourses, as well as in his official agricultural reports. On these subjects, and especially with respect to the immense advantages which would accrue to the community from the unrestricted freedom of commerce, his views were those of the most enlightened policy, and exhibited a sagacity in advance of the times in which he lived.

As a lecturer, he possessed in an eminent degree the power of imparting to his auditors the enthusiasm which glowed within his own breast for the pursuits of natural history. Complete master of the subject of his discourse, his ample stores of knowledge never failed to supply him with illustrations; and even in his extempore effusions, all his ideas were developed in the clearest order, and explained with singular perspicuity *. His chief delight was to afford assistance of every kind to such students as needed it, and in whom he perceived

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* The substance of De Candolle's popular courses of lectures on the physiology of plants is contained in 'Conversations on Vegetable Physiology; comprehending the Elements of Botany, with their application to Agriculture,' by the accomplished authoress of 'Conversations on Chemistry,' 'Natural Philosophy,' and other well-known works The first edition appeared in 1829.

a desire of improvement. His great aim was to inspire and diffuse a taste for the study of botany by rendering it popular among all ranks. His library, which contained the richest collection of works on that subject, and the volumes of his *hortus siccus*, were always open to those who wished to consult them. Often has he been known to discontinue researches which he had commenced, on finding that a similar design was entertained by another person; and he hastened, on these occasions, to communicate to this inquirer his own views on the subject, to place in his hands the materials he had collected, and to put him in possession of the fruits of his own experience. His sole object was the advance of knowledge; and whether this was effected by himself or by others was to him a matter of total indifference.

De Candolle had been visibly declining in health for some years before his end. The sudden death of Cuvier had impressed him with the apprehension that a similar fate might be impending; and that he himself might, in like manner, be cut off before he had accomplished the great works in which he was then engaged. He, in consequence, resolved to set aside all other occupations, and concentrate all his efforts in completing those more important designs. During the last year of his life he undertook, with the vain hope of improving his strength, a long journey, in the course of which he attended the scientific meeting held at Turin, where, as might be expected, he met with the most flattering and cordial reception. His death took place on the 9th of September, 1841, in the 64th year of his age*.

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* An oration by M. Rigaud, the Syndic of Geneva, pronounced at the "*Conseil Réprésentatif*," on the 27th of September, is the source which has supplied the information here given with regard to De Candolle. The following is a catalogue of such of his works as are in the library of the Royal Society:—

1. Essai sur les propriétés médicales des plantes, comparées avec leurs formes extérieures et leur classification naturelle. 8vo. Paris, 1816.

2. Regni vegetabilis systema naturale; sive ordines, genera, et species plantarum secundum methodi naturalis normas; vol. 1 et 2: 8vo. Parisiis, 1818 et 1821.

3. Théorie élémentaire de la Botanique, seconde édition, 8vo. Paris, 1819. (The first edition appeared in 1813.)

4. Prodromus systematis naturalis regni vegetabilis; sive enumeratio contracta ordinum, generum, specierumque plantarum hucusque cognitarum, juxta methodi naturalis normas digesta: partes I.—IV. 8vo. Parisiis, 1824—1830.

5. Mémoire sur la famille des Légumineuses; 4to. Paris, 1825.

6. Plantes rares du Jardin de Genève; livraisons I.—III.; 4to. Genève, 1826.

7. Organographie Végétale, ou Déscription raisonnée des plantes; 2 vols. 8vo. Paris, 1827. (This work has been translated into German by Meissner, in 1828.)

8. Collection de mémoires pour servir à l'histoire du Règne Végétal: 1°. Mémoire sur la famille de Mélastomacées; 2°. Mémoire sur la famille des Crassulacées: 2 vols. 4to. Paris, 1828.

9. Mémoire sur la famille des Ombellifères; 4to. Paris, 1829.

Simon L'Huillier, for many years Professor of Mathematics at Geneva, was born in that city on the 24th of April, 1750. The rapid progress which he made in his collegiate studies was viewed with so much interest by one of his relations, a minister of the reformed church of Geneva, that he bequeathed him a large portion of his fortune, on the express condition that he would embrace the clerical profession: but young l'Huillier, feeling no inclination to the studies which this condition would have imposed upon him, resisted the temptation, and preferred devoting himself to the pursuits of abstract science. The spirit of independence evinced by this sacrifice, together with the extraordinary aptitude he displayed for mathematical acquirements, excited the interest and conciliated the affection of another of his relations, the celebrated Le Sage, by whose instructions and counsels the most salutary influence was exercised over the studies of his pupil. Bertrand, who then occupied the chair of Mathematics in the same college, was also one of those who discerned in l'Huillier the dawn of genius; and even at that early period he regarded him as destined to be his successor in that professorship.

As l'Huillier advanced to manhood, it became necessary for him to engage in some active employment, in which he could turn to account his academical attainments. He had the good fortune, at this critical time of his life, to be chosen tutor to Prince Czartorynski, with whom he remained for a period of thirteen or fourteen years; ever honoured with the friendship and respect of all the members of the Prince's family. He dedicated to the father of his pupil his first work, which was published at Warsaw in 1782, under the title of *De relatione mutuâ capacitatis et terminorum figurarum, geometricè consideratâ; seu de Maximis et Minimis pars prior elementaris*, and in which he treats geometrically, and with singular elegance and vigour of demonstration, all the elementary problems relating to isoperimetric figures and solids. About the same time he presented to the Academy of Berlin a memoir, which was afterwards published in its Transactions, on the minima relating to the figure of the cells of bees, a subject which he appears, in that paper, to have exhausted *.

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10. Mémoire sur la famille des Onagraires; 4to. Paris, 1829.
11. Mémoire sur la famille des Loranthacées; 4to. Paris, 1830.
12. Mémoire sur la famille des Valerianées; 4to. Paris, 1832.
13. Cours de Botanique; seconde partie. Physiologie Végétale pour servir de suite à l'Organographie Végétale, et d'introduction à la Botanique Géographique et Agricole; vol. i.—iii.; 8vo. Paris, 1832.

De Candolle was also the author of an essay on Geographical Botany, prefixed to the second volume of the 'Flore Française' (1805).—Of the article "Géographie botanique et agricole," in the 'Dictionnaire d'Agriculture,' published in 1809.—Of the article "Géographie botanique," in the 'Dictionnaire des Sciences Naturelles,' 1820.—And of the article "Phytographie," in the 'Dictionnaire classique d'histoire naturelle.'

[M. De Candolle's Memoir on the genus *Brassica* was reprinted from, the Transactions of the Horticultural Society, in Phil. Mag., First Series, vol. lxi. p. 87. —Edit.]

[* See Phil. Mag. Second Series, vol. iv. p. 313.—Edit.]

The prize proposed by the same Academy in 1786, was adjudicated to him for a memoir, which was since published under the title of *Exposition élémentaire des principes des calculs supérieurs.* In this masterly essay the differential calculus is derived from a principle which D'Alembert had, in the first edition of the *Encyclopédie,* so happily illustrated, and which is now so generally recognised as the basis of that calculus; namely, the doctrine of limits.

On his return to Geneva in 1789, l'Huillier published an opuscle, which acquired great celebrity, entitled *La Polygonométrie; ou de la mesure des figures rectilignes, et abrégé d'isopérimétrie élémentaire, ou de la dépendance mutuelle des grandeurs et des limites des figures*; at the conclusion of which he gives a masterly summary of his former researches on elementary isoperimetry. In this work are given several formulæ of great generality, and which, at that time, were entirely new, and were calculated to facilitate the study of numerous relations arising from the perimeters and areas of polygons. About the same period, indeed, Mascheroni published formulæ very analogous to those of l'Huillier; but the latter afterwards succeeded in showing that he had arrived at the same results by original processes.

During the tempestuous years of the revolution, l'Huillier sought in Germany the retirement so necessary to his pursuits; and chose Tubingen as his residence. The fruit of his labours during this seclusion was a work almost wholly new, which appeared at Tubingen, in 1795, under the title *Principiorum calculi differentialis et integralis expositio elementaris.*

He was invited, about this time, to the chair of the Higher Mathematics in the University of Leyden; but his attachment to his native country was too deeply rooted to admit of his accepting this flattering offer: and eventually, in June of the same year (1795), he attained the object of his highest ambition, by receiving, after a successful public competition, the appointment of Professor of Mathematics in the Academy of Geneva.

At a subsequent period he was associated with his friend and colleague Professor Prévost in the composition of several memoirs on the calculation of probabilities, which appeared under their joint names in the memoirs of the Berlin Academy. The questions treated of in these memoirs, although they do not reach the higher problems belonging to this department of mathematics, are yet resolved by methods remarkable for their perspicuity and elegance. L'Huillier published, in 1804, his *Elémens raisonnés d'Algèbre, publiés à l'usage des étudians*; in 2 vols. 8vo, a work of considerable merit, as developing with clearness the true principles by which the understanding advances from that which is known to that which is unknown.

His last work, the *Elémens d'Analyse Géométrique et Algébrique, appliquées à la recherche des lieux géométriques,* in 4to, appeared in the year 1809. It was dedicated to his former pupil, Prince Czartorynski, who was, at that time, minister of public instruction in the vast empire of Russia, but who has since become better known to Europe as the most illustrious of the exiled Poles.

The declining health of l'Huillier obliged him at length to resign a professorship which he had held during five-and-twenty years, and the duties of which he had ever discharged with the most undeviating regularity, and the most scrupulous exactness. Even while suffering acutely from a painful attack of sciatica, he insisted on being carried to his class, lest any detriment should arise to his pupils from an interruption to his lectures. Many of these pupils have subsequently distinguished themselves in their scientific career; among these may be cited one of our illustrious foreign members, Professor Sturm.

For the simplicity of his manners and the strict integrity of his character, l'Huillier was no less remarkable than for the vigour and extent of his mathematical powers: by these qualities he was endeared to his friends, and esteemed and respected by all, during a life protracted beyond the ordinary duration. His death occurred on the 28th of March, 1840, when he had nearly completed his 90th year, with a constitution, however, which had some time previously been shattered and broken down by the infirmities incident to so advanced an age*.

Félix Savart, a philosopher distinguished more especially for his researches in the science of Acoustics, was born on the 30th of June, 1791, at Mézières, the capital of the Department of the Ardennes, in France. He very early exhibited a decided turn for mechanical invention, and his greatest delight was to contrive and construct with his own hands musical instruments and apparatus illustrative of Natural Philosophy, a study of which he was passionately fond. His parents had been connected with the school of engineers at Mézières; and several of his relations having been distinguished as artists, he was himself educated with a view to the same destination. But the family afterwards removing to Metz, the path which had at first been marked out for him was abandoned, and he prepared himself for another profession, by directing his whole attention to medicine. In course of time he obtained the appointment of Assistant Surgeon in the Military Hospital. Not satisfied with this probation, he, in 1814, repaired to Strasburg for the purpose of prosecuting his medical studies in the Military Hospital of that town; and he subsequently, in 1816, took a degree in medicine in the University. He then returned to his paternal roof at Metz, with the intention of settling, and of applying himself diligently to the practice of his profession. But on being restored to the scene of his youthful occupations, the renewed sight of those philosophical instruments to which so many delightful associations were attached, rekindled in full force the innate predilection for the physical sciences, which, during so long an interval, had lain dormant in his breast. The charms of science, arrayed in her most attractive colours, glittered before his imagination, and were contrasted, in his ardent mind, with the cares, the toils, and the anxie-

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* The above account is derived from a biographical notice by Professor De La Rive, which forms part of the *Compte rendu de l'état de l'instruction publique de Genève pendant l'année scolaire*, 1839-1840.

ties of the profession in which he was embarking. He yielded to the powerful fascination, and disregarding all considerations of prudence, took the irrevocable step of abandoning the prospects which were opening in a career to which his youth had been devoted, and by which alone it had, till then, been his ambition to earn fortune, reputation and independence. Confiding in his knowledge of Acoustics, which was ever his favourite study, and in which he conceived he had made discoveries, he quitted his provincial domicile and repaired to the metropolis, as to the mart where his acquisitions would be best valued. He arrived in Paris with but scanty means of immediate support, without a friend, and unprovided with a single letter of recommendation. But Fortune took him by the hand, and favoured his first endeavour to obtain notice. He presented himself to Biot, and communicated to him his views, and the results of his researches in Acoustics. He met with the kindest reception from that philosopher, who had himself been occupied with similar inquiries, and was well qualified to appreciate the merits of Savart. Biot was ever after his friend and patron, and it was chiefly through his influence that Savart was, in the year 1820, appointed Professor of Natural Philosophy in one of the Institutions at Paris; an office which he continued to hold till the year 1827, when he was nominated a Member of the Academy of Sciences. Soon after this he was associated with Thénard, as Conservator of the Cabinet of Physics of the College of France. Thus raised to a state of independence, he had full leisure to devote himself to the science he had ever particularly cherished, and of which his labours have greatly extended the boundaries. His admirable researches on the laws of the vibrations of solid bodies of different forms and kinds, and in particular, of cords, of membranes, of rods, whether straight, or bent, or of an annular shape; of flat discs, and of solids of revolution, both solid and hollow, have furnished results of great value and importance. His investigation of the structure and functions of the several parts of the vocal organs, and his theory of the voice, both in man and in the lower animals, show great originality of research, and have thrown considerable light on a very difficult department of Physiology.

Savart was elected, in the year 1839, a Foreign Member of the Royal Society, an honour which his unconquerable prejudice against the English, and everything emanating from England, prevented his ever acknowledging. His premature death, on the 16th of March, 1840, has, unfortunately for science, arrested the brilliant career of discovery, which he was pursuing with so much ardour and success, and will, it is to be feared, deprive the world of the fruits of many of his unfinished labours*.

After the reading of the biographical memoirs, the following gentlemen were elected Officers and Council for the ensuing year:—

President.—The Marquis of Northampton. *Treasurer.*—Sir John William Lubbock, Bart., M.A. *Secretaries.*—Peter Mark Roget,

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* The materials for the above sketch were furnished by the Funeral Ora-

M.D.; Samuel Hunter Christie, Esq., M.A. *Foreign Secretary.*—John Frederic Daniell, Esq. *Other Members of the Council.*—Neil Arnott, M.D.; Francis Baily, Esq.; William Thomas Brande, Esq.; Richard Bright, M.D.; William Henry Fitton, M.D.; Sir William J. Hooker, K.H., LL.D.; William Hopkins, Esq., M.A.; William Lawrence, Esq.; Gideon Algernon Mantell, Esq., LL.D.; William

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tion on Savart pronounced before the Royal Academy of Sciences of the Institute of France, by M. Becquerel, on the 18th of March, 1841.

The following is a list of Memoirs by Félix Savart:—

1. Mémoire sur la construction des instrumens à cordes et à archet. (Paris, 1819.)
2. Mémoire sur la communication des mouvemens vibratoires entre les corps solides. (Annales de Chimie, tome xiv.)
3. Recherches sur les vibrations de l'air. (Ibid. t. xxiv.)
4. Mémoire sur les vibrations des corps solides considérées en général. (Ibid. t. xxv.)
5. Recherches sur les usages de la membrane du tympan et de l'oreille externe. (Ibid. t. xxvi.)
6. Nouvelles recherches sur les vibrations de l'air. (Ibid. t. xxix.)
7. Mémoire sur la voix humaine. (Ibid. t. xxx.)
8. De l'influence exercée par divers milieux sur le nombre de vibrations des corps solides. (Ibid. t. xxx.)
9. Note sur la communication des mouvemens vibratoires par les liquides. (Ibid. t. xxxi.)
10. Mémoire sur la voix des oiseaux. (Ibid. t. xxxii.)
11. Note sur les modes de division des corps en vibration. (Ibid. t. xxxii.)
12. Note sur les sons produits dans l'expérience de M. Clement. (Ibid. t. xxxv.)
13. Recherches sur les vibrations normales. (Ibid. t. xxxvi.)
14. Mémoire sur un mouvement de rotation dont le système de parties vibrantes de certains corps devient le siége. (Ibid. t. xxxvi.)
15. Sur la décomposition de l'ammoniaque par les métaux. (Ibid. t. xxxvii.)
16. Recherches sur l'élasticité des corps qui cristallisent régulièrement. (Ibid. t. xl.)
17. Recherches sur la structure des métaux. (Ibid. t. xli.)
18. Mémoire sur la réaction de torsion des lames et des verges rigides. (Ibid. t. xli.)
19. Note sur la sensibilité de l'organe de l'ouïe. (Ibid. t. xliv.)
20. Note sur la perception des sons graves. (Ibid. t. xlvii.)
21. Mémoire sur la constitution des veines liquides lancées par des orifices circulaires en minces parois. (Ibid. t. liii.)
22. Mémoire sur le choc d'une veine liquide lancée contre un plan circulaire. (Ibid. t. liv.)
23. Mémoire sur le choc de deux veines liquides animées de mouvemens directement opposés. (Ibid. t. lx.)
24. Recherches sur les vibrations longitudinales. (Ibid. t. lxv.)
25. Extrait d'un mémoire sur les modes de division des plaques vibrantes. (Ibid. t. lxxiii.)
26. Note sur les causes qui déterminent le dégre d'élévation des sons. (Ibid. t. lxxv.)
27. Biot et Savart.—Sur la mesure de l'action exercée à distance sur une particule de magnétisme par un fil conjunctif. (Journal de Physique, t. xci.)

H. Pepys, Esq.; The Rev. Baden Powell; George Rennie, Esq.; Lieut.-Col. William H. Sykes; Charles Wheatstone, Esq.; Rev. William Whewell, D.D.; Rev. Robert Willis, M.A.

LONDON ELECTRICAL SOCIETY.

Jan. 18.—Another communication from Mr. Iremonger was read, containing further directions for the construction of the "Hydrostatic Galvanometer." Instead of selecting the heating power of a given arrangement as the standard of graduation, as he at first suggested, he recommends now that the decomposing power should be chosen. He enters into the minutiæ of the arrangement, and illustrates by a drawing several improvements which may be introduced with advantage. The account of the original instrument was published by the Society in the last Number of their Proceedings.

The conclusion of Prof. Jacobi's paper was then read, containing descriptions of apparatus for retaining a voltaic current at a given degree of intensity; this is effected by interposing either a *liquid* or a *solid* resisting medium. It would be difficult, without the drawings, to furnish our readers with an intelligible account of the arrangement. The latter portion of the paper is devoted to a description of experiments made with this apparatus.

The Secretary then briefly communicated the success with which Dr. Leeson had employed the bichromate of potash for the last six months as a material for exciting voltaic pairs. There are several curious facts in the course of development, the result of which he anticipates ere long laying before the Society. The arrangement has been employed with much success in the deposition of metals.

Mr. Weekes's Register for December was submitted to the Society, and extracts were read. The character of the atmosphere relative to the earth during this month was (to employ electric language) mostly neutral.

XXXVIII. *Intelligence and Miscellaneous Articles.*

ACTION OF NITRATE OF LEAD ON OXAMIDE—TRISOXALATE OF LEAD. BY M. PELOUZE.

A BOILING solution of oxamide is not altered either by nitrate or acetate of lead; but add to either of these salts a little ammonia, and there is soon precipitated an abundance of small, white, brilliant laminæ, soft to the touch, and which are formed of 90·5 of oxide of lead and 9·5 of anhydrous oxalic acid; this is a new oxalate of lead, a trisoxalate $3\,\mathrm{Pbo} + \mathrm{C}^2\mathrm{O}^3$, in which the oxygen of the base and that of the acid are equal, and which consequently corresponds to oxalic acid crystallized in water.

The decomposition of the oxamide into oxalic acid and ammonia, facilitated undoubtedly by the insolubility of the new salt, is much more rapid in this case than with the alkalies and aqueous acids.

This trisoxalate or tribasic oxalate of lead is also formed when oxalate of ammonia is poured into a solution of tribasic acetate of lead; but in this case it is pulverulent, and devoid of lustre.

In whichsoever mode prepared this salt absorbs carbonic acid from the air, and is eventually converted into a mixture of carbonate and neutral oxalate. Nitrate of lead boiled with this salt is rapidly changed into neutral oxalate, whilst the nitrate becomes bibasic. Nitrate of lead, slightly heated with carbonate of lead and water, becomes monohydrated bibasic nitrate; there occurs, during the change, so rapid and abundant a disengagement of carbonic acid, that it might be supposed free nitric acid was present in the solution.—*Journal de Pharm. et de Chim.*, January 1842.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1842.

Chiswick.—January 1. Very fine. 2. Slightly overcast: sleet. 3. Clear. 4. Overcast: clear: slight snow at night. 5. Frosty: overcast. 6. Frosty: clear and fine. 7. Snow-flakes: cloudy and frosty: snow at night. 8. Sharp frost: overcast. 9. Frosty: snowing. 10. Frosty throughout with dry cold haze. 11. Drizzly. 12. Frosty: slightly overcast. 13. Snowing. 14. Cloudy and fine. 15. Frosty: fine: severe frost at night. 16. Overcast. 17. Clear. 18. Hoar-frost. 19. Foggy. 20. Hazy. 21. Foggy. 22. Hazy. 23. Clear: snowing: clear and frosty. 24. Frosty: very fine. 25. Drizzly: fine: clear. 26. Boisterous with rain. 27, 28. Clear and fine. 29. Sleet. 30. Overcast and fine. 31. Hazy: heavy rain at night.

Boston.—Jan. 1. Cloudy. 2. Fine. 3. Cloudy: snow early A.M. 4. Fine: snow early A.M. 5. Snow: rain A.M. and P.M. 6. Cloudy. 7. Fine. 8—11. Cloudy. 12. Snow. 13. Cloudy. 14. Snow. 15. Cloudy. 16. Cloudy: rain A.M. 17. Cloudy. 18. Fine. 19—21. Cloudy. 22. Cloudy: snow P.M. 23. Fine. 24. Fine: heavy snow at night. 25. Snow. 26. Cloudy: stormy with rain A.M.: rain P.M.: stormy night. 27. Cloudy. 28. Fine. 29. Cloudy. 30. Fine. 31. Cloudy.

Sandwick Manse, Orkney.—Jan. 1, 2. Foggy. 3. Cloudy. 4. Cloudy: clear and frosty. 5. Clear and frosty: aurora borealis. 6. Clear and frosty. 7. Frosty. 8. Clear: rain. 9, 10. Cloudy: drizzling. 11. Clear: frost. 12. Clear: cloudy. 13. Cloudy. 14. Cloudy: clear: frost. 15. Frost: aurora borealis. 16. Cloudy: dropping. 17. Clear: aurora borealis. 18. Damp. aurora borealis. 19. Cloudy. 20. Cloudy: clear. 21. Clear: cloudy. 22. Rain. 23. Snow-showers. 24. Snow lying: sleet. 25. Clear. 26. Rain. 27, 28. Sleet-showers. 29. Sleet-showers: fine: frosty. 30. Rain: clear. 31. Cloudy: clear.

Applegarth Manse, Dumfries-shire.—Jan. 1, 2. Fog and rain. 3—5. Frost and clear. 6, 7. Frost but cloudy. 8—10. Dull and cloudy, with frost. 11. Clear frost. 12. Frost, but threatening change. 13. Fall of snow: frost P.M. 14. Snow continuing: frost. 15. Clear frost: snow lying. 16. Thaw: snow and sleet. 17. Frost again: snow lying. 18. Frost: fog. 19. Thaw: rain: snow melting. 20. Thaw, but no rain. 21. Frost, pretty severe. 22. Rain, hail and sleet. 23. Snow: clear frost. 24. Snow-drift: frost. 25. Frost: clear: snow lying. 26. Snow: wind: sleet: stormy. 27. Clear frost: one shower. 28. Partial thaw. 29. Frost: clear. 30. Frost A.M.: thaw and rain P.M. 31. Thaw with slight rain.

Sun shone out 16 days. Rain fell 7 days. Snow 6 days. Frost 21 days. Hail and sleet 2 days. Fog 3 days.

Wind east-north-east 1 day. East 11½ days. South-east 6 days. South-south-east 2 days. South 2½ days. South-west 1 day. West-south-west 1 day. West 1 day. West-north-west 1 day. North-west 3 days. North-north-west 1 day.

Calm 19 days. Moderate 6 days. Brisk 1 day. Strong breeze 1 day. Boisterous 3 days. Stormy 1 day.

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary*, Mr. Roberton; *by* Mr. Thompson, *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *by* Mr. Dunbar, *at Applegarth Manse*, DUMFRIES-SHIRE; *and at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.								Thermometer.											Wind.					Rain.					Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8¾ a.m.	Dumf.-shire.		Orkney, Sandwick.			London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R.S. 9 a.m.
1842. Jan.		Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mm.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.												
1.	30·230	30·197	30·171	29·75	29·97	30·01	30·09	30·14	36·3	42·0	35·0	38	27	35	45	42	41	41		sc.	se.	sw.	sw.	se.	......	......	......	......	·03	34
2.	30·192	30·170	30·053	29·84	30·05	30·05	30·18	30·20	34·7	40·7	33·3	37	25	34·5	45	41	41	41		n.	nw.	calm	e.	se.	......	......	......	......	·01	33
☾ 3.	30·022	30·013	29·947	29·75	29·99	29·95	30·17	30·15	34·2	38·8	33·0	37	22	33	36	31½	38	37½		n.	nw.	calm	e.	se.	......	......	......	......	......	32
4.	29·944	30·005	29·929	29·66	30·00	30·10	30·24	30·30	30·3	34·5	28·2	34	29	31	36	29	35	36½		n.	n.	calm	c.	calm	......	......	......	......	......	28
5.	30·050	30·051	30·008	29·66	30·09	30·07	30·24	30·21	32·7	34·0	30·6	36	30	33	37½	28½	36	30		w.	n.	calm	e.	calm	......	......	......	......	......	30
6.	30·180	30·311	30·168	29·85	30·15	30·30	30·30	30·40	34·7	37·3	32·9	37	23	34	36	31	29	35		nw.	ne.	nw.	se.	calm	......	·01	·39	......	......	36
7.	30·490	30·450	30·445	30·16	30·37	30·41	30·39	30·34	29·5	37·7	29·6	35	28	26	35	26	32	36½		n.	ne.	calm	wsw.	calm	......	......	......	......	......	29
8.	30·496	30·410	30·316	30·16	30·35	30·23	30·20	30·10	31·2	35·2	29·5	34	28	31·5	35	29	34	39½		n.	e.	calm	se.	se.	......	......	......	......	......	26
9.	30·238	30·233	30·140	30·00	30·14	30·07	30·05	30·05	29·2	34·0	28·8	30	27	29·5	34½	32	42	40		n.	ne.	calm	s.	s.	......	......	......	......	·12	25
10.	30·128	30·125	30·027	29·82	29·97	29·84	29·94	29·84	30·3	31·0	28·8	31	25	29	32	27½	39	36		n.	se.	calm	sse.	se.	......	......	......	......	·10	23
● 11.	30·002	30·007	29·984	29·65	29·85	29·87	29·93	29·98	33·8	34·5	30·4	34	30	30	34	27½	39	35		s.	s.	calm	w.	nw.	......	·10	......	......	·11	30
12.	30·066	30·050	29·989	29·72	29·80	29·73	29·88	29·87	33·7	36·2	32·8	36	24	32	37	23	39	40		s.	s.	calm	e.	s.	·075	......	......	......	......	30
13.	29·872	29·878	29·961	29·57	29·53	29·50	29·68	29·64	33·0	38·0	33·0	33	29	32	35½	31	39	39		sse.	s.	se.	e.	se.	......	·28	......	......	......	32
14.	29·514	29·704	29·558	29·31	29·50	29·57	29·67	29·84	36·7	37·3	30·2	36	22	32	35	29	37	32		s.	sw.	calm	e.	se.	·091	......	......	......	......	34
15.	29·948	29·953	29·919	29·53	29·78	29·70	29·96	29·76	34·2	37·7	32·5	39	20	33	32	30½	31	36		s.	sw.	calm	e.	calm	......	·02	......	......	·16	32
16.	29·782	29·900	29·681	29·48	29·45	29·58	29·62	29·71	37·7	38·2	33·0	42	31	33	36	13½	37	37½		sse.	s.	calm	se.	se.	·022	·02	......	......	......	34
17.	30·072	30·207	30·042	29·63	29·90	30·10	29·98	30·15	36·7	42·4	34·3	43	22	38	38	30	41	41		sw.	nw.	w.	nnw.	nw.	......	......	·93	0·78	·12	34
18.	30·380	30·399	30·353	30·00	30·19	30·21	30·13	30·19	30·6	42·6	30·3	33	31	29	35	19½	42	42		w.	nw.	calm	nw.	ssw.	......	......	......	......	......	32
☽ 19.	30·480	30·452	30·197	30·15	30·20	30·09	29·98	29·94	33·7	38·2	30·0	33	27	29	40½	32	45½	48		w.	sw.	calm	sse.	s.	......	......	......	......	·04	33
20.	30·174	30·098	30·028	29·92	29·95	29·88	29·93	29·86	33·4	34·6	29·3	33	32	27	40	34	43	40½		s.	ne.	calm	se.	se.	......	......	......	......	......	33
21.	30·044	30·024	30·009	29·79	29·89	29·80	29·83	29·66	34·3	35·3	33·2	35	30	32	35	24	39	40		n.	e.	calm	e & s.	s.	......	......	......	......	......	33
22.	29·880	29·881	29·285	29·28	29·42	29·14	29·32	29·25	34·3	35·3	33·8	38	29	33	36½	29	38	37		c.	se.	calm	se.	s.	......	......	......	......	·39	32
23.	29·240	29·584	29·220	28·88	29·16	29·52	29·43	29·64	32·4	38·8	32·3	38	18	31	35	30	33	32		w.	nw.	calm	ene.	se.	·119	·12	......	......	·42	32
24.	29·846	29·853	29·634	29·60	29·61	29·20	29·62	29·18	28·7	36·2	27·3	36	23	16·5	33½	20	34¼	33		w.	w.	w.	e.	se.	......	·12	......	1·16	·07	27
25.	29·318	29·745	29·327	29·14	29·22	29·48	29·20	29·43	35·3	35·6	29·3	46	35	32	38	31	36½	35		w.	nw.	calm	e.	s.	·091	·10	......	......	·32	32
○ 26.	29·446	29·518	29·175	29·19	28·85	28·77	29·18	29·07	39·3	41·8	33·0	43	21	32	38½	32	38	37		se.	s.	se.	e.	e.	·041	·14	......	......	......	33
27.	29·678	29·794	29·638	29·20	29·30	29·50	29·25	29·42	38·4	44·6	37·6	45	31	38	52	26	39	38		s var.	w.	w.	nw.	nw.	·133	......	1·08	......	·72	34
28.	29·850	29·907	29·831	29·48	29·59	29·70	29·50	29·71	36·4	44·2	36·0	45	23	34	41	32	39	35		w.	nw.	calm	wnw.	nw.	......	......	......	......	·15	35
29.	29·990	30·075	29·921	29·60	29·85	30·05	29·97	30·09	36·0	42·3	33·2	45	36	34	39	33	38	33		w.	sw.	calm	nw.	w.	......	·03	......	0·32	·29	35
30.	30·262	30·219	29·906	29·90	30·05	29·90	29·90	29·91	36·7	41·7	35·6	43	32	30	40	27	41	41		nw.	w.	calm	s.	s.	......	......	......	......	......	35
31.	30·134	30·124	29·930	29·75	29·78	29·78	29·90	29·72	40·7	41·2	36·5	47	30	36	42	35½	39	37		s.	sw.	calm	se.	calm	......	·12	......	......	·23	37
Mean.	29·999	30·043	29·896	29·65	29·80	29·81	29·860	29·862	34·2	38·1	32·0	37·80	27·10	31·6	37·5	29·2	37·90	37·48							Sum. ·572		2·40	2·26	3·28	Mean. 32

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[THIRD SERIES.]

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XXXIX. *On the Preparation of Cyanide of Potassium, and on its Applications. By* Prof. LIEBIG. *Translated by* Mr. W. FRANCIS, *and communicated through him by the Author*[*].

ONE of the best methods of preparing the cyanide of potassium is founded on the decomposition of the yellow prussiate of potash at a red heat; it is, however, attended with much inconvenience, and a third part of the cyanogen contained in the salt is lost. Considered as a compound of two atoms of cyanide of potassium with one atom of protocyanide of iron, the former undergoes no change on exposure to a red heat, while the latter is decomposed into carburetted iron with evolution of nitrogen gas. The carburet of iron, like a sponge, imbibes the fused cyanide of potassium, to obtain which free from iron and without loss a solvent such as alcohol is required. As the cyanide of potassium is possessed of properties which render it highly valuable as a means of reduction and separation in chemical analysis, I have endeavoured to simplify the preparation of it. When eight parts of the yellow prussiate are sharply dried (slightly roasted) on a hot plate of iron, then finely powdered and mixed well with three parts of dry carbonate of potash, and thrown at once into a Hessian crucible previously heated to redness, and kept at that temperature, the mixture fuses into a brown magma with a lively evolution of gas. After a few minutes, when the fluid mass has become red-hot, its dark colour is seen to become brighter, and on continued fusion the salt becomes clear and of an amber-yellow tint. If from time to time a hot glass rod be immersed in it, the adhering mass on cooling is at first brown, afterwards yellow, and at last, towards the close of

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[*] Read before the Chemical Society, March 1, 1842.

the operation, clear and colourless as water, and solidifies into a shining white crystalline mass.

During the fusion brown flocks are seen floating in the fluid mixture, which subsequently unite into a spongy mass and assume a light gray colour. If the crucible be now removed from the fire and allowed to cool somewhat, the gray powder generally settles entirely at the bottom; this is facilitated by stirring once or twice with the glass rod. The fused mass may now be easily poured into a warm porcelain dish without a particle of the sediment accompanying it. This mass consists of two combinations, of which the cyanide of potassium forms the chief portion; the other compound is the cyanate of potash, in the proportion of five of the former to one of the latter. The reaction in the fusion of the yellow prussiate with carbonate of potash is as follows:—at the commencement, the protocyanide of iron of the ferrocyanide is decomposed, and forms cyanide of potassium with the potash of the alkaline carbonate, and the protocarbonate of iron, which is deprived of all its oxygen at a higher temperature by the cyanide of potassium. In consequence of this reduction, cyanate of potash and pure metallic iron are obtained.

Let us suppose in the mixture two atoms of ferrocyanide of potassium and two atoms of carbonate of potash, we then have as sum of the constituents,

Ferrocyanide of potassium.		Carbonate of potash.
$Cy^6 Fe^2 K^4$	and	$K^2 O^2 + 2 C O^2 = Cy^6 Fe^2 K^6 O^2$ and $2 C O^2$.

And we obtain after fusion,

Cyanide of potassium.	Cyanate of potash.	Iron.	Carbonic acid.
$Cy^5 K^5$	$Cy O + K O$	Fe^2	$2 C O^2$.

From two atoms of the ferrocyanide of potassium we thus obtain five atoms of the cyanide, that is, one-fourth more than by fusing it alone. The cyanate of potash mixed with it has no injurious influence in any one of its applications. The presence of cyanate is readily detected by the effervescence caused from the escape of carbonic acid on the addition of an excess of acid, and an ammoniacal salt is afterwards found in the liquid. The explanation given above of the formation of the cyanide of potassium under the conditions described is not absolutely correct, as the protocarbonate of iron previous to its reduction is decomposed into carbonic acid, carbonic oxide, and the proto-peroxide (black oxide) of iron, at the expense of which an undeterminable quantity of cyanate is formed more than the formula indicates. The metallic iron precipitated and the sides of the crucible are covered by cyanide of potassium, to obtain which it is best to remove with cold water all that is soluble, and to warm the solution with some sulphuret of iron, which easily dissolves in it. The cyanide of potassium is thus obtained, on evaporation, again in the form of ferrocyanide, and sulphuret of potassium remains in the motherley.

Preparation of Hydrocyanic Acid.—The cyanide of potassium of the preceding process is much better adapted for the preparation of prussic acid than the ferrocyanide; a larger quantity is obtained, and the distillation is easier. It is well known, that on distilling the yellow salt with sulphuric acid a bluish-white powder is deposited, a combination of cyanogen, potassium and iron, the composition of which is analogous to that of cyanide of iron and zinc, and is represented by the formula $2\,\mathrm{Cfy} + \left\{\begin{matrix}\mathrm{K} \\ 3\,\mathrm{Fe}\end{matrix}\right.$; where $\mathrm{Cfy} = \mathrm{Cy}^3\,\mathrm{Fe}$.

From the formation and composition of this body, it results, that not more prussic acid can be obtained from five atoms of the ferrocyanide, which contain fifteen atoms of cyanogen, than from nine atoms of cyanide of potassium, which only contain nine; the other six atoms remain in the bluish-white precipitate. When the yellow salt is converted, according to the above method, into cyanide of potassium, twelve and a half equivalents of hydrocyanic acid are obtained from five atoms of the ferrocyanide, or three and a half equivalents more.

It is usual to take so much sulphuric acid to decompose one atom of the yellow salt as will suffice to form with the alkali the bisulphate of potash: on employing cyanide of potassium, only one atom of the hydrate of the acid is requisite. Equal parts of the cyanide of potassium and proto-hydrate of sulphuric acid are the best proportions for preparing hydrocyanic acid; that quantity of sulphuric acid exactly suffices to form with the whole of the potash a neutral sulphate, and with the ammonia originating from the decomposition of the cyanate, the bisulphate of oxide of ammonium. The cyanide of potassium is dissolved in double its weight of water, and the sulphuric acid diluted with three times its weight of water slowly added in small portions: previous to each addition the effervescence must have subsided.

Preparation of Cyanate of Potash.—The cyanide of potassium, prepared according to the method above described, is an excellent means of procuring easily and with very little loss the cyanate of potash. It is most advantageous to make use for this purpose of common litharge which has been previously heated slightly. The cyanide of potassium is fused in a Hessian crucible, and the powdered litharge thrown from time to time into it; the oxide of lead is instantaneously reduced to metal, which at first remains as a fine powder mixed with the cyanate formed, but melts with greater heat into a regulus. The fluid mass is poured out, and the salt, which is nothing else than cyanate of potash, being finely pounded, is boiled so long with alcohol as crystals are obtained. The crystallization of the cyanate of potash salt from alcohol is not requisite in the application of that salt to the preparation of urea.

Cyanide of Potassium as a reducing agent.—It is difficult to conceive with what extreme facility the cyanide of potassium deprives certain metallic oxides and sulphurets of their oxygen and sulphur, for it approaches nearest in that respect to pure potassium. The process of preparation of cyanide of potassium and that of the cyanate afford two instances of this reducing power; the iron remains either as powder mixed with the fused cyanate, or its particles aggregate together and form a spongy mass. A process might be founded on this reduction for determining in the dry way the amount of metal in an iron ore. When a weighed quantity of the ore is exposed with a mixture of cyanide of potassium and carbonate of potash in a porcelain crucible to a strong red heat, the alumina and silica go into the slag, from which the reduced iron can then be separated by cold water and weighed. The protoxide of manganese is not reduced by the cyanide of potassium; this, when contained in the ore, must be determined by a separate operation. When oxide of copper is sprinkled on melting cyanide of potassium, it is immediately reduced with evolution of heat and light; after washing, a regulus of pure metallic copper is obtained. The reduction proceeds most beautifully with the oxides of tin and antimony. At a low red heat the oxide of tin is converted into a bright regulus, which may easily be separated from the slag. The oxide of antimony and antimonious acid may be reduced in the same way to the metallic state. All these reductions ensue at a low red heat scarcely visible in daylight, which consequently has the advantage, that not a trace of the reduced metal is lost by volatilization. Sulphuret of tin and sulphuret of antimony are reduced by gently melting them with cyanide of potassium before the blowpipe or in the porcelain vessel, with the same ease as their corresponding oxides; the slag then contains the sulphocyanide of potassium. The cyanide of potassium does not only possess this reducing power in the dry way, but likewise in a dissolved state: mixed with a solution of alloxan, a heavy crystalline precipitate, scarcely soluble in water, of dialurate of potash, is formed in a few seconds.

Cyanide of Potassium as an agent of separation in Quanti-

tative Analysis.—Nickel, cobalt and manganese are so nearly related in their properties, that their separation is attended with great difficulties. In one single form of combination only does nickel differ from cobalt to such an extent that this might be used as an absolute means of separation.

The oxide, protochloride, or any salt of cobalt warmed with cyanide of potassium and an excess of hydrocyanic acid, is converted into the percyanide of cobalt and potassium (the cobalti-cyanide of potassium), the aqueous solution of which, according to the observation of L. Gmelin, does not undergo the slightest decomposition from boiling with hydrochloric, sulphuric or nitric acid.

The oxide of nickel and its salts are thrown down by the cyanide of potassium; this precipitate dissolves in an excess of the precipitating agent of a yellow colour; and the double compound of cyanide of nickel and cyanide of potassium, although not decomposed by acetic acid, is perfectly so by dilute sulphuric acid, and the cyanide of nickel again precipitated.

When a mixture of a cobalt and nickel salt containing free acid is treated with an excess of cyanide of potassium, so that the precipitate formed is redissolved, there are in solution free hydrocyanic acid, cyanide of potassium, cyanide of nickel, and the protocyanide of cobalt; the latter changes immediately, on being slightly warmed, into the cobalti-cyanide of potassium; if now dilute sulphuric acid be added in the cold, three cases present themselves.

If the cobalt and nickel in solution are in the proportion by weight of two cobalt to three nickel (quantities which correspond to their atomic proportions in the cobalti-cyanide of nickel), the precipitate produced is cobalti-cyanide of nickel, and is of a bluish-white colour. The filtered liquid contains not a trace of cobalt or nickel.

If the solution contains less nickel than corresponds to the above proportions, there remains in solution a certain quantity of cobalti-cyanide of potassium, and the precipitate is still cobalti-cyanide of nickel.

If there is more nickel present in the solution, the precipitate is a mixture of cyanide of nickel and cobalti-cyanide of nickel.

In the first and second cases, the precipitate produced by dilute sulphuric acid is boiled so long with the acid fluid in a vessel until not a trace of hydrocyanic acid is observed to escape (or it may be evaporated to dryness in a water-bath), and then slightly warmed with an excess of carbonated or caustic potash; the cobalti-cyanide of nickel is decomposed by this into (1) pure oxide of nickel, or the carbonate, which is washed on a filter, dried and weighed, and (2) an alkaline liquid which contains the whole of the cobalt. The latter is evaporated to dryness, some nitre being added to it, and the residuum ignited. On being treated with water, the oxide of cobalt remains behind.

This method is applicable in all analyses of cobalt ores in which the amount of cobalt predominates. For nickel ores, in which the quantity of cobalt amounts merely to a minimum, the following precaution must be attended to:—a somewhat considerable excess of muriatic acid must be taken to precipitate the cyanides dissolved in the cyanide of potassium, and the mixture must be kept boiling at least one hour.

The precipitate contains in this case cyanide of nickel intermixed, which is decomposed by potash into cyanide of potassium and oxide of nickel; this cyanide of potassium retains however another portion of nickel in solution. On boiling the precipitate with muriatic acid, the cyanide of nickel is decomposed into chloride of nickel and hydrocyanic acid, which last is removed by boiling, and no longer prevents the entire precipitation of the nickel. The cobalti-cyanide of nickel is not attacked by boiling hydrochloric acid, so that a complete solution cannot be expected when any quantity of cobalt is present. When the smell of hydrocyanic acid is no longer perceptible, the boiling has continued long enough.

Experiments made to separate the solution of the two cyanides in cyanide of potassium by boiling with peroxide of mercury, gave less certain results. The following points must be attended to in this process:—as the cyanide of potassium used contains a certain quantity of cyanate of potash, a portion of an ammoniacal salt originates on its decomposition by a mineral acid; accordingly, after boiling and the addition of caustic potash, ammonia is set free, which retains some oxide of nickel in solution. This nickel is entirely thrown down by boiling for a few minutes, or by a larger addition of potash.

The same method may be followed for the separation of manganese from cobalt, only in this case a complete solution of the precipitate produced by the addition of the cyanide of potassium to the mixture of the two metallic salts cannot be expected; the greater portion of the protocyanide of manganese remains undissolved. The residue is filtered, and the liquid treated as if cobalt and nickel were to be separated.

The cyanide of potassium is not less applicable with advantage for the separation of the oxide of chromium from protoxide of iron. A mixture of the two is previously saturated with ulphuretted hydrogen, to be certain that the iron is contained in the liquid as protoxide (an addition of a few drops of the sulphuret of ammonium answers the same purpose), and then thrown down by cyanide of potassium, and an excess of the latter added. The iron then dissolves immediately as ferrocyanide of potassium, while the oxide of chromium is left behind. In many cases the cyanide of potassium can be employed to advantage in separating iron from alumina (little iron from much alumina), as the protoxide as well as the sulphuret of iron are so easily soluble in that salt, while alumina is perfectly insoluble. The cyanide of potassium well deserves to be studied as a general agent of separation. Unfortunately the composition of the numerous double compounds it forms with other cyanides is only imperfectly known, while their relation to mineral and vegetable acids is wholly unknown, so that the entire investigation must necessarily be repeated.

XL. *Examination of Cetine, Ethal, Oils of Laurel Turpentine, Hyssop, and Assafœtida. By Dr.* JOHN STENHOUSE*.

Cetine.

CHEVREUL gave the name of Cetine to spermaceti when rendered absolutely pure. The spermaceti of commerce always contains more or less of a yellowish oil, which it retains with great tenacity, and by which its melting point is greatly lowered. The best means of purifying spermaceti, is to treat it two or three times with boiling alcohol, in which, however, it is very slightly soluble, and then to subject it to nine or ten crystallizations in æther. These solutions and crystallizations must be repeated till the temperature at which the cetine solidifies reaches 120° F. or 121° F., when it may be regarded as perfectly pure.

The cetine which I subjected to analysis, was prepared in the manner just described, and solidifies at 121° F. The following are the results:—

(1.) 0·236 gramme gave 0·6805 carbonic acid, and 0·2831 water.

(2.) 0·3198 gramme gave 0·9223 carbonic acid, and 0·378 water.

(3.) 0·2533 gramme gave 0·7286 carbonic acid, and 0·3008 water.

(4.) 0·2928 gramme gave 0·8468 carbonic acid, and 0·3476 water.

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* Communicated by the Chemical Society, having been read November 17th, 1841.

	1.	2.	3.	4.
Carbon.	79·72	79·74	79·53	79·96
Hydrogen	13·32	13·13	13·19	13·19
Oxygen	6·96	7·13	7·28	6·85
	100·00	100·00	100·00	100·00

These analyses differ considerably from that of Chevreul, though I have repeated them with every attention to accuracy. Chevreul found

Carbon	81·660	
Hydrogen . . .	12·862	
Oxygen	5·578	
		100·000

It is needless, however, at present attempting to deduce any formula from these analyses, as the acids which spermaceti contains have not been accurately determined. Spermaceti is usually supposed to consist of margarate and oleate of ethal. From experiments, I have reason to think that one of them is margaric acid; but as spermaceti, when distilled, yields no trace of sebacic acid, there is every reason to conclude the other acid it contains, the quantity of which is extremely small, is certainly not the oleic acid.

In order to ascertain how far cetine differs in composition from ordinary spermaceti, I was induced to submit a portion of the latter also to analysis. The melting point of the crude spermaceti analysed was only 107° F.

(1.) 0·3279 gave 0·9499 carbonic acid, and 0·3904 water.
(2.) 0·349 gave 1·0065 carbonic acid.
(3.) 0·3755 gave 1·0887 carbonic acid, and 0·4399 water.

	1.	2.	3.
Carbon.	80·10	79·74	80·16
Hydrogen . . .	13·23		13·01
Oxygen	6·67		6·83
	100·00		100·00

It is evident from these analyses, that the composition of crude spermaceti is precisely the same with that of the purest cetine. The small quantity of oil, therefore, which accompanies the former is probably isomeric with the more solid fat.

Ethal.

The ethal which I analysed was prepared by saponifying spermaceti with powdered potash. The saponification was twice repeated, in order that none of the spermaceti might escape decomposition. The lime-soap was then formed by precipitation with chloride of calcium. It was dried with a gentle heat and the ethal extracted by æther: alcohol was found inadmissible, as a large quantity of the lime-soap was also dissolved by it. I also found it advantageous to mix the lime-soap with a considerable quantity of pounded glass, as this prevented its adhering to the sides and bottom of the vessel when heated, and thus enabled the æther to act more equally on every part of the mass. The ethal first obtained was again boiled with milk of lime, again extracted with æther, and repeatedly crystallized. Its melting point was 119° F.

(1.) 0·5307 gave 1·519 carbonic acid, and 0·665 water.
(2.) 0·2881 gave 0·8295 carbonic acid, and 0·361 water.
(3.) 0·302 gave 0·8645 carbonic acid, and 0·383 water.

	1.	2.	3.
Carbon . . .	79·14	79·61	79·15
Hydrogen . .	13·92	13·02	14·08
Oxygen . . .	6·94	6·47	6·77
	100·00	100·00	100·00

These analyses agree very closely with the calculated numbers, and with the analyses of Chevreul and Dumas.

	Calculated numbers.	
	Atoms.	Per cent.
Carbon	32 =	79·69
Hydrogen.	34 =	13·82
Oxygen	2 =	6·51

Laurel Turpentine.

For some years past, an essential oil, to which the name of Laurel has been improperly given, has been imported in considerable quantities from Demerara and some other parts of South America. It has been successfully employed as an external application for the cure of rheumatism. It is also an excellent solvent for caoutchouc, as it dissolves that substance very readily, and leaves it in a firmer and less altered state than either naphtha or oil of turpentine. Its comparatively high price, however, 1*s.* per oz., precludes its employment for this purpose. The botanical nature of the tree which produces it is unknown. The Spaniards call the tree "acaita de sassefras." I think it probable that it is a species of pine. These trees are not very abundant, but the quantity of oil they contain is exceedingly great. It runs out abundantly when incisions are made near the root of the tree, and it also not unfrequently exudes spontaneously. The oil as it occurs in commerce is transparent, but of a slightly yellow colour, owing to its containing a little resinous matter, which is easily removed by distilling it with water. The smell of this oil reminds one of that of turpentine, but it is much more agreeable, and approaches more nearly that of oil of lemons; its specific gravity is 0·8645 at 56° F. Oil of laurel is accompanied with a volatile acid, the quantity of which, however, is extremely small. When this acid is boiled with nitrate of silver, the oxide is reduced to the metallic state. The acid is probably therefore the Formic.

To prepare the oil for analysis it was distilled with water to remove the resin it contained, and then rendered anhydrous by fused chloride of calcium. When rectified on the oil-bath, it began to boil at 301° F., but the boiling point gradually rose to 325° F. It was then transparent and colourless. The first portion that distilled over was set aside, but the second and third, which contained nearly an ounce each, were separately collected, and subjected to analysis with oxide of copper.

(1.) 0·2677 gramme, boiling at 301° F., gave 0·857 carbonic acid, and 0·279 water.

(2.) 0·2839 gramme, boiling at 325° F., gave 0·9062 carbonic acid, and 0·2956 water.

	1.	2.	Calculated numbers.	Atoms.
Carbon.	88·51	88·29	88·46	= 5
Hydrogen . . .	11·57	11·57	11·54	= 4
	100·08	99·83	100·00	

It is evident from these analyses, that oil of laurel consists of two or more isomeric oils belonging to the numerous tribe of carburetted hydrogens of which oil of turpentine is the type, containing carbon and hydrogen in the proportion of 5 to 4. The action of the reagents on oil of laurel is so similar to that on oil of turpentine as to render details unnecessary. The reason which has induced me to change the name of oil of laurel to that of laurel turpentine is, that there are two oils of laurel already, one fixed and the other volatile, with which it might otherwise be easily confounded.

Oil of Hyssop.

The essential oil of hyssop is easily obtained by the usual process of distilling the plant with water. The quantity which it yields is pretty considerable. The oil has the smell of the plant, and its taste, like that of the other essential oils, is very pungent. When fresh it is transparent and colourless; but when kept some time, especially if the air is not carefully excluded, it becomes yellowish, owing to the formation of a small quantity of resin. Oil of hyssop is lighter than water and quite neutral; its boiling point is not at all fixed; it begins to boil at 288° F., but the boiling gradually rises till it reaches 325°, soon after which it begins to pass over coloured:

it is evidently a mixture of several oils. In order to determine this more certainly, the anhydrous oil was rectified, and the product of its distillation at different temperatures collected separately and subjected to analysis. The following are the results:—

(1.) 0·289 gramme, boiling at 288° F., gave 0·8794 carbonic acid, and 0·2875 water.

(2.) 0·3022 gramme, boiling at 299° F., gave 0·8885 carbonic acid, and 0·298 water.

(3.) 0·2838 gramme, boiling at 335° F., gave 0·8243 carbonic acid, and 0·2671 water.

	1.	2.	3.
Carbon	84·13	81·29	80·31
Hydrogen. . .	11·05	10·95	10·45
Oxygen.	4·82	7·76	9·24
	100·00	100·00	100·00

It will at once be perceived from these results, that the portion of the oil richest in carbon and hydrogen distils over at a comparatively low temperature, and that as the quantity of oxygen in the oil increases, its boiling point rises. This is what usually takes place with oils which consist of a mixture of a carburetted hydrogen, and more or less oxygenated oils. I was induced therefore to try if these different oils could be separated by treating them with fused potash—the method so successfully employed by Messrs. Gerhardt and Cahours with oil of cumin, and which promises to be extremely useful in the investigation of this class of bodies. The oil of hyssop was dropped upon the potash through a capillary opening in the tubes of a retort. As soon as the oil came in contact with the melted potash, the greater portion of it was converted into a brownish resin, but a part of it passed into the receiver. This portion was again subjected to the action of the potash, when still more of it was converted into resin. What distilled over was considerably different in taste and smell from ordinary oil of hyssop. When subjected to analysis,

0·3047 gramme gave 0·955 carbonic acid, and 0·313 water, =

Carbon	86·65
Hydrogen.	11·41
Oxygen	1·94
	100·00

It is evident, therefore, that I did not succeed in converting oil of hyssop into a pure carburetted hydrogen, though the quantity of the oxygenated oil was considerably diminished.

Oil of Assafœtida.

It is to this oil that assafœtida owes its highly offensive smell. The quantity of oil which the resin yields, varies according to its freshness. A pound of the resin generally yields about one-third of an ounce of oil, which is obtained by distillation with water in the usual way. It is advisable to mix the resin with pounded glass, as this prevents the resin from adhering to the bottom of the retort, and both hinders it from burning and diminishes the violence of the succussions with which the distillation would otherwise be attended. The oil has usually a slightly yellowish tint: its specific gravity is 0·9428 at 60° F.; its taste is first mild and then acrid. When exposed for some time to the air it oxidizes, and a resinous matter forms in it. In order to prepare it for analysis, the oil which had been twice distilled with water to remove all the resin was rectified over chloride of calcium on the oil-bath. Its boiling point is by no means constant; it began to boil at 325° F., and continued to rise till it reached 370° F. The receiver was changed three times during the distillation, and the products separately collected and analysed. The presence of sulphur in oil of assafœtida was first noticed by Zeise. It differs from oil of mustard, by containing no nitrogen. The carbon and hydrogen were estimated by analysis with oxide of copper, and the sulphur was determined by passing the oil in vapour over a mixture of nitre and carbonate of baryta at a red heat. The following are the results:—

(1.) Analysis of 1st quantity, 0·2967 oil, boiling at 325° F., gave 0·710 carbonic acid, and 0·2625 water.

(2.) Analysis of 1st quantity, 0·2915, gave 0·6935 carbonic acid, and 0·253 water.

Per cent.

0·382 oil gave 0·635 sulphate of baryta = 22·93 sulphur.

0·391 oil gave 0·639 sulphate of baryta = 22·54 sulphur.

	1.	2.
Carbon	66·16	65·78
Hydrogen . .	9·83	9·64
Sulphur . . .	22·93	22·54
Oxygen . . .	1·08	2·04
	100·00	100·00

(1.) Analysis of 2nd quantity of oil, boiling at 341° F., 0·2312 gave 0·523 carbonic acid, and 0·1967 water.

(2.) Analysis of 2nd quantity, 0·2728 gave 0·6177 carbonic acid, and 0·2224 water.

(3.) Analysis of 2nd quantity, 0·2889 gave 0·6461 carbonic acid, and 0·2447 water.

0·413 oil gave 0·601 sulphate of baryta = 20·12 per cent. sulphur.

0·421 gave 0·610 sulphate of baryta = 19·99 per cent. sulphur.

	1.	2.	3.
Carbon.	62·54	62·60	61·83
Hydrogen . . .	9·45	9·05	9·41
Sulphur	20·12	19·99	
Oxygen	7·89	8·36	
	100·00	100·00	

(1.) Analysis of 3rd quantity of oil, boiling at 370° F., 0·3036 gave 0·6415 carbonic acid, and 0·2493 water.

(2.) Analysis of 3rd quantity of oil, 0·2947 gave 0·6185 carbonic acid, and 0·2413 water.

0·344 gave 0·421 sulphate of baryta = 16·88 per cent. sulphur.

0·382 gave 0·436 sulphate of baryta = 15·74 per cent. sulphur.

	1.	2.
Carbon.	58·42	58·03
Hydrogen . . .	9·12	9·09
Sulphur	16·88	15·74
Oxygen	15·58	17·14
	100·00	100·00

It is evident from these results, that oil of assafœtida is a mixture of various oils, one or more of which consist probably only of carbon, hydrogen and sulphur, with other oils containing more or less oxygen. The less oxygenated portion is the most volatile. It is therefore unnecessary to attempt to deduce any formula from these analyses. Though oil of assafœtida was twice treated with fused potash in the same manner as oil of hyssop, the greater portion of the sulphur was removed, but I could not succeed in getting rid of the whole. The greater portion of the oil was converted into a blackish resin. This resinous matter is soluble in alkali, from which it is precipitated by acids. It is not in the least degree crystalline. The action of reagents on oil of assafœtida was as follows:—salts of silver, lead and protoxide of mercury gave black precipitates. When brought in contact with peroxide of mercury, heat was evolved, and a part of the oxide was converted into a greenish yellow mass, which was insoluble in water. A very small portion of the oil was acted on however. Corrosive sublimate immediately produced a copious flocculent white precipitate. It was insoluble in water, alcohol and æther. It was soluble in nitric acid, and when boiled with solution of potash, the mercury was precipitated in the state of protoxide. Oil of assafœtida does not combine with ammonia. It is very little acted on either by aqueous or alcoholic solutions of potash. Nitric acid acts on this oil with great energy, and the evolution of deutoxide of azote. It is converted into a resin, and on adding a salt of baryta an abundant precipitate of sulphate of baryta is obtained. Sulphuric acid first reddens, and with the assistance of heat, chars it. It dissolves iodine readily, but without explosion

Glasgow, 14th October, 1841.

XLI. *On the Constitution of the Atmosphere.* *By* JAMES IVORY, *K.H., M.A., Hon. M.R.I.A., Instit. Reg. Sc. Paris, et Reg. Soc. Götting. Corresp.*[*]

IN the last Number of this Journal (p. 197) I assumed that the atmosphere is composed solely of oxygen and azote: but besides these gases, it is found to contain a small proportion of carbonic acid gas, the quantity of which has been in some degree appreciated. It may not therefore be improper briefly to resume the subject, in order to take in all the constituent parts of the atmosphere, as far as they are known, leaving out the consideration of aqueous vapour, which requires a separate discussion. Suppose three several portions of oxygen, azote, and carbonic acid gas, their common temperature being θ, and their pressures, densities, and volumes being represented respectively by $p, \rho, v, p', \rho', v', p'', \rho'', v''$; and let the three gases be confined in an envelop, the volume of which is $V = v + v' + v''$. According to the theory of mixed gases, as explained in this Journal for February last (p. 81), each of the three gases will be uniformly diffused through the envelop, and the elasticity of the mixture, resulting from the mutual action of the three elasticities, will be

$$p \cdot \frac{v}{V} + p' \cdot \frac{v'}{V} + p'' \cdot \frac{v''}{V}:$$

and if we suppose $p = p' = p''$, the same elasticity will be simply p. Let R be the density of atmospheric air, the pressure being p and the temperature θ: then the four densities R, ρ, ρ', ρ'', will be proportional to the numbers 1, 1·1057, 0·972, 1·52[†], for which we may write 1, A, A′, A″: so that we shall have

$$\begin{aligned} \rho &= A \,.\, R, \\ \rho' &= A' \,.\, R, \\ \rho'' &= A'' \,.\, R: \end{aligned}$$

and if we multiply the densities by the respective volumes, and add the products, there will result

$$v\rho + v'\rho' + v''\rho'' = (A\,v + A'\,v' + A''\,v'')\,R.$$

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[*] Communicated by the Author.

[†] The more exact number is 1·5196.—Biot, *Précis de Phys.*

The volume of the mixture being V, if D be its density, we shall evidently have

$$v\rho + v'\rho' + v''\rho'' = \mathrm{V\,D}:$$

and hence

$$\mathrm{V\,D} = (\mathrm{A}\,v + \mathrm{A}'\,v' + \mathrm{A}''\,v'')\,\mathrm{R}.$$

Now we may assume

$$\mathrm{V} = \mathrm{A}\,v + \mathrm{A}'\,v' + \mathrm{A}''\,v'';$$

and in consequence $\mathrm{D} = \mathrm{R}$: so that the mixture can be no other than atmospheric air, since both have the same pressure, density, and temperature. If to the equation just found we join the former one, viz.

$$\mathrm{V} = v + v' + v'',$$

both are not sufficient for determining the three volumes when a given value is assigned to V: and therefore it seems necessary to ascertain one of the volumes by experiment in order to arrive at the knowledge of the other two. It is generally admitted that the carbonic acid gas never exceeds $\frac{1}{1000}$th of the volume of air. It has even been established that the carbonic acid gas in a volume of air varies between $\frac{4}{10000}$th and $\frac{6}{10000}$th[*]. Wherefore, taking $v'' = \frac{\mathrm{V}}{1000}$, and $\mathrm{V} = 100$, the two equations will become

$$\begin{aligned} 99{\cdot}848 &= \mathrm{A}\,v + \mathrm{A}'\,v', \\ 99{\cdot}9 &= v + v'; \end{aligned}$$

whence we obtain

$$\begin{array}{lrl} \text{Oxygen} \ldots\ldots & v &= 20{\cdot}5 \\ \text{Azote} \ldots\ldots\ldots & v' &= 79{\cdot}3 \\ \text{Carbonic acid} & v'' &= 0{\cdot}1 \\ & & \overline{}\ 99{\cdot}9 \end{array}$$

It thus appears that the carbonic acid gas taken at the greatest valuation, very slightly alters the proportion of the oxygen and azote.

Besides the three gases already mentioned, the atmosphere generally contains a portion of aqueous vapour; and we have next to inquire in what manner this vapour will mix with dry air in an atmosphere at rest and in equilibrium. Let V denote a volume of moist air consisting of aqueous vapour of the tension ϖ, measured in inches of mercury, and of dry air, the barometric pressure of which is p, and its volume v: and, first we may suppose that v is less than V, in which case the elasticity p of the dry air diffused through the volume of

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[*] *Comptes Rendus*, June 7, 1841, p. 1023.

moist air, will be reduced to $p \cdot \frac{v}{V}$; so that the whole elasticity of the moist air will be $p \cdot \frac{v}{V} + \varpi$. But the pressure of the external air upon the volume V of moist air is evidently equal to $p + \varpi$, which is greater than the elasticity of the mixture: wherefore, in an atmosphere at rest, the volume of moist air would not be permanent in its figure but would contract its dimensions till the external and internal elasticities were reduced to an equality. If we next suppose that v is greater than V, it is obvious that the elasticity of the moist air would be greater than $p + \varpi$; and in consequence the volume V will expand in order to produce an equilibrium with the external air. The only remaining supposition is, that v is equal to V; or that every infinitesimal volume in the moist atmosphere contains equal volumes of dry air and vapour; which supposition evidently satisfies the conditions of equilibrium, the elasticity of the air within the partial volume and the pressure of the external air being alike equal to $p + \varpi$.

The same conclusion might have been deduced from the theory of the diffusion of gases and vapours. For if vapour be added to a quiescent atmosphere of dry air, it will diffuse itself through the dry air, till every partial volume of the mixture is at rest by the mutual action of the elasticities of the two fluids.

It now remains to determine the relation between the densities of the same volume in the dry and moist atmospheres; which will enable us to ascertain the proportion between the weights of a volume of the two atmospheres, when the pressure p of the dry air and the tension ϖ of the vapour, are given. Let ρ be the density of the dry air; δ that of the vapour, and Δ that of the mixture: then, the volume being V, the sum of the masses of the dry air and vapour will be equal to the mass of the mixture, and we shall have

$$\rho V + \delta V = \Delta V, \text{ and } \Delta = \rho + \delta.$$

Now, the temperature being θ, the density is ρ when the pressure is p; it will therefore be $\frac{\varpi}{p} \rho$ when the pressure is ϖ: so that $\frac{\varpi}{p} \rho$ and δ are the densities of dry air and vapour, under the pressure p and the temperature θ. Wherefore, V being any volume, the mass or weight $\frac{\varpi}{p} \rho V$ of dry air, and the mass or weight δV of vapour, both under the same pressure and temperature, will be to one another as 8 to 5, as has been shown by Gay-Lussac. Thus we obtain

$$\delta = \frac{5}{8} \cdot \frac{\varpi}{p} \rho : \text{ and } \Delta = \rho + \delta = \rho \left(1 + \frac{5}{8} \cdot \frac{\varpi}{p}\right).$$

At the surface of the earth the tension ϖ of the vapour has a maximum value, which is small when compared to p the pressure of the dry air. The tension ϖ continually decreases in rising above the earth's surface.

Although the foregoing reasoning seems clear and unexceptionable, yet the result is different from that obtained by Poisson* and Biot, who concur in giving this formula, viz.

$$\Delta = \rho \left(1 - \frac{3}{8} \cdot \frac{\varpi}{p}\right).$$

It will not be necessary to enter upon any discussion of this point; because both the eminent philosophers assume that, in an atmosphere of dry air and vapour, a volume of the moist air is greater than the volume of dry air which is one of its constituent parts: whereas it has been shown that a volume so constituted must be variable in its bulk in the moist atmosphere. And, moreover, if vapour be added to a dry atmosphere, the densities must be increased: but, according to the formula, the densities in the moist atmosphere are less than the densities in the atmosphere of dry air.

XLII. *Discussion of a new Equation in Hydrodynamics. By the Rev.* JAMES CHALLIS, *M.A., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.*

THE argument maintained in my communication to the February Number of this Journal (p. 84) may be briefly recapitulated as follows. The two fundamental equations of fluid motion, which, when impressed forces are left out of consideration, are the following,

$$\frac{d\varrho}{dt} + \frac{d.\varrho u}{dx} + \frac{d.\varrho v}{dy} - \frac{d.\varrho w}{dz} = 0, \quad \ldots\ldots \quad (1.)$$

$$(dP) + \left(\frac{du}{dt}\right) dx + \left(\frac{dv}{dt}\right) dy + \left(\frac{dw}{dt}\right) dz = 0, \quad (2.)$$

are available for the determination of the motion whenever u, v, w are the partial differential coefficients with respect to x, y, z of a function of x, y, z, and t. That is, if ϕ be the function, when

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* Poisson, *Mécanique*, 2nd edit. tom. ii. p. 634. Biot, *Additions à la Con. des Tems*, 1839, p. 15.

$$u = \frac{d\phi}{dx}, \quad v = \frac{d\phi}{dy}, \quad w = \frac{d\phi}{dz}.$$

For by these three equations and the two preceding, u, v, w, and ϱ may be eliminated, and the resulting partial differential equation in ϕ, x, y, z, and t being integrated, furnishes the means of satisfying the given conditions of the motion. The above are not, however, the most general expressions for u, v, w; and the partial differential equation thus obtained is applicable only to limited instances of motion. I have given reasons for supposing the general values to be

$$u = \mathrm{N}\,.\frac{d\phi}{dx}, \quad v = \mathrm{N}\,.\frac{d\phi}{dy}, \quad w = \mathrm{N}\,.\frac{d\phi}{dx},$$

N being a function in general of x, y, z and t, the reciprocal of which makes $u\,dx + v\,dy + w\,dz$ integrable. The quantity N is given by an additional equation, hitherto, I believe, unnoticed by writers on hydrodynamics, viz.

$$\frac{d\phi}{dt} + \mathrm{N}\,.\left(\frac{d\phi^2}{dx^2} + \frac{d\phi^2}{dy^2} + \frac{d\phi^2}{dz^2}\right) = 0. \; . \; . \; . \; . \quad (3).$$

This equation, the discussion of which is the chief object of the present communication, was arrived at by the following considerations:—

First, it was shown that

$$\frac{u}{\mathrm{N}}\,dx + \frac{v}{\mathrm{N}}\,dy + \frac{w}{\mathrm{N}}\,dz = 0,$$

is the differential equation of a surface which cuts at right angles the directions of the motion at any given instant of the fluid particles through which it passes, and which may therefore be called a surface of displacement. The integral of this equation, since the left-hand side of it is equal to $(d\phi)$, is $\phi = 0$, an arbitrary function of the time being included in ϕ. It is plain, that as the reasoning applies to the whole of the fluid in motion during the whole time of its motion, there will at each instant be an unlimited number of surfaces of displacement, differing according to different values assigned to the arbitrary quantities involved in ϕ, and that these surfaces will be continually changing their positions. Next, it was argued that if x, y, z be coordinates of any point of a given surface of displacement at the time t, $x + u\,dt$, $y + v\,dt$, $z + w\,dt$ will be the coordinates of the same surface in the position, indefinitely near the former, which it takes at the time $t + dt$; and consequently, if when t is changed to $t + dt$ in the equation $\phi = 0$, x, y, z be changed to $x + u\,dt$, $y + v\,dt$, $z + w\,dt$, that equation will still be satisfied. This consideration immediately led to equation (3).

The validity of this equation was confirmed, first, by showing that it holds good at the same time that $u\,dx+v\,dy+w\,dz$ is integrable *per se*, if N be a function of t only, which for this case it manifestly must be; and then by employing it to obtain from equation (2.) the equation

$$f(t)-\mathrm{P}=\int\frac{d\,\mathrm{V}}{d\,t}\,d\,s+\frac{\mathrm{V}^2}{2}, \quad \ldots\ldots\ldots \quad (4.)$$

in which V is the velocity at any point of a line s drawn at a given instant in the direction of the motion of the particles through which it passes.

The equation (4.) is readily shown to be true when $u\,dx+v\,dy+w\,dz$ is assumed to be an exact differential. That it is true without any limitation, as the reasoning to which I am referring demonstrates by means of equation (3.), may be concisely shown as follows. If F be the sum of the resolved parts in the direction of an arbitrary line s of the forces impressed on a mass of fluid, then in case of equilibrium $d\,\mathrm{P}=\mathrm{F}\,d\,s$; and hence, by D'Alembert's principle, when there is motion and the effective accelerative force in the same direction is f, $d\,\mathrm{P}=(\mathrm{F}-f)\,d\,s$. If, therefore, there be no impressed force, $d\,\mathrm{P}+f\,d\,s=0$: and supposing the arbitrary line to be taken in the direction of the motion of the particles through which it passes, $f=\left(\frac{d\,\mathrm{V}}{d\,t}\right)$. But on this supposition

$$\left(\frac{d\,\mathrm{V}}{d\,t}\right)=\frac{d\,\mathrm{V}}{d\,t}+\frac{d\,\mathrm{V}}{d\,s}\cdot\frac{d\,s}{d\,t}=\frac{d\,\mathrm{V}}{d\,t}+\mathrm{V}\cdot\frac{d\,\mathrm{V}}{d\,s}.$$

Hence $-d\,\mathrm{P}=\frac{d\,\mathrm{V}}{d\,t}\,d\,s+\mathrm{V}\cdot\frac{d\,\mathrm{V}}{d\,s}\,d\,s$, which gives by integration equation (4.).

I proceed now to adduce another argument in confirmation of equation (3.), by employing this equation in making a deduction from equation (1.), the truth of which may be established by independent considerations. For this purpose it will be necessary to make use of the formula for the sum of the reciprocals of the principal radii of curvature of a curve surface whose equation is $\phi=0$, expressed in partial differential coefficients of ϕ. To those conversant with the processes of analytical geometry, there will be no difficulty in proving that, if r and r' be the radii of curvature, this formula is

$$\left(\frac{1}{r}+\frac{1}{r'}\right)\left(\frac{d\,\phi^2}{d\,x^2}+\frac{d\,\phi^2}{d\,y^2}+\frac{d\,\phi^2}{d\,z^2}\right)^{\frac{3}{2}}=\frac{d^2\,\phi}{d\,x^2}\cdot\frac{d\,\phi^2}{d\,x^2}+\frac{d^2\phi}{d\,y^2}\cdot\frac{d\,\phi^2}{d\,y^2}$$

$$+\frac{d^2\phi}{d z^2}\cdot\frac{d\phi^2}{d z^2} + 2\,\frac{d^2\phi}{d x\,d y}\cdot\frac{d\phi}{d x}\cdot\frac{d\phi}{d y} + 2\,\frac{d^2\phi}{d x\,d z}\cdot\frac{d\phi}{d x}\cdot\frac{d\phi}{d z}$$

$$+\,2\,.\,\frac{d^2\phi}{d y\,d z}\cdot\frac{d\phi}{d y}\cdot\frac{d\phi}{d z} - \left(\frac{d^2\phi}{d x^2}+\frac{d^2\phi}{d y^2}+\frac{d^2\phi}{d z^2}\right)$$

$$+\left(\frac{d\phi^2}{d x^2}+\frac{d\phi^2}{d y^2}+\frac{d\phi^2}{d z^2}\right).$$

Now equation (1.) is equivalent to the following,

$$\frac{d\varrho}{\varrho\,d t}+\frac{d\varrho}{\varrho\,d x}u+\frac{d\varrho}{\varrho\,d y}v+\frac{d\varrho}{\varrho\,d z}w+\frac{d u}{d x}+\frac{d v}{d y}+\frac{d w}{d z}=0,$$

which, by substituting $\frac{d x}{d t}$ for u, $\frac{d y}{d t}$ for v, and $\frac{d z}{d t}$ for w, and putting $(d\,\mathrm{P})$ for the complete differential of P, or k Nap. log. ϱ, with respect to *space*, assumes the form

$$\frac{d\,\mathrm{P}}{d t}+\frac{(d\,\mathrm{P})}{d t}+k\left(\frac{d u}{d x}+\frac{d v}{d y}+\frac{d w}{d z}\right)=0.$$

It must, however, be borne in mind, that on account of the preceding substitutions for u, v, and w, the variation in $(d\,\mathrm{P})$ is from one point to another *in the line of motion*. Hence $\frac{(d\,\mathrm{P})}{d t}$, or $\mathrm{V}\,.\,\frac{(d\,\mathrm{P})}{\mathrm{V}\,d t}=\mathrm{V}\,.\,\frac{d\,\mathrm{P}}{d s}$. Also $\frac{d\mathrm{P}}{d s}=k\,.\,\frac{d\varrho}{\varrho\,d s}$, and $\frac{d\,\mathrm{P}}{d t}=k\,.\,\frac{d\varrho}{\varrho\,d t}$. Consequently, by substitution,

$$\frac{d\varrho}{d t}+\mathrm{V}\,.\,\frac{d\varrho}{d s}+\varrho\,.\left(\frac{d u}{d x}+\frac{d v}{d y}+\frac{d w}{d z}\right)=0. \quad . \; . \; . \quad (5.)$$

Again, since $u=\mathrm{N}\,.\,\frac{d\phi}{d x}$, equation (3.) gives

$$\frac{d\phi}{d x}\cdot\frac{d\phi}{d t}+u\left(\frac{d\phi^2}{d x^2}+\frac{d\phi^2}{d y^2}+\frac{d\phi^2}{d z^2}\right)=0,$$

whence by differentiating with respect to x,

$$\frac{d u}{d x}=-\frac{\frac{d^2\phi}{d x^2}\cdot\frac{d\phi}{d t}+\frac{d^2\phi}{d x\,d t}\cdot\frac{d\phi}{d x}}{\frac{d\phi^2}{d x^2}+\frac{d\phi^2}{d y^2}+\frac{d\phi^2}{d z^2}}$$

$$+\,\frac{2\,\frac{d\phi}{d t}\cdot\frac{d\phi}{d x}\left(\frac{d\phi}{d x}\cdot\frac{d^2\phi}{d x^2}+\frac{d\phi}{d y}\cdot\frac{d^2\phi}{d x\,d y}+\frac{d\phi}{d z}\cdot\frac{d^2\phi}{d x\,d z}\right)}{\left(\frac{d\phi^2}{d x^2}+\frac{d\phi^2}{d y^2}+\frac{d\phi^2}{d z^2}\right)^2}.$$

Similar expressions having been obtained for $\frac{dv}{dy}$ and $\frac{dw}{dz}$, it will be found, by adding the three together and paying regard to the formula for $\frac{1}{r}+\frac{1}{r'}$, that

$$\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right)\left(\frac{d\phi^2}{dx^2}+\frac{d\phi^2}{dy^2}+\frac{d\phi^2}{dz^2}\right)=\frac{d^2\phi}{dx^2}\cdot\frac{d\phi}{dt}$$
$$-\frac{d^2\phi}{dx\,dt}\cdot\frac{d\phi}{dx}+\frac{d^2\phi}{dy^2}\cdot\frac{d\phi}{dt}-\frac{d^2\phi}{dy\,dt}\cdot\frac{d\phi}{dy}+\frac{d^2\phi}{dz^2}\cdot\frac{d\phi}{dt}$$
$$-\frac{d^2\phi}{dz\,dt}\cdot\frac{d\phi}{dz}+2\frac{d\phi}{dt}\left(\frac{d\phi^2}{dx^2}+\frac{d\phi^2}{dy^2}+\frac{d\phi^2}{dz^2}\right)^{\frac{1}{2}}\left(\frac{1}{r}+\frac{1}{r'}\right),$$

which equation may be reduced as follows to one of a simpler form.

Since $V^2=u^2+v^2+w^2=N^2\left(\frac{d\phi^2}{dx^2}+\frac{d\phi^2}{dy^2}+\frac{d\phi^2}{dz^2}\right)$, equation (3.) gives $\frac{d\phi^2}{dt^2}=V^2\left(\frac{d\phi^2}{dx^2}+\frac{d\phi^2}{dy^2}+\frac{d\phi^2}{dz^2}\right)$. Hence

$$\frac{d\phi^2}{dx^2}+\frac{d\phi^2}{dy^2}+\frac{d\phi^2}{dz^2}=\frac{1}{V^2}\cdot\frac{d\phi^2}{dt^2}.$$

Also by multiplying equation (3.) by $N\frac{d\phi}{dx}$, it will appear that

$$u\frac{d\phi}{dt}+V^2\frac{d\phi}{dx}=0;$$

and this equation, by differentiating with respect to x, gives

$$\frac{d^2\phi}{dx^2}\cdot\frac{d\phi}{dt}-\frac{d^2\phi}{dx\,dt}\cdot\frac{d\phi}{dx}=\frac{1}{V^2}\cdot\frac{d\phi^2}{dt^2}\left(\frac{2u}{V}\cdot\frac{dV}{dx}-\frac{du}{dx}\right).$$

So $$\frac{d^2\phi}{dy^2}\cdot\frac{d\phi}{dt}-\frac{d^2\phi}{dy\,dt}\cdot\frac{d\phi}{dy}=\frac{1}{V^2}\cdot\frac{d\phi^2}{dt^2}\left(\frac{2v}{V}\cdot\frac{dV}{dy}-\frac{dv}{dy}\right);$$

and $$\frac{d^2\phi}{dz^2}\cdot\frac{d\phi}{dt}-\frac{d^2\phi}{dz\,dt}\cdot\frac{d\phi}{dz}=\frac{1}{V^2}\cdot\frac{d\phi^2}{dt^2}\left(\frac{2w}{V}\cdot\frac{dV}{dz}-\frac{dw}{dz}\right).$$

When the several values thus obtained are substituted in the foregoing equation, the result is

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=\frac{u}{V}\cdot\frac{dV}{dx}+\frac{v}{V}\cdot\frac{dV}{dy}+\frac{w}{V}\cdot\frac{dV}{dz}+V\left(\frac{1}{r}+\frac{1}{r'}\right).$$

If now the condition be introduced that the variation from one point to another of space be in the line of motion, we shall have

$$\frac{u}{V}=\frac{dx}{ds},\quad\frac{v}{V}=\frac{dy}{ds},\quad\frac{w}{V}=\frac{dz}{ds}:$$

and the above result is reduced to the following,

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=\frac{d\mathrm{V}}{ds}+\mathrm{V}\left(\frac{1}{r}+\frac{1}{r'}\right).$$

Consequently, by substituting in equation (5.), we finally obtain

$$\frac{d\varrho}{dt}+\varrho\frac{d\mathrm{V}}{ds}+\mathrm{V}\frac{d\varrho}{ds}+\mathrm{V}\varrho\left(\frac{1}{r}+\frac{1}{r'}\right)=0. \quad . \quad . \quad (6.)$$

It will be evident, from an inspection of the foregoing reasoning, that equation (6.) is arrived at whether N be a function of t only, or a function of t and the coordinates; that is, whether $u\,dx+v\,dy+w\,dz$ be integrable *per se*, or integrable by a factor. Hence this equation may be derived from the equations (m) and (n) of the *Mécanique Analytique* (part ii. sect. xii., arts. 7 and 8), in which $u\,dx+v\,dy+w\,dz$ is assumed to be an exact differential. I have, in fact, deduced it in this way, but it is needless to insert the mathematical reasoning here. It is, however, important to remark, that equation (6.) obtained by this process is proved to be true only for *rectilinear* motions of the fluid particles, whilst the proof by equation (3.) is inclusive of the other, and extends to *curvilinear* motions. I will endeavour to illustrate this remark.

In the Transactions of the Cambridge Philosophical Society (vol. v. part ii. p. 196), I have given a proof of equation (6.) totally unlike that above, and as it will serve to confirm the truth of equation (3.) I will briefly state it here. In whatever manner fluid is in motion, we may conceive at a given instant an unlimited number of surfaces to be drawn, cutting at right angles the directions of motion of the particles through which they pass. In two such surfaces separated by an indefinitely small interval, let two rectangular elements be taken opposite to each other, so that the lines joining the corresponding angular points, when produced, are normals to both surfaces. By the nature of curve surfaces, if the sides of the rectangles be assumed to be in planes of greatest and least curvature, the normals will meet two and two at distances equal to r and r', the greatest and least radii of curvature; and if m be the rectangular element of the inner surface, and δr be the interval between the surfaces, the element of the other surface will be

$$m\,.\,\frac{(r+\delta r)(r'+\delta r)}{rr'}.$$

Also, if V, ϱ be the velocity and density at the inner surface, and V$'$, ϱ' be the same for the outer surface, the velocity being considered positive when it is directed from the former to the latter, then the increment of matter between them in the time δt is

$$-m\,\mathrm{V}'\varrho'\,\delta t\,.\,\frac{(r+\delta r)\,(r'+\delta r)}{r\,r'}+m\,\mathrm{V}\,\varrho\,d\,t,$$

or, neglecting quantities of the second order,

$$-m\,\delta t\,(\mathrm{V}'\varrho'-\mathrm{V}\,\varrho)-m\,\mathrm{V}\,\varrho\left(\frac{1}{r}+\frac{1}{r'}\right)\delta r\,\delta t.$$

And this increment is also equal to $m\,\delta r\,\delta\varrho$. Hence by equating the two values and passing from differences to differentials, we have the equation sought, viz.

$$\frac{d\varrho}{dt}+\frac{d\,.\,\mathrm{V}\,\varrho}{dr}+\mathrm{V}\,\varrho\left(\frac{1}{r}+\frac{1}{r'}\right)=0.$$

But this proof does not possess more generality than that which, as mentioned above, is derivable from the equations of the *Mécanique Analytique*; because it is here assumed that the *focal lines* to which the lines of motion are directed, are fixed in space; in other words, that the motion is rectilinear. To prove that the same equation is arrived at when the focal lines change their positions so that the motion of the fluid particles is curvilinear, requires considerations which I have entered into in the Numbers of this Journal for December 1840 and June 1841. In the method of obtaining equation (6.), employed in the present communication, the distinction between the two cases can only be made by the factor N. According as N is a function of t only, or a function of t and the coordinates, the motion is rectilinear or curvilinear; and hence we may draw the inference, that *motion in a fluid is rectilinear or curvilinear according as* $u\,dx+v\,dy+w\,dz$ *is integrable of itself or by a factor.*

The truth of this inference appears also from the following argument. When $u\,dx+v\,dy+w\,dz$ is assumed to be an exact differential, the resulting partial differential equation involving the variables ϕ, x, y, z and t, is of the second order, and its integral contains two arbitrary functions. But when that quantity is made integral by a factor, the resulting equation, as I have shown in the communication to the February Number, rises to the third order, and its complete integral consequently contains three arbitrary functions. The third function has reference to the variation of density which must exist at each instant at a surface of displacement when the motion is curvilinear, and which disappears when the motion becomes rectilinear, because in this case the surface of displacement coincides with a surface of equal density.

The discussion I have now gone through may suffice to establish the truth and importance of equation (3.), which appears to be absolutely necessary for giving to the differential equations of hydrodynamics the generality requisite to meet every proposed instance of motion. In a Report on the Analytical Theory of Hydrodynamics, read at the third meeting of the British Association, I mentioned that it was a desideratum in this department of applied mathematics, to determine under what circumstances of the motion $u\,dx+v\,dy+w\,dz$ is an exact differential. The conclusions I have now arrived at seem to clear up this difficulty.

Cambridge Observatory, Feb. 11, 1842.

XLIII. *On certain discontinuous Integrals, connected with the Development of the Radical which represents the Reciprocal of the Distance between two Points. By* William Rowan Hamilton, *LL.D., P.R.I.A., Member of several Scientific Societies at Home and Abroad, Andrew's Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland*[*].

1. It is well known that the radical

$$(1-2xp+x^2)^{-\frac{1}{2}}, \quad \ldots \ldots \quad (1.)$$

in which x and 1 may represent the radii vectores of two points, while p represents the cosine of the angle between those radii, and the radical represents therefore the reciprocal of the distance of the one point from the other, may be developed in a series of the form

$$P_0 + P_1 x + P_2 x^2 + \ldots + P_n x^n + \ldots; \quad \ldots \quad (2.)$$

the coefficients P_n being functions of p, and possessing many known properties, among which we shall here employ the following only,

$$P_n = [0]^{-n} \left(\frac{d}{dp}\right)^n \left(\frac{p^2-1}{2}\right)^n; \quad \ldots \quad (3.)$$

the known notation of factorials being here used, according to which

$$[0]^{-n} = \frac{1}{1} \cdot \frac{1}{2} \cdot \frac{1}{3} \ldots \frac{1}{n}. \quad \ldots \quad (4.)$$

It is proposed to express the sum of the first n terms of the development (2.), which may be thus denoted,

$$\Sigma_{(n)\,0}^{n-1} P_n x^n = P_0 + P_1 x + P_2 x^2 + \ldots + P_{n-1} x^{n-1}. \quad (5.)$$

2. In general, by Taylor's theorem,

$$f(p+q) = \Sigma_{(n)\,0}^{\infty} [0]^{-n} q^n \left(\frac{d}{dp}\right)^n f(p); \quad \ldots \quad (6.)$$

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[*] Communicated by the Author.

hence, by the property (3.), P_n is the coefficient of q^n in the development of

$$\left(\frac{(p+q)^2-1}{2}\right)^n; \quad \ldots\ldots\ldots\ldots \quad (7.)$$

it is therefore also the coefficient of q^0 in the development of

$$\left(\frac{p^2-1}{2\,q}+p+\frac{q}{2}\right)^n. \quad \ldots\ldots\ldots \quad (8.)$$

If then we make, for abridgment,

$$\vartheta = p + \frac{p^2}{2}\cos\theta + \sqrt{-1}\left(1-\frac{p^2}{2}\right)\sin\theta, \ldots (9.)$$

we shall have the following expression, which perhaps is new, for P_n:

$$P_n = \frac{1}{2\,\pi}\int_{-\pi}^{\pi} \vartheta^n\, d\,\theta; \quad \ldots\ldots\ldots \quad (10.)$$

and hence, immediately, the required sum (5.) may be expressed as follows:

$$\Sigma_{(n)\,0}^{n-1}\, P_n\, x^n = \frac{1}{2\,\pi}\int_{-\pi}^{\pi} d\,\theta\, \frac{1-\vartheta^n\, x^n}{1-\vartheta\, x}; \quad \ldots \quad (11.)$$

in which it is to be observed that x may be any quantity, real or imaginary.

3. We have therefore, rigorously, for the sum of the n first terms of the series

$$P_0 + P_1 + P_2 + \ldots, \quad \ldots\ldots\ldots \quad (12.)$$

the expression

$$\Sigma_{(n)\,0}^{n-1}\, P_n = \frac{1}{2\,\pi}\int_{-\pi}^{\pi} d\,\theta\, \frac{1-\vartheta^n}{1-\vartheta}; \quad \ldots\ldots \quad (13.)$$

of which we propose to consider now the part independent of n, namely,

$$F(p) = \frac{1}{2\pi}\int_{-\pi}^{\pi} \frac{d\,\theta}{1-\vartheta}, \quad \ldots\ldots\ldots \quad (14.)$$

and to examine the form of this function F of p, at least between the limits $p = -1$, $p = 1$.

4. A little attention shows that the denominator $1-\vartheta$ may be decomposed into factors, as follows:

$$1-\vartheta = -\tfrac{1}{2}\,(\alpha + e^{\theta\sqrt{1}})\,(1-\beta\, e^{-\theta\sqrt{-1}}); \ldots (15.)$$

in which,

$$\alpha = 2\,s\,(1-s), \quad \beta = 2\,s\,(1+s), \quad \ldots\ldots \quad (16.)$$

and

$$p = 1 - 2\,s^2; \quad \ldots\ldots\ldots \quad (17.)$$

so that s may be supposed not to exceed the limits 0 and 1, since p is supposed not to exceed the limits -1 and 1. Hence

$$\frac{1}{1-\vartheta} = \frac{-2\,(\alpha+e^{-\theta\sqrt{-1}})\,(1-\beta\,e^{\theta\sqrt{-1}})}{(1+2\,\alpha\cos\theta+\alpha^2)(1-2\beta\cos\theta+\beta^2)}; \qquad (18.)$$

of which the real part may be put under the form

$$\frac{\lambda}{1+2\,\alpha\cos\theta+\alpha^2}+\frac{\mu}{1-2\,\beta\cos\theta+\beta^2}, \quad \ldots \quad (19.)$$

if λ and μ be so chosen as to satisfy the conditions

$$\lambda\,(1+\beta^2)+\mu\,(1+\alpha^2)=2\,(\beta-\alpha), \quad \ldots \quad (20.)$$

$$\lambda\beta-\mu\alpha=1-\alpha\beta, \quad \ldots\ldots\ldots \quad (21.)$$

which give

$$\lambda=\frac{1-\alpha^2}{\alpha+\beta}, \qquad \mu=\frac{\beta^2-1}{\alpha+\beta}. \quad \ldots\ldots\ldots \quad (22.)$$

The imaginary part of the expression (18.) changes sign with θ, and disappears in the integral (14.); that integral therefore reduces itself to the sum of the two following:

$$\left.\begin{aligned} \mathrm{F}(p) &= \frac{1}{4\,s\,\pi}\int_0^\pi \frac{(1-\alpha^2)\,d\theta}{1+2\,\alpha\cos\theta+\alpha^2} \\ &+ \frac{1}{4\,s\,\pi}\int_0 \frac{(\beta^2-1)\,d\theta}{1-2\,\beta\cos\theta+\beta^2} \end{aligned}\right\}; \qquad (23.)$$

in which, by (16.), $\alpha+\beta$ has been changed to $4\,s$. But, in general if $a^2 > b^2$,

$$\int_0^\pi \frac{d\theta}{a+b\cos\theta}=\frac{\pi}{\sqrt{a^2-b^2}}, \quad \ldots\ldots \quad (24.)$$

the radical being a positive quantity if a be such; therefore, in the formula (23.),

$$\int_0^\pi \frac{(1-\alpha^2)\,d\theta}{1+2\,\alpha\cos\theta+\alpha^2}=\pi, \quad \ldots\ldots \quad (25.)$$

because, by (16.), α cannot exceed the limits 0 and $\frac{1}{2}$, s being supposed not to exceed the limits 0 and 1, so that $1-\alpha^2$ is positive. On the other hand, β varies from 0 to 4, while s varies from 0 to 1; and β^2-1 will be positive or negative, according as s is greater or less than the positive root of the equation

$$s^2+s=\tfrac{1}{2}. \quad \ldots\ldots\ldots\ldots \quad (26.)$$

Hence, in (23.), we must make

$$\int_0^\pi \frac{(\beta^2-1)\,d\theta}{1-2\,\beta\cos\theta+\beta^2}=\pi, \text{ or } =-\pi, \quad \ldots \quad (27.)$$

according as

$$s > \text{ or } < \frac{\sqrt{3}-1}{2}; \quad \ldots\ldots\ldots \quad (28.)$$

and thus we find, under the same alternative,

$$\mathrm{F}(p) = \frac{1}{4s}(1 \pm 1), \quad \ldots\ldots\ldots \quad (29.)$$

that is,

$$\mathrm{F}(p) = \frac{1}{2s}, \text{ or } = 0. \quad \ldots\ldots \quad (30.)$$

But, by (17.),

$$s = \sqrt{\frac{1-p}{2}}; \quad \ldots\ldots\ldots\ldots \quad (31.)$$

the function F (p), or the definite integral (14.), receives therefore a sudden change of form when p, in varying from -1 to 1, passes through the critical value

$$p = \sqrt{3}-1; \quad \ldots\ldots\ldots\ldots \quad (32.)$$

in such a manner that we have

$$\mathrm{F}(p) = (2-2p)^{-\frac{1}{2}}, \text{ if } p < \sqrt{3}-1; \quad \ldots\ldots \quad (33.)$$

and, on the other hand,

$$\mathrm{F}(p) = 0, \text{ if } p > \sqrt{3}-1. \quad \ldots\ldots \quad (34.)$$

For the critical value (32.) itself, we have

$$s = \frac{\sqrt{3}-1}{2}, \quad \alpha = 2\sqrt{3}-3, \quad \beta = 1, \ldots \quad (35.)$$

and the real part of (18.) becomes

$$\frac{1-\alpha}{1+2\alpha\cos\theta+\alpha^2}; \quad \ldots\ldots\ldots\ldots \quad (36.)$$

multiplying therefore by $d\theta$, integrating from $\theta = 0$ to $\theta = \pi$, and dividing by π, we find, by (25.) and (14.), this formula instead of (29.),

$$\mathrm{F}(p) = \frac{1}{1+\alpha} = \frac{1}{4s}, \quad \ldots\ldots\ldots \quad (37.)$$

that is,

$$\mathrm{F}(p) = \tfrac{1}{2}(2-2p)^{-\frac{1}{2}}, \text{ if } p = \sqrt{3}-1. \quad \ldots \quad (38.)$$

The value of the discontinuous function F is therefore, in this case, equal to the semisum of the two different values which that function receives, immediately before and after the variable p attains its critical value, as usually happens in other similar cases of discontinuity.

5. As verifications of the results (33.), (34.), we may consider the particular values $p = 0$, $p = 1$, which ought to give

$$\mathrm{F}(0) = 2^{-\frac{1}{2}}, \quad \mathrm{F}(1) = 0. \quad \ldots\ldots \quad (39.)$$

Accordingly, when $p = 0$, the definitions (9.) and (14.) give

$$\vartheta = \sqrt{-1}\sin\theta, \quad \ldots\ldots\ldots\ldots \quad (40.)$$

$$\mathrm{F}(0) = \frac{1}{2\pi}\int_{-\pi}^{\pi}\frac{d\theta}{1-\sqrt{-1}\sin\theta} = \frac{1}{\pi}\int_{0}^{\pi}\frac{d\theta}{1+\sin\theta^2}; \quad (41.)$$

which easily gives, by (24.),

$$F(0) = \frac{2}{\pi}\int_0^{\pi}\frac{d\theta}{3-\cos 2\theta} = \frac{1}{\pi}\int_0^{2\pi}\frac{d\theta}{3-\cos\theta} = 2^{-\frac{1}{2}}. \quad (42.)$$

And when $p = 1$, we have

$$1-\vartheta = -\tfrac{1}{2}(\cos\theta + \sqrt{-1}\sin\theta), \ldots\ldots \quad (43.)$$

$$\frac{1}{2\pi}\,\frac{d\theta}{1-\vartheta} = -\pi^{-1}(\cos\theta - \sqrt{-1}\sin\theta)\,d\theta, \quad (44.)$$

of which the integral, taken from $\theta = -\pi$ to $\theta = \pi$, is $F(1) = 0$.

6. Let us consider now this other integral,

$$G(p) = \frac{1}{2\pi}\int_{-\pi}^{\pi}\frac{\vartheta^n\,d\theta}{\vartheta-1}. \ldots\ldots\ldots \quad (45.)$$

The expression (13.) gives

$$\Sigma_{(n)\,0}^{n-1}\,P_n = F(p) + G(p); \quad \ldots\ldots \quad (46.)$$

therefore, by (34.), we shall have

$$G(p) = \Sigma_{(n)\,0}^{n-1}\,P_n, \text{ if } p > \sqrt{3}-1. \quad \ldots \quad (47.)$$

For instance, let $p = 1$; then multiplying the expression (44.) by

$$-\vartheta^n = -\left(1+\tfrac{1}{2}e^{\theta\sqrt{-1}}\right)^n, \quad \ldots\ldots\ldots \quad (48.)$$

the only term which does not vanish when integrated is $\frac{1}{2}\,n\,\pi^{-1}\,d\theta$, and this term gives the result

$$G(1) = n, \quad \ldots\ldots\ldots\ldots \quad (49.)$$

which evidently agrees with the formula (47.), because it is well known that

$$P_n = 1 \text{ when } p = 1, \quad \ldots\ldots\ldots \quad (50.)$$

the series (2.) becoming then the development of $(1-x)^{-1}$.

7. On the other hand, let p be $< \sqrt{3}-1$; then, observing that, by (33.),

$$F(p) = (2-2p)^{-\frac{1}{2}} = \Sigma_{(n)\,0}^{\infty}\,P_n, \quad \ldots\ldots\ldots \quad (51.)$$

we find, by the relation (46.) between the functions F and G,

$$\left.\begin{aligned} G(p) &= -\,\Sigma_{(n)\,n}^{\infty}\,P_n \\ &= -\,(P_n + P_{n+1} + P_{n+2} + \ldots). \end{aligned}\right\} \ldots \quad (52.)$$

For instance, let $p = 0$; then, by (40.) and (45.),

$$G(0) = \frac{-(\sqrt{-1})^n}{2\pi}\int_{-\pi}^{\pi}\frac{d\theta\,(\sin\theta)^n}{1-\sqrt{-1}\,\sin\theta}; \quad (53.)$$

that is,

$$G(0) = \frac{(-1)^{i+1}}{\pi}\int_0^{\pi}\frac{d\theta\,\sin\theta^{2i}}{1+\sin\theta^2}; \quad \ldots\ldots \quad (54.)$$

if n be either $= 2i-1$, or $= 2i$. Now, when $p = 0$, P_n is the coefficient of x_n in the development of $(1+x^2)^{-\frac{1}{2}}$; therefore,

$$\mathrm{P}_{2i-1} = 0, \text{ when } p = 0, \quad \ldots\ldots \quad (55.)$$

and, in the notation of factorials,

$$\mathrm{P}_{2i} = [0]^{-i}[-\tfrac{1}{2}]^{i} = (-1)^{i}\,\pi^{-1}\int_0^{\pi} d\,\theta \sin\theta^{2i}; \quad (56.)$$

so that, by (54.),

$$\mathrm{G}\,(0) = -\,(\mathrm{P}_{2i} + \mathrm{P}_{2i+2} + \ldots), \quad \ldots\ldots \quad (57.)$$

when $p = 0$, and when n is either $2\,i$ or $2\,i-1$.

8. For the critical value $p = \sqrt{3}-1$, we have, by (38.),

$$\mathrm{F}\,(p) = \tfrac{1}{2}\,\Sigma_{(n)\,0}^{\;\infty}\,\mathrm{P}_n; \quad \ldots\ldots\ldots \quad (58.)$$

therefore, for the same value of p, by (46.),

$$\begin{aligned}\mathrm{G}\,(p) &= \tfrac{1}{2}\,\Sigma_{(n)\,0}^{\,n-1}\,\mathrm{P}_n - \tfrac{1}{2}\,\Sigma_{(n)\,n}^{\;\infty}\,\mathrm{P}_n\\ &= \tfrac{1}{2}\,(\mathrm{P}_0 + \mathrm{P}_1 + \ldots + \mathrm{P}_{n-1} - \mathrm{P}_n - \mathrm{P}_{n+1} - \ldots); \quad (59.)\end{aligned}$$

so that the discontinuous function G, like F, acquires, for the critical value of p, a value which is the semisum of those which it receives immediately before and afterwards.

9. We have seen that the sum of these two discontinuous integrals, F and G, is always equal to the sum of the first n terms of the series (12.), so that

$$\mathrm{F}\,(p) + \mathrm{G}\,(p) = \mathrm{P}_0 + \mathrm{P}_1 + \ldots + \mathrm{P}_{n-1}; \quad (60.)$$

and it may not be irrelevant to remark that this sum may be developed under this other form:

$$\frac{1}{2\,\pi}\int_{-\pi}^{\pi} d\,\theta\,\frac{\vartheta^n - 1}{\vartheta - 1} = \Sigma_{(k)\,1}^{\;\;n}\,[n]^{k}\,[0]^{-k}\,\mathrm{Q}_{k-1}; \;\ldots \quad (61.)$$

in which the factorial expression $[n]^k\,[0]^{-k}$ denotes the coefficient of x^k in the development of $(1+x)^n$; and

$$\mathrm{Q}_k = \frac{1}{2\pi}\int_{-\pi}^{\pi} d\,\theta\,(\vartheta - 1)^{k} \;\ldots\ldots\ldots \quad (62.)$$

Thus

$$\left.\begin{aligned}&\mathrm{P}_0 = \mathrm{Q}_0;\\ &\mathrm{P}_0 + \mathrm{P}_1 = 2\,\mathrm{Q}_0 + \mathrm{Q}_1;\\ &\mathrm{P}_0 + \mathrm{P}_1 + \mathrm{P}_2 = 3\,\mathrm{Q}_0 + 3\,\mathrm{Q}_1 + \mathrm{Q}_2;\\ &\qquad \&\text{c.};\end{aligned}\right\}\ldots (63.)$$

and consequently

$$\left.\begin{aligned}&\mathrm{P}_0 = \mathrm{Q}_0\;;\\ &\mathrm{P}_1 = \mathrm{Q}_0 + \mathrm{Q}_1;\\ &\mathrm{P}_2 = \mathrm{Q}_0 + 2\,\mathrm{Q}_1 + \mathrm{Q}_2;\\ &\qquad \&\text{c.};\end{aligned}\right\}\ldots\ldots\ldots\ldots \quad (64.)$$

which last expressions, indeed, follow immediately from the formula (10.)

10. With respect to the calculation of Q_0, Q_1, &c. as functions of p, it may be noted, in conclusion, that, by (15.) and (62.), Q_k is the term independent of θ in the development of

$$2^{-k}(\alpha + e^{\theta\sqrt{-1}})^k (1-\beta e^{-\theta\sqrt{-1}})^k ; \quad \ldots\ldots \quad (65.)$$

thus

$$\left.\begin{array}{l} Q_0 = 1, \\ Q_1 = 2^{-1}(\alpha-\beta), \\ Q_2 = 2^{-2}(\alpha^2-4\alpha\,\beta+\beta^2), \\ Q_3 = 2^{-3}(\alpha^3-9\,\alpha^2\beta+9\,\alpha\,\beta^2-\beta^3), \\ \quad \&c. \end{array}\right\} \quad \ldots \quad (66.)$$

in which the law of formation is evident. It remains to substitute for a, β, their values (16.) as functions of s, and then to eliminate s^2 by (17); and thus we find, for example,

$$\left.\begin{array}{l} Q_1 = p-1 ; \\ Q_2 = \frac{1}{2}(p-1)(3p-1) ; \\ Q_3 = \frac{1}{2}(p-1)^2(5p+1) ; \\ Q_4 = \frac{1}{8}(p-1)^2(35p^2-10p-13). \end{array}\right\} \quad (67.)$$

This, then, is at least one way, though perhaps not the easiest, of computing the initial values of the successive differences of the function P_n, that is, the quantities

$$\left.\begin{array}{l} Q_0 = \Delta^0 P_0 = P_0, \\ Q_1 = \Delta^1 P_0 = P_1 - P_0, \\ Q_2 = \Delta^2 P_0 = P_2 - 2P_1 + P_0, \\ \quad \&c. \end{array}\right\} \quad \ldots\ldots \quad (68.)$$

And we see that it is permitted to express generally those differences, as follows:

$$\Delta^k P_0 = s^k \Sigma_{(i)0}^{\;k}(-1)^i ([k]^i [0]^{-i})^2 (1+s)^i (1-s)^{k-i} ; \quad (69.)$$

in which

$$s^2 = \tfrac{1}{2}(1-p). \quad \ldots\ldots\ldots \quad (70.)$$

Observatory of Trinity College, Dublin,
Feb. 12, 1842.

XLIV. *On the Constant Voltaic Battery.* *By* J. F. Daniell, *For. Sec. R.S., Prof. Chem. in King's College, London; in a Letter addressed to R. Phillips, Esq., F.R.S., &c.*

My dear Sir,

IN the *Annales de Chimie et de Physique* for December 1841, there is a paper by M. Edmond Becquerel entitled "Sur les piles à Courant Constant," upon which I beg your permission to make a few observations in the Philosophical Magazine. The object of the author is thus stated:—

"A l'époque actuelle, où l'on cherche de toutes partes à appliquer les sciences physiques et chimiques, et par conséquent l'action des forces électriques, aux arts industriels, je pense qu'il peut être utile de présenter succinctement l'exposé de toutes les recherches qui ont été faites pour obtenir des piles dont l'action soit constante pendant un certain temps." (*Ann. de Chimie*, tome iii. p. 436.)

In this opinion I perfectly concur; and it is for the purpose of rendering such an account more perfect that I desire to correct some misconceptions into which M. E. Becquerel has fallen, and which have the effect of greatly mystifying the origin of constant batteries. After stating the well-known defects of voltaic batteries of the common construction, in which the two metals are plunged into the liquid in the same cell; and the necessity of constructing batteries of constant action before they could be usefully employed in the arts, he proceeds to make the following comprehensive claim:—

"C'est mon père qui a donné le premier les principes sur lesquels est fondée la construction de ces piles et qu'a formé les premiers piles de ce genre." (*Ann. de Chim.* p. 437.)

Now as I have claimed to be the inventor of "the constant battery" (which was so named by myself), and as the Council of the Royal Society have so far sanctioned this claim as to award me the Copley Medal for my invention; and as I have most undoubtedly worked out the principles of its construction by experiments and legitimate induction without the slightest suspicion that M. Becquerel had preceded me in the investigation, I was naturally very anxious to examine the evidence upon which this assertion is founded.

Previously to stating this evidence, M. E. Becquerel makes some remarks upon the phænomenon which has been most inappropriately termed the *polarization* of the plates of a voltaic battery, of which his father, he observes, has given the simple explanation.

"Dans le passage d'un courant électrique à travers un liquide conducteur voici ce qui a lieu: quand le courant primitif traverse le liquide conducteur, des élémens acides sont transportés au pôle positif, et des élémens alcalins au pôle négatif; alors, en interrompant la communication entre les lames décomposantes (ou électrodes) et la pile, les deux lames se comportent vis-à-vis l'une de l'autre comme deux lames que l'on aurait plongées, l'une dans un faible dissolution alcaline, l'autre dans une faible dissolution acide: c'est-à-dire, qu'en les mettant en rélation avec un multiplicateur, il y aura production d'un courant électrique dû à la réaction des molécules acides ou se comportant comme telles, sur les molécules alcalines par l'intermédiaire du liquide: ce courant sera par conséquent dirigé en sens inverse du courant initial. Cet effet se manifeste toujours lorsqu'un courant traverse un liquide conducteur quelconque au moyen de deux lames métalliques. Cette polarisation des électrodes a donc lieu dans toutes les piles, lorsque les lames qui compose chaque couple plongent dans un même liquide et dans un même auge." (Pp. 437–8.)

This hypothesis, however, though it borders upon the true explanation, is not correct; for in my first paper upon "Voltaic Combinations" (Phil. Trans., 1836, p. 116), I have shown that the polarization of the plates, and the rapid decline, and final cessation, of the current in batteries of the common construction, is owing to the deposition of metallic zinc upon the conducting plates. This I at first ascribed to the deoxidating power of the hydrogen upon the oxide of zinc, but have since adduced evidence to prove is owing to the direct electrolysis of the sulphate of zinc formed. The deposit I was able to detach from the platinum, upon which it had been precipitated, in plates of considerable thickness. This opposition of zinc to zinc in the acid is sufficient to account for the result without supposing "the reaction of alkaline matter transported to the conducting plate upon acid transported in like manner to the zinc plate through the intervening fluid." It is difficult, indeed, to conceive that the latter should not rather increase the power of the direct current, by its action upon the zinc with which it must be in contact, than produce a counter current by its action upon the alkaline matter upon the distant platinum plate.

The problem of a constant battery, which M. E. Becquerel says that his father has solved, is thus stated:—

"Si donc on pouvait enlever continuellement les élémens alcalins et acides qui se déposent sur les lames, on anéantirait le courant secondaire, et la pile aurait une intensité constante, en tant cependant que les surfaces des lames resteraient à peu près aussi nettes et que l'action du liquide sur le zinc serait à peu près la même." (P. 438.)

It appears from the statement of M. E. Becquerel, that in the year 1829 his father contrived an apparatus consisting of a glass cell or rectangular vessel, the interior of which was divided into three compartments by two diaphragms of goldbeater's skin, admitting of no communication except through the membranes placed for the purpose of retarding the mixture or combination of the liquids contained in each.

The bottom of the cell was perforated only in the centre compartment by a small opening, so that upon plunging the cell into another vessel containing a conducting fluid, the liquids in the two extreme compartments would mix with great difficulty: the central aperture admitted also of being closed, and the middle compartment could then be filled with the liquid of one of the extreme compartments. A plate of zinc was immersed in one and a plate of copper in the other, and the two were placed in metallic communication by means of the wires of a galvanometer.

Under these circumstances, when all the compartments were charged with dilute sulphuric acid ($\frac{1}{50}$th acid), a deviation of the galvanometer needle was produced, which, as with an ordinary couple, gradually decreased. Upon consulting M. Becquerel's original memoir (*Ann. de Phys. et de Chim.*, tome xli. p. 21), I find the following tabular record of the results:—

Time of Immersion.	Deviation of Needle.
0′	63°
15′	53
30′	46

Upon re-commencing the experiment with the addition of $\frac{1}{50}$th nitric acid to the dilute sulphuric acid in the copper compartment, the intensity of the current changed, but gradually diminished as in the following table:—

Time of Immersion.	Deviation of Needle.
0′	81°
15′	73
30′	65

Upon the substitution of nitrate of copper for the nitric acid, the results were sensibly the same. M. Becquerel varied these experiments in many ways, and with the aid of this apparatus arrived at the following conclusions:—

"The maximum of intensity is obtained by immersing the copper plate in a solution of nitrate of copper, and the zinc in a solution of sulphate of zinc, *but there is also a diminution with time; a little less rapid however than with an ordinary couple.*"

The following is the table of the last results:—

Time of Immersion.	Deviation of Needle.
0′	84°
15′	72
30′	68

The next paragraph of M. E. Becquerel's 'Notice' I will give in his own words for fear of mistakes:—

"Si l'on met de l'acide nitrique étendu dans la case zinc, un peu d'eau et d'acide sulfurique dans la case cuivre, et que dans la caisse A A′ on n'emploie qu'un diaphragme, ou que l'on rapproche les deux diaphragmes de telle sorte que l'acide nitrique de la case zinc puisse passer lentement dans la case cuivre, afin d'augmenter la conductibilité, alors on peut, au lieu de rendre les intensités décroissantes, les rendre croissantes pendant la première demi-heure; cela dépend de la quantité d'acide nitrique versée dans la case zinc: *si l'on en met avec précaution, on peut arriver à obtenir un courant constant pendant une demi-heure ou une heure.* Cette disposition du couple réalise donc pendant un certain temps le résultat que nous avons annoncé, c'est-à-dire détruit le courant secondaire; *car l'acide nitrique qui se trouve dans la case zinc s'empare constamment du cuivre de la solution, qui, après avoir traversé les diaphragmes, se dépose sur le zinc.*

"Ainsi ces expériences montrent la possibilité d'obtenir un courant constant en détruisant le courant secondaire." (P. 440.)

Now, most assuredly, the principles and results derived from these experiments are not the principles and results of "the constant battery:" they are not the principles and results of my battery, which M. Becquerel has nevertheless described as—"construit d'après les principes exposés précédemment," and, as one amongst other similar ones, "d'un usage plus facile et dont l'action est constante pendant un temps plus long." (P. 444.)

The amount of *constancy* which M. E. Becquerel obtained, and the certainty with which he obtained it, are thus described in his original memoir:—

"Il m'est arrivée *plusieurs fois* d'obtenir une compensation telle que les déviations de l'aiguille aimantée étaient constants pendant une heure, avantage que l'on n'a jamais avec les piles ordinaires;" and M. E. Becquerel, as we have just seen, says that "by adding nitric acid with *precaution* one may *manage* to obtain a constant current for half an hour or an hour."

With my constant battery, constructed with siphon tubes, as described by M. E. Becquerel, a steady current might be kept up, if necessary, for a week together, provided a proper supply of materials were furnished; and the amount of force set in action by it would be measured not by some 60° of the galvanometer, but by the number of yards of platinum wire which it would fuse or render red-hot.

Even in the use of the diaphragm*, which might at first sight appear to be similar in the two constructions, there is direct opposition; for my object is to keep the two electrolytes which I employ perfectly separate, so that no portion of one may penetrate to the other, except in the process of electrolysis, while, according to M. Becquerel's principles, it

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* The passage of the voltaic current through diaphragms of bladder was well known to experimentalists before the publication of M. Becquerel's researches. The late Dr. Ritchie, amongst others, made frequent use of them; and in the Phil. Trans. for 1829, p. 363, there is a paper by that gentleman, in which "a small rectangular box divided into two compartments by a diaphragm of bladder" is described, which was used for the purpose of exposing the plates of a galvanic circle to two different liquids.

is necessary that they should slowly mingle; and the diaphragm is placed to regulate the mixture. In his own words it is necessary " que l'on approche les deux diaphragmes de telle sorte que l'acide nitrique de la case zinc *puisse passer lentement* dans la case cuivre."

The purpose which the nitric acid is said to effect, is to me perfectly incomprehensible, namely, that of " taking up the copper of the solution, which after having traversed the diaphragms is deposited upon the zinc."

According to my experience, under these circumstances the deposited copper would be untouched and the local solution of the zinc greatly promoted: but my present object is not to controvert M. E. Becquerel's statements further than is necessary to show that the principles which he has derived from his father's experiments are not the principles of my "constant battery."

M. Edmond Becquerel, however, seems to have some misgivings that the premises upon which he has founded his father's claim may not be deemed sufficient for the purpose; for he proceeds,—

" Ainsi ces expériences montrent la possibilité d'obtenir un courant constant en détruisant le courant secondaire; c'est-à-dire en faisant plonger les lames dans des liquides différents. *Plus tard mon père a résolu complètement la question.*" (P. 440.)

This *complete* solution of the problem is comprised in the apparatus which M. Becquerel has named "chaine simple à oxygène." It consists of two small glass vessels, of which one contains a concentrated solution of caustic potassa, and the other a solution of strong nitric acid. These two vessels are connected together by a bent glass tube fitted with porcelain clay wetted with a solution of sea salt. In each vessel is placed a small plate of platinum; and when a metallic communication is made between the two, a current is established of sufficient energy to cause the evolution of oxygen from the plate in the potassa, while the equivalent hydrogen is absorbed by the nitric acid at the opposite plate. When the communication is made between the plates by means of a galvanometer, the needle is deflected. This slow current will remain constant for twenty-four hours, or longer; but it ceases when the nitrate of potassa formed in the connecting-tube crystallizes.

Now, it will be observed, that the porous diaphragm is here again employed, not to produce, as in my "constant battery," as complete a separation as possible between the liquids, but to regulate their mixture, which is essential both to the production of the primary current and, according to M. Becquerel, to the destruction of the secondary one.

"Il faut que les métaux qui composent chaque couple plongent dans des liquides différents, séparés par une membrane ou un diaphragm *capable de laisser traverser peu à peu les liquides*, cette disposition étant la seule par laquelle le courant secondaire produit par la polarization des électrodes puisse être détruit." (P. 442.)

M. E. Becquerel, however, allows that even this apparatus, which *so completely* solves the problem of the constant battery, is incapable of application.

"Ce dernier appareil résout complètement la question sous le point de vue scientifique, mais *pour les applications* le courant électrique n'aurait pas une énergie suffisante, car alors il faudrait de plus grands éléments et en plus grand nombre, ce qui serait assez coûteux."

He adds, "il s'ensuit que ces appareils, qui forment pile, ont une intensité d'action constante pendant un temps considérable: *cette condition n'est remplie que quand l'action est très lente*." (P. 442.)

It is difficult, indeed, to conceive that M. E. Becquerel can be serious in attempting to trace an analogy between this arrangement, or any of the similar apparatus formed of "tubes en U," which his father has so successfully applied to the investigation "des actions lentes" and "the constant battery," whose object is the generation of voltaic power, with a constancy and rapidity which promises to render it applicable not only to manufacturing but mechanical purposes.

The very source of the current in the "chaine simple à oxygène" is different from that of "the constant battery." M. Becquerel, in his *Traité de l'Electricité* (tome iii. § 526), ascribes it to the slow and direct combination of the acid and alkali; but this explanation will not be deemed satisfactory, since Dr. Faraday has shown that the decomposition and polarization of the molecules of an electrolyte are essential to the establishment of a current. I have myself suggested an explanation of the undoubted phænomena of current and decomposition which take place, which is quite compatible with these conditions (Phil. Trans. 1840, p. 223). The origin of the force is however, doubtless, to be found at the point of contact of the two liquids, and not at the electrodes; and I repeat, that in my "constant battery" nothing depends upon the contact and action of the two liquids upon each other.

The amount of force is also perfectly insignificant with regard to its application to the arts. It would be difficult to obtain by this arrangement a decomposition amounting to $\frac{1}{100}$th of a cubic inch of hydrogen and oxygen per minute, while with a constant battery of my construction it is easy to maintain a current equivalent to 20 cubic inches of the mixed gases in the same time. It is scarcely necessary to observe, that the difficulty of maintaining constancy of action is, of course, proportioned to the energy of the action.

It is this almost unlimited command of steady force which renders its application, either in the electrolytic or magnetic direction, possible to mechanical or manufacturing purposes: magnetic locomotive engines never could have been dreamt of without the solution of this preliminary problem.

M. E. Becquerel proceeds to say, "M. Daniell, *d'après les principes exposés précédemment*, a construit une pile dont nous allons donner la déscription, et qui est à présent généralement employée." (P. 442.)

Now I not only most emphatically deny that I was in any degree guided by these principles, but I assert that they were incapable of leading me to any such conclusion; as they, in fact, failed to lead M. Becquerel to the same.

In proof of this, I will briefly recapitulate the real principles of "the constant battery;" the different steps of the investigation which preceded its invention being all clearly stated in my papers in the Philosophical Transactions.

1. In the first place, I traced the origin of the decline and ultimate annihilation of the current in the common voltaic battery not to the evolution of acid and alkaline matter at the opposite plates, and the consequent establishment of a counter current by their mutual reaction, but to the deposition upon the conducting plate of a substantial coating of pure metallic zinc, the weight of which upon a platinum plate of three inches by one I have stated to be nearly 29 grains. In consequence of this, zinc becomes opposed to zinc in the circuit, and all current is stopped.

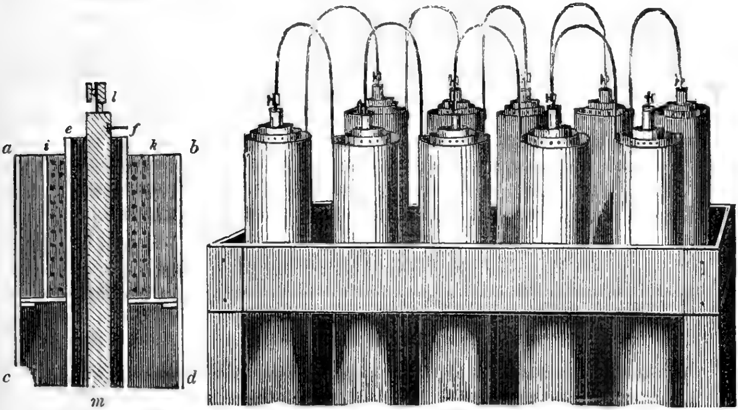
2. To prevent this deposition of active metal upon the conducting metal, it occurred to me,—1st, to divide the portion of the electrolyte in contact with the generating plate from that in contact with the conducting plate by a porous diaphragm, by which the solution of zinc is prevented from reaching the copper or platinum; and 2ndly, to provide for the perpetual renewal of the surface of the conducting plate by the deposition of fresh inactive metal upon it, transferred by the process of electrolysis from the solution of a salt selected for the purpose and placed in the partition next to it, or precipitated by the secondary action of the hydrogen evolved upon it. By this contrivance, not only is the surface of the conducting plate perpetually renewed, but the opposing influence of the hydrogen during its evolution is removed, and the battery is at once rendered constant and its power greatly exalted.

As copper, from its comparative inactivity, its great conducting power, and its commercial value, is the only metal which is capable of being applied extensively for the construction of voltaic batteries upon a large scale, I constructed my cells of that metal; placing in contact with them a saturated solution of sulphate of copper in dilute sulphuric acid (8 measures of water, 1 oil of vitriol), separating this portion of the electrolytes from that which is in contact with the amalgamated zinc, and which is composed of the same diluted acid without the salt of copper, by a diaphragm of membrane or porous earthenware. Under this arrangement a part of the current is conveyed by the electrolysis of the sulphate of copper and a part by the dilute sulphuric acid: from the former the copper is directly deposited in that compact form which is now so well known from the formation of voltatypes, and by the latter it is thrown down in a less coherent form by the secondary action of the hydrogen; but both concur in the effects which have been previously described.

The only practical difficulty in this construction is the formation of diaphragms, capable, at once, of allowing the electrolytic action to proceed through them, and of perfectly preventing the mixture of the liquids on their opposite sides; the latter action being, it will be remembered, one of the essential principles of the batteries "pour les actions lentes" upon M. Becquerel's construction. If a portion of the solution of copper penetrates to the zinc side of the diaphragm it is decomposed, and the copper deposited upon the zinc producing strong local action and loss of the generating metal; for I have not been fortunate enough to find, with M. Becquerel, that the copper can be dissolved under such circumstances.

Such are the principles which I have derived from experiments, and which are capable of application to the construction of powerful constant batteries of different forms and materials. The form which I have preferred is that of hollow cylinders of conducting metal, with central rods of generating metal. This form is dependent upon another principle, which I also worked out from experiments upon the diffusion of the force of a small generating surface over a large conducting surface. These have also long since been published in detail in the Philosophical Transactions, but they are not alluded to in M. E. Becquerel's 'Notice.' This cylindrical arrangement is one of great capacity, and admits of the employment of large quantities of the electrolytes which are required for energetic and long-continued action.

I subjoin a sketch of the simple form in which the constant battery is now generally constructed, which differs from that which I first employed, and which is described by M. E. Becquerel, in the omission of the siphons; which are not necessary except in cases where the action is required to be maintained for more than ten or twelve hours at a time.



a b c d is a copper cylinder, in which is placed a smaller cylinder *e f* of porous earthenware; upon the upper part of the copper cylinder rests a perforated colander *i k*, through which the earthenware cylinder passes: *l m* is a cast rod of amalgamated zinc, resting upon the top of the interior cylinder by a cross piece of wood, and forming the axis of the arrangement. The cell is charged by pouring into the earthenware cylinder water acidulated with one-eighth part of its bulk of oil of vitriol, the space between the earthenware tube and the copper being filled with the same acidulated water saturated with sulphate of copper, and solid sulphate of copper being placed in the colander.

But, of course, the principles of the construction are independent of form and materials, and are capable of application to flat, square and equal surfaces of the two metals as well as to concentric arrangements. They admit also of the employment of different metals and of different electrolytes. They are not changed by placing the zinc on the outside instead of the inside of the copper, nor even by altering the name of "the constant battery" to that of the "sustaining battery."

I will embrace this opportunity of observing that M. Becquerel, in his *Traité de l'Electricité* (tome v. livre xiv. p. 195), has inadvertently committed an error affecting the history of "the constant battery," in giving priority to Professor Grove's experiments with the nitric acid battery over mine. He describes that gentleman's construction, and then proceeds, "*Les choses en étaient là* lorsque M. Daniell a repris la question, qu'il a analysé avec sagacité. Ses recherches l'ont mis à même de doter la science d'une pile construite *d'après les principes précédentes*, et qui est aujourd'hui généralement adoptée dans les expériences de physique."

It is only necessary to recall the data of my first paper upon the subject in the Philosophical Transactions, viz. February 11th, 1836, and that of Prof. Grove's communication to the French Academy, I believe, April 1839. Prof. Grove has never spoken of his battery but as the further application of principles which I had previously deduced.

In conclusion, I cannot but express my regret that the filial piety of M. Edmond Becquerel should have betrayed him into an act of injustice by preferring a claim for his father which is totally unsupported by the facts of the case, and from which his well-earned reputation can derive no permanent extension.

I remain, dear Sir,
Very faithfully yours,
J. F. Daniell.

King's College, London,
March 1842.

To Richard Phillips, Esq., &c. &c.

XLV. *On the Theory of the Dispersion of Light. By* S. Earnshaw, *M.A., Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I HAVE observed with great interest the efforts which mathematicians have made to establish the undulatory theory of light on a firm basis. That theory (its fundamental hypothesis being admitted) has succeeded so well in accounting for a great variety of intricate and delicate phænomena, that I am not much surprised to observe that some of its leading admirers have adopted the opinion that its truth is now beyond controversy. Among its greatest advocates, however, are some who do not hesitate to allow that certain optical phænomena have not been fully accounted for on their favourite theory, and that others have been as yet explained only in a doubtful manner. I hope therefore that, without incurring the risk of being reckoned an anti-undulationist, I may be permitted to make a few remarks on that "opprobrium of all theories—the dispersion of light." A compendious view of the present state of this portion of the general theory of undulations may be found in a book lately published by Professor Powell[*]. In that work are contained several sets of tables, which exhibit,

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[*] The Undulatory Theory as applied to the Dispersion of Light, &c.

for a very extensive range of substances, the indices of refraction as found both by experiment and by theory. The near agreement of results, as there exhibited, is such as cannot fail to impress the reader, who is content with a general view of them, with most favourable sentiments respecting the theory from which they are derived. I have however dipped deeper into the matter, and by executing the calculations of several cases have arrived at results which I desire now to set before your readers, chiefly with a view of eliciting from some one such an explanation as shall induce me to assent to the opinion of Professor Powell, that "the theory of undulations supplies at once both the laws and the explanation of the phænomena of dispersion."

The first method of calculation is the one which was made use of by Professor Powell in three papers printed in the Transactions of the Royal Society, under the title "Researches towards establishing a theory of the Dispersion of Light," and distinguished as Nos. I. II. III. The formula employed is equivalent to the following:—

$$\mu = \mathrm{H}\left\{\frac{\left(\frac{\mathrm{A}}{\lambda}\right)}{\sin\left(\frac{\mathrm{A}}{\lambda}\right)}\right\}. \quad \ldots\ldots\ldots (1.)$$

This equation being directly furnished by theory, it is a matter of intense interest to see how far its truth is supported by experiment. I have not however met with any calculations founded upon it, except in the papers just referred to, and therefore it is that in my remarks upon it I shall be under the necessity of referring to an inadvertence in those papers which I should otherwise gladly have left unmentioned. After comparing the results of theory with those furnished by experiment, Professor Powell comes to the conclusion that "the refractive indices are related to the lengths of waves, as nearly as possible, according to the formula deduced from M. Cauchy's theory." Now, if I mistake not, in all those papers, the author, by forgetting to reduce Fraunhofer's values of λ in air to the proper medium, did in fact employ a formula which differs from the correct one furnished by theory; for, rectifying the oversight, the true formula supplied by M. Cauchy's theory is the following:—

$$\mu = \mathrm{H}.\frac{\sin^{-1}\left(\frac{\mathrm{A}}{\lambda}\right)}{\left(\frac{\mathrm{A}}{\lambda}\right)}, \quad \ldots\ldots\ldots (2.)$$

H and A being constant for the same medium, and the values of λ to be used being those which belong to air.

Both these formulæ are of the form $\frac{\theta}{\sin\theta}$, but though from this circumstance they might be supposed to be similar, they do in fact materially differ; for in the former it is the arc θ which for different rays is proportional to $\frac{1}{\lambda}$, while in the latter it is the sin θ which is proportional to $\frac{1}{\lambda}$; a distinction which will be considered very important, when it is remembered that the results are expected to be accurate in the third place of decimals, and to approximate to the figure in the fourth place, and that the values of θ are by no means small. Let us consider the case of flint glass, No. 13: the values of θ in "Research No. I." range from 16° 10′ for letter B, to 27° 39′ for H. In the formula (2.) I find the corresponding values to be $15^{\circ}\ 21\frac{1}{2}'$ and 27° 22′; consequently the range of θ in the latter case is greater than in the former by above half a degree; an excess, which, being nearly the 30th part of the value of θ for B, and about the 20th part of the *whole range* of θ, must be expected to produce some manifest discrepancy of results.

As no applications of the formula (2.) have ever, to my knowledge, been made public, I have appended the following tables, which are computed by it. The second and third columns contain the indices of refraction by experiment and by theory, and the last shows the error of theory. In the first table $H = 1{\cdot}6083$, and for line B, $\theta = 15^{\circ}\ 21\frac{1}{2}'$; in the second table, the corresponding quantities are $H = 1{\cdot}5548$ and $\theta = 22^{\circ}\ 43'$.

Flint Glass, No. 13.				Oil of Cassia, No. i.			
Ray.	True Index.	Computed.	Error.	Ray.	True Index.	Computed.	Error.
B	1·6277			B	1·5963		
C	1·6297	1·6297	0	C	1·6007	1·6009	+ 2
D	1·6350	1·6347	—3	D	1·6104	1·6131	+27
E	1·6420	1·6422	+2	E	1·6249	1·6297	+48
F	1·6483	1·6486	+3	F	1·6389	1·6449	+60
G	1·6603	1·6609	+6	G	1·6698	1·6723	+25
H	1·6711			H	1·7039		

The constants H A were in both these cases determined by assuming the indices for B and H to be the same by theory as experiment.

How much soever we might be disposed, after inspecting the former of these tables, to think it confirmatory of theory, the wide discrepancies of the latter entirely obliterate the favourable impression. If upon the same line of abscissæ we construct two curves, the ordinates of which respectively represent the values of μ as found by theory and by experiment, the abscissa being proportional to the corresponding values of $\frac{1}{\lambda}$, we find that for flint glass they intersect in four points, the first and last of which are the extreme points, which were assumed to be the same in the two curves; the other two points are at C, and a point about midway between D and E. An inspection of the figure will show, that for about a third part of the whole there is a kind of proximity of which we might be disposed to think favourably, were it not that in the remaining $\frac{2}{3}$rds of the figure there is precisely the same *sort* of dissimilarity as is exhibited in a magnified form by the oil of cassia, where there is no temptation to consider the curves as having the least trace of similarity, for they have merely common extremities. To exhibit this in the case of oil of cassia in a still stronger light, I have determined the constants H and A, on the supposition that the two curves coincide for letters B and F. The following table exhibits the errors for the other fixed lines:—

B	C	D	E	F	G	H
*	−5	+6	+7	*	−34	−149

From this statement it will be clear that the errors of theory in the case of the oil of cassia are far too great to allow of their being ascribed to the experimental results; and when we consider the extreme accuracy of Fraunhofer, by whom the data for the flint glass are furnished, and that it has just been shown that the errors in this case are of the *same nature*, though not so great, as in the other, I am inclined to assert that they are also *as real* in one case as in the other, and am led to infer, that the formula deduced from M. Cauchy's theory does not agree sufficiently with experiment to warrant us in considering "the opprobrium of all theories of light" as having been completely removed by it from the undulatory theory.

As, however, the formula we have been considering is the one uniformly arrived at by all who have written on the subject, whatever were the peculiar hypotheses upon which their investigations were commenced and conducted, we ought not hastily to throw it aside. It has besides a strong claim upon our indulgence arising from the fact stated by Prof. Powell, "that it is certain that such a formula affords the *closest* accordance with truth throughout the whole range of low-dispersive substances; and even among many of the higher it gives a very near approach to such an agreement." The following remark may perhaps on this account be worthy of notice.

The constant represented by A' in equation (1.) is equal to $\pi \Delta x$, where Δx is the distance between two adjacent particles of the refracting medium. Now in flint glass for the ray B Professor Powell found $\frac{\pi \Delta x}{\lambda} = 16° \ 10'$; from which it follows that $\frac{\lambda}{\Delta x}$ is less than 12; for the ray H it is less than 7: and in oil of cassia it is less than 5. $\frac{\lambda}{\Delta x}$ is the number of particles of the medium which lie within a wave's length; and it has always appeared to me highly improbable that this number should ever be so small as 5, 7, or 12; in fact it seems more likely (from the analogy of sound) that it ought to be a very high number. On this ground I feel less reluctance in yielding to the force of the argument before stated. It is also worth inquiring what effect the comparative largeness of Δx (if allowed) may produce upon the convergency of the series which occur in the investigations from which our formula for μ is derived.

This letter having already exceeded the limit which I proposed to myself in setting out, it is necessary that I should state in as few words as possible my remarks on the other methods which have been made use of in the verification of theory. The principle of them amounts to this:—A series in inverse powers of λ with indeterminate coefficients is assumed to represent μ. This series is then assumed to be so rapidly convergent that all the terms after the first three may be omitted: the coefficients of these terms are found by assuming that the abridged series accurately gives the values of μ for three of the seven fixed lines; and lastly, the remaining four lines are used as a test of the truth of the *undulatory theory*. Now I cannot refrain from asking, what has all this process of assumption to do with the undulatory theory? Surely it will not be denied, that it is a common and long-used *method of interpolation*. Then where has such a connexion been proved to exist between the undulatory theory and the principles of interpolation, as to make it follow, that when the latter furnishes true results the former is necessarily true? Perhaps it will be said that the undulatory theory suggested the *form* of the assumed series. It is a sufficient answer to this, that that form might have been conjectured from the mere inspection of the corresponding values of λ and μ. After all, are we sure that the form suggested is the correct one? Let any one, who is inclined to think that it is, determine the coefficients from the lines B, C, D, and with them calculate the indices for E, F, G, H, and I entertain no doubt he will be soon willing to resign the opinion. Again, if the method be one of interpolation only, it will naturally follow that the more widely the quantities are separated between which we interpolate, *cæteris paribus,* the greater are the errors to be expected in our results. Now this is so invariably found to be the case, that I believe Professor Powell has allowed that the errors increase with the greater dispersion of the substances employed. But that which strongly inclines me to the opinion that the method we are discussing is wholly unconnected with the truth of the undulatory theory, or indeed of any theory, and is a method of interpolation only, is the fact, that in the first applications of the method the same inadvertence with regard to the values of λ was committed as I have before alluded to, as occurring in "Researches Nos. I. II. III.," yet the results, though deduced from values of λ *not countenanced* by theory, were hailed as *confirmatory* of the truth of theory.

But it is time to conclude these remarks, and therefore I will only add, as a general observation, that as the index for letter G is notoriously the most refractory, no method is in my opinion worth attention, even though it *were* furnished by theory, in which it is necessary to assume the indices for both F and H in order "to tame that ray." Yet even with the assumption of these, the tables for the more highly dispersive substances given in Professor Powell's book exhibit errors which show that G is as untamed as ever, to say nothing of the other lines in several instances. On these grounds, I cannot avoid coming to the conclusion, that the methods of computation employed in compiling the tables contained in the book just referred to are such as are wholly unconnected with a physical theory of dispersion, and therefore their results, were they even coincident with experiment, add nothing to the strength of M. Cauchy's theory; and, were they even more discordant than they are with experiment, tend in no degree to overturn it. In this opinion I may perhaps be singular; but as it is just possible that a similar idea may have presented itself to others who, like myself, wish to have the impression, if erroneous, removed, I have ventured to make my doubts public, and shall be thankful if by doing so I should be so fortunate as to elicit an explanation from some of your more experienced correspondents.

I am, Sir, your obedient Servant,

January 1, 1842. S. Earnshaw.

XLVI. *On Atmospheric Refraction. By the Rev.* R. Murphy, *M.A., &c.*[*]

§ 1. *Hypothesis.*

THE refracting power of the atmosphere is a function of its distance from the centre of the earth, and tends in the direction of that centre. The curve described by a luminous ray in its passage through the air is governed by the usual laws of the trajectories of bodies acted on by centripetal forces.

§ 2.—Let the velocity of light entering the atmosphere = 1, and v that at which it arrives at the earth's surface.

Let r be the distance of a point in the trajectory of the ray from the earth's centre, and $\phi(r)$ the force acting then on light; we have

$$v^2 = 1 + 2 \int_r \phi(r);$$

the limits of r are 1 (the earth's radius) and $1+h$ (h being the height of the atmosphere). Hence v^2 is constant for all incidences, and we may put $v = 1 + m$ where m is a certain constant.

§ 3.—Let θ be the angle made by the radius vector drawn to the earth's centre when the ray enters the atmosphere with that ray, and z' be the apparent zenith distance.

The perpendicular on the ray as it enters the atmosphere drawn from the earth's centre $= (1 + h) \sin\theta$, and that as it enters the eye $= \sin z'$. These are inversely proportional to the corresponding velocities, viz. 1 and $1+m$; hence

$$(1 + h) . \sin\theta = (1 + m) \sin z'.$$

§ 4.—Let the observed body be the moon, at a distance a, reckoning in radii of the earth, from the centre.

Let z be its true zenith distance seen from the earth's centre, corresponding to the apparent zenith distance z'.

Let p be the moon's parallax at that zenith distance, and let ω be the angle at the moon subtended by the bent trajectory of the luminous ray. Then $p + \omega$ is the angle at the moon made by the radius vector from the earth's centre with the *issuing* ray, which being the same that enters the atmosphere to reach the eye, we have

$$a \sin(p + \omega) = (1 + h) . \sin\theta.$$

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[*] Communicated by the Author.

Hence by section 3,

$$\sin z' = \frac{a}{1+m} \cdot \sin (p + \omega).$$

§ 5.—Now z being the true zenith distance, $z + p + \omega$ is the angle which the issuing ray makes with the spectator's vertical, and this angle diminished by the refraction r is the apparent zenth distance. Hence

$$\sin (z + p + \omega - r) = \frac{a}{1+m} \sin (p + \omega).$$

§ 6.—The right line drawn from the moon to the spectator makes with that drawn to the centre of the earth the angle p, and with the spectator's vertical $z + p$; hence

$$\sin (z + p) : \sin p :: a : 1,$$

or

$$\sin (z + p) = a \sin p.$$

Hence

$$\sin z \cos p = (a - \cos z) \sin p$$

$$\tan p = \frac{\sin z}{a - \cos z}$$

$$\sec^2 p = \frac{a^2 - 2 a \cos z + 1}{(a - \cos z)^2}$$

$$\cos^2 p = \frac{(a - \cos z)}{a^2 - 2 a \cos z + 1}$$

$$\sin^2 p = \frac{\sin^2 z}{a^2 - 2 a \cos z + 1}.$$

Therefore

$$\cos p = \frac{a - \cos z}{\sqrt{(a^2 - 2 a \cos z + 1)}}$$

$$\sin p = \frac{\sin z}{\sqrt{(a^2 - 2 a \cos z + 1)}}.$$

§ 7.—By section 5,

$$\sin (z + \omega - r) . \cos p + \cos (z + \omega - r) \sin p$$

$$= \frac{a}{1+m} (\sin \omega \cos p + \cos \omega \sin p).$$

Hence

$$\cos p \{(1 + m) \sin (z + \omega - r) - a \sin \omega\}$$

$$= \sin p \{a \cos \omega - (1 + m) \cos (z + \omega - r)\}.$$

Therefore, by section 6,

$$\sin z \{a \cos \omega - (1 + m) \cos (z + \omega - r)\}$$

$$= (a - \cos z) \{(1 + m) \sin (z + \omega - r) - a \sin \omega\}.$$

This would be the complete solution of the question, were ω known.

§ 8.—The angle ω is a function of the parallax, and is evidently very small compared with it, being less than the angle at the moon, subtended by a tangent to the ray drawn from the eye and bounded by the atmosphere.

Moreover, ω vanishes and changes sign at the same time with the parallax p, and is therefore of the form $k\,p + l\,p^3$, &c., where k, l, &c. are constants: for an approximation we may reject the higher powers of p and find $\omega = k\,p$, where k is a small fraction.

§ 9.—By comparing sections 5 and 6 with this result, the law of atmospheric refraction would be given by the two equations,

$$\begin{cases}\sin\{z-r+(1+k).p\} = a'\sin(1+k)\,p\\ \sin(z+p) = a\sin p.\end{cases}$$

The elimination of p between these equations, and the determination of the constants by observation, would give the approximate formula for the relation between the refraction and true zenith distance, which of course would apply to light coming from any heavenly body, though originally deduced from but one of them.

§ 10.—The equations of section 9 may be written in terms of the apparent zenith distance z',

$$\begin{cases}\sin z' = c\,a\sin(1+k)\,p\\ \sin(z'+r-k\,p) = a\sin p,\end{cases}$$

where c is an absolute constant $= \dfrac{1}{1+m}$; a is arbitrary, but had better be large, and k is a small constant and a function of a. The elimination of p gives the relation between z' and r.

§ 11.—Approximation by rejecting k^2p^2, $a\,p^3$, &c., and eliminating

$$\sin z' = \frac{\alpha\sin(z'+r)}{1-\dfrac{2\beta}{\alpha}.\cos(z'+r)},$$

or

$$\sin z' = \alpha\sin(z'+r) + \beta\sin 2(z'+r),$$

α differing but little from unity, and β being a small fraction.

R. M.

XLVII. *On the Composition of Wolfram.* *By* W. H. Miller, *Esq., Professor of Mineralogy in the University of Cambridge*[*].

THE analyses of wolfram from a number of different localities by Count Francis Schaffgotsch, show (1.) That the sum of the bases is always found to be larger than in a

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[*] From Poggendorff's *Annalen*, Band lii.

neutral Scheelate of protoxides of iron and manganese. (2.) That more scheelic acid would be contained in the mineral that theory assumes, whence an excess of from 4·5 to 6·6 per cent. arises. (3.) That the proportion of $\dot{\mathrm{Fe}}$ to $\dot{\mathrm{Mn}}$, though different in specimens of wolfram from different localities, is always in the ratio of low multiples of their chemical equivalents.

Wolfram from Monte Video and Ehrenfriedersdorf.

$4\,\dot{\mathrm{Fe}}\,\ddot{\mathrm{W}} + \dot{\mathrm{Mn}}\,\ddot{\mathrm{W}}$. Specific gravity = 7·544.

	Monte Video analysis.	Ehrenfriedersdorf analysis.	Formula.
$\dot{\mathrm{Fe}}$	19·24	19·16	19·26
$\dot{\mathrm{Mn}}$	4·97	4·74	4·89
$\ddot{\mathrm{W}}$	75·89	76·10	75·85

Wolfram from Chanteloupe. $3\,\dot{\mathrm{Fe}}\,\ddot{\mathrm{W}} + \dot{\mathrm{Mn}}\,\ddot{\mathrm{W}}$.
Specific gravity = 7·437.

	Analysis.	Analysis.	Analysis.	Formula.
$\dot{\mathrm{Fe}}$	17·81	18·33	17·71	18·06
$\dot{\mathrm{Mn}}$	6·20	5·67	6·29	6·11
$\ddot{\mathrm{W}}$	75·99	76·00	76·00	75·83

Wolfram from Zinnwald. $3\,\dot{\mathrm{Mn}}\,\ddot{\mathrm{W}} + 2\,\dot{\mathrm{Fe}}\,\ddot{\mathrm{W}}$.
Specific gravity = 7·191.

	Analysis.	Analysis.	Analysis.	Analysis.	Analysis.	Formula.
$\dot{\mathrm{Fe}}$	9·55	9·49	9·50	...	24·13	9·62
$\dot{\mathrm{Mn}}$	15·12	14·85	...	14·57		14·64
$\ddot{\mathrm{W}}$	75·33	75·66	...	...	75·87	75·74

XLVIII. *New particulars relating to Nathaniel Torporley's attack upon Vieta, the celebrated Analyst.* *By* J. O. HALLIWELL, *Esq., F.R.S., Hon. M.R.I.A., &c.*

ANTONY WOOD tells a story of Torporley having been amanuensis to Vieta, and then quarrelled with him, writing a book against Vieta under the assumed name of *Poulterey*, which he says is "Torporley transposed." The late Professor Rigaud attempted unsuccessfully to discover the origin of this relation, and to examine its correctness. In a letter to me dated March 14th, 1838, he thus writes:—"I have never been able to meet with this book [viz. the one which Torporley wrote against Vieta], and I remember having been puzzled about the anagram. The name is often spelled *Torporley*, but still there is no *u*. It however occurred to me that this was probably a fault of the printer; that the word manufactured in this way was *Porlterey*, which the man, thinking of his cocks and hens, had altered to the form in which it now stands." I have recently met with a passage in Aubrey's MS. collections in the Ashmolean Museum at Oxford which confirms one part of Professor Rigaud's conjecture, and makes known the source whence Wood derived his information. It is as follows:—

"Mr. Hooke affirms to me that Mr. J. Torporley was amanuensis to Vieta, but from whence he had that information he has now forgot; but he had good and credible authority for it; and bids me tell you that he was certainly so. He printed something against Vieta, by the name of John Porlterey, a digested name, the same letters a little transposed."

This is quite satisfactory as far as it goes, and shows how little credit is frequently to be placed on unauthenticated relations of this kind.

XLIX. *Abstract of Chemico-Physiological Researches.* By JOSEPH SCHERER, *M.D.*[*]

AFTER some general observations upon the extent to which the aids derivable from chemistry have been neglected by physiologists, and upon the real limits of the application of chemical results to the explanation of vital phænomena, Dr. Scherer proceeds to his experimental results on the composition of the animal substances.

The analyses were executed in the laboratory at Giessen, under the guidance of Professor Liebig. The first object was to demonstrate the identity of composition of the vegetable alimentary principles, gluten, albumen and casein (legumine).

Gluten from wheaten meal, prepared as pure as possible, was dissolved in dilute caustic potash liquor, and the solution neutralized by acetic acid. The flocculent very glutinous precipitate was treated first with boiling alcohol and then with boiling æther, as long as either took up anything; then dried at 212°, and analysed by combustion.

0·604 gramme gave 0·0072 of ashes.

I. 0·251 (or ashes abstracted 0·248) gave, burned with oxide of copper, 0·476 carbonic acid, and 0·159 water.

II. 0·202 (or without ashes 0·200) gave, by combustion with chromate of lead, 0·395 carbonic acid, and 0·132 water.

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[*] From the *Annalen der Chemie und Pharmacie*, Band xl. Heft 1. October 1841.

Another portion of this vegetable fibrine was prepared from rye meal by boiling with dilute sulphuric acid, by which the starch was removed as dextrine. The boiled mass was filtered, and the residual gluten dissolved in dilute caustic potash liquor and precipitated by acetic acid. Having been treated with boiling alcohol and æther, it was analysed by combustion with chromate of lead.

III. 0·245 gave 0·480 of carbonic acid, and 0·164 ofw ater, after allowing for ashes.

These three analyses gave,—

	I.	II.	III.
Carbon	53·064	54·603	54·617
Hydrogen...	7·132	7·302	7·491
Nitrogen......	15·359	15·810	15·809
Oxygen ...... Sulphur ...... Phosphorus	24·445	22·285	22·083
	100·000	100·000	100·000

The nitrogen was determined by comparing the relative volumes of the nitrogen and carbonic acid obtained by the combustion.

Of ten tubes, three gave the proportion of 1 to 7, three gave 1 to 7·1, and four gave 1 to 7·2. But Dr. Scherer concludes the true relation to be 1 to 8, and points out the grounds of that opinion in detail.

A specimen of vegetable casein prepared, by the method described by Liebig, from beans (legumine), gave from 0·357 of substance, 0·699 carbonic acid, and 0·230 of water. The relation of nitrogen to carbon was found to be 1 to 8. Vegetable casein contains, therefore,

Carbon	54·138	100·000.
Hydrogen...............	7·156	
Nitrogen	15·672	
Oxygen.................. Sulphur	23·034	

To ascertain the action to which the azotized nutriment is subjected during digestion, Dr. Scherer digested in one portion of artificial gastric juice, prepared by Eberle's method, some boiled beef, and in another some boiled gluten, at a temperature of 100° Fahrenheit; in fourteen hours they were for the most part dissolved. The filtered liquor did not coagulate by heat, but was slightly troubled by alcohol. Carbonate of potash threw down a few flocks redissolved by an excess. The liquors from the beef and gluten reacted alike, as did also the liquid filtered from the acid half-digested natural food contained in a calf's stomach.

What remained after the above reactions of the artificially digested beef and gluten, were mixed with fresh calf's gall, and placed, each by itself, in a piece of clean duodenum of the same animal, closed accurately above and below with a ligature, and immersed in distilled water. After ten hours the water was examined; it coagulated by heat, strongly, and was precipitated by alcohol and corrosive sublimate.

It hence follows, that the alimentary matters, as well animal and vegetable, when dissolved by the acid gastric juice, are converted by the alkaline bile into albumen, which is then absorbed by the intestinal vessels.

Composition of Albumen, Fibrine, and Casein.—Dr. Scherer first undertook the examination of the circumstances of the remarkable conversion of fibrine into albumen, by solution with saltpetre, and was enabled to explain the contradictory results obtained by many chemists, as follows:—

The fibrine of arterial blood does not dissolve in solution of nitre.

The fibrine of healthy venous blood dissolves readily, but the solution is precipitated by dilution with much water.

This precipitation is prevented by the addition of a small quantity of alkali. If the free alkali of the serum of the blood be neutralized by acetic acid, it becomes turbid, and deposits flocks of albumen when diluted.

The proportions given by Denis for dissolving fibrine, are 150 parts of moist fibrine, 50 of saltpetre, 3 of alkali, and 270 to 300 of water. The solution so obtained possesses all the characters of a strong solution of albumen.

The fibrine of the buffy coat (*crusta inflammatoria*), that obtained by whipping venous blood with a twig, or fibrine which has been long exposed moist to the air, or boiled or digested in alcohol for a few minutes, do not possess this property of dissolving; it belongs completely only to venous fibrine, obtained by tying the clot up in a cloth, and washing out the serum and colouring matter with water. These circumstances made Dr. Scherer suspect the differences to be due to the action of oxygen, which was confirmed by experiment. He found that fresh venous fibrine rapidly absorbs oxygen from the air and gives out carbonic acid, but when boiled this property is lost. The boiled fibrine is also destitute of action on deutoxide of hydrogen. Hence he remarks, that ordinary fibrine is not in a coagulated state; it separates in a solid form, but its *coagulation* is effected only by boiling or treatment with alcohol.

The following observations indicate that albumen may change back again into fibrine by a continuation of the metamorphosis by which fibrine is changed into albumen. A solution of venous fibrine in saltpetre liquor was left in a tall glass for fourteen days; it became turbid at the top, and this extended downwards and increased in quantity: the white matter which thus separated was fibrine in its arterial state, insoluble in the saltpetre liquor.

This change appears to take place also in blood: venous blood being received in a solution of Glauber's salt, it remained without coagulation for six or eight hours; but after that, a colourless, nearly transparent coagulum, two inches thick, formed on the surface. When this was removed, another gradually formed, and this formation of fibrine on the surface of the liquid blood could be very often repeated.

Oxygen does not exert the same action on albumen of the blood or of eggs as upon fibrine; but if uncoagulated albumen, prepared by the spontaneous drying of serum, be washed with water, to remove its saline constituents, the residual portion, which becomes then insoluble in water, acts on oxygen as fibrine does, absorbing it and evolving carbonic acid.

The water with which the albumen had been washed, contains all the salts and free alkali, besides a quantity of organic matter which has the properties of casein. During its evaporation a skin forms on its surface as upon milk, and it is not at all coagulated by boiling. On incineration it leaves a strongly alkaline ash with much common salt.

The albumen of serum may be totally changed into this caseous form by the addition of free alkali, in such proportion that the liquor slightly browns turmeric paper; it then does not coagulate by heat, but forms a pellicle, which is renewed as often as removed, just as with milk. That the composition of this pellicle is identical with that of casein, results from the following analyses:—

I. 0·259 of milk pellicle gave, by combustion with chromate of lead, 0·524 carbonic acid, and 0·179 water.

II. 0·289 of the same gave 0·593 of carbonic acid, and 0·200 of water.

III. 0·177 gave 0·360 carbonic acid, and 0·120 of water.

0·233 of the pellicle of blood serum warmed with free alkali, gave 0·470 carbonic acid, and 0·162 of water.

0·178 of milk pellicle gave by the method of Will and Varrentrapp, 0·445 of chloride of platina and ammonium.

0·212 of the serum pellicle gave by the same method 0·522 of the chloride of platina and ammonium.

Hence the milk pellicle contains in 100 parts,

	I.	II.	III.
Carbon.........	55·940	56·735	56·237
Hydrogen ...	7·679	7·689	7·532
Nitrogen	15·871	15·871	15·871
Oxygen	20·510	19·705	20·360

and the serum pellicle,

Carbon	55·774	100·000
Hydrogen...........	7·725	
Nitrogen	15·627	
Oxygen	20·874	

The formation of the pellicle is attended with absorption of oxygen.

The casein thus formed artificially is found to possess, like natural uncoagulated casein, the property of dissolving in boiling alcohol and separating in flocks on cooling.

Dr. Scherer finds that the solubility of casein is due to the existence of a large quantity of alkali, and that if this be neutralized by an acid the casein separates, coagulated, and then gives but 2 or 3 per cent. of an ash which consists of phosphate of lime, and is not at all alkaline: whilst soluble casein, and also the casein precipitated by alcohol, gives 7 or 8 per cent. of the strongly alkaline ash.

Dr. Scherer has, in repeating Sanson's experiments, fully proved that the iron in the blood particles is independent of the colouring substance, and has obtained the latter intense red, and yet giving a white ash perfectly free from iron.

Composition of Fibrine.—In this and the subsequent series of analyses, Dr. Scherer generally used for determining the carbon and hydrogen, combustion with chromate of lead, and a stream of oxygen gas at the conclusion. For the nitrogen the new method of Will and Plantamour was employed.

Fibrine from venous human blood, fully freed from colouring matter and fat, gave as follows:—

0·518 gramme gave 0·007 ash = 1·3 per cent.

I. 0·291 (without ashes 0·289) gave 0·561 carbonic acid, and 0·179 water.

	Carbonic acid.	Water.
II. 0·334 gramme gave	0·650	0·210
III. 0·280	0·550	0·180
IV. 0·352	0·688	0·214

V. 0·288 (or without ashes 0·225) gave 0·559 chloride of platina and ammonium, equivalent to 15·817 per cent. of nitrogen.

VI. 0·324 of substance gave 0·807 of the chloride of platina and ammonium, or 15·763 per cent. of nitrogen.

Hence the composition, the carbon being to the nitrogen in the proportion of 8 to 1.

	I.	II.	III.	IV.
Carbon	53·671	54·454	55·002	54·976
Hydrogen	6·878	7·069	7·216	6·867
Nitrogen	15·763	15·762	15·817	15·913
Oxygen, sulphur, and phosphorus	23·688	22·715	21·965	22·244
	100·000	100·000	100·000	100·000

Venous fibrine, dissolved by Denis's method, was precipitated by boiling alcohol, and analysed.

0·473 gave 0·0065 ash, or 1·37 per cent.

0·243 gave 0·465 carbonic acid, and 0·149 water.

0·365 gave 0·714 carbonic acid, and 0·223 water.

0·407 gave by a direct (Dumas's) determination of the nitrogen, 15·720 per cent.

Hence results the composition per cent.

	I.	II.	
Carbon	53·571	54·686	100·000
Hydrogen	6·895	6·835	
Nitrogen	15·720	15·720	
Oxygen, sulphur, and phosphorus	23·814	22·759	

Fibrine obtained by whipping blood was dissolved by means of acetic acid, and precipitated by carbonate of potash. It gave, after extraction of all fats by alcohol and æther, 2·3 per cent. ashes.

0·250 gave 0·486 carbonic acid, and 0·161 of water.

0·298 gave 0·739 of chloride of platina and ammonium, hence = 16·065 nitrogen per cent.

Its composition was, therefore,

Carbon	54·844	100·00.
Hydrogen	7·219	
Nitrogen	16·065	
Oxygen, sulphur, and phosphorus	21·872	

[To be continued.]

[We find, from the last Number of the *Annales de Chimie et de Physique* which has reached us, that M. Dumas is engaged in a discussion with Prof. Liebig, as to the priority and originality of the views contained in his Lecture given in last volume (pp. 340, 456). Which of these eminent men has best discharged the honourable duty of fully and freely giving credit to their cotemporaries and to all who have preceded them in chemico-physiological researches, cannot be uninteresting as a matter of historical inquiry.—Edit.]

L. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 262.]

Dec. 16, 1841. THE following papers were read, viz. :—

"Papers from the several Magnetic Observatories established in India, addressed to the Secretary of the Royal Society, by direction of the Honourable East India Company." Communicated by P. M. Roget, M.D., Sec. R.S.

1. The Magnetic Observatory at Singapore.

Twenty-seven anemometer curves. Eight magnetic observations for February 1841. Anemometer curves for March, April and May 1841. Magnetic observations made on the term-days in November and December 1840, and January 1841, with an abstract of the magnetic and meteorological instruments, from the commencement of December 1840 to the end of January 1841.

Daily curves of certain magnetic instruments during the months of December 1840 and January 1841. Monthly curves for December 1840 and January 1841.

2. The Magnetic Observatory at Madras.

Term-day curves for the months of January, February, March, April, May and June 1841.

Monthly observations of the magnetical and meteorological instruments for August; also two absolute determinations of the horizontal intensity, taken in January and April 1841.

Monthly and term-day observations for May, June and July 1841.

Magnetic dip and intensity taken at Johanna, Madras and Singapore.

3. The Magnetic Observatory at Simla.

Magnetic and meteorological observations for January, February, March and April 1841; also transcripts of sheets D. for January, February and March 1841, to be substituted for similar sheets for those months.

Preliminary observations regarding the Magnetic Observatory at Simla for May 1841.

Observations for June 1841, including term-day curves; also a general abstract of the mean readings of the instruments.

Magnetic term observations for July and August 1841, made at the Magnetic Observatory, Simla.

4. "Variations de la déclinaison et intensité horizontale magnétique observées à Milan, pendant 24 heures de suite, le 28 et 27 Novembre 1841." Par Signor Carlini, For. Memb. R.S.

5. "On a Calculating Machine." By the Rev. Henry Moseley, M.A., F.R.S., Professor of Natural Philosophy and Astronomy in King's College.

The object which the author proposes to accomplish in the construction of this machine, is to determine mechanically the products, quotients, logarithms, squares, and other powers of the natural numbers, by means of combinations of greater simplicity than have hitherto been applied to the purposes of mechanical calculation. The paper is accompanied by a figure illustrating the principle of the machine, but not representing the mechanical details of its construction. An outline is then given of the essential parts of the instrument, and of the theory of their operation.

6. A paper was also in part read entitled, "On Fibre." By Martin Barry, M.D., F.R.SS. Lond. and Edin.

Jan. 6, 1842.—The reading of a paper entitled, "On Fibre," by Martin Barry, M.D., F.R.SS. Lond. and Edin., was resumed and concluded.

The author observes, that, in the mature blood-corpuscle, there is often seen a flat filament, already formed within the corpuscle. In Mammalia, including Man, this filament is frequently annular; sometimes the ring is divided at a certain part, and sometimes one extremity overlaps the other. This is still more the case in Birds, Amphibia, and Fishes, in which the filament is of such length as to constitute a coil. This filament is formed of the discs contained within the blood-corpuscle. In Mammals, the discs entering into its formation are so few as to form a single ring; and hence the biconcave form of the corpuscle in this class, and the frequent annular form of the filament it produces. In the other Vertebrata, the discs contained within the blood-corpuscle are too numerous for a single ring; and they consequently form a coil. At the outer part of this coil, the filament, already stated to be flat, often presents its edge; whence there arises a greater thickness of the corpuscle, and an appearance of being cut off abruptly at this part; while in the centre there is generally found the unappropriated portion of a nucleus; and hence the central eminence, surrounded by a depression, in those corpuscles which, from the above-mentioned cause, have the edge thickened. The nucleus of the blood-corpuscle in some instances resembles a ball of twine; being actually composed, at its outer part, of a coiled filament. In such of the invertebrata as the author has examined, the blood-corpuscle is likewise seen passing into a coil.

The filament, thus formed within the blood-corpuscle, has a remarkable structure; for it is not only flat, but deeply grooved on both surfaces, and consequently thinner in the middle than at the edges, which are rounded; so that the filament, when seen edgewise, appears at first sight to consist of segments. The line separating the apparent segments from one another is, however, not directly transverse, but oblique.

Portions of the clot in blood sometimes consist of filaments having a structure identical with that of the filament formed within the blood-corpuscle. The ring formed in the blood-corpuscle of Man, and the coil formed in that of Birds and Reptiles, have been seen by the author unwinding themselves into the straight and often parallel filaments of the clot; changes which may be also seen occurring in blood placed under the microscope before its coagulation; and similar coils may be perceived scattered over the field of view, the coils here also appearing to be altered blood-corpuscles, in the act of unwinding themselves; filaments, having the same structure as the foregoing, are to be met with apparently in every tissue of the body. The author enumerates a great variety of organs in which he has observed the same kind of filaments.

Among vegetable structures, he subjected to microscopic examination the root, stem, leaf-stalk, and leaf, besides the several parts of the flower: and in no instance of phanerogamous plants, where a fibrous tissue exists, did he fail to find filaments of the same kind. On subsequently examining portions indiscriminately taken from ferns, mosses, fungi, lichens, and several of the marine algæ, he met with an equally general distribution of the same kind of filaments. The flat filament seen by the author in all these structures, of both animals and plants, he states to be that usually denominated a *fibre*. Its appearance is precisely such as that of the filament formed within the corpuscle of the blood. It is known, he remarks, that discoid corpuscles circulate in plants; and it remains to be seen whether or not filaments are formed also in these.

By gradually tracing the fibre or filament above-mentioned into similar objects of larger size, the author endeavours to show that it is not possible to draw a line of separation between the minutest filament, and an object being to all appearance composed of two spirals running in opposite directions, and interlacing at certain regular intervals; an arrangement which produces in the entire object a flattened form, and gives it a grooved appearance. It is, in fact, the structure which, for want of a better term, he has called a *flat filament*. The edge of this filament presents what, at first sight, seem like segments, but which, in reality, are the consecutive curves of a spiral thread. A transverse section of such an object is rudely represented by the figure 8. This is also precisely the appearance presented by the minutest filament, generally termed *Fibre*: and the author particularly refers to the oblique direction of the line separating the apparent segments in the smaller filament, in connexion with the oblique direction of the spaces between the curves of the spiral threads in the larger one.

The spiral form, which has heretofore seemed wanting, or nearly so, in animal tissues, is then shown to be as general in animals as in plants. Nervous tissue, muscle, minute blood-vessels, and the crystalline lens, afford instances in proof of this. And if the author's view of identity in structure between the larger and the smaller filaments be correct, it follows that spirals are much more general in plants themselves than has been hitherto supposed; spirals would thus appear, in fact, to be as universal as a fibrous structure.

The tendency to the spiral form manifests itself very early. Of this the most important instance is afforded by the corpuscle of the blood, as above described. The author has also obtained an interesting proof of it in cartilage from the ear of a rabbit; where the nucleus, lying loose in its cell, resembled a ball of twine, being composed at its outer part of a coiled filament, which it was giving off to weave the cell-wall;—this cell-wall being no other than the last-formed portion of what is termed the intercellular substance—the essential part of cartilage. These nuclei in cartilage, as well as those in other tissues, there is ground for believing to be descended, by fissiparous generation, from the nuclei of blood-corpuscles.

The author then describes the mode of origin of the flat filament or fibre, and its reproduction in various animal and vegetable tissues, which he enumerates. He conceives that each filament is a compound body which enlarges, and, from analogy, may contain the elements of future structures, formed by division and subdivision, to which no limits can be assigned.

He then traces the formation of muscle out of cells, which, according to his observations, are derived from corpuscles of the blood, to the state where there exists what is denominated the *fibril*. In this process, there are to be observed the formation of a second order of tubes within the original tube; a peculiarly regular arrangement of discs within these second tubes; the formation, first of rings and then of spirals, out of discs so arranged; the interlacing of the spirals; and the origin, in the space circumscribed by these, of spirals having a minuter size; which in their turn surround others still more minute; and so on. The outer spirals enter for the most part into the formation of the investing membrane discovered by Schwann, but for the only complete description of which, in a formed state, we are indebted to Mr. Bowman. The inner spirals constitute what are denominated the *fibrillæ*. The fibril appears to the author to be no other than a state of the object which he designates a *flat filament*; and which, as he shows, is a compound structure. The fibril he finds to be, not round and beaded, as it has been supposed, but a flat and grooved filament; the description above given of the structure of the filament being especially applicable here. This flat filament is so situated in the fasciculus of voluntary muscle, as to present its edge to the observer. It seems to have been the appearance presented by the edge of this filament, that is to say, by the curves of a spiral thread, that suggested the idea of longitudinal bead-like enlargements of the fibril, as producing striæ in the fasciculus of voluntary muscle. In the author's opinion, the dark longitudinal striæ are spaces (probably occupied by a lubricating fluid) between the edges of flat filaments, each filament being composed of two spiral threads, and the dark transverse striæ, rows of spaces between the curves of these spiral threads. The filament now mentioned, or its edge, seems to correspond to the *primitive marked thread* or *cylinder* of Fontana—to the *primitive fibre* of Valentin and Schwann—to the *marked filament* of Skey—to the *elementary fibre* of Mandl—to the *beaded fibril* of Schwann, Müller, Lauth, and Bowman—and to the *granular fibre* of Gerber. The changes known to be produced by the alternate shortening and lengthening of a single spiral are exhibited in the microscope by a fasciculus of spirals, not only in its length and thickness, but in the width of the spaces (*striæ*) between the curves of the spirals. And a muscle being no other than a vast bundle of spirals, it is in contraction short and thick; while in relaxation it is long and thin; and thus there occurs no flattening of bead-like segments in contraction. The author has found no segments that could undergo this change. These observations on the form of the ultimate threads in voluntary muscle, were first made on the larva of a Batrachian reptile; and have been confirmed by an examination of this structure in each class of vertebrated animals, as well as in the Crustacea, Mollusca, Annelida, and Insects.

He finds that the toothed fibre, discovered by Sir David Brewster in the crystalline lens, is formed out of an enlarged filament; the projecting portions of the spiral threads in the filament, that is, the apparent segments, becoming the teeth of that fibre.

The compound filaments are seen with peculiar distinctness in the blood-vessels of the arachnoid membrane. In connexion with the spiral direction of the outer filament in these vessels, the author refers to the rouleaux in which the red blood-discs are seen to arrange themselves, in the microscope, as probably indicating a tendency to produce spiral filaments. To form rouleaux, corpuscle joins itself to corpuscle, that is to say, ring to ring; and rings pass into coils. The union of such coils, end to end, would form a spiral. But the formation by the blood-corpuscles of these rouleaux is interesting in connexion with some facts recorded by the author in a former memoir; namely, that many structures, including blood-vessels, have their origin in rows of cells derived from corpuscles of the blood. The human spermatozoon presented a disc with a pellucid depression, each of the two sides of the peripheral portion of which was extended into a thread; these two threads forming by being twisted the part usually designated as the tail. The occurrence of two tails, observed by Wagner, is accounted for by the author by the untwisting of these threads.

The author has noticed very curious resemblances in mould, arising from the decay of organic matter, to early stages in the formation of the most elaborate animal tissues, more particularly nerve and muscle. Flax has afforded satisfactory evidence of identity, not only in structure, but in the mode of reproduction, between animal and vegetable fibre.

Valentin had previously stated that in plants all secondary deposits take place in spiral lines. In the internal structure of animals, spirals have heretofore seemed to be wanting, or very nearly so. Should the facts recorded in this memoir, however, be established by the researches of other investigators, the author thinks the question in future may perhaps be, where is the "secondary deposit" in animal structure, which is not connected with the spiral form? The spiral in animals, as he conceives he has shown, is in strictness not a secondary formation, but the most primary of all; and the question now is, whether it is not precisely so in plants.

In a postscript the author observes, that there are states of voluntary muscle in which the longitudinal filaments ("fibrillæ") have no concern in the production of the transverse striæ; these striæ being occasioned by the windings of spirals, within which very minute bundles of longitudinal filaments are contained and have their origin. The spirals are interlaced. When mature, they are flat and grooved filaments, having the compound structure above described. With the shortening of the longitudinal filaments ("fibrillæ") in muscular contraction, the surrounding spirals, and of course the striæ, become elongated and narrow; while in relaxation these changes are reversed*.

[* We learn that the author has satisfactorily demonstrated to Professor Owen and others, since the reading of the above paper, the leading facts described in it.—Edit.]

Jan. 13.—A paper was in part read, entitled, "Researches in Physical Geology:" Third Series. By William Hopkins, Esq., M.A., F.R.S.

GEOLOGICAL SOCIETY.

(Continued from p. 64.)

June 30, 1841.—The following papers were read:—

1. "A brief note to accompany a series of specimens from Lockport, near Niagara, in the State of New York," by William Jory Henwood, Esq., F.R.S., F.G.S.

Mr. Henwood commences by calling attention to Mr. J. Hall's Geological Reports of the fourth division of the State of New York, particularly to that for 1840, which contains an important stratigraphical account, with lists of organic remains, of the beds near Lockport (p. 452), and in which the deposit called the Lockport limestone is placed in the lowest portion of a series of beds considered by Mr. Hall to be the equivalents of the Wenlock limestone. Mr. Henwood likewise especially alludes to that geologist's description of several beds classed with the Wenlock limestone of the State of New York, but which have no representative in Shropshire and the adjacent counties.

At Lockport the strata are nearly horizontal, and are well exposed in a section of great length, and about 100 feet in altitude, along the banks of the Erie Canal. The uppermost bed Mr. Henwood believes to be the Lockport or Wenlock limestone; and it is succeeded in descending order by several other thin beds of similar or slightly varying characters, and they are all traversed by joints, the principal bearing about magnetic N. and S. Organic remains are stated to be abundant in these limestones.

The next subjacent formation is the Rochester shale, considered by Mr. Hall to be the equivalent of the Wenlock shale. It extends to the bottom of the section, except at a few points, and consists of beds of green shale abounding in organic remains, including *Asaphus longicaudatus ?, Homalonotus delphinocephalus, Platynotus Boltoni, Orthoceras annulatum ?, Conularia ————?, Leptæna transversalis, L. depressa, Terebratula aspera, Avicula striata ?, Caryocrinites ornata**. Beds of limestone occur in the shale at irregular intervals, but they contain no organic remains.

At the few points where the cutting has penetrated the shale, a bed of limestone is exposed, and is stated to contain, on the authority of Mr. Forman, who has made the fossils of the district his particular study, Orbiculæ and other organic remains; and the same gentleman informed Mr. Henwood, that beneath this limestone there are strata of sandstone enclosing fucoides and leaves of plants.

At Rochester the Genessee traverses a channel 500 feet wide and

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* The above species have been determined by specimens which Mr. Henwood presented to the Society. The *Asaphus longicaudatus* is distinguished from the fossil described by Mr. Murchison in the absence of the large protuberance "on the anterior edge of the buckler," Silur. Syst., p. 656.

80 feet deep, and falls in the middle of the town over a ledge of that depth, composed of beds of quartzose limestone and calcareous sandstone containing shells, and abounding in hollows lined with stalagmitic incrustations.

2. "Notes to accompany a series of specimens from Chaleur Bay and the river Ristigouche in New Brunswick," by Mr. Henwood.

Granitė constitutes the lowest rock in the neighbourhood of Bathurst (47°40′ N. lat., 65° 42′ W. long.), appearing about a mile from the town on the banks of the Nepisiguit, and extending up its course for three miles; it is often traversed by granite veins of a finer and more quartzose nature, particularly at the Pabineau falls. For the whole of the above distance it is surmounted by the sandstones and conglomerates of the coal-measures, the bedding of which conforms almost perfectly to the surface of the granite. Near Long Meadow, a greenish slate-rock, very much contorted in the cleavage planes, is in contact with the granite and overlaid by a coarse quartzose conglomerate with apparently a ferruginous basis, and belonging to the coal-measures. The greenish slate extends to the grand falls, containing numerous quartz veins, and occasionally, as at the chain of rocks, irregular masses of greenstone.

Granite also runs for some miles up the courses of the Little and Middle rivers, and near Molloys, on the latter, it is overlaid by a thick-bedded greenish slate, which is traversed near the junction of the two rocks by numerous granite veins. In the bed of the Little river, about eight miles from Bathurst, a fine glossy clay-slate appears. A fine deep blue clay-slate forms both banks of the Tattigouche from the sea to Clarke's Camp, a distance of twenty-two miles, and is overlaid near Blackstock's Mills by the quartzose conglomerate of the coal-measures; while at the Tattigouche falls a reddish brown rock contains numerous small vermicular and nodular masses of oxide of manganese. At this spot Mr. Henwood found a portion of an encrinite, the only organic body seen by him.

The coarse sandstones and quartzose conglomerates of the coal-measures extend, Mr. Henwood believes, over the greater portion of New Brunswick, being continuous, so far as he could discover, from Fredericton (45° 55′ N. lat., 66° 45′ W. long.) on the St. John's river, to Boice's town, Newcastle and Chatham (about 47° N. lat., 65° 30′ W. long.), on the Miramichi river, and thence to Bathurst, to the northward of which they apparently terminate. In the last-mentioned locality so great abundance of vegetable remains have been found, charged with vitreous and the blue and green carbonates of copper, that mining operations have been conducted for the purpose of procuring the metallic minerals; but the quantities obtained have not repaid the expense, though the ores have been found over a considerable tract. The bed containing the copper lies between two strata of a coarse quartzose conglomerate, and appears to be a soft bluish shale, enclosing in some places abundance of ferns and other plants. Wherever there are any traces of woody fibre, the copper appears to have been attracted to them; but the largest quantity of ore has been obtained in small nodular concretions, the centre of which is sometimes composed of vitreous copper and the exterior of copper pyrites, or the reverse; whilst a few nodules have been found to consist wholly of vitreous copper, and a still fewer wholly of pyrites. Some portions of the shale give out, on being broken, a most powerful garlic or arsenical odour.

At the Capes, about ten miles east of Bathurst, the sandstones contain thin beds of coal, and the greater part of the plants which accompanied the paper was obtained from them. The coal is bituminous and stated to burn well; but the quantity is too small to be worked profitably. On the beach Mr. Henwood noticed enormous quantities of nodular iron-stone, though he was able to discover *in situ* only a few in one of the beds of sandstone.

Towards Belle Dune, on the coast of Chaleur Bay, hummocks of serpentine, traversed by small veins of steatite and calcareous spar, rise through the sand; and in a few places beds of sandstone and conglomerate, assuming a flinty character, are in contact with the serpentine. Some of the strata of conglomerate are displaced or heaved by certain joints; but other joints traverse the strata without displacing them.

At Chambers's, near Belle Dune Point, is a quartzose ferruginous limestone, which contains remains of *Favosites polymorpha* and other corals. The beds strike about N.E. and S.W., and dip 20° S.E.

At Dumerisque's on the Ristigouche, near Dalhousie, the western extremity of Chaleur Bay, Mr. Henwood observed a series of strata, extending less than a quarter of a mile in length, and bounded by two trap dykes, beyond which he was unable to detect any traces of the beds. The strike of the strata was nearly N.E. and S.W., and dip from 40° to 50° towards the S.E.

The following ascending sectional list is given by the author:—

1. Lowest bed, impure limestone, containing species of Cyathophyllum, Favosites, and Syringopora, also crinoidal remains.
2. An impure limestone not very well exposed, but it contains *Favosites Gothlandica, Producta depressa, Atrypa aspera?*
3. Calcareous shale abounding with *Producta depressa,* and yielding also a species of Cyathophyllum and of Favosites, likewise *Atrypa aspera.*
4. Calcareous shale, distinguished by the author as the Trilobite bed, on account of the remains found in it. In addition to the undeterminable portions of Trilobites, Mr. Henwood procured specimens of *Producta depressa* with fragments of Orthocera and other Testacea.
5. Calcareous lower earthy shale, in which *Producta depressa* and other shells and Crinoidea are stated to be abundant, but corals few in number.
6. Earthy shale, containing numerous specimens of Favosites, crinoidal stems, *Producta depressa, Leptæna euglypha, Atrypa aspera,* and several of apparently unnamed species.

A few other beds much decomposed rest upon No. 6, and lime is stated to be apparently more plentiful in them.

7. Above these the strata are more gritty, and fossils are rare, but the specimens obtained by the author include *Atrypa aspera,* and obscure traces of vegetable remains.

8. Resting upon No. 7. is a shelly limestone, agreeing apparently in dip and strike with the subjacent strata. The fossils are for the greater part the same as in the other beds, consisting of *Producta depressa, Atrypa aspera,* and a Spirifera which occurs also in No. 6.

The rock near Dalhousie is a reddish slate, in which Mr. Henwood sought in vain for organic remains. The dip of the laminæ is 70° to the S.E.

On the opposite side of the river in Lower Canada, the strata consist wholly of a brick-red sandstone.

Near Cambellton, both the Canadian and the New Brunswick banks of the Ristigouche are composed of sandstone and conglomerate with imperfect fragments of vegetable remains. The strata are cut through by numerous trap dykes, many of which have produced faults, but in an equal number of instances no similar effects are visible.

At John Pratts, seven miles above Cambellton on the New Brunswick side of the Ristigouche, is a slaty limestone enclosing Crinoidea and a few other obscure organic bodies. The beds dip W., and strike 20° E. of N. A thin-bedded limestone, in which the author could discover no fossils, extends to the Maramajaw, and thence to the Upsalguilch river. In some places it is much traversed by trap dykes, and at the Little Falls of the Upsalguilch by dykes of felspar porphyry resembling the Cornish elvans. The strike of the strata is N. and S., and the dip W.

3. "On the locality and geological position of *Cucullæa decussata,*" by Joshua Trimmer, Esq., F.G.S.

The object of this communication is to determine the geological formation to which the siliceous casts of the *Cucullæa decussata* really belong, it having been stated that they occur at Faversham in Kent*, in a bed of greenish siliceous sand, placed by Mr. Webster above the chalk†; in the upper greensand of Kent, but with a doubt‡; in the lower greensand of Kent and Sussex, and in the greensand of Blackdown§.

The fossil was first described by Mr. Parkinson, who states, on the authority of the late Mr. Francis Crow, that it was found at Faversham associated with a silicified shell exactly agreeing with the *Strombus pes Pelicani* of the Devonshire whetstone pits; but he adds, that it is specifically different from the Cucullæa of Devonshire, and he proposes to designate it by the name of *C. decussata.* The collection of the late Mr. Crow, now in the Canterbury Museum, contains three specimens of this fossil, which, with two others presented by Mr. E. Crow to the Geological Society, are said by the author on the authority of that gentleman, to have been found in digging a ha-ha at Nash Court, about two miles from Faversham. Mr. Trimmer like-

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* Parkinson, Org. Rem., vol. iii. p. 171. Min. Con., vol. iii. p. 8. Geol. Trans., Second Series, vol. iii. pl. 1. p. 212.

† Geol. Trans., First Series, vol. ii. p. 195.

‡ Geol. Trans., Second Series, vol. iv. pl. 2. pp. 203, 356. Ibid., vol. iii. pl. 1. p. 213; vol. iv. pl. 2. pp. 128, 157, 356.

§ Ibid., vol. iv. pl. 2. pp. 240, 356.

wise mentions as independent evidence of these fossils having been found at Nash Court, that it is stated on both editions of Mr. Greenough's Map, that siliceous fossils occur there.

A careful examination of the specimens presented to the Geological Society by Mr. E. Crow, has verified the correctness of Mr. Parkinson's opinion, that the Faversham shell is specifically distinct from the Cucullææ of the greensands.

The strata of Nash Court, Mr. Trimmer says, undoubtedly belong to the lowest sands of the London clay, and to that portion which is very near the junction with the chalk.

In the village of Boughton, not far from Nash Court, Mr. Trimmer has examined two sections situated to the east and west of the 50th mile-stone, and nearly on a level with the ha-ha, in which the *Cucullæa decussata* was found. The strata consisted of white and ferruginous sand with layers of ferruginous clay, in some parts considerably indurated. He did not observe any organic remains, but shells are reported to have been found in the eastern section. At a greater elevation on the side of Boughton Hill, and a little below the junction of the sands with the brown clay, which forms the summit of the hill, are courses of geodes of a ferruginous sandstone very like some of the Wealden sandstone, and lined with mammillary siliceous deposits and quartz crystals. Casts of plastic clay shells are occasionally found in the sandstone, but more abundantly in the alternate layers of indurated ferruginous clay. From a bed four feet thick of this sandstone worked in a quarry in the wood on the side of Boughton Hill, Mr. Trimmer obtained casts of *Calyptræa trochiformis, Rostellaria Sowerbii* (*Strombus pes Pelicani* of Mr. Parkinson), *Potamides intermedium,* and a Venus which has been considered a variety of *V. ovalis,* but is clearly distinct. Similar remains are stated by the author to occur in the upper part of the cliffs at Reculver, either in loose masses, or sand slightly indurated. In conclusion, Mr. Trimmer acknowledges the assistance which he received in preparing the communication.

4. "A description of a portion of the skeleton of the Cetiosaurus, a gigantic extinct Saurian Reptile occurring in the Oolitic formations of different portions of England," by Professor Owen, F.R.S., F.G.S.

The remains described in this memoir consist of vertebræ and bones of the extremities obtained by Mr. Kingdon from the oolite quarries of Chipping Norton, in Oxfordshire; of vertebræ and other bones from the oolite of Blisworth, near Northampton, transmitted to the author by Miss Baker; and of other remains from the oolite of Staple Hill, Wotton, three miles north-west of Woodstock; from the oolite near Buckingham; the Portland stone at Garsington and Thame, in the collection of Dr. Buckland: Mr. Owen has likewise examined a vertebra and some bones of the extremities of the same saurian from the Yorkshire oolite, and preserved in the Scarborough Museum.

Caudal Vertebræ.—A caudal vertebra from near Buckingham, which presented the anchylosed neural arch entire, but with the transverse, oblique and spinous processes broken off, equalled in dimensions a middle caudal vertebra of a full-sized whale, the antero-posterior diameter being five inches, the transverse eight inches six lines, and the vertical seven inches. The sides and under part of the centrum are described as very concave; and the shape of the articular extremities as nearly circular, with a greater concavity in the anterior one than in the posterior. The posterior hæmapophysial articular surfaces slope downwards and forwards in the form of semicircular facets for nearly two inches upon the under surfaces. The neurapophyses commence close to the anterior surface of the centrum, their antero-posterior extent being three and a half inches, and they meet at a rather acute angle above the spinal canal. The vertical diameter of the spinal canal was one inch nine lines, the transverse two inches, and the breadth of the base of the neural arch, from the outside of the neurapophyses, five inches three lines. The transverse process is developed from the centrum just below the neurapophysial suture. In all the caudal vertebræ of the Cetiosaurus the posterior half of the centrum is left uncovered by the neural arch.

The substance of another fractured vertebra, showing the upper third of the centrum, presented an uniform coarse spongy texture; whilst in a third specimen, which exhibited also a similar texture, the layers affected a direction parallel with the articular extremities for about half an inch from their surfaces, and inclined to the longitudinal course in the intermediate space. This structure, Mr. Owen states, proves that the vertebra cannot belong to the *Poikilopleuron Bucklandi*.

A caudal vertebra also from Buckingham, and assigned by Professor Owen to the middle part of the tail, on account of the development of short, narrow transverse processes just below the neurapophysial sutures, exhibited a centrum of a subtrihedral form, with one angle inferiorly and the other two at the origin of the transverse processes, but all three largely rounded off. The marginal circumference of the centrum was convex, and separated from the lateral or free surface by a rough, irregular, elevated ridge, the inferior part of which encroached upon the under surface of the vertebra in the form of two semicircular facets, both anteriorly and posteriorly. The free surface of the vertebral centre is marked by the coarse lines of the bony fibrous structure, decussating like an irregular net-work. The spinal canal of this specimen did not sink into the body of the vertebra. The size of this vertebra, and the proportions and position of neurapophyses and hæmapophysial articulations, might suggest a relationship of the animal to which it belonged with the Cetacea; but it differs, Mr. Owen says, in the concavity of the terminal articulations, which show no sign of separation as laminar epiphyses, and more particularly in the place of the origin of the transverse process being close to the neurapophysis instead of proceeding from the middle of the side of the centrum. In these deviations from the Cetacea, the Cetiosaurus approaches, the author states, the saurian order.

Mr. Owen then describes, with his wonted minuteness and perfect acquaintance with the subject, other caudal vertebræ found at Blisworth, but it is not possible to abridge the details.

Among the remains discovered near Chipping Norton are eleven caudal vertebræ without transverse processes, and therefore assigned by the author to the terminal half of the tail. They progressively diminished in transverse diameter from five inches to two inches, but without losing in equal ratio their length, which continues the same, or five and a half inches in the vertebra which has only three inches and three lines of breadth, five inches in that which is two inches and nine lines broad, and four inches in that which has a breadth of two inches. These eleven vertebræ do not constitute, Mr. Owen shows, a regular sequence, but detached links of the termination of the spinal column. In all the existing genera of Cetacea the posterior caudal vertebræ become shorter in proportion to their thickness, and the terminal ones are depressed. The slender elongated form of the corresponding vertebræ in the Cetiosaurus, is, Mr. Owen shows, a striking crocodilian character; and he adds, it is important to observe that not any of the series of caudal vertebræ described in this paper exhibit the vertical canals or perforations of the side of the centrum or base of the transverse process which so peculiarly characterizes most of the cetacean caudal vertebræ.

In his comparison between the vertebræ of the Cetiosaurus and the Poikilopleuron, Professor Owen states that the caudal vertebræ of the former resemble those of the latter and most other reptiles from strata below the chalk in the articular surfaces being slightly concave; and the vertebræ of the Poikilopleuron, especially in the elongated and rounded form of the body; in its median compression, and in the articulation of the hæmapophyses to the inferior part of the vertebral interspaces, though they are larger; on the contrary, the Cetiosaurus vertebræ differ in their proportions, in their structure, as in the absence of the remarkable medullary cavity in the middle part of the centrum of the Poikilopleuron; in the shortness of the neurapophyses as compared with the centrum; and in other minor points, which are fully detailed by Professor Owen.

The author then proceeds to institute further comparisons between the vertebræ of the Cetiosaurus and other reptilia: thus he shows that they differ from the vertebræ of the Crocodilians in retaining the cylindrical form of the body to the end of the tail, instead of being compressed and four-sided; that there is no trace of the vertical median division which the bodies of the caudal vertebræ present in Iguanæ, Anolides and other Lacertians; that they are not only larger than in the Megalosaurus, but relatively longer; that they differ from the anterior caudal vertebræ of the Iguanodon, which are nearly as large, in the absence of the well-marked concavity below the transverse processes, in the form of the centrum not being so quadrilateral, and especially in the transverse breadth of the inferior surface being less; and from the posterior caudal vertebræ of the Iguanodon, which slightly increases in length, in being less compressed and the centrum not having a triangular form;

the slender terminal caudal vertebræ of the Iguanodon are also hexagonal, and not cylindrical as in the Cetiosaurus.

As there is no known extinct saurian which can so nearly compete in size with the Cetiosaurus as the Iguanodon, it is fortunate, Prof. Owen observes, that the distinguishing characters are so well marked and easily recognizable.

Dorsal vertebra.—The only portion of a dorsal vertebra described in the memoir is the extremity of a spinous process, the posterior surface of which is rough and flattened, 4 inches across, at about the same distance below the end of the spine; the sides are traversed to a certain extent by a longitudinal ridge, anterior to which they are concave and smooth, but their anterior margin is again flattened and rough, though it is not so broad as the posterior.

In referring all the vertebræ described in this paper to the same species of saurian, Prof. Owen admits that they present a somewhat greater variety of form and proportion in different regions of the tail than is observable in that part of the vertebral column in the smaller and recent species of Crocodile or Lizard; not only becoming larger in proportion to their thickness, but increasing slightly in length for a short distance as they recede from the sacrum. They appear likewise to exchange from a cylindrical to a subtriedral form of the body, but to resume the cylindrical shape in the terminal half of the tail. These modifications, he says, are possible, as in the *Plesiosaurus brachydeirus* still greater discrepancies in the proportions of the vertebræ prevail; and they are inferior in degree to any of the modifications which distinguish the vertebræ of known genera of saurians from those under consideration, in pointing at their distinguishing features from the hitherto known sauria; and in thus treating of them collectively, the inference that they belong to the same gigantic species is, the author observes, almost irresistible, that they belong to a new and distinct genus, which, on account of the vertebræ approximating in size and structure to the vertebræ of the whale, he has termed Cetiosaurus.

In the cuttings for the London and Birmingham Railway near Blisworth, there were found, scattered over an area of 12 feet by 8 feet, the following remains:—1. A bone resembling the episternal of an Ichthyosaurus, the length or antero-posterior extent of the preserved portion of the median plate being 1½ foot, and the breadth of the posterior fractured end 5 inches, from which it gradually expands to the root of the side branches, where its breadth is 1 foot. From its obtuse termination to the end of the longest branch is 2½ feet, and from this end to that of the opposite branch 4½ feet. 2. The remains of a coracoid and scapula apparatus of equally gigantic proportions. 3. A fragment, considered to be the shaft of a humerus, 1 foot 9 inches in length, 6 inches in diameter across the middle and 8 inches across the widest end. 4. A portion of the opposite humerus. 5. Another fragment, believed to be part of a radius or ulna, about a yard in length, 6 inches across the proximal end and 5 inches across the middle of the shaft. 6. A slightly curved portion of a rib, a yard long and from 1½ to 2 inches thick. 7. Five caudal vertebræ agreeing in dimensions with the vertebræ of Chipping Norton.

Numerous fragments of long bones without a trace of a medullary cavity have been found at Chipping Norton, and correspond in magnitude with the vertebræ. The articular surfaces which are preserved are covered with large tubercles for the attachment of thick cartilages. The best preserved fragments are considered to belong to metacarpal or metatarsal and phalangeal bones, and are therefore, Prof. Owen says, decisive evidence against the cetacean nature of the animal; but he adds, they possess characters by which they may be distinguished from the corresponding bones of known extinct gigantic saurians. One of these bones, believed to be a metacarpal or a metatarsal, is double the bulk of the largest analogous bone of a full-grown elephant, though the metacarpals or metatarsals are much smaller in proportion in Saurians than in Pachyderms. The bone is 7 inches in length, 9 in circumference in its middle, 5 in the antero-posterior diameter of its proximal end, and 4 inches 8 lines in the transverse diameter of the distal end. A proximal phalanx is shown to be remarkable for its short and broad proportions, which are more massive than those of the phalanges of existing Crocodilians or of the Poikilopleuron.

An ungueal phalanx, also found at Chipping Norton, was 6 inches in length, $2\frac{1}{2}$ in breadth, and upwards of 3 in depth. It was slightly curved, obliquely compressed, obtusely terminated with a shallow, concave, trochlear articular surface, divided by a vertical convexity; it was marked on each side by a smooth curved groove, 3 inches in length, with the concavity downwards, and the lower edge projecting beyond the upper at the posterior part of the groove; but it is shown to be by no means produced in so large and thick a ridge as that which characterizes each side of the more depressed and broader phalanx of the Iguanodon. From the ungueal phalanges of that Saurian it differs in being much less compressed from side to side and less curved downwards. It vastly surpasses in size any of the ungueal phalanges of the Poikilopleuron. A smaller ungueal phalanx, resembling in general shape the above, was found at Chipping Norton; and portions of metacarpal or metatarsal bones, agreeing in form and size with the fragments obtained at Chipping Norton, have been discovered at Buckingham: also a fragment 8 inches long, which Prof. Owen considers to have belonged to a radius, a fibula, or a long distal phalanx.

With reference to a comparison of the remains of the Cetiosaurus with those of the Polyptychodon, the bones of the extremities present in both cases the cancellous structure throughout the central part, which indicates aquatic rather than terrestrial habits. Prof. Owen states that he has not found any of the remains of the extremities of the Cetiosaurus to agree exactly in shape with those belonging to the Polyptychodon; also that no specimen of a tooth agreeing in characters with the teeth of the Polyptychodon has been detected in secondary strata inferior to the greensand. Certain large conical teeth, found in the Malton oolite, may, Mr. Owen thinks, appertain to the Cetiosaurus, but he is of opinion that they more probably belong to the Steneosaurus.

In conclusion, it is stated that the vertebræ described in the paper prove the existence of a saurian genus distinct from the Megalosaurus, Steneosaurus, Poikilopleuron, Plesiosaurus, or any other large extinct reptile, remains of which have been discovered in the oolitic series; that the vertebræ, as well as the bones of the extremities, prove its marine habits; and that the surpassing bulk and strength of the Cetiosaurus were probably assigned to it with carnivorous habits, that it might keep in check the Crocodilians and Plesiosauri.

5. "On the age of the Tertiary beds of the Tagus, with a Catalogue of the Fossils," by James Smith, Esq., of Jordan Hill, F.G.S.

During a visit to Portugal in 1840, Mr. Smith made a collection of the organic remains in the tertiary deposits near Lisbon, for the purpose of ascertaining their relative geological age. Since his return to England he has carefully examined the collection, assisted by Mr. George Sowerby, and ascertained that the series of beds from which they were obtained belong to the miocene division of the tertiary system, and to that portion of it which includes the Bordeaux and Dax beds, rather than to any other yet described deposit. He has, however, determined, by a careful comparison of the Lisbon fossils with those given in the works of MM. de Basterot and Grateloup, and with his own collection of Bordeaux organic remains, that there is a greater difference than can be ascribed to geographical distance alone; but he hesitates to assign to the Lisbon beds either a more ancient or a less ancient date. The proportion of recent shells, he states, affords no assistance, as, according to M. de Basterot, the existing species in the Bordeaux basin equal 23 per cent., and according to M. Grateloup, 37 per cent., whilst Mr. Smith's collection of Lisbon fossils gives 28 per cent. The author is fully convinced of the soundness of the principle of determining the comparative age of a tertiary deposit by the proportion of recent species; but he is of opinion, on account of the great difficulty of defining species, that it is only possible to arrive at an approximation sufficiently near to decide to which of the great divisions of the tertiary system a set of beds may belong, and not to the precise relative antiquity of two deposits of nearly the same age.

Prof. Agassiz has decided that several of the new species of Lisbon shells occur in the molasse of Switzerland, and he considers the two series of strata as nearly contemporaneous.

Mr. Smith refers to Mr. D. Sharpe's memoir on the neighbourhood of Lisbon*, for a description of the mineral structure of the formation, confining his own remarks to pointing out the localities and position in the series from which the fossils were obtained.

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* Proceedings, Geol. Soc., vol. iii. p. 28, 1839; also Geol. Trans., Second Series, vol. vi. p. 1. A list of tertiary shells is given in p. 113.

In the upper beds, consisting of sand and gravel, and known as the Golden Sands of the Tagus, Mr. Smith found no organic remains; but in the next inferior series of strata, composed of yellow sand, calcareous sandstone and blue marl, the Almada beds of Mr. Sharpe, marine remains abound. Of upwards of 150 species collected by the author, 124 have admitted of being carefully determined, and of these, 20 are new, 51 occur near Bordeaux, 17 in the Faluns of Touraine, 15 in the Sub-apennine and Sicilian beds, 8 in the London and Paris basins, and 35 are recent. Several of the species also occur in the tertiary deposits of Vienna, Switzerland, Turin, and the Morea.

A list of five new species of Echinodermata is also given, one of which M. Agassiz has identified with a Molasse species. The following fishes have likewise been determined by M. Agassiz:—*Oxyrhina Xiphodon*, a Bordeaux ichthyolite; *Carcharias productus*, *C. megalodon*, which occurs in the London basin; *Galeus aduncus*, and *Lamna denticulata*; also a species of Delphinus.

Appended to the paper is a descriptive catalogue of the new species by Mr. G. Sowerby, and drawings of the shells by Mr. G. Sowerby, jun.

6. "Some remarks on the Silurian Strata between Aymestry and Wenlock," by Charles Lyell, Esq., V.P.G.S.

Two points are more particularly discussed in this paper:—1st, the inferences which may be drawn respecting the dislocation of strata from the position of fossil corals in the bed in which they occur, and of the subsidences which beds containing Polyparia underwent during the accumulation of the upper Silurian strata; and 2ndly, certain features in the physical geography of the district between Aymestry and Wenlock, dependent on geological structure.

1. *Inferences from the position of Corals, &c.*—The corals which abound in the Aymestry and Wenlock limestones, in the neighbourhood of Aymestry, retain, Mr. Lyell states, the position in which they grew, the points of attachment being inclined towards the lower part of each stratum, and the convex surface of the hemispherical masses being upwards. At Lower Lye, near Aymestry, this arrangement is advantageously exhibited near the junction of the Wenlock limestone with the lower Ludlow formation, in consequence of the layers of shale or mudstone marking more clearly the stratification than in places where the limestone is almost exclusively an aggregate of organic remains. The Rev. T. T. Lewis has also noticed some rare instances of the roots and base of the stem of an *Encrinus* growing on the top or convex surface of a coral. These facts, with the great size and extent of the corals, (the *Catenipora escharoides* sometimes spreading continuously in a horizontal direction for nine feet and even more, and a hemispherical mass of *Cyathophyllum* in the Ludlow Museum being four feet in diameter) imply, Mr. Lyell states, the slow accumulation of the materials composing the upper Silurian strata.

The vertical position of the corals with respect to the plane of stratification is sufficiently general to deserve particular attention, with a view of determining the amount of dislocation which the enclosing beds may have undergone, and of deciding in some cases whether the strata have been completely inverted; but considerable caution, the author says, is necessary in the application of this test, and that the inference must be drawn, not from a single specimen or a few corals being reversed or inclined, but from the prevailing disposition of the great masses. At Gleedon Hill and Bradley, near Wenlock, he noticed that some of the Polyparia, particularly beds of *Catenipora*, maintained their original vertical direction, while others were inclined or reversed and mingled with broken stems of Crinoidea, leaving no doubt upon his mind that the dislocated specimens were fragments which had been broken off by the action of the waves, and thrown down upon the reef.

From the inquiries of Mr. Darwin and other naturalists, it appears that stone-corals do not flourish at a depth exceeding 120 feet. Without assuming that the habits of extinct species were precisely similar to those now living, Mr. Lyell says, it may nevertheless be inferred from analogy, that the stone-corals of the Silurian period did not live at a depth of many hundred feet; and, consequently, that those parts of the Wenlock limestone in which the corals preserve their natural position, were produced at a moderate depth from the surface. This conclusion, he shows, is also supported by the occurrence of the inverted and broken corals noticed above, and associated with others in the position in which they grew.

A further inference drawn by Mr. Lyell from the limited depths at which corals grow beneath the surface of the ocean, is the subsidences which must have consequently taken place during the accumulation of the upper Silurian strata. Thus in the Gatley escarpment near Aymestry, he shows, that the lower or Wenlock coralline limestone is separated from the upper or Aymestry limestone by more than 400 feet of mudstone or lower Ludlow strata, and that in the same neighbourhood a great thickness of mudstone, amounting at the New Bridge, Ludlow, to 700 feet, is superimposed on the Aymestry limestone. It is, therefore, evident, he says, that at least two great subsidences took place during the accumulation of the upper Silurian strata of Herefordshire and Shropshire, the first of which carried down the Wenlock limestone to a depth exceeding 500 feet, to allow the deposition of the lower Ludlow beds and the Aymestry limestone; and the second of which depressed the whole of these formations to a depth sufficiently great to permit the upper Ludlow strata to be deposited upon the surface where the Aymestry corals had grown. He thinks, however, from analogy, that the sinking of the bed of the ocean probably went on during the whole period, but perhaps at different rates.

2. The attention of the author was drawn to the phænomena which form the subject of the second point in the communication, by the Rev. T. T. Lewis; but before he enters upon their details, he states, that the effects of upheaval and denudation on the upper Silurian strata of this part of England are strictly of the same order as those in the Bernese Jura, described by M. Thurmann[*]; there

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[*] *Essai sur les Soulèvemens Jurassiques.*

being in the latter case an upper oolite or coral rag, reposing on Oxford clay, which is succeeded by an inferior oolite resting upon lias, in the same manner as the Aymestry limestone reposes on the lower Ludlow mudstone strata, and the Wenlock limestone on the Wenlock shale; and there being in both countries two escarpments of calcareous rocks, each having at its base a soft, argillaceous formation.

The fact which Mr. Lewis pointed out to the author is, that in the Wenlock Edge, the lower escarpment, consisting of Wenlock limestone, forms an uninterrupted ridge; while the upper escarpment, composed of Aymestry limestone and associated Ludlow rocks, is divided into many knolls by transverse breaks; but that after crossing the Onny, we find that, in the district between Shelderton and Aymestry, the phænomena are reversed, the upper or Aymestry limestone escarpment being undivided, and the lower or Wenlock limestone ridge being formed of knolls. The cause of this difference, Mr. Lyell is of opinion, exists in the variations in the thickness of the limestones, and their consequent amount of resistance to denuding agents. In the Wenlock Edge, the calcareous strata which form the summit are from 50 to 80 feet thick *, and there are many solid beds in the underlying shale; on the contrary, in the upper escarpment, the capping of Aymestry limestone is inconsiderable: and in the district between Shelderton and Aymestry, where the phænomena are reversed, the Aymestry limestone, with the accompanying solid beds of the upper Ludlow, is from 80 to 90 feet thick, but the Wenlock is of inconsiderable dimensions. In each instance, moreover, the two escarpments are so near to each other, that it is highly improbable that there could have been any great difference in the amount of fracture and fissuring, or that they were not equally affected by the same movement.

In conclusion, Mr. Lyell alludes to Mr. Murchison's description† of the transverse valleys or fissures which divide the Aymestry and Ludlow beds into knolls in the ridge which ranges parallel to Wenlock Edge; and he calls upon those geologists who may have the opportunity, to examine carefully the escarpment of the Edge itself, for the purpose of ascertaining if there be any traces of the prolongation of these fissures. Should they be found to exist, Mr. Lyell says, the comparative integrity of the escarpments may be attributed with still greater confidence to the resistance of the limestone beds which constitute its upper part.

7. "Notes on the Silurian Strata in the neighbourhood of Christiania, in Norway," by Charles Lyell, Esq., V.P.G.S.

In a paper read at the Meeting of the British Association at Liverpool, in 1837, Mr. Lyell inferred that the fossiliferous strata invaded and altered by granite in the neighbourhood of Christiania belong to the Silurian period, in consequence of their containing Graptolites and Catenipora‡; and in this communication he states that, by the assistance of Mr. Lonsdale, he has been enabled to as-

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* See Mr. Murchison's Silurian System, chap. xvii.

† Silurian System, p. 236 *et seq.*

‡ See Seventh Report of the British Association, Notices and Abstracts p. 67; and Athenæum for 1837, No. 516, p. 683.

certain that the fossils contained in the transition rocks of the islands and shores of the fiord of Christiania agree most nearly with those in the lower part of the English Silurian system; and, Mr. Lyell adds, that in mineral character the Norwegian rocks also resemble more closely that part of the system as exhibited in Shropshire and Radnorshire than the upper.

The two principal divisions of the Christiania group consist, first, of dark shale, slate and clay, some of the beds being highly calcareous, and enclosing Graptolites, Trilobites and other fossils; also of beds of grit; and secondly, of strata of smoke-grey limestone abounding in corals, and of sandstone, shale and conglomerate. Prof. Keilhau, who has long studied these formations, of which the beds are much disturbed, inclines to the opinion that the second division is the uppermost deposit. Among the fossils common to the Christiania series and the English lower Silurian strata are *Calymene punctata, Trinucleus Caractaci, Orthoceras conicum, Bellerophon bilobatus, Pentamerus oblongus* and *Graptolites Murchisonius*: other species of Trilobites, which are not British, partake of the same type as those which characterize the Caradoc sandstone or Llandeilo flags.

In the island of Langoen in the fiord of Christiania, a few miles from Holmstrand, Mr. Lyell examined a limestone rich in fossils. It dips regularly towards the west, or in the direction of Holmstrand; and he believes that it constitutes, together with the quartzose sandstone near that town, one of the uppermost divisions of the Christiania formation. Among the corals which he obtained from the limestone, the following have been determined as identical with British species; and as five of them have been found in England, hitherto, only in the upper Silurian strata, and others both in the upper and lower, Mr. Lyell is of opinion that the Langoen deposit may indicate a passage from the lower to the upper Silurian rocks.

Genus and Species.	Stratigraphical position in the English Silurian System.
Catenipora escharoides	Aymestry limestone to Llandeilo flags.
Ptilodictya lanceolata......	Wenlock limestone.
Stromatopora concentrica ..	Wenlock limestone and shale.
Favosites Gothlandica	Aymestry limestone to Caradoc limest.
——— fibrosa	Ditto, ditto.
——— polymorpha ? ..	Upper Ludlow and Aymestry limestone.
Limaria fructuosa	Wenlock limestone and shale.
Millepora ? repens........	Wenlock limestone.

The same beds also contain *Euomphalus subsulcatus, Producta euglypha,* and *Cytherina Baltica.*

A series of fossils lately obtained at Christiania by Mr. Bunbury, lead to precisely similar results*. The total number of species contained in Mr. Lyell and Mr. Bunbury's collections amounts to sixty,

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* Mr. Bunbury has informed Mr. Lyell that *Asaphus expansus, Illænus crassicauda* and a *Sphæronites*, common in the Silurian strata of Christiania, are also characteristic, according to M. de Verneuil, of Silurian beds near St. Petersburg. These species are apparently unknown in England, but Mr. Lyell suggests that the Russian strata containing them will probably prove to be lower Silurian.

at least one-third of which are unknown as British—a want of agreement, Mr. Lyell observes, which may be partly ascribed to an imperfect knowledge of the Silurian Fauna of both countries, and partly to the laws which influence the geographical distribution of existing animals. The author does not deny, that, when the more ancient rocks were formed, the marine species may not have enjoyed a wider geographical range than now; for when coral reefs existed between the 50th and 70th degrees of latitude, a more uniform temperature must have prevailed than at the present day; but he contends that there are no data for imagining that the same species were ever universally distributed.

A description of the igneous rocks of the fiord of Christiania is not within the object of this communication; but Mr. Lyell states, that the island of Langoen is traversed in an east and west direction by several dikes of greenstone, from two to three feet thick, but without dislocating the strata; he likewise mentions, that the junction of the quartzose sandstone of Holmstrand with a vertical dike of felspar porphyry thirty feet thick, is finely exhibited at Smörsteen. The same porphyry also overlies the sandstone at Engnaes. The trap of this district passes into a reddish granite, and the contact of the latter with horizontal, thin beds of Silurian limestone and shale is exposed to the height of fifteen feet at Sotfjeld, N.E. from Holmstrand; and the contact is visible on the opposite side of the fiord. At the line of junction the limestone is white, and the shale is converted into Lydian stone. No veins of granite penetrate the fossiliferous strata in that neighbourhood, as at some places near Christiania; and the occurrence of a breccia at one point, where the limestone joins the plutonic rock, induced Mr. Lyell to suspect that the latter had been there protruded in a solid form.

CHEMICAL SOCIETY OF LONDON.

Nov. 2, 1841.—The following communications were read:—

An extract of a letter from M. Dumas "On the Analysis of Atmospheric Air."

The method of analysis adopted in these experiments was to cause the air under examination to pass through the combustion tube employed in organic analysis, charged with reduced metallic copper, into an exhausted flask, and then weighing the resulting oxide of copper and the nitrogen in the flask. M. Dumas says, "You may be assured that no combination of nitrogen with copper is formed in the circumstances under which we operate, a decided red heat being used; besides, all our analyses agree, as you will be able to judge by the following numbers:—

			By weight.			
" April	27th, 1841,		2292	oxygen in 10,000 of atmospheric air.		
....	28	....	2309	....	....	
....	29	....	2304	....	....	
May	29	....	2301	....	....	during heavy rain.
July	20	....	2303	....	....	during rain, at 1 P.M.
....	22	....	2300	....	....	12 P.M., clear.
....	24	....	2308	....	....	12 A.M., cloudy.

"Thus the three first figures expressing the proportion of oxygen contained in the air are constant, the fourth figure variable. I do not consider, however, that the whole of this difference can arise from errors of observation; it is a subject requiring still further examination. MM. Melloni and Piria are performing the same experiments, at Naples, by the same means; and also M. Stas, at Brussels. M. Levy, who has assisted in the above experiments, intends to repeat them in Denmark."

M. Dumas urges the repetition of these experiments to be made at various times and in various places all over the world, to which the English chemist has more easy access than others, in order to resolve this curious physical problem.

"The density of nitrogen appears to me," he adds, "to be between 0·970 and 0·973. That of oxygen, with which we have been particularly occupied, and upon which we have made twenty different experiments, is always found comprised between 1·105 and 1·108; it appears to be represented very nearly by 1·106. That of carbonic acid has varied between 1·526 and 1·528; if 75 is adopted for the atom of carbon, then oxygen is condensed some thousandths in forming carbonic acid.

"The density of hydrogen is always found above 0·0691, it has varied between 0·0692 and 0·0696; we have operated on quantities of about 17 litres of this gas. As to the composition of water by weight, which has occupied me personally during nearly two months, and on which I am still experimenting, I remain doubtful. I have never found less than 12·50 for the equivalent of hydrogen, and often 12·55, and at present I cannot choose between them. In adopting the first of these numbers no error of any practical consequence can result; but as a philosophical question I take so high an interest in it, that I shall continue my experiments until they leave no doubt on the subject."

"On the Analysis of Cetine and Ethal," by Dr. John Stenhouse. (See Memoirs, Vol. I., Art. 7.)

"Notice on the Artificial Magnetic Oxide of Iron," by Thomas Starkey Thomson, Esq.

After adverting to the process given, in the last edition of Turner's Elements of Chemistry, for the preparation of the artificial magnetic oxide of iron, the discovery of which is attributed to Abich and Gregory, Mr. Thomson says, "Recollecting that this oxide had been produced some years ago, by a process surprisingly similar to that of Dr. Gregory, I corresponded with the inventor of it, Mr. John Mercer, one of the original members of this Society, and partner in the firm of Fort, Brothers and Co., calico-printers, from whose letters I extract the following remarks:—'This substance was prepared by me in 1831, and in 1833 applied extensively as a medicine with great success. Mr. Gossage, of the Stoke Prior Alkali Works, who was staying with me at that time for a few days, was so impressed with its value as a medicine, that, upon his return home, he wrote to me for a quantity of it to send to his friend Dr. Jephson of Leamington, to whom I forwarded a quantity, with the receipt for its preparation, and the dose. This receipt was published by Dr. Jephson and given away among his friends; Dinneford was also employed to make it, and also an agent for the sale of it in Manchester.'"

The following is Mr. Mercer's mode of preparing this oxide:—"Take a quantity, say one pound of the common crystallized protosulphate of iron, dissolve it in water, and add nitric acid in sufficient quantity to peroxidize it, and afterwards expel carefully all excess of nitric or nitrous acid by boiling. To this add one pound of protosulphate of iron, with water sufficient for its solution. Pour the mixture into a solution of caustic potash sufficient in quantity and strength to decompose the whole, and then boil. The precipitate thus thrown down consists of a mechanical mixture of the protoxide and peroxide of iron atom to atom; raise the temperature of the mixture to 212° Fahrenheit, and their chemical union is effected. That such is the succession of changes is proved, by dipping into the mixture, previous to boiling it, a piece of clean cotton cloth, which, after exposure to the air for a few minutes and washing in water, exhibits the buff stain peculiar to peroxide of iron precipitated upon cotton fibre. But if this is performed after the boiling a dirty black stain is obtained, indicating the formation of the black oxide."

This fact is further proved by the oxide, after boiling, having a crystalline structure, when examined under the microscope, the minute plates having a brown colour and being transparent, although the edges of the crystals are not sufficiently defined to trace the form.

Mr. Thomson adverts to the application of the artificial magnetic oxide of iron, either in a dry or moist state suspended in water, as a substance well adapted, from its extreme susceptibility of magnetic influence, to indicate the direction of magnetic or galvanic currents, the magnetic curve described by Dr. Brewster being beautifully exemplified by the use of this oxide.

"On the Influence of Water in Chemical Reactions," by Mr. E. A. Parnell.

Nov. 16.—The following communications were read:—

"On the Analysis of the Oils of Laurel Turpentine, Hyssop, and Assafœtida," by Dr. John Stenhouse. (See Memoirs, Vol. I., Art. 7.)

An extract from a letter from Dr. Clark, "On the Revision and more exact Determination of Atomic Weights."

Dr. Clark finds, that when the proper correction, for weighing in a vacuum instead of in air, is applied to the weighings made by Berzelius, in his experiments on the formation of water, by passing hydrogen gas over ignited oxide of copper, the results are very sensibly altered. "Berzelius gives

"Copper (metal)	395·6	Water produced	112·433	Mean 112·491
Peroxide of copper	495·6	...	112·619	+ ·128
Increase, oxygen	100	...	112·429	− ·062
		Hence hydrogen	12·49	

"But if weighed in a vacuum the increase of 100 for oxygen and the weight of 112·491 for water would both have been greater. The following would be the corrected numbers:—oxygen, 100·0266; water, 112·613; or oxygen being 100, water will be 112·583. Hence hydrogen 12·583, in air 12·491, correction + 0·092. As to Berzelius and Dulong's experiments on the specific gravity of gases, however strange, it is true that the results appear, almost all, to have been miscalculated. The specific gravity of hydrogen, instead of being calculated 0·0687, should have been 0·06986, or with Rudberg's dilatation, 0·06988. With the received specific gravity of oxygen, this would give 12·67 for the equivalent of hydrogen; Dumas's specific gravity of oxygen would give 12·64. On all these considerations, I regard the numbers authorised by the experiments where Berzelius has taken part to be 12·6 for hydrogen."

"On a more simple and correct Mode of Reducing the Indications of the ordinary Saccharometer and Hydrometer to each other," by Robert Warington, Esq.

The great utility of some ready means of effecting these operations was first pointed out on the following grounds:—1st, from the great variety of saccharometers in general use; 2ndly, from their being constructed of brass, which, from its liability to loss of weight, from abrasion and corrosion, causes frequent errors of indication; 3rdly, from some of these instruments, as in that employed by the Excise, reading off degrees of specific gravity of which the saccharometer equivalent is found by referring to a printed table sold with the instrument; and, 4thly, to the practical chemist, from the great cost of these instruments, and from his always having in his hands the means of accurately ascertaining the specific gravity of any samples of worts, or other material on which he may be called to experiment, and therefore only requiring a correct formula for reducing such specific gravities to those of the saccharometer.

The saccharometer is a hydrometer of great delicacy, having its zero point corresponding to the specific gravity of distilled water, and its scale, which has usually a range of specific gravity from 1·000 to 1·150, divided into 54 principal divisions, each of which is again subdivided into 5 or 10 equal parts. The object which is professed to be attained in this instrument, is the indicating the number of pounds of saccharine matter contained in "the barrel" of the infusion of malt and other grain. The imperial barrel contains 36 gallons of distilled water of 10 pounds each, or 360 pounds of water. Of wort, whose indication is 1 on the saccharometer, a barrel weighs 361 pounds; 2 on the saccharometer, 362 pounds, and so on for the 54 divisions of the scale. This instrument does not fulfil its professed object, as,—1st, it does not indicate directly the absolute quantity of solid matter per barrel, but only the change of density which this occasions; 2ndly, it is equally effected by the other ingredients in the infusion of malt, as mucilage, vegetable albumen, &c., as by its sugar. The saccharometer must therefore only be regarded as an instrument of comparison.

The rule usually followed in calculating the specific gravity from the saccharometer indication, is to add 360, the weight of the barrel of water, to the saccharometer indication, and then multiply the result by $2{\cdot}77^{\cdot}$ or $2\frac{7}{9}$ths being the value of each saccharometer pound expressed in terms of specific gravity; 360 multiplied by $2{\cdot}77^{\cdot}$ being equal to 1000, the specific gravity of water. Hence, if 36 be the observed saccharometer indication, the specific gravity is $36 + 360 \times 2{\cdot}77^{\cdot} = 1100$. Reversing the operation, and dividing the number expressing the specific gravity by $2{\cdot}77^{\cdot}$, and then deducting 360 from the result, gives of course the saccharometer indication or gravity; thus $\frac{1100}{2{\cdot}77^{\cdot}} - 360 = 36$. Many works, held in high estimation, by way of facilitating this operation, have adopted the use of the factor 2·78, but this must of necessity involve error without materially shortening the calculation; some parties have gone so far as to state that 2·7 is a sufficient approximation; this, however, with the gravity taken as an illustration, will give an error of 11·4 pounds in excess, or 47·4 instead of 36. The rule adopted by the author for converting real specific gravities, or hydrometer indications, into saccharometer gravities, is as follows:—From the specific gravity observed, expressed in terms of distilled water as unity, deduct 1, and then multiply the result by 360; the product is the equivalent saccharometer indication; thus for specific gravity 1·100; $1{\cdot}100 - 1{\cdot}000 \times 360 = 36$ of the saccharometer. The saccharometer gravity again is calculated from the real specific gravity, by the converse of this operation; divide the saccharometer indication by 360 and then add 1; thus $\frac{36}{360} + 1{\cdot}000 = 1{\cdot}100$.

Dec. 7.—The following communications were read:—

"On a new Class of Cacodyl Compounds containing Platinum," by Professor Bunsen * of Marburg. (See Memoirs, Vol. I., Art. 10.).

"On the Preparation of Chromic Acid," by Robt. Warington, Esq.

In the number of *L'Institut* for 9th July 1840, under the head of "Proceedings of the Imperial Academy of Sciences of St. Petersburgh," a notice is given "On an easy process for preparing chromic acid, and the manner in which it behaves with sulphuric acid," by M. I. Fritzsche. The author pours concentrated sulphuric acid with care into a hot and saturated solution of the bichromate of potash, and obtains a voluminous scarlet crystalline precipitate, which is separated and dried, first by heat, then in a vacuum. This is the chromic acid, which must be washed with a small quantity of cold water to remove the mother liquors and sulphuric acid which may still adhere to it. As to the compound of sulphuric acid and chromic acids described by M. Gay-Lussac in the *Annales de Chimie et de Physique*, vol. xvi. p. 102, the author says "he has never been able to make it, and is very much disposed to doubt its existence." On repeating this process, I found that the chromic acid does not fall alone, but is contaminated by admixture with a considerable quantity of a white saline substance, which on examination proved to be the bisulphate of potash, and which, on account of the great solubility of both these substances as precipitated, there is great difficulty in separating. The modification of this process, which I have found to give chromic acid in a crystalline form and nearly in a state of purity, is to take 100 measures of a cold saturated solution of the bichromate of potash (prepared by boiling and then allowing the solution to cool and deposit the excess of the salt), and add to this from 120 to 150 measures of concentrated sulphuric acid; the latter should be free from sulphate of lead, as otherwise it

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[* A translation of Prof. Bunsen's Researches on the Cacodyl Series will appear in the forthcoming Part of Taylor's Scientific Memoirs.—Ed.]

will fall as chromate and sulphate of lead, with the chromic acid, on dilution with the solution of bichromate. The mixture is then allowed to cool, and the chromic acid gradually crystallizes in beautiful dark crimson needles. Decant the fluid part, and place the crystals with the adhering sulphuric acid on a thick flat tile of biscuit porcelain; another tile is then to be placed upon the crystals, and the whole submitted to pressure for a considerable time. On removing the chromic acid, it will be found in a perfectly dry state, and yielding a mere trace of sulphuric acid on examination.

"On the employment of Chromic Acid as an agent in Galvanic arrangements," by Robert Warington, Esq. (See Memoirs, Vol. I., Art. 9.)

LI. *Intelligence and Miscellaneous Articles.*

ON FIBRE. BY DR. MARTIN BARRY.

DR. BARRY requests us to add the following, in connexion with his Memoir on Fibre, an abstract of which is given at p. 321.

The "white substance of the nervous fibre," surrounding Remak's "band-like axis," consists of filaments having the remarkable structure above described, and often curiously interlaced with one another, as though each of them had a spiral direction. In examining the substance of the optic, olfactory, and auditory nerves, as well as that of the brain and spinal chord, Dr. Barry employed for the most part such as had been preserved in spirit; and, besides using extremely minute portions, he very often avoided adding any covering whatever, the weight of thin mica itself being sufficient to rupture or to flatten this delicate substance, and thus entirely prevent its structure from being seen. In the parts last mentioned, he finds red discs, which pass first into rings, and then into spirals. In fasciculi from the spinal chord, and surrounded by spiral filaments, he met with a "band-like axis," which perhaps corresponds to that of Remak in the nerves: but if so, Dr. Barry's observations go farther even than Remak's. The "axis" described by this observer was found by him to be susceptible of division into filaments. So also is the one described by Dr. Barry. But the latter adds, that each filament is a compound object, which enlarges, and, from analogy, may contain the elements of future structures, formed by division and subdivision, to which no limits can be assigned. The spermatozoa, mentioned in the abstract, were from the epididymis of a person who had died suddenly. The depression noticed in their discoid extremity—corresponding apparently to the "sugient orifice" of some authors—is probably analogous to the source of new substance in other discs. In these examinations, Dr. Barry has generally added to the objects dilute spirit (sp. gr. about 0·940), containing about $\frac{1}{200}$th of corrosive sublimate. Spirals from the leaf-stalk of the strawberry, after the addition of this reagent, were seen to have divided into parallel filaments having the same structure as those above described. Flax presented a quadruple coil of such filaments. In early states of voluntary muscle also, there were seen double and quadruple coils, evidently produced by the same means—division.

Dr. Barry compares the appearance of the vegetable "dotted duct," in its several stages, with that of objects found in mould, in the cornea, in the crystalline lens, and in voluntary muscle; all of which are produced by associations of minute spiral threads. The distribution of the remarkable filaments above described is so universal, that they are found in silk, in the incipient feather, in hair, in the feather-like objects from the wing of the butterfly and gnat, and in the spider's web.

Dr. Barry informs us that he has had the opportunity of showing to several physiologists the principal appearances described in his memoir on fibre. And Professor Owen permits him to state, that he has exhibited to him spirals in voluntary muscle,—muscular "fibrillæ" having a flat, grooved, and compound form,—the filamentous structure of the "white substance in nervous fibre,"—the vegetable spiral becoming double by division,—a coiled filament within red blood-discs,—and the incipient unwinding of the coil in coagulating blood.

FURTHER REMARKS ON FIBRE. BY MARTIN BARRY, M.D.

Dr. Barry examined the following objects, from two of the Mollusca, at the desire of Professor Owen, who dissected them out for the purpose: namely, from the *Oyster*, the branchial ganglion, and the branch connecting it with the labial ganglion; from the *Loligo*, the optic and brachial nerves. In all of these Professor Owen recognised filaments ("fibres") having the same remarkable appearance as those which Dr. Barry had previously shown to him in muscle.

On a subsequent occasion—several physiologists being present, one of whom was Professor Owen—there were seen muscular "fibrillæ," not only flat, grooved, and compound, but separated at the end into their single and simply *spiral* threads,—the really ultimate threads of muscle. In this instance chromic acid was substituted for the reagent above-mentioned (p. 344.) as usually employed by Dr. Barry in these researches: and for the examination of muscle he now finds the chromic acid to be even preferable thereto*.

To find the muscular "fibrillæ" of a size proper for examination, and so loosely held together that they may be separated with ease, the heart of a fish or reptile should be employed. Dr. Barry has used the heart of various fishes, as well as that of the turtle, newt, and frog—and chiefly the frog.

To find those states of voluntary muscle in which the transverse striæ are produced by the windings of comparatively large interlaced spiral filaments (see abstract of the postscript to Dr. Barry's paper "On Fibre," p. 324), he recommends muscle from the tail of the *very minute* tadpole—when this larva is only 4 or 5 lines in length (as at the present season)—or muscle from the leg of a boiled lobster, as being very easily obtained. In these states of muscle, the interlacing spirals are seen to dip inwards, towards the centre of the fasciculus, in a manner that may be represented by making the half-bent fingers of the two hands to alternate with one another, and then viewing them on the extensor side.

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* We are indebted to Dr. Hannover for bringing into notice the use of the chromic acid for such purposes.

To find the filament in red blood-discs, Dr. Barry recommends the blood of a batrachian reptile, such as the frog or newt, on account of the large size of the discs in these animals. The blood should be examined just before its coagulation, as well as at various periods during the formation of the clot. Dr. Barry has usually added one of the above reagents, or nitrate of silver.

ON THE TOTAL ECLIPSE OF THE SUN, JULY 7, 1842.

(From the Royal Astronomical Society's Monthly Notice for March 1842.)

Path of the Moon's Shadow over the Southern Part of France, the North of Italy, and part of Germany, during the Total Eclipse of the Sun on July 7, 1842*. By Lieut. W. S. Stratford, R.N.

During the total eclipse of the sun on July 7, 1842, the moon's shadow will pass over Spain, the south of France, the north of Italy, and part of Germany; and it may induce travellers and others in those countries to prepare for the observation of this important phænomenon, if the means of so doing be furnished.

The following table has, therefore, been computed to enable them to trace the path of the moon's shadow on a large scale, and with very considerable accuracy.

Greenwich Mean Time.	Northern Line.		Central Line.		Southern Line.	
	Long.	N. Lat.	Long.	N. Lat.	Long.	N. Lat
h m s 17 34 39	° ′ 3 44 W.	° ′ 40 57	° ′ 0 15 W.	° ′ 41 26	° ′ 2 3 E.	° ′ 41 29
35 39	0 5 W.	42 30	2 49 E.	42 39	4 36 E.	42 31
36 39	2 43 E.	43 37	5 5 E.	43 34	6 48 E.	43 22
37 39	5 2 E.	44 31	7 4 E.	44 22	8 46 E.	44 6
38 39	7 5 E.	45 18	8 59 E.	45 3	10 33 E.	44 45
39 39	8 58 E.	45 59	10 44 E.	45 42	12 12 E.	45 21
40 39	10 40 E.	46 36	12 22 E.	46 16	13 46 E.	45 53
41 39	12 17 E.	47 10	13 53 E.	46 47	15 14 E.	46 23
42 39	13 50 E.	47 41	15 21 E.	47 17	16 38 E.	46 51
43 39	15 16 E.	48 10	16 43 E.	47 43	18 0 E.	47 16
44 39	16 40 E.	48 36	18 3 E.	48 9	19 19 E.	47 40
45 39	18 2 E.	49 1	19 24 E.	48 32	20 35 E.	48 2
17 46 39	19 20 E.	49 25	20 38 E.	48 55	21 49 E.	48 23

It contains for each minute, from $17^{h}\ 34^{m}\ 39^{s}$ to $17^{h}\ 46^{m}\ 39^{s}$, mean astronomical time at Greenwich, the geographical positions (the longitudes being reckoned from Greenwich) of points on the earth's surface, where the following phænomena occur:—

1. Contact of the upper limbs of the moon and sun.
2. Contact of the centre of the moon and sun.
3. Contact of the lower limbs of the moon and sun.

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[*] July 8, *civil time.*

PATH OF THE MOON'S SHADOW OVER THE SOUTH OF FRANCE, NORTH OF ITALY, AND PART OF GERMANY, DURING THE TOTAL ECLIPSE OF THE SUN ON JULY 7. 1842.

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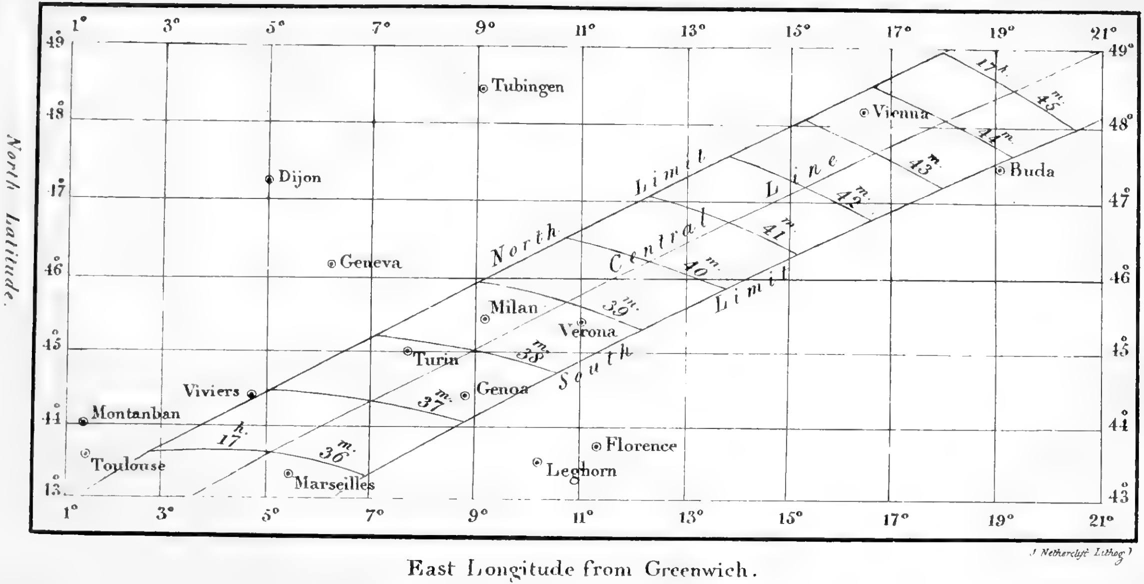

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The connexion of the several points in (1) will trace out the northern limiting line of total eclipse, those in (2) the central line, and those in (3) the southern limiting line of total eclipse.

To indicate some of the principal places over, or near to which the shadow will pass, a small map, on Mercator's projection, is added (Plate IV.); representing the table from 43° to 49° of north latitude, and from 1° to 21° of east longitude. The transverse curve-lines represent arcs of vertical circles; and for the same moment of time, inserted above them, their intersections with the north, central, and south lines respectively, represent the phænomena numbered 1, 2, 3, previously explained. The central line will pass

North of Marseilles . . .	0° 28′	or	32·2	miles.
South of Turin	0 27	or	31·0	...
North of Genoa	0 38	or	43·7	...
South of Milan	0 20	or	23·0	...
North of Verona	0 24	or	27·6	...
South of Vienna	0 32	or	36·9	...
North of Buda	0 58	or	66·8	...

The beginning of the *total* eclipse at a place on the central line, being the contact of the eastern limbs of the moon and sun, and the ending the contact of the western limbs, the interval representing the duration of the total eclipse at any point will be about $2^{\mathrm{m}}{\cdot}4$.

Should the darkness be sufficiently intense, as has been sometimes the case during total eclipses of the sun, to render some of the planets and brighter stars visible, the planet Mercury may be looked for about 5° south of the sun and moon. The planet Mars about west by north, Mars being 15^{m} of right ascension to the west, and 1° 16′ of declination to the north of the sun and moon. The planet Venus is below the horizon until the shadow has passed Vienna, and will scarcely be visible at the eastern limit of the map. Jupiter and Saturn are invisible, being below the horizon during the whole interval. The Georgian is $7^{\mathrm{h}}\ 13^{\mathrm{m}}$ of right ascension to the west, and 23° 53′ to the south of the sun and moon.

The sun and moon are in the constellation Gemini, and will have Castor and Pollux not far distant in a N.N.E. direction; Ursa Major to the northward and eastward; Procyon to the south and east; Orion to the south and west; Taurus to the west; and Auriga and Perseus to the north and west.

Those persons who wish for more detailed information regarding the circumstances and phænomena of solar eclipses, will be amply gratified by consulting 'A Memoir relative to the Annular Eclipse of the Sun, which will happen on Sept. 7, 1820, by Francis Baily (London, 1818);' the works therein referred to, viz. Phil. Trans., vol. xxix. p. 245, vol. xl. p. 177, vol. xlv. p. 582, &c. &c.; and a paper by the same author, in the tenth volume of the Memoirs of the Royal Astronomical Society, 'On a remarkable Phænomenon that occurs in Total and Annular Eclipses of the Sun.'

The memoir alluded to, though unfortunately not printed for sale, was circulated with the author's known liberality so widely, that there is little doubt of its being to be found on the shelves of the libraries of persons who feel interested in these matters.

Since the preceding matter was in type, a copy of Professor Silliman's Journal of Science and Arts, for January 1842, has arrived from America, containing an article " On the Solar Eclipse of July 8, 1842' (*civil time*), from which the following is extracted, as meriting particular attention :—

" As the approaching eclipse will excite great interest throughout Europe, and especially in those places where it will be total, it is earnestly hoped that particular attention will be paid by those favourably situated, and in possession of suitable instruments, to the determination of the correctness of a recent suggestion, that the irregularities so frequently noticed at the second and third contacts of nearly central eclipses, and at all the contacts of the transits of Venus, may be seen or not at the pleasure of the observer, according as the colour of the dark glass he applies to his telescope is red or green. These irregularities, as seen by many, have been minutely described by Francis Baily, Esq. of London, in an article in the tenth volume of the Memoirs of the Astronomical Society, although it particularly relates to the appearances, observed by himself, in the south part of Scotland, during the eclipse of May 15th, 1836, which was annular there. Many of the appearances described by Mr. Baily were seen through a *red* glass at the second and third contacts of the eclipse of February 12th, 1831, which was annular in the south-eastern part of the State. Shortly afterwards, however, it having been ascertained that a double screen, composed of one light red and one light green glass, would not only render the light of the sun very pleasant to the eye, but would far better define the limbs, and would sometimes even enable me to see a small spot, that was invisible through the dark red alone, a screen of that kind was adapted to the telescope, and was used for the partial eclipses of 1832 and 1836, and those that were central in 1834 and 1838. Through this screen no one of the irregularities described by Mr. Baily has ever been perceived, although carefully looked for. Indeed, so remarkable was the difference between the observed and expected appearances of the sun's limbs at the second and third contacts at Beaufort, S. C. on November 30th, 1834, that even then a suspicion was excited that the entire absence of all distortion or irregularity in the cusps, just before and after the total obscuration, was to be attributed to the colour of the screen, especially since other observers in the vicinity of Beaufort saw through red screens many or most of the usual phænomena. This suspicion was strengthened by the observations on the large, but not central eclipse of May 1836; it was therefore communicated to several of our astronomers, who paid particular attention to it, at the formation and rupture of the ring on September 18th, 1838. In Philadelphia and its vicinity there were many observers, provided with telescopes of nearly equal optical capacity, but protected by screens of different colours. The result appears to be, that in every, or nearly every instance, in which the red glass was used, many or all of the usual irregularities were seen, whilst those observers who used yellow or green screens saw these appearances either greatly modified or not at all. At Princeton, near the northern boundary of the ring, two skilful astronomers, provided with $3\frac{1}{2}$-feet telescopes by Dollond and Fraunhofer, were enabled distinctly to see some of these appearances through the red eye-piece of the former, though none was visible through the green screen of the latter instrument. At Washington, where the eclipse was nearly central, no distortion of the limb of the moon could be seen through the double screen above mentioned, and the cusps of the sun, just before and after the ring, were as pointed as needles. The Committee of the Philosophical Society of Philadelphia, in their report on this eclipse, say, 'This suggestion is one of great importance, as it seems to furnish evidence of the existence of a lunar atmosphere, through which, as through our own, the red rays have the greatest penetrative power. It also leads to new views concerning the cause of the remarkable appearances of the beads of light and the dark lines frequently noticed; since it shows that their appearance may be completely modified by a change in the colour, and, consequently, in the absorbing power of the screen glass through which they are observed.' It is believed that on another account will this suggestion, if well founded, be of great importance, viz. in its obvious tendency to diminish, if not wholly remove, the discordances not unfrequently found in the best observations on solar eclipses and transits of Venus, and which, with regard to the latter in 1761 and 1769, were so great as materially to diminish the value of this method of determining the distance between the earth and the sun.

"Phases of the Eclipse at some of the principal Cities of Europe."
The longitudes are reckoned from Greenwich, and the times indicate mean civil times *at each place respectively, on July* 8, 1842.

	Lat. North.	Long.	Beginning of Eclipse.		Ending of Eclipse.	
			Partial.	Total.	Total.	Partial.
	° ′	° ′	h m	h m s	h m s	h m
Brescia ...	45 32	10 13 E.	5 24	6 19 18	6 21 44	7 21
Genoa......	44 24	8 54 E.	5 18	6 12 53	6 14 31	7 14
Gratz	47 4	15 27 E.	5 46	6 43 14	6 45 44	7 48
Lemberg...	49 52	24 3 E.	6 25	7 24 33	7 27 24	8 33
Madrid ...	40 25	3 42 W.	bef.⊙rise	5 18 45	5 20 30	6 16
Marseilles	43 18	5 22 E.	5 3	5 57 3	5 59 5	6 57
Milan	45 28	9 12 E.	5 20	6 15 4	6 17 18	7 17
Nice	43 42	7 17 E.	5 11	6 5 36	6 6 52	7 6
Padua	45 24	11 52 E.	5 30	6 26 28	6 27 56	7 29
Pavia	45 11	9 9 E.	5 20	6 14 28	6 16 52	7 17
Presburg...	48 8	17 6 E.	5 54	6 51 44	6 54 14	7 57
Turin	45 4	7 42 E.	5 14	6 8 35	6 10 34	7 10
Venice	45 26	12 20 E.	5 32	6 28 49	6 29 33	7 31
Verona.....	45 26	10 59 E.	5 27	6 22 26	6 24 40	7 25
Vienna......	48 13	16 23 E.	5 51	6 48 58	6 50 55	7 54

DECOMPOSITION OF BROMATE OF POTASH BY HEAT.

According to M. Fritzsche, when bromate of potash is subjected to a high temperature in a mercury-bath, the crystals of the salt decrepitate, and are reduced to powder. If this powder be thrown into water it immediately disengages pure oxygen gas: the evolution is most rapid when the temperature of the water is raised from about 160° to 175°. In the opinion of the author, the bromate influenced by heat is decomposed into bromite and oxibromate, the latter being immediately decomposed by the water into bromate and oxygen gas. It appears that the bromite possesses the property of readily absorbing oxygen, and reproducing bromate, for the author found that the powder, dissolved in water and evaporated in the air, reproduced exactly the quantity of bromate originally submitted to experiment.

The production of oxibromate, under these circumstances, is a curious fact, which connects the bromate with the chlorate of potash; and the decomposition which the oxibromate undergoes in water, explains how it happens that it has been found impossible to obtain oxibromate of potash in the moist way.—*Journal de Pharm. et de Chim.*, Jan. 1842.

ON THE LIGHT WHICH APPEARS DURING CRYSTALLIZATION.

BY M. H. ROSE.

By fusing a mixture of one equivalent each of sulphate of potash and soda in a platina crucible, the author obtained a vitreous mass devoid of crystalline texture; this was dissolved in boiling water and the solution quickly filtered, and allowed to cool slowly in the dark; it exhibited the same appearance of light as was observed by M. Rose in 1836 during the crystallization of a solution of vitreous arsenious acid in hydrochloric acid; the formation of each rudiment of a crystal was announced by a spark. The crystals thus obtained exhibit nothing similar when re-dissolved; but if crystals which have been formed with the disengagement of light be taken from the solution, they become again phosphorescent when strongly rubbed or pressed; they do not retain this property for more than a few hours, and have the usual crystalline form of the salt.

No phosphorescence occurs during the crystallization of the sulphate of potash, when the vitreous mass is dissolved more than 24 hours after its fusion; it appears then to have passed to the crystalline state. The crystals deposited with phosphorescence are not formed of pure sulphate of potash; they are a true double salt, which possesses the same crystalline form as sulphate of potash and most of its physical properties. M. Rose found in several experiments a double salt formed of 2 atoms of sulphate of potash and 1 atom of sulphate of soda; in other cases, on the contrary, the proportions were 3 atoms of sulphate of potash and 2 atoms of sulphate of soda.

The phænomenon of phosphorescence, in the case of the double sulphate of potash and soda, appears to depend on its vitreous state, which it retains in solution, and passes to the crystalline state at the moment only of its separation from solution.

M. Rose has found, by a great number of analyses, that the sulphate of potash of commerce contains sulphate of soda, forming with it a double salt, in which, contrary to what usually happens, the potash is isomorphous with soda.

The double sulphate of potash and soda is produced, not only by fusing the two mixed sulphates, but by fusing sulphate of potash with chloride of sodium, or carbonate of soda, or chloride of potassium with sulphate of soda.

Equivalents of neutral chromate of potash and anhydrous sulphate of soda, exhibited after fusion the same phænomena as the compound of the two sulphates; the crystals formed were composed of the two acids and two bases.

The double chromate of potash and soda, free from sulphate, exhibit very lively phosphorescence, and so also does the seleniate of potash and soda.

The light which appears during the crystallization of certain bodies is due, as proved by the foregoing statements in the opinion of the author, to the passage of a salt from one state to another isomeric with it. These transitions are very often accompanied with phænomena, which appear of the same nature as phosphorescence, during the crystallization of some salts; among the most common may be cited the sudden incandescence of oxide of chromium, titanic acid, &c.—*Journal de Pharm. et de Chim.*, January 1842.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1842.

Chiswick.—February 1. Overcast: very fine: clear. 2. Very fine: slight rain in the evening. 3. Slight haze. 4. Calm with slight haze. 5. Hazy. 6. Dry haze: fine. 7. Sleet. 8. Foggy. 9. Overcast: fine. 10. Slight haze. 11, 12. Cloudy. 13. Clear and fine. 14—16. Very fine. 17. Foggy. 18. Clear and fine. 19. Frosty and foggy. 20. Drizzly. 21. Overcast: clear. 22. Drizzly: cloudy. 23. Thickly overcast: cloudy: rain. 24. Cloudy. 25. Cold rain: showery. 26. Showery: clear and cold. 27. Stormy showers: heavy rain: densely overcast. 28. Fine: overcast: stormy at night.

Boston.—Feb. 1. Fine: rain early A.M. 2, 3. Fine. 4. Foggy. 5, 6. Cloudy. 7. Cloudy: snow A.M.: rain P.M. 8. Cloudy: rain P.M. 9. Foggy. 10. Cloudy. 11. Fine: rain P.M. 12. Cloudy. 13—16. Fine. 17, 18. Cloudy. 19. Foggy. 20. Fine. 21. Cloudy: rain A.M. and P.M. 22. Fine. 23. Cloudy: rain P.M. 24. Rain. 25. Rain: rain early A.M.: snow A.M. 26. Cloudy: snow early A.M. 27. Cloudy: rain A.M. 28. Stormy.

Sandwick Manse, Orkney.—Feb. 1. Cloudy: showers. 2. Showers: aurora. 3. Cloudy: clear. 4, 5. Fine. 6. Damp: frost. 7. Frost: aurora. 8. Frost: cloudy. 9, 10. Clear: rain. 11. Clear: showery. 12. Showery. 13. Clear: cloudy. 14. Cloudy: aurora. 15. Showers: aurora. 16. Cloudy. 17. Fine. 18. Cloudy: showers. 19. Cloudy: rain. 20. Showers. 21. Clear. 22. Clear: cloudy. 23. Cloudy. 24. Clear: cloudy. 25. Clear. 26, 27. Frost: sleet-showers. 28. Showers.

Applegarth Manse, Dumfries-shire.—Feb. 1, 2. Fine: thaw. 3. Frost A.M.: fog P.M. 4. Thaw and slight rain A.M. 5. Frost: fine. 6. Dull and cloudy, but freezing. 7. Frost: still dull. 8. Frost: clear. 9. Thaw and heavy rain P.M. 10. Wet morning: blew strong. 11, 12. Very wet and stormy. 13. Wet. 14. Slight showers. 15. Slight showers: cleared P.M. 16. Frost A.M.: fair all day. 17. Dull and moist. 18, 19. Fine and fair. 20. Fine and fair, but high wind. 21—23. Rain A.M.: cleared. 24. Frost A.M.: fine. 25. Wet A.M.: cleared P.M. 26. Rain P.M. 27. Snow, hail, rain and wind. 28. Rain.

Sun shone out 24 days. Rain fell 15 days. Frost 7 days. Snow and hail 1 day. Fog 1 day.

Wind north-north-east 1 day. East-north-east 1 day. East 3 days. East-south-east 3 days. South 4 days. South-south-west 7 days. South-west 4 days. West-south-west 1 day. West 5 days.

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary*, Mr. Roberton; *by* Mr. Thompson, *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *by* Mr. Dunbar, *at Applegarth Manse*, DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.								Thermometer.										Wind.					Rain.					Dew-point.
1842. Feb.	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8½ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R. S. 9 a.m.
		Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mm.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.											
1.	30·140	30·130	30·100	29·74	29·92	29·87	29·80	29·54	39·5	47·0	37·8	46	26	35	42	32½	39	43½	w.	nw.	calm	w.	s.	·166	......	·22	......	·04	42
☾ 2.	30·198	30·241	30·059	29·79	29·85	30·09	29·72	29·94	41·7	45·7	38·0	50	40	37	46	39	40	42	sw.	w.	calm	w.	w.	......	·02	......	......	·13	38
3.	30·436	30·409	30·377	30·03	30·21	30·24	30·06	30·20	43·8	49·2	41·7	45	37	35	43½	31½	45	43½	nw.	nw.	calm	ssw.	sw.	......	......	......	......	·05	41
4.	30·480	30·439	30·351	30·12	30·21	30·20	30·15	30·18	39·8	46·3	40·3	42	30	36	41½	30½	41	41	ne.	e.	calm	ssw.	s.	......	......	......	......	......	40
5.	30·286	30·267	30·177	29·99	30·17	30·05	30·15	30·16	35·7	40·0	33·3	42	27	30	42	30	42	40	ene.	e.	calm	w.	sw.	......	......	......	......	......	35
6.	30·118	30·109	29·892	29·83	30·00	29·85	30·07	29·97	32·8	35·7	32·0	38	26	33	32½	27½	37	33½	e.	e.	calm	e.	se.	......	·06	......	0·00	......	29
7.	29·724	29·786	29·697	29·52	29·70	29·68	29·84	29·81	35·2	37·6	33·0	40	30	35	37	26	35	34	ene.	e.	calm	e.	se.	·033	·11	......	......	......	33
8.	29·828	29·816	29·786	29·55	29·69	29·69	29·87	29·86	34·8	40·6	33·0	46	28	35	41	30½	36½	37	ne.	e.	calm	ene.	se.	·094	·06	·20	......	......	33
9.	29·812	29·795	29·512	29·50	29·55	29·41	29·71	29·51	40·8	45·0	35·2	51	43	40	43	32	39	41	s.	s.	calm	ese.	se.	·063	......	·06	......	......	38
● 10.	29·946	29·986	29·895	29·53	29·57	29·55	29·52	29·35	45·7	49·0	41·2	51	41	45	46	39	41	42	s.	s.	calm	ssw.	se.	......	......	......	......	·17	41
11.	30·026	29·985	29·970	29·49	29·42	29·54	29·20	29·25	47·3	50·3	45·8	52	47	47	47	42	46	40	s.	s.	sw.	ssw.	s.	......	·14	......	......	·32	46
12.	30·072	30·065	30·004	29·50	29·49	29·53	29·35	29·20	50·2	52·2	47·4	54	30	50	48	39½	41	40	s.	s.	sw.	s.	s.	·094	......	·11	......	·15	46
13.	30·220	30·180	30·020	29·74	29·79	29·76	29·62	29·58	44·2	53·0	42·5	52	28	47·5	43½	37½	41	38½	sse.	s.	calm	ese.	sw.	·009	·04	......	1·48	·08	43
14.	30·456	30·452	30·390	29·94	30·10	30·11	29·94	29·94	40·3	50·7	38·5	52	37	38	46	34	42½	45	w.	s.	w.	sw.	s.	·061	......	......	......	·07	41
15.	30·500	30·446	30·424	29·98	30·00	30·24	29·81	29·05	46·8	49·7	40·3	54	40	41	47½	41	44	43	s.	sw.	s.	w.	sw.	......	......	......	......	·06	44
16.	30·546	30·488	30·416	30·05	30·30	30·24	30·12	30·06	46·3	51·6	41·8	53	33	39	46	32	44	45	w.	sw.	calm	sw.	sw.	......	......	......	......	......	43
17.	30·410	30·372	30·252	29·90	30·12	30·08	30·03	30·05	44·3	49·2	43·7	49	25	43	45	34	42½	39	s.	s.	calm	sw.	sw.	......	......	......	......	......	43
☽ 18.	30·374	30·367	30·316	29·94	30·12	30·20	29·93	30·12	36·8	48·2	36·0	47	24	36·5	47½	40	44½	41	s.	nw.	calm	sw.	w.	......	......	......	......	......	38
19.	30·402	30·377	30·194	29·98	30·12	29·90	29·89	29·68	34·7	43·7	33·3	49	30	33·5	48	40	42	44½	sw.	w.	calm	s.	sw.	......	·02	......	......	......	39
20.	30·054	30·030	29·881	29·68	29·75	29·59	29·52	29·53	35·3	45·0	35·0	40	31	37	48	40½	43	41	s.	sw.	calm	s.	sw.	......	......	......	0·21	·44	37
21.	29·782	29·813	29·771	29·40	29·45	29·60	29·49	29·52	39·7	40·0	35·0	48	27	36	44	43	40	39	se.	sw.	se.	ssw.	w.	......	·16	......	......	·18	36
22.	29·756	29·753	29·616	29·47	29·50	29·31	29·44	29·27	41·8	47·8	38·8	50	34	33	45	30½	40	40½	c.	s.	calm	s.	s.	·161	......	·13	......	......	39
23.	29·444	29·444	29·167	29·14	29·17	28·95	29·24	29·05	44·8	48·8	42·0	50	40	39	43	37	40	41	sse.	s.	calm	e.	se.	......	·11	......	......	......	42
24.	29·152	29·171	29·115	28·77	28·98	28·98	29·08	29·10	41·9	49·5	41·8	47	33	41·5	43	29½	37	36	s.	s.	calm	nne.	calm	·059	·11	·16	......	·15	41
○ 25.	29·216	29·250	29·180	28·80	29·07	29·08	29·12	29·09	39·3	48·0	36·3	45	23	39	44	35½	39	37	w.	w.	calm	w.	s.	·130	·07	·20	......	......	41
26.	29·400	29·570	29·367	29·00	29·00	29·24	28·93	29·15	39·2	45·3	35·0	48	30	34	42	31½	34	38	se.	w.	calm	wsw.	w.	·052	·04	·31	......	......	36
27.	29·550	29·470	29·377	29·17	28·95	28·87	29·00	28·52	40·5	46·0	35·2	44	36	35	42	29½	36	38½	s.	s.	se.	ssw.	se.	·133	·05	......	1·00	·10	36
28.	29·622	29·593	29·388	29·12	28·90	28·97	28·65	28·90	41·7	43·6	39·0	51	42	40	46	36	42	41½	s.	s.	w.	ssw.	w.	·291	·33	·09	......	·26	38
Mean.	29·998	29·953	29·882	29·59	29·681	29·671	29·610	29·590	40·9	46·4	38·4	47·71	32·78	38·2	39·3	34·9	40·50	40·21						Sum. 1·346	1·32	1·48	2·69	2·20	Mean. 39

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[THIRD SERIES.]

MAY 1842.

LII. *Reply to the Objections and Strictures of* Dr. Hare, *with reference to the Whirlwind Theory of Storms. By* W. C. Redfield*.

IN this Journal for December 1841 (S. 3. vol. xix. p. 423), there appears an article "On the Theory of Storms, with reference to the views of Mr. Redfield," by Robert Hare, M.D., &c. &c., which is also found, substantially, in Silliman's American Journal of Science for January 1842, and affords occasion for the notes and remarks which follow.

The several series of facts and observations, showing both the rotary and progressive movement of great storms, which I have published, together with those which have also been adduced by Reid, Milne, Dove and Piddington†, are deemed sufficient to establish the whirlwind character of these storms. In the absence, therefore, of contravening facts of a reliable character, it seems incumbent on an objector to set aside these facts and observations as unfounded and inaccurate, or to show that the results which they appear to establish have been deduced erroneously. This task Dr. Hare has not attempted; and I might therefore have been excused from replying to his objections and strictures, as these cannot affect the results which it has been my chief aim to establish.

But the observations which I have published extend also to the so-called tornado or water-spout, and with similar re-

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* Communicated by the Author.

† See American Journal of Science, xx. 20–40; xxv. 114–121; xxxi. 115–130; xxxv. 201–223; xlii. 112–119; also this Journal for January 1841 (S. 3. vol. xviii.), p. 17–19. Reid on the Law of Storms. Weale, Lond. 1837. Transactions of the Royal Society of Edinburgh, vol. xiv. p. 467–487. Poggendorff's *Annalen*, Jan. 1841, &c. Piddington's three Memoirs on the Law of Storms in India. Calcutta.

sults*; while Mr. Espy and Dr. Hare have each in turn advanced his theory of tornadoes and storms, founded on *à priori* reasoning or speculation, and on alleged deductions from phænomena observed. Hence, perhaps, originates this fourth attempt, from one or other of these sources, to discredit the results of my principal inquiries; being, however, the first from Dr. Hare.

Moreover, I have sometimes ventured to offer summary sketches of other results or conclusions which seemed to follow from the above-mentioned and other developments, which came under notice in pursuing my meteorological inquiries†. These sketches or conclusions were given, partly as *notifications*, and partly because I was not willing it should appear in after years, that such results or conclusions as I have noticed had been overlooked in conducting my examinations. These inceptive statements seem to have occasioned many of the strictures and criticisms which I am now to notice.

Dr. Hare says, that my "idea that tornadoes and hurricanes are all whirlwinds, involves some improbabilities," and that it requires, that "during every hurricane there should be blasts of nearly equal force coinciding with every tangent which can be applied to a circle," and that "thirty-two ships, equidistant from the axis of gyration and from each other, should each have the wind from a different point of the compass with nearly equal force." The only modification he admits, "is that resulting from the progressive motion which tends to increase the velocity of the wind" on one side, "and to diminish it upon the other."

I could never have imagined that any "idea" of mine necessarily involved the conditions here specified; and if the fact be such, Dr. Hare would have rendered some service by making it manifest. The modification admitted by him, vitally important as it is, shows only *one* of the conditions which would doubtless prevent any such perfect symmetry of results as he demands; to say nothing of the practical error of supposing that the course of the wind in a whirlwind must coincide with the tangents of a circle. He alleges also, "that as respects any one station, the chances would be extremely unfavourable that the same hurricane should twice proceed from the same quarter." If by this is meant that the changes of wind at any one station in the same gale are not likely to

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* See Silliman's American Journal of Science, vol. xli. (July 1841) p. 69–77. Journ. Frank. Instit., vol. iii., third series, p. 40–49; also this Journal for January 1841, p. 20–29.

† See Silliman's Journal, xxxiii. 50–65; also various incidental remarks and statements in other papers.

come back to the same point of the compass from which it had before blown, except by an extraneous force or influence, we shall in this be able to agree. He states further, that " in the course of time it would be felt, at any station, to proceed from many different directions, if not from every point of the compass." The first of these conditions is verified by observation, except as I have shown that the changes in a regular whirlwind storm will not, in the true wind of the gale, be likely to exceed sixteen points of the compass at any one station. It will be difficult, however, for Dr. Hare to show, that the regular changes in a progressive whirlwind storm, as truly exhibited at any fixed station, should run through every point of the compass; although this may sometimes happen to a ship moving in the storm.

Dr. Hare does not appear to perceive, that the several conditions above referred to are for the most part no more predicable of the whirlwind storm than of the affluent theory of storms which he advocates.

Dr. Hare states (in the American Journal), that " the fact that during the same storm different vessels variously situated are found to have the wind in as many different directions, may be explained by the afflux of winds from all quarters to a common focal area, as well as by supposing them involved in a great whirlwind." This might be true, as I have virtually stated elsewhere, provided that the direction of the wind at such vessels was found, at a given time, to be towards such a " focal area," *which does not happen*; the observed differences of the winds from these centripetal directions being nearly equal to ninety degrees, or a right angle, as has been repeatedly shown[*].

I have formerly stated, that " I have observed in the effects of the New Brunswick tornado numerous facts which appear to demonstrate the *whirling* character of this tornado, as well as the *inward* tendency of the vortex at the surface of the ground[†]." But Dr. Hare thinks, " that the survey of Bache and Espy shows that it would not be consistent with the facts to suppose such motion, unless contingently; and that it could only be a casual effect." Now, without inquiring whether the constant whirling action to which I alluded be a contingent or a necessary result, it is proper to notice, that the great question between us is and has been, *have storms a gyratory*

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[*] See Silliman's Journal, xxv. 116; xxxi. 117–118; xxxv. 210–215; xlii. 112–119. Journ. Frank. Instit. 1839, p. 323–336, and p. 363–378. Dove in Poggendorff's *Annalen*, Jan. 1841, pp. 10, 11 *seq.*; also this Journal for January 1841, p. 17–19, and map.

[†] See Silliman's Journal, xxxv. 207. Nautical Magazine, Jan. 1839, p. 6.

character? To me, the facts established by all the strict observations which have been made and properly stated, proclaim the affirmative. We shall probably find, on a strict examination, that even the surveys of Prof. Bache, though not comprising all the particulars which I deem essential to a right view of the case, may yet be best explained by admitting a general and continued whirlwind action.

Dr. Hare next adduces an imperfect quotation on the law of atmospheric circulation, as depending on the earth's rotation, centrifugal action, &c.; and presumes me to mean, "that the centrifugal force communicated to the air at the equator, causes it to rise and give place to those portions of the atmosphere," from adjacent latitudes, which "have less rotary motion;" and proceeds to comment on this presumption. I beg leave to assure Dr. Hare that he has greatly misapprehended my meaning; and furthermore, that I have never found any evidence of the supposed general ascent of the air from the lower to the upper atmosphere in the equatorial regions.

In my first essay, the prevalence of westerly winds in the upper regions of the atmosphere was incidentally and partially ascribed to the deflection of the trade-winds by mountains. Dr. Hare alleges that this explanation harmonizes with the theory of Halley. He adds, "In fact, as the water accumulated by these winds in the Gulf of Mexico is productive of a gulf-stream, is it not reasonable that there should be an aërial accumulation and current corresponding with that of the aqueous current above mentioned?" This comes nearer to my views of the *course* of circulation in the atmosphere, but does not so well accord with the common theory of the trade-winds. That the alleged accumulation of water in the Gulf of Mexico by the trade-winds is the main cause of the gulf-stream, Dr. Hare may perhaps show hereafter. The contrary would appear to have been settled by the levelings which have already been obtained.

Dr. Hare intimates that the trade-winds "cannot be explained without the agency of temperature;" he alleges also that I "admit of no other cause of atmospheric currents besides that of gravitation;" and he inquires, "what other effect could gravitation have, in the absence of calorific and electrical reaction, unless that of producing a state of inert quiescence?" He also speaks of my treating momentum as "the antagonist of gravitation." (p. 424, par. 7–10.)

Now to all this I answer,—1. That, to my apprehension, the essential features of the trade-winds can be best explained without assigning the agency of temperature as the chief moving power. 2. It is an error to say that I admit no other cause of winds besides gravitation, if by this is meant that I reject the influence of heat, as is alleged in the American Journal. 3. I consider the influences of momentum, centrifugal force, and centripetal action, as being comprised in the laws of gravitation. 4. It is true that I do not consider "electricity" as a general *cause* of atmospheric currents; for the reason, that so far as I know, this has never been shown. 5. That the only effect of gravitation, without calorific or electrical reaction, would be to produce "a state of inert quiescence" in the atmosphere of a *moving* and *rotative planet* like our own, is to me inconceivable. 6. I have never considered nor asserted "momentum" to be "the antagonist of gravitation." In the paragraph quoted by Dr. Hare, I had suggested, *the courses of great storms as indicating the law of circulation in our atmosphere, and which I deemed to be founded mainly on the laws of gravitation.* By some mistake, he has given the phrase "*causes* of great storms" instead of *courses*; and proceeding on this error, he calls it a summing up of the "causes" of atmospheric currents; although he alleges that I here admit but *one* cause.

It is next asked, "If the minuteness of the altitude of the atmosphere, when compared with its horizontal extent, be an objection to any available currents being induced by calorific rarefaction," as he states I have alleged, "wherefore, for the same reason, should not momentum, or any other cause *diminishing or counteracting the influence of gravity*, be equally inefficient?" To this I answer,—1. Momentum, and the other modifications of the gravitating power, are of far greater magnitude and force than the influence of the mere difference of temperature in the several geographical or climatorial zones. 2. The main tendency or result of this greater force is to produce *horizontal*, not vertical motion. 3. The words which I have italicised, show only the misapprehension corrected above, and which appears to run through the strictures which I am noticing. By "available currents," as above quoted, I here understand the great currents of the atmosphere constituting the trade-winds, &c.

In succeeding paragraphs (12—14) Dr. Hare criticises the terms by which I have endeavoured to point out, that a whirling or rotative movement *is the only known cause of a violent and destructive force in winds or tempests*, as the last clause of the paragraph quoted by him should read. There is little probability that my meaning has been misunderstood by general readers; and it appears afterwards to have been divined by Dr. Hare himself.

After a short comment on the functions of gravitation, Dr. Hare next inquires, "But if neither gravity, nor calorific expansion, nor electricity be the cause of winds, by what are they produced?" I answer,—1. According to my apprehension, the gravity which induces a nearly equal "distribution of the atmosphere over the surface of the globe," may and does, in its modified influences, constitute the main basis of winds and storms. 2. That calorific expansion is *a* "cause of winds," is universally admitted; but that it is the *chief* cause, I cannot perceive. 3. If "electricity" be the cause of winds, it seems incumbent on Dr. Hare to show it.

For my own part, having never attempted to write out or establish a *theory* of the winds, in the common acceptation of the term, nor yet of the origin or first cause of storms, I have no occasion to go into these inquiries any further than relates to my present purpose. It is true that I entertained some definite views on these points, which have resulted from observation and inquiry; but the choice of time and occasion for their more full development, and also of the evidence on which they rest, belongs to myself rather than to another. I do not intend being diverted from my ordinary business, or from the results of direct observations in storms, by engaging in a controversial discussion of those general views of the alleged cause of winds, and of the physico-mechanics of the atmosphere, which now prevail, and which are held by men of the highest attainments in physical science. And in relation to storms, I have long held the proper inquiry to be, *What are storms*? and not *How are storms produced?* as has been well expressed by another. It is only when the former of these inquiries is solved, that we can enter advantageously upon the latter.

I have stated, incidentally, that all fluid matter has a tendency to run into whirls or circuits, when subject to the influence of unequal or opposing forces, &c. Dr. Hare says, that "if this were true, evidently whirlpools, or vortices of some kind, ought to be as frequent in the ocean, as, agreeably to my observation, they are found to be in the atmosphere;" that "the aquatic gulf-stream, resulting from the impetus of the trade-winds, ought to produce as many vortices in its course as the aërial currents derived from the same source;" and he adds, "there are few vortices or whirlpools in the ocean," for reasons which he has chosen to assign. (17–18.)

Now the alleging of an equal tendency of aqueous currents to run into "vortices" with the aërial ones, belongs to Dr. Hare, not to me. In the ocean, we can but partially observe the upper surface of superficial currents, moving apparently unobstructed on the more quiescent waters beneath; and with the relative equality of motion in the parts generally maintained, I see not how the unimpeded movements of this denser and nearly non-elastic fluid are to produce vortices equal in number or magnitude to those which occur in the inferior layers of an elastic aërial current, moving on or near the surface of the earth, over obstructions and inequalities, and with other disturbing conditions almost innumerable. Of Dr. Hare's views of aqueous vortices it is unnecessary to speak; but there are mariners, if I remember their statements aright, who can give him an account of the frequency of ocean or gulf-stream vortices, somewhat different from that which he advances. Whenever a stream or current of water of moderate depth moves over an unequal bottom, there is found no lack of vortices, of various forms and dimensions, some of which exhibit both upward and downward movements, often of some considerable velocity.

In the American Journal, Dr. Hare doubts if a whirlpool ever takes place without a centripetal force resulting from a *vacuity*. I see not how this doubt can militate against my views of vortical action; but I have myself seen many hundreds of such whirlpools or vortices, and have occasionally watched their developments with much interest.

After commenting on certain arbitrary conditions of "opposing or unequal forces" in the atmosphere, Dr. Hare desires to be informed how "unequal or opposing forces are generated in the atmosphere;" producing sometimes whirlwinds of unmeasured violence (19–20). It may be readily seen, that aërial currents of unequal temperature and velocity, superimposed one upon another, and all moving over a surface of unequal character and with frequent elevations, and subject also to the influence of adjacent currents, must often move *unequally* and in *unconformable directions*; thus unavoidably running, to some extent, into vortices, eddies or circuits, of various magnitudes and activity; some of which may occasionally become extended and spin on an upright and moving axis, with that violent and continued action which characterizes the tornado or water-spout. Indeed, it must be obvious, that uniformly direct lines of motion belong not to our atmosphere or system. But, as before observed, I have here no special concern with the origin of these or other vortices; the simple fact of their existence being all that is necessary for me to maintain.

Dr. Hare then proceeds to state, that in former papers on the causes of tornadoes he has adduced facts and arguments "tending to prove that the proximate cause of the phænomena of a tornado is an ascending current of air, and the afflux of wind from all points of the compass to supply the deficiency thus created." He also states, that " in this mode of viewing the phænomena, no difference of opinion exists between Bache, Espy and himself, however they may differ respecting the cause of the diminution of atmospheric pressure," &c. (21–22.)

I have no desire to offer strictures upon the views of a respected professor of science; but it seems proper here to inquire how an ascending current of air is thus obtained, and whether this *effect*, which perhaps may be due only to an excess of lateral and subjacent pressure on the exterior of the tornado, be not here adduced as the cause of the effect.

Dr. Hare has been " led to consider gyration as a contingent, not an essential feature" in tornadoes, and he adduces the dislocation and partial turning of a chimney top on its base, in the New Brunswick tornado, as being due to a *local* and not a *general* whirl in the body of the tornado, and proving " that in tornadoes and hurricanes there are local whirls." (23–24.)

I have long since ascertained that local whirlwinds are not of very rare occurrence in great whirlwind storms; the New Brunswick tornado itself having been one of several violent local whirlwinds which occurred within the limits of a somewhat remarkable storm of the above character. This tornado also sent off a duplicate vortex or whirl not long after its passing the Raritan, the path and violent effects of both whirlwinds having been distinctly traced on a field of unripe grain; the smaller one branching off to the right of the main track, where, after causing some prostrations, it passed into the Raritan marshes, and was no more seen. But the whirling motion, so far from being only of " casual " and limited occurrence, appears to be a constant attribute of the tornado, though not always exhibited with uniform intensity and effect in its path; owing apparently to the frequent rising or narrowing of the vortex, and perhaps other causes.

Dr. Hare next says, " A fact which is admitted by Mr. Redfield, was considered by Espy and Bache, as well as myself, to be irreconcilable with the idea that a general whirling motion is essential to tornadoes. I allude to the circumstance, that when several trees were prostrated one upon the other, the uppermost was found to have fallen with the top directed towards the point towards which the meteor was moving; while the direction in which the lowermost trees were found to have fallen indicated that they were overthrown by a force in a direction precisely the opposite of that which had operated upon those above-mentioned." (24).

It is an error to allege that I have "admitted" a fact such as is here stated. On the contrary, in careful explorations made on foot, through an aggregate extent of more than fifty miles of the tracks of various tornadoes, I have never met with such "a fact," or combination of facts, as Dr. Hare describes. In all the cases I have met with in which trees have fallen one upon another, if their tops pointed in opposite or nearly opposite directions, these directions *have never been parallel to the course pursued by the tornado,* but always in directions more or less transverse to the same; and I consider the opposing allegation as one of the chief errors of my opponents.

The trees which have fallen in directions which are more or less *backward* from the course pursued by the tornado, are almost invariably found *on the left side of the track exterior to the line of its axis.* But few of these point directly backward, and still fewer can be found near the axis, as the hypothesis of my opponents requires. Of the trees found with their tops pointing *directly forward,* or nearly so, a small number have been seen on or near *the right margin of the track,* with appearances which showed them to lie as they first fell; a fact which seems equally fatal to their hypothesis. Some trees, along and near the line of the axis, are, however, found pointing in this onward direction, and much stress is laid on *this* fact by one of my opponents: but it appears, on examination, that in all these cases the trees *have been torn or twisted from the transverse position in which they first fell*; owing, as I infer, to the more violent force exhibited at and immediately behind the centre of the whirl, or at the point which may not inaptly be termed the *heel of the vortex**.

It is true, however, as I *have* "admitted," that when trees are found to have fallen one upon another, the top of the uppermost tree points in a direction *more* onward than the one beneath; as is seen by the diagrams and schedules of Professor Bache, and as may be inferred, perhaps, from the sketches given by Professors Olmstead and Loomis †: and it is *equally* true, that this fact no more favours the hypothesis of a directly inward motion, than that of a whirlwind being, as an abstract deduction, "reconcilable" with either. The proper generalization of this class of facts I have attempted to give in my paper on the New Brunswick tornado; which is, "that the uppermost or last fallen of these trees points *most* [or more] nearly to the course pursued by the tornado;" *i. e.* more nearly than the underlying tree which fell first; diver-

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* See this Journal, January 1841, p. 20–29, and map.

† See Silliman's American Journal, vol. xxxiii. p. 369; vol. xxxvii. p. 343.

gence from the course of the tornado being still a marked feature of these overlying prostrations.

I have never found a directly backward prostration on the line of the centre, or axis of a tornado. This, and the above-mentioned facts, will be found sufficiently "irreconcilable" with a direct "afflux of the wind from all points of the compass," "in a central and non-whirling course," "towards a common focal area."

Dr. Hare "cannot understand how the opposite forces belonging respectively to the different sides of the whirlwind, can be made to bear successively *upon one spot*, so as to cause trees to fall in diametrically opposite directions." (25.) Neither can I understand this, if each of these "opposite directions" *be parallel to the course or track of the tornado!* as is alleged by Dr. Hare in the passage last noticed.

We are next informed, that "Another fact, irreconcilable with a general whirling motion, was adduced by Messrs. Espy and Bache. One of the four posts, upon which a frame building was supported, was first moved towards the tornado, as it advanced; in the next place as it moved away, so as to make two furrows in the ground. In the interim the frame was protected by a larger building, which intervened between it and the tornado. I am utterly unable to understand how the transient tangential forces of a whirlwind blowing oppositely, on the opposite margins of its track, could thus move the post in question, so as to make two distinct furrows in the ground indicating two successive impulses, in directions of which one was at right angles with the other." (26.)

Dr. Hare here alleges that one of the posts "*was first moved towards the tornado*, as it advanced;" but I have been called to answer this case in another shape, in the American Journal; and Prof. Bache, on whose descriptions he doubtless relies, shows us that the tornado advanced from south 80° west, to north 80° east; and that the post was first moved "*to the west of north.*"

But on what grounds this "fact" is pronounced "irreconcilable with a general whirling motion," I am wholly unable to perceive. For, had he closely examined the whole case, he would hardly have failed to see that the movements of this building, as described by Professor Bache, are fully "reconcilable" to an involute "whirling motion," such as I allege to be characteristic of these tornadoes; and that there was no necessity for resorting to the gratuitous hypothesis of its being "protected by a larger building," or even that of "the suction of the tornado," as alleged in the American Journal.

If a whirlwind figure having a diameter of three or four hundred yards by the scale of Professor Bache's figure, (plate iii. fig. 3*) be drawn on tracing paper, with involute whirling lines representing, horizontally, the course of the wind from the exterior to the interior of the tornado, and if the centre or axis of this figure be passed from west to east along the line pursued by the axis of the tornado as indicated on the plate, revolving at the same time to the left with a velocity greatly exceeding its advancing motion, it may be seen that the wind of the whirl will be indicated as beginning at this building from nearly south, *i. e.* moving "to the west of north," nearly, or in the general direction of the first furrows in the ground. It will also be seen, that the wind of the whirl, changing by south-west, and having its gyrations quickened near the centre, would, immediately after the passing of its axis, exhibit its greatest force from the western quarter, corresponding to the second movement of the posts in the ground; the wind veering from thence towards the north-west as the tornado passed away: thus showing two directions of wind which sufficiently coincide with the two movements of the posts of the building "to the west of north," and subsequently "to the eastward," or "nearly at right angles" to its first course, according to the descriptions and plan of Professor Bache, who gives the course of the axis as "east 10° N.," *the building being to the southward or on the right of this line.*

I say nothing here of the protection afforded by another building, which after the first moment, according to the hypothesis of motion adopted by Messrs. Espy and Hare, was constantly more or less to *leeward* of the building so protected. By applying to Prof. Bache's plan, as before, a compass card, moved from west to east without revolving, we shall find their wind to commence nearly at east, passing thence through south to south-west, and possibly to west-south-west, near which it would terminate. Thus the first effects of the wind, when, even upon the hypothesis of "suction," the building was unprotected, could not produce the first motion in the direction "to the west of north," which perhaps may be fairly taken at 5° or 10° west of north; and the wind, on their hypothesis, would hardly appear to have reached a point which could produce the second movement "to the east."

I have been thus particular in this examination, because the case thus alleged by Dr. Hare is a further specimen of the erroneous inductions which have been made and relied on by my opponents. In examining the plans referred to,

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* See Journal of the Franklin Institute, vol. iii. third series, 1841, pp. 273 and 276; also American Phil. Trans., vol. v.

it should be observed, that the sketch of prostrations in the orchard, which is included in fig. 3, is evidently on a more reduced scale than that given in the plan of the building; otherwise the buildings must be of size sufficient nearly to have covered the orchard. This change of scale may cause some confusion unless particularly noticed.

That the velocity and consequent force of the whirling movement of the tornado is maintained by the direct *pressure* of the surrounding atmosphere, rather than by the "suction" which has been alleged by Dr. Hare, I can readily conceive; but that the "transient forces of a whirlwind" of this character are generally found to be "tangential" to its axis, which he seems to consider a necessary condition, I do not admit.

Dr. Hare appears to concede, that my survey of this tornado shows effects which accord with whirlwind action; but he seems desirous of limiting this admission to the prostration of "certain trees," and alleges that this survey "does not demonstrate gyration to be an essential feature of tornadoes;" and that "it is sufficiently accounted for by considering it as a fortuitous consequence of the conflux of currents rushing into a space partially exhausted." (26.)

Now I cannot but think, that readers who have no theory to support, will view the results of my survey in a very different light. Dr. Hare omits to mention, that the survey comprised the entire breadth of the visible track, at perhaps its broadest place; that it was intended to include every tree prostrated within its limits; that it essentially agrees with the main features of the more partial surveys of Professor Bache; that I have shown by clear inductions from all the prostrations in the survey that the whirling motion was one general effect, comprising the entire width of the track; that the tornado must have arrived at this ground in nearly its most perfect action, having just left the surface of the Raritan river; that the axis of prostration was not found in the centre of the track, but nearest its left margin; that the main rotation was wholly to the left, or in one constant direction; and that the leading features of the prostration found in this survey, have also been observed as constantly occurring in the tracks of many other tornadoes*.

I may add, that in a careful exploration of the track of this tornado for several miles, I found nothing to contravene the results of my published survey; the general features of the prostration being greatly analogous to those which I have given.

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* See this Journal, January 1841, p. 17–29, and map. Journ. Frank. Instit. vol. ii. third series, p. 40–49.

Dr. Hare thinks it singular, that I should have declined noticing the "insuperable difficulties" of the hypothesis of "a central and non-whirling course in the wind of the tornado," to which I have alluded in bringing forward facts and inductions which seem to contravene this hypothesis. He states, also, that "the advocates of the disputed hypothesis are not aware of any such difficulties," and intimates the propriety of the allusion "without naming the facts and arguments" which justify it. (27.)

I consider it more proper, however, to rely solely on the survey and inductions which I then presented; as these appear sufficient to set aside, not only the hypothesis itself, but also some of the chief deductions from the phænomena of this tornado which have been put forth and relied on by Mr. Espy and Dr. Hare*. Besides, I had no wish to assume a controversial attitude, in assailing by argument an hypothesis which virtually discards the observations of mankind in all past ages down to the year 1835. The testimonials of these observations appear in the names and terms applied by all people in all languages to this small but violent class of storms. "The facts" demanded, I had supposed, were furnished on that occasion in sufficient numbers.

Dr. Hare next adduces "the statement of a most respectable witness, that while the tornado at Providence was crossing the river, the water, which had risen up as if boiling within a circle of about three hundred feet, subsided as often as a flash of lightning took place;" which he alleges to be a "fact which is utterly irreconcilable with Mr. Redfield's 'rotary theory.'" He adds, "Now supposing the water to have risen by a deficit of pressure resulting from the centrifugal force of a whirl, how could an electric discharge cause it to subside?" (27.)

For the supposition here made, as well as for "the water which had risen up," Dr. Hare seems alone accountable; as his witness, Mr. Allen, speaks only of "the effervescence produced by the tornado in the water" having "perceptibly abated." The water he states to have been "in commotion like that in a huge boiling cauldron;" but *that which rose up* from the surface he describes as "misty vapours resembling steam," which "after the flash seemed sensibly to diminish for a moment†." I cannot perceive that the fact thus alleged has the least unfavourable bearing upon my views of rotative action. Therefore, without considering the optical effect

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* See Journal of the Franklin Institute, vol. xx. new series, 1837, p. 56–61; also vol. ii. third series, 1841, p. 356–359.

† See Silliman's Journal, vol. xxxviii. p. 76; also Mr. Allen's letter in this Journal for December 1841, p. 430.

which may result from a flash of lightning, or the immediate conversion of clouded vapour into rain, which oftentimes suddenly follows, I will only state, that another competent observer, who was very near this whirlwind when it left the western shore, and who watched its progress across the river, has described to me the appearance of the cloudy sprays or mists blown from the surface of the water, and which filled the lower extremity of the tornado, but he has mentioned no sudden disappearances of the same. He did, however, observe the *whirling action* of the tornado with great distinctness, both when it first entered upon the river, and in its effects upon the sails and position of a schooner with which it came in contact; and likewise, as exhibited by the circling or whirling directions of the various objects carried into the air, as it came off the high grounds on its approach to the river. The highly intelligent eye-witness of my opponent also describes "the misty vapours" as "*entering the* WHIRLING VORTEX;" thus showing, from his own observation, a fact which fully supports my views, and is fatal to the objections and hypothesis of motion set forth by Dr. Hare. Moreover, there were decisive memorials of a general whirling action found along the path of this tornado.

Dr. Hare chooses also to say, "I have already, I trust, sufficiently shown that the abortive explanation which Mr. Redfield dignifies with the title of his 'theory of rotary storms,' amounts to no more than this; that certain imaginary, nondescript, unequal and opposing forces produce atmospheric gyration; that these gyrations, by their consequent centrifugal force, create about the axis of motion a deficit of pressure; and hence the upward force displayed by tornadoes and hurricanes. I cannot give to this alleged theory the smallest importance, while the unequal and opposing forces upon which it is built 'remain in perfect obscurity,' or, according to his American version, 'exist only in the imagination of an author who disclaims the agency either of heat or electricity.'" —(28, Amer. Journ. of Science, vol. xlii. p. 145.)

This recital appears necessary, on account of the error into which Dr. Hare has here fallen. I have never attempted to dignify any "explanation," induction, sketch, or essay, "with the title" of my "theory of rotary storms." It must, at least, have been a mistake of person. I have little fondness for theory-making; and as little respect for hypotheses of winds or storms, other than those which result directly from sufficient and reliable observations. Neither have I disclaimed "the agency of heat," as already stated; but it may have been my offence to have disclaimed "electricity" as a known cause of storms. My cursory explanations of the action of a whirlwind or tornado, even as shown up by Dr. Hare, are, in my view, better suited to the observed facts of the case than any which he or Mr. Espy has offered.

I do not solicit for my views even that "smallest importance" which is denied them in the mind of my critic; but the attention with which he has treated them, on both sides the Atlantic, does not appear to agree well with the disavowment. With the facts before him which are shown in my survey of the tornado, and also with the numerous observations made in great storms, which I have published, it is vain to pretend that my views of their rotation are either "abortive," or founded only in imagination. I am not conscious of having "built" or indicated any "theory," views, suggestions or explanations of storms or whirlwinds which have not been based on observations of my own and facts otherwise ascertained, sufficient in my view to warrant them, the "unequal and opposing forces" even included; although I have not always urged these facts upon the attention of my readers, having not unfrequently reserved them for more appropriate occasions. Hence my alleged proofs have been chiefly confined to the progressive course and rotative action developed in storms, which last, strangely enough, has been so pertinaciously denied by Mr. Espy, and now by Dr. Hare.

My opponent next attempts to show, that "a deficit of pressure about the axis" of a whirlwind "consequent to the resulting centrifugal force, could only cause a descending aërial current, while it could not tend in the slightest degree to carry solids or liquids aloft." (29–30.) I was also surprised to find this hypothetical downward current in the midst of a whirlwind alleged as a necessary condition, on former occasions, by Mr. Espy. If the allegation be true, it must be easy to show that the ascending currents in chimneys should become inverted; for, so far as simple gravitation is concerned, it can make little difference whether the rarefaction be mechanical or calorific.

But the ascending effects in the interior of a whirlwind have been too often witnessed by myself and others to require discussion. Indeed, it would almost seem that the objectors had been precluded from all opportunities for correct observation. There are numerous cases, however, in which the upward movement of the objects elevated cannot be seen in the central and lower parts of the whirlwind, owing, as I have had good occasion to know, to the great angular velocity of the central gyrations.

Dr. Hare appears to suppose, that gyration in a revolving "matter" or "fluid mass" will not quicken as it approaches the centre, unless as resulting from a centripetal force "caused by suction at the axis." (31–32, Amer. Journ. xlii. p. 146.)

A constant centripetal *force* I have already recognised on this as well as former occasions. But this by no means requires or produces a direct centripetal *course* in the moving air which yields to its influence. But in the cause assigned for this force, as well as in the specific directions of the movements produced, we differ essentially. So far from ascribing this quickened gyration to the "suction" alleged by Dr. Hare, I know of no such power in the uninclosed atmosphere, conceiving that neither rarefaction nor any other known cause can here occasion "suction," according to the common use of this term. Air, whether rarefied or not, can never ascend but in obedience to a *pressure* or *force*, sufficient to exceed both its own weight and that of all the atmosphere which lies immediately above it, or in the immediate direction or locality of its motion. This erroneous hypothesis of "suction," in some form or other, appears to lie at the bottom of the various speculations and inductions of my opponents.

In noticing the spirally involute and quickening motion which I allege as observable in "all narrow and violent vortices," Dr. Hare gives in the American Journal an erroneous reference for his quotation; and the latter seems also to be somewhat inaccurate. I do not see that his speculations on this quickened motion "towards the centre or axis of the whirl*," can affect either my views or the disputed fact of gyration; and they are sufficiently answered by observations published in my first paper†, as well as by the remarks made above on centripetal force.

Dr. Hare thinks that so far as my observations show the quickening of the whirling motion towards the centre of the tornado, they tend to confirm the views of my opponents and to refute those which I uphold. To me it appears that this is an entire abandonment of his ground. It is the general fact of gyration which I am chiefly concerned to uphold, and which has been combated by him and his predecessor in this controversy,. I dispute with no one as to how it may be produced. Should better explanations of this fact than mine be offered, they will be cheerfully adopted. In the mean time, I shall adhere to my observations and opinions, rather than to the hypotheses and speculations of my opponents.

Dr. Hare thinks it will be conceded, "that any theory of

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* Not "the centre of the axis of the whirl," as erroneously quoted by Dr. Hare.

† Silliman's Journal, vol. xx. pp. 45–46.

storms which overlooks the part performed by electricity, must be extremely defective." I do not perceive that the part performed by electricity in a gale of wind, squall, tornado, or other storm, ever constitutes an essential feature of the same; but the part so performed appears to me to be only incidental and subordinate to the action and main effects of the storm. Electricity is not wind, nor water, nor vapour, but an imponderable matter or effect, which is not known to exert any constant mechanical force or action upon the efficient currents of the atmosphere. "Thunder and lightning, and convective discharge," are but momentary or transient exhibitions of electricity, producing no visible effects upon these currents, whatever may be their agency in restoring the disturbed equilibrium of the different atmospheric elements. The electricity developed by a steam boiler is not considered as producing the steam or its jet, or the condensation of the latter, but is itself produced by these. Even were it shown that a stream of electricity was constantly developed between the rarefied column of a moving tornado and the surface beneath, I cannot see how this could be assumed as the *cause* rather than the *effect* of the local rarefaction. If the part which electricity performs in a storm be essential, or controlling, its functions ought to be distinctly pointed out.

I would humbly suggest that the old practice of forming or inventing theories or schemes of action for the powers of nature, ought to be mainly abandoned. The Wernerian and Huttonian theories are well remembered; and how small would have been the progress of the science to which they relate, had its cultivators continued to exhibit only the spirit and philosophy of the early advocates of these theories; and how much less, if guided by a philosophy so speculative and untenable as that of the affluent and up-moving hypotheses of winds and storms! More strict and extended observations and inquiry, with more caution in the adoption of hypotheses, whether old or new, would, in my opinion, tend greatly to the advance of meteorological science.

Observation, rather than "lucubration," has been my employment when exempted from other duties; and if the results of observation do not accord with the "lucubrations" of Messrs. Espy and Hare, I conceive that I am in no degree responsible for the difficulties of their position.

New York, January 20, 1842.

LIII. *On the Motion of Luminous Waves in an Elastic Medium, consisting of a system of detached particles, separated by finite intervals.* *By* S. Earnshaw, *M.A., of St. John's College, Cambridge*[*].

I RECEIVE with distrust results on the subject of the transmission of waves of light through a medium of detached particles, when those results are derived from investigations based in any degree upon an assumed *geometrical* symmetry in the arrangement of the *molecules* of the medium. That there exists in nature a *mechanical* symmetry amongst the *forces* which regulate the motions of the particles of the luminiferous medium, is proved by innumerable experiments: for, in those media which are denominated uncrystallized, the forces are symmetrical with respect to every direction; in those which are denominated uniaxal the forces are symmetrical with respect to a certain line, and to all lines at right angles to it; and in biaxal media there exist three rectangular directions, with respect to which there is mechanical symmetry. Now if we consider the circumstances of the last two classes of media, we shall perceive that the directions of mechanical symmetry are very rarely directions of geometrical symmetry; and that even when there are no lines of geometrical symmetry, there yet exist three lines of mechanical symmetry. With these facts before me, well established by almost innumerable experiments, I cannot read with satisfaction the researches of those mathematical philosophers who make an assumed (may I not even say an *unfounded*?) geometrical symmetry the basis of their investigations. If we inquire into the reason of this fondness among writers on the subject of physical optics for this imaginary geometrical symmetry, we shall find that it is forced upon them by the complex nature of the equations which present themselves in their investigations; and it is used as a means of simplification, without which there seems to be no hope of obtaining any result at all. I shall show presently that simple results can be obtained, when the investigations are conducted on proper hypotheses, warranted by experiment, without the assistance of this means of simplification.

I am not prepared to deny that, to a certain extent, geometrical symmetry may secure mechanical symmetry; but, as remarked above, experiment shows that the latter exists in every system whether the former exist or not; and there-

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[*] Communicated by the Author.

fore it appears that if we would inquire into the secrets of optical refraction as they exist in nature, we must be careful not to make geometrical symmetry a necessary condition in our investigations. If we neglect to observe this caution, we can never be sure that some of our results may not be due to our geometrical assumption: and here, perhaps, I may be allowed to state, *en passant*, that after carefully weighing what has been done within the last few years on the theory of the transmission and refraction of light, I cannot avoid suspecting that a long list of theoretical results which have been obtained is due to the assumption of geometrical symmetry and the misinterpretation of analytical expressions.

In a paper printed in the Cambridge Philosophical Transactions, vol. vii., I have shown by a very simple analysis that there exist for each particle three rectangular directions of mechanical symmetry in every system of detached particles, whether those particles be arranged in geometrical symmetry or not. Under certain conditions the directions of mechanical symmetry are parallel for all particles. Now we learn from experiment that these conditions are fulfilled in nature. Again, experiment shows that the superposition of undulations is an optical principle which exists in nature: on the authority of this fact, I shall neglect in the equations of motion all powers of the displacements of the particles above the first. As I shall avoid any assumption based on geometrical symmetry, I shall consider my results as belonging equally to crystallized and non-crystallized media. For simplicity, I shall consider only the transmission of plane waves through the medium in any direction.

Let $x\,y\,z$, $x'\,y'\,z'$ be the coordinates of the positions of rest of an attracted particle m, and any attracting particle m'; and let r' be their distance. At any time t let $x+\xi$, $y+\eta$, $z+\zeta$, and $x'+\xi'$, $y'+\eta'$, $z'+\zeta'$ be the coordinates, and R' the distance of the same particles. Also let the law of attraction of m' upon m be represented by $m'f(\mathrm{R}')$; and assume F to be such a function that $\mathrm{F}(u)=\int f(u)\,d\,u$. Then the force exerted by m' on m parallel to the axis of x, at the time t,

$$=m'f(\mathrm{R}')\cdot\frac{x'+\xi'-x-\xi}{\mathrm{R}'}=-m'f(\mathrm{R}')\cdot\frac{d\,\mathrm{R}'}{d\,x}=-m'\,\frac{d\,\mathrm{F}\,(\mathrm{R}')}{d\,x}.$$

Now since $\mathrm{R}^2=(x'-x)^2+(y'-y)^2+(z'-z)^2$, and $\mathrm{R}'^2=(x'+\xi'-x-\xi)^2+(y'+\eta'-y-\eta)^2+(z'+\zeta'-z-\zeta)^2$, it is evident R' is derived from R by writing $x+\xi-\xi'$, $y+\eta-\eta'$, $z+\zeta-\zeta'$ for x, y, z: consequently, by Taylor's theorem,

$$\frac{d\,\mathrm{F}(\mathrm{R}')}{d\,x} = \frac{d\,\mathrm{F}(\mathrm{R})}{d\,x} + \frac{d^2\,\mathrm{F}(\mathrm{R})}{d\,x^2}(\xi - \xi') + \frac{d^2\,\mathrm{F}(\mathrm{R})}{d\,x\,d\,y}(\eta - \eta') + \frac{d^2\,\mathrm{F}(\mathrm{R})}{d\,x\,d\,z}(\zeta - \zeta').$$

Hence

$$\frac{d^2\,\xi}{d\,t^2} = -\Sigma\left(m'\frac{d\,\mathrm{F}(\mathrm{R})}{d\,x}\right) - \Sigma\left(m'\frac{d^2\,\mathrm{F}(\mathrm{R})}{d\,x^2}\,.\,\overline{\xi - \xi'}\right) - \Sigma\left(m'\frac{d^2\,\mathrm{F}(\mathrm{R})}{d\,x\,d\,y}\,.\,\overline{\eta - \eta'}\right) - \Sigma\left(m'\frac{d^2\,\mathrm{F}(\mathrm{R})}{d\,x\,d\,z}\,.\,\overline{\zeta - \zeta'}\right).$$

Now the first term of the right-hand member of this equation is zero, because it refers to the equilibrium position of the medium. With respect to the next term, we observe that $\xi - \xi'$ has the same value for every particle situated in the same wave surface, though its value is not the same for particles which are in different wave surfaces, except those surfaces be distant from each other by an exact wave's length. We see also that the particles of the wave surface in which m is situated exert no influence upon m. Let us, then, setting out from m and passing over a wave's length, number the wave surfaces 1, 2, 3, ... in order. Denote by A_r the value of $\Sigma\left(m'\frac{d^2\,\mathrm{F}(\mathrm{R})}{d\,x^2}\right)$ for all particles which are in the rth wave surface, and in all other surfaces through the whole medium distant from the rth by multiples of a wave's length. Then

$$\Sigma\left(m'\frac{d^2\,\mathrm{F}(\mathrm{R})}{d\,x^2}\,.\,\overline{\xi - \xi'}\right) = \Sigma\left(\mathrm{A}_r\,.\,\overline{\xi - \xi_r}\right),$$

the symbol Σ now referring to summation for all values of r. I shall now assume the law of displacement at the time t to be

$$\xi_r = a \sin(r\,h + \mathrm{T}),$$

T being an unknown function of t, and h a constant depending upon the nature of the medium. I am borne out in assuming this law by experiment; but if it be objected to as not sufficiently general, it will be understood that what follows applies only to media in which this law of disturbance can be transmitted. From this equation we find $\xi = a \sin \mathrm{T}$; and therefore $\xi - \xi_r = 2\,a \sin^2\frac{r\,h}{2}\sin\mathrm{T} - a\sin r\,h\cos\mathrm{T}$

$= 2\sin^2\frac{r\,h}{2}\,.\,\xi - \sin r\,h\sqrt{a^2 - \xi^2}$; and consequently

$$\Sigma\,(\mathrm{A}_r\,.\,\overline{\xi-\xi_r}) = 2\,\Sigma\left(\mathrm{A}_r\ \sin^2\frac{r\,h}{2}\right).\,\xi = \mathrm{A}\,\xi.$$

I omit the part involving $\sqrt{a^2-\xi^2}$, because it is not linear with regard to ξ. In a similar manner we may put

$$\Sigma\left(m'\,\frac{d^2\,\mathrm{F}\,(\mathrm{R})}{d\,x\,d\,y}\,.\,\overline{\eta-\eta'}\right) = \mathrm{F}\,\eta,$$

and

$$\Sigma\left(m'\,\frac{d^2\,\mathrm{F}\,(\mathrm{R})}{d\,x\,d\,z}\,.\,\overline{\zeta-\zeta'}\right) = \mathrm{E}\,\zeta.$$

And proceeding in the same way to obtain the equations of motion with regard to the forces respectively parallel to the axes of y and z, we finally obtain,

$$d_t^2\,\xi = -\,\mathrm{A}\,\xi - \mathrm{F}\,\eta - \mathrm{E}\,\zeta,$$
$$d_t^2\,\eta = -\,\mathrm{F}\,\xi - \mathrm{B}\,\eta - \mathrm{D}\,\zeta,$$
$$d_t^2\,\zeta = -\,\mathrm{E}\,\xi - \mathrm{D}\,\eta - \mathrm{C}\,\zeta.$$

These then are the general equations for the transmission of common light through any transparent medium whatever. As I have already occupied a considerable portion of your valuable space, I shall reserve my inferences from these equations for a future Number of your Journal.

March 12, 1842.

LIV. *Remarks on a paper by* Mr. O'Brien *relative to the application of the Undulatory Theory to the explanation of Dispersion. By the Rev.* P. Kelland, *M.A., F.R.SS. L. & E., F.C.P.S., &c., Professor of Mathematics in the University of Edinburgh, late Fellow and Tutor of Queen's College, Cambridge.*

To Richard Taylor, Esq.

My dear Sir,

WILL you do me the favour to insert in your Magazine a few remarks on a paper which has just appeared (Phil. Mag. for March, page 201), the object of which is to investigate the influence which material particles exert on the vibrations of the molecules of æther? The result of the investigation is the establishment of a relation between the velocity of transmission and the length of a wave of light, without introducing the hypothesis of a finite ratio between the mutual distance of two consecutive particles of the luminiferous æther, and the length of an undulation. I have hitherto abstained from taking a part in the discussions which have arisen since the appearance of my publication on the subject, from the feeling that I could add little to the impetus which research had acquired in the able hands of Professor Powell and others, and from the firmest reliance on their zeal and desire for the advancement, not of their own views, but of science itself. The appearance, however, of a new writer in the field, whilst I hail it with real pleasure, compels me to take my part, if not in what is yet to be done, at least in keeping alive a due attention to what at present has been effected. I confess, too, that I am further influenced to the present step, by the words with which your correspondent closes his paper:—"I believe also that the result of this second approximation may be made use of to prove that the hypothesis of finite intervals cannot be correct." I shall not, however, digress from my path to anticipate what may be the weight of the forthcoming objection to the hypothesis of finite intervals, but shall confine myself strictly to the following points:—

1. To state what is the precise hypothesis I have myself admitted, and refer to the conclusions based on it.
2. To examine what is Mr. O'Brien's hypothesis; and
3. To compare his results with those previously arrived at.

1. In my memoir on the 'Dispersion of Light' (Trans. Camb. Phil. Soc., vol. vi. p. 157), the conclusions and calculations are based on the following suppositions,—1st, that the distance between the particles of æther is sufficiently large, compared with their difference of motion, to allow the square of the latter quantity to be neglected in comparison with that of the former; 2ndly, that the same distance bears an appreciable ratio to the length of a wave; and 3rdly, that the medium is one of *perfect* symmetry. These hypotheses are conformable with the views of Cauchy and others, and may be considered as the simplest form of the theory of finite intervals. From a conviction of the utter impossibility of an arrangement of perfect symmetry, I was led to adopt, subsequently, the notion of a disposition of particles similar to that deduced by M. Mossotti, which is shown in Mr. O'Brien's figure 4. (See Scientific Memoirs, vol. i. p. 448.) It is true I did not succeed in proving that the conditions resulting from such an arrangement are the same as those which depend on the supposition of perfect symmetry. Mr. O'Brien proposes to do this, and if he succeeds, it will, I am sure, be an important step in our theoretical investigations. In my first essay on Dispersion, the presence of material particles was taken no account of. The exhibition of the results modified on this account, was reserved to a subsequent memoir, to which I shall refer in the sequel. It will be sufficient for my

present purpose that I direct attention simply to that form of the hypothesis which I gave in my treatise on 'The Theory of Heat.' From the difficulty of accounting for the want of dispersion in a vacuum, I was led to attribute to the immediate presence of the material particles, an influence on the velocity of transmission, exerted in a two-fold manner:—1. by their vibrations along with the particles of æther: 2. by the actions which they put in play, or rather (which is really the same thing) exclude, through the particles of æther which they displace (Arts. 161, 162).

Of the two hypotheses now mentioned, the former leads to a very satisfactory explanation of the phænomena of dispersion, provided the difficulty which is alluded to can be got over. That this can be done, M. Cauchy endeavours to show by calculations based on a certain assumed law of force. But, even if it cannot, the latter hypothesis, which arises out of the former, leads very simply to the desired conclusion, inasmuch as it introduces into our equations of motion a term depending on the action of the æther which would occupy the place of a material particle, in *addition* to other terms similar to those which apply to a vacuum. It happens that the *form* of the result deduced from the second hypothesis differs in no material respect from that which arises out of the first (Theory of Heat, p. 154).

2. We shall be able to ascertain the exact nature of Mr. O'Brien's hypothesis, as well by an examination of the equations which result from the supposition that the particles of matter are absolutely fixed, as from the more general equations. We are desirous, indeed, of avoiding all incidental remarks relative to the correctness of the equations themselves, and this we could not do at present in regard to the more general ones, nor, in fact, can we admit anything concerning these equations until the legitimacy of the hypothesis, that $\alpha_{,}$, $\beta_{,}$, $\gamma_{,}$ may be regarded as independent of the sign Σ, has been proved. It is evident. that, except in this particular, the equations differ from those which I deduced on the same hypothesis (with a limitation) in the Transactions of the Cambridge Phil. Soc., vol. vi. pp. 239, 245, &c., only in appearance. But the results with which we are concerned, are those which appear at the top of p. 210 of Mr. O'Brien's paper. Now it will readily appear to those conversant with the subject, that these equations differ from the ordinary equations of motion of a vibrating system, only in having the additional terms $-C\alpha$, $-C\beta$, $-C\gamma$. The other parts of the expressions for the forces are identical with those always given in similar cases; and they will be found at p. 135, vol. iv. of

Cauchy's *Exercices*, for by turning to the following page we find that equations (26.) (which give the values of the constants) render equations (23.) in Mr. O'Brien's notation the following:—

$$\frac{d^2\alpha}{dt^2} = (3\,\mathrm{R}+\mathrm{G})\frac{d^2\alpha}{dx^2} + (\mathrm{R}+\mathrm{G})\left(\frac{d^3\alpha}{dy^2} + \frac{d^2\alpha}{dz^2}\right)$$
$$+ 2\,\mathrm{R}\left(\frac{d^2\beta}{dx\,dy} + \frac{d^2\gamma}{dx\,dz}\right).$$

Thus the difference between the medium and a common elastic medium is only this, that the particles of æther are attracted by the particles of matter. Now the same *hypothesis* precisely is made by myself in my memoir 'On the Motion of a System of Particles,' p. 244. But as I have adopted the law of force varying inversely as the square of the distance, the quantity C vanishes. The effect of such a term, however, I felt clearly to be needed, and accordingly, I restored it in my 'Theory of Heat', p. 153, by the consideration stated above. The equation which is there obtained is nearly the same as one of Mr. O'Brien's equations, and the conclusion from it appears to me the very same, in form at least, as his. The expression which I obtained for the square of the velocity of transmission is this:—

$$v^2 = \frac{\mathrm{D}'}{\mathrm{D}}\mathrm{V}^2 + 2(\mathrm{Q}-\mathrm{M})\,\Sigma\,.\,\mathrm{F}\,\frac{\sin^2\dfrac{k\Delta x}{2}}{k^2}.$$

3. But I proceed to examine Mr. O'Brien's results in order. The first is, that the velocity of propagation is in general *different for transversal and for direct vibrations.* Here I cannot help observing that it is much to be desired that an appearance of generality should be avoided whenever it can be done with propriety. By making the axis of x (which is arbitrary) that of transmission, the equations assume the form

$$\frac{d^2\alpha}{dt^2} = \mathrm{A}\frac{d^2\alpha}{dx^2} - \mathrm{C}\alpha$$
$$\frac{d^2\beta}{dt^2} = \mathrm{B}\frac{d^2\beta}{dx^2} - \mathrm{C}\beta$$
$$\frac{d^2\gamma}{dt^2} = \mathrm{B}\frac{d^2\gamma}{dx^2} - \mathrm{C}\gamma,$$

and the result is an interpretation of the difference of value of A and B. M. Cauchy arrives at the same conclusion in the *Comptes Rendus* for 1840, vol. x. p. 905: and, of course, he gets it in the same way. (See also *Exercices*, vol. v. p. 69.) It is not a little remarkable that M. Cauchy should have stated his conclusions very differently in other memoirs. For instance, in the *Comptes Rendus* for 1836, p. 343, he states that Fresnel is justified in saying that the vibrations of æthereal molecules generally lie in the planes of the waves; and to the same effect in the *Comptes Rendus* for 1838, p. 866. The fact is, that the whole depends on the law of force, as I have shown more than once. If the law be that of the inverse square of the distance, A is negative, and the vibrations are *transversal only.* (See my memoir on Dispersion, p. 180.)

The second result of Mr. O'Brien presents nothing which belongs to our present purpose. The third is, *that the dispersion of light may be completely accounted for, without having recourse to the hypothesis of finite intervals.* This result is referred to the hypothesis, that the material particles influence the vibrations of the æthereal ones by their attractions or repulsions.

I think I have shown clearly that the *principle* of the explanation has not been lost sight of by the advocates of the *interval* theory. The point at issue then is simply this, Will the *direct* attractions or repulsions of the particles of matter, symmetrically immersed in those of æther, affect the velocity of vibration of the latter? According to my views they will not; and consequently in my second hypothesis I confine my attention to their indirect effect. But that effect depends on their mutual distances, and thus *finite intervals*, not indeed of the particles of æther, but of those of matter, necessarily play a conspicuous part. Until, then, it shall have been *proved* "that the hypothesis of finite intervals cannot be correct," which I do not think can well be done by means of such assumptions as those on which the equations, adopted by Mr. O'Brien, depend (see p. 205, or Cauchy's *Ex.* vol. iii. p. 190), the real difference between the received theory and that before us is this; that the former rejects the direct attraction of the particles of matter as producing no effect on the time of vibration of a particle of æther, whilst it retains the *variation* of action due to the distance between the particles, either of one system or of the other; the latter retains the former and rejects the latter. Now as it has been shown (on the hypothesis of the Newtonian law) that the term which we reject is zero, it is incumbent on those who refuse to receive our conclusions, to show that what we retain is either zero or imaginary. Until this, or something equivalent, shall have been done, the former supporters of the theory of finite intervals will hardly be inclined to give it up.

I have the honour to be, dear Sir,
Your obliged Servant,
P. KELLAND.

P. S. It has been pointed out to me, that in my memoir on Dispersion, the calculations are affected with an error, in that I have neglected to shorten λ for the interior of the substances. The rectification of this error will change all the values of q. But as the formulæ are *of necessity* capable of fulfilling the conditions required of them, it does not appear necessary to re-calculate them.

Edinburgh, March 4, 1842.

LV. *On the Specific Gravity of Sulphuret of Nickel. By* W. H. Miller, *M.A., F.R.S., Professor of Mineralogy in the University of Cambridge**.

IN a description of the form of sulphuret of nickel (Haarkies) which appeared in the London and Edinburgh Philosophical Magazine for February 1835, I stated that the specific gravity of the mineral, obtained by weighing not quite two grains and a half of it in air and in water at 15° C., according to one observation was 5·28, according to another 5·262. Professor Breithaupt, at my request, repeated the observation with crystals from Johann-Georgenstadt and Kamsdorf, and in both cases obtained a specific gravity of 5·00 (Poggendorff's *Annalen*, Band li. S. 511). The difference between this and the former determination induced me to repeat the observations with the utmost care. About three grains and a half of the crystals, the whole quantity at my disposal fit for such a purpose, were inclosed in a light silver tube having small openings in it to permit the escape of the air. The tube was suspended in a vessel of distilled water which was kept boiling for some time. When the temperature had descended to 15° C. the apparent weight of the tube and crystals was observed. The crystals were then taken out and the apparent weight of the tube determined, using the same precaution as before. The balance employed was made by the late Mr. Robinson. With this instrument, the greatest error of any weighing does not amount to 1-2000th grain in air, or 1-200th grain in water. Three different observations gave for the specific gravity,

5·2774, 5·295, 5·277, at 15° Centigrade.

The mean of the five observations is 5·278. The very great difference between this result and 5·00, that obtained by Professor Breithaupt, renders it not improbable that the crystals he examined do not belong to the same species as those in my possession. The latter cleave very readily in three directions, normals to which make with each other angles of 35° 52′,

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* Communicated by the Author.

and an angle of 20° 50′ with the axis of the rhombohedron. They were found in cavities in nodules of the carbonate of iron used in the Iron-works at Dowlais, near Merthyr Tydvil.

Cambridge, Feb. 3, 1842. W. H. MILLER.

LVI. *On the Velocity of Propagation of Radiant Heat. By* Baron J. VON WREDE[*].

THIS investigation is founded on the principle, that if the heat and light in the solar rays move with different velocities, they must show an unequal aberration; and consequently the *luminous* and *thermal* images of the sun in a telescope cannot overlap each other completely, but must be separated in a direction parallel to the ecliptic. Hence the temperatures of the eastern and western edges of the sun's *luminous* image (i. e. *of the disc we view*) cannot be equal. In order to discover this difference, the author attached the following apparatus to the eye-end of a 10-foot telescope mounted parallactically, and which apparatus he laid before the physical section.

To one end of a brass tube inserted in the telescope he attached a four-sided brass box in such a manner, that its central line could, by means of a graduated circle, be placed at a certain measurable inclination to the plane of the declination-circle passing through the optical axis of the telescope. In this box he placed a small thermopile (*thermokette*) composed of bismuth and antimony bars, their points of verticality (*löthstellen*) being in one straight line, and in such a manner that the pile could be moved by means of a fine micrometer-screw in a direction perpendicular to its length. Parallel to the pile's line of verticality and in a plane perpendicular to the axis of the telescope, he fixed a spider's thread at a distance from the pile nearly equal to the sun's apparent diameter, and perpendicularly to this thread he fixed another which intersected the pile in about the middle of its length. The first thread may be called the vertical, and the other the horizontal one. To the back of the box a terrestrial eye-piece was fastened, so placed that the cross threads took up the middle of its field of view.

Before experimenting, the apparatus was thus adjusted. The horizontal thread was first made parallel to the ecliptic,

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[*] We are indebted for this article to S. M. Drach, Esq., F.R.A.S., by whom it has been translated from the German, as given in Poggendorff's *Annalen*, vol. liii. part 4. The original is a provisional account of the investigation given in the *Fordhandlinger ved de SkandinaviskeNaturforskeres an det Möde, der holdtes i Kjöbenhavn fra* 3 *bis* 9 *Juli* 1840.

then the pile was placed to the *east* of the telescopic axis, and the intersecting point of the two threads was brought over the western edge of the sun's disc. It follows from above, that in this position the eastern edge of the disc was a tangent to the line passing through the points of verticality of the pile.

In this position was the apparatus allowed to remain uninterruptedly for five minutes by the aid of a screw attached to the parallactic stand. The position of the galvanometer attached to the pile was next observed, and noted for every semi-oscillation*. The average of these thirty galvanometrical determinations gave, therefore, a measure of the temperature at the eastern edge of the sun's disc. The box was then turned round 180°, and the experiment continued in the same manner, only that now the eastern edge fell on the crossing threads, and the western edge touched the pile.

This experiment was repeated several times, the pile being after every time approximated one screw-length nearer to the vertical thread by turning the micrometer-screw once completely round. Hence each pair of such observations could independently of the others decide the question, whether heat and light possess an equal or a different velocity of propagation, and which of these is the greater.

To obtain a quantitative measure of the difference of the two velocities, it was necessary to compare all the observations together. By expressing through interpolation the temperatures of the eastern and western edges as a function of the distance from the solar image measured in turns of the micrometer-screw, it could hence be concluded how much more or less the screw ought to have been turned in order to obtain an equal temperature in both cases. The half of this measurement must therefore show how far the thermal is distant from the luminous image; and this magnitude reduced to its equivalent angle gives the difference of the thermal and luminous aberrations.

Such an inquiry needs of course a great number of observations in order to arrive at a satisfactory result. The author complained, that although the apparatus was mounted three weeks before his departure from Stockholm, he was prevented by unfavourable weather from making more than two sets of observations, and of which only one took place under quite favourable circumstances. In this set six pairs of observations were made with different positions of the screw, and by all these observations the temperature of the *eastern* edge of

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* This galvanometer has been noticed in vol. xlii. p. 308 of Poggendorff, note; the deviations of this instrument are observed by means of a mirror and a telescope.

the sun's disc was *higher* than that at the western edge. The regularity in the quantitative determinations by this one set made it extremely improbable that the observed difference of temperatures at the eastern and western edges were owing to errors of observation. By the other set (which consisted likewise of six pairs of observations, and whereat the telescope was reversed in order to eliminate that cause of error), the results were *without exception* in favour of the higher temperature of the eastern edge, although the quantitative determination did not exactly observe the same regularity that the first set did. But this regularity was not to be expected, partly owing to the sky being not free from small clouds, and partly by a stronger breeze causing sometimes an agitation of the apparatus. Although hitherto the requisite number of observations has not been made in order to solve the problem *completely*, yet the author thinks it extremely probable, from the already made experiments, that *heat* has a *greater aberration*, and consequently a *less velocity* than *light*.

The average result of these two sets gives the displacement of the thermal image of the sun from the luminous disc equal to 0·28 turns of the screw. As each of these = 1-119th Swedish decimal inches (ten to the foot? Tr.), and the focal distance of the telescope = 101·5 of the same inches, the difference of these aberrations $= \dfrac{0{\cdot}28}{119 \times 101{\cdot}5 \sin 1''} = 4''{\cdot}78$, whence the velocity of heat: that of light :: $20''{\cdot}25 : 25''{\cdot}03$ as 4 : 5 nearly. (153,000 English miles per second. Tr.).

Note by the Translator.—Although approving of the principle and method of these observations, yet before assenting to this numerical result, it would be advisable, I think, to test by a photometer the intensity of the solar light on each side of the sun's centre in the direction of a great circle of the sphere, or, as in this case, in the declination-circle for that day. The light will be found to be intense from the centre to the edge of the disc; but if beyond this the light sensibly diminishes in intensity with the elongation of the direction of the instrument from the sun's centre, the above-found result of the greater thermal intensity of the eastern edge only proves the thermal image to be to the east of the luminous one; but they may be distant some degrees apart with the same result. M. Wrede has however asserted that he has found the two points which possess equal temperatures: it is much to be regretted that this otherwise valuable paper does not contain the numerical measurements and the method of interpolation, which would have at once removed or confirmed the translator's doubts. The displacement of the *thermal* prismatic spectrum, compared with the *luminous* one, will likewise give the difference of aberrations, if any, by means of a residual part which the refraction of the heat-rays considered as coming from the luminous centre will not account for. For this purpose the thermal experiments ought to be continued for some time *after* the sun has passed the aperture in the window-shutter, and the thermometers or galvanometers ought to be arranged parallel to the ecliptic.

London, Jan. 2, 1842. S. M. D.

LVII. *On the Radical of the Cacodyl Series of Compounds. By Professor* Bunsen *of Marburg*[*].

1. *Isolation of Cacodyl.*

SOME of the cacodyl compounds have the remarkable property of being decomposed by metals. When sulphuret of cacodyl is heated in contact with mercury in a large vessel to 200° or 300° C., the mercury becomes covered with a stratum of sulphuret of mercury, without any apparent disengagement of gas. The fluid which condenses in the vessel gives off fumes and takes fire of itself in air, if the heat has been continued long enough, and the temperature sufficiently high. This process is, however, not available for the exhibition of cacodyl, as the mercury only acts upon the sulphur compound of cacodyl at a temperature at which cacodyl already begins to be decomposed. Bromide of cacodyl behaves in the same manner; under similar circumstances a mixture of bromide of mercury, and a fluid which fumes in the air, is produced:—

$$\left.\begin{matrix}\mathrm{Kd\ Br}\\ \mathrm{Hg}\end{matrix}\right\} = \left\{\begin{matrix}\mathrm{Kd}\\ \mathrm{Hg\ Br.}\end{matrix}\right.$$

When this mixture is boiled in water, the bromide of mercury is reduced, and bromide of cacodyl is regenerated and given off with the watery vapour:—

$$\left.\begin{matrix}\mathrm{Kd}\\ \mathrm{Hg\ Br}\\ \mathrm{H\ O}\end{matrix}\right\} = \left\{\begin{matrix}\mathrm{Kd\ Br}\\ \mathrm{Hg}\\ \mathrm{H\ O.}\end{matrix}\right.$$

The last reduction also takes place at too high a temperature for the exhibition of the radical. The isolation is most easily and perfectly effected by using a metal capable of decomposing water and forming a chloride, particularly zinc, iron or tin. When tin, or any of the foregoing metals, is added to anhydrous chloride of cacodyl, the metal is dissolved at a temperature of 90° to 100° C., without any evolution of gas. The

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[*] Communicated by the Chemical Society, having been read December 21, 1841.

solution, which is at first clear, becomes of a dark colour on further solution of the metal. Water separates the pure chloride of tin, and leaves cacodyl mixed with a trace of chloride of cacodyl behind:—

$$\left.\begin{matrix}\text{Kd Cl}\\ \text{Sn}\end{matrix}\right\} = \left\{\begin{matrix}\text{Kd}\\ \text{Sn Cl.}\end{matrix}\right.$$

As zinc, however, effects the reduction of the chloride with the greatest facility, and as no further decomposition takes place in the chloride of zinc formed, I have in my experiments exclusively used this metal for the isolation of the radical.

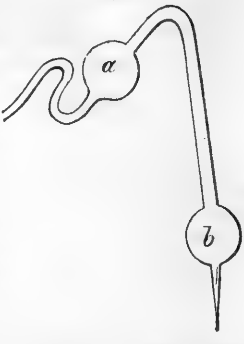
Notwithstanding that the reduction appears so easy, still, as it is very difficult to prevent subsequent decomposition in repeating the distillation and crystallisation of a substance which is as inflammable as the vapour of phosphorus, I think it necessary to enter into further details regarding the method of producing it.

Very thin sheet zinc, the surface of which has been previously cleaned with dilute sulphuric acid and afterwards well washed, is cut into small pieces, to be employed for this purpose. The chloride of cacodyl must be quite free from oxygen. By digesting oxide of cacodyl three times over in concentrated hydrochloric acid, a pure substance is procured, which does not give off any vapour. This chloride must be allowed to remain in a close vessel with chloride of calcium and caustic potash, without being distilled, in order to deprive it of any water it may contain, and also of any excess of acid. To prevent all access of air in this operation, a glass vessel of this description is employed (fig. 1.). At the opening *a*, a stream of carbonic acid is conducted through the vessel with the bulb *c*, to contain the substance to be dried. When the atmospheric air is entirely displaced, both ends, *a* and *b*, are sealed. When the vessel is required for use, the point *a* is broken and attached by a caoutchouc tube connected to an air-pump; the point *b* is then broken and put under the surface of the chloride of cacodyl; the latter is sucked up into the apparatus, and then immediately closed: I will call this the drying apparatus. The reduction and distillation is carried on, in a somewhat similar manner, in an atmosphere of carbonic acid, in a closed vessel (fig. 2.), the bulb *a* being the distillation tube, and the bulb *b* the receiver.

Fig. 1.

The whole apparatus being previously filled with carbonic

Fig. 2.



acid, the chloride of cacodyl is sucked into the bulb *a*, also containing the zinc. The open end of the vessel is then immediately closed with the blowpipe. It is exposed to the temperature of 100° C. for three hours, in a water-bath. The zinc is readily dissolved without any evolution of gas, and the solution becomes of a dark colour. On cooling to 50° C. large cubic crystals are formed, which are redissolved by heating. These crystals are probably a combination of chloride of zinc and chloride of cacodyl. When the zinc is no longer acted upon at 103° C., the contents of the bulbs appear converted into a dry mass of salts, which, upon an increase of temperature to 110° or 120° C., melts into an oily-like liquid. After the whole apparatus is warmed, the point of the receiver *b* is opened under cold water previously boiled. Upon the entrance of the water, upon the cooling of the apparatus, it is again sealed, and the water is conducted into the distillation bulbs. After a short digestion, a solution of chloride of zinc is formed, the zinc in excess remaining with a clear surface, and leaving the radical at the bottom as an oily liquid. This liquid is then transferred into the drying apparatus, and when perfectly dry is sucked up again into the distilling apparatus, and digested for a short time with clear zinc, by which means a small quantity of chloride of zinc is formed. It is then distilled, and comes over as clear as water. At a temperature of —6° C. large prismatic shining crystals are formed. After two-thirds of the solution has crystallized, the remaining solution is again distilled, and this is repeated three times over. The solution is finally put into a tube filled with carbonic acid.

The analysis of this liquid was conducted in the usual manner with oxide of copper. The arsenic sublimed in fine crystals in the back part of the tube without the formation of any arsenical copper or any arsenical salts. The quantity of arsenic was ascertained by weighing the tube before and after heating. The analysis gave the following results:—

	1.	2.
Substance	0·620 gr.	0·500 gr.
Carbonic acid	0·500	0·402
Water	0·306	0·200
Tube before heating .	62·681	60·670
... after heating .	61·869	60·020

The composition of this radical is therefore

		Calculated.	1.	2.
Carbon	4 equivalents	23·15	22·30	22·23
Hydrogen	6 ...	5·67	5·48	5·33
Arsenic	2 ...	71·18	71·29	71·
Loss and Oxygen . . .		0·00	0·93	1·44
		100·	100·	100·

The trifling difference between the quantities found and the calculated quantities, arises probably from the impossibility of obtaining this compound free from oxygen. If the results obtained are reckoned in the 100 parts, without taking notice of the oxygen, the carbon and the hydrogen agree still closer. The quantity of arsenic on the contrary appears rather too much. The facility with which cacodyl can be separated from its compounds by simple substances, renders it very probable that the oxide might be also reduced by means of carbon as well as hydrogen, upon the application of a higher temperature. Dumas's analysis, as well as my previous one, of the liquor of Cadet, renders this supposition nearly certain, and fully explains the cause of our arriving at different results. Dumas found, as I did also, in my first experiments, a constant excess of arsenic, carbon, and hydrogen, which is accounted for by the impurity of the oxide of cacodyl.

There was no difficulty in ascertaining the density of the vapour of the liquid, as the temperature at which it is decomposed is considerably higher than its boiling point.

Substance	0·2500 gramme.
Volume measured	55·98 Cbr.
Temperature	200° C.
Barometer	328·5 lines.
Column of oil	38 lines.
Col. of merc. at 200° C.	44·5 lines.

This gives the density of 7·101, which agrees as nearly as could be expected with the calculated density, viz.—

4 volumes	of vapour of carbon .	3·371
12 ...	hydrogen	0·825
2 ...	vapour of arsenic .	10·367
		14·563 ÷ 2 = 7·281.

The difference of 0·18 in the result obtained is fully accounted for by the tension of the mercury vapour in the barometer at the temperature of 200° C.

The agreement of both the analysis and density of the vapour with the respective calculated quantities is a matter of considerable interest. Berzelius has shown that when a certain density of a gaseous organical radical is assumed, the relative condensation which the compounds of this radical present, exactly agree with those of inorganic or simple radicals. This circumstance has given a weight to the theory of the compound radicals which the law of substitution could not reach. But this, in connexion with the phænomena of substitutions, does not advance the idea of organic radicals beyond the limits of a hypothesis. The proof of their reality is connected with three other conditions, viz. on their isolation, on the direct formation of their compounds, and on the actual agreement of the density of their simple elements with their theoretical density. All these conditions are fulfilled in regard to cacodyl: it may be isolated, it enters into direct combinations, and it has the density required if the laws of condensation of the inorganic elements are valid for organic bodies, as may be observed by the following statement:—

		Observed.	Calculated.
Cacodyl	4 vol. C + 12 vol. H + 2 vol. As = 2 vol. Kd	7·101	7·281
Cacodyl oxide ...	2 vol. Kd + 1 vol. O = 2 vol. Kd $\dot{O}$	7·555	7·833
Sulphuret Cacodyl	2 vol. Kd + 1 vol. S = 2 vol. Kd S	7·810	8·39
Chloride Cacodyl .	1 vol. Kd + 1 vol. Cl = 2 vol. Kd Cl	4·56	4·86
Chloride Cacodyl .	3 vol. KdCl + 1 vol. Kd O = 4 vol. 3 KdCl + Kd O	5·46	5·30
Cyanuret Cacodyl .	1 vol. Kd + 1 vol. Cy = 2 vol. Kd Cy	4·65	4·54.

This radical possesses the following properties :—

It is a clear, thin, highly refracting liquid, very similar to oxide of cacodyl; it has the same smell but is more inflammable. A glass rod moistened with it immediately takes fire when exposed to the air: its boiling point is about 170° C. At − 6° C. it crystallizes in large square prisms; if the substance is pure it becomes like ice. It burns in oxygen gas with a pale blue flame, and forms water, carbonic and arsenic acids, which rise in the form of a white smoke. If the air is not in sufficient quantity for the combustion Erytrarsin is formed, and a black stinking mass of arsenic remains. In chlorine it burns with a clear flame and deposits carbon. Digested with hydrochloric acid and metallic tin it is converted with the appearance of various products into erytrarsin. The same substance is produced by the action of phosphorous acid, chloride of tin, and other powerful reducing agents. Fuming sulphuric acid dissolves the radical without combining with it. In the cold a quantity of sulphurous acid is evolved, and on distillation it gives off a substance with an agreeable æthereal odour, which appears to be sulphate of ætherol.

2. *Formation of the Compounds of Cacodyl from their Radical.*

The relative condensation of the gaseous compounds of cacodyl and the transformations which they undergo, give a great degree of probability to the theory of organic radicals, which is now rendered perfectly incontrovertible by the power of this radical to form directly the compounds from which it was separated. The whole series of compounds already considered can be formed either in the direct or in the indirect way, and the conditions under which this happens are precisely those observed with regard to the metals. The indirect action of oxygen, as well as the action of most of the oxidizing agents, occasions an increase of temperature in the formation of both the oxide and the acid of this radical; and from the first, by the action of hydracids, we obtain the corresponding combinations with sulphur, selenium, tellurium, chlorine, iodine, bromine, and cyanogen. By the treatment of the so-formed chloride with chloride of copper, chloride of platinum, chloride of palladium, &c., certain double chlorides are formed, which I intend to refer to hereafter. When the radical is dissolved in nitric acid, and nitrate of silver is added, a very considerable precipitate is produced in the form of regular octahedral crystals, consisting of a combination of the latter salt with oxide of cacodyl, which appears to act the same part as constitutional water in salts. A solution of corrosive sublimate occasions the immediate formation of an oxychloride in the form of fine silky crystals, composed of 1 atom of oxide of cacodyl combined with 2 atoms of chloride of mercury.

Oxidizing agents are not the only bodies which act in a direct manner; other combinations are also formed in the same way. Sulphur in small quantities is acted upon by the radical, being dissolved by it, and forms a clear solution possessing all the properties of sulphuret of cacodyl, producing with solutions of oxides of lead and silver, sulphurets of these metals, and sulphuretted hydrogen with acids. Upon the addition of a large quantity of sulphur a higher sulphuret is formed, which is solid, and soluble in æther; from which latter solution it may be obtained in fine crystals. When to cacodyl a solution of chlorine is added, its yellow colour is immediately destroyed, together with its bleaching power; chloride of cacodyl is formed, which, acted upon by acids, gives hydrochloric acid. All these reactions, to which many more might be added of a not less striking nature, prove that this radical acts the part in every instance of a simple electro-positive element, *and that it is in fact a true organic metal.*

3. *Decomposition of the Radical.*

When the radical is distilled with anhydrous chloride of zinc it is decomposed, and forms several compounds at different temperatures. In order to ascertain more precisely the nature of this decomposition, pure chloride of cacodyl was digested with zinc in a distillation tube, until the whole solution was converted into a white mass of salt; the heat was then increased by means of an oil-bath to 200° C.; a perfectly clear fluid distilled over. When at this temperature nothing further passed over, the heat was increased to 220° C. and then to 260° C. It appeared to me dangerous to attempt any further decomposition by increasing the temperature; the attempt was therefore given up at this point.

After the apparatus was cool and the receiver taken off, there was no perceptible smell of any gaseous product. The substance which distilled over was again sucked up into a fresh distillation tube containing zinc, and by means of a continued digestion the last traces of chlorine were separated. The distillation was effected by means of an oil-bath. When at the temperature of 100° C. nothing more came over, the receiver was separated; its contents (No. 1.) were removed into a tube filled with carbonic acid, with all the precautions already mentioned, and again sucked up into a fresh distillation tube, and re-distilled at from 100° to 170° C. The product (No. 2.) was put up also into tubes. The residue which remained in the distillation tube at 170° C. was again, for the third time, removed into a fresh distillation apparatus, and again distilled at from 170° and 200° C. without leaving behind any perceptible residue, and forms No. 3. All the three distilled products were quite transparent, æther-like, very liquid, and quite free from chlorine. The first scarcely took fire of itself, had a strong æthereal smell, and remained liquid at −18° C. The two others were exceedingly inflammable, and crystallized at − 8° C. in large prismatic crystals like cacodyl.

Tested with corrosive sublimate, the first gave but little appearance of containing cacodyl; on the contrary, the two last appeared to contain a considerable quantity. Analysis gave—

No. 1. First Distillation.

Substance	0·561
Carbonic acid	0·5875
Water	0·3665
Tube before burning	80·261
... after burning	79·310.

No. 2. Second Distillation.

Substance	0·5403
Carbonic acid	0·5140
Water	0·3145
Tube before burning	74·976
... after burning	74·147.

No. 3. Third Distillation.

Substance	0·5930
Carbonic acid	0·4265
Water	0·2635
Tube before burning	83·0195
... after burning	82·3270.

These results (a repetition of which, I think, unnecessary, as the weighing of the tube after burning serves as a check upon them) give the following compositions:—

	1st distillation, at 90° to 100° C.		2nd distillation, at 100° to 170° C.		3rd distillation, at 170° to 200° C.	
	equiv.		equiv.		equiv.	
Carbon	4	28·95	4	26·31	4	19·88
Hydrogen	6·1	7·26	6·05	6·46	6·1	4·82
Arsenic	1·3	64·31	1·7	67·15	2·55	75·50
		100·52		99·92		100·23.

It follows from the analysis, that this radical, on distillation with chloride of zinc, undergoes a catalytic decomposition without the separation of arsenic, dividing into two or more compounds, in which the same quantity of carbon is combined with different quantities of arsenic; a circumstance of much interest as regards the theory of organic radicals. It is therefore probable that cacodyl, like arsenic, is a binary radical composed of $C_4 H_6$, and that its constituent elements are combined in such a manner that the compound of the cacodyl series are repeated in a similar way, only of a higher order. The above-described products of decomposition undergo, at a temperature of about 400° to 500° C., a decomposition, which I am in hopes, from the peculiarities in the constitution of the radical, to direct attention to. When cacodyl, or the before-mentioned mixture of the product of decomposition, is heated in a bent retort over mercury, the gas of this substance is decomposed, at a temperature little exceeding the boiling point of mercury, into metallic arsenic and a mixture of a compound of carbon and hydrogen, without the separation of a particle of carbon.

This gaseous substance burns with a variegated light flame, with a very slight deposition on glass of metallic arsenic. A solution of sulphate of copper, or nitrate of mercury, has no action upon the gas, however long it may remain in contact. With chlorine over water it takes fire, like a mixture of phosphuretted hydrogen, and burns with deposition of carbon, producing a red-coloured flame. Mixed with oxygen gas and inflamed by the electrical spark, it explodes more powerfully than fulminating gas, and generally breaks the vessel. Eudiometrical examination of the gas gives the following results:—

	1.	2.	Calculated.
Volume of the gas	1·4	1·5	1·5
Oxygen gas consumed . .	3·5	3·4	3·5
Carbonic acid formed . . .	2·0	2·0	2·0

These trials exactly agree with a compound in which the combination with the carburetted hydrogen in the cacodyl gives

gives	4	volumes of vapour of carbon,
	12	volumes of hydrogen,
condensed into	6	volumes.

I was at first induced to suppose that a similar decomposition of cacodyl took place as in the case of cyanide of mercury, as the action of this gas with chlorine did not agree with the action of any of the compounds from which this mixture of gases could in any manner arise; but the uncommon condensation, the essential circumstance in this case, appeared little to support this view. I have therefore continued the examination, and found that the burning with chlorine arises from the presence of a small quantity of a volatilizable compound of arsenic, which does not separate from the mixture, and which is at the same time the cause of the small stain of arsenic, which, on burning this gas in oxygen, remains on the side of the eudiometer. The true nature of this gas, given out by heat from cacodyl, is shown by the action of fuming sulphuric acid. This absorbs nearly one-third, and leaves behind an inodorous gas, burning pale blue, which is not altered by chlorine in the dark: in the direct rays of the sun, however, as Melsens has shown of the gas of the acetates and of marshes, it is condensed into oily camphor-like odorous bodies, in the state of small white radiating crystals. From a eudiometrical analysis of this gas, it appeared to be pure marsh gas. I found

From the volume examined . . .	19·2
Oxygen consumed	41·1
Carbonic acid formed	21·8

There can, therefore, be no doubt that the carburetted hydrogen, $C_4\,H_6$, formed on the decomposition of cacodyl at a high temperature, is not separated as such, but that there are formed under these circumstances, two volumes of marsh gas and one volume of olefiant gas, viz.

$$C_4\,H_6\,As_2 = \begin{cases} C_2\,H_4 \\ C_2\,H_2 \\ As_2 \end{cases}$$

The examination of the gas not absorbed by the sulphuric acid confirms this view of the question; as one volume and a half of the pure gaseous mixture, which contains one volume of olefiant gas, and two volumes ($C\ H_2$) of marsh gas, must, in fact, upon burning with three and a half volumes of oxygen, produce two volumes of carbonic acid.

Whilst the absence of arseniétted hydrogen and free hydrogen decidedly proves that the first is not to be considered as a constituent element of cacodyl; the conclusion may be drawn, at the same time, from these appearances of decomposition, that if the radical $C_4\ H_6$ can exist independently, it is most unstable and is decomposed much below a red heat.

Among the products of the decomposition of cacodyl, there is one substance which I have mentioned several times, and to which I have given the arbitrary name of *erytrarsin*. I shall now consider this substance, as it is in close connexion with the foregoing substances. I have not hitherto succeeded in obtaining any quantity of this remarkable substance. It is formed as a secondary product in the formation of chloride of cacodyl, sometimes in a great and sometimes in a small quantity. It is also deposited upon the distillation of oxide of cacodyl with water. Upon conducting the vapour of cacodyl, or oxide of cacodyl, through tubes slightly heated, this substance is produced in large quantities by an imperfect combustion; but, obtained in this manner, it is always contaminated with arsenic, from which it is impossible to separate it. The substance next made use of in preparing it was obtained in the following manner.

About 100 grammes of oxide of cacodyl was added to concentrated hydrochloric acid; chloride of cacodyl was formed, and a red flocculent precipitate fell, which, after distillation of the chloride, remains behind in the retort. The precipitate became during the distillation of a thick consistence, which increased, and became of a darker colour, with the appearance of finely divided red oxide of iron. After six or eight boilings with absolute alcohol, the substance was obtained quite pure and free from chlorine. It is necessary during this boiling to protect it from the air and to dry the substance in a vacuum with sulphuric acid, as otherwise it is liable to absorb oxygen slowly. Prepared in this manner, erytrarsin is of a steel blue shading into dark red, free from smell, and without the least appearance of crystallization. It is easily rubbed down into a red powder, which absorbs oxygen slowly from the air, with the appearance of the formation of arsenious acid, as it becomes covered with a white powder. This decomposition does not take place until after exposure for several weeks. It is not soluble in alcohol, æther or water—even caustic potash does not act upon it. In concentrated and not fuming nitric acid, it is soluble with decomposition. Red fuming acid occasions oxidation with inflammation. Heated in the air, it burns with an ash-coloured arsenical flame, without leaving any residue. Heated in a glass tube, it gives out vapours smelling of cacodyl, and deposits carbon, arsenious acid, and a ring of arsenic. The quantity produced from 100 grammes of oxide amounted to a little above 0·5 gramme. From the want of a sufficient quantity of this substance, I have only been able to make one analysis, which I however trust is sufficient, as every precaution was taken to ensure its accuracy.

0·394 gr. of the dried substance was burned with oxide of copper, and gave 0·1223 carbonic acid, and 0·074 water.

The arsenic was ascertained from the contents of the burning tube. These were dissolved in nitric acid, the solution diluted with water, and partly precipitated by carbonate of soda. The solution filtered from the copper was perfectly free from arsenic. The precipitate dissolved in hydrochloric acid, to which sulphuret of soda was added, also produced no precipitate of arsenic. The filtered solution gave, after being boiled with sulphurous acid in the usual manner, 0·7191 sulphuret of arsenic; of which 0·6333, acted upon by nitric acid, gave 0·0528 sulphur, and 2·1566 of sulphate of barytes. The following are the results:—

		Calculated.	Found.
C_4 . . .	305·76	8·73	8·58
H_6 . .	74·88	2·14	2·08
As_6 . .	2820·24	80·56	81·56
O_3 . . .	300·00	8·57	7·78
	3500·88	100·00	100·00

The difference of one per cent. in the arsenic found is accounted for from a small quantity of sulphuret of copper which was contained in the sulphuret of arsenic, which, on account of its small amount, could not be ascertained. The atomic weight of this substance I have not been able to ascertain in a direct way, as it does not enter into any direct combination; but the probability is, from the relation it holds to cacodyl, and to oxide of cacodyl, that that stated above is correct. I have therefore shown that the radical of the cacodyl series is converted, at a temperature approaching to redness, into marsh gas and oil gas, which gases may be considered as decomposing products of a non-isolated carburetted hydrogen, $C_4\ H_6$. From what precedes it also follows, that of three atoms of oxide of cacodyl two atoms are decomposed in the manner described, while one atom of erytrarsin is left behind:—

$$\begin{array}{ll} \text{3 atoms of oxide of Cacodyl,} & C_{12}\ H_{18}\ As_6\ O_3 \\ \text{2 atoms of } C_4\ H_6\ \ldots\ldots & \underline{C_8\ \ H_{12} \qquad\qquad} = 4\,C\,H_2 + 4\,C\,H \\ & C_4\ \ H_6\ \ As_6\ O_3 \end{array}$$

The rational constitution of this compound can only be conjectured. As cacodyl in combination with oxygen undergoes the same decomposition at a higher temperature as in an uncombined state, it follows that erytrarsin may be considered as the oxide of a ternary radical, which can be distinguished from cacodyl only by its containing three times as much arsenic. The complete examination of such a substance would be attended with great danger and many difficulties.

LVIII. *On the employment of Chromic Acid as an Agent in Voltaic Arrangements.* *By* R. Warington, *Esq.*[*]

In a paper "On the Action of Chromic Acid upon Silver," published in the Philosophical Magazine for December 1837, which action was effected by means of a mixture of bichromate of potash in solution and sulphuric acid, I concluded by stating, that in a future communication I hoped to consider the action of the same agents on other metallic bodies. The investigation has been resumed when my engagements permitted, and a great variety of interesting facts on this subject collected; but many analyses will still be necessary to render the subject complete, before the whole results can be submitted to the scientific world.

On making some new experiments, some time since, with the mixture of bichromate of potash and sulphuric acid referred to, I was led to believe that it would form a valuable and powerful agent in voltaic arrangements from possessing the following advantages over every other liquid hitherto employed for the same purpose, namely, the high degree of energy with which it acts upon certain metals, the facility with which it is decomposed by deoxidizing agents, as hydrogen gas and numerous others, with the circumstance that in all these actions of oxidation no gaseous matter is evolved.

My first endeavour was to substitute this mixed fluid for the nitric acid in the powerful arrangement of Professor Grove, so as, if possible, to obviate the inconveniences arising during the action of that battery, without diminishing the splendid effects produced by it. In doing this it was absolutely necessary, from the nature of the materials to be employed, to modify to a certain extent the details of the construction of the battery, retaining the metallic elements unaltered, but enlarging considerably the cell appropriated for the

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[*] Communicated by the Chemical Society, having been read Dec. 7, 1841.

nitric acid. Now as the dilute sulphuric acid in the zinc cell of the battery remains the same in both cases, it will be only necessary to show, by the constitution of the nitric acid and the bichromate of potash, the relative value of these two oxidizing agents in terms of the quantities of the available oxygen they contain, such oxygen combining with the hydrogen elicited by the action of the dilute sulphuric acid on the zinc element.

Liquid nitric acid, of 1·48 sp. gr., is composed of 74 parts by weight of real acid and 26 of water, and these 74 parts contain 32·9 of oxygen and 41·1 of binoxide of nitrogen, which latter body is given off in a gaseous state, as soon as the undecomposed nitric acid has become saturated with it, and assumed a deep green tint. When liberated from the solution the gas combines with the oxygen of the air, generating the nitrous and hyponitric acids, the red noxious vapours which render the use of this form of battery so inconvenient. There must, I imagine, be also a considerable loss of power from this evolution of gaseous matter. I am not aware to what extent the decomposition of the nitric acid can be carried in Grove's battery, for after the action has been going on about five hours, an effect of endosmosis commences between the cells through the pores of the biscuit earthenware, and the amalgamated zinc plates are attacked with rapidity and quickly destroyed. Not expecting such an occurrence, I had left a small battery in action, on one occasion, through the night, and found in the morning, to my great annoyance, that the whole of the zincs were destroyed, and the arrangement all fixed together.

Bichromate of potash is composed of 2 equivalents of chromic acid, or 104 parts by weight, and 47·5 of potash, and these 104 parts contain 80 of the green oxide of chromium and 24 of oxygen. Consequently, to obtain the same quantity of available oxygen as we have in the 100 parts of nitric acid, supposing the decomposition of these to be complete, we shall require 206·9 of bichromate; and to convert this into the double sulphate of chromium and potash, or chrome alum, 275·8 of concentrated sulphuric acid will be necessary. These proportions of materials are requisite, as it is the strong affinities leading to the formation of chrome alum which give rise to the energetic oxidizing action of this mixture.

A number of experiments were tried, to ascertain whether the action of a battery excited by the acid element described would be sustained and continuous, and the results have fully established that it is so. In the action of such a battery no gaseous matter is given off, the oxygen of the chromic acid combining with the hydrogen from the zinc cell to form water, as is the case where nitric acid is employed. And as the deoxidized chromic acid, or the oxide of chromium formed, combines with the sulphuric acid and potash immediately as it is produced, no injurious effect can arise from diffusion between the cells; the whole process goes on steadily and without intermission, until either the sulphuric acid in the zinc cell is saturated with the oxide of zinc, or the whole of the chromic acid of the bichromate is deoxidized.

Various other arrangements, in which bichromate of potash is used mixed with sulphuric, muriatic, nitric, and acetic acids, with the usual, and also with different, metallic elements, are under investigation; and the results obtained, with their comparison with other batteries, will be laid before the Society at an early period.

LIX. *On a new Class of Cacodyl Compounds containing Platinum. By Professor* Bunsen *of Marburg**.

In a former paper I have endeavoured to prove, from the numerous instances of substitution presented by *alcarsin*, that this substance contains a ternary radical, composed of *arsenic* united to a *carbo-hydrogen* ($C_4 H_6 + As_2$), and entering into composition with elementary bodies like a metal, in a manner not hitherto observed. This opinion has been confirmed by my subsequent experiments, and may be considered of considerable importance in the question of compound radicals. The *chloride* of this radical is reduced by those metals which decompose water, at a temperature not exceeding that of boiling water; the free radical separating in the form of a clear æthereal fluid, which oxidates in the air with more rapidity than potassium, and produces two degrees of oxidation by its combustion, namely, an acid and an oxide, both of which can be again reduced by deoxidizing agents. The analogy between cacodyl and the metals extends still further; for that radical unites directly with the non-metallic elements, forming substances of the same nature as are produced when hydracids combine with the elements of metallic oxides, water being produced.

It will be seen, from what has been said, that this substance bears a greater resemblance than most other compound bodies to *ammonia*. Under this impression I tried the action of *chloride of platinum* on it, and have been fortunate in obtaining a class of compounds analogous in composition to those of Gros and Reiset; supposing the ammonia in the latter

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* Translated from the German MS. of the author by Dr. T. G. Tilley. Communicated by the Chemical Society, having been read Dec. 7, 1841.

replaced by cacodyl. The results obtained tend to throw a new light on the relations in which the organic bases, or alkaloids, stand towards the simple oxides of metals.

Chloride of Cacoplatyl.

By mixing an alcoholic solution of chloride of platinum with a similar solution of chloride of cacodyl, a precipitate of a reddish-brown colour is obtained, which, when washed with alcohol and reduced to powder, becomes yellowish-red, and is inodorous. When this powder is heated, it melts into a clear yellow gummy mass, gives off hydrochloric acid and vapours smelling of chloride of cacodyl, and leaves behind a gray-coloured arseniuret of platinum. Both the chlorides of platinum and cacodyl are indicated in this compound by reagents. Should this compound be analogous to the chloride of cacodyl, its composition would be, $Pt\,Cl + Kd\,Cl$. This body, however, could not be analysed, for it is so easily decomposed as not to be of uniform composition. If the precipitate in question be boiled with water, a yellowish solution is formed, alcargen being generated at the same time, and the solution on cooling deposits white needle-shaped crystals. This substance may be named *chloride of cacoplatyl*, and from this name the others will be derived. To obtain the chloride of cacoplatyl in larger quantities and in a more easy manner, an aqueous solution of chloride of platinum is boiled with chloride of cacodyl. The precipitate, which falls first of a brown colour, is changed by boiling into a wine-yellow colour. The precipitation of the chloride of cacoplatyl commences even during the boiling, and by cooling still more is deposited. The mother liquid contains nothing except a little alcargen (or perhaps a true salt of cacodylic acid and oxide of platinum). The crystals are collected on a filter, and purified by redissolving. This compound possesses the following properties:—it crystallizes from a hot solution in long sharp needles, which are beautifully formed, is inodorous, its taste disgustingly arsenious. It is soluble in hot alcohol and water, more sparingly so in these liquids cold. When heated it becomes yellow, then brown, and without melting takes fire and burns like tinder, giving off vapours smelling of arsenic, and leaving behind fusible arseniuret of platinum. Sulphuric acid, by depriving the compound of water, turns it yellow. Hydrochloric acid has no action. In ammonia it is soluble in all proportions; by evaporating the solution, imperfect crystals are formed, which are insoluble in alcohol. Iodide of potassium produces in the solutions of chloride of cacoplatyl a yellow precipitate, which dissolves of a reddish-brown colour in ammonia. With bromide of potassium, a compound crystallizing in long silky needles is formed. Cyanide of potassium gives a yellowish white precipitate. By nitrate of silver, the chloride of silver is thrown down, without destroying the neutrality of the solution.

The elementary analysis of the chloride of cacoplatyl, dried at 110° C., was made by means of oxide of copper in a combustion tube, the free space left in the tube being filled with turnings of copper*. In a second analysis, chromate of lead was used; the results were the same:—

	1.	2.
Substance	1·440	1·0194
Carbonic acid . .	0·494	0·3480
Water.	0·356	0·2475

The chlorine was estimated by heating the compound to redness with caustic lime. 1·0873 gramme of substance gave 0·580 chloride of silver, or 0·0225 silver; by direct precipitation from the solution by nitrate of silver, from 0·987 of substance only 0·353 chloride of silver, or 0·1405 silver was obtained. The estimation of the platinum and arsenic is attended with some difficulty, from the circumstance that chloride of cacoplatyl is not perfectly oxidized by nitric acid. 0·850 gramme was therefore heated in a combustion tube, with a mixture of 1 part of carbonate of soda and 3 parts of chlorate of potash. The contents of the tube, after digestion with water, left a quantity of arseniuret of platinum. The solution, which was coloured yellow by a little of the double chloride of platinum and potassium, was thrown on a filter, and the arsenic containing platinum again collected. This last was dissolved in aqua regia, and some silicic acid derived from the combustion tube separated. The fluid, freed from silicic acid, and evaporated to dryness, was again dissolved in weak alcohol, and gave 0·752 gramme chloride of platinum and potassium. Besides this, 0·018 platinum was obtained by heating a quantity of sulphuret, formed by transmitting through the solution a stream of sulphuretted hydrogen. The fluid, when filtered, was made use of for obtaining the quantity of arsenic; it was freed from alcohol by boiling, deoxidized by sulphurous acid, and precipitated by sulphuretted hydrogen. It gave 0·458 of sulphuret of arsenic, from which, by oxidation with nitric acid, 1·254 of sulphate of barytes was obtained.

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* This precaution is necessary, lest some chloride of copper be carried into the chloride of calcium tube with the watery vapour; when metallic copper is present, a basic chloride of copper is formed, which is not volatile.

These analyses conduct to the following formula for chloride of cacoplatyl:—

			1.	2.	
Carbon	C_4 . .	305·7	9·44	9·49	9·52
Hydrogen	H_7 . .	87·4	2·70	2·75	2·73
Arsenic	As_2 .	940·0		29·54	29·29
Platinum	Pt . .	1233·3		37·98	38·34
Chlorine	Cl . .	442·6	13·48	13·85	13·79
Oxygen	O_2 . .	200·0		6·39	6·32
		3209·0		100·00	100·00

The agreement between the carbon, hydrogen, and chlorine found, and the numbers obtained by calculation, proves without doubt that the following empirical formula, $C_4\ H_7\ As_2\ Pt\ Cl\ O_2$, is correct. It appears certain that this compound contains an atom of water, not as water of crystallization, but in another form, for the compound may be heated to 164° C. without decomposition. At that temperature the colour is changed to a citron yellow, and an atom of water is given off, which however is reacquired when the substance is boiled with water. 0·9767 loses, by 210° C., 0·037, and no more, although the temperature is raised to 240° C. The compound contains therefore 3·79 per cent. of water, which corresponds to 1 atom, and can be replaced by 1 atom of ammonia.

Bromide of Cacoplatyl.

This compound is formed when a hot solution of the chloride of cacoplatyl is mixed with bromide of potassium; the crystals obtained are redissolved and recrystallized twice. They possess great similarity to the chlorine combination, and form small yellow needles by the quick cooling of the aqueous solution; but when the solution is allowed to cool gradually, the crystals formed are large, well-shaped, and colourless. They are pretty soluble in hot, but only sparingly soluble in cold water. They have a feeble acid reaction, are inodorous, but possess a decidedly disagreeable arsenical taste, which is bitter and astringent, and remains long on the palate, suggesting alcarsin. At 120° C. they lose their water and become yellow. At 240° C. this compound begins to be decomposed, becoming gray at that temperature; and when the heat is increased to 246° C. it melts into a black fœtid mass. At a higher temperature it takes fire in the air, and burns like tinder, leaving the arseniuret of platinum in shining scales. The analysis of this compound is equally simple with that of the preceding chlorine compound, and is made by combustion with oxide of copper; the anterior part of the tube being filled with copper turnings.

Dried at 100° C.	1.	2.
Substance	0·8835	1·1347
Carbonic acid . .	0·268	0·3473
Water	0·190	0·0249

To estimate the quantity of the bromine, 0·7145 gramme was dissolved in water, precipitated by nitrate of silver, and boiled some time with nitric acid, by which 0·452 bromide of silver was obtained. This research gives the following numbers:—

			Calculated.	1.	2.
Carbon	C_4 . .	305·76	8·17	8·16	8·37
Hydrogen	H_7 . .	87·36	2·33	2·39	2·41
Arsenic	As_2 .	940·08	25·10		
Platinum	Pt . .	1233·26	32·93		
Bromine	Br . .	978·30	26·13	26·56	
Oxygen	O_2 . .	200·00	52·34		
		3744·76	100·00		

To determine the proportion of water in the substance, 1·2534 gramme was dried at 100° C. and then heated in an oil-bath at 200°, till no more weight was lost. The loss was 0·040, which is equal to 3·200 per cent. It will be seen that this compound, like that of chlorine, contains 1 atom of water.

The formula for the hydrous and anhydrous compounds respectively are

$$\mathrm{Pt\,O_2\,C_4\,H_7\,As_2\,Br},$$
$$\mathrm{Pt\,O\;C_4\,H_6\,As_2\,Br}.$$

In this compound also the water can be replaced by ammonia.

Iodide of Cacoplatyl.

The yellow precipitate which iodide of potassium forms with chloride of cacoplatyl is this iodide. By mixing the two solutions, boiling hot and tolerably dilute, the iodide separates in the form of glistening scales of a silky lustre, resembling the iodide of lead. It possesses nearly the same degree of solubility in water as the last-named substance.

The iodide differs from the other compounds of cacoplatyl in losing its whole water at 100° C.; it becomes then of a brown violet colour without melting. The brown crystals dissolve in water, forming a yellow solution, which deposits crystals again on cooling.

This compound also is inodorous, and may be submitted to a high temperature without decomposition. It is injured at 260° C., when the compound melts and becomes black, giving off dark vapours smelling like alcarsin, and, lastly, burns like tinder, leaving the arseniuret of platinum. For the analysis of this substance, it was dried and burned with oxide of copper and copper-turnings, which last prevent any error arising from iodine passing over.

	1.	2.
Substance	1·4322	1·146
Carbonic acid	0·3745	0·300
Water	0·2510	0·400.

To estimate the iodine, 0·6685 of the salt, dried at 100° C., was precipitated from solution by nitrate of silver, the precipitate being afterwards boiled in nitric acid. This trial gave

		Calculated.		1.	2.
Carbon	C_4	305·76	7·22	7·23	7·24
Hydrogen	H_6	74·88	1·77	1·95	94
Arsenic	As_2	940·08	22·22		
Platinum	Pt	1233·26	29·14		
Iodine	I	1578·28	37·29	36·58	
Oxygen	O	100·00	2·36		
		4232·62	100·00		

Sulphate of the Oxide of Cacoplatyl.

To prepare this compound, a solution of 20 parts of the chloride of cacoplatyl, dried at 100° C., is boiled with 12·17 parts of dried sulphate of silver till the solution is not rendered turbid by salts of silver or chlorine. The filtered fluid is evaporated *in vacuo* over sulphuric acid till crystallization begins. At this degree of concentration a trace of the chloride of silver, which had remained dissolved, is precipitated. The chloride of silver is separated by filtration, and the solution again evaporated *in vacuo* and over sulphuric acid, till the greater part of the salt is deposited. The salt is purified by pressing it between folds of bibulous paper. Thus prepared it has the form of white hard crystalline grains, which appear, under the microscope, to be prismatic. This salt is inodorous, but possesses a bitter and astringent taste, which after a time suggests a relation to the cacodyl compounds. It does not deliquesce, nor is it decomposed by contact with air. It may be heated to 160° C. without injury; a few degrees higher its colour becomes gray, then black, giving off vapours smelling of cacodyl, and, lastly, it takes fire and burns like amadou, leaving behind an arsenical compound containing platinum, which is fusible.

To ascertain the quantity of water contained in this compound, 1·078 gramme was dried for 24 hours over sulphuric acid, and then again for six hours at a temperature of 100° C.: the loss of weight was 0·0045 gramme. By heating it for three hours longer at 140° C. it lost 0·0025 gramme. It thus appears that this compound parts with its hygroscopic water with difficulty. It contains no more water which can be driven off by any elevation of temperature. 1·0390 gramme of this salt, dried at 140° C., burnt with chromate of lead, gave 0·2395 water, and 0·340 carbonic acid.

0·5491 gave 0·1290 water, and 0·175 carbonic acid.

1·0474 gramme dissolved and precipitated by nitrate of barytes, gave 0·04746 sulphate of barytes.

These determinations give the following composition:—

			Calculated.	1.	2.
Carbon	C_4 . .	305·7	9·08	8·81	8·81
Hydrogen	H_7 . .	87·4	2·60	2·56	2·61
Arsenic	As_2 . .	940·0	27·91		
Platinum	Pt . .	1233·3	36·62		
Oxygen	O_3 . .	300·0	8·91		
Sulphuric acid	SO_3 .	501·2	14·88	15·57	

The characters of the chloride of cacoplatyl are so well marked, and its relations to other bodies so manifest, that we cannot be in doubt for a moment as to its rational composition. One glance at its empirical formula will satisfy us that here, as in the compounds of cacodyl, the most electro-negative element, chlorine, can by analogy be replaced by bromine and iodine, just as oxygen is replaced by sulphur. The manner in which this substitution takes place is not different from that which we observe in the inorganic saline compounds. The chloride of cacoplatyl, treated with the iodide of potassium, gives up its chlorine to the potassium, while the iodine goes over to the other element of the formula, from which the potassium has withdrawn the chlorine. The order of affinity of chlorine, iodine, and bromine, for the substance in question, bears a perfect analogy to what we observe in the inorganic haloid salts. The iodine is here set free by chlorine and bromine, as in these salts, while bromine is removed by chlorine only.

Such an agreement in relation shows a similarity in the form of the groups of the elements, and indicates that here, as in the inorganic haloid compounds, there are two divisions in the formula, one of which represents the metal, the other the halogenous body or salt-radical. We can express it thus:—

$$Pt\,O_2\,C_4\,H_7\,As_2 + Cl.$$

The first division of this formula, which I have called cacoplatyl, represents a peculiar and remarkable radical, forming classes of compounds possessing great interest, and giving an insight into the relation in which the vegeto-alkalies stand with regard to organic radicals. As the vegeto-alkalies, when heated, give off ammonia, so our compound gives off water, and this water can be replaced by oxides of metals. If we remove this atom of water in the formula, we have remaining one atom of oxide of platinum, and one atom of cacodyl, which will explain the formation of these compounds in the simplest manner. The rational expressions may be thus given:—

For the anhydrous chlorine compound	$Pt\,O\,Ka + Cl$
For the hydrous	$H\,O\ \ Pt\,O\,Ka + Cl$
For that containing ammonia	$N\,H_3\ Pt\,O\,Ka + Cl$
For the oxide	$H\,O\ \ Pt\,O\,Ka + O$
For the sulphate	$(H\,O\,Pt\,O\,Ka + O)\,S\,O_3$

The nature of this composition proves that the power of inorganic *acids* to unite with certain organic bodies, without losing their power of saturation, is not alone possessed by acids, but that *bases* have also the same property; for in the present case, the oxide of platinum bears a relation to the oxide of cacodyl similar to that which sulphuric acid does to benzoic acid in sulpho-benzoic acid. In the latter, the benzoic acid is as little indicated by reagents as the oxide of platinum in the cacoplatyl; and as the double acid referred to neutralizes only one atom of base, so the double base in question saturates only one atom of acid, or that quantity which the quantity of oxygen in the oxide of platinum indicates.

A comparison of this new class of compounds with that discovered by Gros and Reiset, will afford another reason for admitting the constitution which has been assigned to them. Reiset has rendered certain the existence of a body, composed of the elements of 1 atom water, 2 atoms ammonia, and 1 atom oxide of platinum, which does not lose its atom of water when it enters into combination with oxygen acids, and contains, precisely as cacoplatyl, 2 atoms of oxygen, and saturates 1 atom of acid. Berzelius affirms that these salts contain the oxide of ammonium. Here ammonia is combined with the oxide of platinum, as the naphthaline is in sulpho-naphthalic acid, viz. $(Pt\,O\,N\,H_3,\ N\,H_4 + O)\,S\,O_3$.

The simple relation in which this salt stands to the cacoplatyl compound must therefore not be passed over. The latter is nothing else than such a salt, in which the ammonium is replaced by cacodyl. Its relation to ammonium in the electrical series of compound radicals is like that of an electro-negative metal to an electro-positive one, as, for instance, iron to potassium. It cannot, however, be denied, that, while the compound of Reiset is a strong caustic base, the oxide of cacoplatyl forms only salts of an acid reaction. The analogy which the vegeto-alkalies and their composition show is so great, that it permits no doubt as to the identity of their constitution with that of this body. It now only remains for me to show, by comparison, the greatness of this analogy by the substitution of the platinum compound by an organic oxide.

Reiset's Compounds.	Comps. of Cacoplatyl.	Compounds of Quinine.
$NH_3, PtO, NH_4 + Cl$	$\dot{H}\dot{P}tKd + Cl$	$(C_{20}H_8)O_2NH_4 + Cl$
$NH_3, PtO, NH_4 + I$	$\dot{H}\dot{P}tKd + I$	$(C_{20}H_8)O_2NH_4 + I$
$NH_3, PtO, NH_4 + O$	$\dot{H}\dot{P}tKd + O$	$(C_{20}H_8)O_2NH_4 + O$
$(NH_3, PtO, NH_4 + O)SO_3.$	$(\dot{H}\dot{P}tKd + O)SO_3.$	$(C_{20}H_8O_2NH_4 + O)SO_3.$

The formation of *urea* (a body which possesses all the properties of an organic base, and may be considered as a cyanate of oxide of ammonium) belongs to the same class of phænomena. In that compound an oxide of cyanogen (cyanic acid) occupies the place of the oxide of platinum in Reiset's ammonium compound, and unites with ammonium to give rise to a compound radical, if such it may be considered. The radical $(Cy O, N H_4)$ which forms part of urea, is in every relation similar to cacoplatyl; the oxide of that radical, or urea, being of all this class of compounds that which approaches most closely to the oxide of cacoplatyl, $(Cy O, N H_4) + O =$ urea.

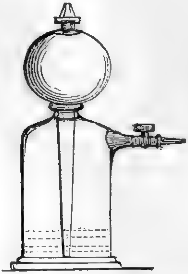
LX. *On detecting minute quantities of Arsenic and Antimony.*

By R. H. Brett, *Ph. D.*

To Richard Phillips, Esq.

My dear Sir,

HAVING been lately engaged in a set of experiments made for the purpose of ascertaining the minimum of arsenic and antimony capable of being detected by decomposing the gases which they respectively form with hydrogen, according to the principle of Marsh, I have sent you the results of such experiments, as I think they may perhaps be of some importance, especially in a medico-legal point of view. I may here observe, that the apparatus I employed in these experiments is one which I have been in the habit of using for the last three or four years for the purpose of detecting arsenic, and is a mere modification of Dœbereiner's hydrogen lamp: the lower vessel for generating the gas is 8½ inches in height and 4 inches internal diameter, capable of containing about a quart of fluid; near the upper part is a glass tube forming a right angle with the sides of the bottle, and to this glass tube is cemented a brass stop-cock 2 inches in length, with a small pin-hole jet. When generating hydrogen gas in this apparatus, I prefer using rolled zinc cut into portions of about one inch or half an inch square; these are placed at the bottom of the



generators, about the circumference. I use oil of vitriol of commerce, which if not free from arsenic, I purify by diluting it with water and submitting it for some considerable time to the action of sulphuretted hydrogen gas. The diluted sulphuric acid employed has a specific gravity of 1·231; and this may be obtained by mixing 1 measure of the concentrated acid with 4 measures of water: I find these proportions better than any others for obtaining a speedy and liberal supply of hydrogen gas.

It is generally considered that the zinc of commerce contains arsenic; I may here observe, however, that I have examined many specimens both of English and foreign zinc during the last two or three years, and when the sulphuric acid employed was free from arsenic I could never get any indications of that metal by means of the apparatus just described. It will be presently seen how minute a quantity can be detected when really present in zinc.

The first set of experiments was made on alloys of zinc and arsenic.

200 grs. of zinc were placed in the hydrogen apparatus. I may just state that all the zinc employed in this set of experiments was from the same sample, with the requisite quantity of diluted sulphuric acid; after a short time the gas issuing at the jet was fired and the flame kept up for ten minutes, or a quarter of an hour: no indications of arsenic could be procured on white porcelain.

200 grs. of zinc were melted in a crucible, and one grain of metallic arsenic dropped in and stirred about; a small quantity of the latter metal was volatilized; the alloy, still fluid, was cast on a clean iron plate; when cold, cut into fragments and introduced into the hydrogen apparatus with the diluted acid; very large and abundant arsenical stains were obtained. In this experiment, therefore, rather less than one part of arsenic was alloyed with 200 parts of zinc.

200 grs. of zinc were melted in a new crucible and a quarter of a grain of metallic arsenic dropped in and stirred about; a small quantity of the latter in this case too was volatilized; the alloy was cast, broken up, and placed in the apparatus as before: abundant arsenical stains were obtained on porcelain.

In this experiment there was rather less than one part of arsenic to 800 parts of zinc.

400 grs. of zinc were melted and ·4 grain of arsenic stirred

in; scarcely any of the latter was lost by volatilization. This alloy was cast and divided into eight parts equal by weight. Three of these parts gave abundant arsenical stains in the hydrogen apparatus: here the proportions are one and a half part of arsenic to 1500 parts of zinc.

Two parts out of the eight were fused with 400 grains more zinc, cast and divided into five equal parts by weight; two out of the five parts were placed in the hydrogen apparatus; very faint but characteristic arsenical stains were obtained; here the proportion of arsenic to zinc was four parts of the former to 20,000 of the latter, or one part to 5000. This appeared to be the minimum quantity capable of being detected in the apparatus.

The next experiments were for the purpose of ascertaining the minimum quantity of arsenious acid which could be detected in the apparatus. The zinc employed was from a different sample to that used in the first set, and was previously well tested and found incapable of affording any stains.

2·24 grs. of recently sublimed arsenious acid were dissolved in sixty measures of water; the quantity of metallic arsenic = 1·69 gr.

Half a measure of the solution, when placed in the hydrogen apparatus with zinc and sulphuric acid, gave several strongly marked arsenical stains; the quantity of metal was therefore = ·01407.

One-third of a measure of the solution gave several faint but characteristic arsenical stains; the metal = ·00938.

One-sixth of a measure of the solution gave only two or three very faint stains; the metal = ·00469. This was the minimum quantity.

The next experiments were made upon alloys of antimony and zinc.

200 grs. of zinc were fused and 1 grain of antimony stirred into the fused mass; the alloy when cold was introduced into the hydrogen apparatus, together with diluted sulphuric acid; large and abundant antimonial stains were obtained.

200 grs. of zinc were fused and ·2 gr. of antimony stirred into the fused mass; the alloy when cold was placed in the apparatus; abundant antimonial stains were obtained: the proportion in this case of antimony to zinc was as 1 to 1000.

400 grs. of zinc were fused and ·4 grain of antimony stirred in; the alloy was divided into eight parts; one of these parts was again fused with 200 grains of zinc, and the alloy thus obtained was placed in the apparatus; abundant and characteristic antimonial stains were obtained: the proportion of antimony to zinc in this case was as 1 to 5000.

One out of the eight parts just referred to was fused with 500 grs. of zinc, and 150 grs. of this alloy were placed in the apparatus; several distinct antimonial stains were obtained: the proportion of antimony to zinc in this case was as 1 to 11,000.

One out of the eight parts was fused with 600 grs. of zinc; 150 grs. of the alloy yielded in the apparatus a very few small but sufficiently characteristic antimonial stains: the proportion of antimony to zinc in this case was as 1 to 13,000. This appeared to be the minimum quantity which could be satisfactorily detected in an alloy of zinc and antimony. I then proceeded to ascertain the minimum quantity of antimony which could be detected by these means; in the form of sesquioxide of antimony well crystallized tartar emetic was used.

5·24 grs. of transparent well crystallized tartar emetic equal to 2·23 grs. of sesquioxide of antimony = 1·88 metallic antimony, were dissolved in 60 measures of water.

Half a measure placed in the hydrogen apparatus gave very distinct antimonial stains; the quantity of metal in this case was = ·0156 gr.

One-third of a measure under similar circumstances gave distinct stains, small in size, and approximating in appearance arsenical stains: the quantity of metal in this case was = ·01044 gr.

One-sixth of a measure under analogous circumstances gave stains very small, and two or three only in number: the quantity of metal in this case was = ·00522 gr. This appeared to be the minimum quantity capable of being detected by such means.

The colour of the flame when antimony is present in quantity sufficient to produce large and dark stains does not differ materially from that observed when arsenic is present, both possessing a bluish tint, more distinct, however, in the case of arsenic; very small quantities of arsenic render the flame blue, but when the quantities of antimony approach the minimum the blue colour is not observable. The mere hydrogen flame becomes considerably enlarged, especially when arsenic is added to the apparatus, and the disengagement of gas bubbles in the generating vessel becomes more active. The same phænomena may be noticed when antimony is present, but not in so remarkable a degree. I have many times had occasion to observe that when very minute quantities of arsenic or antimony are present, that metallic stains are not sometimes made manifest upon white porcelain until the lapse of some minutes; it is of importance to be aware of this circumstance, so as not to allow one prematurely to assert the absence of the metallic bodies. It is also of importance to be aware that the deposition of metallic stains, both in the case of antimony and arsenic, may go on for a considerable time; I have collected such stains on clean white porcelain for a quarter of an hour or twenty minutes, even when the quantities of metal or its oxide present have been small. This form of apparatus appears therefore to me to have a decided advantage over any apparatus so arranged that a quantity of the gas to be tested is first collected in a receiver and then inflamed and examined; for, from what has been stated above, it may very well happen that in cases where the quantity of metal present is very small, that the first, second, or even third charge of gas in the receiver may not give any indication by combustion, of metallic stains, because the metal is frequently not present in the first portions of gas liberated.

The colour of the arsenical stain is generally speaking very distinct from the antimonial. In the former case, it is invariably, I believe, of a brown colour, the intensity of which, as well as that of the metallic lustre, increases with the quantity. I suspected at one time that this brown colour might be owing to the presence not of mere metallic arsenic, but to that of a solid hydruret of the metal analogous to that brown coloured solid obtained when water is decomposed by a battery, the negative electrode of which is metallic arsenic. In order to test this opinion by experiment, I collected a sufficient quantity of brown arsenical stains upon white porcelain, carefully removed them from the surface of the latter, and exposed them to a temperature between 200° and 300° Fahr. for some time to drive off any adhering moisture. The brown powder was introduced upon a platinum tray into a tube of German glass, and exposed to the flame of a spirit-lamp, whilst a current of perfectly dry oxygen gas was past over it; a white crystalline sublimate of arsenious acid was soon obtained lining the cool end of the tube, *but no water was formed.* From this experiment I conclude that the brown stains are metallic arsenic mixed perhaps with a little arsenious acid. The antimonial stains, on the other hand, are generally of a very dark colour, almost black in some cases; whenever, indeed, the stains are of moderate size, they never, as far as I have observed, assume the brown colour so highly characteristic of arsenical stains; it must, however, be confessed that when the quantity of antimony very nearly approaches the minimum, that the stains do begin to assume a brown colour in some cases, closely resembling arsenical stains. When the stains are of a compound nature, such as may be obtained from the mixed gaseous hydrurets of arsenic and antimony, I have invariably found, unless they be excessively minute and attenuated, that each manifests its characteristic colour, the brown arsenical stain appearing around the dark and almost black antimonial stain: this position of the arsenical stain is due, no doubt, to its greater volatility, for in collecting pure arsenical stains, the central portions are almost always wanting when the porcelain is held in such a manner as to be perpendicular to the axis of the flame: this is not the case with the antimonial stain when collected under precisely similar circumstances.

Liverpool, Feb. 14, 1842.

LXI. *On* Fernel's *Measure of a Degree. By* Professor DE MORGAN.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I MUST trouble you with one more communication on the subject of Fernel's degree, as I have now direct evidence that all I have heretofore advanced is perfectly correct.

I will first recapitulate the steps of the discussion. In the Magazine for December, I called your attention to the fact that the French historical writers had grossly misinterpreted Fernel's account of his own measure. In that of February, I made it appear from his own words that if those historians were correct, Fernel must have been in the habit of taking steps of 38 inches each, at least. Also in February, my friend Mr. Galloway replied to my first communication, maintaining from the probability of the case, and from the authority of the most celebrated astronomers and metrologists of the seventeenth and eighteenth centuries, that the French historians were substantially right, and that Fernel's *geometrical* foot was the French foot of his time, which (we both agreed) differed by no material length from its present value. In the Number for March, I replied to Mr. Galloway, showing, as I thought, that throughout the sixteenth century there was a system of measures current among mathematicians, expressly intended to get rid of the great diversities of common measures, by introducing the smaller diversities of measures directly derived from the human body: in confirmation of this, I produced the direct authority of Clavius, who lived through a great part of the century in question.

In looking further through writers of the sixteenth century at the British Museum, I fell upon the *Monalosphærium* of

Fernel himself, published at Paris in 1526, two years before the *Cosmotheoria*: it is likely enough that he corrected the proofs of the former about the time when he made the measure of a degree which is described in the latter. In this *Monalosphærium* (leaf 25, page 2) I found *Fernel's own geometrical foot*, marked on a line extending down the page, divided into four palms, one of which is divided into four digits, one of which is divided into four grains. Fernel's words on it are, "Cæterum virga quædam mensoria omni molimine nobis deligenda est," mensurarum diversitate locupletatâ*. "Hac quippe duce faciliore negotio in omnes provehemur operationes; magnoque usui futura, si quintupedalem quantitatem toti concesseris." The title of the line is *Figuratio pedis geometrici.* Fernel's geometrical foot is then the line thus figured *plus* an allowance for the shrinking of the paper.

Before I give the measure, I point out what it ought to be nearly, from Fernel's own description of his step. The average step of a man is 30 English inches, or very near to it: Fernel says that five of his paces make six geometrical paces. At 60 inches to a pace, 300 inches make six geometrical paces, whence the geometrical pace should be 50 inches, and the geometrical foot *ten inches* (English).

On measuring the geometrical foot in the *Monalosphærium* (that is, in the copy of it at the Museum), I find it to be within a sixtieth of an inch of *nine inches and two thirds.*

On examining the plate of Dr. Bernard's work on Ancient Weights and Measures (1687), I find that in my copy, the length described as 7 inches has shrunk to $6\frac{25}{30}$ inches. Having no better means of forming a correction for Fernel's *paper foot*, I adopt this one, and increase $9\frac{2}{3}$ inches in the ratio of 41 to 42, which gives nine inches and nine tenths, for the foot of Fernel's paper† before shrinking; say ten inches, as in all probability the above allowance may be a little too small, on account of the greater age of Fernel's work.

What then was Fernel's degree, which the historians, one and all, think to have been so near the truth that they exclaim at his luck? His geometrical pace is 50 English inches, his Italian mile 50,000 inches, and his degree of 68·096 Italian miles is therefore 340,4800 English inches, or *fifty-three miles and three quarters*, instead of more than *sixty-nine miles.* And the fact turns out to be, that whereas I was unable to allow Fernel a longer than the Roman foot, and the French historians

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* I am obliged to quote these three words from memory, having forgotten to write them down.

† I believe it is the opinion of printers that much the greater part of the shrinking of paper takes place *in the drying.*

give him an inch and a half more, he should have been assigned an inch and a half less.

There is somewhere in Paucton, but I have not any note of the place, a surmise that the geometrical pace was about $4\frac{1}{2}$ Roman feet. This surmise seems to have arisen out of the difficulty he found in otherwise reconciling the metrological statements of the middle ages. A pace of $4\frac{1}{2}$ feet (Roman) would have been 52 inches English, answering to Fernel's geometrical pace.

It is not necessary for me now to give the results of the further inquiry which I made into the writings of the metrologists and cosmographers of the sixteenth century: I will merely mention two things which struck me. George Agricola, whose work on weights and measures was several times published in the first half of the century in question, uses words which seem to imply that measures absolutely derived from the human body were in use in commerce, though his expressions are not conclusive: he is followed by several others. Antonius Nebrissensis, whose work on Cosmography was published at Paris in 1533, asserts that his own foot and his own pace (he being, as he says, a man of moderate size) were the measures actually used by geographers, and coincided with the Roman measures. He mentions two places in Spain, the distance of which was known in Roman miles from the Roman itineraries, between which he had paced to ascertain this point. So that Fernel seems to have used a less measure than even the geographers of his time; and the difference cannot be easily explained unless the supposition of Paucton be adopted. It is hardly to be thought that Fernel laid down an arbitrary measure for himself. His own words, that the standard was to be selected "omni molimine," imply the contrary, for *molimen* means difficult endeavour. Neither, had the measure been one of his own invention, would he have failed to repeat the configuration in his *Cosmotheoria*. That he omitted to do so was his "luck," and the misfortune of Picard, Cassini, Montucla, Lalande, and Delambre, all of whom, as Mr. Galloway truly states, took it for granted that he used the Paris foot of his time; to which I add, without, as far as appears, thinking it necessary to make a single inquiry about the usages of that time.

The imagined good fortune of Fernel has acted unfavourably upon opinion as to the measure of Norwood, which comes also very near the truth, and was performed in a manner which shows, that, like more modern observers, he laid himself out for luck, by taking care to give every error equal chances of being positive and negative. With such a measure as that of Fernel, and a supposed result so near the truth, it was not difficult to attribute to hazard the success of another operation of a somewhat similar kind. But those who attend to the subject, and whom I am able to convince that Fernel's result was on the wrong side of 54 miles, instead of the right side of 69, will perhaps take another look at the mode pursued by Norwood, in which case I am inclined to think they will be able to satisfy themselves, that instead of rowing in the same boat as Fernel, with luck for the steersman, he is well entitled to have his conclusion considered as the result of such skill and patience as could not have led to any other.

I remain, Gentlemen, yours faithfully,

University College, April 2, 1842. A. DE MORGAN.

LXII. *Inquiry respecting a correction requisite in the working of Dr. Olbers's method of determining the Elements of the Orbits of Comets.* *By* R. TEMPLETON, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

HAVING read over with much interest the method of determining the elements of the orbits of comets by Dr. Olbers in the last volume of your Magazine for 1835, I selected from Lieut. Stratford's Ephemeris of Halley's three positions, intending to familiarize myself with the practical working of the method, but a difficulty met me at the onset. It will be remembered that this comet was detected early in August, when its distance was about twice the radius of the earth's orbit; observations were afterwards made when it was only one-eighth of this quantity, and again when it had nearly acquired the same amount. Now in combining these observations, the omission of an allowance for the time elapsed since the comet had *actually* the positions observed, would introduce an error of a quarter of a minute in right ascension, and of half this in declination, which, however small or modified by errors of observation, ought not to be neglected; I wish in consequence to be informed by some of your Correspondents through the medium of your valuable Journal, whether it has been usual to apply a correction $-8^{m}\cdot125\,\rho$ to the time of observation after the first approximation admits of tolerably accurate values of the curtate distances being obtained.

I remain, Gentlemen, yours &c.

Colombo, Jan. 26, 1842. R. TEMPLETON.

LXIII. *Abstract of Chemico-Physiological Researches by* JOSEPH SCHERER, *M.D.*

[Continued from p. 319, and concluded.]

Of Albumen.

A. PURE albumen from serum, dried without coagulation, was digested successively with water at 86° Fahrenheit, alcohol and æther, and then analysed.

B. Serum of the blood, merely boiled successively with water, alcohol and æther, until nothing more was dissolved.

C. Albumen from hens' eggs, dried without coagulation, was digested with water at 86°, which dissolved the pure albumen (as in A), which was then precipitated by alcohol, and boiled with alcohol and æther.

These specimens were analysed by combustion with chromate of lead, and the nitrogen determined both by Will's method and directly; the results are,

	A.	B.	C.
Carbon	55·461	55·097	55·000
Hydrogen	7·201	6·880	7·073
Nitrogen	15·673	15·681	15·920
Oxygen, sulphur and phosphorus	21·665	22·342	22·007
	100·000	100·000	100·000

The albumen derived from various morbid secretions was also analysed by the same methods; from the fluid of hydrocele and of ordinary ascites; from pus (the ashes of which contained a little *iron*); from a congestive abscess. The analytical results per cent. were identical with those for normal albumen already given.

Casein.

A. Fresh milk being precipitated by alcohol, the curd was taken out and boiled repeatedly in alcohol and æther, until all traces of butter were removed. To deprive it totally of sugar it was then again boiled with water, and finally dried at 212° Fahr.

B. Milk which had become spontaneously somewhat sour, was heated; it then curdled. The curd was boiled in water, alcohol and æther, as long as anything was dissolved, and then dried at 212°.

C. Casein prepared by Liebig, by precipitating milk with acetic acid, dissolving the precipitate by carbonate of soda, and again precipitating; then extracting the butter by boiling æther.

D. The material (coagulating by heat, like albumen) which remains dissolved in milk which has become sour, was boiled with alcohol and æther, and dried.

Of these specimens of casein,

A	gives	10·0	per cent.	of ashes.
B	...	2·0	...	...
C	...	1·5	...	...
D	...	2·0	...	...

The methods of analysis being the same as in the former instances, the results were,—

	A.	B.	C.	D.
Carbon . .	54·825	54·721	54·580	54·507
Hydrogen	7·153	7·239	7·352	6·913
Nitrogen .	15·628	15·724	15·696	15·670
Oxygen Sulphur	22·394	22·316	22·372	22·910
	100·000	100·000	100·000	100·000

Protein.

Dr. Scherer prepared this body according to Mulder's method, by solution in caustic potash liquor and precipitation by acetic acid. The results with protein from three different sources agree very well with those of Mulder, and are as follows:—

	Protein from crystalline lens.	Protein from albumen.	Protein from fibrine.
Carbon	55·300	55·160	54·848
Hydrogen . .	6·940	7·055	6·959
Nitrogen . . .	16·216	15·966	15·847
Oxygen	21·544	21·819	22·346
	100·000	100·000	100·000

As the compounds of protein do not admit of our deducing any positive rational formula for protein, Liebig proposes the empirical formula $C_{48} H_{36} N_6 O_{14}$, which gives a per cent. composition coinciding with the results of the above analyses.

Having thus determined the composition of the fundamental materials of the animal organization, Dr. Scherer proceeds to examine the secondary tissues. These are merely to be considered as products of animal life, and are not met with in vegetable structures, whereas the former class exist equally in both departments of organized nature. The substances to be analysed were obtained in the condition of greatest anatomical purity, and carefully freed from all intermixed fat, &c. by such chemical means as were least likely to alter their own texture. Although such textures are not anatomically homogeneous, but under the microscope still show traces of the presence of nerves and blood-vessels, yet these could not be supposed to modify their composition in any important degree, as their quantity is excessively minute, and their composition not very different from that of the tissue under examination.

Tissues which yield Gelatine.

The substances selected as yielding pure gelatine, were isinglass and the tendons of the feet of young calves. They were both perfectly soluble in boiling water. The isinglass, when freed from all traces of fat by boiling in æther, and dried at 212°, gave by incineration 0·5 per cent. of ashes. The calves' tendons macerated for some time with a solution of nitre to remove any investing membrane, and then washed perfectly out with pure water, and boiled with alcohol and æther, gave on incineration 1·6 per cent. of ashes.

Each material was analysed several times, but the following results need only be given, the others were almost coincident:—

	Isinglass.	Calves' tendons.
Carbon	50·557	50·960
Hydrogen . .	6·903	7·188
Nitrogen . . .	18·790	18·320
Oxygen . . .	23·750	23·532
	100·000	100·000

The *sclerotic coat of the eye* is composed as the gelatinous tissues, although in its insolubility in boiling water it resembles those tissues which yield chondrin. It gave—

Carbon	50·995	100·000.
Hydrogen	7·075	
Nitrogen	18·723	
Oxygen	23·207	

If we take double the carbon which exists in the formula already given for protein as a standard, and calculate from it a formula to express the composition of gelatine as given above, we obtain $C_{96}\,H_{82}\,N_{15}\,O_{36}$, and subtracting from it two equivalents of protein, there remain $3.N\,H_3$ with $H\,O$ and $7\,O$, for

$$\left.\begin{array}{l} C_{96}\,H_{82}\,N_{15}\,O_{36} \\ -C_{96}\,H_{72}\,N_{12}\,O_{28} \end{array}\right\} = H_{10}\,N_3\,O_8.$$

Tissues which yield Chondrin.

Chondrin was shown by its discoverer Müller to exist in the permanent cartilages, in the bones before ossification, in the cornea of the eye, and also in some morbid enlargements of the bones.

For analysis, Dr. Scherer selected the cartilages of the ribs of young calves, and the cornea. They were digested with water and some saltpetre to dissolve out all albuminous material, and then boiled with alcohol and æther to remove the fat.

This chondrin dried at 212° contained 6·6 per cent. of ashes.

Analysed by the usual methods the results were—

	Ribs.		Cornea.
	I.	II.	III.
Carbon	49·496	50·895	49·522
Hydrogen......	7·133	6·962	7·097
Nitrogen	14·908	14·908	14·399
Oxygen.........	28·463	27·235	28·982
	100·000	100·000	100·000

If we take the carbon here as being of the same magnitude as that of protein, the formula resulting from these analyses is $C_{48} H_{40} N_6 O_{20}$, which differs from protein by containing $4 \,.\, H\,O$ and $2 \,.\, O$ in excess.

The Elastic Coat of the Arteries.

This tissue, purified in the same manner as the other substances analysed, and dried at 212°, gave 1·7 per cent. of ashes.

The analysis of it yielded—

	I.	II.
Carbon	53·750	53·393
Hydrogen	7·079	6·973
Nitrogen	15·360	15·360
Oxygen	23·811	24·274
	100·000	100·000

The formula given by these results is $C_{48} H_{38} N_6 O_{16}$, and deducting thence that of protein, $C_{48} H_{36} N_6 O_{14}$, it appears that the elastic coat of arteries differs from protein in containing plus the elements of two atoms of water.

Corneous Tissues.

This substance presents itself in two forms, *membranous* and *compact.* The first constitutes the epidermis and the epithelium in its various forms; the latter constitutes hair, horns, nails, &c.

A. *Membranous Corneous Tissue.*—For analysis, Dr. Scherer selected the epidermis of the sole of the foot where it is thickest and most easily separated. It was purified by boiling in water, alcohol and æther. Dried at 212°, it gave 1·0 per cent. of ashes.

It was analysed as usual, except that the nitrogen was determined directly by volume in the old manner.

The results were,—

	I.	II.
Carbon ……	51·036	50·752
Hydrogen…	6·801	6·761
Nitrogen …	17·225	17·225
Oxygen……	24·938	25·262
	100·000	100·000

B. *Compact Corneous Tissue.*

I. *Hair of the beard*, which gave 0·72 per cent. of ashes.

II. *Hair of the head* (light coloured), which gave 0·3 per cent. of ashes.

III. *Hair of the head of a Mexican* (very black), which gave 2 per cent. of ashes.

IV. *Brown hair from the head.*

The analytical results were—

	I.	II.	III.	IV.
Carbon ……	51·529	50·652	49·935	50·622
Hydrogen …	6·687	6·769	6·631	6·613
Nitrogen ……	17·936	17·936	17·936	17·936
Oxygen and Sulphur…	23·848	24·643	25·498	24·829
	100·000	100·000	100·000	100·000

C. *Buffalo horn* purified as the other substances, gave 0·7 per cent. of ashes.

With this and the hair, the relative volumes of nitrogen and carbonic acid were found to be 1 : 7 when chromate of lead was used.

The nitrogen was also determined by the method of Will and Plantamour to be 16·380 per cent., but came out 17·284 by a direct determination.

The analytical results in four cases were—

	I.	II.	III.	IV.
Carbon……	51·990	51·162	51·620	51·540
Hydrogen …	6·717	6·597	6·754	6·779
Nitrogen……	17·284	17·284	17·284	17·284
Oxygen and sulphur	24·009	24·957	24·342	24·397
	100·000	100·000	100·000	100·000

Nails and *wool* purified as usual gave the following results :—

	Nails.	Wool.
Carbon ……………	51·089	50·653
Hydrogen …………	6·824	7·029
Nitrogen …………	16·901	17·710
Oxygen and sulphur	25·186	24·608
	100·000	100·000

If we seek to establish for these horny tissues a formula comparable with that of protein, we may best adopt $C_{48} H_{39} N_7 O_{17}$, from which if we subtract protein ($C_{48} H_{36} N_6 O_{14}$) there remains $H_3 N O_3$, or the horny material consists of protein $+ N H_3 + 3 O$.

When horn, wool, or hair is dissolved in solution of potash, ammonia is evolved. On decomposing the liquor by sulphuric acid, removing the precipitate by filtration and distilling, a very sensible quantity of acetic acid may be obtained. If the alkaline solution be decomposed by acetic acid, sulphuretted hydrogen is evolved, and a precipitate formed which agrees perfectly with protein in its character and composition; but if this precipitate be removed and the liquor decomposed by a new quantity of acetic acid, a grayish solid separates which is of a different nature. Its composition, and that of protein from hair or horn, is as follows:—

	1st precipitate, true protein.	2nd precipitate.
Carbon	55·408	53·536
Hydrogen . . .	7·238	6·956
Nitrogen . . .	15·593	14·801
Oxygen	21·761	24·707
	100·000	100·000

The membrane which encloses the albumen and lines the interior of the shell of the egg was found to be also a horny tissue.

Feathers.

This material, which is usually considered as horny, has been found by Dr. Scherer essentially distinct. It contains to the same carbon, hydrogen and nitrogen, an atom less of oxygen than true horn, the formulæ expressing the analytical results being $C_{48} H_{39} N_7 O_{16}$.

Black Pigment of the Eye.

This substance, the details of the preparation of which in the pure state may be passed over, consists of

	I.	II.	III.
Carbon	58·273	58·672	57·908
Hydrogen . .	5·973	5·962	5·817
Nitrogen . . .	13·768	13·768	13·768
Oxygen . . .	21·986	21·598	22·507
	100·000	100·000	100·000

LXIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

Extracts from the Address delivered on the Anniversary, February 19th (1841), *by the Rev. Professor Buckland, D.D., P.G.S.*

PHYSICAL GEOLOGY.

IT is not long since, in the Transactions of the Cambridge Philosophical Society (Vol. IV., 1838), we rejoiced to see a mathematician of such high authority as Mr. Hopkins, in a paper entitled "Researches in *Physical Geology*," adopting this term as one of acknowledged and deserved acceptance in our nomenclature, and to find him asserting, "that we are now arrived at that stage of geological science, in which we are able to recognize certain well-defined geological phænomena distinctly approximating to geometrical laws," and following up this assertion by the first example of a geological investigation conducted on principles supplied by mathematical analysis. The apparent irregularities which the disturbances of the globe seem at first sight to present, being thus reduced under the dominion of mathematical calculation, we hail in this paper the commencement of a series of physical deductions, explanatory of the law of parallelism, which is so constantly observed in the case of mineral veins, faults, and anticlinal lines; and referring this law to a mechanical cause, demonstrable by the test of exact geometrical proof.

We have recently witnessed another investigation of this high order, respecting the necessary relations between observed phænomena and the physical cause to which they owe their origin, in a communication to our Society by Mr. Hopkins "On the parallel lines of simultaneous elevation in the Weald of Kent and Sussex*." In this highly philosophical paper, he shows that these lines exactly correspond with the deductions of mathematical theory, resulting from the hypothesis of the elevation having been caused by an expansive force acting from below upon stratified rocks, within the nearly elliptic area of the Wealden formation, in the S.E. of England, and the Bas Boulonnais.

Prepared with the geometrical results of theory as an antecedent basis of his observations, and introducing this new and most efficient auxiliary as a fundamental element in the machinery of Descriptive Geology, he has added to the views of preceding observers a mathematical precision, which forms the commencement of a new method of demonstrative investigation, more exact than has been hitherto applied to problems of such universal extent as those relating to the causes that have produced the movements of stratified rocks in every portion of the globe.

Assuming theoretically the application of an expansive force acting uniformly upwards within an elliptic area, he finds that the longitudinal fissures thereby produced would nearly coincide with the outlines of the ellipse, forming cracks that are portions of smaller concentric ellipses, parallel to the margin of the larger ellipse; and that these longitudinal fissures would be numerous, and parallel to

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* A district long ago and ably illustrated by the researches of Mr. Mantell.

the strike of the elevated strata; and would also be intersected perpendicularly in the direction of the dip of the strata by many transverse fissures. In all these fundamental deductions from theory, Mr. Hopkins finds an almost mathematically exact coincidence with actual observation of the longitudinal and transverse fractures in the Weald; the former are respectively parallel to the strike of the N. and S. Downs which bound the area of the Wealden district, and are convergent to a point near Petersfield; the latter pervade many minor longitudinal ridges in the same district, and are most obvious in the well-known transverse valleys that intersect at right angles the chalk escarpments of the North and South Downs, forming the only outlets of the nine rivers that take their origin within the ellipsoid area of the Weald.

Many of the minor transverse valleys that intersect the minor longitudinal ridges, give origin to perennial springs, which are thrown out by the dislocation of the strata, where the faults to which these valleys owe their origin intercept the progress of the subterranean waters, by breaking the continuity of the strata they percolate.

From these fundamental observations, he concludes that the Wealden district owes its elevation to one simple elementary cause acting simultaneously, and perhaps at successive intervals, at every point within the area in question; and producing dislocations, not, as some have supposed, along one single central axis of elevation, on the long diameter of the ellipse, but simultaneously on many lines, and causing many minor elevations parallel to the curvatures of the margin of the ellipsoid area in question.

The theory of the simultaneous action of the moving forces within all parts of the elevated area, does away the mechanical difficulty of forming these fissures by a force applied only along one single axis of elevation; whilst the entire series of phænomena accords with the hypothesis of a broad expansive force acting below, not along one single line, but generally and uniformly under the whole district, with equal intensity at every point.

In this great physical problem, the form of the elevated area is a most important element, and in the case of the Weald, its elliptic form is highly favourable to the comparison which has been instituted by Mr. Hopkins: other important elements are the constitution of the strata, their equable thickness, equable cohesion, and the direction of their natural joints.

In the same simultaneous elevations that have extended from Boulogne through the area of the Wealden formation to the east of Hampshire, near Petersfield, Mr. Hopkins would include also (as Dr. Fitton has done in his observations on the Strata of the South-East of England) the parallel elevations of Portsdown, the Isle of Wight, the Purbeck and Weymouth districts, and the vales of Tisbury, Pewsey and Highclere, on the west and north margins of Wilts and Hants*.

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* The term "Valleys of Elevation" was first introduced to English Geology in a paper "On the Valley of Kingsclere and other Valleys," by Dr. Buckland.—*Geol. Trans.*, 2nd Series, vol. ii. part 2. 1827.

Mr. Hopkins has also arrived at similar conclusions respecting the longitudinal and transverse fractures which he has investigated in the mountain limestone and coal formations of Derbyshire; commencing, as in the present instance, with a theoretical investigation of the mathematical results of expansive forces acting from beneath, and comparing these results with observations on the longitudinal fissures and transverse fractures examined by himself in Derbyshire, and with the answers returned by practical miners in that district to a series of printed questions as to facts which theoretical calculations had indicated as probable, and which have been fully verified by the answers thus obtained.

In these Memoirs of Mr. Hopkins on the Wealden district, and on Derbyshire, we have the first instances of the geological investigation of any portion of the earth for the express purpose of exemplifying a theory founded on the solution of a mechanical problem; the results he has obtained in the coincidence of the phænomena with the mathematical theory by which they have been tested, have been remarkably approximate, and make us feel that the time is arrived when the investigations of geology have begun to exalt themselves beyond the exquisite and delicate investigations of Mineralogy, and the grand and universal laws of co-existence that give dignity and beauty to Palæontology, into those lofty regions of General Physics which connect them with the most sublime demonstrations of Astronomy.

It may be seen, by reference to the Ordnance Geological Survey of Cornwall, that the elevations and depressions of the older slate rocks in the West of England have been attended by numerous parallel fissures and transverse fractures, similar to those in the Weald of Kent and Sussex. In the mining districts of Cornwall, particularly near Redruth, these rents and fissures are known in all their various and curious details, from their having been excavated in search of the metallic ores which they contain. The main direction of these fissures being east and west, they are intersected, like those in the south-east of England, by transverse fractures or cross courses, running nearly north and south. Both these systems in Cornwall obviously result from the same mechanical laws which have not only caused transverse fractures to intersect the longitudinal lines of elevation, in the districts of the Weald and Derbyshire, where Mr. Hopkins has demonstrated their accordance with the theoretical laws of physical induction; but will be found to have affected every mountain chain produced by angular elevation upon the surface of the globe.

In the Annals of Philosophy, 1821, p. 453, I published a Memoir on the Structure of the Alps, in which it was shown that all the rivers which descend on the north side of this greatest European mountain chain, escape from longitudinal valleys parallel to the general axis of elevation and to the escarpments of the elevated strata, by a series of gorges transversely intersecting these escarpments; in the same manner as the four gorges, that intersect the Chalk escarpment of the South Downs, give outlet to four rivers formed in longitudinal valleys on the south side of the central axis of the Wealden elevation, namely, the Arun, Adur, Ouse and Cuckmere rivers; whilst five gorges in the escarpments of the North Downs give exit to five rivers formed in longitudinal valleys on the north side of the same central axis of the Weald, namely, the Wey, the Mole, the Darent, the Medway, and the Stour.

An objection has been sometimes raised to the theory which attributes the existing position of inclined strata to elevation, grounded on an assumption that the same relative positions of the strata in mountains and the valleys adjacent to them may have been caused by the *subsidence* of the lower parts of the strata into the basins, as by the *elevation* of those portions which now occupy the highest place; but these objections are overruled by mechanical and mathematical reasons, arising from observation of the relative positions of the dislocated strata on each side of the "upcast dykes" or faults that run parallel to these assumed lines of elevation; namely, that the dislocated strata, in almost all cases, occupy the place which an upward movement would have given to them respectively on each side of the fault, and which they could not have received from a downward movement under any process of depression*.

Mr. Martin, of Pulborough, has also resumed his consideration of the structure of Western Sussex, and of the anticlinal lines of the London and Hampshire Basins published in 1828 and 1829, with a paper on the relative connection of the eastern and western chalk denudations; in which he traces westward, from the Wealden district of Sussex, a system of six nearly parallel anticlinal lines, across the high table-land of chalk in Hants, Wilts, and Dorset; three of these lines of elevation proceed westward from the Wealden district, and three penetrate the chalk in an easterly direction from the valleys of Wardour, Warminster, and Pewsey. The continuity of these lines is occasionally interrupted for considerable intervals,

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* It is due to the memory of Mr. Farey, the cotemporary and fellow-labourer of Mr. Wm. Smith, that we should here notice the fact of his having many years ago presented to this Society an unpublished section across the Weald of Sussex, along the road from London to Brighton, to which due credit was not then attached. In this section, together with the general direction of the component strata of the district, as given in the sections of Mr. Mantell and Dr. Fitton, he introduces a series of faults, twenty-five in number, between Ryegate Hill upon the North Downs and Clayton Hill on the South Downs, representing minor movements and longitudinal fractures parallel to the great escarpments that bound the area of the Weald; many of these faults have been recognised where he had placed them by Mr. Hopkins. Mr. Farey also, in his "View of the Agriculture and Minerals of Derbyshire," 1815, has given an account of great systems of faults and denudations in Derbyshire and five adjacent counties; together with the coloured figures before alluded to explanatory of the nature of faults and dislocations, or tilts of the strata, and the subsequent effects of denudation upon them; which, though not confirmed in all their details by modern observations, show him to have been a most ingenious original observer, whose merits in this department have not been sufficiently appreciated.

and again resumed on the same parallel along the great elevated plain of the chalk.

Mr. Martin traces the most northerly and greatest of these anticlinal lines from the vale of Peasmarsh, between Guildford and Godalming, along the entire base of the North Downs, eastwards to the sea at Folkstone, and westwards to Farnham, Alton, and Popham Beacon, where it terminates in the high flat dome or tableland of chalk. The most southerly anticlinal line extends from Greenhurst, near Steyning, eastward to Lewes, and along the base of the escarpment of the South Downs to East Bourne and Beachey Head; and westwards by Midhurst and Petersfield to the Downs of East Hampshire, through which it emerges in valleys of elevation at East and West Meon, and in the valley between St. Giles's and St. Catherine's Hill at Winchester. The central anticlinal line of the Wealden he traces westward from Hazlemere to Liphook, Selbourne, and Candover near Arlesford, and Beacon Hill near Amesbury.

The anticlinal elevation of the valleys of Wardour, Warminster, and Pewsey, after advancing some miles eastward into the chalk, terminate in the high table-lands of Salisbury Plain and the North Hampshire Downs, which form a great flat dome of elevation between the counties of Sussex, East Somerset, and North Wiltshire.

Mr. Martin considers many of the higher crests and ridges that run in an eastern and western direction above this elevated plain, to be due to saddle-shaped elevations on one or other of the great lines of fracture that attended the upward movement of the chalk. In the details of his paper he confirms and extends the observations of Mr. Mantell and Dr. Fitton, upon the very interesting district which forms the subject of their common investigations*.

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* In his Geological Memoir on a part of Western Sussex, Mr. Martin put forth in 1828 some judicious remarks, showing, on the theory of derangement and denudation, that the Weald of Kent and Sussex, as well as the London and Hampshire Basins, had a common origin in a system of elevatory movements posterior to the formation of the tertiary strata. He considers that the strata which compose these basins, and were originally horizontal, suffered great disruption in the act of forming basins, either by the elevation of the sides or subsidence of the central portions of each basin; that in this operation deep and extensive fissures were formed in certain parts of the strata thus disturbed, analogous to those we see in the elevation and cracking of the flour which covers the fermenting nucleus of dough in a baker's trough; that the great undulations of the strata are not due to original deposition, but result from subterraneous movements, attended by enormous pressure. Mr. Martin also makes some judicious observations on the too-prevalent habit of using the term *chalk basin* in a manner that seems to imply local depressions peculiar to the site of each so-called basin, forgetting that the chalk itself (although it forms a very convenient and obvious geological horizon) is only an intermediate layer in a succession of basin-shaped strata; and contends that as the formations superincumbent upon and subjacent to it have a conformable disposition, it is just as correct to call them London clay, or greensand, or galt basins, as chalk basins. Again he observes, respecting the deposits of the basin of Paris, that their occurrence elsewhere in horizontal and apparently undisturbed positions, indicates the strata above the chalk to be of

POSITIVE GEOLOGY.—EXTENSIVE RECOGNITION OF SILURIAN AND DEVONIAN SYSTEMS ON THE CONTINENT.

We may congratulate ourselves on the advance that has been made during the past year, by the extension of our knowledge as to the existence of the Silurian, Devonian and Carboniferous systems over large districts of the continent of Europe. In my last address I endeavoured to explain the reason why the old red sandstone formation, which occupies so very extensive a place in England, had been scarcely anywhere recognised on the Continent; namely, because we had till lately failed even in our own country to refer to this system those extensive slaty forms of it, which, both here and upon the Continent, had been referred to the grauwacke of the Wernerian series, and had applied the name of old red sandstone only to a part of this formation, which had hitherto been considered as the type of the whole, namely, to the red marly, sandy, and conglomerate strata of Herefordshire and the adjacent counties, omitting the Killas and other slate-rocks of the Devonian system, which have now been shown to appertain to it.

I further stated, that it would probably be found that this Devonian system includes a large amount of strata upon the continent of Europe, which had been hitherto known by the Wernerian name Grauwacke; and expressed my satisfaction that this name was likely to retain its place in the nomenclature of geology, as a generic term co-extensive with the transition series of the school of Freyberg, and divisible into three great subordinate formations, namely, the Devonian, Silurian, and Cambrian systems.

The labours of Professor Sedgwick and Mr. Murchison in the Rhenish provinces and adjacent parts of Germany, in the summer of 1839, have furnished important additions to our knowledge of the older rocks of the continent, and brought them into comparison with the recently established *palæozoic* types of England; the first efforts of those authors were directed to the right bank of the Rhine, where taking the coal-field of Westphalia as a fixed horizon, they proceeded to deduce therefrom the descending order of the older formations which emerge southwards from beneath that deposit, and established a perfect sequence along a frontier of fifty miles in length, from a true coal-field with carboniferous limestone downwards into Silurian rocks, by passing through an intermediate group loaded with Devonian fossils*.

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a date anterior to their present curvilinear disposition in the form of basins. He further shows, that the act of denudation was not confined to the district of the Weald along the lines of movement in which the greatest elevations took place, but equally laid bare the highest summits of the chalk hills and elevated plains, and swept away much of the contents of the basins; and endeavours to establish the connexion of these elevations and subsidences with diluvial action, by showing that an adequate cause for this action may be found in the elevatory movements produced by forces acting upwards from the interior of the globe.

* This order was not made clear until some startling difficulties were overcome. All the German authorities had laid down as one continu-

In following out these strata to the E.N.E. the authors were astonished at the vast flexures, first laid down by Von Buch and Hoffman, and since more elaborately made out by Von Dechen and Erbreich; and perceived that the shales become more crystalline and slaty, and charged with mineral veins, and the limestones assume the state of marble or highly ferriferous rocks; these strata are also abundantly interrupted by ridges of Trap and frequently *inverted*, the carboniferous and Devonian deposits plunging under the older Grauwacke or Silurian rocks.

Our authors also found that the Devonian strata reappeared in irregular troughs among the Silurian Grauwacke (often with inverted inclination) in various parts of Nassau; many of the limestones, particularly on the river Lahn, being identical, both in structure and in coralline remains, with the beautiful marbles of Babbacombe, Torquay, and Plymouth. In many parts of this region the strata are in a highly mineralized condition, copper and lead ores, as well as the more prevalent iron mines, occurring at intervals; whilst numberless eruptive rocks diversify the surface; and the strata, particularly those of the Devonian age, alternate with a peculiar stratified contemporaneous trap-rock called "Schaalstein," the more schistose varieties of which contain Devonian fossils. The various mineral waters of Nassau are supposed to be due to the last expiring effects of the same causes which produced, in former times, the numerous eruptions of Greenstone, Porphyry, and other igneous rocks.

The quartz rock of the Taunus mountains, the southern limit of the region they examined, is considered to be an altered deposit of the Silurian epoch*.

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ous band (defining the same as *berg-kalk*), the limestone which at Ratingen is undoubtedly true mountain-limestone, and the calcareous zone which passes from W.S.W. to E.N.E. by the towns of Elberfeldt and Iserlohn. Now although at a first glance the physical features of the country seemed to favour this view (which was indeed adopted in the new map of Von Dechen), the close examination of the authors detected, that whilst the Ratingen limestone contained the fossils of the carboniferous system, that of Elberfeldt and Iserlohn was charged with different types, most of which exist in the lower limestone of Devonshire. Having assured themselves, therefore, that there was an error in the works of previous observers, they returned to Ratingen, and following the carboniferous limestone eastward along its strike they found it to be separated from that of Elberfeldt, gradually changing in its structure, and passing into thin-bedded black limestone associated with much flinty schist (kiesel schiefer) and chert, and assuming the lithological characters and fossils of the black or culm-limestone of Devonshire.

This black limestone is overlaid by unproductive measures of the coal series, similar to the upper strata of the great trough of North Devon, and is underlaid by psammites, schists, and limestone (Elberfeldt and Iserlohn) containing Devonian fossils, and reposing upon schistose and grauwacke rocks which contain Silurian fossils.

* The most characteristic Devonian Mollusca are Strygocephalus, Gypidium, two or three species of Turritella, Euomphalus, the Terebratula of

The authors next institute a comparison of the formations of Westphalia and Nassau with those of Liège, the Ardennes, and Eifel on the left bank of the Rhine. Starting from the country around Liège, which M. Dumont has rendered classic by his illustrations and his map, Messrs. Sedgwick and Murchison confirm the views of that author, and bear testimony to the great value of the method employed by him in bringing into symmetrical condition that highly tortuous and convulsed tract. They admit that he has most successfully demonstrated the replicatures of the different members of the Carboniferous and infra-carboniferous systems, and established on clear physical evidence, the fact that *whole basins* have been *inverted*. They differ from him, however, in the comparison he has made between the older rocks of his own country and those types of classification which the authors have established in the British Isles. In his table of comparison, M. Dumont supposes that the Old red sandstone of England has no equivalent in Belgium, and that the formations which there occur beneath the Carboniferous limestone (his *terrain anthraxifère*) are the equivalents of the *Silurian system*; our authors show that the psammites, schists, and limestones next below the coal-field and carboniferous limestone of Liège are the exact equivalents of the series which in Westphalia represent the Devonian system. The fossils are the same as those of Elberfeldt, Paffrath, and Devonshire. These beds also contain fishes of the genus *Holoptychius*, which Agassiz has identified with types of the old red sandstone; and on all these grounds, as well as by complete lithological and stratigraphical passage into the overlying carboniferous group, our authors establish that the *terrain anthraxifère* of D'Omalius and Dumont is, like the schistose rocks of Devonshire, the true equivalent of the old red sandstone.

The mountains of the Ardennes consist in their upper members of equivalents of the Silurian system, as indicated both by order of infraposition to the Devonian rocks, and by containing the same types of fossils which characterize the Silurian strata on the right bank of the Rhine; whilst the oldest slaty rocks, in which no fossils have been discovered, are presumed to be in the parallel of the Upper Cambrian group.

The limestones of the Eifel, well known by their fossils, lie in a basin supported by Silurian rocks, and are identical with the lower Devonian limestones of Liège, Westphalia, and Nassau; whilst the shales beneath them graduate into Silurian grauwacke, and contain so many Silurian species that (together with the well-known schists of Wissenbach on the right bank of the Rhine) they are considered to form the uppermost members of the Silurian division.

A similar succession to that from the Eifel to the Ardennes is

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Devonshire, with the very peculiar trilobite, *Brontes flabellifer* of Goldfuss. The upper members of the Silurian system are distinguished by Orthoceratites, Homalonoti, and other Trilobites, Pterineæ, Orthis, &c., some of which are identical with species found in the Silurian region; with these are some remarkable forms not yet detected in the British Isles, such as *Delthyris macroptera* and *D. microptera*.

observable between the Eifel and the Hundsruck, the upper Silurian flagstones being highly fossiliferous, but much contorted and disturbed and altered in their mineral condition; the banks of the Moselle offer the finest proofs of such disturbances. The fossils found in the quartzose rocks of the Hundsruck prove this mountain chain, which is a prolongation of the Taunus, to be, like it, of Silurian formation.

In the Hartz, the authors traced the same succession of mineral masses, each characterized by their peculiar fossils; and, if possible, in still more dislocated positions. In one section, however, they point out a tolerably regular descending order, from the mining tracts of Clausthal, where the beds are the equivalents of the carboniferous strata (floetzlehrer sandstein of the Germans) down to limestones charged with Devonian types; but in other parts, as near Goslar, the still older Silurian rocks occur upon the flanks of the Brocken, and *overlie* the Devonian schists; whilst it is shown that the granite of the Brocken was in a molten condition after the formation of these old rocks, fragments of which full of shells are found included in this Granite. Other sections show that the chain has subsequently been heaved up "en masse," and all the secondary strata on its northern flanks set on edge, and in some instances inverted, from the Muschelkalk and New red sandstone to the Greensand inclusive. The authors believe that the last great dislocations of the Hartz may be due (as suggested by Von Buch) to the eruption of the Porphyry, which on the southern and south-eastern limits of the tract is associated with the newest Coal strata and the oldest beds of the New red system (*Rothe todte liegende*).

The Thuringerwald is considered to exhibit the same succession of the older strata as the Rhenish provinces and the Hartz, the central masses being equivalents of the Silurian and possibly of the Upper Cambrian group; but the authors, having passed rapidly over these parts, attach importance only to their observations on the southern limits of that region, near the foot of the Fichtelgebirge, where they indicate a clear descending series, from the true Mountain-limestone with large Producti into lower fossiliferous limestones and slaty rocks, the fossils of which have been elaborately described by Count Münster, and which they place in the parallel of the Devonian system.

The authors express their very great obligations to Mr. Lonsdale, whose intimate knowledge of the Devonian fossils has enabled them to speak with confidence, and whose advice has often dispelled obscurities which must ever attend the elimination of the order of succession of rocks which have been so extremely dislocated, and in many instances so much altered. They also acknowledge the valuable cooperation of their friend M. de Verneuil, who accompanied them during a portion of the time devoted to this laborious survey, and to whose intimate acquaintance with the older fossils they are largely indebted; and who, uniting with his countryman M. d'Archiac, will describe the Mollusca of these regions as a sequel to the geological memoir of the authors.

Mr. Murchison's recent journey over large tracts of Russia was intended to test the accuracy of the new classification of the palæozoic rocks upon a still wider scale than any to which it had been applied. Believing from the works of Strangways, Pander, and Eichwald, that some members of these formations occur near St. Petersburgh, and prompted by the suggestions of M. Von Buch, that the threefold succession of Carboniferous, Old red, and Silurian systems would be found to prevail in Livonia and North-western Russia, Mr. Murchison, accompanied by M. E. de Verneuil, has made during the last summer a most extensive and instructive tour in Russia. The principal results of this journey were offered to the Geological Section of the British Association in September last at Glasgow, showing that the Silurian rocks occupy several islands in the Baltic and large parts of Livonia and Courland, and range by St. Petersburgh to the W.N.W. On the south they are overlaid by a great red formation which was formerly supposed to be the New red sandstone on account of its saliferous and gypseous beds, but which is now proved to be the Old red sandstone by containing the Ichthyolites which characterize that deposit in the British Isles; these fishes, *Holoptychius, Coccosteus, Diplopterus*, &c., are associated with Mollusca similar in species to some of the fossils of the Devonian rocks of England, Belgium, and the Rhine. The old red or Devonian rocks of Russia, spreading over a very wide area, are surmounted in the Waldai Hills by Mountain or Carboniferous limestone; the latter formation (in great part resembling in mineral condition a Tertiary deposit of white limestone) may be said to range from Moscow to Archangel, and even into the country of the Samoides, preserving the same lithological and geological characters, and occurring almost universally in horizontal unbroken masses for the distance of nearly one thousand miles. Thus the examination of Russia has not only confirmed the palæozoic classification of the Carboniferous, Devonian, and Silurian systems, but has given new materials for the establishment of correct geological theories as to the formation of the surface of the globe; for we now learn that deposits of this high antiquity have been left in undisturbed positions over very large areas, and that under such circumstances their structure has undergone little or no modification; whilst the large Producti of our Mountain-limestone occur in Russia in a white deposit, resembling the most incoherent parts of the Calcaire grossier of Paris. The general results and details of this important examination of Russia will shortly be brought before our Society.

DEVONIAN SYSTEM.

After reviewing the vast European extent which the equivalents of the Old red sandstone have been shown to occupy on the Continent, we cannot forget how much we owe to the sagacious and exact researches of Mr. Lonsdale, set forth in his most masterly and highly scientific communication to us respecting the age of the limestones of South Devon, wherein, after showing the state of former erroneous and inconsistent opinions upon the subject, he details the steps that led him to infer from zoological evidence alone, that they were of an intermediate age between the Carboniferous and Silurian rocks.

Mr. John Phillips had already observed the resemblance between many of these Devonian shells and those of the Mountain-limestone, and Mr. De la Beche had long ago noticed the position of the Torbay limestones to be incumbent on strata of Old red sandstone; and in 1839 suggested that their organic remains would seem to indicate relations to this formation. The cause of the obscurity that overhung this subject arose partly from the absence of any evidence from superposition, in consequence of the insulated place which these rocks occupied in the south of Devon; and partly from the non-existence, until a recent period, of any extensive catalogues of the organic remains of the Mountain-limestone and Silurian systems with which these fossils of South Devon might be compared.

In 1837 Mr. Lonsdale had ascertained, from an extensive collation of the shells and corals of the south of Devon with those of the Silurian system supplied in the catalogue of Mr. Murchison, and of the Carboniferous system in that of Mr. J. Phillips, that a large proportion of the Devonian fossils presented a character intermediate between those of the formations which lie above and below the Old red sandstone; and therefore concluded that the strata in which they are found must be subordinate parts of this intermediate formation. The suggestion was adopted by Mr. Murchison and Professor Sedgwick in 1839, and at once shed forth a new and brilliant light that has rapidly dispelled the darkness in which the slate rocks of this extensive formation had, until this discovery of Mr. Lonsdale, been involved. The first application that was made of this new instrument of identification to the continental rocks led to the immediate solution of the difficulties that had attended the attempts of preceding observers to ascertain the equivalents of the English series in the districts adjacent to the coal-fields of Liège and in the Bas Boulonnais; and we have already noticed the vast extent to which, during the past year, a similar identification has been carried in the Rhenish provinces and in Russia.

We should, however, not forget, that, by the recent examination of Russia, the distribution of fossil animals has been found to be materially connected with *mineral conditions*; for Mr. Murchison and M. de Verneuil have shown us, that with the resumption of its *red* and *green* characters, the vast Old red system of that empire resumes the very same zoological types as in the North of Scotland.

A short time will probably produce an abundant recognition of the same palæozoic classification in America. We have long been learning an instructive lesson as to the comparatively small value of mineral character in determining the age of strata, where there is no opportunity of appealing to the test of superposition; and organic remains have been found to supply the surest and safest criterion whereby formations can in such cases be made out; thus, the evidence of fossil shells has recently enabled us to identify the Oolite formation in Cutch and the deserts adjacent to the Indus, and on the

Tartar side of the Himalaya Mountains. Cases of this kind teach us to appreciate even still more highly than we have been wont to do, the paramount value of Palæontology in determining geological equivalents.

ORIGIN OF COAL.

In the early part of last year some very interesting papers came before us tending to throw light on the obscure and difficult question of the formation of coal.

Mr. J. Hawkshaw, having communicated to us in June 1839 a description of several large fossil trees found in a cut on the Bolton Railway, near the Dixon-fold Station, five miles and a half N.W. of Manchester, standing immediately upon a thin bed of coal perpendicularly to its surface, has added a statement of further facts, confirming his opinion that these trees grew in the place and position where they are now found. His reasons are grounded on observations he made near the shores of the Caribbean sea, on the rapid decomposition of the trunks of solid dicotyledonous trees in hot and moist climates. This decomposition in a few months entirely destroys the timber, leaving only the bark unbroken and hollow, like an empty mould in a foundry; the form of this bark remains perfect after the interior is rėduced to dust. He infers from this example, that it does not follow that fossil trees in the coal formation were originally hollow because we find their interior entirely filled with indurated clay or sand, since it appears from effects now proceeding in tropical climates, that the entire bark may have retained its place and form and have been filled with sand or silt after the interior of the trees had rapidly perished. Similar observations as to the rapid decay of timber have been made by Mr. Schomburgh.

Mr. J. E. Bowman also has endeavoured to prove that coal has been formed from plants which grew on the present areas of the coal seams, and that these beds of vegetable matter were at successive intervals submerged, and covered by sediments, which accumulated until they formed a surface fit for the growth of another series of land plants; and that these processes were repeated in the production of each bed of coal. In this manner he would explain the uniformity in thickness of individual coal beds over very large areas. He further admits, that other trees, branches, and leaves, may have been drifted from the neighbouring lands, and scattered through the beds of shale and sandstone, whilst they were in process of accumulation upon the subsiding or subsided beds of coal. Mr. Bowman agrees with Mr. Hawkshaw in believing the large trees upon the Bolton Railway, near Manchester, to be in their native place and position, and to have been *dicotyledonous*. He further mentions a similar case of at least forty trees, only three or four feet apart, found in 1838, standing erect upon the *upper* surface of a seam of coal fifteen inches thick in the railway tunnel at Clay Cross, five miles south of Chesterfield; these had no traces of large roots, and their exterior consisted of a thin film of coal, furrowed and marked like a *Sigillaria reniformis* ?, the interior being occupied by fine-grained sandstone. Mr. Bowman considers the trunks of fossil trees in the coal formation, which are thickened at their base, and terminate in large expanding forked roots, to have been dicotyledonous, whilst the monocotyledonous trees maintain throughout a nearly uniform thickness, and their roots probably consisted of an assemblage of succulent fibres; and argues, that if beds of coal were, like modern peat bogs, the accumulated remains of many generations of vegetables that grew upon the spot, they may, during such process of gradual accumulation, have afforded a surface adapted for the growth of the trees in question. He attributes the fact of the roots standing above the upper surface of the coal, as we sometimes see the roots of fir-trees above the surface of peat, to the shrinking of the vegetable matter in which they grew, and considers the actual thickness of each bed of solid coal to be about one-third that of the vegetable mass from which it has been derived*.

Mr. W. E. Logan has also communicated to us a series of minute results of extensive examinations made by himself, and in many cases confirmed by Mr. De la Beche, on the character of the beds of clay immediately *below* the coal seams in South Wales, from which it appears that immediately beneath every bed of coal in that extensive district is a substratum, called the *underclay*, varying in thickness from six inches to more than ten feet; and that this underclay so universally and inseparably accompanies nearly a hundred seams of coal throughout South Wales, that the collier seldom finds coal where this substratum is wanting: it is usually a fire-clay, containing sometimes an admixture of sand, and near Swansea passes into a hard, fine-grained, siliceous sandstone. This never-failing substratum of the coal is everywhere characterized by the exclusive presence of innumerable remains of *Stigmaria ficoides*, the stems of which are often of great length, and usually parallel to the plane of the bed, and more abundant near the top than the bottom of the underclay. From each of these stems there proceeds a series of very long and narrow leaves, forming an entangled mass, which traverses the fire-clay in every direction and to great distances; fragments of the stems of Stigmaria occur in other parts of the coal formation, but in the underclay alone are the long thin leaves attached to them. In 1818 the Rev. H. Steinhauer published in the American Philosophical Transactions, vol. i. p. 273, a similar account of the occurrence in the English coal formation near Brad-

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* I wish to correct an error in my Address of last year (Phil. Mag., vol. xvii. p. 512), where it is stated, that the place of the roots of the upright trees discovered in the Bolton Railway was immediately *under* a thin bed of coal; the fact is, that they were all *above* this coal: the difference is material, for if the roots be all *above* the coal seam, these trees, like fir-trees in a peat-bog, may have grown upon the accumulating bed of vegetable matter which is now converted to coal.

The theory that coal, like peat, owes its origin to vegetables that grew on the spot it now occupies, has been entertained by DeLuc, Macculloch, Jameson, Brongniart, Lindley, and other writers, but I have nowhere before seen such convincing proofs of this hypothesis as are furnished by the facts advanced by Mr. Hawkshaw, Mr. Bowman, and Mr. Logan, taken in connexion with one another.

ford in Yorkshire, of continuous stems and leaves of Stigmariæ, differing from those lately observed by Mr. Logan only as to the greater vertical range to which the leaves extended. Mr. Logan has traced them in a vertical direction seven or eight feet from the stem, and more than twenty feet horizontally*, and concludes that it is impossible to account for these phænomena by any theory of drift. He further supposes the Stigmaria to be the plant of which fossil coal is mainly composed.

I think we may derive, from the important facts above quoted, a probable illustration of the processes by which the formation of a coal-field has been conducted. We may assume the areas now covered with coal to have been extensive flats and estuaries, receiving at intervals, during seasons of flood, large deposits of silt and sand, interspersed with leaves and broken branches and trunks of trees, drifted down with the detritus of not far distant lands. We may conceive large portions of the surface of these sedimentary deposits, after the cessation of the floods by which they were respectively transported, to have become the site of broad and shallow ponds or lagoons, which were speedily filled with a matted mass of floating stems and leaves of Stigmaria, to the exclusion of all other plants, in the same manner as the social plant, *Stratiotes aloides,* now crowds the ditches and shallow ponds in Holland, until the water is filled with a dense assemblage of individuals of this single species, leaving no intervals for the growth of any other plants. We may further admit, that by the deposition of mud or silt between the stems and leaves of Stigmaria, the bottom of each lagoon might have been overspread with the earthy sediments that compose the beds of fireclay immediately below the coal; and that the same lagoon, after the deposition of these sediments, continued crowded with Stigmariæ, accumulating on one another until they had entirely filled the lagoon with a matted mass of stems and leaves, as modern shallow lakes are gradually filled up and converted into peat-bogs. The surface of the lagoon thus changed to a morass may forthwith have become covered with a luxuriant growth of marsh plants, *e.g.* with Calamites, Lepidodendra, Sigillariæ, &c., the exuviæ of which formed a superstratum of vegetable matter convertible to coal, resting upon a substratum composed exclusively of remains of Stigmariæ. The regions which were the site of this vegetable growth may, by success-

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* Mr. John Craig, of Glasgow, in an excellent paper on the coal formation of the West of Scotland read to the British Association at Glasgow, 1840, remarks that " the *Stigmaria ficoides* is frequently found in the shales, with the leaves attached to the stem and spread out laterally, in a manner which never could have occurred had the plant been drifted from a distance. The ripple-marks also (he adds), which are observable on almost all the shales and laminated sandstones throughout the whole carboniferous formation, show that these portions of the coal strata were deposited in very shallow water."

I learn from Mr. Binney that stems and leaves of Stigmaria abound in the beds of clay or fine sand that lie immediately below many beds of coal in the district of Manchester.

ive subsidences, have been so reduced below the level of the water, as to make them the receptacles of alternating deposits of sand and clay (now converted to strata of sandstone and shale) between the several beds of incipient coal. During these processes, successive series of lagoons may have covered large portions of each last-formed drift; and every lagoon becoming the site of a renewed growth of Stigmariæ, may thus continuously have been laying the foundation and nourishing the materials of future beds of inestimably precious fuel.

In the case of beds of coal that alternate with marine deposits, it has been suggested that extensive subsidence of the estuaries on which lacustrine and terrestrial plants were growing, may have reduced these estuaries below the level of the sea, where the submerged strata of vegetable matter became covered with beds of encrinal limestone and other marine sediments; and that as these received upon their surface further sediments of sand and mud drifted by land-floods into the salt-water, the estuaries were gradually filled up, and again converted into lagoons, upon which a renewed growth of lacustrine and land plants forthwith began to accumulate the materials of other beds of coal.

Both in the marine and the freshwater strata that alternate with the coal-beds, we appeal to the three same intermitting and alternate processes of subsidence, drift, and vegetable growth; the subsidence being in the former case to a depth below the level of the sea, in the latter case to a depth which left the last-formed strata in a position to become the site of vast swampy flats and shallow lagoons. In both cases intermitting accumulations of the earthy materials of the strata over the subsided districts are referred to the transport of sand and mud by powerful land-floods over areas which by subsidence had acquired a place that made them receptacles of the detritus of distant mountains; as we now see vast sheets of sediment transported from the Rocky Mountains and spread over the great flats and vast estuaries of the Red River, the Missouri, and the Missisippi. The regions on which these ancient alternations of salt-water and fresh-water deposits were going on, must in the mean time have presented extensive surfaces that were periodically oscillating between small distances above and below the level of the sea.

The concentric rings of growth which may be counted in a transverse section of the large coniferous trees whose roots are found resting on the upper surface of a coal-bed, may be quoted as evidence of the time during which it was fixed in this its place of growth; and as such trees may probably be found on the surface of many successive beds in the section of a coal-field, each stage of trees affords a chronometer by which we may calculate the number of years that intervened between the growth of each bed of coal.

In the Newcastle collieries, after the excavation of the coal, short trunks of trees drop down frequently from the roof of the mine, leaving vertical cavities, which the miners call pot-holes; these trees probably grew upon the surface of the vegetable mass by which the coal has been formed; and the occasional assemblage of large numbers of cones and seed-vessels of the same species, *e.g.* of Lepidostrobus and Trigonocarpum, upon one spot, seems to indicate that they dropped into their present place from the trees on which they grew.

Should the above hypotheses be correct, we may expect to find corresponding differences of organic structure on microscopic examination of the vegetable remains in the lower and upper portions of many beds of coal; and the attention of observers may at this time be profitably directed to the examination of thin slices of coal, carefully selected from different regions of the same bed, for the purpose of ascertaining whether differences exist between the component vegetables of the upper and lower regions of individual strata, sufficiently obvious and constant to justify us in referring the lower region of certain strata to a sub-aqueous, and the upper region to a sub-aërial origin. Should an entire bed of coal exhibit no other vegetable structure than that of Stigmaria, it may be inferred that these plants had not so far filled up the lagoon in which they grew, as to convert it to a sub-aërial swamp, before fresh floods of water from the land overwhelmed these sub-aqueous vegetables with sand and silt. Should we find another coal-bed without any Stigmaria, and interspersed through its whole vertical extent with Calamites and other sub-aërial plants, indicating a swampy soil, we may conclude that the vegetables which formed this bed of coal grew upon humid and swampy flats adjacent to lagoons; and that whilst the latter were accumulating beneath their shallow waters the materials of a future bed of coal, formed exclusively of the aquatic Stigmaria, the adjacent flats were simultaneously accumulating materials destined for a similar function from the sub-aërial swamp-plants of the same era. But in the compound case of coal formed by the conversion of a shallow lagoon into a morass, we should find in the lower portion, next above the fire-clay, no other plants than the aquatic floating Stigmaria, and in the upper region of the same bed no traces of Stigmaria, but many kinds of sub-aërial plants; whilst in its middle region we should discover a contact of aquatic with sub-aërial plants.

We may explain the frequent occurrence of erect trees immediately above the upper surface of a bed of coal, as in the cases we have spoken of near Bolton and Chesterfield, by supposing the roots of these trees to have found support and nutriment in the entangled remains of other plants which had preceded them on the same spot, as the Scotch firs grow in peat without touching any subsoil; but cases of trees thus standing erect are comparatively rare exceptions to their ordinary state of prostration, caused either by decay or tempests, or by the violence of the currents that submerged and buried with sand and silt the morasses in which they grew.

Fragments and large stems of trees that are found truncated at both ends, and inclined in all directions in thick beds of sandstone, like the coniferous trees at Craigleith and Newhaven, near Edinburgh, seem to have been torn from their native bed and drifted with the sand to the place in which they are now imbedded.

Mr. Logan and Mr. L. L. Dillwyn have discovered pebbles or rounded fragments of coal in certain grit beds of the coal formation, from which we learn that some of the older beds of coal had assumed an indurated state before the deposition of the more recent strata of this great formation, the total thickness of which in South Wales is 12,000 feet. At Penclawdd, on the Bury river near Swansea, Mr. Logan first found, in 1839, a rounded pebble of cannel-coal in a bed of clay; he subsequently discovered that in the Pennant grit of Kilvey Hill, near Swansea, there are many conglomerate beds containing pebbles of coal, intermixed with sand and pebbles of ironstone, and very rarely with boulders of granite and mica-slate. The pebbles are chiefly of common bituminous coal; two *only* have been found composed of cannel-coal, the *only* seams of which known in the lower coal-measures are 2000 feet below the Pennant grit. Mr. Logan believes that coal-pebbles occur throughout the whole mass of the Pennant sandstone, the thickness of which is 3000 feet, but he has seen no such pebbles in the lower coal-measures.

Mr. Buddle has lately found similar pebbles of coal in the Pennant grit of the Forest of Dean.

(To be continued.)

ROYAL IRISH ACADEMY.

January 11, 1841.—The Rev. Thomas H. Porter, D.D., read a paper "On the Deposits of Gravel in the Neighbourhood of Dublin."

After detailing the facts commonly known as to the stratified beds and ridges of limestone gravel, lying over the great central limestone region of Ireland, and the continuance of deposits containing a large proportion of rounded pebbles and stones of the same material, over the granite and other primitive rocks to the eastward of the limestone country, it was argued that there were clear indications of a great diluvial action from west to east, by which the surface of the limestone was reduced to its present level, and the remains of its upper portions spread over the limestone region itself, and carried eastward to the sea. The occurrence of similar calcareous deposits in the seaward glens and valleys of the Dublin and Wicklow mountains for some miles south, and on their sides to a considerable height, was ascribed to the current of the same deluge, sweeping the transported substances over the lower parts of the mountain range, and then turning southwards along the sea coast, after passing the north flank of the mountains. Similar facts, but in an inverted order, from south to north, have been observed towards the southern flank, in the County Wexford.

It was urged, that the subsiding waters of this inundation, rushing down the valleys, and meeting below with the main current on the plains, would throw up those ridges along the sides of the hills, and on the flats beneath; of which a remarkable example is presented in the glen of Ballynascorney (through which the Dodder descends from the Dublin mountains), and in the gravel hills in front of that, from Tallaght to Crumlin.

The direction assumed in this paper for the diluvial current agrees remarkably with that assigned by Professor Phillips as the cause of the distribution of the Shapfell boulders over the north-east of England. A conjecture was proposed as to the possible occasion of such a movement of water over the country. The limestone tract was evidently formed under the sea. Its elevation may have been connected with the last great convulsion, which determined nearly the present form of the surface. Great disturbances are seen at Killiney, the Scalp, &c., to have attended the appearance of the granite, and even to have followed that period, affecting the granite itself. Many parts of the Irish coasts present such abrupt terminations towards the sea, as to indicate either a violent raising of the island from a continuous tract at the bottom, or a sudden sinking of an extent of dry land around the present surface. Either of these events would create immense commotions in the waters.

Reference was made in the course of this argument to the theory of Professor Agassiz, respecting the supposed evidence, that glaciers once existed in the mountains of this island, and produced, as *moraines*, some of the accumulations of mountain debris commonly attributed to the agency of water. This theory having been pushed so far by some eminent British geologists as to have almost every ridge of gravel and stones unhesitatingly called a *moraine*, it was urged, that their principle could not be applied here at least, since the limestone abounding in the deposits of the glens could never have been brought down by ice from mountains in which no limestone rocks exist. It is but justice to Professor Agassiz to state, that *he* did not ascribe the limestone gravel ridges at Ballynascorney to a glacier, but professed to find the traces of one higher up the course of the stream.

Against the glacier theory, in general, it was maintained, that evidences of a glacier having existed in any locality must be derived from the existing form of the ground; and that, therefore, no considerable change of the surface could be admitted, since the time when the moraines were imagined to have been thrown up. More especially, no *deluge* could have taken place since their formation; for in that case the moraines must have been swept away. Hence they must be supposed to have existed between Noah's flood and the commencement of the historical periods. This interval, it was contended, would not allow time for their formation and disappearance.

A gradual change of the temperature of the whole northern hemisphere would be at variance with the fact established by geologists, that the heat of the earth's surface had been formerly much greater than now.

Had the degree of cold necessary for the formation of glaciers been owing to a greater elevation of this entire country, its sinking to its present level must have been attended with convulsions and floods, which could scarcely have failed to obliterate all vestiges of moraines.

An objection, brought from the known change of temperature in Greenland within modern times, was met by observing, that Greenland in its best days was always a land of glaciers, in the extent of which it is easy to suppose an occasional increase or diminution.

The reading of a paper by the Rev. T. R. Robinson, D.D., "On the Constant of Refraction, determined by Observations with the Mural Circle of the Armagh Observatory," was commenced.

A paper by Dr. Andrews of Belfast, "On the Heat developed during the Combination of Acids and Bases," was read, for an abstract of which see p. 183 of the preceding Volume.

January 25.—The Rev. Charles Graves, F.T.C.D., read a paper "On certain general Properties of the Cones of the Second Degree."

Let a sphere be described whose centre is at the vertex of a cone of the second degree, and through the vertex let two planes be drawn parallel to the planes of the circular sections of the cone; the curve formed by the intersection of the cone and sphere is called a *spherical conic*, and the two planes meet the surface of the sphere in two great circles which are called the cyclic arcs of the conic. These arcs, as M. Chasles has observed, possess properties relative to the conic exactly analogous to those of the asymptotes of a hyperbola. Moreover, many of their properties depend on the most elementary ones of the circle; but, as all the properties of cones, and therefore of spherical conics, are double, each theorem relative to the cyclic arcs furnishes a corresponding one relative to the foci of the supplementary conic, formed by the intersection of the sphere with a cone whose generatrices are perpendicular to the tangent planes of the cone on which the proposed conic is traced. And further, the theorems relating to spherical conics become applicable in general to the plane conic sections, by supposing the radius of the sphere to become infinite.

These considerations, for which we are indebted to M. Chasles, are calculated to direct the attention of geometers to the cyclic arcs of the spherical conics. In following this track, Mr. Graves has been led to many new and general properties of the cones of the second degree, amongst which the following deserve to be noticed:—

1. If two fixed tangent arcs be drawn to a spherical conic, and any third tangent arc be drawn meeting them in two points, the arcs passing through these two points and through the pole of a cyclic arc will intercept on that cyclic arc a portion of a constant length.

2. If from two fixed points in a spherical conic, arcs be drawn to any third point on the curve, and produced to meet one of the director arcs, they will intercept between them on that director arc a portion which will subtend a constant angle at the corresponding focus.

3. A spherical conic and one of its cyclic arcs being given, if, round the pole of this cyclic arc, as vertex, a spherical angle of variable magnitude be made to turn, whose sides intercept between them on the cyclic arc a portion of a constant length, the arc joining the points in which the sides of the moveable angle meet the given conic will envelope a second spherical conic: the given cyclic arc will be a cyclic arc of the new conic, and this arc will have the same pole with relation to the two curves.

4. A spherical conic and one of its foci being given, if round that focus, as vertex, a constant spherical angle be made to turn, and from the points in which its sides meet the director arc corresponding to the given focus, two arcs be drawn touching the given conic, their point of concourse will generate a second spherical conic: the given focus will be a focus of the new conic, and the corresponding director arc will be the same in the two curves.

5. If a variable spherical angle turn round a fixed point on the surface of a sphere so as to intercept between its sides a constant segment on a fixed arc, the arc joining the points in which its sides meet two other fixed arcs will envelope a spherical conic touching these two fixed arcs.

6. If a constant spherical angle turn round a fixed point on the surface of a sphere, the arcs joining the points in which its sides meet a fixed arc with two other fixed points will intersect in a point, the locus of which will be a spherical conic passing through these two last-mentioned fixed points.

If two tangents to a parabola intersect at a constant angle, the radii vectores drawn from the focus to the two points of contact will also contain between them a constant angle. But, as is well known, in any conic section, the point of concourse of the tangents at the extremities of two focal radii vectores, which contain between them a constant angle, will generate a conic section. Hence we deduce the following very general properties of spherical conics.

7. If two tangent arcs to a spherical conic intercept between them a segment of a constant length on a fixed tangent arc to the curve, their point of concourse will generate a second spherical conic.

8. If a constant spherical angle turn round a fixed point on a spherical conic, the arc joining the points, in which its sides meet the curve, will envelope a second spherical conic.

9. In theorem 7, if the segment intercepted on the fixed tangent arc be a quadrant, the point of concourse of the tangent arcs will move along an arc of a great circle.

10. In theorem 8, if the constant angle be right, the arc which it subtends in the spherical conic will pass through a fixed point.

The two following theorems may be obtained by the aid of the equation of a spherical conic, expressed in spherical coordinates:—

11. From two fixed points on the surface of a sphere, the distance between which is 90°, let arcs p, p' be drawn perpendicular to a moveable arc, and let a, β be arcs of a given length; if $\frac{\sin^2 p}{\cos^2 a} + \frac{\sin^2 p'}{\cos^2 \beta} = 1$, the moveable arc will envelope a spherical conic whose principal diametral arcs are $2\,a$ and $2\,\beta$; they will pass through the fixed points, and the centre of the conic will be the pole of the great circle passing through the two fixed points.

12. The base of a spherical triangle being a quadrant, if its base angles, a, b, be such that $\frac{\cot^2 a}{\tan^2 a} + \frac{\cot^2 \beta}{\tan^2 b} = 1$, where a and β are given arcs, the locus of the vertex will be a spherical conic, whose principal diametral arcs are $2\,a$ and $2\,\beta$; they will pass through the extremities of the given quadrant, and the centre of the conic will be the pole of the quadrant.

Some of the preceding theorems lead to new and very general properties of the conic sections: and one (No. 6) gives rise to a new and remarkably simple organic description of them. It should be observed that the arcs here spoken of are all arcs of great circles.

His Grace the Archbishop of Dublin having taken the Chair, the President continued the reading of Dr. Robinson's Paper "On the Determination of the Constant of Refraction by Observations with the Mural Circle of the Armagh Observatory."

The author remarks, that the problem of astronomical refraction is embarrassed by two causes of error. The differential of the refraction is obtained by supposing the atmosphere to consist of spherical shells concentric with the earth; and the integral of this, by assuming some mathematical relation between the height above the earth and the corresponding density of the air. He shows that the first of these cannot be rigorously true; and that the relation between density and height, besides being unknown in general, may be expected to vary with the latitude. He therefore considers all existing refraction tables as approximations which require correction for each individual observatory.

For about 74° from the zenith, the refraction is independent of the law of density, and requires only an exact knowledge of the air's refractive power; this, however, has not been yet obtained with sufficient accuracy by direct experiment, and therefore must be deduced from astronomical observations. At greater zenith distances some constitution of the atmosphere must be assumed, and if its expression contain a sufficient number of arbitrary constants, the resulting refraction can always be made to represent with sufficient exactness what is actually observed. As, however, neither the formula of Bessel, nor that of Ivory, very readily admits such modifications, Dr. R. used the method given by the late Bishop of Cloyne, in the twelfth volume of the Royal Irish Academy's Transactions, which, however, he has extended to 85° zenith distance.

If the atmosphere be supposed of uniform temperature the refraction has been computed by Kramp; it is found greater than the truth. If the density be supposed to decrease uniformly as the height above the surface increases, the refraction is given by Simson; it is nearly as much in defect as the other in excess, and it is found that their mean is very near the truth. If, then, the differential equation of refraction be developed in terms of the tangent of the apparent zenith distance, it is found, on integrating, that the first term belongs to an atmosphere bounded by parallel planes; the second depends on the equilibrium of the strata, and the others alone are affected by the assumed hypotheses. Their geometrical means are found to satisfy the Armagh observations as far as 85° zenith distance, below which the series ceases to converge, and the mean changes its relation to the true refraction according to the temperature and pressure. The expression thus obtained for refraction admits of being tabulated with the corrections for the thermometer and barometer in a couple of pages; and he thinks this form of tables more convenient than any other with which he is acquainted.

To compute them are required the expansion of air by heat; the ratio of the height of the homogeneous atmosphere to the radius of curvature of the meridian at the place of observation, and the refractive power of air at a given temperature and pressure. For the first he has used the value given by Rudberg, namely, that 1 of air at 32° becomes 1·365 at 212°. This differs from Gay-Lussac, but is identical with that deduced by Bessel from astronomical observations. The second is derived from the researches of Arago and Biot, corrected for the change of gravity from Paris to Armagh.

Of the refractive power of air there are different values of high authority. Denoting by the symbol μ the quantity $\frac{\sin \text{I} - \sin \text{R}}{\sin \text{R} \times \sin 1''}$ for 50° Fahrenheit and 29·60 inches pressure, the experiments of Arago and Biot give it 57·82. The observations of Delambre with a repeating circle give 57·72, which is also adopted by Brinkley. But the barometer used by this great astronomer is shown by Dr. R. to require the correction + 0·078, which would change the constant to 57·567; and as he also used the internal thermometer, perhaps a further diminution might be necessary. That of Bessel is 57·524, and that deduced by Dr. R. from his own observations is 57·546; but they cannot be exactly compared without a knowledge of the length of the pendulum at each station, as the measure of density given by the barometer depends on local gravity.

It was determined as follows by circumpolar stars. The refraction is obtained by subtracting from the subpolar distance 270° plus the declination observed above the pole. If the constant of refraction require a correction, it affects this declination both at the star and at the polar point, and the latter also affects the subpolar observation; hence, calling the tabular refraction μ v, and the difference between it and the observed d R, we have for each observation the equation of condition.

$$d\,\text{R} = d\,\mu \left\{ \text{v} - \text{v}' - 2\text{P} \right\} = d\,\mu \times \text{K};$$

combining which by minimum squares, the value of $d\,\mu$ for that star is obtained. If the values of it at different zenith distances are equal, or differ only by what may reasonably be considered error of observation, then it may be also inferred, that the formula correctly assigns the refraction through the range of zenith distance included by the observations.

Dr. R. then gives details respecting the mural circle which he used, the permanence of its microscopes as to *run*, and the mode of obtaining its index correction, and the correction of its divisions. When the stars are spectra, he bisects the yellow near the green, and remarks that the fluctuations of irregular refraction are often of considerable duration. The hygrometric state of the air does not seem to produce any effect, and he shows that the external thermometer is to be used at his observatory. The details of observation are then given for seventeen stars, from 77° 10′ to 84° 56′ zen. dist., of which there are 317 subpolar observations.

If a southern star be determined at a place when it passes near the zenith, so that its place may be assumed as free from error of refraction, the value of $d\,\mu$ is multiplied by a much larger factor. This advantage, however, is more than balanced by the uncertainty caused by the difference of instruments and local circumstances at the two observatories; but Dr. R. has given the result of such a trial. He used the declinations of Mr. Johnson (St. Helena Catalogue), and, in many instances, those of Mr. Henderson at the Cape, and by 241 observations from 77° 53′ to 84° 40′ he found for μ 57·586; but conceives the result obtained from the northern stars decidedly preferable, and has used it alone in computing the tables which are given at the end of the paper.

His Grace the Archbishop of Dublin communicated some observations "On the Leafing of Plants."

It is well known that there is a diversity in the times of leafing and shedding in individual trees of the same species; e. g. hawthorn, sycamore, horse-chestnut, beech, &c., sometimes as much as a fortnight; and the earliest in leaf are also the earliest shed, the same individuals keeping their time every year. Hence the question, whether this diversity arises from the "separable accidents" of soil, situation, &c., or whether from "inseparable accidents" which constitute what physiologists call *varieties*?

An experiment was tried by grafting an early hawthorn on a late, and *vice versa*. The scions kept their times (about a fortnight's difference) as if on their own stocks; thus proving that it was a case of "*seedling variety*."

Many other such varieties are known, not only of apples, peaches, &c., but of wild trees also, differing in shape of leaf, form of growth, colour and size of fruit, &c., and also *time of ripening*. It was therefore to be expected that there should be the like in respect of times of leafing.

This may throw some light on the question respecting "*acclimating*." It may be, that species may be brought to bear climates originally ill-suited,—not by any especial virtue in the seeds *ripened in any particular climate*, but—by multiplying seedlings, a few of which, out of multitudes, may have qualities suited to this or that country, e. g. some to cold, some to drought, some to wet, &c.

In some cases, a plant's beginning to vegetate later may secure it from spring frosts, which would destroy a precocious variety; in others, earlier flowering may enable a tree to ripen fruit in a climate in which a later would be useless, &c.

Further, the experiment shows that the common opinion respecting the commencement of spring vegetation,—the rise of the sap from the roots, through the trunk and branches to the twigs,—is groundless; since a scion of an early variety, on a late stock, will be in leaf while the stock is torpid.

LXV. *Intelligence and Miscellaneous Articles.*

ON GISMONDINE. BY M. KOBELL.

THIS mineral has been long known, but different opinions have been entertained as to its nature. According to Monticelli, the gismondine of Capo di Bove has the form of the octohedron and the rhombic dodecahedron, which is frequently elongated into a four-sided prism. Carpi found its composition to be—

Silica	41·4
Lime	48·6
Alumina	2·5
Magnesia	1·5
Protoxide of iron	2·5——96·5.

According to Brooke the crystalline system of this species is the prism with a square base, terminated by a pyramid, the angles of which are 122° 55′ on the summit, and 85° 2′ on the sides.

Since the discovery of the lime harmotome, gismondine has often been compared with it. Gmelin has included it in this species, and has concluded from his chemical researches, that it belongs also to zeagonite and abrazite. The identity of these latter minerals, as also of aricite and phillipsite with gismondine, has been shown crystallographically by Brooke, so that most mineralogists consider gismondine as a variety of lime harmotome. According to Kohler, the crystallization of the lime and barytes harmotome strongly resembles that of gismondine. M. Kobell analysed a pure specimen of gismondine from Capo di Bove; the crystals of this substance are brilliant and perfectly resembling those of harmotome, except that the re-entering angles. The angles of the pyramid with four faces are about 121°, as in harmotome. The angles of gismondine cannot be accurately measured, because the crystals are composite and are never very regular.

The hardness of gismondine is nearly equal to that of quartz, between 7 and 7·5; that of lime harmotome is between 4 and 5. The specific gravity is 2·18; gismondine dissolves readily and completely in hydrochloric acid; the mean of two analyses made by M. Kobell gave—

Silica	42·72
Alumina	25·77
Lime	7·60
Potash	6·28
Water	17·66——100·03.

The formula of which is $K\,Si^4 + 2\,C\,Si^3 + 12\,A\,Si + 15\,Aq$, or otherwise, $\left.\begin{matrix}C\\K\end{matrix}\right\}Si^3 + 4\,A\,Si + 5Aq.$

It is evident, according to M. Kobell, that Carpi either analysed a different mineral, or has given an incorrect result.

There are several analyses of lime harmotome; three are below stated; the two first are by Gmelin and Kohler, of the variety from Marbourg; the third is by Kohler of a variety from Canel.

	I.	II.	III.
Silica	48·02	50·445	48·222
Alumina	22·61	21·783	23·333
Lime	6·56	6·500	7·222
Potash	7·50	3·949	3·889
Protoxide of iron	0·18	...	...
Water	16·75	16·815	17·555
	101·62	99·492	100·221

The formula deduced from the above analyses is

$$\left.\begin{matrix}C\\K\end{matrix}\right\} Si^2 + 4\,A\,Si^2 + 6\,Aq.$$

Gismondine cannot therefore be confounded with harmotome, although it considerably approaches it. As to the crystallization, it is very possible that it belongs to the prism with a square base; although it resembles harmotome in its crystallization, it wants the re-entering angles, which are so common in the harmotome, and which are characteristic of its form.—*Annales des Mines*, tom. xvii.

ANALYSIS OF THULITE. BY M. C. G. GMELIN.

Pure thulite occurs in crystalline masses of a rose and blood-red colour, with quartz, fluor spar, and idocrase, coloured with oxide of copper, at Suland near Tellemark in Norway. Thomson found it to consist of—

Silica	46·10	
Oxide of cerium	25·95	
Lime	12·50	
Oxide of iron	5·48	
Potash	8·00	
Water	1·58	99·55.

M. Gmelin states that this extraordinary result induced him to analyse a very pure specimen of thulite, which he obtained several years since from Prof. Esmark of Christiana. He obtained—

Silica	42·808	
Alumina	31·144	
Lime	18·726	
Soda and trace of potash	1·891	
Oxide of iron	2·888	
Oxide of manganese	1·635	
Water	0·640	99·132.

This agrees perfectly with the previous analysis of Berzelius (*Comptes Rendus*, N. 12. S. 217), who does not find any oxide of cerium in thulite; Thomson must therefore have analysed a different mineral. According to the preceding analyses, Brooke and Levy (Phil. Mag. 1831, vol. x. p. 109) announced that thulite approximated to epidote, which is perfectly confirmed, for we have

Thulite. . . . . $(\dot{C}a, \dot{F}e, Mn)^3\ \dddot{Si} + 2\,\underline{\dddot{Al}}\,\dddot{Si}$

Epidote . . . . $(\dot{C}a, Fe)^3, \dddot{Si}^3 + 2\,\underline{Al}\,\dddot{Si}$

Zoisite . . . . $\dot{C}a, \dddot{Si} + 2\,\underline{\dddot{Al}}\,\dddot{Si}.$

Thulite, therefore, is only a variety of epidote.—*Annales des Mines*, tom. xvii.

[We may remark that the crystalline form of zoisite is the same as that of euclase, and has not a single face or cleavage in common with epidote.]—Ed. Phil. Mag., &c.

ANALYSIS OF POONAHLITE.

M. C. G. Gmelin remarks that this mineral belongs to the family of zeolites, and its appearance is similar to that of the mesotype species (skolerite, natrolite, for example). But whereas at present the minerals ranged in the mesotype species have the angle of the two faces of the prism 91° or 91° 38′, that of the poonahlite, according to the measurement of Brooke, is 92°, which induced him to consider this mineral as a peculiar zeolite, and to name it poonahlite from *Poonah*, the place at which it is found in the East Indies. The poonahlite occurs in very long prisms, and among several hundreds, none of the terminations were modified by planes; it occurs in druses in apophylite, and the rock which contains it. Its specific gravity is 2·2622.

The analysis gave as its constituents,—

Silica	45·120	
Alumina	30·446	
Lime	10·197	
Soda and traces of potash	0·657	
Water	13·386	99·806.

This agrees with the formula

$$3\,\dot{Ca}\,\dddot{Si} + 5\,\dddot{Al}\,\dddot{Si} + 12\,\underline{\dot{H}}.$$

Poonahlite is therefore a variety of mesotype, the formula of which has great analogy with skolerite and natrolite.

Poonahlite	$3\,\dot{Ca}\,\dddot{Si} + 5\,\dddot{Al}\,\dddot{Si} + 12\,\underline{\dot{H}}$
Skolerite......	$3\,\dot{Ca}\,\dddot{Si} + 3\,\dddot{Al}\,\dddot{Si} + 8\,\underline{\dot{H}}$
Natrolite......	$3\,\dot{Na}\,\dddot{Si} + 3\,\dddot{Al}\,\dddot{Si} + 6\,\underline{\dot{H}}$

Annales des Mines, tom. xvii.

ANALYSIS OF WASSER-GLIMMER. BY M. MORIN.

This mineral was found at the foot of a glacier of Mont Rose, in Zermanthal, two years ago; it has since been found at the Simplon and several other places in the Valais.

It has all the appearance of a mica, is lamellar, of a deep green colour, opake, except at the angles, feels greasy and is scratched by the nail; the faces in the direction of the laminæ are brilliant and green, the lateral faces are duller and black; by transmitted light parallel to the laminæ, a very marked ruby colour is observed.

The crystals are easily divided into very thin laminæ by the knife; they are flexible, but quite inelastic; the cleavage in any other direction is difficult, still it is ascertainable that the fundamental form is a right prism whose base is a parallelogram; two of the edges of the prism are usually replaced by a plane.

At 212° the wasser-glimmer loses no weight, but by calcination it loses much water; its powder is white, but by long calcining it becomes yellow. Hydrochloric acid partly attacks it with the evolution of chlorine, but sulphuric acid does not act upon it; it contains no alkali. The analyses made either with carbonate of potash or carbonate of barytes, gave the following results:—

Silica	34·8	
Alumina	10·2	
Lime	8·4	
Magnesia	1·8	
Protoxide of iron	18·0	
Deutoxide of manganese	5·0	
Water	14·4	98·9

The formula for this mineral is, therefore,

$$(Ca\,O,\ Mg\,O,\ Fe\,O)^6+(Si\,O^3,\ Al_2\,O^3,\ Mn_2\,O^3)^5+(H^2\,O)^8,$$

supposing all the manganese to be in the state of deutoxide, and the iron in that of protoxide. This formula is not simple, and probably the composition may be really represented by the following somewhat different formula:—

$$(Ca\,O,\ Mg\,O,\ Fe\,O)^3+(Si\,O^5,\ Al_2\,O^3,\ Mn_2\,O)^5+(H_2\,O)^4.$$

This mineral differs essentially from the micas principally by the large proportion of water which it contains.—*Annales des Mines*, tom. xvii.

ON SODALITE AND CANCRINITE.

The sodalite of Ilmengebirge is distinguished from other known varieties, especially by its generally very fine sapphire-blue colour, varying in intensity in different specimens: it is not crystallized, and is found only in small fragments. This mineral has the vitreous lustre and is transparent; it is not so hard as felspar; its specific gravity is 2·288. It gelatinizes and dissolves in hydrochloric acid. Before the blow-pipe it loses its colour, and fuses into a white glass containing bubbles.

According to Hoffmann's analyses performed in 1830, it is composed of—

Silica	38·40	
Soda	24·47	
Alumina	32·04	
Lime	0·32	95·23

The loss of 4·77 consists of chlorine: from this analysis the following formula is readily derived:—

$$\dot{N}^3\,\dddot{Si}+3\,\dddot{Al}\,\dddot{Si}+Na\,Cl^2,$$

a formula already given by M. Kobell, according to an analysis of Arfwedson's, of the sodalite of Vesuvius.

The cancrinite is very remarkable as a compound of a silicate and a carbonate. Like the sodalite it is found only in small masses; it cleaves readily in three directions, which meet at angles of 120°, parallel to the faces of a regular six-sided prism.

This mineral is of a light red colour, transparent, of a vitreous lustre on the cleavage faces, and of a greasy lustre in the other directions; its hardness is between that of apatite and felspar; specific gravity 2·453.

In hydrochloric acid, cancrinite dissolves with effervescence and gelatinizes; it becomes a white bubbly glass before the blow-pipe. By calcination it lost 6·18 per cent.; the residue effervesced but slightly in hydrochloric acid, but still it gelatinized. It yielded by analysis,—

Silica	40·59	40·26
Soda	17·38	17·66
Potash	0·57	0·82
Alumina	28·29	28·24
Lime	7·06	6·34
Loss	6·11——100	6·68——100

The alumina contains a little oxide of iron; and it also contains a trace of chlorine, which also exists in the elæolite of Ilmengebirge.

The formula, according to this analysis, is

$$\dot{N}^3 \dddot{Si} + 3\,\dddot{Al}\,\dddot{Si} + \dot{Ca}\,\ddot{C}.$$

The cancrinite is thus a combination of elæolite with carbonate of lime, as the sodalite is a combination with chloride of sodium.—*Annales des Mines*, tom. xvii.

[*Note.*—All the specimens which have been received in this country named cancrinite are *blue.* The specimens named sodalite, are reddish and opake.—Ed.]

ANALYSIS OF HAÜYNE. BY M. F. VARRENTRAPP.

Haüyne is found in small and large grains in the porous basalt of Nieder-Mendig, near Andernach on the Rhine. When this mineral is treated with hydrochloric acid, it soon becomes evident that a metallic sulphuret is decomposed, and it is important to ascertain the proportion of sulphur; to effect this the powdered mineral is put into a retort connected with a Woulfe's apparatus containing a solution of chloride of copper, and hydrochloric acid is to be poured upon the mineral previously moistened: the hydrosulphuric acid expelled precipitates the copper from the chloride in the state of sulphuret. When this operation is over, the solution remaining in the retort is diluted with water, and the silica is separated in the usual way, and the sulphur, using the requisite precautions, is determined by the quantity of sulphuret of copper obtained. The chlorine is ascertained by decomposing a small portion of the mineral with nitric acid; the sulphuric acid is precipitated by chloride of barium.

The results obtained were the following:—

Sulphuric acid	12·602
Silica	35·012
Soda	9·118
Lime	12·552
Magnesia	27·415
Sulphur	0·239
Iron	0·172
Chlorine	0·581
Water	0·619——98·310

Annales des Mines, tom. xvii.

ON THE PRODUCTS OF THE ACTION OF POTASH ON INDIGOTIN —CHRYSANILIC ACID.*

M. Fritzsche states that when indigotin is treated with a hot concentrated solution of potash, the liquor becomes a crystalline

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* See preceding volume, p. 191, and present volume, p. 35.

mass on cooling. If the crystalline mass, separated by pressure between two bricks from the excess of the alkaline solution, be treated with water, traces of unaltered indigotin are precipitated, and there remains in solution a salt of potash containing a new acid, which the author has named *chrysanilic acid*; when the chrysanalate of potash is decomposed by an acid, the new acid is precipitated in red flocculi. The author has examined the compounds which it forms with bases, and has submitted them to analysis; he does not, however, consider his analytic results as satisfactory; but he has ascertained that 0·250 gramme of indigotin yielded 0·203 gramme of the acid; that is to say, more than 80 per cent: when chrysanilic acid is treated with dilute sulphuric acid, it yields a bluish red liquor, which deposits a crystalline body of a blackish blue colour, and retains in solution a new acid, which the author calls *anthranilic acid*. This acid may also be formed by the action of the air on chrysanalate of potash.

Anthranilic acid crystallizes in scales; it is slightly soluble in cold water, but very soluble in alcohol and æther; it fuses at 276° Fahr.

The anthranilates of potash, soda, and ammonia, are soluble and crystalline; the metallic salts are precipitated in small crystals; this acid is decomposed by heat into carbonic acid and anilin, which has been previously described by the author, but he has not yet determined the composition of the blackish blue substance deposited from the liquor resulting from the action of sulphuric acid on chrysanilic acid; it appears to reproduce indigotin under certain circumstances.—*Journal de Pharm. et de Chim.*, Jan. 1842.

ON RECENT CONGLOMERATE FORMED ON THE SEA-COAST AROUND IRON. BY H. N. NEVINS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

An account appeared some years ago in your Journal (vol. x. p. 10.) of an anchor found at Seaton encrusted by sand and pebbles.

At Tramore, a bathing-place within six miles of this city, such a substance is frequently observed on the sandy beach, but generally containing a nail or portion of iron from which no probable date can be derived. However, some time since, I picked up a piece of conglomerate, composed of sand and the common rolled pebbles, and strongly cemented by oxide of iron,—my attention having been arrested by the latter circumstance, as otherwise it had the appearance of a rolled stone from the cliffs to the eastward.

The first thing that attracted me on cracking it was a strong bituminous smell, which on carefully opening the mass I found to proceed from the lackering of a large padlock firmly imbedded in and completely hidden by the conglomerate.

The outside of the lock is much decayed, and requires the greatest care to prevent it from falling to pieces.

The crust of sand, &c. varies in thickness from two inches to a quarter of an inch; and but very little having penetrated into the works, the construction of the padlock is quite exposed.

Wishing to ascertain its probable age, I showed it to a person who has been an ironmonger for nearly fifty years, and by him the lock was pronounced such as was frequently brought to be repaired about the time of his commencing business. The bolt is round; the key must have been without a pipe, similar to a door-key, and the form of the lock is different from that now made.

The complete preservation of the lackering—a circumstance in itself worth notice—has led me to suppose that the lock was nearly new when the formation around it commenced.

Waterford, March 15th, 1842. I remain, &c., HUGH N. NEVINS.

SCIENTIFIC MEMOIRS, Part X., is just published, containing,—Gauss, on Forces acting inversely as the square of the distance; Prof. Dove of Berlin, on the Law of Storms; Dove, on Non-periodic Variations in the Distribution of Temperature on the Surface of the Earth; Liebig, on the Azotized Nutritive Principles of Plants; Cauchy, on the Theory of Light; Bunsen, on the Cacodyl Series; Ehrenberg, on numerous Animals of the Chalk Formation which are still to be found in a living state.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1842.

Chiswick.—March 1. Showery: clear. 2. Overcast: stormy and wet. 3. Cloudy. 4. Overcast. 5. Clear and very fine. 6. Slight haze: very fine. 7. Cloudy: slight rain. 8. Very fine: heavy rain. 9. Stormy with heavy rain. 10. Boisterous: clear at night. 11. Densely overcast. 12, 13. Cloudy and fine. 14. Drizzly. 15. Hazy: very fine. 16. Hazy: overcast. 17. Fine: stormy and wet. 18. Cloudy and fine. 19. Clear and cold: stormy with hail. 20. Cloudy: showery: squall in afternoon. 21. Stormy: showers. 22. Cloudy: rain. 23. Very clear: hail-shower. 24. Hazy. 25. Cloudy: stormy with rain. 26. Clear: cloudy. 27—29. Very fine. 30. Rain. 31. Rain: cloudy: boisterous with rain at night.—The mean temperature of the month was nearly 3° above the average.

Boston.—March 1. Rain: rain early A.M.: rain P.M. 2. Cloudy. 3. Rain: rain early A.M. 4. Cloudy: rain early A.M. 5—7. Fine. 8. Cloudy. 9. Fine: rain P.M. 10. Stormy: rain early A.M. 11, 12. Cloudy. 13. Fine: rain early A.M. 14. Cloudy. 15. Fine. 16. Cloudy: rain at night. 17. Fine: rain at night. 18. Windy. 19. Windy: rain at night. 20. Cloudy: rain A.M. and P.M. 21. Windy. 22. Fine: rain P.M. 23. Fine: snow A.M.: rain P.M. 24. Cloudy: rain A.M. 25. Fine. 26. Windy. 27. Fine: rain P.M. 28. Fine. 29. Fine: rain early A.M. 30. Cloudy. 31. Rain: rain early A.M.

Sandwick Manse, Orkney.—March 1. Cloudy: aurora. 2. Clear. 3. Cloudy: rain. 4. Clear. 5. Cloudy: showers. 6. Bright: cloudy. 7. Cloudy. 8. Shower: clear. 9. Clear. 10. Sleet-showers: clear. 11. Showers: rain. 12. Bright: clear. 13. Hail-showers: clear. 14. Cloudy: clear. 15. Cloudy: aurora. 16. Cloudy: rain. 17. Clear: rain. 18. Cloudy: showers. 19. Cloudy: large hail. 20. Showers. 21. Cloudy. 22. Snow-showers: cloudy. 23. Cloudy: drizzle. 24. Drizzle. 25. Showers: sleet. 26. Snow-showers. 27. Snow-showers: cloudy. 28. Rain: showers. 29. Sleet-showers: showers. 30. Cloudy: showers. 31. Damp.

Applegarth Manse, Dumfries-shire.—March 1. Cold, with slight showers. 2. Shower of snow: rain. 3. Heavy rain. 4. Showers. 5, 6. Fair and mild. 7. Slight showers: rain P.M. 8, 9. Hail-showers. 10. Hail-showers, with snow. 11. Heavy rain. 12. Fair but threatening. 13. Fair. 14. Rain all day. 15. Mild and moist. 16. Drizzling: rain P.M. 17. Fair A.M.: heavy rain P.M. 18. Showers. 19. Slight showers. 20. Fair: snow on the hills. 21. Fair and clear. 22. Slight snow: frost. 23. Frost: fair. 24. Fair and fine: slight frost. 25. Wet P.M.: violent wind. 26. One slight shower. 27. Frost A.M.: rain P.M. 28. Rain P.M. 29. Fair. 30. Showery. 31. Wet A.M.: cleared up.

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary*, Mr. Roberton; *by* Mr. Thompson, *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse*, DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.								Thermometer.										Wind.					Rain.					Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8½ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R. S. 9 a.m.
1842. Mar.		Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mm.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.											
1.	29·236	29·588	29·226	28·83	28·97	29·11	28·79	28·96	49·8	50·5	42·0	51	34	47	45	34½	41	41	s.	w.	calm	sw.	w.	·047	·02	·09	......	·04	46
2.	29·770	29·766	29·701	29·33	29·25	29·35	29·21	29·43	43·7	51·0	37·8	52	49	38	40	33½	40	40	s.	sw.	w.	sw.	nw.	......	·41	·04	......	·05	40
3.	29·916	29·856	29·839	29·35	29·50	29·38	29·66	29·35	52·3	52·6	43·7	57	41	51	46	37	39½	36	ssw.	sw.	calm	sse.	e.	·402	·01	·28	......	......	47
☾ 4.	30·008	29·957	29·918	29·43	29·62	29·65	29·38	29·55	45·3	55·8	43·8	48	29	47	45½	35	41	41	sw.	w.	calm	sw.	w.	......	......	·10	......	·91	45
5.	30·042	30·036	30·008	29·52	29·75	29·80	29·73	29·81	41·3	50·6	39·0	54	27	42	49	35½	41	40½	s.	w.	calm	wsw.	sw.	......	......	......	......	·06	42
6.	30·010	29·992	29·813	29·59	29·79	29·60	29·78	29·55	42·0	52·2	39·2	54	26	40·5	51	39	43	42	ssw.	se.	calm	sse.	s.	......	......	......	......	·12	41
7.	29·750	29·720	29·477	29·26	29·37	29·10	29·35	29·07	44·3	51·0	39·0	54	46	40	45	35	42	43	s.	s.	calm	sse.	se.	......	·02	......	0·93	......	40
8.	29·528	29·500	29·416	28·97	29·17	29·20	28·93	29·15	51·5	51·8	44·3	57	35	48	47	38	41	38	s.	sw.	w.	sw.	sw.	......	·17	......	......	·03	46
9.	29·660	29·606	29·152	29·13	29·20	29·10	29·16	29·10	43·3	54·6	39·0	48	37	41	42½	32	39	36	s.	w.	calm	sw.	s.	·150	·47	......	......	......	42
10.	29·574	30·018	29·463	29·04	29·33	29·72	29·22	29·55	41·5	49·4	38·6	47	31	37	44	33	38	39	w.	nw.	nw.	w.	sw.	·455	·01	·72	......	·06	40
11.	30·040	30·040	29·825	29·55	29·55	29·40	29·39	28·51	45·3	48·6	40·0	50	27	41	45½	35½	39	43½	s.	s.	calm	sw.	s.	......	......	......	......	·14	40
● 12.	30·094	30·045	29·938	29·59	29·78	29·58	29·66	29·50	44·3	53·6	41·3	58	40	41	48	36	44	41½	w.	sw.	calm	sse.	w.	......	·06	......	......	·23	42
13.	29·940	30·132	29·888	29·39	29·65	29·89	29·53	29·84	46·3	56·0	43·7	57	37	46	49	37	43½	41	w.	w.	w.	w.	sw.	·094	......	·03	......	·18	44
14.	30·276	30·284	30·223	29·82	29·95	29·98	29·82	29·86	45·4	51·7	44·2	49	45	41·5	49	38	44	46	s.	s.	calm	sw.	se.	·022	·02	......	......	......	43
15.	30·398	30·332	30·324	29·84	30·08	30·10	29·94	30·03	49·5	50·8	45·3	54	46	52	49	44	51	46	s.	sw.	calm	sw.	sw.	......	......	......	0·50	......	46
16.	30·360	30·316	30·138	29·82	30·05	29·83	30·09	29·96	51·3	54·6	49·8	56	44	51	48	43	46	42	s.	sw.	calm	sw.		......	......	......	......	......	49
17.	30·188	30·131	29·761	29·63	29·89	29·32	29·81	29·33	50·3	52·7	47·4	56	43	46	46½	36½	42	42½	w.	sw.	calm	ssw.	sw.	......	·05	·12	......	·14	48
18.	29·792	29·739	29·609	29·19	29·30	29·24	29·10	29·01	48·7	55·3	45·7	52	35	45	48	37½	44	42½	w.	w.	w.	w.	sw.	·041	......	·04	......	·15	48
☽ 19.	29·558	29·562	29·280	29·06	29·12	28·98	29·07	29·23	43·6	52·3	39·0	46	40	41	44	33	43½	40	w.	sw.	w.	w.	w.	......	......	......	......	·07	40
20.	29·298	29·638	29·400	28·95	29·30	29·63	29·51	29·90	41·7	46·7	41·4	48	38	41·5	45	33	40	37	nw.	nw.	n.	nw.	n.	·080	·10	·13	......	·05	42
21.	29·890	30·157	29·890	29·52	30·01	30·16	30·23	30·20	41·7	48·0	39·4	49	34	43	46	34	39	38	nw.	n.	n.	n.	n.	·105	·08	·16	......	·02	39
22.	30·158	30·151	29·951	29·78	30·00	30·06	30·16	30·32	39·5	46·4	36·9	48	30	37·5	47½	31	40	36	nw.	n.	calm	n.	n.	·091	·08	......	......	·08	36
23.	30·124	30·183	30·102	29·75	30·18	30·15	30·26	30·15	39·5	45·2	34·3	46	28	39	45½	30½	40½	45	n.	n.	calm	nw.	w.	·094	·03	·09	0·55	......	35
24.	30·240	30·221	30·167	29·85	30·10	30·10	30·16	29·98	37·8	43·2	33·6	46	34	37	57	33	46	44	n.	w.	n.	wnw.	w.	·025	......	·05	......	·04	35
25.	30·128	30·130	29·607	29·69	29·82	28·98	29·52	29·02	44·7	45·2	37·9	54	36	45	51	39½	44½	33	sw.	w.	calm	w.	w.	......	·07	·03	......	·05	39
○ 26.	29·636	29·629	29·581	29·12	29·26	29·42	29·14	29·34	44·8	53·4	39·8	50	29	42·5	45½	34	34	35	w.	nw.	w.	wnw.	nw.	·086	·01	......	......	·74	40
27.	29·774	29·763	29·746	29·34	29·52	29·46	29·46	29·49	43·3	49·0	36·0	52	41	41·5	46	30½	37	36	w.	w.	calm	w.	nw.	......	·02	......	......	·02	40
28.	29·720	29·740	29·704	29·25	29·37	29·27	29·16	29·24	50·3	50·8	43·0	60	47	48	54	40	47	41½	w.	w.	calm	sw.	sw.	......	......	·06	......	·44	43
29.	29·912	29·921	29·860	29·38	29·61	29·70	29·32	29·56	50·2	58·5	49·2	59	48	48	49	38½	41	43	w.	w.	calm	sw.	w.	......	......	·03	......	·26	46
30.	29·896	29·914	29·836	29·32	29·46	29·57	29·43	29·34	50·7	57·4	49·2	59	42	51	53	40½	45	43	s.	sw.	calm	s & w.	s.	·016	·05	......	0·76	......	48
31.	29·780	29·795	29·426	29·26	29·22	29·20	29·18	29·22	48·8	57·4	45·6	56	43	45	55	40	41½	40	sse.	sw.	w.	w.	n.	·069	·13	·11	......	·10	47
Mean.	29·893	29·931	29·718	29·40	29·584	29·549	29·520	29·501	45·6	51·5	41·6	52·48	37·48	43·7	47·6	36·0	41·87	40·41						Sum. 1·777	1·81	2·08	2·74	3·98	Mean. 42

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[THIRD SERIES.]

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LXVI. *Some Observations on the Action of Light on Revolving Discs. By* A. MILWARD, *Esq.**

1. THE object of this paper is to explain some interesting effects produced by the action of light, and especially electric light on discs in rapid revolution, figures or representations of various kinds being depicted on their surfaces.

2. The general action of light in producing upon the retina the image of any object when in a state of rest, consists in a number of successive waves impinging upon all parts of the object, and being reflected thence to the eye. Each wave reflected from each portion of the surface, produces by itself a minute effect: and a series of such successive waves would evidently increase such effect, in a degree dependent upon the constitution of the eye. It is found by experiment that the retina is capable of retaining an image impressed upon it; or in other words, that the nerves of the retina continue their vibration for a definite period of time, which we will represent by n''; if $\therefore$ a'' be the interval between the waves, and n'' the time of duration of the first wave of a system, the duration of the effect of the successive waves before the extinction of the first, will be $(n''-a'')$ $(n''-2\,a'')$ &c. ...... $(n''-)\,(n-1)\,a''$, and at the completion of n'' the $\frac{n}{a}$ waves will have produced on the retina the greatest effect which it is capable of receiving, since the further addition of waves will be compensated for by the corresponding loss of all those anterior, to the $\frac{n\text{th}}{a}$ wave from the last. Hence the time required for the formation of

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* Communicated by the Author.

the most perfect image will be equal to the duration of the effect of a wave of light on the retina.

3. We will begin by directing our attention to the effect of a continuous light upon revolving wheels. In this case we may consider the successive changes through which the surface passes until it comes again into the situation which it at first occupied, as constituting an infinite number of positions whose differences are indefinitely small, *i. e.* that a whole revolution comprises an infinite number of such positions. The action of a continuous light, on the contrary, is made up of a series of successive waves of appreciable length, and separated by a definite though exceedingly small interval. Now a small number of waves of light acting successively will affect the same nerves of the retina, or nearly so, and thus produce either the *same* image, or one differing from it merely by being attended with a certain amount of confusion; and thus will act in the same manner as though the representations on the disc were confused in their outline. During the time of the duration of this small system of waves, the disc will pass through a small portion of a revolution which we will represent by $\therefore \frac{1}{m}$, and consider the number of positions producing images on the retina during a revolution of the disc as m.

The duration of the system of waves above mentioned being very minute, we will consider that one image is impressed on the retina at *one* definite epoch during the mth part of a revolution, and neglect the modifications arising from the fact, that the image is the result of *several* successive waves. If therefore the images thus impressed upon the retina remained only during an indefinitely short space of time, each position would be appreciated by the eye, and the wheel would be distinctly seen during its motion. We know, however, that such is not the case; each image impressed upon the retina remains there for an appreciable length of time, during which the wheel moves through a number of the positions above described. Thus where the light is continuous, each system of waves impresses the image of a position on the retina, and a number of these positions being depicted at once upon the optic nerves, confusion will of course occur. The amount of this confusion, from the nature of the case, depends upon the velocity of the wheel. The effect of the motion is to reflect to the eye a number of successive positions, producing different effects at the same time, and the intensity of each image so produced, diminishes as the lapse of time, from its first impression, becomes greater, and vanishes when that time is equal to the retention of the image on the retina (i. e. n''). It is evident, that if the last-mentioned time be equal to the period in which the wheel revolves, all the positions of the wheel (m) will be impressed upon the retina at once, and the confusion will be very great. Thus when the motion is slow, the action on the eye may be considered as caused by groups of mth positions* whose duration is n'', and each of which gives an indistinct and complicated image. The length of these groups is evidently the space passed through during the time of the duration of the first image on the retina. As the motion therefore becomes slower a less number of mth positions is contained in each group, the length of the group (or the range of confusion) becomes less, and the confusion decreases. We may here perhaps mention the operation of another cause which interferes with the principle we have been considering†.

We have in the former part of the subject supposed the axis of the eye to be stationary; but we find by experience that this is not the case. When a wheel moves very slowly the eye follows some particular part which attracts attention, so that by the spontaneous action of the muscles of the eye the axis of the optic lens is always directed towards that particular part, and the images may be reflected to it nearly in the same manner from each portion (according however to the method in which the experiment is conducted). Both the eye and the wheel being thus in similar motion, the effect is in some degree the same as if the wheel were at rest. This disturbing influence appears to exist in all experiments in which motion is admitted, but must from necessity decrease very rapidly as the velocity of the wheel becomes greater.

We will now consider the effect of light upon revolving wheels, when the duration of the former is very short. In this case we perceive clearly that if the light consist only of one system (such as we have supposed), or in other words, do not exceed the time of the mth part of a revolution, one image only will be reflected to the eye, which, according to our previous hypothesis, will be accompanied with only a slight degree of confusion; and thus we have the general principle, that a light of sufficiently short duration produces a clear image of the entire revolving wheel, and at the same time that as the duration of light increases, the number of images of

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* By the term mth position is meant the group of positions contained in the mth portion of a revolution, and reflecting nearly the same image to the eye.

† An interesting illustration of the confusion above mentioned, is afforded in a railway train moving along a raised embankment when the circumferences only of the wheels are distinguishable, and the wheels thus partially seen appear at rest, and to be gliding along the line.

*m*th positions will be likewise augmented, and confusion will in proportion arise.

It is on these principles that a popular toy has been constructed, by which various deceptions are represented in a pleasing manner. It consists of two circular discs, on one of which are represented figures, or other objects, in various positions; while the other, which is larger, has a number of small holes pierced at equal intervals around its circumference. Both are screwed firmly together (the first disc being on the outside) and held before a plane mirror; whilst the eye is directed towards the reflected image through the apertures in the circumference, the whole being put in rapid revolution, the figures appear in motion as the images of the different and slightly varying positions depicted on the disc are successively reflected through the revolving apertures to the eye. The cause is easily explained. The holes perform the office of dividing the continuous light reflected from the mirror, into a certain number of systems of very short duration. It is evident that we may consider the experiment as resolving itself into that of two discs facing one another, and revolving simultaneously, the one containing the figures and represented by the image on the glass, the other provided with the apertures. As the second disc revolves, a few images of *m*th positions pass through each of the apertures during its transit; and thus each hole provides the eye with a small group of positions, and these groups are separated by the interval between the openings. Thus a confused image of the wheel is reflected to the eye through each of the openings, and remains there undisturbed, until as its effects upon the eye become very weak, it is replaced by another indistinct image passing through the succeeding aperture and slightly different in position. In this manner images replace one another upon the retina, as succeeding sounds in music do upon the ear. It is evident, that if there be as many distinct forms on the disc as there are openings, each holding the same relation to the apertures as to position, the forms will appear stationary, in consequence of the repetition of the same image upon the retina through each of the apertures.

Our attention may now be directed to the application of the above principles to the purpose of proving the extreme velocity of electricity, which is often said to be effected by causing a disc to revolve with considerable velocity, and to be then illuminated by a flash from a Leyden jar, when it is seen in an apparent state of rest, with the whole of its surface distinctly visible. Referring to the principle which we have previously considered, viz. that a light of very short duration must produce a clear image on the retina, which will of course convey no idea of motion, we shall immediately perceive from the experiment in question, that the duration of the electric spark does not exceed the period of revolution of the disc through an exceedingly small space (that is through the *m*th part of a revolution); and if it can be proved that the electric fluid passes through a definite space during the exceedingly short duration of the light, it follows that it moves with a velocity bearing a known relation to that with which the circumference of the disc describes the *m*th part of a revolution. It will be seen, however, that by this experiment nothing more is proved than that the duration of the light is exceedingly short. The main point depends upon the consideration of the actual and definite motion of the fluid during that short duration; and this I apprehend cannot be proved by the spark from a Leyden jar; for though that spark evidently arises from the motion of the fluid, we have no proof at all that the fluid has moved during the interval for which the spark is in existence, and it is upon this point that the value of the experiment must depend.

The case is different when the electric discharge takes place in the form of a stream of light; for here we have good reason to believe that the fluid causes a succession of sparks along the line of its progress; and consequently that it has described the space between the first and last spark, or the two extremities of the apparent stream, during the collective duration of the continuous light. If therefore such a light produces the same effect as the spark of the Leyden jar when tested by the wheel, we have high presumptive evidence that the fluid has moved through the length of the apparent stream of light, while the disc has described only the *m*th part of a revolution. The same is the case with a flash of lightning, which is the agent mentioned by Mrs. Somerville. It is probable, however, that the experiment has never been actually tried. I have termed the evidence of the above-mentioned motion only highly presumptive, because in our present uncertainty as to the manner in which the electric light is produced (whether by the compression of the medium, through which it passes, or otherwise) we have no absolute proof that the light and motion are connected in the manner which we have supposed. However this may be, it would appear that the experiment when performed with the Leyden jar proves nothing, the main point being left out of consideration.

I may here mention another experiment, the aim of which according to some persons is to prove the velocity of the electric fluid,—I refer to that of passing the electric current along a system of wires with various interruptions, when the sparks must theoretically occur in succession, but which, in consequence of the velocity of the fluid, all occur at the same time. This however only proves that the eye cannot estimate the lapse of time between the several sparks, and confounds them together; or in other words, that the time of passing from one extreme of the wire to the other, is less than the duration of the effect of the first spark upon the retina. But this would not show that the velocity of the fluid so greatly surpasses that of ordinary agents, since the same effect may be produced by a velocity proportionally moderate.

It seems evident that by combining the two experiments we shall obtain a satisfactory test of the velocity in question; for if we illuminate the disc by a succession of sparks passing from distant parts of an interrupted wire of considerable extent, we shall be able to ascertain whether the sparks do or do not succeed one another with such extreme rapidity, since, as we have previously observed, the disc acts as a delicate test of the duration of light. If the lapse of time between the first and last spark do not exceed the period in which the wheel describes the *m*th part of a revolution, the whole of the sparks will be effective in reflecting only one of these *m*th positions to the eye, and therefore the image will be comparatively clear and unconfused; but should this prove otherwise, *i. e.* should the velocity of the wheel be less than the above hypothesis premises, a multiple number of *m*th positions would be acting at once, though in different degrees, upon the retina, and confusion would to a greater or less extent arise.

We may consider also, that by increasing the velocity of the wheel and the distance between the points of interruption of the wire, an attempt might be made to obtain an approximate value of the velocity of the electric fluid, for we may suppose the length of the wires to be so greatly increased that the sparks proceeding from them should produce a confused image of the disc, while a less length of wire affords comparatively no confusion; thus we shall have as a limit a length of wire greater than the space which measures the electric velocity; and since we may estimate within certain limits the space which we term the *m*th portion of a revolution, we can establish an approximate ratio between the velocities of the disc and the electric fluid; thus by knowing the velocity of the former, which may be mechanically measured, the approximate value required may be obtained.

A beautiful experiment somewhat similar to the combinations which we have just considered, has been the result of Professor Wheatstone's exertions connected with this subject.

The light is afforded by means of an extended wire of considerable length, from interruptions in which sparks are emitted. Immediately opposite this is placed a circular disc obliquely inclined to its axis, so that no two of its positions are in the same plane, and two successive rays of light will be reflected in different lines. Hence it is evident, that unless the sparks emitted from the parts of the wire be separated by so indefinitely small an interval of time that the wheel cannot in that time move through an appreciable space, the light thus falling on the disc will be reflected in different directions, and thus we have a test of the utmost delicacy.

It has been suggested that this experiment may be applied to the purpose of measuring the velocity, and with much greater chance of success than that which I have previously mentioned; for if the light be reflected from the disc upon a screen, the space between the projection of the rays upon it may be measured; and since the position of the screen with relation to the disc is known, we may calculate the angle of revolution actually passed through by the latter. If therefore the velocity of the wheel be known, the exceedingly short period in which it passes through the angle just determined, may be likewise calculated; and it is evident that this is the period required by the electric fluid for its passage along the wire, by which means the velocity will be determined.

Let us now turn our attention to an experiment of a very beautiful description, which is more immediately connected with our subject. Of the name of the author I am not aware, but I once saw it performed by Mr. Janson of Pennsylvania, near this city, and was particularly struck with it; for from the concealed nature of the cause it appears at first sight almost inexplicable. It consists in illuminating a rapidly revolving disc by means of the light emitted in the circuit of Clarke's magnetic electrical machine, when a singular effect is produced which varies according to the velocity with which the machine is turned. The two velocities may be so adjusted, that the disc though in rapid revolution appears to be absolutely at rest, and its surface distinctly visible. By decreasing the velocity the disc appears to move slowly *forward*, while by increasing it a *retrograde* movement appears to take place.

The cause of this depends entirely upon the nature of the light. When the machine moves slowly we perceive that a succession of sparks is emitted at the surface of the mercury; but as the velocity becomes greater, the light appears continuous, in consequence of the duration of the effect upon the retina; and here, as in an experiment previously mentioned, the disc acts as a test, when the unassisted powers of vision are incapable of appreciating the interval between the sparks.

Let us first consider the case in which the disc appears stationary. By referring to our previous principle, that a spark of very short duration produces a clear image of the disc, as reflecting only one *m*th position, we may consider the effects produced by a succession of such sparks, the intervals between which are definite and equal: and here it is evident that each spark reflects an image to the eye which will be separated from that which succeeds it, by the space through which the wheel passes during the elapsed interval*. Now if this interval be exactly equal to the time in which the disc performs an entire revolution, it is evident that each spark as it is produced will reflect the same image to the eye, and always in an identical position, so that the disc must appear stationary. Thus, in the first case, the phænomenon arises from the interval between the sparks being exactly equal to the time in which the wheel revolves. In the second case, it is clear, that if during the interval between the sparks the disc describe a space exceeding a revolution by a very small quantity, each spark will reflect to the eye the image of a position slightly in advance of the preceding, and thus the effect will be that of a disc, which in place of revolving equally, is seen only in successive positions, separated by the small quantity above mentioned, and the interval between which will be equal to the lapse of time between the successive sparks. The cause of its distinctness depends not only upon the principles which influence a disc in slow motion, viz. the small number of *m*th positions contained in a group, by which the range of confusion is decreased, and also the disturbing effect of the optic muscles, by which the eye spontaneously follows the revolving image, but also upon the advantage of the length of time elapsing between the sparks (and therefore between the formation of the images), which permits the effect of the last image to decrease in intensity before its place is supplied by another; thus diminishing the confusion. Thus in the second case, the velocity of the wheel being greater than the interval between the sparks, the effect becomes that of a slowly revolving wheel, whose motion is direct.

By the same reasoning it is evident, that if the time of revolution be slightly greater than the interval between the sparks, the space described will fall short of a revolution by a very small quantity, and the effect will be the same as in the

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* I speak here of only one revolution, but it is evident that the phænomenon will be the same if the wheel perform two or more revolutions in the interval between the sparks: so also in the other cases.

second case, except that the motion will be retrograde: and the interval of time between the occurrence of the positions will be diminished by the time of describing the small quantity just mentioned, instead of increased thereby.

It is clear, from the nature of the phænomenon, that unless the excess or deficiency be very small, the continuous image will not be distinct, but will have a flickering and irregular appearance.

Exeter, March 4, 1842.

LXVII. *On the Influence of Atmospheric Currents upon the Height of the Barometer.* *By* WILLIAM BROWN, Jun.*

IN this essay I have endeavoured to explain the cause of the oscillations in the height of the barometer. It is a question which has claimed the attention of philosophers from the time of the discovery of Torricelli, and has not hitherto received a solution; it is therefore with much diffidence that I attempt to give an explanation of this phænomenon. I think, however, that in the opposition, given to that current of the atmosphere which is caused by the greater specific gravity of cold air than of warmer, and by which the equality of its pressure can only be maintained, by descending particles of an upper current flowing in the contrary direction, there exists a cause fully adequate to the production of the principal variations in the atmospheric pressure, as indicated by the barometer.

Notwithstanding the great differences of temperature which exist between the poles and the equator, and the consequent differences in the density of the air, the mean height of the barometer is nearly the same at every place on the surface of the globe. This equality of pressure, which is the necessary consequence of the properties of a fluid, can therefore only be maintained by the greater height of the columns of least density; their height being in inverse proportion to their specific gravity.

The condition in which an atmosphere surrounding a sphere decreasing in temperature from the equator to the poles is placed, is thus given by Professor Daniell †:—"The elasticity of the air as measured by the barometer remaining the same, its specific gravity is very much greater at the poles than at the equator; and hence it is clear that the atmospherical column must be proportionally shorter at the former than at the latter point.

"The further conclusion follows, that this heavier fluid

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* Communicated by the Author.

† Meteorological Essays, p. 19.

must, by the laws of hydrostatics, press upon and displace the lighter; and a current will be established from the poles to the equator.

"This difference of gravity becomes less as we ascend from the surface, and at a certain point is neutralized; while, on the other hand, the elasticity which is equal at the surface varies with the height, and the barometer stands higher at equal elevations in the equatorial than in the polar column, and, at some definite height, must more than compensate the unequal density of the lower strata, and occasion a counter flow from the equator to the poles."

The calculations of the opposite forces of density and of elasticity, which Professor Daniell has given, it is not necessary here to repeat; but he adds, "As we have calculated that those currents, with such respective degrees of force, are the consequence of the equal height of the barometer all over the surface of our sphere, so we conclude that this equal height is maintained by this constant and regular flow; and any irregularity communicated to the currents would immediately be shown by a change in the mercurial column. Let us imagine, for an instant, that any cause (no matter at present whence originating) should retard the velocity of the polar current without at first affecting the equatorial, it is obvious that the barometer would fall at the equator and rise at the poles, for the balance of forces would be disturbed by the want of compensation for the matter removed at one extremity and accumulated at the other."

The terms polar and equatorial, when applied generally, must of course be considered as merely relative, and not confined to the two extremes of temperature; and, as in the present essay we may confine ourselves to the northern hemisphere, they may be used indiscriminately with north and south.

I have quoted these passages from the writings of this distinguished philosopher, because it was necessary that these principles, which are strict and evident deductions from the laws of hydrostatics, should be made the foundation upon which the present inquiry should be conducted, and it would not have been easy to have stated them more clearly or concisely.

The question is now, therefore, simply resolved into this: How far the regularity of these compensating currents is maintained, and when, as occurs over the greater part of the surface of the earth, the lower one is frequently interrupted, what are the origin and the nature of the obstacles presented to it?

In the intertropical regions, and those a few degrees beyond them, these currents flow with great constancy; and as the decrease in temperature in those latitudes, in receding from the equator, is small and comparatively regular, they are so nicely balanced that the oscillations of the barometer are within very small limits. But beyond these regions the constancy of the currents is not maintained, and the oscillations of the barometer increase. We are not less sure, however, that the same tendency of the air to flow from colder to warmer regions, which is manifested between the tropics, must always exist, and that therefore there must be some force by which the atmospheric current is impelled in the contrary direction.

The origin of this opposing force is universally acknowledged. Sir John Herschel has shown, in his 'Treatise on Astronomy*,' that it arises out of that grand system of atmospheric currents, to which, where the constancy of their course is maintained, the trade-winds belong. On that latitude where these winds cease, and in all latitudes between it and the pole, the air of the upper or equatorial current descends, as it must do, to supply the place of that flowing to the equator in the north or north-east wind. But it does not descend as a simply falling body; it has acquired, in flowing from the equator, a momentum, which, together with its greater relative velocity of revolution, causes it to advance along the surface of the earth in a direction from south-west, thus overcoming, by its impetus, the force of gravity which would urge on the north wind, as the maintenance of the equilibrium requires.

It is not my purpose at present to enter on the consideration of the various phænomena presented by the winds, upon which much light has been thrown by Professors Dove† and Kämtz ‡, more especially by the observations of the former. It is sufficient that it has been shown by these philosophers, that every direction of the wind (excepting, of course, that depending on merely local causes), even when most inconstant, may be referred to the action of these two currents. In speaking therefore of their effect upon the pressure of the atmosphere, it will suffice to consider, that whatever deflection the wind may have from east or west, as it blows from the north or south of these points, the polar or equatorial current predominates, and its effects are due to that current.

Here, then, we are presented with an obstacle to the influx of the air from the polar to the equatorial regions, and hence, when the force of the polar current is unable to overcome it, there will be an accumulation of air towards the pole, and a

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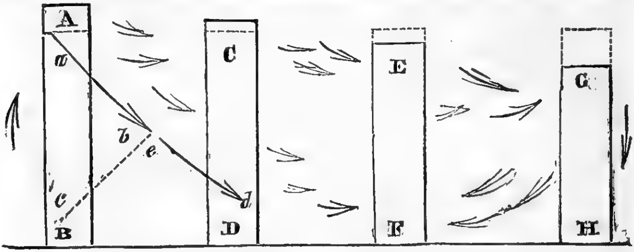
* (Lardner's Cyclopædia, p. 131.)

† Phil. Mag. 1837. ‡ Edinb. New Philosophical Journal, vol. xxvi.

consequent deficiency towards the equator. This, however, will be better understood by the following illustration:—

Let A B, C D, &c. represent atmospheric columns situated anywhere, in a direction from south to north, having the equator at the side of A B and the pole at that of G H, and decreasing in height from B to H, in inverse proportion to their density, their upper limit being supposed as well defined as their lower one. Their pressure upon the surface of the earth is exactly equal; and were the circulation of the air by means of the current ascending at A B, flowing from A to G, and descending at G H, kept up, it would remain so, because the air, removed from the apex of the column A B by the upper current, would be compensated by that flowing to its base. But let the upper current, instead of being confined to the higher strata until it reaches G, descend between A B and C D as shown by the line *a b*; if the air fell as a simply falling body, it would immediately, on its arrival in the lower current, assume its direction, and move as shown by the dotted line *c b*; thus this current would be undisturbed by its descent. Such, however, we have seen to be far from being the case, for it has acquired, on its arrival from the equator at the upper part of the column A B, the momentum or velocity due to the height of its fall from higher columns; it therefore advances in its original direction shown by *e d*, forcing back the air flowing by the force of its gravity towards the column A B. The effect of such a state of the currents upon the atmospheric pressure is very evident.



The upper current is flowing on in the upper strata without interruption towards the lower columns G H, &c., whilst the lower one, which should restore the air carried from the top of A B to its base, is wholly driven back.

The air is thus advancing through the whole height of the atmosphere towards the pole from the column A B, which can receive no compensation but that afforded from more southern regions, or from lateral columns, where the south wind does not prevail on the surface, and consequently the heights of all the columns are advancing towards a state of equality, A B falling and G H rising. Hence, as they approach to this, their pressure becomes as their specific gravity; and supposing the strength of the falling current to be overcome at F, as shown by the arrows, there will be a point to the south of E F where the mean height of the atmosphere is maintained, and the barometer will fall at B and D with the pressure of the columns of least density, and rise at F and H where the density is greatest. Thus, then, consistently with observation, the barometer is depressed by a south wind and rises with a north wind; and when the latter maintains its course only to a certain point, as has been supposed at F, where it is stopped, the rise will be above the mean height, as at F and H. When however the south wind is at last driven into the upper regions of the atmosphere, the northerly current, impelled by the increased pressure of the columns E F and G H, in addition to their originally greater density, will flow with accelerated velocity, and a rapid rise in the barometer will ensue.

The barometer, however, frequently rises with a southerly wind; and it is easy to see, on the supposition that the preceding conclusions are correct, that this must necessarily be the case; for let the force of the south wind be overcome at F as before supposed, here there will be an approach to a calm; and it is most probable that our calms (or, as the air is scarcely ever perfectly calm during the day, our very light winds from north-west or south-west, one of which being always the direction of the wind when the air is nearly still for any continued period,) are the results of the equality of the forces of the two contrary currents; a conclusion which the frequent shifting of these scarcely perceptible breezes between south and north of west, and without affecting the barometer, tends to confirm. Now it is at these times that the pressure of the atmosphere is unusually great.

Let then the calm be confined to a locality of greater or less extent about F, the air of the polar current continues to flow from H to this place, but can proceed no further; here therefore an accumulation takes place, which can only be carried off by the increased flow of the upper current towards north, occasioned by the increased height of the column; but this force can only be communicated to it after some elevation has been attained; and when it does ensue, the increased pressure given by it to the polar columns will urge forward yet more the current of the lower strata from H to F. Now it is obvious that the increased height of the atmospheric columns must extend to the south as well as to the north of F; and hence, even when the south wind is blowing with some strength, there may be a rise of the barometer to above its mean elevation; for if we suppose the place of calm to be removed to H, and the heights of the columns to arrange themselves as shown by the dotted outlines, the pressure of E F will be above its mean, although the south wind is blowing at F.

The elevation, however, which the barometer sometimes attains during the prevalence of these extremely light winds, is greater than can be justly due to this cause alone; and it is maintained with such constancy, that it is quite evident it is not the counter effect of an extreme reduction in some more southern region, arising from a sudden and violent disturbance of the equilibrium of the atmosphere. But the almost universal attendant of such greatly increased pressure under these circumstances, is a temperature much below that of the season. For the sake of greater clearness I will take a particular instance.

The one I have selected, and it was the first I happened to meet with of a pressure much above the mean, is from Howard's Journal, published in the 'Annals of Philosophy.' It occurred in January 1826. The mean height of the barometer for four days, during two of which the wind was north-west, and the other two south-west, was 30·690 inches. The mean temperature of those days was 24·7°, and the mean temperature of the month 33°; a cold therefore of 8° below the temperature of the season.

Now the wind for some days previously had varied very little from west, and at the time of this great elevation the weather was exceedingly fine; hence we may infer that this cold was produced by local causes, probably by radiation unimpeded by clouds, its cooling effect being unmitigated by the precipitation of vapour. If then we suppose that the mean temperature of the month, 33°, was that due to the latitude, the temperature was at this time relatively 8° below that of the latitudes on each side north or south.

The temperature then at F, a portion of the earth of more or less extent, is 8° below that which it would be in a regularly progressive decrease from B to H; its height will therefore be diminished in proportion, and there will be a rapid decrease in the height of the atmosphere from A to E, and a proportionally slight one from E to G; hence the upper current will be accelerated in the former part and retarded in the latter. If, however, the lower current maintained its course with a relative velocity, there would be no increase of height in the column E F, and consequently no increase of pressure; but in this case the efflux of the air from its base is either wholly, or almost wholly, prevented by the opposition of the equatorial current; and therefore that which flows to its top will accumulate, and will tend to produce the same gradation of heights in the atmospheric columns as though no deviation from the relative mean temperature of E F existed.

The pressure of this column, therefore, supposing this effected, would be in the proportion which its density bears to the density of a column whose temperature is that due to the latitude. In the present instance this temperature is 33°, and the actual temperature of the column E F 25°. Now the relative densities of air at these temperatures, under a pressure of 30·000 inches of the barometer, are 1000 and 1023, which will give 30·690 inches for the pressure of the column at 25°.

According to this calculation, therefore, an unseasonable reduction of the thermometer of 8°, under the circumstances here considered, would produce a rise of the barometer of 0·690 inch; but it would, in fact, be very much more than this, because no allowance has been made for the continued increase of density that would ensue from the increased pressure; and it is clearly impossible to calculate to what height it might rise, because the result here supposed could never be perfectly attained, on account of the increased elasticity given to the column, beyond that of lateral columns, where the cold does not prevail, which would cause a flow to take place longitudinally in the upper strata that would partially carry off the accumulation; therefore the greater the extent in longitude to which the cold prevails, the greater will be the elevation of the barometer.

It is also obvious, that when the height of the atmosphere has been greatly reduced by a strong south wind, a decrease in the force of the wind, with no change in its direction, will (as occasionally happens) cause a rise of the barometer, because the loss sustained will then be partially restored by the influx of air from lateral columns, which have been beyond the limits of the wind.

In these various cases of increase of atmospheric pressure, therefore, when the wind is southerly, we have the explanation of the fact, that though the south wind in the majority of instances depresses the barometer, its mean height during north winds and south winds is nearly the same.

Thus far I have confined myself to the consideration of the mechanical effects alone, of the atmospheric currents upon the height of the barometer, persuaded that its variations are mainly owing to them. But the connexion long established between its height and the state of the weather, a low and falling barometer generally attending precipitations of vapour, demands an explanation.

It is in some measure already accounted for in the preceding remarks, because the south wind, that which depresses the barometer, coming from warmer regions, brings with it a larger quantity of vapour than its decreasing temperature will support, and consequently a precipitation takes place. But the north wind, coming from colder climates, is comparatively dry; and thus the barometer rises when the weather is fine, and falls when it is the contrary.

But the connexion here noticed cannot be wholly explained in this way, because it almost invariably happens, that a south wind with rain causes a greater depression of the barometer than the same wind without rain.

This, however, as has been fully shown by Professor Daniell, is precisely what ought to take place, because the latent heat emitted by the vapour in condensing into clouds, together with that absorbed by the clouds themselves from the sun's rays, instead of allowing it to penetrate to the surface of the earth, will, by expanding the upper strata of the atmosphere, diminish the density of the columns in which the precipitation takes place to a yet greater degree below that of those to the north of them, thus increasing their height, and consequently the elasticity of their upper strata, so as to retard the influx of air from the equatorial, or higher columns, and urge on the efflux towards the polar or lower ones; and it is also obvious that a slight depression of the barometer will be occasioned, even when the lower current is flowing in due course, because the expansion being confined to the higher strata of the atmosphere, the temperature of the lower is not immediately affected by it, and therefore the lower current which depends upon this temperature, and the elasticity of the whole column, is not accelerated; so that the increased efflux of the air from the higher regions of the atmosphere will occasion a decrease in the pressure; and a fall of the barometer, as is frequently the case when rain falls with a northerly wind, will be the consequence.

This reasoning also applies to the explanation of the fact, that at those places which are peculiarly situated with regard to the precipitation of moisture, more especially in mountainous localities, the lines showing the oscillations of the barometer, deviate in a greater degree from the mean than those drawn at stations within a comparatively short distance, and differently circumstanced, and present also small irregularities not perceptible in the latter, although a general accordance between the lines is maintained*.

Without noticing any particular deviations which may be

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* Daniell's Meteorological Essays, p. 562.

found from these laws, it may be sufficient to remark, that any general theory can only be founded upon a condition liable to great variations, such as a continual decline of temperature from equatorial latitudes to polar; for it not unfrequently happens from local causes, that the temperature of a station is considerably below that of one to the north of it, and that stations on the same latitude, and not very far removed from each other in longitude, have very different temperatures; but such irregularities must be the causes of disturbance to the general order of the phænomena.

It will be necessary, however, before closing this essay, to apply the foregoing conclusions to the explanation of various facts of a very interesting nature presented by the oscillations of the barometer in connexion with storms, situation with regard to latitude, and with seasons.

The greatest depression of the barometer is during storms from south-west or south; those from north-east depress the barometer very little, and sometimes not at all.

As the north-east wind is attended by a counter current flowing above, if the force of both is equally accelerated, the pressure of the atmosphere will be unaltered; but if the lower one is unequalled by the upper, a diminution of pressure in some parts of its course, and an increase in others, will be the consequence; but it is obvious it cannot be very great, because, as the force of the upper current depends upon the comparative elasticity of the upper parts of the atmospheric columns, and any change in the elasticity of the lower strata must be transmitted through the whole column, its acceleration will very quickly follow that of the under current; but in storms from south-west, which are most frequent and violent, the case is widely different.

As the south wind depresses the barometer by the flowing of the air from higher columns to lower without the compensating return, the greater the velocity of the wind the greater of course will be the depression. But its degree will also depend on the extent of the storm, because, as the height of the atmosphere in its locality is diminished, the velocity of the air, flowing from the upper strata of the columns, which are beyond the limits of the storm both in latitude and in longitude towards the depression, will be increased, and consequently the smaller the extent of the storm, the more effectual will be the supply from this source; this, I believe, is quite consistent with observation. Westerly storms, however, though beginning from the south, pass before subsiding to the north of west, and then, although the force of the wind is undiminished, the barometer rises with a rapidity equal to that of its fall.

Now it is evident, that when the barometer has been greatly depressed by the velocity of the south wind, the density of the air will be so diminished by the great reduction in its pressure, that the tendency of the denser air of the polar or northern columns, pushed back by this wind, will be greatly increased, and, therefore, so soon as the strength of the south wind is overcome, the north wind, though still deflected by its remaining force (its course being north-west), rushes forward to restore the air to its former density and elasticity; and hence the stronger the wind the more rapid the rise of the barometer.

The phænomena here described are so constant and so well known, that their truth does not seem to require confirmation; for this, however, I need only to refer to the description of storms in high latitudes given by Col. Reid*. One short extract will suffice. "On the south coast of England violent gales usually set in with the wind about south, and veer by the west towards north-west. The barometer, falling at the commencement, rises as the wind becomes northerly. In the corresponding latitude, in the southern hemisphere, this order, as regards both the wind and barometer, is reversed." This description, as may be seen by reference to this work, applies generally to the storms of temperate regions.

It is evidently impossible to obtain, by calculation, the extreme limits to which the depression of the barometer might be carried; but it is easy to show, that, within the limits which might be assumed on this theory, there is ample room for the greatest reduction ever observed.

Let us suppose that a violent storm occurs at some latitude whose temperature at the time is 40°, and allowing nothing for the expansion which must immediately ensue on the diminution of the pressure of the atmosphere by a portion of its height being removed, supposing its density to remain as before, and that its height is reduced to that of the northern columns whose temperature is 7°. The densities of air under equal pressures at these temperatures, corrected for the presence of aqueous vapour, are 10·000 and 10·722, which, taking the mean pressure of the column of 7° temperature at 29·900 inches, will give 27·886 inches for the pressure of the column at 40°; so that it is obvious, that considering the progressively increasing reduction in density, which will go on as the height of the latter is diminished, and that the whole mass of the atmosphere is moving in one direction from this locality, whilst a disproportionate supply of air is moving towards it from columns on the equatorial and longitudinal sides, the depression may be very rapid and very great without the proportion-

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* Law of Storms, chap. ix.

ate heights of atmospheric columns being very materially altered.

The difference of temperature here supposed may be thought too great, but the mean temperatures of the coldest month of the year at Edinburgh and St. Petersburgh are given by Humboldt at 38·3° and 8·6°, and yet there are only about 5° of latitude between these cities. It is true that the two localities are situated in different systems of climates, but the rotation of the earth will cause the air of the West of Europe to flow in the equatorial current towards the colder regions of Eastern Europe and Western Asia.

Moreover, great falls of the barometer are always accompanied by an unseasonably high temperature.

Storms in high latitudes are more frequent and violent in winter than in summer, and as a necessary concomitant of this fact, the oscillations of the barometer are greatest during the former season. The difference in the force of the wind during these seasons has been given numerically by A. Follett Osler, who laid before the last meeting of the British Association for the Advancement of Science the results obtained by his Anemometer during a space of three years. The strength of the wind during the winter half of the year appears to be to that in the summer half as 1000 to 530*.

The cause of this difference is sufficiently obvious.

The length of day increasing with the latitude during summer, and decreasing with it during winter, gives to high latitudes a very great relative temperature in summer, and a proportionately low one in winter. This is shown by the following table given by Humboldt in his 'Essay on Isothermal Lines;' it includes the longitudes between 1° west and 17° east. The first column gives the mean temperature, the second that of winter, the third the decrease of winter temperature in receding from the equator, and the fourth and fifth the summer temperatures and decrease.

Isothermal line of temp.	Winter.		Summer.	
	Temp.	Decrease.	Temp.	Decrease.
68°	59·0°	°	80·6°	°
59	44·6	14·4	73·4	7·2
50	35·6	9·0	68·0	5·4

The origin of atmospheric currents being the decrease of temperature from lower to higher latitudes, their force will be

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* Report of British Association. Athenæum, October 1840.

in proportion to the amount of the decrease, which we find in winter to be to that in summer nearly in the ratio of two to one, and accordingly the force of the wind varies nearly in this ratio also; whilst the proportionally increased difference in the densities of atmospheric columns of different latitudes, affords greater scope for elevations and depressions in their relative heights, and consequently for variations in their pressure; and thus increases the extent of the oscillations of the barometer.

The lines marking the oscillations of the barometer, coincide in direction for great distances. For a confirmation of this fact I must refer to those drawn or graduated lines inserted at the end of Professor Daniell's 'Meteorological Essays,' which are selected from the volumes of the Ephemerides of the Meteorological Society of the Palatinate.

It is clear that any elevation or depression of the atmosphere must be communicated by the flowing of the upper current to great distances, both in the direction of the meridian by the proper course of this current, and laterally by the rotation of the earth; and from this it occasionally happens, that fluctuations in the height of the barometer are caused by circumstances occurring at a distance from the place of observation. Thus in December 1821, the height of the barometer at Tottenham, near London, was 27·83 inches on the 24th; and though windy weather prevailed at the time, yet no storm of wind of any consequence succeeded this great depression, but a like state of the barometer was extensively observed at the same time on the Continent, and very tempestuous weather attended it far to the south of our island*.

The deviations of these lines from the mean, decrease towards the equator.

The oscillations of the barometer in the more southern latitudes, which are merely communicated from northern parallels, will of course decrease in extent with their distance from their origin; and in those originating near the tropics, in which latitudes the differences of temperature are so much less, and so much less exposed to irregularities than in those nearer the poles, that much less variation in the heights of atmospheric columns can take place in the former than in the latter, the fluctuations of the mercury will be proportionally slight.

The extent of the oscillations of the barometer below the mean height diminishes, and the extent of those above the mean increases, from the temperate to the polar regions. The

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* Howard's Meteorological Journal. Annals of Philosophy, vol. xix. p. 159.

height of the barometer at London has been reduced to 27·83 inches, as before mentioned, and 30·70 inches is an elevation, which, at any rate, has been very little exceeded in this country; but from the observations of M. Kupffer*, it appears that at St. Petersburgh the greatest depression of the barometer, during thirteen years, was to 28·35 inches, and the greatest elevation was 31·29 inches.

It is obvious that the nearer we approach the atmospheric columns of least height, the smaller must be the quantity of air which can flow from their tops, and the greater that which can be accumulated upon them; so that the degree in which their pressure may be diminished is lessened, and their capability of receiving additions to it is increased.

I think I have now noticed the whole of the more important atmospheric phænomena connected with the height of the barometer at the surface of the earth, excepting the horary variations of this instrument; but these evidently arise from a distinct cause, which, there can be little doubt, is the disturbance given to the balance of the two currents by the alternate heating and cooling of the air during day and night, as has been set forth by Professor Daniell.

LXVIII. *On the Differences of the mean Pressure of the Atmosphere on different Latitudes.* *By* WILLIAM BROWN, Jun.†

IN the foregoing paper I have attempted to show, that the oscillations of the barometer are caused by the opposition presented by the descending equatorial current of the atmosphere, to the influx of air from higher to lower latitudes. When that paper was written, I was not aware of the observations collected by M. Shouw, showing the differences which exist in the mean height of the barometer at different latitudes. The original essay in which these are given, published in the *Comptes Rendus des Séances de l'Académie*, I have not seen; but it is referred to in the 'American Journal of Science' for October 1841, by Professor Loomis, who has extracted from it a table containing his results; the height of the barometer being corrected for the differences in the force of gravity.

Now allowing for errors of observation, the numbers in this table representing the height at various places on the same latitude, agree too well with each other, and the almost undisturbed order of the variations is too well marked, to admit

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* Edinburgh Philosophical Journal, 1840.

† Communicated by the Author.

of any doubt as to its general correctness. Without inserting here its details, it may be of use to give its principal features in a tabular form.

Name of place.	North lat.	Height of barom.	Increase.
Christianborg	5° 24′	29·849	
La Guayra	10° 48′	29·856	·007
Macao	22° 10′	29·985	·129
Madeira*	32° 37′	30·093	·108
			Decrease.
Mean of four places	43° 47′ to 45° 24′	30·001	·092
Mean of five places	53° 32′ to 55° 57′	29·942	·059
Mean of three places	64° 00′ to 65° 50′	29·675	·267
			Increase.
Mean of three places	73° 00′ to 75° 30′	29·843	·168

From this it appears that the mean pressure of the atmosphere is greatest at latitude 32°; decreasing on each side of it, but in the greatest degree towards the pole, its minimum being at latitude 64°; whence it again increases until 75°, that being the limit of the observations, though from the regularity of the variations, we may infer that the increase goes on to the pole itself.

Now it is obvious that these results are quite contrary to what would be produced by a constant, or almost constant opposition to the influx of air towards tropical latitudes; as this would occasion an accumulation of air in high latitudes, and a corresponding deficiency in regions near the equator. But were there no counteracting cause, the same effect would result from other circumstances. It would be the natural consequence of two currents flowing in opposite directions between the pole and the equator. As the air flowing towards the pole surrounds during its course, circles of constantly diminishing diameter, it will occupy a smaller horizontal area, and will arrive in high latitudes in over abundance; whilst the lower current for the same reason will bring an insufficient supply to the atmosphere encompassing circles of greater diameter; an effect which would be yet further increased by the retardation of the lower current by friction on the surface

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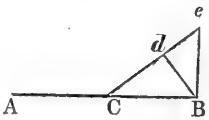
* By referring to the table from which these numbers are taken, it will be seen that the height for Tripoli (latitude 32·55) 30·182 inches is omitted; this however is obviously an error.

of the earth; these therefore would conspire to produce a greater mean height in high latitudes than in equatorial, a result the reverse of what we find by observation. The causes therefore which tend to produce it must be counteracted in their effects by an opposite force, which is able, in addition, to sustain the greater equatorial pressure. Such a one may be found in the centrifugal force of the earth's rotation on its axis.

At the equator the centrifugal force is in direct opposition to that of gravity, but at all latitudes between it and the pole, it may be resolved into two forces, the one directly opposed to gravity as *d e* (C being the centre of the earth, and A B its axis), and the other in the direction of a tangent to the surface of the sphere as B *d*. With the former of these we have here nothing to do, but the effect of the latter is to urge on the atmosphere from the poles towards the equator, so as to cause the air to accumulate in tropical latitudes, until its pressure produces a resistance, sufficient to counterbalance the force of the current.



We find from the table, that this force is predominant only between the 32nd and 64th degrees of latitudes, and that on both sides of this zone it is more than counteracted by the causes previously mentioned as having an opposite tendency. Now in this zone the tangent force is the greatest, decreasing from latitudes 40° and 50° in nearly equal proportions in both directions.

Without attempting to show by calculation the correspondence btween the resistance, caused by the greater tropical pressure of the atmosphere, and this force, for the sake of comparing its different degrees of strength, I have subjoined some calculations of its force at different latitudes.

The centrifugal force at the equator is equal to a velocity of 0·1112 foot per second, and decreases in proportion to the cosine of the latitude; therefore at latitude 60° it is equal to $0{\cdot}500 \times 0{\cdot}1112 = 0{\cdot}0556$; and at any latitude the whole force is to that portion of it which is a tangent to the sphere, as cotang. to cosine; therefore $\frac{0{\cdot}500 \times 0{\cdot}0556}{0{\cdot}5773 \text{ (cot of } 60^\circ)} = 0{\cdot}0481$ foot; the velocity which this force would give to the air in one second at latitude 60°.

Again, at 30° the centrifugal force is $0{\cdot}866 \times 0{\cdot}1112 = 0{\cdot}0933$; and $\frac{0{\cdot}866 \times 0{\cdot}0933}{1{\cdot}732 \text{ (cot } 30^\circ)} = 0{\cdot}0466$ foot per second;

nearly the same as the velocity at latitude 60°; and if we take the means of the forces at two latitudes 10° apart, on each side of 40° and 50°; we find the same degree of force overcome on the polar side as on the equatorial, thus

Lat.	Force.	Lat.	Force.
Mean of 40° & 30°	0·051	Mean of 50° & 60°	0·051
Mean of 30 & 20	0·042	Mean of 60 & 70	0·042
Mean of 20 & 10	0·028	Mean of 70 & 80	0·027

Thus then between 40° and 30° of latitude, a uniformly accelerating force of 0·051 foot per second, to which the air is subjected in a direction from the pole towards the equator, predominates over the contrary forces; whilst between latitudes 30° and 20°, a force of 0·042 foot per second is unequal to them, and between latitudes 60° and 70° the same degree of force is overcome; and between 50° and 60° the force equal to that between 40° and 30°, viz. 0·051 foot is again predominant, although in the former regions the difference of height is much greater than in the latter; showing that those causes which tend to counteract the centrifugal force are much modified in different zones, as must evidently be the case by such circumstances, as the relative proportions of easterly and westerly winds; an easterly wind being, we may suppose, when flowing towards the equator, in some part of its course an ascending current, and therefore tending to check the upper current by the velocity, acquired by its motion in the contrary direction; thus coinciding in its effects with the centrifugal force, or if there is no ascending movement, at least not opposing it;—an opinion, which is in accordance with the fact generally mentioned by navigators, that westerly winds prevail in much greater degree between latitudes 30° and 50° than between 50° and 65°, where the increase in the height of the barometer towards the equator is the greatest, the greater rate of increase being probably occasioned by the greater proportion of easterly winds.

LXIX. *On the Volume of a Segment of a Surface of the Second Order, bounded by parallel Planes. By* James Booth, *Esq., M.A., M.R.I.A., Principal of, and Professor of Mathematics in Bristol College**.

IN the Geometry of Legendre, Brewster's edition (page 216), an expression is given for the volume of a segment of a sphere bounded by parallel planes; an analogous expression of great simplicity may be found when the investigation is so generalized as to include surfaces of the second order.

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* Communicated by the Author.

I. Let $a\,b\,c$ be the semiaxes of a surface of the second order, $a'\,b'$ the semiaxes of a diametral section of the surface parallel to the given planes, c' the semiconjugate diameter to this section, p the perpendicular from the centre on the tangent plane parallel to these planes, α and β the semiaxes of the section of the surface made by any parallel plane; let u and ϖ denote the segments of c' and p between the centre of the surface and this plane, putting u'' and u' for the segments of c' between the centre and the given bounding planes, and calling the volume of the slice V, we shall have

$$V = \pi \int_{\varpi'}^{\varpi''} d\varpi\,[\alpha\,\beta] \quad \dots\dots\dots\dots\dots\dots \quad (1.)$$

or changing the independent variable,

$$V = \pi \int_{u'}^{u''} du \left[\alpha\,\beta\,\frac{d\varpi}{du}\right]. \quad \dots\dots\dots\dots \quad (2.)$$

Let a plane section of the surface be drawn through a' and c', then

$$\alpha : a' :: \sqrt{c'^2 - u^2} : c';$$

similarly,

$$\beta : b' :: \sqrt{c'^2 - u^2} : c'.$$

Multiplying these proportions,

$$\alpha\,\beta = \frac{a'\,b'}{c'^2}\,(c'^2 - u^2). \quad \dots\dots\dots\dots\dots \quad (3.)$$

Now $u : \varpi :: c' : p$; hence $\dfrac{d\varpi}{du} = \dfrac{p}{c'}$, and $a'\,b'\,p = a\,b\,c$, by a well-known property of surfaces of the second order. Making these substitutions in (2.).

$$V = \frac{a\,b\,c}{c'^3}\,\pi \int_{u'}^{u''} du\,[c'^2 - u^2]; \quad \dots\dots\dots \quad (4.)$$

or integrating and taking the limits,

$$V = \frac{a\,b\,c}{c'^3}\,\pi \left\{ c'^2\,(u'' - u') - \frac{(u''^3 - u'^3)}{3} \right\}; \quad \dots\dots \quad (5.)$$

or $$V = \frac{a\,b\,c}{6\,c'^3}\,\pi\,(u'' - u') \left\{ 6\,c'^2 - 2\,u''^2 + 2\,u''\,u' - 2\,u'^2 \right\}.$$

Adding and subtracting $u''^2 + u'^2$, this equation may be transformed into

$$V = \frac{a\,b\,c}{6\,c'^3}\,\pi\,(u'' - u') \left\{ 3\,(c'^2 - u''^2) + 3\,(c'^2 - u'^2) + (u'' - u')^2 \right\}. \quad (6.)$$

Let $\alpha''\ \beta''$, $\alpha'\ \beta'$ be the semiaxes of the sections of the surface made by the bounding planes, and t the distance between them, or the thickness of the slice, then

$$c'^2 - u''^2 = \frac{c'^2 p \alpha'' \beta''}{a\,b\,c}, \quad c'^2 - u'^2 = \frac{c'^2 p \alpha' \beta'}{a\,b\,c},$$

and $(u'' - u') : t :: c' : p$.

Making these substitutions in (6.), we shall have

$$V = \tfrac{1}{2} \pi t \left\{ \alpha'' \beta'' + \alpha' \beta' \right\} + \left(\frac{a\,b\,c}{p^3} \right) \frac{\pi t^3}{6},$$

or putting A and B for the areas of these sections and S for the sphere whose diameter is t,

$$V = \tfrac{1}{2} t (A + B) + \left(\frac{a\,b\,c}{p^3} \right) S \quad \ldots\ldots \quad (7.)$$

or *the volume of any segment of a surface of the second order bounded by parallel planes is equal to one half the volume of the two cylinders whose bases are the sections of the surface made by the bounding planes, and common altitude the thickness of the slice, together with the sphere whose diameter is this thickness multiplied by the constant coefficient* $\frac{a\,b\,c}{p^3}$.

II. When the surface is a sphere, $a = b = c = p$, and

$$V = \tfrac{1}{2} t (A + B) + S, \quad \ldots\ldots\ldots \quad (8.)$$

the formula given by Legendre for the volume of a slice of a sphere.

III. *When the surface is a discontinuous hyperboloid or one of two sheets.*

The semiaxes in this case are $a \sqrt{-1}$, $b \sqrt{-1}$ and c, the formula (7.) therefore is changed into

$$V = \tfrac{1}{2} t (A + B) - \left(\frac{a\,b\,c}{p^3} \right) S. \quad \ldots\ldots\ldots \quad (9.)$$

Should the planes cut the surface in hyperbolic sections, p becomes imaginary, and the expression in this case fails as it evidently should, A and B, as also V, becoming infinite.

IV. *When the surface is a continuous hyperboloid or of one sheet.*

The semiaxes are a, b, $c \sqrt{-1}$, and the formula becomes

$$V = \tfrac{1}{2} t (A + B) + \sqrt{-1} \left(\frac{a\,b\,c}{p^3} \right) S, \quad \ldots\ldots \quad (10.)$$

an imaginary expression; but when p is imaginary, the expression for V is real, as it evidently should, the sections A and B being in this case real, and therefore V.

For putting $p \sqrt{-1}$ for p, p^3 is changed into $-p^3 \sqrt{-1}$,

and $$V = \tfrac{1}{2} t (A + B) - \left(\frac{a\,b\,c}{p^3} \right) S. \quad \ldots\ldots \quad (11.)$$

V. *When the surface is an elliptic paraboloid.*

The general expression for a perpendicular from the centre of a surface of the second order on a tangent plane is

$$p^2 = a^2 \cos^2 \lambda + b^2 \cos \mu + c^2 \cos^2 \nu.$$

Let l and l' denote the parameters of the principal sections in the planes of $a\,b$ and $a\,c$, then $b^2 = a\,l$, $c^2 = a\,l'$, hence

$$\frac{p^2}{a^2} = \cos^2 \lambda + \frac{l}{a} \cos^2 \mu + \frac{l'}{a} \cos^2 \nu.$$

Now when the surface becomes a paraboloid a becomes infinite, and $\frac{l}{a}$, $\frac{l'}{a}$ each equal to zero. Hence p, while it has a finite ratio with respect to a, is infinite compared with b and c; therefore the coefficient

$$\frac{a\,b\,c}{p^3} = \frac{\sqrt{l\,l'}}{a \cos^3 \lambda} = 0, \text{ when } a = \infty.$$

Hence $$V = \tfrac{1}{2}\,t\,(A+B). \quad \ldots\ldots\ldots\ldots \quad (12.)$$

VI. *When the surface is a cone.*

We may consider the cone as the limiting surface of an hyperboloid of two sheets.

Let c be the real axe, then in the hyperboloid

$$p^2 = c^2 \cos^2 \nu - a^2 \cos^2 \lambda - b^2 \cos^2 \mu,$$

or dividing by c^2,

$$\frac{p^2}{c^2} = \cos^2 \nu - \frac{a^2}{c^2} \cos^2 \lambda - \frac{b^2}{c^2} \cos^2 \mu;$$

but when the surface becomes a cone,

$\frac{a}{c} = \tan \alpha$, $\frac{b}{c} = \tan \beta$, α and β being the semiangles of the cone, and $\cos^2 \nu = 1 - \cos^2 \lambda - \cos^2 \mu$; hence

$$V = \tfrac{1}{2}\,t\,(A+B) + \frac{\tan \alpha \tan \beta}{\{1 - \sec^2 \alpha \cos^2 \lambda - \sec^2 \beta \cos^2 \mu\}^{\frac{3}{2}}} \cdot S. \quad (13.)$$

It is plain that the difference between any slice, and the sum of the cylinders on the bases of this slice, is independent of the distance of the bounding planes from the centre of the surface.

VII. A plane cuts off from a surface of the second order a segment of constant volume, to find the surface enveloped by this plane.

Let the surface be an ellipsoid, and let the volume be the nth part of the semiellipsoid, then the volume of the slice between this variable plane and the parallel diametral plane will be $\frac{2\pi}{3} abc(1-n)$, $\frac{4\pi}{3} a\,b\,c$ being the volume of the ellipsoid; to determine the volume of this slice; in equation (5.) let $u' = 0$, and we shall have

$$V = a\,b\,c\,\pi\left\{\frac{u''}{c'} - \frac{u''^3}{3\,c'^3}\right\};$$

let $\frac{u''}{c'} = x$, then

$$V = a\,b\,c\,\pi\left(x - \frac{x^3}{3}\right).$$

But V is also $= \frac{2\pi}{3} a\,b\,c(1-n)$.

Equating these values of V,

$$x^3 - 3\,x + 2(1-n) = 0. \qquad (14.)$$

Now as this cubic equation falls under the *irreducible case*, assume the formula

$$\sin^3\phi - \frac{3}{4}\sin\phi + \frac{\sin 3\,\phi}{4} = 0,$$

or, multiplying this formula by 8,

$$(2\sin\phi)^3 - 3\,(2\sin\phi) + 2\sin 3\,\phi = 0.$$

Comparing this equation with

$$x^3 - 3\,x + 2\,(1-n) = 0,$$

we shall have $x = 2\sin\phi$, $\sin 3\,\phi = 1 - n$;

but $x = 2\sin\varphi = \frac{u''}{c'} = \frac{\varpi}{p}$,

hence $\varpi = 2\,p\sin\phi$.

In the ellipsoid,

$$p^2 = a^2\cos^2\lambda + b^2\cos^2\mu + c^2\cos^2\nu;$$

or multiplying by 4 $\sin^2\phi$, and putting ϖ for $2\,p\sin\phi$
$\varpi^2 = (2\,a\sin\phi)^2\cos^2\lambda + (2\,b\sin\phi)^2\cos^2\mu + (2\,c\sin\phi)^2\cos^2\nu$, therefore ϖ the perpendicular on the enveloping plane, is a perpendicular on the tangent plane which envelopes the ellipsoid whose equation is

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 4\sin^2\phi.$$

the equation of an ellipsoid similar to, and similarly situated with the given one.

Let $n = \frac{1}{2}$, then $\sin 3\,\phi = (1-n) = \frac{1}{2}$ ∵ $3\,\phi = 30°$, or $\phi = 10°$, and the locus in this case is

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 4\sin^2 10°.$$

Again, when x is given we can find n from the equation

$$x^3 - 3x + 2(1-n) = 0,$$

or

$$n = \frac{2 + x^3 - 3x}{2}.$$

Thus let the axes of the two ellipsoids be in the ratio of 2 : 1, $x = \frac{1}{2}$ and $n = \frac{5}{16}$, or a plane which cuts off $\frac{5}{32}$ of an ellipsoid envelopes another whose axes are one half those of the given one.

Hence also the theory of floating bodies, whose surfaces are of the second order, may be reduced to the comparatively simple problem, of similar and similarly situated bodies, resting in equilibrio on a horizontal plane.

LXX. *On the Horary Deviations of the Barometer as observed at Plymouth.* By S. M. Drach, *Esq., F.R.A.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the Report of the British Association for 1839, the deviations of each hour from the line of mean pressure (= 29·7999 inch.) is given in Tab. xix. p. 164. Taking the unit for the avoidance of decimals to be the ten-thousandth of an inch, the deviation at

A.M.		A.M.		P.M.		P.M.	
1 h^{r} =	+18	7 h^{r} =	+ 3	1 h^{r} =	− 42	7 h^{r} =	+ 20
2	− 6	8	+33*	2	− 77	8	+ 62
3	−55	9	+49	3	− 91	9	+ 95
4	−71	10	+62	4	−104	10	+100
5	−71	11	+46	5	− 61	11	+ 93
6	= −39	12	= + 3	6	= − 29	12	= + 66

Adding together hours of the same name (as 1 a.m. and 1 p.m.), there results,

1 hr. =	− 24	7 hr. =	+ 24
2	− 83	8	+ 95
3	−146	9	+144
4	−175	10	+162
5	−132	11	+139
6	= − 68	12	= + 69

Hence, from the hours 1 and 7, 3 and 9, 6 and 12, it might be inferred that the law of these aggregate oscillations is nearly representable by the formula $B \sin 2t + b \cos 2t$; where B, b are constant for one day, and t = the time from midnight, supposing one day = 360°.

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* Instead of 32, to agree with Tab. I. p. 150.

The discrepancies in the other pairs are even removable by referring to plate v. in the volume, where it will be noticed that, first, the observed deviation (denoted by a *) must be reduced to that of the curve by adding to

2 a.m. about	−15	3 p.m. about	− 5
9 ……	+10	4 ……	+ 3
		5 ……	−10

and that, secondly, the p.m. upper curve appears also to be somewhat too flattened at its summit in order to pass *exactly* through the observed 10 p.m., which is remediable by applying to

10 p.m. about +10, and to 11 p.m. about +7.

With these corrections, the sums of the homonymous hours become for

Hour.	Hour.	Sums.	Diff.
1 = − 24	7 = + 24	= 0	= − 48
2 − 98	8 + 95	−3	−193
3 −151	9 +154	+3	−305
4 −172	10 +172	0	−344
5 −142	11 +146	+4	−288
6 − 68	12 + 69	= +1	= −137

Hence may a rule be deduced of great value for observatories and single travellers:—

The mean height of the barometer for the day of observation is exactly equal to the average of four observations made at intervals of six hours, whatever be the time of the first observation.

This rule is far more accurate than the average of the maximum and minimum, or of those at 9 hours and 3 hours, 4 hours and 10 hours, &c.; as probably the time of mean pressure is not the same for all points of the globe. The same rule is applicable to thermometrical observations. Indeed the height may be generally represented by the converging series $h = \Sigma_0^\infty m_i t^i$, and these may to some extent be reduced to functions of sin $i\,t$, cos $i\,t$, leading to the rule in question, as will now be shown.

The Plymouth observations may be represented by the formula $h = \text{H} + \text{A} \sin t + a \cos t + \text{B} \sin 2t + b \cos 2t + \text{C} \sin 3t + c \cos 3t + \text{E} \sin 4t + e \cos 4t$, where $h =$ the height for the time t, reckoned as above; H, A, a, &c. are constants.

Denote the height for $t + \frac{\pi}{2} = t + 6$ hours by h'

… … … $t + \pi = t + 12$ … h''

… … … $t + \frac{3\pi}{2} = t + 18$ … h'''

$$\therefore\ h+h''=2\mathrm{H}+2\,\mathrm{B}\sin 2t+2b\cos 2t+2\,\mathrm{E}\sin 4t+2e\cos 4t \quad (1.)$$

$$h'+h'''=2\,\mathrm{H}-2\,\mathrm{B}\sin 2t-2b\cos 2t+2\,\mathrm{E}\sin 4t+2e\cos 4t \quad (2.)$$

$$h-h''=2\,\mathrm{A}\sin t-2a\cos t+2\,\mathrm{C}\sin 3t+2c\cos 3t \ \ldots\ldots \quad (3.)$$

$$h'-h'''=2\,\mathrm{A}\cos t-2a\sin t-2\,\mathrm{C}\cos 3t+2c\sin 3t \ \ldots\ldots \quad (4.)$$

$$h+h'+h''+h'''=4\,\mathrm{H}+4\,\mathrm{E}\sin 4t+4e\cos 4t \ \ldots\ldots\ldots\ldots \quad (5.)$$

$$h+h''-h'-h'''=4\,\mathrm{B}\sin 2t+4b\cos 2t \ \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (6.)$$

Now equation (5.) compared with the sums of the homonymous hours, gives 24 H = + 5. It is however evident that H = the mean height, and as only the *deviations* are conceived here, *i t* = 0. This error of + 5 is partly owing to the approximate estimation of the distance of the * from the curve in plate v., and it might be suggested that in these graphical charts the network of coordinates should sometimes present *oblong* instead of *square* divisions, so as to allow of the deviations from the regular curve to be better measurable.

Equation (5.) gives mean values of E = − ·505, and *e* = + ·125.

Equation (6.) compared with the sums of the homonymous hours, gives three values for B, and the same number for *b*. The averages are B = 78·3, *b* = 35·6.

Applying the corrected deviations to the separate hours, we obtain

Hour.	A.M.	P.M.	Diff.	Hour.	A.M.	P.M.	Diff.
1	+18	− 42	+60	7	+ 3	+ 20	−17
2	−21	− 77	+56	8	+33	+ 62	−29
3	−55	− 96	+41	9	+59	+ 95	−36
4	−71	−101	+30	10	+62	+110	−48
5	−71	− 71	+ 0	11	+46	+100	−54
6	−39	− 29	−10	12	+ 3	+ 66	−63

Grouping separately the hours (1, 5, 7, 11), (2, 4, 8, 10), (3, 6, 9, 12), there result three values for each of the quantities A, *a*, C, *c*; the means are

$$\mathrm{A}=-0{\cdot}7,\quad a=+33{\cdot}3,\quad \mathrm{C}=+2{\cdot}9,\quad c=-3{\cdot}8.$$

Hence at Plymouth we have in English inches,

$$h-\mathrm{H}=-0{\cdot}00007\sin t+0{\cdot}00333\cos t-0{\cdot}00783\sin 2t+0{\cdot}00356\cos 2t+0{\cdot}00029\sin 3t-0{\cdot}00038\cos 3t-0{\cdot}00005\sin 4t+0{\cdot}00001\cos 4t.$$

$$=\cdot 00333\sin(t+91^\circ\,12')+\cdot 00860\sin(2t+155^\circ\,33')+\cdot 00048\sin(3t+307^\circ\,20')+\cdot 00005\sin(4t+161^\circ\,6').$$

In the Transactions of the Royal Society of Edinburgh vol. x., Mr. Blackadder proposed a self-registering barometrical instrument, consisting of several barometers, the open ends of which could be closed by air-tight lids at any required time by means of clock-work. Four barometers arranged cylindrically on a stand and shut by machinery at intervals of six hours, would, from the preceding investigation, indicate the mean daily pressure at whatever time the instrument were wound up.

As the aërial tide rushing over a mountain-flanked valley greatly resembles the aqueous tide rushing upwards over the bed of a river, may not some local constant, similar to the establishment of the port, be attached to the place of observation?

London, March 14, 1842. S. M. D.

LXXI. *Contributions to the Minute Anatomy of Animals. By* GEORGE GULLIVER, *F.R.S., &c. &c.*[*]

As minute anatomy is now become more generally interesting than formerly, and begins to assume the character of an extensive and comparatively accurate science, so as to give a new complexion to some of the most important questions in physiology and pathology, and to enable us to submit many old doctrines to a more exact scrutiny than most of our classical anatomists had the means of employing, it appears to me that considerable advantage might arise if different independent observers would more frequently publish a brief yet clear account of the results of their inquiries. Hence I propose to communicate occasionally to the Philosophical Magazine, a series of short notes on the ultimate structure of various animal tissues and on the elementary forms which occur in the fluids, taking the descriptions in all cases from my own observations, and frequently illustrating them with wood-cuts. It will thus be attempted to give either a more precise account than we yet possess of some of the healthy and diseased parts of man and the lower animals, to present certain particulars of structure in novel physiological relations, or to record facts which may appear to have escaped the attention of previous observers: in short, to contribute summary and plain notes concerning numerous detached anatomical points which may perhaps be treated of as profitably in this manner as by set dissertations.

On the Lymph-Globules of Birds.

It is well known that the blood of the vertebrate animals contains, besides the numberless red discs, a few pale globules which have very commonly been regarded as those of lymph. In birds, however, the globules which constitute the greater part of the juice of the lymphatic glands are generally rather

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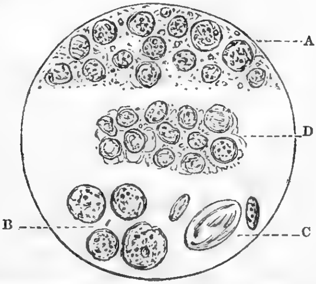
[*] Communicated by the Author.

smaller than the pale globules of the blood; and, as I have noticed in the Appendix to Gerber's Anatomy, the same fact is observable in the mammalia. Yet the descriptions given since Hewson's time of the lymph-globules of birds have always been drawn from the pale globules of their blood.

The distinguished inquirer just mentioned states that the particles of the fluid of the lymphatic glands of birds are oval, like the nuclei of their blood-corpuscles. In the Philosophical Magazine for February 1840, I gave an account of the lymph-globules of the Musk Deer, from which it appears that these scarcely differ in size from those of man, notwithstanding the blood-discs of this little ruminant are the smallest at present known; and although the Camelidæ have oval blood-corpuscles, I found that the globules of the thymus, of the lymphatic glands, and of the pus of these animals, had the usual circular figure, and nearly the same size as the corresponding globules in other mammalia*. It was to be expected therefore that the lymph-globules of birds would possess a similar form, and this I have lately ascertained to be the case. The annexed figure represents particles from the Magpie, all of which are magnified exactly to the same degree, to wit, about 800 diameters.



A shows the lymph-globules in the juice of a lymphatic gland from the neck; also smaller circular particles, probably free nucleoli: B, four pale globules from the blood of the heart: C, one perfect blood-disc and two detached nuclei, the latter exposed by dilute acetic acid: D, portion of the pulp of the spleen chiefly composed of cell-nuclei, very similar in all respects to the globules of lymph: this representation will be referred to in a future communication.

The lymph-globules of birds are commonly rather smaller than those of mammalia, yet this difference of size is not ob-

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* See Medico-Chir. Trans., vol. xxiii.; and Lancet, 1840–41, vol. ii. p. 101.

servable to the same degree in the pale globules of the blood of these two classes. The account of the chemical characters of the lymph-globules of mammalia, as given in the Appendix to Gerber's Anatomy, is generally applicable to the corresponding globules of birds. Professor Wagner observes, that the chemical properties of the pale globules of the blood and the nuclei of the blood-discs of birds and reptiles are identical. This appears to be true in most respects; but in certain experiments the two kinds of particles seem to me to be differently affected under precisely the same treatment. Thus the nucleus of the blood-corpuscle is not so prone to change in drying as the lymph-globule. The former, whether exposed in recent blood by acids, or in dry blood by the moisture of the breath, may be quickly dried, and the form of the nucleus thus completely preserved, on the slip of glass used to make the observation; while the lymph-globule after similar treatment, and even if dried without any addition, becomes either faint, tumid, or misshapen. Certain saline solutions too, which, in a few hours, either injure the shape of the lymph-globules or render them almost invisible, do not act so remarkably on the nuclei of the blood-discs.

The pale globules noticed in this paper are those well-known white and slightly granular corpuscles which are generally seen at once very plainly in the blood, as they appear, under certain adjustments of the object-glass, with a distinct and dark circumference. But there are other pale particles in the blood. Some of these are isolated, and agree in all respects with the globules of the lymphatic juice, being smaller, often rather fainter and with a less definite contour than the pale globules first mentioned. In the blood after death there is also frequently observable small shapeless white fragments consisting of circular or oval granules hardly as large, seldom larger, than the globules of the lymphatic fluid; and minute oil-like particles are often seen in the fragments. In birds this granular matter often exactly resembles in structure the colourless fibrine, obtained from their blood by washing it in a linen bag, and the granules are frequently just like the nuclei of the blood-discs. The white granular matter is often abundant when the pale globules are either difficult to be found or entirely absent.

The engraving may be considered as showing the relative size of the pale globules of the blood and those of the juice of the lymphatic glands in birds generally. The following measurements of the lymph-globules are expressed in fractions of an English inch; the common sizes are first noted, then a space is left, after which the measurements of the small and large globules are given; and lastly, beneath the lines, the mean size deduced from the whole observations.

1. Pigeon (*Columba Livia*, var. Briss.).
1-6000
1-5333

1-7110
1-3800
1-5274

2. Song Thrush (*Turdus musicus*, Linn.).
1-6000
1-4800

1-8000
1-3500
1-5090

3. Common Fowl (*Gallus domesticus*, Briss.).
1-6400
1-6000

1-8000
1-3200
1-5261

4. White Owl (*Strix flammea*, Linn.).
1-6000
1-4800

1-7110
1-4000
1-5227

5. Young Heron, half-grown (*Ardea cinerea*, Lath.).
1-5672

1-6400
1-4000
1-5150

6. Rook (*Corvus frugilegus*, Linn.).
1-5333

1-6400
1-4000
1-5053

7. Jackdaw (*Corvus Monedula*, Linn.).
1-6000

1-8000
1-3555
1-5238

8. Starling (*Sturnus vulgaris*, Linn.).
1-5600

1-7110
1-3800
1-5152

9. Jay (*Garrulus glandarius*, Flem.).
1-6000
1-4000
1-3600

1-6400
1-3200
1-4414

10. Magpie (*Corvus Pica*, Linn.).
1-6000
1-5333
1-4800

1-6400
1-3555
1-5001

Pale globules of the blood.
1-4000
1-3200

1-5333
1-2666
1-3555

11. Greenfinch (*Fringilla Chloris*, Temm.).
1-6000
1-4800

1-6400
1-3555
1-4924

12. House Sparrow (*Fringilla domestica*, Linn.).	13. Yellow Bunting (*Emberiza Citrinella*, Linn.).
1-6000	1-5333
1-4570	1-4572
1-6400	1-6400
1-3200	1-3200
1-4682	1-4572

These measurements, excepting No. 5, were obtained from the particles of adult birds at different seasons. The lymph-globules in most of the above-named species, do not differ much in magnitude; and it is possible that further observations may show as much variety in the size of the globules of any one bird, especially if examined at different periods and compared in growing and mature specimens. In a few instances from one to five of the globules were seen to be inclosed with granular matter in a cell, the diameter of the latter varying from 1-2600th to 1-1114th of an inch. If the lymph-globule be regarded as a cell-nucleus, of course the smaller rounded particles which it frequently contains will be nucleoli. Sometimes from two to six of these may be observed in one lymph-globule, in which case they are very minute; and it is not unusual to see a single central and larger nucleolus varying from a quarter to half the size of the lymph-globule.

LXXII. *A Reply to some Observations of* Professor Kelland *in the Philosophical Magazine for May* 1842. *By the Rev.* M. O'Brien, *late Fellow of Caius College, Cambridge*[*].

THE observations made by Professor Kelland upon my paper published in the Philosophical Magazine last March, are calculated, I think, to produce an impression, that I have done nothing more than he has already done in his memoirs in vol. vi. of the Camb. Phil. Trans., and his ‘Theory of Heat.’ This imposes upon me the disagreeable task of defending myself in the following manner.

I shall briefly state *how far* I have pursued the same course as Professor Kelland, and *in what* we differ.

I have made use of a notation similar to that employed by Prof. Kelland; I have assumed that the particles of æther are acted upon by those of matter; and I have employed the equations of M. Cauchy, viz.

$$\frac{d^2\alpha}{dt^2} = \Sigma\, m\left\{ f(r)\,\delta\alpha + \frac{1}{r} f'(r)\,\delta x\,(\delta x\,\delta\alpha + \delta y\,\delta\beta + \delta z\,\delta\gamma) \right\},$$

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[*] Communicated by the Author.

and similar expressions for $\frac{d^2 \beta}{d t^2}, \frac{d^2 \gamma}{d t^2}$, adapting them to the case of a set of æthereal particles acted on by material.

So far as this I lay no claim to originality, nor has Professor Kelland any right to do so either.

After this there is not a single step we have taken alike. Professor Kelland's equations and results are essentially different from mine; and not only do we differ, but we are diametrically opposed to each other, so that if *one* of us be right the *other must* be wrong altogether.

I lay claim to originality in the following particulars, viz.

I have arrived at a result never obtained before, namely, that dispersion must arise from the *direct* action of the particles of matter upon those of æther. This result is denied by Professor Kelland.

I have obtained by a simple process, the equations

$$\frac{d^2 \alpha}{d t^2} = \mathrm{A} \frac{d^2 \alpha}{d x^2} + \mathrm{B} \left(\frac{d^2 \alpha}{d y^2} + \frac{d^2 \alpha}{d z^2} \right) + (\mathrm{A} - \mathrm{B}) \frac{d}{d x} \left(\frac{d \beta}{d y} + \frac{d \gamma}{d x} \right),$$

and similar expressions for $\frac{d^2 \beta}{d t^2}, \frac{d^2 \gamma}{d t^2}$. These equations were originally obtained by M. Cauchy and the late Mr. Green by very complicated methods: they prove that Mr. Kelland's equations in the Camb. Phil. Trans., vol. vi. page 159, are *essentially erroneous.*

I have given a simple proof of what was only *asserted* by Mr. Green, viz. that transverse and normal vibrations are in general propagated with different velocities. I have learned since that M. Cauchy had previously arrived at the same result. Professor Kelland distinctly denies the correctness of this result in the Royal Edinb. Trans., vol. xiv. p. 396.

And here I must enter a decided protest against *all* Professor Kelland's reasoning on the subject of transverse and normal vibrations. I assert that the equations at the foot of page 162 of the Transactions of the Camb. Phil. Soc. are *essentially erroneous*; they ought to have been in the form $\frac{d^2 \alpha}{d t^2} = -\mathrm{A} \alpha - \mathrm{B} \beta - \mathrm{C} \gamma$, and similar expressions for $\frac{d^2 \beta}{d t^2} \frac{d^2 \gamma}{d t^2}$; for the term $\Sigma \left\{ \frac{\mathrm{F}(r)}{r} \delta x \, \delta y \sin^2 \frac{k \, \delta \varrho}{2} \right\}$ which Professor Kelland makes out to be zero (in the same page) *is not zero,* as may easily be seen by putting for $\delta \varrho$ its value $e \, \delta x + f \, \delta y + g \, \delta z$, and expanding $\sin \frac{k \, \delta \varrho}{2}$ in the series $\frac{k \, \delta \varrho}{2} - \frac{k^3 \, \delta \varrho^3}{8} \frac{1}{1.2.3}$, &c.

Now this error in his *fundamental equations* vitiates all his results, so far as they relate to the nature of the vibrations and the velocity of propagation; indeed his expression for the velocity of propagation is manifestly erroneous, inasmuch as it is the same for transverse and for normal vibrations and is different for different values of $e\,f\,g$. This error runs through *all* Professor Kelland's papers and his 'Theory of Heat,' so far as I have read them; and it fully accounts for the manner in which he has spoken on this subject in the Royal Edinb. Phil. Trans., vol. xiv. p. 396.

I have also proved that the velocity of light in transparent bodies cannot be uniform unless the vibrations obey the cycloidal law. From this result I have shown, in a paper read before the Camb. Phil. Soc. last April, that *homogeneous light must in general suffer dispersion* in passing through a prism, and dispersion of a *discontinuous* nature; and from this I have shown that dark lines will be formed in the spectrum.

I have in the same paper shown that the results I have obtained on the hypothesis of perfect symmetry, are also true when the symmetry is disturbed by the action of the particles of matter.

Having thus stated how far I lay claim to originality, I must notice a formula for v^2, which Professor Kelland has brought forward in page 376 of the Philosophical Magazine for May 1842. Just before he mentions this formula, he speaks of the terms $C\alpha$, $C\beta$, $C\gamma$, which I have made use of in explaining dispersion, in such a manner as to imply, that he has done something of the kind himself. Now I cannot at all admit that he has anywhere made use of such terms, or anything equivalent to them, for he proceeds upon a hypothesis which, according to his own statement, makes C zero. He certainly endeavours, in his 'Theory of Heat,' to account for dispersion independently of the hypothesis of finite intervals (see pages 152–154), and accordingly by a process, which I confess I cannot understand, and which, *be it observed, Professor Kelland himself considers too uncertain to be trusted* (see middle of page 154), by this process he obtains the following formula for v^2:—

$$v^2 = \frac{D'}{D} V^2 + 2\,(Q-M)\,\Sigma\left\{ F \frac{\frac{\sin^2 k\Delta x}{2}}{k^2} \right\}.$$

Now I would ask Professor Kelland in the first place, why he has brought forward this formula in the manner he has done in the Philosophical Magazine, without at the same time quoting the words which immediately follow it in his book, viz. "*we shall not stop to discuss this formula as the subject is too uncertain to allow us to pursue it into detail.*" In the second place, I would ask Professor Kelland, is it possible that he thinks this formula capable of accounting for dispersion independently of the hypothesis of finite intervals? Is it not very evident, except that hypothesis be true, that $k \Delta x$ is extremely small, and the formula becomes

$$v^2 = \frac{D'}{D} V^2 + 2 (Q - M) \Sigma \left(F \frac{\Delta x^2}{4} \right),$$

which gives a value of v quite independent of the length of the wave $\left(\text{the constant } k = \frac{2\pi}{\lambda}\right)$. Why then has Professor Kelland produced this expression *as equivalent* to mine?

I need say nothing more on this point, except that Professor Kelland has no where else even attempted to account for dispersion independently of the hypothesis of finite intervals, and therefore I may fairly lay claim to originality on this head at least.

Professor Kelland asserts, on what grounds I know not, that the law of molecular force must be such as to make C zero. I have shown in the paper before alluded to, which was read before the Camb. Phil. Soc. last April, that if such be the case the whole universe is in a state of neutral equilibrium. I refer Professor Kelland to a paper in the Camb. Phil. Trans., vol. vii. p. 97, by Mr. Earnshaw, where I think quite enough is proved to show that the Newtonian law cannot be the law of molecular force.

In answer to the remark made by Professor Kelland at the foot of page 376, on my needless generality, a few words will suffice; the axis of x is not arbitrary, it was assumed by me to be an axis of symmetry, and every axis is not an axis of symmetry. To have arrived at my result, therefore, in the way recommended by Professor Kelland, I should have had first to prove that any line whatever drawn through the medium may be regarded as an axis of symmetry *; the simple method I have pursued avoids all this.

[The author promises the continuation of his former paper very shortly.—EDIT.]

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* Or in other words, that any three axes satisfy the condition

$$\Sigma f(r) \, \delta x^n \, \delta y^p \, \delta z^q = 0,$$

except $n \; p \; q$ be all even, which certainly should not be assumed.

LXXIII. *On the unexplored Coast of North America.* By Richard King, *Esq.*[*]

Of the northern configuration of America there remains unexplored but a small portion, nevertheless in that portion, small as it is, rests the grand problem of the north-west passage.

If Melville Peninsula forms the north-eastern boundary of America, there remains to be explored the space between its north-west termination, and Dease and Simpson's eastern limit in latitude 68° 28′ 27″ N. and longitude 97° 3′ W.; but on the contrary, if the land of North Somerset is a part of the American continent, and consequently its north-eastern boundary, the western coast of Boothia will have to be traced to Cape Nikolai the first, as well as the short space between Point Scott and Dease and Simpson's eastern limit.

I have no hesitation in stating that the whole may be explored in one summer at the very trifling outlay of a thousand pounds, by means of a small overland expedition. With this conviction I forwarded to Lord Stanley, in January last, the following communication:—

"My Lord, "4 Piccadilly, Jan. 24, 1842.

"In 1836 I had the honour of laying before Lord Glenelg, the then Colonial Secretary, a very humble and œconomical plan for determining the great question of the north-west passage, and of the northern configuration of America; but as His Lordship declined to entertain the plan, and having ascertained that such a course would not be offensive to the Government, I made an attempt to raise the necessary funds of 1000 pounds by public subscription. The subscription was going on favourably, when the Admiralty, at the recommendation of the Geographical Society, determined to fit out Sir George Back in the Terror, with instructions to make for Wager or Repulse Bays; then to cross the isthmus dividing those seas from the Gulf of Boothia, and by a boat navigation to survey the unknown polar coast line; and at the same time the Hudson's Bay Company despatched a surveying party overland to carry out my views; consequently, My Lord, I was forced to retreat into the shade with reflections of deep sorrow at the course adopted by Government, and of extreme delight at that pursued by the Hudson's Bay Company. From that time until the present moment I have never intruded myself either on the Government or the public. When the Government expedition failed my voice was not heard, although prior to its departure I had foretold the result, and had pronounced the

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[*] Communicated by the Author.

Geographical Society's recommendation to the Government as most impolitic. When the realization of the objects of the Hudson's Bay Company's expedition reached this country, although it proved the correctness of my most sanguine hopes, and confirmed those views which had been contended against by all the great northern authorities, I did not intrude myself upon the notice either of the Government or the public; and when title and honour and emolument were conferred upon the successful as well as unsuccessful travellers, I entertained no selfish feelings, nor the slightest sensation of regret at my less fortunate position; on the contrary, My Lord, my heart gladdened at the success of my more favoured associates, and even at this moment my happiest thoughts are those which dwell upon the route of the successful exploring party.

"But, My Lord, the interest which I excited after considerable expense and labour in 1836, has now died away. The death of Mr. Simpson by suicide in a fit of insanity, has put a stop to the new service which he contemplated, and I conceive that I may now come forward without being charged with intrusion, to urge Your Lordship to entertain my long-cherished plan; and although I shall be too happy to accept the command of that service, still if Your Lordship should think fit to appoint another person, I shall not repine, but be ever ready to further his views by imparting the knowledge which I have acquired.

"The plan which I have now to propose to Your Lordship, is precisely that which I published in 1836, with this exception, that the second division of the survey then laid down has now been completed, and a new line of coast has in consequence risen into importance. But in order to give the whole a connected form, I have thought it right to give the plan entire. I beg then respectfully to propose, that an expedition consisting of one officer and six men should proceed, towards the close of April, from Montreal in Lower Canada by the usual route of the Fur Traders to the Athabasca Lake, and having obtained an Indian guide, make direct for the Fish river by a route well known to the Chipewyan and Copper Indians. On the head waters of that stream the expedition should winter, and having ascertained the position of a tributary to the Great Fish river which takes its rise close to the proposed waters, in the ensuing spring follow its course, and afterwards the main stream to the Arctic Sea. By coasting along the eastern boundary of the Great Fish river estuary, there will be no difficulty in reaching the land of Boothia Felix, or in case of its insularity, the Hecla and Fury state. In the latter case the northern configuration of America will be complete; but if the former should prove to be as Sir John Ross has described, it will be necessary to follow the land to its northern limit, which will determine its connexion with or separation from the land to the eastward. If separated from that land, and the dividing sea proves of any extent, the grand problem of a practicable passage from the Atlantic into the Pacific is at once solved.

" Then the trending of the coast-line of the gulf of Boothia is but of minor importance; nevertheless, if the season is not too far spent, it will be desirable to add its boundaries to our knowledge. It is very probable that the Fish river falls into that gulf; if so it will be interesting to trace it to its source, and thus make a shorter route to the winter quarters. To render this practicable, it will be necessary that a party of natives should be engaged prior to starting from winter quarters in the spring, to wait at its mouth, in order to act as guides. There will be no difficulty in effecting this. The Camarade de Mandeville, a Chipewyan Indian, through whose hunting grounds the Fish river flows, offered to do so if Sir George Back thought proper to return by that route, instead of retracing his steps by the Great Fish river. It is impossible, My Lord, that the plan which I have sketched can fail but with the commander's life, and experience has proved that such a service can now be undertaken without more than the ordinary risk attendant on man's daily occupations.

" I have the honour to be,
" My Lord,
" Your Lordship's most obedient Servant,
" RICHARD KING."

" The Right Hon. Lord Stanley,
Principal Secretary of State for
the Colonies, &c. &c. &c."

To this I received the subjoined reply:—

" SIR, " C. O., Feb. 16, 1842.

" I have been directed by Lord Stanley to acknowledge the receipt of your letter of the 24th ult., offering to conduct an Expedition for the discovery of the N. W. Passage and of the Northern configuration of America; and to inform you that H. M^{s} Government do not at present contemplate any such undertaking.

" I have the honour to be,
" Your obedient Servant,
" E. B. WILBRAHAM,
" Pr. Sec."

" Richard King, Esq."

That the whole of the unexplored coast may be surveyed in one season is demonstrated from the fact, that the lands between the Mackenzie and the Coppermine, and between the

Coppermine and Great Fish river were delineated, in both instances, in less than six weeks of boat navigation. Moreover, the form and aspect of the unsurveyed lands are peculiarly favourable for ensuring complete success. In the contemplated survey, there are two western coasts and one bay which from past experience will almost for a certainty be found free of ice,—the only obstruction really to be dreaded in the prosecution of northern geographical research. There were no obstructions of importance in tracing the west coast of Greenland, the western coast of Cockburn Island, or the various extensive bays which indent the polar coast. Sir Edward Parry has pointed out this important feature in the geography of North America in the Narratives of both his second and third voyage. At page 46 of the former, he states, while anchored in the Duke of York's Bay of Southampton Island, " Scarcely a piece of ice was seen in any part; and the appearance of the beach, on which were no heavy-grounded masses, showed that here, as in all other well-sheltered harbours or inlets in the Polar Seas, little or none had ever found access, except that which is found in it, and which the annual process of dissolution has usually destroyed before the middle of August." At page 150 of the latter, he observes, " A circumstance which has particularly forced itself upon my notice in the course of our various attempts to penetrate through the ice in these regions is, that the east coast of any portion of land, or which is the same thing, the western sides of seas or inlets, having a trending at all approaching to the north and south, are at a given season of the year generally more encumbered with ice than the shores which have an opposite aspect."

Having said thus much in support of my progress along the coast, it only remains for me to allude to my overland journey to the sea. This I conceive would hardly have been necessary, as we have no instances on record of any difficulties of a serious nature obstructing the progress of an exploring party, had it not happened that in 1836, when I submitted the same overland plan to the consideration of the Geographical Society, Sir John Franklin raised several objections, which I shall first give in his own words, and then proceed to meet his objections.

REPORT OF SIR JOHN FRANKLIN.

" Mr. King's plan for the completion of the survey of the northern coast of America, which has been referred to me by the Council, appears to me so meagre in its details, that it will not furnish any satisfactory information for the guidance of the Council.

"The statements made in it respecting the river situated to the north and east of the Athabasca Lake, *have been gathered from the Indians, and have been repeated to every traveller since Hearne*, who has visited that part of the country. *Yet none have thought it prudent to follow the course which would lead to it, on account of the acknowledged difficulties in traversing the barren grounds for near* 300 *miles*, which is the estimated distance of its source from Fond du Lac. *The river there does not, it is imagined, flow to the northward, but to the eastward*, and probably falls into the sea in Knapp's Bay, or between the Chesterfield and Wager inlets. *Mr. King seems indeed to be aware of this supposition by his proposal of crossing a portage, and getting to Back's river by one of its tributaries*, whence he proposes to follow Captain Back's course to the sea: but the only ultimate object which I can discover in such a plan, is *to ascertain whether beyond Cape Hay the isthmus of Boothia be either met with or can be proved not to exist.*

"I do not know how Mr. King is to find fuel at the winter quarters which he proposes to make on the banks of the Thelewdezeth, or near the source of it, *as I have always understood that part of the country to be destitute of wood.* I much doubt also the possibility of a party getting a sufficient quantity *of fish in any river in that quarter to support it during a winter*; though as the barren grounds abound with deer, a sufficient quantity of these might, by possibility, be killed for a winter's supply, if a party arrived on the spot where it proposed to winter early in the autumn before *the deer quit these lands.* But this seems so improbable on Mr. King's plan, that on the whole I cannot recommend his proposal to the favourable consideration of the Council.

(Signed) "JOHN FRANKLIN."

1st. Suppose the information had only been gathered from the Indians, is it the less valuable on that account? Is it not, on the contrary, information of the most valuable kind? Let the existence of the Polar Sea, and the Fury and Hecla Strait; the source, course and outlet of the Great Fish river; and the trending of the east coast of Melville Peninsula, bear testimony to its value. But I deny that the existence of the Fish river depends solely upon Indian authority. At pages 75 and 154 of Sir George Back's Narrative, it is clearly stated that the river is also known to the fur traders.

2nd. One would suppose, that as the existence of the river has been mentioned to every traveller since Hearne, that a long list of travellers had visited that part of the country. Now it so happens that the only traveller since Hearne's time that could have the most remote idea of following that river to the sea was Sir George Back, and there has been none since his time.

3rd. That the long list of travellers, but which I have shown to contain but one, have not thought it prudent to follow the course which would lead to it, on account of the acknowledged difficulties in traversing the barren grounds for near 300 miles. Now it unfortunately happens for Sir John Franklin, that the whole route is well-wooded, and this he might have known if he had paid ordinary attention to Sir George Back's Narrative. At page 152 it is stated that the woods extend to 63¼°.

4th. As to the easterly trending of the river to the Chesterfield or Wager inlets, I know not of a single opinion in favour of it, but I know that at pages 80, 81, 84, 131, 197 of Sir George Back's Narrative, the river is stated to have the course I have assigned to it, which is N.E.

5th. As to the supposition that I was aware of that fact by my proposal to follow Back's river instead of the Fish river, it is very evident that Sir John Franklin had paid as little attention to my plan as he had to Sir George Back's Narrative, which before he undertook to report upon the subject, the Council of the Royal Geographical Society most undoubtedly had a right to expect of him. I should be mad indeed to follow the Fish river to the sea, when the whole course of the Great Fish river is not only known to me, but falls into the sea most conveniently for tracing the coast to the land of Boothia Felix; while on the contrary it is my opinion that the Fish river falls into the gulf of Boothia, and consequently if the isthmus of Boothia exists, I should have a barrier of land to the eastward to cut me off from the survey of the west coast of the land of Boothia Felix, where I imagine the passage lies.

6th. As to the ultimate object being merely to ascertain whether the isthmus of Boothia exists or not, I would reply, and quite enough when the problem of the north-west passage, which has been a favourite object with the British nation for more than three centuries, would by that means be solved.

7th. With regard to the country about the proposed wintering being destitute of wood, the very contrary is asserted to be the case at pages 81–86 of Sir George Back's Narrative; besides, the known confines of the woods place the fact of its being well-wooded beyond a doubt.

8th. I quite fall in with Sir John Franklin's doubts as to the success of a fishery in any river in the neighbourhood of my proposed wintering; but I cannot consent that he has a right to draw an inference that I should make so fruitless a search.

It must be evident to the most careless observer that my proposed wintering is situated on a height of land, as the opposite courses of the Fish river, the tributary to the Great Fish river, and other streams demonstrate. Under these circumstances, however slight may be my pretensions to the title of a naturalist, I should be sorry to have it supposed that I am so ignorant of the habits of fishes as to look for them in the sources of rivers during the inclement season. It is in the lakes and not in the sources of rivers that I should seek for the finny tribes.

9th. I have yet to learn that the deer quit the barren lands during the winter. I know to the contrary; but I have already made reference to the various parts of Sir George Back's Narrative, where it is stated that the country is well-wooded, and therefore abounding in animals of every kind. In my own Narrative of Sir George Back's expedition, I have entered more fully into the subject of the Fish river and its resources, but as at the time Sir John Franklin drew up his report that Narrative was not published, I have kept entirely to Sir George Back's Journal, which Sir John Franklin ought to have made himself acquainted with, but which it is evident he had never consulted.

When we consider the great results arising out of the various services that have from time to time been set on foot since the fifteenth century, both in a commercial and general scientific point of view, it is impossible not to wish for the further prosecution of the subject; and since a practicable north-west passage may yet be found, it does appear to me that the British nation will be altogether inexcusable if it allows the subject now to drop, when the accomplishment of the work, begun ages ago, is brought within a very narrow compass. It is by no means chimerical to express the opinion, that a practicable passage for commercial purposes will yet be discovered. The easterly set of the current through Behring and Barrow Straits, and the southerly set down Baffin's Bay, clearly demonstrates the existence of a communication between the Pacific and Atlantic. The result of four voyages has shown that the navigation of Barrow Strait may be effected with tolerable certainty; and that of the overland expeditions, there are no impediments between the straits of Behring and James Ross. All that is required, therefore, is a spacious sea to the north in the direction of the north Georgian group of islands of Parry. The channel once ascertained, any obstruction from ice would be easily obviated, as the annual passage of the Hudson's Bay ships through Hudson's Strait has for years demonstrated. The existence of such a sea will be determined by adopting the very œconomical plan which I have suggested.

LXXIV. *On some of the Substances contained in the Lichens employed for the preparations of Archil and Cudbear.* By EDWARD SCHUNCK, *Esq., Manchester*[*].

OUR knowledge concerning that department of organic chemistry which embraces the colouring matters, and other principles nearly allied to them, is of the most imperfect kind. Though many other branches of organic chemistry have been so thoroughly and accurately investigated, that little or nothing remains to be known concerning them, this may be called an unexplored field. Most of the colouring matters are so little known, as regards even their most essential characters, as not to allow us either to justify or to question the propriety of throwing them together into one general class; a class distinguished from those nearly allied to it merely by the (as far as we know) adventitious circumstance of the substances belonging to it being endowed with certain more or less vivid colours. Among all the colouring matters there are none, the study of whose properties and reactions is calculated to throw more light on the nature of the whole class, than those which are prepared by an artificial process from certain kinds of lichens, and on this account it is desirable that they should be carefully examined. It was the circumstance of these substances being prepared artificially from plants perfectly devoid of colour that first attracted to them the attention of chemists, and led to a series of investigations by which a number of highly interesting substances was brought to light, and a process elucidated which belongs to the most remarkable and unparalleled in the whole range of organic chemistry.

Robiquet first discovered a colourless crystallizable substance in them (orcin), capable of being converted by the joint action of ammonia and oxygen into a true colouring matter, which contains neither the original substance nor ammonia as such. This interesting discovery was followed by others. The researches of Heeren made us acquainted with a series of substances contained in the *Roccella tinctoria*, possessed of the same property, and another substance, phloridzin, was shown by Stas to bear a complete analogy to orcin in this respect. The subsequent labours of Dumas, who subjected orcin and the bodies derived from it to an accurate examination, and of Kane, who has determined the composition of the substances discovered by Heeren, and of the colouring matters contained in archil and litmus, seemed to have

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[*] Communicated by the Chemical Society, having been read January 4.

sufficiently elucidated the subject. Some obscurities, however, in a part of Dr. Kane's late paper seemed to make it desirable that some of his results should be confirmed before being finally adopted, and at the suggestion of Professor Liebig I undertook the re-investigation of this subject, and performed it in his laboratory.

Instead of the *Roccella tinctoria* I employed in my experiments the lichens that grow on the basalt rocks of the Vogelsberg in Upper Hessia, where they are collected for the purpose of preparing a dye from them. These lichens were all crustaceous and belonged to the genera *Lecanora*, *Urceolaria*, *Variolaria*, &c. From them I extracted the following substances:—

1. A white crystalline substance, soluble in alcohol and æther but insoluble in water, bearing in its properties great resemblance to the substance called by Heeren *Erythrin* and by Kane *Erythrilin*, but different in composition, and giving other products of decomposition. This substance I call *Lecanorin*.

2. A crystallizable substance identical in properties and composition with Heeren's *Pseuderythrin* and Kane's *Erythrin*.

3. A fatty substance of acid properties, soluble in alcohol but insoluble in æther and water.

The method by which these substances were extracted and separated from one another, was the following. The lichens were reduced to a coarse powder and then treated with æther, in an apparatus of displacement, until the æther dissolved nothing more. The æthereal extract, which had acquired a green tinge from chlorophyll in solution, was distilled off, leaving as a residue a greenish yellow mass, consisting for the greater part of lecanorin. This mass was brought into a glass funnel and washed with small quantities of æther, until it had lost its green colour in part. It was then treated with boiling water in order to remove every trace of pseuderythrin, and, lastly, purified by dissolving it in a small quantity of boiling alcohol, which deposited on cooling a snow-white crystalline mass, consisting of lecanorin in a state of purity. The dark green æthereal fluid obtained by washing the impure lecanorin, contained besides lecanorin the greatest part of the pseuderythrin which had been extracted by the æther. The fluid was evaporated to dryness and the residual mass treated with boiling water, which deposited on cooling a mass of shining plates and needles of pseuderythrin, which was purified by re-crystallization. More of this substance was obtained by treating the lichens, which had been exhausted with æther, with boiling alcohol and filtering rapidly. The alcohol was distilled off and the residue treated with boiling water, which dissolved all the pseuderythrin and deposited it on cooling. The mass left undissolved was washed with æther, which dissolved all the chlorophyll and left behind the fatty substance mentioned above, which was purified by re-dissolving in alcohol.

I will now proceed to a more minute description of the properties of these several bodies.

Lecanorin.

This substance, when pure, is perfectly white. If prepared in the manner described above, it has the appearance of a white mass composed of acicular needles. When its solutions are slowly evaporated, it crystallizes in silky needles grouped together in star-shaped masses. It is insoluble in boiling water, but soluble easily in alcohol and æther. Its solutions redden litmus paper. It is soluble in alkaline liquors, from which it is precipitated unchanged by acids, provided the solutions be not boiled and be not left to stand too long. It is insoluble in all weak acids, with the exception of acetic acid. Strong nitric acid converts it ultimately into oxalic acid. It combines with metallic oxides by double decomposition. Heated on platinum foil it melts, emits a dense vapour, and burns off, leaving but little carbonaceous residue. When heated in a tube closed at one end, it melts, and, under violent ebullition, gives off a dense vapour, which condenses in the upper part of the tube into a thick liquid, which after some time solidifies, forming a crystalline mass. The nature of this sublimate will be explained further on.

The action of the alkalies on this substance is of course the most interesting point connected with its history. A solution of lecanorin in ammonia when exposed to the air, acquires after some time a beautiful deep purple colour: from this solution acids precipitate a red colouring matter. A solution in potash, under the same circumstances, becomes of a deep red colour. Being desirous of ascertaining whether the lecanorin was immediately converted into the red colouring matter, or whether it passed first through any intermediate state, which was not improbable, I dissolved some of the substance in ammonia, excluding the solution from contact with the air. After a lapse of some hours, the solution, though perfectly colourless, was found no longer to contain any lecanorin; for acids, instead of producing a thick gelatinous or flocculent precipitate, as they do when applied immediately after solution has been effected, merely caused a brisk effervescence of carbonic acid, plainly showing that the substance had been completely decomposed without a colouring matter having been formed. The same effect was brought about instantaneously when the solution was boiled. In order to observe the process more clearly, I dissolved a quantity of lecanorin in baryta water in the cold. The solution, on being boiled or allowed to stand, deposited a great mass of pure carbonate of baryta. The liquid was filtered rapidly, and the excess of caustic baryta precipitated by a stream of carbonic acid: on slow evaporation it yielded large prismatic crystals of a substance which possessed characters in every respect identical with those of *orcin*. It had an extremely sweet taste, was capable of being volatilised without change and without leaving any residue, gave a deep blue colour when dissolved in ammonia and exposed to the air, struck a blood-red colour with nitric acid, and precipitated a solution of basic acetate of lead. Lecanorin thus is converted by the action of alkalies into orcin and carbonic acid, in the first instance, this decomposition always preceding the formation of colouring matters. The same decomposition is produced by the carbonated alkalies, by long boiling with water and by dry distillation, the heavy vapour mentioned above as being produced by heating lecanorin to decomposition, being vapour of orcin.

The composition of lecanorin is expressed by the formula $C_{18} H_8 O_8$. The results of the combustions which I made of it admit of no other interpretation. All attempts to determine its atomic weight by means of combining it with metallic oxides, failed. These compounds can only be prepared by double decomposition; but the facility with which lecanorin is decomposed when alkalies are added to its solutions, always renders the purity of the compounds formed liable to doubt. The compound with oxide of silver, formed by adding nitrate of silver to an alcoholic solution of lecanorin, and then precipitating by means of a few drops of ammonia, though it changed colour but slightly in drying, gave no consistent results. The compound with oxide of lead, formed by precipitating a solution of lecanorin with basic acetate of lead, was so basic and its formula so unusual, that I am led to suppose that one or two atoms of basic acetate of lead were precipitated together with it. By decomposing, however, a weighed quantity of lecanorin with caustic baryta, and determining the quantity of carbonate of baryta formed, I obtained very accurate results, confirming the formula $C_9 H_4 O_4$, or $C_{18} H_8 O_8$, for lecanorin. In regard to the composition of orcin, I have been induced to replace the generally received formula for its composition by a new one. Dumas's formula for anhydrous orcin is $C_{18} H_7 O_3$, and for crystallised orcin $C_{18} H_{12} O_8$, which evidently cannot be brought into accordance with the formula for lecanorin as given above. If, however, the formula $C_{16}H_6O_2$ be taken for anhydrous orcin, and $C_{16}H_{11}O_7$ for crystallized orcin, then the decomposition which lecanorin undergoes with alkalies may be expressed as follows:—

1 atom of anhydrous orcin . .	$C_{16}H_6O_2$
2 atoms of water	H_2O_2
2 atoms of carbonic acid . . .	C_2O_4
1 atom of lecanorin	$C_{18}H_8O_8$

Two atoms of water are furnished by the decomposition of the lecanorin itself, and three more by the fluid, to form from $C_{16}H_6O_2$ one atom of crystallized orcin, $C_{16}H_{11}O_7$. The combustions which I have made of this substance agree perfectly with these formulas, but Dumas's analyses of the lead compound of orcin, which I have myself not yet examined, do not coincide with them, unless it be supposed that this compound contains acetate of lead, either in chemical combination or mechanically mixed.

In regard to the numerical results from which the above formulas have been deduced, I shall reserve them for a future occasion, when, having completed the investigation of the whole class of substances of which those here described are only a part, I shall be able to enter more minutely into details, and exhibit the facts and numbers brought to light in their proper connexion and order. I have merely been desirous of showing, on the present occasion, that our knowledge of this series of bodies is far from being complete. I have shown above, that the action of alkalies on lecanorin is twofold; it consists, first, in abstracting from the substance carbonic acid, a process not requiring the co-operation of the oxygen of the atmosphere; secondly, in inducing in contact with the air the formation of colouring matters. The first action seems to have been overlooked in the case of all the bodies nearly allied to lecanorin. I have found the most complete analogy in the case of Heeren's pseuderythrin; and, if I am not mistaken in the interpretation of his statements, his erythrin also undergoes the same decomposition as lecanorin, for the former is converted into erythrin-bitter by the very same agencies by which lecanorin is converted into orcin, and in fact there is the same relation in regard to all general properties between erythrin and erythrin-bitter as between lecanorin and orcin. This circumstance is of some importance, for in order to arrive at a knowledge of the exact composition of such complex bodies as the colouring matters formed by the action of alkalies on these substances, and to understand perfectly the nature of the process by which they are produced, it is absolutely necessary to know the exact substance out of which each is in the last instance formed, the last link of the chain which precedes its formation.

Pseuderythrin.

For this substance it would be advisable to substitute another name, as in this case the substance by which it is accompanied is not erythrin but lecanorin. It is contained in very small quantities in the lichens that I examined. It is sparingly soluble in cold water, but easily soluble in boiling water, from which it crystallizes on cooling in shining plates and needles. If more of the substance is taken than the boiling water can dissolve, the part left undissolved melts and collects at the bottom of the fluid in oily drops, which, on the temperature falling a little below 212°, congeal and form crystalline masses. This is a characteristic property of pseuderythrin, and one distinctly mentioned by Heeren. It is easily soluble in alcohol and æther, and also in alkaline solutions. It gives compounds with metallic oxides by double decomposition. When dissolved in ammonia and exposed to the air, it gives, like lecanorin, a red colouring matter; but its conversion into the latter is much more slowly effected than that of lecanorin. When subjected to dry distillation it also gives a crystalline sublimate, accompanied by a copious disengagement of gas. When its solution in an alkali is boiled or left to stand some time, it imparts carbonic acid to the alkali, the decomposition being accomplished, however, with much more difficulty than with lecanorin. The exact nature of the substance left in solution after this decomposition I was unable to determine, on account of the very small quantity of pseuderythrin which I had at my disposal.

The combustions which I made of this substance confirmed the formula established by Liebig at the time of Heeren's investigation, viz. $C_{20} H_{12} O_8$.

The *fatty substance* mentioned above I have examined but slightly. It is soluble in alcohol, but insoluble in æther and water. From an alcoholic solution it is deposited in small pearly-white scales; if the solution be spontaneously evaporated, it is obtained in small, hard, shining, transparent crystals. It is soluble in alkalies, forming soapy solutions, and is re-precipitated by acids. Its alkaline solutions do not become coloured when exposed to the air. It cannot be melted without being decomposed.

LXXV. *On the Conversion of Benzoic Acid into Hippuric Acid in the Animal Economy.* *By* Mr. Alfred Baring Garrod, *of University College*[*].

A PAPER has appeared in the Medico-Chirurgical Transactions for last year, and also in the first Number of the Pharmaceutical Transactions, by Dr. Alexander Ure, in which it is stated, that by the internal administration of benzoic acid, or any of its salts, hippuric acid is formed in the system, and is eliminated from the kidneys in the form of a soluble hippurate, and that this hippurate is formed by the benzoic acid uniting with uric acid. It is also stated, that no trace of uric acid, or any of its salts, could be found in the urine after the administration of the benzoic acid.

I have repeatedly performed Dr. Alex. Ure's experiment, swallowing from a scruple to half a drachm of benzoic acid at a time, and have always obtained a copious crop of crystals of hippuric acid, amounting to from fifteen to twenty-nine grains, by the addition of hydrochloric acid to the urine passed about three or four hours afterwards (evaporated or not, according to its state of dilution). These crystals possessed all the characters of hippuric acid, with the crystalline form, the small solubility in cold water and æther, the ready solubility in alcohol, the evolution of nitrogen, and also the odour of the tonquin bean when heated to destruction; and my experiments therefore so far confirm Dr. A. Ure's fundamental observation. He also mentions another test of hippuric acid, viz. that when evaporated to dryness with dilute nitric acid, and ammonia added, a beautiful purple colour is produced. This is certainly true of the crystals obtained from the urine, but it is not a character of pure hippuric acid. The cause of this colour will be shown presently.

Dr. A. Ure states that no trace of uric acid could be found in the urine; but on examination I have always been able to obtain a distinct trace of uric acid from a drop or two of the urine, by adding a little nitric acid, carefully evaporating, and holding the capsule containing it over ammonia, when a distinct trace of murexide was formed; also, when the dish containing the crystals of hippuric acid is carefully examined, minute grains are found at the bottom, which are uric acid crystals; and on examining the crystals of hippuric acid with the microscope, uric acid crystals are found adhering to them in immense numbers, and this is the cause of the pro-

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[*] Communicated by the Chemical Society, having been read January 18.

duction of the purple colour spoken of, and which has been given as a test of hippuric acid. When the crystals are dissolved in alcohol the uric acid is precipitated, and the hippuric acid crystallized from the alcoholic solution no longer gives the purple colour. On collecting the uric acid from the same quantity of urine, formed on successive days, the same food being taken, one containing about twenty-seven grains of hippuric acid, and the other none, the following results were obtained:—

From 4½ oz. of urine, when no benzoic acid had been taken, uric acid 1·07 grain.

From 4½ oz. of urine, after taking 30 grains of benzoic acid, uric acid 0·96 grain.

Difference in favour of first, 0·11 grain.

In the second experiment also, a small loss might have occurred from the greater washing of the crystals necessary in that experiment. Now if we suppose that uric acid is decomposed to afford the elements necessary to be added to benzoic acid to form hippuric acid, we find that each equivalent of benzoic acid requires the addition of $C_4\ H_6\ O_4\ N$. To obtain the nitrogen, four atoms of benzoic acid would require one atom of uric acid, or half a drachm of benzoic acid would require rather more than ten grains of uric acid. Now the quantity of urine, in the experiment without the benzoic acid, only contained 1·07 grain of uric acid, and yet that quantity was not materially diminished when twenty-eight grains of hippuric acid were found in the urine. It cannot therefore be from the uric acid that the hippuric acid is formed.

If we examine the subject theoretically, it does not seem probable that such a body as benzoic acid, possessing such feeble affinities, and producing no sensible action on the body when taken, should be able to break up such a stable compound as uric acid; to abstract from the latter the requisite elements for its conversion into hippuric acid. But as hippuric acid is really formed in the urine, from whence does it obtain the necessary addition? The quantity of urea was noticed in several experiments to be deficient; could this be the source? We can find no rational formula for the explanation of the conversion if we suppose it to be from urea alone. We can, it is true, select the elements required; but, as in the last case, we should leave some compound in the system, which cannot be resolved into any known compounds, as ammonia, water, carbonic acid, &c., while from the ready conversion of the benzoic acid into hippuric acid we should expect that the change was one which could easily take place, without the action of any unusual affinities being brought into play. It occurred to me that it might be the lactate of urea, instead of pure urea, which is taken up; and upon comparing the formulæ for hippuric acid, benzoic acid, and the lactate of urea, it appeared that one equivalent of lactate of urea minus three eqs. of water, gave exactly the requisite elements for the conversion of 2 eqs. of benzoic acid into 2 eqs. of hippuric acid. 2 eqs. of benzoic acid + 1 eq. of lactate of urea = 2 eqs. of hippuric acid + 3 eqs. of water.

Hippuric acid (anhydrous)	$C_{18}\ H_8\ O_5\ N_1$
Benzoic acid (Do.)	$C_{14}\ H_5\ O_3$
Difference	$C_4\ H_3\ O_2\ N_1$
Twice the difference . .	$C_8\ H_6\ O_4\ N_2$
Lactic acid	$C_6\ H_5\ O_5$
Urea	$C_2\ H_4\ O_2\ N_2$
Lactate of urea	$C_8\ H_9\ O_7\ N_2$

Lactate of urea $- 3\ HO = C_8\ H_6\ O_4\ N_2$.

Now the urea has by MM. Cap and Henry been found to exist in human urine as lactate, and the separation of the elements of water is a change which might be expected to take place in the system under such circumstances. The benzoic acid merely taking up the lactate of urea, and throwing off water, is certainly a more probable occurrence than the destruction of such a stable compound as uric acid.

In analyses for the quantity of lactate of urea, according to the method of Cap and Henry, I found that although I could not obtain it in crystals, yet the quantity in a syrupy state was much reduced after taking the benzoic acid, and the same appeared on forming nitrate of urea from it. I obtained 14 grs. less of urea in 4½ ounces of urine when the benzoic acid had been taken. In another experiment I obtained 17 grs. less of urea when 30 grs. of benzoic acid had been taken; this is a greater loss than can be accounted for by the formation of the hippuric acid; but this can be referred to the urine, from some accidental circumstance, being of nearly as high specific gravity in this case as when the benzoic acid had been taken. 30 grs. of benzoic acid, swallowed, usually increased the specific gravity of the urine from four to six-thousandths.

From these results two inquiries suggest themselves:—1st, May not hippuric acid be formed artificially out of the body? 2nd, If sufficient benzoic acid were swallowed at such a time when least urea was contained in the urine, would the benzoic acid not cease to be all converted into hippuric acid, part of it then appearing in the urine unchanged?

LXXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 325.]

Jan. 20, 1842.—THE reading of a paper, entitled, "Researches in Physical Geology:" Third Series. By William Hopkins, Esq., M.A., F.R.S., was resumed and concluded.

In a paper formerly read to the Society, the author had investigated an analytical expression for the precession of the pole of the earth, on the hypothesis of the earth's being composed of a heterogeneous solid shell inclosing a heterogeneous fluid; and showed that its amount, deduced from that hypothesis, could not agree with its actual observed amount, unless the ellipticity of the interior surface of the shell were less by a certain quantity than that of the exterior surface. As the ellipticity of the inner surface (assuming always that the earth was originally fluid) depends on the thickness of the shell, the author, in the present paper, determines the least thickness which can be deemed compatible with the observed amount of precession.

In his former communication, the author had contemplated only the case in which the transition from the solidity of the shell to the fluidity of the mass contained in it was immediate; but in the case of the earth it must be gradual and continuous. It is remarked, however, that if in the actual case we were to consider all that portion of the mass as solid which is not perfectly fluid, we should take the thickness of the shell too great; and, on the other hand, if we were to consider the whole of that as perfectly fluid which is not perfectly solid, we should take the thickness of the shell too small. There must, consequently, be some surface of equal fluidity, (or, if we please, of equal solidity,) such that if all above it were perfectly solid, and all beneath it perfectly fluid, the precession would be the same as in the case in which the transition from the solidity of the shell to the fluidity of the interior mass is continuous. This surface is termed by the author the *effective inner surface*; and the distance between this surface and the outer one, the *effective thickness* of the shell.

The degree of solidity or fluidity at any point in the interior of the earth must depend partly on the temperature at that point, and may also depend partly on the pressure there. Both causes are here assumed to be effective: if the latter be not so, it will easily be seen that the conclusion arrived at will, *à fortiori*, be true.

If through any point in the interior of the earth, (as, for instance, a point in the axis of rotation,) we take a surface of equal temperature, and through the same point, a surface of equal pressure, it is evident that the surface of equal fluidity (or solidity) through that point must be intermediate to these two surfaces. Its exact position cannot be determined without an experimental knowledge, which we do not possess, of the relative effects of temperature in opposing, and of pressure in promoting the process of solidification. It is sufficient, however, for the purpose now in view, to know that it must necessarily lie between the surfaces of equal temperature and of equal pressure as its extreme limits; and of these the author proceeds to determine the position.

The forms of the isothermal surfaces within a spheroid have never been completely determined. The determination given by the author is an extremely approximate one when the ellipticity is small, and the time during which the process of cooling has been going on is very great, as it is presumed to be in the case of the earth.

The author then enters into the analytical investigation of this problem; and deduces the conclusion that we must descend to a depth greater than about one-fifth of the earth's radius before we arrive at a surface of equal fluidity (or solidity) having an ellipticity of the requisite value: that is, the effective thickness of the crust must be at least equal to one-fourth or one-fifth of the earth's radius, in order that the precession may have its observed value: a conclusion, the author observes, which entirely removes the foundation of certain vague and somewhat fanciful speculations in geology, proceeding on the hypothesis of the thickness of the earth's crust not being greater than twenty or thirty miles. It has been imagined that in active volcanos, the volcanic vent may communicate directly with the central fluid nucleus, whence the ejected fluid mass has been supposed to be derived. This notion, the author conceives, is rendered totally inadmissible, when it is proved that the thickness of the solid portion of the globe cannot be less than 800 or 1000 miles. It is also remarked, that it follows from the great thickness of the crust, that the present interior temperature of the earth cannot be due to its original heat unless pressure be effective in promoting solidification, a fact not yet established by experiment: for, if the present temperature be due to that cause, it is certain that it must be sufficient at the depth of probably less than fifty miles to reduce the matter composing the crust of the globe to a state of fusion under the atmospheric pressure; whereas it has been proved that the earth is solid to a very much greater depth; which can be accounted for, therefore, only by supposing its solidity to be preserved by the enormous pressure to which, at considerable depths, the mass is subjected. The author then offers an explanation of the phænomena of volcanos on the supposition that a portion of matter more fusible than the general mass of the globe exists in a state of fusion in subterranean reservoirs, forming so many subterranean lakes of determinate extent; in some cases originally distinct; in others, communicating with adjoining lakes, by more or less obstructed channels; a theory which will also account for all the obscure geological elevations, except perhaps the earliest, as being produced by a simultaneous action of a fluid pressure on every portion of the lower part of a solid mass of definite extent. The author considers this harmony in his general views with the results of analytical investigation as constituting for them a strong claim to the attention of geologists.

Another important conclusion which the author deduced from his researches is, that if the interior temperature of the earth be due to its primitive heat, pressure must be effective in promoting solidification of masses at high temperatures.

2. The following paper was read:—"Contributions to Terrestrial Magnetism," No. III. By Lieut.-Colonel Edward Sabine, R.A., F.R.S.

In this memoir, the author gives a detailed account of the observations on the magnetic intensity made at sea by the officers of the Erebus and the Terror on their passage from England to Kerguelen's Land; the unreduced observations transmitted to the Admiralty by the Commanders of these ships, Captain James Ross and Captain Crozier, having been placed in his hands for that purpose.

The first part of the paper relates to the observations made between England and the Cape of Good Hope; and the second, to those made between the Cape and Kerguelen's Land. These observations, made at various stations, are given in the form of tables; and their accordance with the isodynamic lines drawn from Mr. Dunlop's observations, contained in the first number of the author's contributions on this subject, is pointed out.

Jan. 27.—The following papers were read, viz.—

1. "Barometrical Observations made at Yarmouth, Norfolk, on the 21st of June and 21st of September 1841." By Arthur Utting, Esq. Communicated by Captain Edward J. Johnson, R.N., F.R.S.

2. "On the Anatomy and Physiology of the Decidua." By Robert Lee, M.D., F.R.S.

In this paper, the author describes some appearances which he has observed in the structure of the human decidua, and which apparently prove that the circulation of the maternal blood in the ovum is carried on during the early months of gestation, chiefly by the different layers of this membrane, and the cells of the chorion. He has been led by his observations to the belief, that the veins of the uterine decidua convey blood from the decidual cavity into the veins of the uterus; and that, in all probability, a current of maternal blood is constantly flowing from the cells of the chorion, through the decidua reflexa, into the decidual cavity.

Feb. 1.—At a Special Meeting of the Royal Society, held at nine o'clock A.M.,

Sir John William Lubbock, Bart., V.P. and Treas., in the Chair.

His Majesty the King of Prussia came to the Society, accompanied by Baron Alexander von Humboldt: whereupon the Vice-President in the Chair addressed him in the following words:—

"May it please Your Majesty,

"It is my duty to express to Your Majesty the great regret which we feel, and which we are confident that the Marquis of Northampton the President of this Society will participate in, that, being in a distant country, he is unable to be present upon this auspicious occasion, so interesting to the members, and which will long be gratefully remembered in the history of the Society.

"In his absence, therefore, I must endeavour, however imperfectly, to express to Your Majesty, the great gratification with which the Society will see the august name of Your Majesty, who is venerated as the encourager of art, of literature, and of science, enrolled in our Charter book in the same page with those of our most gracious and beloved Sovereign and her illustrious Consort; and we beg leave accordingly to present the Charter book to Your Majesty for that purpose."

His Majesty then signed his name in the Charter book, and was duly admitted a Fellow of the Society.

His Majesty expressed his gratification at having his name enrolled among the Fellows of the Royal Society.

Baron Alexander von Humboldt, formerly elected a Foreign Member, also signed his name in the Charter book, and was duly admitted a Fellow.

February 3.—The following papers were read, viz.—

1. "Abstracts of the Magnetic Term-day Observations for June, July, August, and September 1841," from the Honourable East India Company's Magnetic Observatory at Singapore.—2. "Graphical representations of the Term-day Observations from April to September inclusive, 1841," from the same Observatory.—3. "Abstracts of the Daily Magnetic and Meteorological Observations for September 1841," made at the Honourable East India Company's Magnetic Observatory at Madras.—4. "Abstracts of the Daily Magnetic and Meteorological Observations for September 1841," made at the Honourable East India Company's Magnetic Observatory at Simla.

The above were presented by the Honourable Court of Directors of the East India Company. Communicated by the Council of the Royal Society.

5. "Variations de la déclinaison et intensité horizontale magnétique observée à Milan, pendant 24 heures de suite, le 22 et 23 Décembre 1841, et le 19 et 21 Janvier 1842." Par Signor F. Carlini, For. Memb. R.S.

6. "A Meteorological Journal for 1841, kept at Allenheads, Northumberland, 1400 feet above the level of the sea, with an Appendix." By the Rev. W. Walton, M.A., F.R.S.

7. "Description of an Observatory constructed at Ardwick, and specification of the work performed in its erection." By John Jesse, Esq., at Ardwick. Communicated by Philip Hardwick, Esq., F.R.S.

8. "On the Steam-wave." By the Rev. Thomas Boys. Communicated by Charles Babbage, Esq., F.R.S.

The term *steam-wave* is employed by the author to denote that peculiar kind of wave which is generated during the motion of steam vessels on the water; and which he shows results from the combination of two separate sets of waves; namely, those occasioned simply by the progressive advance of the vessel, and which consequently recede from it on each side, nearly at right angles to its course; and those arising from the impulses given to the water by the action of the paddles, and proceeding in the same direction as the vessel itself. He ascribes to the cumulative force acquired by these waves at the parts where they cross one another, the extraordinary violence of effect with which they strike against all obstacles opposed to their progress, and which renders them so formidable to boats and other small vessels exposed to the encounter.

The Vice-President in the Chair stated, that he was directed by the Council to call the attention of the Members present, and through them of any philosophical inquirer who might at present be engaged in the prosecution of experimental research, to the existence of a fund at the disposal of the President and Council of the Society, denominated the *Donation Fund*, of which the dividends are to be applied, "from time to time, in promoting Experimental Researches, or in rewarding those by whom such researches may have been made, or in such other manner as shall appear to the President and Council for the time being most conducive to the interests of the Society in particular, or of science in general;" their application to extend to individuals of every country "not being at the time Members of the Council;" and such dividends not to be hoarded parsimoniously, but be "expended liberally, and, as nearly as may be, annually, in furtherance of the declared objects of the Trust."

The fund was instituted by the late Dr. Wollaston, who contributed £2000 three per cent. Consols, and it received the following additions:—from the late Mr. Davies Gilbert, £1000 three per cent. Consols; from Mr. Warburton, £105; from Mr. Charles Hatchett, £105; from Mr. Guillemard, £100; and from the late Sir Francis Chantrey, £105.

The Vice-President in the Chair further stated, that the dividends in the present year would amount to £140 16*s.* 6*d.*

Mr. W. Archibald Armstrong White, F.R.S., present at this meeting, gave £10 to the Donation Fund.

February 10.—The following papers were read, viz.—

1. "Magnetic-term Observations of the Declination, Inclination, and total Intensity, made at the Magnetic Observatory at Prague." By C. Kreil, Director of the Prague Observatory. Communicated by S. Hunter Christie, Esq., Sec. R.S.

2. "On the Chemical Analysis of the contents of the Thoracic Duct in the Human Subject." By George Owen Rees, M.D., Physician to the Northern Dispensary. Communicated by P. M. Roget, M.D., Sec. R.S.

The author, availing himself of a favourable opportunity which presented itself of examining the contents of the thoracic duct in a human subject, procured an hour and a quarter after death by hanging, to the amount of six fluid drachms, obtained by analysis the following result:—

Water, per cent.	90·48	
Albumen, with traces of fibrinous matter	7·08	
Aqueous extractive, or Zomodine	0·56	
Alcoholic extractive, or Osmazome	0·52	
Alkaline chloride, carbonate and sulphate, with traces of phosphate, and oxide of iron	0·44	
Fatty matters	0·92	
		100·

The fatty matters possessed the same general characters as those of the blood, except that they did not contain phosphorus, as appeared from their yielding an alkaline, instead of an acid ash by incineration. The aqueous extractive differed from that of the blood by giving a ferruginous ash. The salts obtained by incineration from the alcoholic extractive yielded a larger proportion of alkaline carbonate than those of the blood. The author is confirmed, by the experiments he made on the present occasion, in his former views concerning the cause of the white colour of the chyle, which he ascribes to the presence of opake white salivary matter as one of its constituents. The author then gives the results of his microscopical examination of the globules of the chyle, which he finds differ totally from those of the blood. He points out as being remarkable the large quantity of fatty matter existing in the chyle, and constituting an hydrocarbonaceous ingredient, which is constantly being added to the mass of blood, and is very rapidly consumed; as appears from the small quantity of this matter discoverable in the blood itself. The proportional quantity of osmazome in the chyle he finds greatly to exceed that contained in the blood.

February 17.—A paper was in part read, entitled, "On the Structure and Use of the Malpighian bodies of the Kidney, with Observations on the Circulation through that Gland." By William Bowman, Esq., F.R.S., Demonstrator of Anatomy in King's College, London, and Assistant Surgeon to the King's College Hospital.

February 24.—The following Meteorological Observations, taken in conformity with the Report drawn up by the Committee of Physics, including Meteorology, for the guidance of the Antarctic Expedition, as also for the various fixed Magnetic Observatories, have been communicated by the Lords Commissioners of the Admiralty and the Master-General of the Ordnance, viz.—

1. "Meteorological Observations taken on board Her Majesty's ship Erebus, for November and December 1840; and for January, February, March, April, May, June and July 1841." By Captain James Clark Ross, R.N., F.R.S., Commander of the Expedition. (*Forms* 1 & 2.)—2. "Meteorological Observations taken on board Her Majesty's ship Terror, for October, November and December 1840; and for January, February, March, April, May and June 1841." By Capt. T. B. M. Crozier, R.N. (*Forms* 1 & 2.)—3. "Meteorological Observations taken at the Magnetic Observatory, Cape of Good Hope, for February, March, April, May, June, July, August and September 1841." By F. Eardley Wilmot, Esq., Lieut. in the Royal Artillery. (*Forms* 1 & 2.)—4. "Meteorological Observations taken at the Magnetic Observatory, Ross-Bank, Van Diemen's Land, for April, May and June 1841." (*Forms* 1 & 2.)

5. The reading of Mr. Bowman's paper, "On the Structure and Use of the Malpighian bodies of the Kidney, &c., was resumed and concluded.

The author describes the results of his examination of the structure and connexions of the Malpighian bodies of the kidney in different tribes of Vertebrata, and shows that they consist essentially of a small mass of vessels, contained within dilated extremities of the convoluted uriniferous tubes. The tubes themselves consist of an outer transparent membrane (termed by the author the *basement membrane*) lined by epithelium. This basement membrane, where it is expanded over the tuft of vessels, constitutes the capsule described by Müller. The epithelium lining the uriniferous tube is altered in its character where the tube is continuous with the capsule, being there more transparent, and furnished with cilia, which, in the frog, may be seen, for many hours after death, in very active motion, directing a current down the tube. Farther within the capsule the epithelium is excessively delicate, and even, in many cases, absent. The renal artery, with the exception of a few branches given off to the capsule, surrounding fat, and coats of the larger blood-vessels, divides itself into minute twigs, which are the afferent vessels of the Malpighian tufts. After it has pierced the capsule, the twig dilates, and suddenly divides and subdivides itself into several minute branches, terminating in convoluted capillaries, which are collected in the form of a ball; and from the interior of the ball the solitary efferent vessel emerges, passing out of the capsule by the side of the single afferent vessel. This ball lies loose and bare in the capsule, being attached to it only by its afferent and efferent vessel; and is divided into as many lobes as there are primary subdivisions of the afferent vessel; and every vessel composing it is bare and uncovered, an arrangement of which the economy presents no other example. The efferent vessels, on leaving the Malpighian bodies, enter separately the plexus of capillaries surrounding the uriniferous tubes, and supply that plexus with blood. The blood of the vasa vasorum also probably enters this plexus. The plexus itself lies on the outside of the tubes, on the deep surface of the membrane which furnishes the secretion; and from it the renal vein arises by numerous radicles.

Thus the blood, in its course through the kidney, passes through two distinct systems of capillary vessels; first, through that within the extremities of the uriniferous tubes; and secondly, through that on the exterior of these tubes. The author points out striking differences between these two systems. He also describes collectively under the name of *Portal System of the Kidney*, all the solitary efferent vessels of the Malpighian bodies, and compares them with the portal system of the liver; both serving to convey blood between two capillary systems. In the latter, a trunk is formed merely for the convenience of transport, the two systems it connects being far apart. But a portion even of this has no venous trunk, viz. that furnished by the capillaries of the hepatic artery throughout the liver, which pour themselves either into the terminal branches of the portal vein, or else directly into the portal-hepatic capillary plexus. On the other hand, in the kidney, the efferent vessels of the Malpighian bodies, situated near the medullary cones, having to supply the plexus of the cones, which is at some little distance, are often large, and divide themselves after the manner of an artery. They are portal veins in miniature. In further confirmation of his view of the existence of a true portal system in the kidney of the higher orders of animals, where it has never hitherto been suspected, the author describes his observations on the circulation through the kidney of the Boa Constrictor, an animal which affords a good example of those in which portal blood derived from the hinder part of the body traverses the kidney. He shows that here the Malpighian bodies are supplied, as elsewhere, by the artery, and that their efferent vessels are radicles of the vena portæ within the organ, and join its branches as they are dividing to form the plexus surrounding the tubes; thus corresponding with the hepatic origin of the great vena portæ. In other words, the vena portæ is an appendage to the efferent vessels of the Malpighian bodies, and aids them in supplying blood to the plexus of the tubes. Thus in this variety of the kidney, as in the liver, there is an internal as well as an external origin of the portal system; while in the kidney of the higher animals, this system has only an internal or renal origin, viz. that from the Malpighian bodies.

A detail of the results of injection by the arteries, veins and ducts is then given, and they are shown to accord with the preceding description. Many varieties in the Malpighian bodies in different animals are also pointed out, especially as regards their size.

The author then proceeds to found on his previous observations, and on other grounds, a theory of a double function of the kidney. He conceives that the aqueous portion of the secretion is furnished by the Malpighian bodies, and its characteristic proximate principles by the walls of the tubes. After giving in detail his reasons for entertaining this view, he concludes by referring to the striking analogy between the liver and kidney both in structure and function, and by expressing his belief, first, that diuretic medicines act specially on the Malpighian bodies, and that many substances, especially salts, which when taken into the system have a tendency to pass off by the kidneys with rapidity, in reality escape through the Malpighian bodies; secondly, that certain morbid products occasionally found in the urine, such as sugar, albumen, and the red particles of the blood, also, in all probability, pass off through this bare system of capillaries.

This paper is illustrated by numerous drawings from injected and recent specimens.

March 3.—A paper was read, entitled, "On the Diurnal Temperature of the Earth's Surface, and the discussion of a simple Formula for ascertaining the same." By S. M. Drach, Esq. Communicated by John Lee, Esq., LL.D., F.R.S.

The author observes, in his introductory remarks, that during a period of twenty-four hours the quantity of calorific rays emitted from the sun, and falling on the exposed atmosphere of the earth, is proportional to one day's area as swept by the radius vector divided by the square of that radius; or is proportional to the true angular motion for that day; which is equivalent to substituting the declinations resulting from the true longitudes for those deduced from the mean ones at mean noons. On the arrival of the rays at the superior limit of our atmosphere, they undergo refraction, absorption, and difficulty of conduction; and when arrived at the surface of the earth, they suffer radiation and reflection; the absorption alone, at a vertical distance, amounting to upwards of one-fourth. The maximum sensible heat, he proceeds to observe, appears to follow the sun in its diurnal revolution, being similar, in this respect, to the point of maximum tidal height of the ocean; hence he applies the term *thermal esta-*

blishment to the retardation of the effects caused by atmospherical conduction and localities, in the same manner that the term *tidal establishment* has been employed to denote the local constant by which the astronomical effects on the tides are delayed.

The tables annexed to the paper assume that the degree of the thermometer is proportional to the cosine of the sun's meridian altitude, commencing with that on the day of observation, and ending with the altitude thirty days previously. After explaining the formation of these tables, and detailing the conclusions derivable from them, the author gives a sketch of the perturbing causes, such as oceanic evaporation, mountain ranges, and other local influences; he then enters into a discussion of the mathematical expression for the daily heat; and he concludes with some observations on the theories of temperature and isothermal lines, as affected by the electrical and magnetical conditions of the earth, dependent on its rotation on its axis.

March 10.—The following papers were read, viz.—

1. "Meteorological Observations, taken in conformity with the Report drawn up by the Committee of Physics, including Meteorology, for the guidance of the Antarctic Expedition; as also for the fixed Magnetic Observatories, at the Magnetic Observatory, Ross-Bank, Van Diemen's Land, for July and August 1841." Communicated by the Master-General of the Ordnance.

2. "Meteorological Register kept at Port Arthur, Van Diemen's Land, during the Year 1839." By Deputy Assistant Commissary-General Lempriere. Communicated by Captain Beaufort, R.N., F.R.S.

3. A paper was in part read, entitled, "Contributions to the Chemical History of the Compounds of Palladium and Platinum." By Robert Kane, M.D., M.R.I.A. Communicated by Francis Baily, Esq., V.P.R.S.

GEOLOGICAL SOCIETY.

Extracts from the Address delivered on the Anniversary, February 19th (1841), *by the Rev. Professor Buckland, D.D., P.G.S.*

[Continued from p. 434.]

BLACK BAND OF IRONSTONE IN SCOTLAND.

A most important discovery has recently been made in the coal formation of the West of Scotland, of several beds of ironstone (locally called the Black Band), which are of such great importance in the manufacture of iron, that its application to the smelting furnace has lately raised the value of a single estate at Airdrie more than 10,000*l.* per annum. There are several beds of this ironstone, varying from fourteen to twenty-two inches in thickness; they contain very little clay, and nearly as much carbonaceous matter as serves to calcine the iron; for this reason it is more valuable than the clay ironstones hitherto used, of which in this Scotch coal-field there are sixty-six. As it is probable that similar beds of this most valuable kind of iron ore may have hitherto been overlooked in other coal-fields, the attention of all coal-owners cannot too soon be directed to the discovery of the "Black Band" upon their own property*.

COAL IN SICILY, NEW ZEALAND, NEW HOLLAND, BORNEO, SOUTH AMERICA, AND KERGUELEN'S LAND.

At a time when steam navigation is assuming a character of incalculable importance to the world, the discovery of coal in any maritime position in distant regions that lie upon the great commercial highway of nations, demands the attention of all whose duty or interest it is to facilitate the means of rapid intercourse between the most distant extremities of the habitable globe.

Respecting Sicily, we have been informed by Dr. Calvert that he has himself seen a bed of good tertiary coal three feet thick, close to Messina, in a Fiumera to the left of Fort Gonzago, from which thirty years ago the English commander and himself laid in a stock for their winter fires, and which was used by our dragoons for their forge; although this is probably of tertiary formation, it may, like that of Cadebona, afford useful fuel.

From New Zealand I have seen a specimen of coal very like that of Staffordshire, found on the north shore of the southern island, near Cape Farewell, by the crew of a boat accidentally landing at the base of a cliff, in which the first thing noticed was a bed of coal three feet in thickness projecting over their heads. This coal in all probability will not only have material influence on the future destiny of the neighbourhood in which such a valuable repository of fuel has been found, but will also facilitate the intercourse by steam between this rising colony and our flourishing establishments in Van Diemen's Land and Australia.

In New Holland, in 1840, the Australian Company sold about 27,000 tons of coal at Newcastle on the river Hunter, with a rapidly increasing demand. And we learn from the Port Phillip Gazette, Oct. 28, 1840, that at Western Port, near Port Phillip, an exploring party has discovered coal of excellent quality, but at some distance from water-carriage.

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* Mr. Hawkshaw's observation as to the manner in which flashes of bituminous mud, from putrescent lagoons, overflow the country adjacent to them, in the tropical regions of Venezuela, on the arrival of rains after a season of drought, may illustrate the cause of the presence of the large quantity of inflammable matter which occurs in the rich iron ore of the so-called Black Band.

A similar discharge of bituminous mud from lagoons over the surface of certain beds of growing vegetables in the time of the coal formation, may have been the cause of converting the beds thus overflowed and impregnated with bitumen into Kannel or Candle coal; and an argument in favour of this hypothesis is supplied by the fact of the microscopic structure of the plants in Candle coal being more distinctly and universally preserved throughout the entire mass, than in ordinary coal. Similar bituminous irruptions may have caused the sudden death and perfect preservation of the fossil fishes that swarm in certain beds of highly bituminous shale of the coal formation, as also in the copper slate of the Hartz, and other bituminous shales.

Mr. Tradescant Lay has also laid before us a notice of the existence of coal, or valuable lignite, in the island of Borneo: should a large supply of it be found in this island, it may become a station of inestimable value for effecting intercourse by steam between China, India and Australia, and the great islands of the Malay Archipelago.

It appears by recent accounts from Valparaiso, that an abundant supply of good coal has lately been obtained at Talcahuano, with which the steamer Peru has made a successful voyage to and from Copiapo*.

We have just learnt from Captain James Ross that good coal has been discovered in Kerguelen's Land in the Southern Ocean.

IGNEOUS ROCKS.

We have, from the Rev. D. Williams, an account of a mass of trap, intersecting the mountain limestone, red marl and lias at the W. end of Bleadon Hill, on the Bristol and Exeter Railway. It resembles in its character that of Hestercombe, on the flank of the Quantoc Hills N.W. of Taunton, and is the first discovery of trap connected with the line of elevation of the Mendip chain. This protrusion of trap is attended by a remarkable fault, which brings the edges of bent strata of lias into contact with those of mountain limestone. Mr. Penistone has also supplied an instructive section of this cutting. The nearest known trap rocks to the Mendips are that of Hestercombe in the Quantoc Hills just mentioned, and that near Tortworth and Berkeley.

In a paper on the Isle of Madeira, Mr. Smith of Jordan Hill has supplied, I believe, the first geological description of this island, the structure of which has long been a desideratum to geologists. Little

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* As no more coal is in process of formation, and our national prosperity must inevitably terminate with the exhaustion of those precious stores of mineral fuel which form the foundation of our greatest manufacturing and commercial establishments, I feel it my duty to entreat the attention of the legislature to two evil practices which are tending to accelerate the period when the contents of our coal-mines will have been consumed. The first of these is the wanton waste which for more than fifty years has been committed by the coal-owners near Newcastle, by screening and burning annually in never-extinguished *fiery heaps* at the pit's mouth, more than one million of chaldrons of excellent small coal, being nearly one-third of the entire produce of the best coal-mines in England. This criminal destruction of the elements of our national industry, which is accelerating by one-third the not very distant period when these mines will be exhausted, is perpetrated by the colliers, for the purpose of selling the remaining two-thirds at a greater profit than they would derive from the sale of the entire bulk unscreened to the coal-merchant.

The second evil is the exportation of coal to foreign countries, in some of which it is employed to work the machinery of rival manufactories, that in certain cases could scarcely be maintained without a supply of British coals. In 1839, 1,431,861 tons were exported, and in 1840, 1,592,283 tons, of which nearly one-fourth were sent to France. An *increased duty* on coals exported to any country, excepting our own colonies, might afford a remedy. See note on this subject in my Bridgewater Treatise, vol. i. p. 535.

has hitherto been known beyond the fact that all its shores and its general aspect are volcanic; Mr. Smith has at length discovered sections at the elevation of about 2000 feet, in the central part of the island, which exhibit compact limestone, containing fossil remains of Conus and many other shells of the tertiary period. Nothing is visible beneath this limestone, but above it are lofty precipices which exhibit several beds of sub-aërial lava, lapilli and ashes, alternating with beds of soil converted to brick by the beds of lava incumbent on them. In some of these volcanic beds of loose texture, there occur abundant remains of small roots of trees converted to carbonate of lime, in which few traces of structure have been preserved. I have occasionally seen similar remains of roots, in a state of lac lunæ, in loose calcareous sand, and gravel-beds in England, *e. g.* in the coralline gravel of the lower greensand formation at Coxwell, near Faringdon, and in a diluvial sand and gravel-pit near Claydon in Buckinghamshire.

GEOLOGICAL DYNAMICS.—GLACIAL THEORY.

During the last year M. Agassiz has introduced a new and powerful machinery into the Dynamics of Geology, by asserting the claims of ice to be admitted to the list of locomotive forces that have operated largely not only in forming *morains* (*i. e.* mounds and ridges of gravel and clay intermixed with large fragments of rocks) on the flanks and at the lower extremity of existing glaciers, but also in transporting erratic blocks with the detritus of morains to distant regions, and re-arranging them by the force of floods that originated in the melting of ice and snow.

In the month of June 1840, a notice was read to us by him on the polished and striated surfaces of rocks in the beds of glaciers in the Alps; and another notice in the following November, on the evidence of the existence of glaciers in Scotland, Ireland, and England. In the summer of 1840 he published in Switzerland, in his 'Etudes sur les Glaciers,' a description of facts which lie at the foundation of this question, illustrated by a splendid series of plates, representing the actual condition and residuary effects of existing glaciers in the Alps. These phænomena are so essentially preliminary to the investigation of the evidences of ancient glaciers in regions where they are now unknown, that no man is fully qualified to enter upon this question who has not prepared himself by the study of modern glaciers with a special view to their *residuary phænomena*, which have been overlooked, or referred to other causes by preceding observers in Alpine regions.

After due acknowledgment of the discoveries of Scheuchzer, Gruner, De Saussure, Hugi, Venetz, and Charpentier, M. Agassiz examines the origin of glaciers in the transformation of snow into solid ice, the different conditions of this ice in its various stages of advancement, the causes of its movement, the history of the detritus that falls upon it and is transported along its surface and lodged in the form of morains upon its sides and at its lower extremity, and the modifications of these morains by the waters of temporary ponds and lakes formed upon and within the glaciers. He also investigates the action of modern glaciers in polishing and producing striæ, ridges and furrows, and rounded bosses resembling wool-sacks (*Roches moutonnés* of De Saussure and *Roches bosselées* of Hugi), on the surface of the hardest rocks over which they pass; and also in grinding to the state of pebbles fragments of rocks that are forced along their bottoms, and in transporting to great distances large blocks of stone interspersed through the substance and poised upon the surface of morains.

Within the records of history the lower terminations of many glaciers have varied considerably, and the morains left by them in the valleys show the extent to which the ice has descended in times comparatively modern. Agassiz has recognized the association of similar residuary phænomena not only in valleys of the Alps below the level of the present glaciers, but along the whole south-east flank of the mountains of the Jura, which run parallel to the Alps at the distance of fifty miles on the north-west side of the great valley of Switzerland. He finds on the Jura limestone, at various heights, from the level of the Lake of Neufchatel to three thousand feet, evidences from which he infers that glaciers descending the great valleys of the Alps have extended across the entire valley of Switzerland over the lakes of Neufchatel and Geneva (then converted into ice), until their course was stopped and deflected in directions parallel to the Jura by the obstructing barrier which this mountain-chain presented. These evidences consist, 1st, in erratic angular blocks of the granite of Mont Blanc, and other rocks from the high Alps, lodged on the south-east face of the Jura in insulated positions, and frequently upon banks of sand and gravel analogous to the morains now forming in the Alps; 2ndly, in the frequent occurrence of polished surfaces, striæ and furrows on the Jura limestone, similar to those now produced at the bottom of existing glaciers; 3rdly, in the coincidence of these striæ with the direction in which a glacier from the Alps would have been deflected by the barrier presented to it by the Jura, and their non-coincidence with the slope of these mountains; 4thly, in the existence upon the polished surfaces of the Jura limestone of funnel-shaped cavities (*couloirs*), and small indentations similar to the *lapiaz* we see daily forming at the bottom of glaciers by small and temporary cascades descending through cracks and chasms of the ice.

M. Agassiz contends, that this quadruple series of phænomena, which are common to the south-east slopes of the Jura, and to the bottom of existing glaciers in the Alps, is inexplicable on any theory of aqueous action apart from ice; and still further argues that the concurrent appearance of similar phænomena in other regions of the world justifies the inference that these also have been the site of glaciers. He moreover infers, that very large portions of the now temperate regions of the globe have for a long period been enveloped with a winding-sheet of snow and ice.

In November 1840, the evidence of the existence of glaciers in Scotland and the north of England has been brought before us in three communications: the first detailing the observations of M. Agassiz and Dr. Buckland conjointly during a recent tour in Scotland; the second recording Dr. Buckland's observations in Scotland, Northumberland, Cumberland and Westmoreland; and the third containing evidences of glacial action collected by Mr. Lyell in Forfarshire and the valley of Strathmore.

The phænomena in Scotland, wherein M. Agassiz and Dr. Buckland recognized the evidences of glacial action, consist in the union of rounded, polished, striated and furrowed surfaces with morains and transported blocks, analogous to the similarly associated phænomena upon the Jura and in the Alps. They are described in the six following localities. 1st, the morains on the summit level of the road between Inverary and Loch Awe: 2ndly, the rounded, polished and striated surfaces of granite near the water's edge at the ferry of Bunawe, and the morains adjacent to it near Mucairn: 3rdly, the polished and striated surfaces of granite, between high and low water, at the ferry of Ballahulish on Loch Leven: 4thly, the rounded, polished and striated surfaces, accompanied by morains, in Glen Roy and the valley of the Spean; from the position of which they infer that the lake, to which many writers have referred the origin of the parallel roads of Glen Roy, was caused by two glaciers descending from Ben Nevis across the valley of the Spean, in the same manner as in 1818 a temporary lake was formed by a barrier of ice in the Val de Bagnes above Martigny; and as at this time, a barrier formed by the glacier of Miage protruding across the Allée Blanche is the sole cause of the Lake Combal, which would immediately be left dry like Glen Roy, should any cause remove the protruding barrier of the glacier of Miage*: a fifth locality, in which there is the same concurrent evidence of morains loaded with transported blocks, and of rounded and polished surfaces on the sides and bottom of a mountain valley, occurs near Sir George Mackenzie's residence at Coul, at the south-west base of Ben Wevis: the 6th and last locality visited conjointly was the site and neighbourhood of the town of New Aberdeen, where the polished surface of the granite had been noticed by Dr. Fleming, and where remodified detritus of morains forms the hillocks of gravel between the town and the sea on the north side of the estuary of the Dee, and cliffs of gravel and till or boulder clay occur on the south of the same estuary.

In another communication Dr. Buckland records his observation of similar phænomena in the valley of Strathmore; in the highland valleys of the Tay and Tumel; on the north-east shoulder of Schiehallion; in the high pass of Glen Cofield, between Taymouth and Strathearn; in Glen Lednoch and Glen Turret, on the north of Comrie; on the sides of Loch Earne; and in the valley of the Teith between Loch Katerine and Doune.

In the lowland districts he notices also the occurrence of rounded, polished and striated surfaces upon the top of the basaltic rocks of Stirling Castle, on the north face of the Castle Rock at Edinburgh, at Blackford hill, on Calton hill, the Costorphin hills, and

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* See Captain Basil Hall's Patchwork, vol. i. p. 114.

other hard trap rocks near Edinburgh, many of which have been described and attributed to diluvial action by Sir James Hall.

In Northumberland Dr. Buckland describes an immense accumulation of morains, or detritus of morains, at the east base of the Cheviots, near Wooler; and in the lake districts of Cumberland and Westmoreland he found the sides of many mountain valleys and gorges, by which the waters of these lakes have their exit to the adjacent plains, to bear marks similar to those produced by glaciers, viz. rounded, striated and polished surfaces, accompanied by the accumulation of mounds of gravel and erratic blocks in the low countries subjacent to them.

Mr. Lyell has read a paper on the evidences of the action of ice in Forfarshire, and has re-examined that county in order to satisfy himself whether the boulder formation of the district, which he had previously regarded as the effect of drift-ice on submerged land, might be explained by the agency of ice acting on land already elevated above the sea. This latter conclusion he is now inclined to adopt, believing that it is favoured by the mounds of transported materials bearing the form of morains, and for the most part unstratified, which occur on the sides of almost every valley in the Grampians, and sometimes *across the glens* at right angles, and almost blocking them up. He finds this opinion further confirmed by the local distribution of rocky fragments, and the evidence of their descent from higher to lower levels; and, lastly, he thinks that the rarity of organic remains in the till or boulder clay lends support to the same view. He mentions several deep lakes in the Grampians in Forfarshire, on the lower sides of which enormous accumulations of mud, gravel and angular blocks are strewed, which are derived from precipices on their higher side; these materials would have filled up the lakes, unless we suppose them to have been formerly occupied by ice.

The effects of drift-ice in producing alternations of stratified and unstratified deposits, and in causing curvatures in strata of sand and gravel, while underlying beds remain horizontal and undisturbed, were treated of last year by Mr. Lyell in a paper on the mud-cliffs of Norfolk. But in Forfarshire the till, or unstratified matter containing boulders and angular blocks, is found everywhere underlying the stratified sand and clay; had the whole deposit been accumulated under water, we might have expected alternations; Mr. Lyell therefore conjectures that the older till may have been formed in great part when the glaciers were gradually advancing over the country, at the period of the first coming on of a colder climate, and that portions of the morains may have become subsequently stratified in temporary lakes, or during floods in those valleys where stratification is observable.

Another feature in the distribution of the transported materials of Forfarshire and Perthshire is a continuous stream, from three to three and a half miles wide, of boulders and pebbles, traceable from near Dunkeld by Coupar and the south of Blairgowrie into Strathmore, and thence in a straight line through the lowest depression of the Sidlaw hills from Forfar to Lunan Bay, a distance of thirty-four miles.

No great river follows this course, but it is marked everywhere by lakes or ponds, which afford shell-marl, swamps, and peat-mosses, commonly surrounded by ridges of detritus from fifty to seventy feet high, consisting in the lower part of till and boulders, and in the upper part of stratified beds of gravel, sand, loam, and clay, which in some instances are curved or contorted; the form of the included spaces is sometimes oval, sometimes quadrangular. No organic remains have been found in the surrounding ridges, but they resemble greatly in form the mounds of detritus which may once have constituted the lateral, transverse, or medial morains of a great glacier.

Mr. Lyell compares the chain of this part of the Grampians to the Alps, the parallel chain of the Sidlaw hills to the Jura, and Strathmore to the great valley of Switzerland; and the resemblance, he says, is increased by the occurrence in Strathmore and on the Sidlaw hills of blocks derived from the Grampians. He is of opinion that the agency of ice moving upon dry land may account for many appearances which are inexplicable on any other hypothesis, and that this theory must not be rejected because it fails to remove at once every obscurity; especially as various other geological causes, such as oscillations of level in the land, the temporary submergence of portions of it during the supposed glacial period, and the action of drift-ice, may all have co-operated with glaciers to produce the boulder formation. He also hints, that the glaciers of Switzerland, being situated eleven degrees further to the south, can present but an imperfect analogy to the state of things which may once have prevailed in Northern Europe; it is to Sandwich or Kerguelen's Land, or to South Georgia, and other regions of the southern hemisphere corresponding in latitude to Scotland and England, that we must look for instruction; for these southern and antarctic lands are buried summer and winter beneath perpetual snow, which reaches even to the sea-coast, and yet in the case of South Georgia this perpetual snow is distant only nine hundred miles from Terra del Fuego, a country placed in the same latitude and yet clothed with luxuriant forests. Assuming therefore that the Grampians, Alps, and Jura, and all Scandinavia, were once permanently overspread with snow, he thinks we cannot therefore conclude that the whole globe between the fortieth parallel and the poles was invested simultaneously with a sheet of ice, nor even that the general climate of the whole earth differed materially from that prevailing in our own time.

Mr. Murchison, in an admirable chapter (c.39.) of his Silurian System, on the Position and Mode of Transport of Boulders which occur in the Northern Drift, has stated good reasons for believing that such a change of climate may have taken place at the epoch of the transport of erratic blocks as permitted the formation of icebergs on the shores and rivers of Cumberland, Scotland, and Ireland; which being drifted southwards, strewed their load of large stones and gravel over the bottoms of then adjacent seas. He also quotes with approbation the ingenious imagination by Mr. C. Darwin, of a proportional distribution of the land and water in central and northern Europe, very different from the present, and under which the southern part of Scotland might present an island "almost wholly covered with everlasting snow," having each bay terminated by ice-cliffs, from which great masses yearly detached would transport fragments of rocks to distant regions; and infers, that as in other parts of the world *there are* conditions in which ice becomes a motive power, such conditions *may* also have existed in our latitudes.

Mr. Murchison has also proposed to explain the dispersion of erratic blocks now resting on beds of clay and sand containing recent species of arctic shells over large districts in the interior of Russia, by supposing "that they had been floated in icebergs, which breaking loose from ancient glaciers in Lapland and the adjacent tracts, were drifted southwards into seas which have been since laid dry." He further suggests, that icebergs loaded with detritus may, by grating upon the bottom of these seas, have produced the parallel striæ and polished surfaces on the rocks over which they were drifted; and concludes with admitting so much of the glacial theory as to allow that in former days glaciers probably advanced further to the south, and occupied many insulated tracts, and to a much greater extent than at the present day.

We learn from Professor Hitchcock's excellent work on Elementary Geology (August 1st, 1840), that parallel striæ and furrows, accompanied by rounded and polished surfaces of all the harder rocks, and that vast longitudinal mounds and tumuli of detritus, and erratic blocks sometimes at the distance of many hundred miles from their native place, have been lately observed in so many provinces of the United States, that these phænomena may be placed in the category of geological constants in North America. They have been noticed in Maine, New York, New England, Rhode Island, Massachusetts, Connecticut, Ohio, Michigan, and Illinois, at various elevations, sometimes from 3000 to 4000 feet above the level of the sea; the prevailing direction of these striæ and furrows is from N.W. to S.E.

We have also long been familiar with the streams of erratic blocks that have been traced south and south-eastwards from the mountains of Scandinavia to the shores of Germany; and more recently Sefström and Bötlingk have informed us that polished striated and furrowed surfaces are also of constant occurrence in Norway, Sweden, Finland, and Lapland, their mean direction being, like the course of the erratic blocks, from N.W. to S.E. Bötlingk, however, has observed that some of these furrows have centres of dispersion (as in the case of those produced by modern glaciers that radiate from the Alps), and follow the direction of the major axis of each valley, whilst the general direction of the striæ on the summits in Scandinavia is from N.W. to S.E. He, moreover, states, that in the south of Sweden the striæ incline southwards, but on the east of Lapland northwards to the icy ocean; the same conformity in the direction of the striæ with that of the major axis of each valley, occurs also in Scotland, Cumberland and North Wales.

Thus we find, that not only the highest and northern mountain groups in the British Islands, but vast regions also of the continents of Northern Europe and of North America have been subjected to the same great physical forces, glacial and diluvial, under much colder conditions of the northern hemisphere than prevail at present; and this apparently at a time intermediate between the extinction of European and American elephants by cold, and the creation of the human race. We have not yet, however, sufficient materials for the full admeasurement of the amount of influence which has been exercised by ice in its various forms upon the surface of the globe, and the following are important desiderata. With respect to elongated ridges and tumuli of gravel, it remains to discriminate how far they may have been derived from, or modified by, the action of ice under one or more of the three following conditions: 1. Were they lodged by glaciers alone, without the agency of water, in the form of morains on their flanks and front? 2. Have they been stranded by icebergs loaded with gravel upon the shores of lakes, or estuaries, or seas? 3. Have they been dropped in deep water by floating and melting icebergs, and re-arranged by whirlpools and conflicting currents in the form of oblong reefs and groups of obtuse cones which they actually present? Another large field of inquiry must be forthwith entered upon, in the distinctions we shall have to make between raised sea-beaches and each of the three last-named residuary effects of glacial action.

With respect to *scorings* also and *dressings* on the surfaces of rocks, it is very desirable that we should find some criterion whereby to distinguish between the grinding effects of glaciers marching slowly along dry land, and of icebergs dredging the bottom of the sea, and of large stones and gravel drifted simply by water, in producing striæ, grooves and furrows, together with rounded and polished surfaces on the rocks over which they respectively advance.

I see not yet by what test we may distinguish these residuary phænomena where they occur in regions now remote from either of the causes most competent to their production, viz. in countries that now enjoy a temperate climate and are in some cases elevated nearly four thousand feet above the level of the sea; for where the supposed agent is ice armed and transfixed with stones projecting like the teeth of a file from its base and sides, the effects of similar instruments on similar materials would probably be the same, by whatever cause a slow progressive motion may have been imparted to them; and whether on dry land or beneath the sea.

It remains, moreover, to ascertain to what extent the sudden elevations of land may have produced great movements of water and diluvial inundations by gigantic waves, analogous to those which are occasioned by modern submarine volcanic action; and to inquire into the effects that may have been produced on the sides and bottoms of valleys of denudation by the drifting of the hard materials that must have been swept through them at and after the time of their excavation.

A further subject of inquiry is, whether there be parallel striæ and furrows on the truncated and abraded surfaces of older rocks that have been overlaid by more recent strata, after an interval in which these surfaces had been exposed to the action of the sea. In cases of this kind that have come under my observation, the surfaces have only been cut off transversely and ground smooth, like the shores of the present seas; but they have no such parallel striæ as those which are of general occurrence beneath diluvium or drift*; nor have large erratic blocks from distant regions been found mixed with the gravel of any of the older conglomerate rocks.

One great cause of the difference of opinion between the diluvialists and the glacialists, is the exclusiveness with which each party would insist upon the agency of the cause which they respectively adopt: the diluvialist apparently errs in refusing to admit the agency of glaciers in mountain valleys that are below the existing limits of ice and snow; whilst Agassiz may have erred in urging too far his theory of expansion as the great locomotive power of glaciers over regions whose surface is too little inclined to admit their progression by the force of gravity; a middle way between these two extreme opinions will probably be found in the hypothesis, that large portions of the northern hemisphere which now enjoy a temperate climate have at no very distant time been so much colder than they are at present, that the mountains of Scotland, Cumberland, and North Wales, with great part of Scandinavia and North America, were within the limits of perpetual snow accompanied by glaciers; and that the melting of this ice and snow was accompanied by great debacles and inundations which drifted the glaciers with their load of detritus into warmer regions, where this load was deposited and re-arranged by currents at vast distances from the rocks in which it had its origin. The contest will probably be settled, as in most cases of extreme opinions and exclusive theories, by a compromise; the glacialist will probably abandon his universal covering of ice and snow, and be content with glaciers on the elevated regions of more southern latitudes than now allow of their formation; the diluvialist, retaining his floating icebergs as the most efficient agents in the transport of drift and erratic blocks to regions distant from their place of origin, may also allow to glaciers their due share in the formation of morains and striated surfaces, in latitudes and at elevations that are no longer within the zones of perpetual congelation.

PHOTOGRAPHY.

A valuable application has been made by Captain Ibbetson of a Photogenic process for rapidly producing perfect drawings of fossil shells on metallic plates, from which, when fixt by the engraver's tool, lithographic transfers may be rapidly multiplied to an almost indefinite extent. This process promises to be applicable to organic remains of every kind, and consequently of great utility in Palæontology. From a beautiful fossil starfish I sent by one day's mail to Captain Ibbetson, in London, I received, by the next mail,

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* They are sometimes also perforated by lithodomous molluscs, and otherwise beset with parasites, which indicate a period of tranquillity between the action of the forces by which they were shorn away or made smooth, and the deposition of the stratum that was subsequently formed over them.

a parcel of most exact impressions, taken from a photographic drawing, transferred to stone by the process above mentioned.

NOTICE OF DECEASED MEMBERS.

In Mr. RICHARD BRIGHT, of Ham Green, near Bristol, the Society has lost one of its first members. He was both a patron and cultivator of geology and mineralogy in a generation earlier than our own. Born in 1754, he died in 1840, at the age of 86.

Throughout the more busy years of his life he was an intelligent merchant, much engaged in promoting the commercial improvements of his native city. Honest, warm, and disinterested, he won early and maintained steadily, during a period of more than sixty years, the universal love and respect of his neighbours; and the best proof of this lay in that most enviable power he had acquired of conciliating and guiding men of all sects and opinions in the pursuit of objects of public utility, and the perfect confidence with which his friends resorted to his judgement and advice in the more delicate affairs of private life.

Upwards of sixty years ago Bristol possessed many zealous and intelligent individuals who understood the value of science and had cultivated it; and several had already made good progress in forming valuable geological collections; Catcott had bequeathed a large and interesting collection of minerals and organic remains to the Bristol Library. Bristol was then the cradle of English geology; Townsend, Richardson, and Smith resided in its immediate neighbourhood, and there Smith commenced his most important generalizations.

A love of chemistry acquired in youth under Priestley and Aikin, a personal intimacy with Whitehurst, and a commercial connexion with mines of Cornwall, made Mr. Bright an early collector, and William Smith and Richard Phillips lent him their willing assistance.

Though the metropolis was never his place of residence, he availed himself of frequent visits thither in earlier years to acquire an extensive and accurate knowledge of the pursuits of men of science. Before he had reached the age of manhood in 1774, we find him interested in the best construction of chemical furnaces, and studying Dr. Black's 'Tables of Double and Single Attractions.' In 1780 he was a member of a private Philosophical Society in London, composed of names* which are to this day almost all held in respect or reverence. It met once a fortnight, on Friday evenings, at the Chapter Coffee House, from seven till nine.

When Davy quitted Penzance for Clifton and assisted Dr. Beddoes in delivering chemical lectures, Mr. Bright's attachment to the science revived with double force. He attended these lectures with eagerness and delight; established a well-appointed laboratory

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* Dr. Hunter, Dr. Crawford, Dr. Price, Dr. Priestley, Dr. Kier, Dr. Cleghorne, Dr. Quin, Dr. Wells, Messrs. Nairne, Aubert, Whitehurst, Horsefall, Jones (afterwards Sir William), Howard, Bolton, Kirwan, Blackhall, Bright, Benjamin Vaughan.

in his own garden; and the tarnished dollars, which in 1800, on the announcement of Volta's discoveries, assisted in forming a galvanic pile, are still preserved.

About this time he was much interested in the discovery of large masses of sulphate of strontian in the fields adjoining his house at Ham Green. Specimens of these, beautifully crystallized, were found in nearly a horizontal stratum immediately under the soil: at the same spot, the magnesian conglomerate has since yielded specimens of meiomite.

Mr. Bright's influence and taste and knowledge of architecture were often employed in behalf of his native city. The library, the infirmary, the asylum for the blind, the college, the observatory, are among those establishments for which in succession he has laboured; and on none did he bestow more of his time and thought than on *The Bristol Institution*, both at the period of its formation in 1822, and for the eighteen years which intervened between that and his decease. Provincial establishments of this kind were at this time new experiments, and when political feelings were strong, he co-operated most efficiently with his friends Dr. Beake, then Dean of Bristol, Mr. Harford, Mr. Sanders, and The Rev. W. D. Conybeare, to induce men of all parties to meet together on that neutral ground,—the formation of a scientific society for a common object—to promote the study of the works of nature, and the advancement of literature, science, and art.

Amid all his various scientific interests, mineralogy, geology, and fossil osteology claimed the first place. Cuvier's researches were noted and abstracted in 1835 as earnestly as Adair Crawford's work on Heat was in 1779. He was as eager to possess and examine specimens of the fossil Infusoria of Ehrenberg at the age of 84, as he had been when scarcely twenty to hail a new discovery of his friend Dr. Priestley; and he felt as glad and excited in forming a personal acquaintance with the eminent geologists who came to the meeting of the British Association in 1836, as he had formerly been in his introduction to Franklin at Paris in 1777. At the age of 82, when bodily infirmity prevented him from taking any very active part in the proceedings of the British Association assembled at Bristol, he made his house and collections at Ham Green accessible to all its members. He was at that moment ardently following up the very latest discoveries in geology, and adding to his cabinet, with all the fervour and delight of youth, fresh accessions from the stores of organic remains, then newly discovered at the base of the Himalaya mountains.

He has published nothing. His name however may remain associated with the progress of science, by his liberal co-operation with Professor Whewell and the members of the British Association in the erection of a machine for registering the tides of the Avon upon a cliff overhanging that river, within the grounds of his residence at Ham Green, upon the very spot (though at the time it was unknown to be so) from whence Captain Sturmy made his observations which were transmitted to Sir Isaac Newton. During three years Mr. Bright undertook the special care and superintendence of this machine. The results of this register were peculiarly valuable in establishing the diurnal variation of the tides. When a machine of much superior construction was, after the lapse of three years, erected at the Hotwells, the original gauge at Ham Green became useless, and was removed. The new gauge is under Mr. Bunt's immediate care in *operation* as it was in *construction*, and his unremitting observations have already been rewarded by experimental proof of a very important general law, so recently announced, that it is possible I may be the first to give the intelligence to many of those who hear me, viz. that the variations of atmospheric pressure, as indicated by the barometer, exert a regular and very considerable influence on the height of high water in the Avon; an increase of atmospheric pressure, by which the mercury was raised one inch, producing a depression of fourteen inches in the height of the water.

In his death our Society has to lament the loss of, I believe, the only father, who, during many years, has, together with two sons, been among the number of its most zealous and efficient members. To one of these sons, Dr. Richard Bright, we owe an early paper in our Transactions, on the Geology of his father's neighbourhood. He has travelled in the less-frequented parts of Europe, and published records of his journeys both in Iceland* and Hungary; the medical profession also acknowledge their obligation to him for several important works.

Mr. John Gibson was a native of Yorkshire, engaged in large chemical works at Stratford-le-Bow in Essex, to whom we are indebted for our first knowledge of the existence of fossil remains of extinct animals in the cave at Kirkdale. Being on a visit to his friends near Helmsley in 1821, his attention was attracted by some bones he found thrown upon the road, together with stones from an oolite quarry adjacent to the church at Kirkdale. He at once perceived that they were not, as the quarry-men supposed, the bones of cattle that had perished by some murrain and been cast into a chasm of the rock, but that they were derived from animals no longer existing in the country. These bones were in quantity sufficient, not only to supply the cabinets of gentlemen in the neighbourhood, but also to enable Mr. Gibson to bring a collection of them to London, therewith furnishing an extensive cabinet of his own, and distributing liberally his duplicates to several public museums in London, including the British Museum, the Museums of the College of Surgeons and that of the Geological Society, of which Society he immediately became a member.

Mr. Gibson's attention being thus awakened to the consideration of organic remains, he soon discovered that, at Stratford-le-Bow, he was living in a land once inhabited by pachydermata that were contemporaneous with the ancient inhabitants of the cave of Kirk-

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* He accompanied Sir George M'Kenzie, and contributed to his work on Iceland.

dale; and soon added to his rich osteological collection from Yorkshire the remains of elephants, rhinoceros, hippopotami, oxen and deer, which abound near Stratford in the brick-earth pits, that are extensively excavated at Ilford. In his death we have to deplore the loss of an acute and zealous discoverer and promoter of Palæontology; and it has become the bounden duty of all the cultivators of this science, and more particularly of myself, to record our sense of the judicious sagacity and liberality of Mr. Gibson, but for whom the catacombs of Kirkdale might never have been heard of, and their records of our Yorkshire Hyænopolis might have perished without finding an interpreter.

In Mr. WILLIAM MACLURE we have lost an early and useful labourer in the field of geology, to whom we owe the first connected and systematic accounts of the structure of North America reduced to a comparison with that of Europe.

He was born at Ayr in 1763, and educated in that town. In 1782 he visited New York, and returning to London became a partner in an American mercantile house. He visited France several times between 1782 and 1796, when he went to Virginia and closed his business there as a merchant. In 1803 he returned to Britain, and was appointed a Commissioner for settling the claims of the United States against France. From Paris, as a centre, he afterwards made scientific tours over a large portion of Europe.

In 1807, returning from Europe, he commenced single-handed the Herculean task of exploring the geology of the United States; and after several years of labour, during which he crossed the Alleghany Mountains not less than fifty times, he produced a geological map of the whole country, which, though it gives only the Wernerian classes of rocks, forms a most valuable outline, and is a monument of great industry, perseverance and intelligence*.

His first observations on the geology of the United States, accompanied by the first geological map of that country, were read to the American Philosophical Society in Philadelphia, Jan. 20, 1809, and published in the sixth volume of their Transactions, part 1. In these Transactions also (vol. i. New Series) he published a second paper, read May 10, 1817, upon the same subject, with a geologically coloured map and sections, in which his views were improved and corrected by eight years' additional observations in the United States, and by a geological tour over a great portion of Europe.

This admirable paper was reprinted at Philadelphia in 1817, in a separate 8vo volume, entitled, "Observations on the Geology of the United States of America, with remarks on the effect produced on the nature and fertility of soils by the decomposition of the different classes of rocks."

On this important subject, of the connexion of geology with agriculture, Mr. Maclure has clearly shown that the fundamental basis of the agricultural resources of every country must rest on

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* See Hitchcock's 'Elementary Geology,' 1840, p. 283.

the condition which its soil derives from the rocks or strata that have supplied the materials of which it is composed; and wisely profiting by his suggestions, the different governments of the United States have caused geological surveys to be made of their respective districts; fully aware that not only the agricultural condition of every country must depend on the nature of its soil; but its future capabilities of becoming the site of extensive manufactures must also mainly depend on the presence or absence of subterraneous stores of fossil fuel.

Mr. Maclure's publications upon the geology of this most important part of the Western Hemisphere are marked with the finest appreciation of the just philosophical principles of geological research, and a spirit of combination and generalization of the largest and boldest character, yet never running wild. His map, which presents the synoptical result of the whole, is unrivalled by anything produced before that time. Adopting the Wernerian arrangement, he is far superior to Werner in the philosophical character of his mind; his colours represent primitive, transition, secondary, and what he calls alluvial, which are mostly tertiary, on the east of the Alleghany chain. Under this class he has included the lower cretaceous formations of New Jersey, which he remarks may probably prove to be secondary. The great simplicity of the structure of America, and more extensive continuity of its formations as compared with those of Europe, greatly facilitated his task; his map is therefore a very near general approximation to what would even now be given; his secondary rocks include what would now be called Silurian and Carboniferous, and he notices the absence of the chalk of Europe and of the Jura limestones. Of course he could not enter into the distributions of the Silurian and Carboniferous groups; but he observes, that a red sandstone seems the basis of the whole, and this he calls old red. The more exact local description of portions of the Carboniferous and Silurian groups, and the identification of the lower cretaceous deposits of greensand in New Jersey and skirting the Mississippi below the junction of the Ohio, are the principal materials of importance which have subsequently been added to his spirited and masterly original sketch. His introductory remarks show that he was equally well acquainted with the general outline of the geology of Europe.

He declines entering on the subject of organic remains, not as unaware of its importance, but because they "had not yet been examined." In his preface occur some remarks which may show how unjustly the earlier geologists have been charged with too great inclination to depart from the ordinary laws of nature: "In all speculations on the origin, or agents, which have produced the changes on this globe, we ought," he says, "to keep within the boundaries of the probable effects resulting from the regular operations of the great laws of nature, which our experience or observation has brought within the sphere of our knowledge." It is remarkable that Mr. Maclure mentions galvanism as an agent which may have co-operated in changing and metallizing rocks: "A galvanic pile," he says, "may be formed in the stratifications of a mountain, as well as in a chemist's laboratory."

His treatise ends with two chapters on the probable effects of the decomposition of different classes of rocks on the nature and fertility of soils; being an attempt to apply geology to agriculture. He is the father of American, much more than Smith is of English, geology; and American geology is especially important, because in America and in Russia we have two of the largest classes of formations, the Silurian and Carboniferous, developed at the distance of half an hemisphere. We may, with good cause, congratulate ourselves that this comparison will shortly be consummated by the distinguished author of the 'Silurian System,' whom we have this day elected to be our President for the ensuing year.

In 1822 Mr. Maclure published some speculative conjectures on the probable changes that may have taken place in the geology of the Continent of North America east of the Stony Mountains (Silliman's Journal, vol. vi. p. 98), in which he considers that a very extensive lacustrine condition of the upper country prevailed before these waters were discharged by the gorges that give exit to the present great rivers, and observes, that "the large masses of granite, some of them weighing tons, which are scattered over the secondary strata between Lake Erie and the Ohio, while there is not an atom of granite in place nearer than the north side of the Lake, would seem to point at the only mode by which they could probably be transported—viz. by supposing the Lake extended thus far, and that large pieces of floating ice from the north side might have carried those blocks with them, and dropped them as the ice melted in going south; the fact of few or no blocks being found south of the Ohio, shows that the southern sun melted the ice before it got so far." (Silliman's Journal, 1823, vol. vi. p. 102.)

It must be no less gratifying to the family of Mr. Maclure than it is to the great scientific family of the investigators of nature throughout the world, to learn that the Academy of Natural Sciences of Philadelphia has appointed a member of their body to deliver a discourse in commemoration of their venerable and respected President and benefactor; to whom, "as the pioneer of American geology, the whole country owes a debt of gratitude, and in his death will acknowledge the loss of one of the most efficient friends of science and the arts;" and who, "as the patron of men of science, even more than for his personal researches, deserves the lasting regard of mankind*."

Mr. William Maclure died, 23rd March 1840, at San Angel, near the city of Mexico, where, during some years, his declining health had obliged him to seek a more genial climate than the United States, and he has left a large property to the Academy of Natural Sciences at Philadelphia, of which he was President †.

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* Silliman's Journal, vol. xxxix. July 1840, p. 212.

† Besides the works above mentioned he published an "Essay on the Formation of Rocks," and a work in three volumes, entitled "Maclure's Opinions."

LXXVII. *Intelligence and Miscellaneous Articles.*

ON IODINE IN COMMERCIAL NITRIC ACID.

M. LEMBERT observes, that among the substances which render the nitric acid of commerce impure, there is one which has not been hitherto suspected, but which is not unimportant, namely, iodine. The following are the means by which its presence was demonstrated, for the first time, in purifying nitric acid: in order to obtain pure concentrated nitric acid, M. Lembert takes the nitric acid of commerce, adds nitrate of silver to it, and allows it to remain for some days; then pours off the clear acid, adds an equal weight of concentrated sulphuric acid, and distils the mixture. On one occasion in concentrating the sulphuric acid remaining in the retort, and when this was nearly completed, violet vapours appeared, and afterwards crystals of iodine were formed in the tube used as a refrigeratory.

Saturate nitric acid containing iodine with potash or soda, and to the solution add a little clear solution of starch, and a few drops of sulphuric acid, taking care not to add it till after it has been proved that the quantity already used was insufficient to effect the coloration. The presence of iodine will be thus determined by the blue or purple colour which the liquid will assume.

Reflecting on the origin of the iodine in the nitric acid, it was natural, says M. Lembert, to think that it was derived from the nitrate of soda used in preparing the acid; he consequently examined this salt by adding to a solution of it a small quantity of solution of starch, and then of sulphuric acid, and the colour indicative of the presence of iodine was produced.

M. Lembert remarks, that 1st, concentrated nitric acid, that is to say, of specific gravity about 1·4, contains iodine, while the weaker acid, or of specific gravity about 1·3, does not: 2ndly, neither chlorine nor sulphurous acid indicates the presence of iodine either in natural nitrate of soda or the neutralized acid.—*Journal de Pharm. et de Chimie*, Avril 1842.

ON THE PREPARATION OF HYDROBROMIC AND HYDRIODIC ACID.

BY M. MELLON.

The author obtains hydrobromic acid by introducing into a flask about 230 grains of bromide of potassium and a little water; he then adds about 380 grains of bromine and 30 grains of phosphorus in small pieces; he afterwards adapts a conducting tube to the neck of the flask; in a short time reaction commences, and it may become so strong as to render it necessary to immerse the flask in cold water. When the action diminishes, heat is to be gently applied by a spirit-lamp; the residue is a mixture of unaltered bromine and phosphate of potash; the gas liberated is derived from two different sources; it results from the formation of bromide of phosphorus, which water decomposes into hydrobromic and phosphoric acids; and secondly, from the action of the phosphoric acid thus formed on the bromide of potassium.

The reaction is represented by the following equation:

$$2\,\mathrm{Br\,K} + 5\,\mathrm{Br} + \mathrm{Ph} + 6\,\mathrm{H\,O} = \mathrm{P\,H\,O^5, K\,O} + 6\,\mathrm{Br\,H}.$$

When bromine is replaced by iodine, and the alkaline bromide by the iodide, an abundant and regular disengagement of hydriodic acid is obtained: it is requisite merely to raise the temperature slightly.—*Ibid.*

QUANTITATIVE DETERMINATION OF PHOSPHORIC ACID.

M. Schulze determines the quantity of phosphoric acid in soils, &c. by a process dependent on the following property: the phosphates of lime and of magnesia are soluble in acetic acid, whilst those of peroxide of iron and alumina are insoluble in it. The addition of a sufficient quantity of a solution of peracetate of iron or of acetate of alumina, completely precipitates the phosphoric acid in combination with peroxide of iron or with alumina, from the phosphate of lime and of magnesia dissolved by acetic acid.

The acid liquor procured by treating soils with hydrochloric acid usually contains a much larger quantity of alumina and peroxide of iron than corresponds to the quantity of phosphoric acid. Consequently, if after the oxidation of the iron the excess of acid is supersaturated with ammonia, and the peroxide of iron, alumina and compounds of phosphoric acid are thus precipitated, all this acid will remain in an insoluble state, combined with peroxide of iron and alumina, on the subsequent addition of excess of acetic acid and the application of heat; while the rest will dissolve.—*Ibid.*

NEW MINERALS.—ANDESINE*.

M. Abich has analysed a mineral from the Andes which was called Pseudo-albite on account of its having been found in twin crystals very similar to albite. Its cleavage, however, is less distinct than albite, and the cleavage planes are more undecided and less angular. This mineral is imbedded in a whitish gray mass called andesite, of sp. gr. 3·5924, mixed with hornblende and quartz; the crystals, when broken out, leave a shining impression. The specific gravity is 3·7328, therefore greater than that of albite. Separates before the blowpipe into thin fragments, and fuses into a porous drossy bead.

Analysed with carbonate of barytes it gave,

		Oxygen.	
Silica	59·60	30·90	8
Alumina	24·28	11·70	3
Oxide of iron	1·58		
Lime	5·77	3·79	1
Magnesia	1·08		
Soda	6·53		
Potash	1·08		

Formula $R\,S_2 + 3\,A\,S_2$.

It is therefore a leucite, in which the potash is replaced principally by soda and lime.

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* Berzelius, *Jahresbericht*, and *Journal für Praktische Chemie.*

ALBITE.

An analysis of albite made by Erdmann from the neighbourhood of Brevig in Norway, gave the following results:—

		Oxygen.	
Silica	69·11	35·89	12
Alumina	19·34	9·22	3
Oxide of iron	0·62		
Soda	10·98	2·93	1
Potash	0·65		
Oxide of manganese Magnesia	a trace		
	100·70		

Formula $\begin{matrix}\mathrm{N}\\\mathrm{K}\end{matrix}\,\mathrm{S}_3 + 3\,\mathrm{A}\,\mathrm{S}$.

LEUCOPHAN.

This mineral has been analysed by M. Erdmann of Sweden. It is found on a rock in the sea called Lamnion or Lammanskarett, at the mouth of the Langesundsfjord in Norway. It is imbedded in sienite on the west side of the cliff, accompanied by aegirine, albite, elæolite, grains of yttrotantalite, and another new mineral which M. Erdmann has named mosandrite. It is difficult to obtain it.

It is seldom found regularly crystallized; it has three distinct cleavages. L. J. Wallmark has examined the form of the crystals. A four-sided prism may be obtained, measuring 53° 24·7′ and 36° 26·3′. The colour varies from a pale dirty green to a wine-yellow; thin fragments are transparent and colourless. The cleavage planes are brilliant. Gives a white powder, and has great tenacity, and gives out a blue phosphorescence on hammering as well as by heat. On heating, is slightly electric. It is rather less hard than fluor spar, sp. gr. = 2·974. Melts before the blowpipe into a clear bead of a slight violet colour; with salt of phosphorus it is dissolved, leaving a residue of silica; with borax it is easily dissolved; with a small quantity of soda it gives an opake bead, not absorbed by charcoal. It gives out fluosilicic acid when heated in a tube with salt of phosphorus. It is composed of

		Oxygen.		Calculated.
Silica	47·82	24·84	7	49·20
Glucina	11·51	3·58	1	11·73
Lime	25·	7·25	2	25·10
Protoxide of manganese	1·01			1·04
Potash	0·26		2	0·16
Soda	7·59			7·10
Fluorine	6·17			5·67

This gives the formula

$$2\,\mathrm{Na}\,\underline{\mathrm{Fl}} + \underline{\dddot{\mathrm{G}}}\,\dddot{\mathrm{Si}} + 6\,\dot{\mathrm{Ca}}\,\dddot{\mathrm{Si}}, \text{or } 2\,\mathrm{N}\,\mathrm{Fl} + 3\,(\mathrm{G}\,\mathrm{S} + 2\,\mathrm{C}\,\mathrm{S}_3).$$

MOSANDRITE.

This mineral was found accompanying leucophan, and has been named Mosandrite by Erdmann after Mosander, the discoverer of lantanum. It is a silicate and titanite of oxide of cerium and lantanum. It is sparingly found on Lammanskarett, accompanied by albite, aegirine, titaniate of iron, and violet-coloured fluor spar. It is partly crystallized in flat obscure prisms, partly compact, with a disposition to a columnar separation. It has one distinct and several indistinct cleavages; the first has a glassy and waxy lustre, the latter a resinous lustre. The colour is dark reddish brown, in thin splinters a transparent bright red; gives a grayish brown powder, and is as hard as fluor spar; specific gravity 2·93 — 2·98. Before the blowpipe gives out much water, which has neither an acid nor alkaline reaction. On heating it becomes of a red yellow colour, easily fuses before the blowpipe with intumescence into a brownish green shining bead; is easily dissolved in borax, giving an amethystine-coloured bead, which in the reducing flame is yellow, quickly becoming colourless. It is more difficultly soluble in salt of phosphorus, leaving a residue of silica. In the reducing flame it imparts to the glass the colour of oxide of titanium. Produces with soda on platina a green colour. Mosandrite is entirely decomposed by hydrochloric acid, leaving the silica undissolved; when heated, chlorine is given off, and the colour of the solution changes from dark red to yellow. The quantities of the component parts are not yet determined; they consist of silica, titanic acid, oxide of cerium and lantanum, oxide of manganese, lime, a little magnesia, and potash and water. It consists principally of the four first ingredients.

SAPONITE.

L. Svanberg has described and analysed a mineral from Svardsjo in Dalarna, found in the mines of Bruskved and Svartvik. At the latter place it is found hanging from the rocks in pieces of an inch broad, white like butter or soap, which occasioned its name. It hardens upon exposure to the air, and forms partly white lumps, which may be marked by the nail, and partly falls to powder. It is either white, or partly yellow and partly red; upon pressure with a hard substance receives a polish, with a fatty feel, and adheres to the tongue. Before the blowpipe gives out much water, and blackens like other magnesian minerals; has the appearance of a commencement of fusion, is easily dissolved in borax or in salt of phosphorus, with a residue of silica; with soda it forms an opake glass. It is composed of

			Oxygen.	
Silica	50·8		26·44	5
Magnesia	26·5	10·26	10·47	2
Lime	0·7	0·21		
Alumina	9·4	4·39	5·02	1
Oxide of iron	2·	0·63		
Water	10·5		9·24	2

Formula $2\,M\,S_2 + A\,S + 2\,aq.$

Upon analyzing the soapstone of Cornwall, L. Svanberg found it composed of—

		Oxygen.	
Silica	46·8	24·32	13
Magnesia	33·3	13·10	7
Lime	0·7		
Oxide of iron	0·4	3·87	2
Oxide of manganese, a trace			
Alumina	8·0		
Water	11·0	9·86	5

$= 2\,M\,S_2 + A\,S + 2\,aq.$

PRASEOLITE.

M. Erdmann has described a mineral under the name of Praseolite, found near Brevig in Norway by Pastor Esmark. It is imbedded in granite and chlorite, and is accompanied by titanite of iron and tourmaline. It is irregularly crystallized in four-sided prisms, and sometimes with six, eight, and twelve sides; the angles are rounded as if fused. Colour green, light green, and dark green. Has only one cleavage, and a splintery, flat conchoidal fracture, with little lustre. Hardness between fluor spar and calcareous spar; gives a light green powder; specific gravity 2·754. Gives out before the blowpipe water, not acid; the thin edges fuse with great difficulty into a bluish green glass. Dissolves in borax and salt of phosphorus, giving an iron colour, leaving with the latter a residue of silica; fuses with soda with difficulty into a yellowish green glass. It is composed of

		Oxygen.	
Silica	40·94	21·268	3
Alumina	28·79	13·746	2
Protoxide of iron	6·96	6·969	1
Protoxide of manganese	0·32		
Magnesia	13·73		
Water	7·38	6·560	1
Oxides of lead, copper and cobalt	0·50		
Lime			
Titanic acid	0·40		

Formula $\left.\begin{matrix}Mg\\F\\Mn\end{matrix}\right\}S + 2\,A\,S + aq.$

ESMARKITE.

Under the name of Esmarkite M. Erdmann has described another new mineral found close to praseolite, also imbedded in granite. It is found in large irregular prismatic crystals, with the angles rounded and generally covered with mica. The crystals have a distinct cleavage at right angles to the principal axis, with a slight pearly lustre; the perpendicular fracture is uneven and of a waxy lustre. Hardness between calcareous spar and fluor spar; specific gravity 2·709. Gives off water before the blowpipe, and becomes of a bluish gray colour. Melts at the edges into a grayish glass. Salt of phosphorus and borax dissolve it with the colour indicating iron. Gives a yellow slag with soda. It is composed of

		Oxygen.	
Silica	45·97	23·880	5
Alumina	32·08	14·982	3
Magnesia	10·32	4·956	1
Protoxide of iron	3·83		
Protoxide of manganese	0·41		
Water	5·49	4·879	1
Lime, oxides of lead, copper, cobalt, and titanium	0·45		
	98·55		

Formula $\left.\begin{matrix}M\\F\end{matrix}\right\}S_2 + 3\,A\,S + aq.$

It might be therefore considered as dichroite containing water, or as Fahlunite with half the quantity of water.

APHRODITE.

Berlin has undertaken the examination of this Swedish mineral, which has been considered as meerschaum. That from Taberg in Wermeland and from Salm, has exactly the same composition as serpentine, and is nothing else than serpentine with the form of meerschaum. But the so-called meerschaum of Langbanshyttan, which is similar to the foregoing in other respects, has a different composition, and on account of its similarity to meerschaum is named aphrodite (from ἀφρὸς, Schaum, *froth, foam*). It is composed of

				Oxygen.	
Silica	51·55	51·58		26·79	8
Protoxide of manganese	1·62	1·49	0·34	13·65	4
Protoxide of iron	0·59	0·55	0·13		
Magnesia	33·72	34·07	13·18		
Alumina	0·20	0·13			
Water	12·32	11·34		10·07	3

which gives the formula of $4\,M\,S_2 + 3\,Aq$. We possess therefore three natural compounds of bisilicate of magnesia combined with different quantities of water.

Pikrosmine	$2\,M\,S_2 + aq.$
Pikrophylle	$3\,M\,S_2 + 2\,aq.$
Aphrodite	$4\,M\,S_2 + 3\,aq.$

CORAL REEFS.

Mr. Darwin's long-expected work on this interesting subject has just appeared, it is entitled, "The Structure and Distribution of Coral Reefs: being the first part of the Geology of the Voyage of the Beagle, under the command of Captain Fitzroy, R.N., during the years 1832 to 1836. London, 1842." (pp. xii. and 214.) It is illustrated by three plates, and some engravings in wood: the former of these, 1st, showing the resemblance in form between barrier coral reefs surrounding mountainous islands, and atolls or lagoon-islands; 2nd, map and section of the Great Chagos Bank, and maps of the Menchiooff Atoll, Mahlos Mahdoo Atoll, New Caledonia, and Maldiva Archipelago; 3rd, showing the distribution of the different kinds of coral reefs, together with the position of the active volcanos in the map.

MR. LONSDALE.

We regret to hear that the Geological Society of London will shortly be deprived of the services of their invaluable Curator and Librarian, Mr. Lonsdale. Intimately connected as we have been for many years with this Society, we can well appreciate the sorrow which this announcement has caused to all the members,—who thoroughly estimate the high qualities of this eminent person, and entertain the deepest gratitude for the devotion and unrivalled skill with which he has, during the last thirteen years, arranged their collections and conducted the publication of their volumes. Mr. Lonsdale's retirement, we are grieved to say, is occasioned solely by the state of his health: and to this notice of the loss which the Geological Society is very soon to suffer, we shall only add our hope, that a naturalist and man of letters may be found competent to succeed him as editor of the Geological Transactions*.

* Upon this subject we believe that application may be made to the President and Council of the Society at Somerset House.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1842.

Chiswick.—April 1. Heavy clouds: rain: slightly overcast. 2. Rain: clear and cold, with brisk N.E. wind. 3. Very clear: cloudy: slight hail shower. 4. Cold and dry: clear and frosty at night. 5. Clear and cold, with very dry air: sharp frost at night. 6. Slight haze. 7. Cold and dry: densely overcast. 8. Cold and dry: sunshine through slight haze: clear and frosty at night. 9, 10. Cold and dry. 11. Slight shower: clear and cold. 12. Cold and dry: cloudy. 13. Cold rain. 14. Showers, partly hail. 15. Bleak and cold. 16. Clear and cold, with parching N.E. wind. 17, 18. Overcast. 19. Dry haze: clear and frosty at night. 20, 21. Slight haze: very fine. 22. Foggy: dry haze: clear and fine. 23. Very fine. 24. Very fine: heavy thunder storm in afternoon, with partial showers of rain, and large hail in some parts near London. 25. Very fine. 26. Clear and dry. 27. Fine: air exceedingly dry: slight frost at night. 28. Hot and dry. 29. Slight haze: fine. 30. Fine.

Boston.—April 1. Cloudy: heavy rain early A.M.: rain P.M. 2. Stormy: rain early A.M. 3, 4. Cloudy. 5, 6. Fine. 7. Cloudy. 8—10. Fine. 11—13. Cloudy. 14. Rain: rain early A.M. 15. Cloudy. 16. Cloudy: rain P.M. 17, 18. Cloudy. 19, 20. Fine. 21—23 Cloudy. 24. Fine. 25. Fine: foggy early A.M. 26—28. Fine. 29. Fine: foggy early A.M. 30. Cloudy.

Sandwick Manse, Orkney.—April 1. Showery. 2. Snow showers. 3. Clear: aurora. 4. Clear: cloudy. 5. Cloudy: rain. 6. Clear and warm. 7. Fog. 8. Cloudy and warm. 9. Cloudy. 10, 11. Clear. 12. Clear: aurora. 13. Cloudy: clear. 14, 15. Clear. 16, 17. Cloudy. 18. Drops. 19. Cloudy. 20. Clear. 21. Fine. 22. Clear: fog. 23. Cloudy. 24. Clear. 25. Very clear. 26. Very clear: aurora. 27. Very clear and warm. 28. Very clear: fog. 29. Very clear. 30. Fog: cloudy.

Applegarth Manse, Dumfries-shire.—April 1. Showers. 2. Hail. 3. Frost: slight A.M. 4. Fair and clear: frost A.M. 5, 6. Slight frost A.M. 7. Fair, but cloudy. 8. Fair and fine. 9. Fair and fine: slight frost A.M. 10. Fair and fine: no frost. 11. Fair and fine: frost A.M. 12. Fair and fine, but withering. 13. Cloudy and droughty. 14. Droughty, but threatening rain. 15. Droughty: still fair. 16. Droughty. 17. Droughty: frost A.M. 18. Droughty. 19. Droughty: frost A.M. 20. Droughty and warm. 21. Droughty. 22—30. Droughty: very withering.

Sun shone out 30 days. Rain fell 1 day. Hail 1 day. Slight frost A.M. 9 days. Fair 28 days.

Wind north 1 day. North-north-east ½ day. North-east 5½ days. East-north-east 3 days. East 7 days. East-south-east 1 day. South 2 days. South-west 3 days. West-south-west 2 days. West 1½ day. North-west 2 days. North-north-west 1½ day.

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary*, Mr. Roberton; *by* Mr. Thompson, *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse*, DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.								Thermometer.											Wind.					Rain.					Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston 8½ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R. S. 9 a.m.
1842. April		Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mm.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.											
1.	29·340	29·457	29·303	28·90	29·11	29·37	29·22	29·57	45·0	57·6	43·4	51	32	40	49	34	41	39	s.	w.	w.	w.	w & n.	·172	·02	·87	......	·11	45
☾ 2.	29·606	29·780	29·576	29·21	29·69	29·81	29·84	29·82	41·8	48·6	38·0	47	31	42	45	35½	35	33	nw.	n.	n.	nnw.	n.	·088	·02	·14	......	·17	42
3.	29·836	30·012	29·819	29·50	29·88	30·09	30·06	30·26	38·4	46·5	34·2	47	35	37·5	49½	29½	40	40	nw.	ne.	n.	nnw.	n.	......	......	......	......	·14	36
4.	30·188	30·309	30·182	29·82	30·20	30·21	30·20	30·17	40·7	46·0	37·0	47	27	40·5	51	31	44	44	nw.	ne.	calm	nw.	sw.	......	......	......	......	......	36
5.	30·392	30·355	30·286	29·98	30·20	30·17	30·16	30·18	41·7	46·7	36·5	50	23	41	55	30½	47	45	n.	ne.	calm	nw.	sw.	......	......	......	......	......	35
6.	30·200	30·188	29·957	29·81	30·15	30·00	30·20	30·16	42·4	47·5	35·3	54	37	41·5	57	30½	48	45	ne.	ne.	calm	n& ne.	w.	......	......	......	......	·09	36
7.	29·862	29·907	29·800	29·48	29·94	29·92	30·12	30·15	43·7	52·0	39·6	58	35	46	55	38	45	42	n.	ne.	e.	ne.	se.	......	......	......	......	......	42
8.	30·094	30·190	30·053	29·70	30·08	30·63	30·20	30·31	44·8	56·9	39·4	58	27	46·5	57	35½	45	45	ne.	e.	e.	s.	sw.	......	......	......	0·13	......	43
9.	30·358	30·331	30·307	29·87	30·25	30·25	30·38	30·41	44·2	52·7	37·0	57	34	45·5	56	31	50	43	e.	e.	calm	sw.		......	......	......	......	......	36
● 10.	30·388	30·355	30·281	30·03	30·28	30·23	30·34	30·31	43·2	46·6	37·0	46	29	43·5	55	33½	45	42	e.	e.	calm	wsw.	se.	......	......	......	......	......	33
11.	30·248	30·216	30·137	29·89	30·20	30·21	30·34	30·38	43·7	46·3	36·7	49	34	43	52	31	48	41½	ne.	e.	e.	e.	se.	......	·01	......	......	......	37
12.	30·118	30·104	30·023	29·77	30·21	30·12	30·36	30·34	42·0	46·7	37·6	46	36	41	51	31	45	38	ne.	ne.	e.	ene.	se.	·050	......	......	......	......	33
13.	29·990	29·992	29·957	29·67	30·10	30·12	30·36	30·38	39·2	44·4	37·8	47	36	39·5	48½	34	45½	40½	n.	ne.	e.	ene.	ne.	......	·08	......	......	......	36
14.	29·932	30·025	29·940	29·60	30·12	30·11	30·34	30·39	41·2	43·6	37·8	52	39	41	49	36½	47½	44½	n.	ne.	ne.	ne.	n.	·063	·01	·29	......	......	38
15.	30·084	30·068	30·052	29·72	30·15	30·20	30·45	30·42	44·3	47·6	40·3	51	36	42·5	49½	39	46	41½	n.	ne.	calm	ne.	ne.	......	......	......	......	......	39
16.	30·144	30·147	30·113	29·83	30·22	30·21	30·34	30·33	44·6	49·0	39·5	51	31	46	55	38½	47½	46	ne.	ne.	e.	ene.	nw.	......	......	......	0·00	......	38
☽ 17.	30·206	30·182	30·164	29·79	30·20	30·10	30·27	30·21	44·4	48·7	37·8	51	41	45	59	31	46	45	n.	ne.	calm	s.	w.	......	......	·04	......	......	40
18.	30·210	30·201	30·091	29·75	30·13	30·10	30·14	30·11	44·2	47·4	42·0	47	40	43·5	59	33	48	46½	n.	ne.	calm	sw.	sw.	......	......	......	......	......	40
19.	30·232	30·199	30·154	29·76	30·09	30·10	30·11	30·05	42·3	47·8	41·3	63	26	49	54	33	48	47½	n.	ne.	calm	sw.	sw.	......	......	......	......	......	40
20.	30·182	30·150	30·093	29·69	30·09	30·09	30·11	30·16	49·4	51·8	38·7	64	33	53	68	33	50	45	n.	e.	calm	w.	sw.	......	......	......	......	......	43
21.	30·182	30·140	30·047	29·65	30·07	30·04	30·11	30·12	46·8	59·5	41·3	60	43	44	67	36	54	44	ene.	e.	calm	wsw.	sw.	......	......	......	......	......	44
22.	30·022	29·994	29·899	29·59	29·97	29·90	30·14	30·10	46·8	56·5	45·0	66	36	51	68½	37	46	44½	ne.	ne.	calm	e.		......	......	......	0·00	......	44
23.	29·942	29·901	29·878	29·43	29·90	29·86	30·04	29·98	54·3	61·3	47·0	75	37	53·5	69	41	47½	43	nnw.	nw.	calm	ese.	se.	......	......	......	......	......	50
○ 24.	29·974	29·948	29·921	29·47	29·88	29·91	29·99	30·01	60·5	68·6	52·0	75	37	52	68½	40	50½	46½	n.	se.	calm	e.	se.	......	·01	......	......	......	54
25.	30·080	30·028	29·946	29·54	30·00	30·06	30·14	30·18	57·8	69·3	49·0	73	42	56	68	42½	52	47	e.	e.	calm	ne.	se.	·097	......	......	......	......	54
26.	30·114	30·055	30·018	29·61	30·08	30·09	30·19	30·22	56·7	67·5	47·4	66	39	54	67	43	53	47	ne.	e.	e.	e.	se.	......	......	......	......	......	52
27.	30·048	29·990	29·959	29·55	30·08	30·00	30·22	30·20	54·3	62·3	41·5	63	31	53	63	41	53	43	ne.	e.	e.	e.	se.	......	......	......	......	......	46
28.	30·100	30·050	29·941	29·55	30·02	30·01	30·18	30·18	54·7	59·4	45·8	72	35	53	66½	36	52	45	n.	se.	e.	e.	e.	......	......	......	......	......	48
29.	30·088	30·061	29·881	29·55	30·00	29·93	30·16	30·10	55·3	66·0	45·7	71	43	54	68½	37½	48½	45	ne.	e.	e.	e.	se.	......	......	......	0·00	......	46
30.	29·882	29·835	29·800	29·36	29·92	29·94	30·10	30·13	57·7	64·6	47·6	74	41	48·5	66½	43	47½	45	ene.	e.	e.	ne.	se.	......	......	......	......	......	51
Mean.	30·068	30·072	29·986	29·63	30·373	30·039	30·160	30·178	46·9	53·8	41·1	57·70	31·86	46·2	54·9	35·7	47·18	43·46						Sum. 0·470	0·15	1·34	0·00	0·51	Mean. 42

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SUPPLEMENT TO VOL. XX. THIRD SERIES.

LXXVIII. *On a Re-arrangement of the Molecules of a Body after Solidification.* By ROBERT WARINGTON, *Esq.*[*]

HAVING occasion lately to prepare some alloys of lead for the purpose of lecture illustration, I was much surprised at an alteration taking place in the arrangement of the particles of one of these alloys, as shown by the appearance of the surfaces of fracture after the metal had assumed the solid form. The alloy experimented on was that known as Newton's fusible metal, composed of 8 parts of bismuth, 5 of lead, and 3 of tin. On pouring this alloy in the melted state on a marble slab, and breaking it as soon as solid, and when it may be readily handled, the exposed surfaces were found to exhibit a bright, smooth, or conchoidal metallic appearance, of a tin-white lustre; and the act of disjunction at one part will, frequently, cause the whole to fly into a number of fragments analogous to the breaking a piece of unannealed glass.

The metal after this becomes so hot as to burn the fingers if taken up, and when this evolution of heat has ceased the alloy will be found to have entirely altered its characters, having lost its extreme brittleness, requiring to be bent to and fro several times before it will break, and presenting on fracture a fine granular or crystalline surface of a dark colour and dull earthy aspect. Similar phænomena accompany the casting of the fusible alloy of V. Rose, composed of 2 parts of bismuth, 1 of lead, and 1 of tin.

The fact of the evolution of heat from the alloy of Newton, and its cause, are thus noticed by Berzelius in his *Traité de Chimie*. "If this alloy is plunged into cold water and quickly withdrawn and taken in the hand, it becomes sufficiently hot after a few moments to burn the fingers. The cause of this phænomenon is, that during the solidification and crystallization of the *internal parts* the latent heat of these is set free and

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[*] Communicated by the Chemical Society; having been read January 4.

communicates itself to the surface before the fixing and cooling." The alteration in the internal arrangement of the particles, as proved by the surfaces of fracture, is not however noticed, and the explanation is defective, as it supposes the interior not to have assumed the solid state until the evolution of the heat occurs. If such were the case it would be seen on breaking it in the first instance. The phænomena can only be accounted for by admitting a certain degree of mobility among the particles, and that a second molecular arrangement takes place after the metal has solidified; this may arise from their not having assumed in the first state that direction in which their cohesion was the strongest.

That a very marked and extraordinary alteration in the characters and properties of various substances arises entirely from this change in the position of their component particles, effected either by the communication or abstraction of heat after solidification, there can be no doubt. And these changes are applied to many very important purposes in the arts and manufactures; such as the hardening and tempering of steel, the rolling of commercial zinc, and rendering that metal permanently malleable, the annealing of glass, and a variety of other uses, particularly in crystallization, which might be adduced.

The following experiments were made to ascertain to what extent the emission of latent heat takes place. The melted alloy was poured in a perfectly fluid state on the bulb of a thermometer placed in a small platinum crucible, having a capacity equal to about 70 grain measures of water, and standing in a vessel of cold water or mercury. The thermometer surrounded by the solidified metal and crucible was removed from the cooling medium before it had reached its stationary point, and the greatest decrease of temperature noted. The heat then rose rapidly again, and the maximum effect was registered. The fusing point of the alloy was 202° Fahr. the following results were obtained:—

Exper.		Fahr.		Fahr.	Diff. Fahr.
1.	thermometer fell to	97°	and then rose to	157°	60°
2.	... ...	94	... ...	149	55
3.	... ...	90	... ...	150	60
4.	... ...	87	... ...	147	60
5.	... ...	104	... ...	156	52
6.	... ...	97	... ...	148	51
7.	... ...	92	... ...	152	60
8.	... ...	104	... ...	155	51

So that in four out of the eight trials a difference of 60° Fahr. was rendered apparent.

In a platinum crucible of larger size the effects were not so marked, 34° Fahr. being the greatest difference obtained; this of course would arise from the greater bulk of the melted metal not exposing comparatively so large a surface to the cooling medium.

LXXIX. *On the Constitution of the Sulphates as illustrated by late Thermometrical Researches.* By THOMAS GRAHAM, *Esq., F.R.S., &c.*[*]

PROF. Hess and Dr. Andrews both apply the results of their late inquiries respecting the heat evolved in combination[†] to test the accuracy of a view of the constitution of double and acid salts which was published by myself, and arrive, it is remarkable, at opposite conclusions.

The view in question, I may first state, taking the example of double and acid sulphates. Crystallized sulphate of magnesia, and the double sulphate of magnesia and potash, I have represented thus:—

$$\mathrm{Mg\,O,\ S\,O_3\,(H\,O) + 6\,H\,O,}$$
$$\mathrm{Mg\,O,\ S\,O_3\,(K\,O,\ S\,O_3) + 6\,H\,O;}$$

considering the latter salt to be derived from the former, by the substitution of sulphate of potash for that single atom of water, which is found to be much more strongly attached to the sulphate of magnesia than the other six. This atom of water, which is not basic water, was formerly named *saline* water, to indicate that it is replaceable by a salt; its presence being considered a provision in sulphate of magnesia for the formation of double salts. The water and sulphate of potash are therefore looked upon as equivalent in the construction of the two salts; and the substitution of the salt for the water might therefore be reasonably expected to occur without the evolution of heat.

In accordance with that statement, Dr. Andrews finds that no heat is evolved on mixing solutions of sulphates of magnesia and potash, nor in the formation of any other double salt. On repeating the experiment I found also no heat nor change of temperature on mixing the solutions, although a change of $\frac{1}{20}$th of a degree Fahr. would have been distinctly indicated by my thermometer.

Possibly, however, the double salt may not immediately be formed, and hence no change of temperature at the moment of mixing the two solutions, nor for some time afterwards. To

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[*] Communicated by the Chemical Society; having been read January 18.

[†] Phil. Mag., January 1842.

meet this objection, solutions of sulphate of magnesia and of sulphate of ammonia (the last, from its greater solubility, being preferred to sulphate of potash) were made of such a strength that they might be mixed without the precipitation of the double salt immediately occurring, but strong enough to allow a large quantity of the double salt to fall upon stirring the liquid strongly. The solutions were 1546·88 grains of cr. sulphate of magnesia dissolved in so much water as to form 8000 water grain measures, and 613·5 grains oil of vitriol, neutralized with ammonia and made up to 4000 water grain measures. On mixing one ounce measure of the first with half an ounce measure of the second, both exactly at 50°, not the smallest change of temperature could be observed; but as soon as the double salt began to deposit, the temperature rose, and on stirring strongly much salt was deposited, and the temperature rose 5·40° Fahr. On re-dissolving this salt, however, by substituting for the mother liquor an equal bulk of water, the temperature instantly fell 5·85°. Hence the heat which first appeared was produced by the solidification of the double salt, and disappears upon its liquefaction. There is no heat referable to combination of the two salts. The cold on dissolving was always somewhat greater than the heat on precipitating the double salt, in repetitions of this experiment, chiefly, I believe, from the slowness of the precipitation, which requires a minute or two, so that a portion of the heat is lost from contact with the atmosphere, and the whole not observed, while the subsequent solution of the salt being almost instantaneous, the whole fall of temperature is observed. The same experiment was made with a solution of sulphate of zinc, of the same strength as the sulphate of magnesia, and with similar results, only that the fall of temperature, on solution, was somewhat less than that on solidification, namely, as 9°·22 to 9°·67, difference 0°·45 Fahr. This was principally owing to the time required in re-dissolving this double salt being greater than that occupied in precipitating it, three applications of water being required to re-dissolve the double salt completely, owing to its sparing solubility.

M. Hess's objection is made to the analogous constitution which I have assigned to the bisulphate of potash:—
Sulphuric acid of specific gravity 1·78, $HO, SO_3 (HO)$.
Bisulphate of potash . . . . $HO, SO_3 (KO, SO_3)$.
He maintains that heat is evolved in the formation of a bisulphate, and therefore that the combination is not effected by the equivalent substitution supposed. He mixed sulphate of potash with $HO, SO_3 + HO$, and found heat evolved, but allows that the result here is fallacious, a portion only of the

sulphuric acid being converted into bisulphate, while the other portion is diluted by the displaced water of the first portion, and thus heat evolved.

On performing the direct experiment, which M. Hess appears to have neglected, using a saturated solution of sulphate of ammonia, and sulphuric acid of specific gravity 1·256, I obtained, on mixing, 5·4° of cold instead of any heat. But on diluting the sulphate of ammonia with a volume of water equal to that of the dilute acid, a fall of 1·12° occurred. Deducting this from the former, there remains a fall of 3·88° due to the combination of the two salts, sulphate of water with sulphate of ammonia. But this may be explained. The bisulphate of ammonia formed is an anhydrous salt, unlike the double sulphate of magnesia and ammonia, which carries along with it all the water of crystallization of the sulphate of magnesia. But the sulphate of water itself, as it exists in diluted sulphuric acid, is a largely hydrated salt, like sulphate of magnesia. The water of the former, on being set free in the last experiment, absorbs heat, because heat was evolved originally in the combining of this water with the sulphuric acid.

Although certain small corrections on these experiments for changes in capacity for heat of the liquids have been neglected, yet they are sufficient to demonstrate that no heat is evolved in the formation of double sulphates, and also, as appears by the last experiment, that these compounds are formed at once on mixing the solutions of their constituent salts, whether precipitation occurs or not. Sulphate of potash and water are therefore equivalent in the constitution of such salts, or *equi-calorous*, if a term may be coined to express this relation.

LXXX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

Address delivered at the Anniversary Meeting on the 18*th of February* 1842; *and the announcement of the Award of the Wollaston Medal and Donation Fund for the same Year. By* RODERICK IMPEY MURCHISON, *F.R.S., President of the Society.*

AWARD OF THE WOLLASTON MEDAL AND DONATION FUND.—OBSERVATIONS BY THE PRESIDENT.

THE Wollaston Medal has been this year awarded to M. Leopold von Buch, for "the eminent services he has rendered to Geology by his extraordinary and unremitting exertions during a long series of years, and for his recent researches in Palæontology."

Since geology has been a science no individual has more successfully applied a powerful mind to its cultivation, or more liberally expended his private means in advancing its progress than Leopold von Buch. The chief works by which his fame was reared are well known; but with the numberless memoirs printed and published at his own charge and gratuitously distributed, I regret to say, English geologists are by no means sufficiently acquainted; and justice cannot be rendered to him until the whole of his researches are brought before the public in a combined form. In the mean time we offer our Medal to this distinguished man, to show that we seek to reward him not only for his acknowledged great works, but also for those efforts to advance science, which are too little known. Such, for instance, is the large geological map of Germany, including the Alps and adjacent regions, published without allusion to his name, and commonly known as the map of Martin Schropp and Co.; a most remarkable production, whether we consider the date of its publication or the expenditure of mind, labour, and money which it must have cost the author. And although the result of these labours has since been improved upon by the efforts of several of his countrymen, among whom the names of Hoffmann and Von Dechen stand prominently forward, it is well to know that no one has more untiringly contributed new information to his younger friends than Von Buch. When a traveller at Berlin, upwards of two years ago, and lost in admiration at the progress which physical geography and geological maps were making in that metropolis, I was much surprised to learn, that M. von Buch had in his possession an unpublished geological map of Bohemia, all, be it observed, worked out by his own patient observations on foot. Aware, from a former rapid survey of that country, that our knowledge of Bohemia was still very imperfect, I obtained from the author a coloured copy, which I first exhibited to the British Association at Glasgow (1840), and which I now present to the Geological Society.

Again, after successfully developing, in the spirit of a true philosopher, the recondite phænomena of the metamorphism of rocks by the most laborious pedestrian efforts, have we not seen, that as years rolled on and our veteran leader began to feel, that the toil of gaining the mountain crest must soon pass from his own limbs to those of younger men, he has so vigorously applied his mind to Palæontology as to throw new lights over this department of our many-headed science? No sooner did he grapple with this task, and that too when he had passed the meridian of life, than he displayed the same originality of mind which had marked all his previous inquiries. Subjecting the family of *Ammonites* to revision, and convinced that their innumerable species were not founded on true natural distinctions, he took the lines of suture as a basis, and thereon established a limited number of normal or typical forms, each characteristic of certain strata. The *Terebratulæ*, so common in all the secondary strata, were next passed in review, and types were fixed upon, to which a number of slightly varying forms were referred, a work which our French brethren have considered so important, that they have republished it in the Transactions of the Geological Society of France. Then followed his illustration of the fossils of South America, collected by his great countryman Humboldt. Whilst I merely enumerate these works, I may be allowed to say a few words respecting his last published volume, "On the Fossils of Russia," because, together with my associates, M. de Verneuil and Count Keyserling, I have had the means of forming an opinion of its value. Simply furnished with collections of organic remains from various parts of the Russian empire, M. de Buch, without ever visiting the country, assigned to each form he examined its position in the geological series. As the researches of my friends and myself have confirmed, to a very remarkable extent, the accuracy of the geological views of M. von Buch, drawn from such sources only, you will surely agree with me, that this work affords a most remarkable proof of the acumen of its author and of the efficacy of organic remains in identifying distant strata.

But, Gentlemen, I have already said more than enough to explain the grounds of the award of the Medal to one of the leading characters of the age, and who has exercised a most powerful influence on the present state of our science. The substantial claims of Leopold von Buch are those of a profound and original thinker, and of a most enterprising field geologist, who, casting new and broad lights upon the history of the earth, has gloriously toiled throughout life in our cause, and who, though loaded with the highest academic honours, is continually putting forward fresh claims upon the admiration and gratitude of his associates.

In delivering the Wollaston Medal to the Foreign Secretary, the President then said,—

Mr. De la Beche,

I consign to you the Wollaston Medal awarded to M. von Buch, requesting you to convey it to that eminent man. It was the intention of His Excellency the Prussian Minister, Chevalier Bunsen, to have been present on this occasion; and whilst we must all regret that ill health alone has prevented so distinguished a man of letters from honouring us with his presence, I rejoice that no geologist is better qualified than yourself to be the medium of communication with our great associate, for no one among us is more intimately acquainted with those researches of M. de Buch on which his chief fame rests. Express to him, I beg, our heartiest wishes for the continuance of his good health, and that he may long live to call forth from us proofs of the deep sense we entertain of the value of his labours.

Mr. De la Beche, on receiving the Medal, expressed the great gratification it afforded him, that his office of Foreign Secretary rendered him the channel through which the Wollaston Medal was to be transmitted to the distinguished geologist to whom it has this year been awarded. The works of Von Buch, he observed, are too well known and appreciated to require notice; they have most materially assisted in the great advance which geology has made, and recent publications have proved that his love of science was as ardent as ever, and that the importance of the labours in which he was engaged was undiminished.

In assigning the donation of the Wollaston Fund to Mr. Morris, the President thus expressed himself:—

MR. MORRIS,

The Council of the Geological Society have awarded to you the proceeds of the Wollaston Fund during the past year, to assist you in preparing for publication a table of British Organic Remains, in which you have been for some time engaged, and which, from the specimens laid before us, we believe will be of very great service in promoting the accurate study of Geology. The value of the table of the late Mr. Woodward has been acknowledged; but his premature death having prevented him from enlarging its sphere as our science advanced, a new and much more comprehensive work has been urgently demanded. I am happy that the task of meeting our wants has been undertaken by one well qualified, like yourself, by diligent research and a competent acquaintance with Natural History; whilst in thus consulting your own wishes, the Council of the Geological Society is persuaded that they are acting in the very spirit of Wollaston's bequest, not treasuring up money parsimoniously, but expending it liberally upon the very fitting occasion which your ability and research have called forth.

To which Mr. Morris replied,—

SIR,—I cannot sufficiently express my grateful thanks to the Society for the unexpected compliment that has this day been conferred upon me, and more especially for the disinterested feeling which has actuated the Council in awarding to one almost unknown to them, this honourable testimonial of their approbation, and not the less flattering to me, Sir, your kind and courteous manner in communicating the same; and I trust that my efforts for the promotion of geological science, which have already entailed upon me so many obligations to various members of the Society, may still excite their willing co-operation towards perfecting a catalogue of British Fossil Remains, from which the geologist may reason with confidence, and which the naturalist may consult with advantage.

ANNIVERSARY ADDRESS OF THE PRESIDENT.

GENTLEMEN,

Although acquainted with my intended absence from this country during many months of the past year, you nevertheless honoured me with the station which I occupy, kindly intimating that the active pursuits of geology should not be a bar to the enjoyment of the chief distinction which you can confer. In thanking you sincerely for that proof of your good opinion, permit me to say, that if the presiding over a body of gentlemen so well knit together for a common purpose, were all that you expected from me, light as well as agreeable would be the task. A charge, however, of a more serious nature is the composition of an anniversary discourse, in which I am expected to treat of the progress of geology during the past year. So very expanded is the present condition of our science, that he who attempts to give a clear synopsis of all that has been done in different parts of the globe, even in one year, and to indicate the desiderata to be accomplished, must make himself master of numerous foreign works. An active observer cannot well execute such a task. On the other hand, if your President should simply review the last year's proceedings of our own Society, he will but poorly serve you, for our abstracts make you well acquainted with the prominent facts and opinions of the authors. I will, therefore, adopt a middle course, and without attempting a complete sketch of the progress of geology, or tiring you with a dry analysis of our performances, permit me to select for your consideration what I consider to be the chief subjects of present geological interest, whether foreign or British, and so class them that their bearing upon the advance of our science may at once be seized. If in so doing I should fail to illustrate points which some of you may consider to be better suited to this address than those which I bring before you, I trust you will recollect how brief is the season during which I have been able to detach myself from my own line of inquiry, and how imperfectly, therefore, I have been able to study the works of my contemporaries.

OBITUARY.

Before we enter upon the consideration of the progress of geology, let us pay our homage to the memory of those deceased Fellows who laboured to promote our science.

On this occasion we have to mourn over one whose genius has won for himself an imperishable name. By the purest feeling of the beauties of nature, by the manly simplicity of his character, and by his sterling good sense, CHANTREY was led to his peculiar excellences as an artist. Admiring him for his unrivalled excellence in art, we geologists loved him also for the endearing qualities of the man.

Sir Francis Chantrey was a member of our Council, a frequent attendant both at our social meetings and in the rooms of the Society, and on all occasions was happy to serve us, though invariably on one condition, that he was never put prominently forward. If then his presence has often debarred us from expressing in his hearing those sentiments of esteem with which he inspired us, we have this day, alas, the opportunity of giving full utterance to our sorrow. Even as working geologists his memory has claims upon us in more than one department of our own science. Lest his biographers should not glean the facts, I must now state that we have benefited by his sound advice concerning the application of colours to our geological maps, and on the best means for preserving organic remains, which presented difficulties from their size, their condition, or the nature of the rock in which they were imbedded; and upon several occasions he has assisted us by superintending the moulding of osteological specimens which have been brought to this country, and of which it was important to obtain casts. Indeed at all times was his assistance freely given where it could be useful, and his chisel even has been employed in dissecting from their matrix the bones of fossil reptiles.

Snatched from us in the zenith of his bright career, the strong bias of his mind shines forth in his splendid bequest to the Royal Academy. Persuaded (from whatever cause arising) that art is not appropriately encouraged in our country, he has decreed that British genius shall no longer droop for want of enlightened assistance. His munificent endowment of native art is Chantrey's proudest monument, and must, indeed, produce effects far beyond the portals of our national gallery. But whatever may be the ultimate effect of this patriotic bequest, we must gratefully admire the spirit which dictated it, and ever feel a just pride in having had so good a man for our warm friend, so great a sculptor for a co-operating associate.

Mr. Bowman, whom we have very recently lost, was a naturalist who, as far as his other avocations permitted, did much good service in practical geology. His chief attainments lay in botany, and he is the author of several publications upon that science. Residing formerly at Wrexham, he acquired a very intimate knowledge of the carboniferous tracts to the south and west of that town, and he communicated to myself a good deal of valuable, original matter connected with them and the adjacent older rocks, shortly before the 'Silurian System' appeared. He afterwards favoured this Society with some very excellent details concerning a group of Upper Silurian rocks in Denbighshire, and their junction with old red sandstone and mountain limestone, pointing out some essential mineral variations in these rocks upon the northern frontier of Wales, as compared with the typical strata of the same age in Shropshire and the centre of the Silurian region. After he removed his residence to Manchester, where he died, he pursued science with renewed zeal, and was one of the most active promoters and officers of the Geological Society of that town. To be convinced indeed of his ardour and research, you have only to refer to the first volume of the Transactions of the Manchester Society, and you will find that four out of eleven memoirs are from the pen of our late associate. I shall also have occasion in the sequel to advert to a short memoir upon the glacial question which is amongst his most recent productions [*]. His loss in Manchester must indeed be seriously felt, and from my own knowledge I can state that his absence is not only to be regretted in these rooms, but also that his presence will be much missed in the approaching assembly of the British Association for the Advancement of Science, for he had been a frequent attendant at former meetings, and had never failed both to communicate papers and to serve in any office in which he could be useful. In estimating his character, I should say that Mr. Bowman took a high place in that class of authors who silently but steadily advance science by short and clear monographs on subjects with which they are familiar. As the class is not large, so can we ill afford to spare the assistance of one who, like Mr. Bowman, really distinguished himself in this modest but highly useful walk.

By the death of Mr. Thomas Edington of Glasgow, we lose one of the old and valued members of the Society, and whose name is

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[*] Philosophical Magazine, November 1841.

honourably associated with that of the early school of Scottish mineralogists. Every geologist who has had occasion to visit the West of Scotland, found in his house a hearty welcome, and in his beautiful museum much instruction respecting the vast variety of simple minerals in which that region abounds. At the meeting of the British Association for the Advancement of Science, held at Glasgow, he filled the office of one of the local secretaries, on which occasion he was untiring in his exertions, and unbounded in his hospitality, whilst he was of signal use in cementing the bonds of kind feeling between his countrymen and the men of science who came among them as visitors. Having been informed that Mr. Edington's minerals must be disposed of, I beg to express my hope that a collection so choice, and so highly esteemed by mineralogists, may find some enlightened purchaser worthy of its contents.

Among our other deceased Fellows, I have still to mention three whose names are connected with our pursuits, Mr. SNOW of Highgate, Dr. YELLOLY, and Mr. McENERY. The first of these gentlemen was not only a frequent attendant at our meetings, but an assiduous collector of fossils and a donor to our museum, particularly after the excavation of the Highgate tunnel, during which operation he became possessed of a fine series of shells of the London clay.

Dr. Yelloly was a firm supporter of this Society at a period when it was struggling for existence under the auspices of our first President, Mr. Greenough, and real and efficient friends were put to the test. Dr. Yelloly was among the foremost of these as an active member of the Medico-Chirurgical Society, which body afforded the rising geologists their first place of meeting in Lincoln's Inn Fields, where our founders set up their standard of independence, and claimed to have an existence as uncontrolled by the Royal Society, as the medical men who aided them sought at the hands of the College of Physicians. The advantages which science has reaped from this independence of action and division of labour is now, indeed, admitted even by those who were opponents and have lived to see our success. In late years, as in early life, Dr. Yelloly was forward and at his post when any liberal measure was proposed connected with the progress of science; he took an active part in the formation of the British Association for the Advancement of Science, and when that body met at Birmingham he performed the duties of President of its medical section.

The Rev. J. McENERY, a Roman Catholic clergyman, and a zealous fossil osteologist, was first brought into geological notice by his labours in the bone-caves of Devonshire, near Torquay, where he resided. His prolonged researches in these caves produced an immense collection of fossil bones of the same species of quadrupeds as those which occurred in the celebrated Kirkdale cavern. The most striking inference from this collection, was a perfect demonstration of the agency of hyænas in collecting the herbivorous animals into caverns during long periods, proved by the absence of rolled bones and the abundance of fractured osseous fragments bearing the marks of having been gnawed by teeth; in short, confirming in a very remarkable manner the inhabited cave theory propounded by Dr. Buckland. Mr. McEnery's collection of the bones of British cavern quadrupeds, which is one of high merit, will, I understand, be soon disposed of to the public; and I trust that part of it at least will find a resting-place in our great national collection at the British Museum.

Palæozoic Geology.

SILURIAN—DEVONIAN—CARBONIFEROUS.

It was long after a true principle of classification, founded on the succession of organic life, had been applied to the tertiary and secondary rocks, that the same method was used to work out the order of the oldest strata in which the remains of animals have been discovered. My own efforts, directed for several years to this end, have been so distinctly recognized by those whom I now address, as establishing a step by which the relative age of the older fossiliferous strata has been subsequently developed, that I ought to apologise for offering, on this occasion, even the shortest historical sketch of the process by which we have arrived at our present palæozoic classification. Some statement seems, however, to be called for, now that the subject is passing into many hands and into various countries. Having satisfied myself, after a labour of eight years, that I had amassed all the materials requisite to establish the existence of a sequence of rocks distinct from the Old Red Sandstone and Carboniferous Limestone, and having applied local names to each of the ancient formations so situated, I was strongly urged by many scientific friends, both at home and abroad, to propose some general name for the whole group. I fixed upon the ancient geographical term "Silurian," which was approved of, and has since been adopted, not merely in my own country, but in the most distant parts of Europe and in America*. No sooner, however, had it been proposed, than another seemed requisite to characterize the older slaty rocks, on which the newly-named Silurian system reposed. For these an eminent continental geologist suggested, in a letter to myself, the classical word "Hercynian," derived from the Hartz mountains, where the rocks might be presumed, from their antique aspect and mineral character, to be of remoter age than the soft argillaceous Silurian types of Britain. Alive, however, to the danger of mingling assumptions, drawn from lithological structure, with proofs derived from unequivocal succession of organic remains, and knowing that in our own country there was, in fact, a vast mass of slaty rocks on which the Silurian strata reposed, and which my friend Professor Sedgwick had long studied, I urged him in describing these rocks which he had made his own, to fix on a general British geographical name. He then

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* When Ostorius, the Roman general, conquered Caractacus, he boasted that he had blotted out the very name of Silures from the face of the earth. A British geologist had, therefore, some pride in restoring to currency the word Silurian, as connected with great glory in the annals of his country.

adopted the name of Cambrian. Nothing precise was known at that time of the organic contents of this lower or Cambrian system, except that *some* of the fossils contained in its upper members at certain prominent localities were published, Lower Silurian species. Meanwhile, by adopting the word "Cambrian," my friend and myself were certain, that whatever might prove to be its zoological distinctions, this great system of slaty rocks, being evidently inferior to those zones which had been worked out as Silurian types, no ambiguity could hereafter arise. On the other hand, the adoption of any term derived from a part of the continent, where we had not made ourselves masters of the true sequence, might involve the whole subject in confusion. This would in reality have occurred had the word "Hercynian" been selected, for subsequent researches have taught us, that the greater portion of oldest rocks of the Hartz are younger than the Silurian system, and that their antique impress is due to metamorphic action*. In regard, however, to a descending *zoological* order, it still remained to be proved, whether there was any type of fossils in the mass of the Cambrian rocks different from that of the Lower Silurian series. If the appeal to nature should be answered in the negative, then it was clear, that the Lower Silurian type must be considered the true base of what I had named the *Protozoic* rocks†; but if characteristic new forms were discovered, then would the Cambrian rocks, whose place was so well established in the descending series, have also their own fauna, and the palæozoic base would necessarily be removed to a lower geological position.

In a very comprehensive memoir, recently read, which, when published, will throw a clear light over the ancient rocks of the lake districts, as compared with their equivalents in Ireland, Wales, and Scotland, Professor Sedgwick has answered this appeal himself‡. Reexamining all the ancient fossiliferous rocks in Cumberland, he has become convinced that they are there divisible into two great zones, referable to Upper and Lower Silurian types, the former surmounted by old red sandstone and carboniferous limestone, and the latter reposing on some of the oldest sedimentary rocks of our islands, the Skiddaw slates, in which no organic remains have been detected. Numerous fossils from the Berwyn mountains, Snowdonia, and other Cambrian tracts, which he collected many years ago (but which, owing to the want of space at Cambridge, have been only lately unpacked), have been recently subjected to the same interrogatory, and have given the reply, that vast as the thickness of strata may be, the same forms of *Orthis* which typify the Lower Silurian rocks, not only range through what had been termed the Upper Cambrian (Bala, Berwyns, &c.), but also throughout the whole of North Wales.

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* See Geological Transactions, vol. vi. p. 288.

† Shortly afterwards Professor Sedgwick proposed the word "Palæozoic," as a general name for all the older groups, which, preferring to my own, I immediately adopted as involving no theory.

‡ Proceedings, No. 82, p. 541. [An abstract of Prof. Sedgwick's paper will appear in an early Number of Phil. Mag.]

In the mean time other observers had been working out detailed facts which pointed to the same conclusions. In a part of Cumberland, Mr. James Marshall had established the presence of Silurian deposits, where it was formerly supposed still older rocks prevailed, and more recently Mr. MacLauchlan of the Ordnance Survey, has shown us that all the slaty, and in parts metamorphic tracts of North Pembroke, which are coloured in my Silurian map as Cambrian, or in other words, as strata beneath the Llandeilo flags, contain many of the same forms as the Lower Silurian rocks. Before these inquiries had taken place at home, the researches of Professor Sedgwick and myself in Germany and Belgium, and of M. de Verneuil and myself in Russia, had led to the same conclusions, viz. that wherever it exists, the zone of fossiliferous strata characterized by the Lower Silurian *Orthidæ*, are the oldest beds in which organic life has been detected, and that many of the subjacent rocks, sometimes even when in the form of gneiss, mica schist, talc schist, chlorite slate, &c. are nothing but metamorphic rocks, in less altered parts of which the same typical fossils are observable.

If then our researches teach us that the term Cambrian must cease to be used in *zoological* classification, it being in that sense synonymous with "Lower Silurian," we see the true value of having established a type like the latter, which being linked on through intermediary groups to overlying formations, the age of which was previously well known, we have arrived *gradatim*, and without hypothesis, at the apparently true base of the zoological series in Europe. It is right, therefore, that I should announce that the conventional line which was set up in the map of the Silurian region, between the Lower Silurian and the Cambrian rocks, and which has been adopted by Mr. Greenough, has no longer any reference to strata identified by distinguishing organic remains, for the same fossils are found in strata on each side of that demarcation. Such lines of division, however, when viewed as the signs of local phænomena, are notwithstanding highly useful, both as indicating changes of lithological character, great lines of disruption and lower divisions of the same palæozoic group. In short, all researches up to this day have led to the belief, that the Lower Silurian fossils were the earliest created forms, and that this "protozoic" type prevailed during that vast succession of time which was occupied in the accumulation of all the older slaty rocks, until the Upper Silurian period, when new creatures were called into existence, and when the earlier forms diminished and were succeeded by a profusion of chambered shells which so abundantly characterize that epoch.

This, Gentlemen, is I trust a good step gained. To establish upon sound data the true theory of organic succession in the oldest forms of life, is surely important, and we ought to rejoice that the British islands have afforded us the means systematically to work out the question. Ascending then from these lowest types, the Upper Silurian zone is one of great distinctness in England, and in the Baltic—in the northern provinces of Russia and in North America; the Wenlock, Dudley and Ludlow fossils having been abundantly found in both hemispheres. As soon, however, as we have advanced through this zone, a new era is announced by the presence of the earliest Vertebrata. The minute and curious fishes in the uppermost bed of the Ludlow rock, are the earliest precursors of many singular ichthyolites which succeed in that enormous formation, termed from its mineral character in Scotland and parts of England, the Old Red Sandstone. But in this as in nearly every other deposit, lithological charaeters are fugitive, and the red, green and yellow sands of the North, are found even in our islands, as in Devonshire and the adjacent tracts, to be replaced by black schists and limestones. But here again zoology enables us to interpret the language of nature, for it was merely by seeing the letters of the alphabet spread out before him in a cabinet, and without even having visited the country, that Mr. Lonsdale was led to conceive that a large portion of this tract, though very dissimilar in mineral aspect, would prove to be of the same age as the Old Red Sandstone. I need not tell you how the researches of Professor Sedgwick and myself, which first indicated the presence of some members of the carboniferous system of that tract, afterwards confirmed these views, nor need I remind you that we have since extended them to various parts of Germany and Belgium, for the abstracts are already in your Proceedings and the memoir is about to appear in your Transactions.

I must here, however, acquaint you, that the paper by ourselves upon the Rhenish provinces is admirably illustrated by a description of the Devonian fossils of that region, prepared at our request by M. E. de Verneuil and M. d'Archiac, in which many new genera and species are established, and the group is delineated with closeness of research and profound knowledge of natural history. In the same communication these authors offer a general table of Palæozoic fossils, which in sustaining in the strongest manner the true intermediate character of the Devonian system, as suggested by Mr. Lonsdale, seems to be one of the most valuable documents yet presented to our consideration, in leading us to view the palæozoic rocks as a great tripartite series composed of the Carboniferous, Devonian, and Silurian systems.

Further, I would specially draw your attention to the enlarged views of our French coadjutors, derived from extensive study, in which they estimate the relative increase and decrease of various genera and species of fossils in the three divisions of the earlier periods, and show that whilst a few species (twenty only in upwards of 2750 distinct species or well-marked varieties) range throughout the tripartite series, yet that each system has a distinctly typical fauna, whether we derive our conclusions from researches in our own parts of Europe, or from an examination of American and Russian forms*.

Whilst speaking, however, of this table, I must at the same time

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* M. de Verneuil has, with my full consent, enriched this general table of comparison by the addition of the names of all the new species and characteristic palæozoic types collected in our two visits to Russia, and the description of which we are now preparing.—March 1842.

do justice to one of our own countrymen, Mr. Austen, the value of whose researches in Devonshire you have had previous opportunities of estimating. I have recently seen a MS. table prepared some time since by this able geologist, but the use of which he has liberally granted to Mr. Morris, who is preparing that general synoptical view of British organic remains, the publication of which you have resolved to encourage*.

In remote countries, the palæozoic classification of Silurian, Devonian, and Carboniferous types has been extended, by my companions and myself, from Russia in Europe into Asia, and, may I add, that an inspection of some fossils of the far-distant Altaï leads me to conclude that the examination of that chain will afford the same results? Though our own naturalists have not yet penetrated to Pekin, the Russian Major of Engineers, Kovanko, has acquainted us with the existence of an extensive coal-field not far from that metropolis†; and if time and the wear and tear of life permit, I despair not of planting the Silurian standard on the wall of China, by approaching it through the country of our old allies.

Again, Southern Africa and the South Seas have afforded their quota of Silurian fossils, but above all other foreign countries, North America appears to be rich in rocks of the same age. Of this fact indeed the Geological Society received the clearest evidence in the excellent section of Mr. James Hall, and the fine suite of organic remains which accompanied it‡. We have thus the most convincing proofs that the primæval æras were distinguished by a wide if not universal spread of the same genera and species of animals.

We have yet to analyze the enormous tracts of Australia over which British influence extends, before we can be said to have gathered together *all* the palæozoic data which are essential to a sound general classification. The travels of Cunningham, Mitchell, Grey, and others of our countrymen, permit us however to conclude that the ancient strata of these regions may eventually be worked into a classification approaching to our own. In that singular country, in which so large a portion of the existing terrestrial and marine fauna differ so essentially from those of every other region, it is curious to detect in the rocks many fossil Corallines and Mollusks§ closely analogous to the Silurian species of the British Isles, thus adding another

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* See previous account of the Award of the Wollaston Fund, p. 544.

† Journal des Mines de Russie, 1838, p. 191.

‡ Geological Proceedings, vol. iii. p. 416. [Phil. Mag., Third Series, vol. xix. p. 530.] I was very much struck with the clear, unpretending, and workmanlike manner in which Mr. J. Hall had the kindness to communicate his views to myself, respecting the Silurian and other palæozoic rocks of the United States, in the letter which was read to the Society; and I am glad to find that this able geologist has been of great service to Mr. Lyell in his present tour in America. Mr. J. Hall has since forwarded to me his memoir, entitled 'Notes on the Geology of the Western States.' —Silliman's Journal, vol. xlii. p. 51.

§ See Mitchell's Expedition into the interior of Eastern Australia, vol. i. chap. 1; vol. ii. chap. 15.

proof to many we already possess, that the same climatological and physical conditions were very widely spread during the earlier ages of the earth. Slender as our information is as yet respecting the natural history of that wide and detached continent which British industry is reclaiming, we cannot but anticipate a rapid accession to our knowledge, now that some highly-gifted naturalists are established in it. Whilst I simply allude to Mr. W. MacLeay* and Captain Philip King, whose researches are directed to branches of science connected with our own, it is my duty to mention more specially the Rev. W. B. Clarke, a member of this Society, who has previously contributed to our Proceedings and Transactions, and who in his recent voyage to Australia has afforded us fresh evidence that his leisure hours will still be employed in geological pursuits. A short residence at the Cape of Good Hope enabled him to communicate to us a memoir on the structure of that colony, which seems to confirm what we had previously learnt from Herschel and Smith concerning its northern limits, and leads us to conclude that rocks of the Silurian age constitute the chief sedimentary masses of the southern promontories, though often much altered by the intrusion of igneous rocks.

Having alluded to Australia, I cannot refrain from expressing my delight, that Captain Grey, whose sketches of his arduous journeys in the wildest portions of that land are already placed among our standard works of travels, and whose future researches are certain to enrich our knowledge, should happily have been selected to rear the nascent establishment of Adelaide, at the same time that a most valued member of our own body, Sir John Franklin, is rendering Van Diemen's Land a school of natural knowledge. Under the more euphonous name of *Tasmania* (derived from its real discoverer Tasman), the intrepid polar voyager, though now unaided by the great zoologist, the companion of his former toils, assembling together a few men of science and letters, has founded the "Tasmanian Philosophical Society," to the first Number of whose published labours, printed at Hobart Town, I beg to refer you as containing an introduction and several memoirs which would do credit to any Society in this metropolis. The geological articles contained in it refer only to the structure of Kerguelen's Land, and fossil wood from Macquarrie Plains; but as some very remarkable fossils of very ancient forms have already been procured from the vicinity of Hobart Town, I trust that the energy of the governor and his known devotion to our pursuits, will induce him to procure from some one of the intelligent scientific staff which surrounds him,

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* Although Trilobites so characteristic of the protozoic æra have not yet been detected, my friend Mr. MacLeay acquaints me that he has recently recognized the first fossil crustacean found in Australia, a macrourous decapode, which being discovered by Mr. Emery of the Beagle, has been brought home by that officer, and through Dr. Fitton has found its way into our museum. "This crustacean (writes Mr. MacLeay) is nearly allied to Thalassina, and is interesting as a specimen from being the first fossil crustacean, and I believe the only one yet found in New Holland."

a detailed account of the position and relations of these organic remains, the possession of a good suite of which is still a desideratum in the Geological Society of London*.

In estimating the progress of inquiry in this department of geology in our own country, the recent work of Professor Phillips upon the Palæozoic fossils of Devonshire and the adjacent tracts claims our special attention, not only on account of the talent which he has shown in describing many new forms, but also on account of the classification which he suggests. We are already signally indebted to this author for inquiries in various departments of geology, and especially for his volume upon the organic remains of the Carboniferous Limestone. Without the previous existence of that work, there might have been some difficulty in asserting that the Silurian is, as a whole, independent of the Carboniferous system. The recent inquiry is a part of his duty in a public office in which he is fortunately employed, and the suggestion of which does infinite honour to Mr. de la Beche, and credit to the government who sanctioned the appointment. From Devonshire the Ordnance geological forces, directed by these able leaders, have moved into the Silurian region. Doubtless, under such discerning eyes, and with such a number of assisting hands as are now turned into this formerly deserted tract, many new forms may be expected to appear. If, however, the Silurian catalogues should be much augmented and enriched by the labours now so vigorously directed to that point by government authority, I trust that geologists will pardon the omissions and defects of the person who first toiled to unravel the phænomena of that region, assisted only by a very few of its kind inhabitants†. Such personal considerations are, however, of little moment, and I pass from them to that which is of real importance, the establishment of the best palæozoic classification.

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* The geological notices in the Tasmanian Journal of Natural Science, consist of a description of some silicified wood from Macquarrie Plains by Dr. J. D. Hooker, and a sketch of the mineral structure of the northern part of Kerguelen's Land by Dr. M^cCormick, both attached to Captain James Ross's expedition. The latter acquaints us that this tract is exclusively composed of trappæan (basaltic) and metamorphic ? rocks, with the exception of certain truncated and dismembered beds of coal which are traceable at intervals, pretty much I presume, like the broken and isolated portions of coal which are found in the trap rocks of the northern end of the Isle of Skye.

† In preparing my work I derived much assistance from a valuable original MS. on the Structure of Shropshire by Mr. A. Aikin, the earliest modern geologist, who, with his associate Mr. T. Webster, worked in this field; whilst my chief co-operating friends were the Rev. T. T. Lewis of Aymestry, Dr. Lloyd of Ludlow, and Mr. Davies of Llandoverey. It is, however, to Mr. Lewis that I am more indebted than to any other person, for he had acquired a very accurate knowledge of the order of the strata of his neighbourhood before I visited it. He was, indeed, my companion in the field in visiting several important localities, and as I can truly say "hæc meminisse juvat," I sincerely thank a friendly critic in the Edinburgh Review, April 1841, for having dwelt upon these facts in the history of the "Silurian System."

Now the first question is, have any such new lights been thrown upon the subject of the older rocks by the recent work of Mr. Phillips upon Devonshire, as to change the nomenclature previously adopted both at home and abroad, and to substitute for it that proposed by Mr. Phillips, namely, Upper, Middle, and Lower Palæozoic strata? I confess that as I read this volume I perceived none, except that after describing the species, the author shows that the fossiliferous strata of the Eifel are the equivalents of those of South Devon, a point, however, which had been previously established by Professor Sedgwick and myself.

Adopting from ourselves the word "Palæozoic," Mr. Phillips extends however its original meaning, and applies it to all the strata containing organic remains, from the oldest formation to the Magnesian limestone inclusive. His Lower Palæozoic rocks are admitted to be exactly synchronous with those which were worked out as types under the name of Silurian, and yet he entirely omits that term in his parallel table of equivalents, in which he styles them "Transition and Primary Strata;" whilst for the *ordinary* names to parallel with his "Middle Palæozoics," the much newer terms of Eifel and South Devon are made use of—terms of comparison, it will be recollected, which were introduced by Professor Sedgwick and myself *long after* the establishment of the Silurian type. I ask those geologists who supported me by their approbation throughout my labours, if the name first proposed by him who worked out and defined a system of classification, is to be suppressed when not only no evidence is brought to disprove its value, but when succeeding observers in various parts of Europe and America have sanctioned it. But as this is now simply a subject of nomenclature, and my facts are not disputed, let us see whether for all the practical purposes of our science, the term Silurian, as first proposed, ought to be preferred, in use, to the term "Lower Palæozoic," which is to supplant it. The word was chosen because it was liable to no misconceptions, and never could lead to false theoretical deductions. It is, as before stated, simply a geographical name, derived from a region containing newly defined types of succession. When subsequently we used "Palæozoic" as a comprehensive term for all the older rocks, Professor Sedgwick and myself intended to apply it generally to that great series which embraces the Carboniferous, Devonian or Old Red Silurian and Cambrian groups.

In extending the palæozoic range so as to include the magnesian limestone, Mr. Phillips does so because that formation contains some species of Producti very analogous to carboniferous forms. But he knows well that rocks of the same age in Germany and in our own country, contain the remains of several species of Saurians, and the recent exploration of Russia (1841) further establishes the important fact, that deposits in the very same place in the series as the magnesian limestone, and loaded with Producti, are *also* charged with Saurians. What, then, are the *zoological* bases which ought to define the boundary lines between large groups of strata? Are they the vertebrata or invertebrata? If such a great feature of change in animal life as the earliest appearance of Saurians is to be taken as the limit of one vast geological division, we must exclude the magnesian limetone from the older series, and Mr. Phillips's proposed extension of the term Palæozoic cannot be sustained. Adopting this principle of the vertebrata as our guide, we may go on to say, that the true Silurian type ceases in the ascending order at that band of rocks which, in truth, forms the very uppermost layer or summit of the Silurian strata, in which the lowest order of vertebrata or fishes first appear, and then having ascended through another vast series, loaded with peculiar ichthyolites, we may announce a new æra in the magnesian limestone or zechstein, where we meet in the Saurians with another and higher class of the animal kingdom, wholly unknown in the inferior beds. It was, I beg to say, on this principle that I formerly proposed to divide the strata of England into seven great systems, as expressed in the small map of England which accompanies the map of the Silurian region. I do not assert that this general classification of the British geological series should be preferred to that of Mr. Phillips. He may contend that the universally distributed mollusk affords a more useful horizon line than any class of the higher order of animals. I merely state the case, and I hope fairly, to show that whether geographical terms be ultimately adhered to or rejected, all nomenclature founded *solely* upon our present knowledge of the distribution of animal and vegetable life, must be liable to change with every new important discovery, whilst that terminology which involves no such hypothesis, but is simply based on the proofs, that within a given region certain groups of beings are included, can never be gainsaid. It is on these grounds, therefore, that I am encouraged to hope, that the word "Silurian," which has been warmly sanctioned by the classic authority of Von Buch, which E. de Beaumont and Dufrénoy have engraved upon their splendid map of France, and which our fellow-labourers in America have adopted, will not be obliterated to make way for other names which are not founded upon any *new* distinctions, stratigraphical or zoological. So long, gentlemen, as British geologists are appealed to as the men whose works in the field have established a classification, founded on the sequence of the strata and the imbedded contents, so long may we be sure that their insular names, humble though they may be, will, like those of our distinguished leader William Smith, be honoured with a preference by foreign geologists, who, looking from a neutral ground, are sure to be the most impartial judges. The perpetuity of a name affixed to any group of rocks through his original research, is the highest distinction to which any working geologist can aspire. It is in truth his monument, and therefore, gentlemen, I trust you will pardon me if I have occupied you too long with the allusions to this point, and which have been elicited by the work of one for whom I entertain so high an esteem as Mr. Phillips. I will therefore only add my hope, that now when the term Silurian has been so widely spread, the Director of the British Geological Ordnance Survey, who encouraged the author of the 'System' to propose a separate name for the types he had worked out, will not permit the labours of his friend to be submerged, and thus seem to convey to foreign geologists the idea, which is indeed far removed from the truth, that there are any real differences between his views and my own on this important subject. In terminating these considerations, I beg geologists to recollect, that I never entertained the idea that the local types around Ludlow and Wenlock would be found applicable in detail to strata of the same age in distant places; on the contrary, having shown that even within a very limited radius such subdivisions varied with varying conditions, it was a leading and constant object of my work, to demonstrate that the broad divisions of Upper and Lower Silurian alone, could be maintained as terms of distant and foreign comparison.

There are two short communications by Mr. Lyell on the older rocks to be noticed. The first is on the strata between Aymestry and Wenlock, in which he dwells on the assistance to be derived respecting the amount of dislocation in strata, by attentively noticing the deviation from a vertical position of the inclosed corals; but he adds that great caution should be used to distinguish between those specimens which may have been torn from their position with reference to the horizon while growing and inverted, and those which have lost their original mode of growth by subsequent dislocation of the strata. From the known habits of recent corals, Mr. Lyell also infers that the Silurian strata must have undergone successive depressions during their accumulation, as beds of Polyparia belonging to the Wenlock limestone are overlaid by many hundred feet of sedimentary matter. In the second communication Mr. Lyell offers some remarks on a series of fossils from the neighbourhood of Christiania, and he infers from the evidence they afford, that the limestone to which they belong is of the age oft he Lower Silurian rocks; and on similar grounds he places the limestone of the island of Langoen, one of the highest beds of the country, in an intermediate position between the upper and lower Silurian rocks, constituting a passage from the one to the other.

The last memoir which has been read before us on the British Palæozoic rocks, relates to their development in a part of Westmoreland, and is from the pen of Mr. D. Sharpe; and I rejoice to see so clear and systematic a workman enlisted in the survey of the older rocks. Agreeing on some essential points with Professor Sedgwick and Mr. James Marshall, particularly in reference to the superior and inferior limits of the Upper Silurian group, this author, who had previously made himself acquainted with the best types of the Silurian rocks, conveys to us additional details of this interesting tract, in which he has distinguished upon a map the Upper Ludlow rocks, as characterized by many fossils, from an inferior slaty formation which lies between them and calcareous bands charged with Lower Silurian fossils. Dividing this intermediate formation into three sub-groups (by only mineral characters however), he gives to the whole the local name of "Windermere Rocks,"—a term which I understand he only uses until by the discovery of fossil evidences he may be able to refer these beds to their proper Silurian equivalent. If I were allowed to judge from the experience of one visit to a part of the country described by Mr. Sharpe, in which I found Orthoceratites in mountains marked by him as "Windermere Rocks," and also from his own showing, that these rocks are included between types of the higher portions of the Upper and Lower Silurian strata, such intermediate formation must be on the parallel of the Wenlock strata, which in many parts of the Silurian region, as well as in the North of England—(*i. e.* wherever the subdividing limestone and fossils are suppressed) can only be recognized under the general term of lower members of the "Upper Silurian Rocks." As Mr. Sharpe proposes to revisit the country, and to extend his researches from Westmoreland into Lancashire and Furness, he will have ample opportunity of confirming or rejecting my surmise. In regard to that portion of the memoir which points out the existence of many faults and anticlinal lines, I am not prepared to say to what extent they accord with the previous observations of the great geologist of the lake country, Professor Sedgwick, or of his precursor, Jonathan Otley.

I would now speak of a work which has recently appeared, entitled 'The Old Red Sandstone, or New Walks in an Old Field.' From a pretty accurate acquaintance with the tracts from which Mr. Miller has taken his title, I can assure you that the walks of this author had been little trodden, and that his claims to originality are very just. It is impossible to peruse his pages without delight in tracing how the strong mind of Mr. Miller has enabled him to rise step by step from the stone quarry of his, and I may add my own, native county Ross-shire, to a place in literature and science which few reach, even with all the support derived from an expensive education; or without admiring the ability with which this unassisted observer first succeeded in putting together the dislocated fragments of the very singular fish, called Pterichthys by Agassiz, long before that creature was first understood. Look again to the clear and general view which this author takes of the greatest of Scottish deposits, and how well he conveys to unpractised readers a true idea of its position, importance, and divisions, and you will agree with me that in Mr. Hugh Miller we have to hail the accession to geological writers of a man highly qualified to advance the science. Few persons, and too often least of all those who are, if I may so speak, professed geologists, succeed in imparting to others, who have not studied the science, a clear conception of their views. In this respect the character of Mr. Miller's work is admirable, for it portrays the means by which the author acquired his knowledge, and, from its persuasive manner, is worth, to a beginner, a thousand didactic treatises.

I hoped before now to have seen in print the very valuable memoir prepared by Dr. Malcolmson long ago, on the divisions and development of the Old Red Sandstone in the North of Scotland. This delay has been caused solely by the desire that the description of the various fishes which he has pointed out as characterizing the different stages of the deposit should be given by Professor Agassiz. The numerous avocations in glacial and other geological inquiries, as well as, I regret to say, his partial ill health, might alone have led us to account for the postponement of this labour by M. Agassiz; but in a recent letter to myself he has also given the following important reason:—"When I promised you to occupy myself with the determination of the fossil fishes of Dr. Malcolmson, I believed that it would be as easy a task to me as the determination of other ichthyolites, and I had no doubt that your Devonian system must reveal quite a new world in the class of fishes so very different from existing species. The effort has thrown upon me the obligation of prodigious labour, to arrive at some precise results as to these curious objects; and without giving you something very imperfect, which I look upon as yet to be unworthy of publication, I must have recourse to your indulgence for the delay."

Regretting sincerely that injustice seems to be done to Dr. Malcolmson by this delay, I have, I confess, a pleasure in knowing that Professor Agassiz will well investigate all these curious animals before he pronounces his final opinion. I can even assure him, that strangely formed as these Scottish types may be, he has yet to hear of some still more marvellous fishes which the Devonian or Old Red system contains in assuming its Russian dress. In that empire, where in some mountain tracts the system is black, slaty, and crystalline, there are also vast undulations and plains in which it is composed of slightly coherent, red, green, and yellow sands, shales, and limestones. In some of these beds, near Dörpat, Professor Asmus has detected gigantic fishes, which he is now describing; and Mr. Pander, so distinguished by his palæontological works on the environs of St. Petersburgh, is preparing an account of others, some of which are specifically identical with those of Scotland. I cannot venture to anticipate what these naturalists will shortly lay before the public, but I may be excused from announcing, that the moment I exhibited to Professor Asmus some drawings of the Scottish old red sandstone fishes, his eye at once fell upon the Pterichthys as probably the type in miniature of an enormous creature, five times the dimensions of our largest specimens, which is found in rocks on which the University of Dörpat is situated. Anxious that we should no longer be without some representatives of these Palæo-ichthyolites, whose bones are so gigantic that they were formerly supposed to belong to mighty Saurians, I requested Dr. Asmus to prepare casts of them, which he has obligingly executed, and of these I now present a set to the Society, as one of the fruits of distant comparison resulting from my Russian travels, and as a memento of the instructive researches of Professor Asmus. With the mere announcement, however, of these mighty fishes I must now take leave of the animals of primæval days, by saying that the carboniferous fossils of Russia are most singularly in accordance with those types which have been so ably elaborated by Mr. Phillips, Mr. Sowerby, and other geologists in our own country, a point to which I hope to call your attention at the next Anniversary.

SECONDARY ROCKS.

Pursuing the inquiry in the ascending order, the long period which is specially marked by the presence of gigantic reptiles is now before us. It commences with the magnesian limestone (the Zechstein and associated rocks), and terminates with the cretaceous system. In this wide field Professor Owen has taken the lead as a palæontologist, and will shortly lay before the world the results of his researches into the extinct Saurians of our island. Of this work I cannot speak, but from the knowledge we possess of Professor Owen's consummate acquaintance with comparative anatomy, and of his wonderful ability in detecting the minutest character in masses of bones obscured by matrix and mutilated by accident, we may anticipate that this work will enjoy the proud distinction of becoming a text-book with every natural philosopher in every part of the world. The points of this great inquiry to which he has called our attention during the last year, are the teeth and skeletons of five species of his newly-formed genus Labyrinthodon, found in the new red sandstone of Warwick; the whole of which, after a most elaborate comparison with all collateral and congenerous forms of different families of reptiles, he proves to belong to Batrachians, but with striking and peculiar affinities to the higher Sauria. From the evidence afforded by the comparative dimensions of one species of Labyrinthodon found in the same quarry, Professor Owen likewise shows that the anterior and posterior extremities must have been of disproportionate magnitude, according well with those of the Cheirotherium, and he therefore infers, and with great apparent justness, that the Labyrinthodon and the Cheirotherium were one genus. In a second memoir upon certain remains from the Oolitic Series, he has established a genus of Saurians equal in size to the whale, and in a third upon the remains of a crocodilian Saurian from the "Lower Greensand;" he concludes, from the same unerring evidence in the form and texture of the bones and teeth, that they are quite distinct from any Saurian hitherto described; and he therefore refers them to his new marine genus Polyptychodon. Whilst I delight in seeing that the tenants of those ancient oceans have met with so competent an expositor, I cannot but regret that my place should not at this moment be occupied by our own Conybeare; for the founder of the genus Plesiosaurus would have taught you to admire a multitude of comparisons and osteological adjustments contained in the results of Mr. Owen's researches. Though unequal to enter into a discussion of his merits, I can, however, in common with all my brother geologists, express to him my deep sense of gratitude for the successful efforts he has made to point out to us new links in the scale of nature's works.

On the subject of Saurian remains our knowledge has also been increased during the past year by Dr. Mantell, in a memoir communicated to the Royal Society, on the lower jaw of the Iguanodon, and on the remains of the Hylæosaurus and other Saurians discovered in the strata of Tilgate Forest. Not pretending to have adequate acquaintance with the subject treated of by Dr. Mantell, I am glad that our old and valued associate is once more before the public with one of those original researches with which, during the last twenty-five years, he has so much enriched our science, and which have obtained for his name so high a place in the volumes of the great Cuvier, as to render any eulogium on my part superfluous. Valuing as I do the arduous labours of a man, who, like Dr. Mantell, has occupied the few leisure hours at his disposal, first in discovering, next in dissecting from their stony bed, and lastly in describing the specimens; I am bound to observe that such merits deserve, as they have obtained, the highest praise which working geologists, like ourselves, can offer. In thus estimating, however, the value of Dr. Mantell's researches, I must be permitted to say (and in the most friendly spirit), that whilst I understand the propriety of the motive which led him to communicate his last memoir on the Iguanodon to the same Society to which he had addressed his first account of that Saurian, I regret that he should not have communicated to ourselves other palæontological memoirs, the consideration of which, I must say, pertains particularly to the Society over which I preside. So long as the Royal Society produces volumes adorned by the writings of the first mathematicians, physiologists, and chemists of the age, so long will it maintain its high place, little heeding our humbler pursuits.

Two memoirs have been read before us to illustrate the celebrated "bone-bed," which, lying at the base of the lias, and in contact with the uppermost members of the new red system, has hitherto been classed with the former deposit. The first of these, communicated by Sir Philip Egerton, is entitled "On the occurrence of Triassic Fishes in British Strata;" the second is "On the occurrence of the Bristol Bone-bed in the Lower Lias, near Tewkesbury," by Mr. H. Strickland. The fact to which Sir Philip Egerton adverts is, that out of a series of specimens from this bed at Axmouth and Aust, M. Agassiz determined four species to be well-known forms of the Muschelkalk, whilst fifteen were unknown in that deposit or any other part of the triassic group; and Sir Philip concludes, that the beds in question ought to be removed from the lias, not only because the fishes are specifically distinct from those of that formation, but because the forms of the ganoidians possess the heterocerque tail, a form which the classification of Agassiz confines to deposits of higher antiquity. This reason ought to have great weight, and might, if unconnected with others, at once dispose us to move our base line of the lias some few feet higher.

A fresh-cut section of the Gloucester railway had exposed at Combe Hill, near Cheltenham, the same singular bone-bed which is so well known at Axmouth and at Aust. From an intimate knowledge of that country, I can recognize the fidelity with which Mr. Strickland identifies certain thin layers of sandstone and grit at the bottom of the lias extending to the north with the adjacent bone-bed, which in its further extension loses those ichthyolite characters for which it is so remarkable over an area in our isles as wide, indeed, as that of the famous "Küpfer Schiefer" in Germany. Now in Gloucestershire the bone-bed described by Mr. Strickland contains not only fishes, many of which are of new species, but also many shells, some of which are supposed to be of forms intermediate between those known in the lias and the keuper. In this case, therefore, we are probably in the same position as the inquirer into the Palæozoic rocks, who stands upon the beds of passage from the Silurian into the Old Red or Devonian rocks before adverted to. In both cases, when he finds forms which belong to the inferior and superior systems, whether he may draw his boundary line above or below these equivocal strata, seems at first to be of small importance; for, as with the progress of research, we must expect to find an infinite number of strata which contain fossils indicating a transition from lower to higher formations, so must the lines of separation which geologists set up between formations be liable to undergo small alterations. Adhering, however, to the belief, that in the sequel those limits will most prevail which are most made to depend on great changes in animal economy, I think that the conclusion of Sir Philip Egerton, as based on the existence of the fishes with heterocerque tails, must lead us to place the "bone-bed" as the uppermost limit of our New Red System, or in other words, as the last-formed stratum in which such ichthyolites appear.

A point connected with an important previous deduction has been determined by Mr. Strickland in a cutting of the Gloucester railroad. The period at which the Lickey trap rocks were erupted, is now proved by actual sections to be that which from collateral circumstances had been surmised by myself. By observing that the New Red Sandstone of the Upper Lickey lies unconformably upon a mass of Red Sandstone, Mr. Strickland has demonstrated that the disturbance and elevation of the ridge took place after the deposit of the Lower New Red Sandstone, and anterior to the accumulation of the New Red, properly so called. In this fact some geologists may see an additional reason for classing the Lower (New) Red Sandstone with the coal-measures, both having partaken of the same elevatory movements. Though such a consideration alone ought not to guide us in classification, the facts so recently put forward by Professor Sedgwick of the prevalence of plants of carboniferous species in this red sandstone, both in Cumberland and in Warwickshire*, and the similar data, which I ascertained in Staffordshire, Shropshire, &c., may eventually lead us to consider all the sandstones beneath the magnesian limestone as naturally connected with the carboniferous æra, a view which my last researches in Russia have also led me to adopt. In this respect, indeed, the deposit agrees well with the rothe-todte-liegende of foreign authors, which, like our Lower Red Sandstone, contains both carboniferous plants, and occasional thin seams of coal.

From Mr. Trimmer we have received an account of the true geological position of the *Cucullæa decussata*, verifying that which was originally assigned to it by Mr. Webster, and confirming the justness of Mr. Parkinson's opinion, that the species is distinct from the

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* See Geological Proceedings, November 1841. [The paper here referred to will appear in an early Number of Phil. Mag.]

Cucullææ of the greensand, though in some more recent publications the Faversham fossil has been considered identical with the Cucullææ of Blackdown.

TERTIARY ROCKS.

An important addition to our knowledge of the relations of the Tertiary rocks of Europe proceeds from the pen of Mr. Lyell. On comparing the fossils of the *Faluns* of the Loire with those of the Cotentin, and again, by a comparison of both with the crag of Suffolk, Mr. Lyell has corrected a view which he had formerly adopted, that these deposits were not formed during the same epoch. By an attentive examination of different tertiary localities in Normandy, some of which seem to have escaped the notice of former observers, he has ascertained the existence of many of the true Suffolk crag fossils in deposits extending southwards as far as Sainteny. He then describes the *Faluns*, properly so called, at Dinan, Rennes, Nantes, Angers, Doué, Sevigné, and the tracts S. and S.E. of Tours, in some of which the great abundance of corals and echinoderms, and the small number of mollusks, present a perfect analogy to the white or coralline crag of Suffolk, though the fauna is quite distinct in species from the fauna of the coralline crag. From the existence of a number of detached points of *Faluns*, Mr. Lyell infers that a large part of France, now drained by the Loire and its tributaries, was submerged during the Miocene period. Finally, he convinces himself that all the shells of these French deposits belong to one group, and that they are really contemporaneous with the crag of Suffolk, though there may be shades of difference in their relative ages. It is well to observe that so sound a geologist as Mr. Lyell does not shrink from identifying two distant deposits in which eighty-five per cent. of the fossils are of distinct species, fifteen species only being found common to the two, because he shows that both these deposits correspond exactly in the analogy which they bear to the fauna of the present day. Having also detected freshwater and land remains in the intervening tract, Mr. Lyell further offers us a satisfactory explanation of how the Miocene *Faluns* of the Loire and our Suffolk crag should be contemporaneous deposits and yet so different in contents, the seas in which they were respectively accumulated having been separated by dry land; that in which the crag was deposited opening to the north, and those in which the *Faluns* were accumulated opening to the south.

Mr. Lyell's works being before us, I seize this opportunity of congratulating the Society that a geologist possessing his powers of classification should now be occupied in studying the structure of North America. In that wide field, in which for the last few years the native observers have been gathering together both a vast profusion of valuable detailed sections, as well as many general comparisons with our own divisions, it is impossible that a good European geologist can fail to reap an abundant harvest; and whether it be in his own tertiary domain, of which he has so largely extended our knowledge, or by grappling with the Palæozoic rocks, which in that vast continent are developed on so splendid a scale, our science is sure to profit from such a revision as our associate will be enabled to present to us. He has indeed already given us an earnest of his future communications, first in a letter to Dr. Fitton, on the older deposits in the state of Pennsylvania, and cites evidences in one tract confirmatory of the theory of terrestrial and lagoon origin of coal-beds, which was pointed out by Mr. Logan, who, having led the way in this inquiry, is now extending it in America. Notwithstanding the real value justly attached to these views, which have been supported by the labours of Mr. De la Beche, and which received an ample illustration in the last discourse of Dr. Buckland, I must caution geologists against applying this theory generally to all coal-fields because it has been found true in some, for it is manifest, that in those tracts (and they are numerous and large) where marine shells, ironstone and shale, filled with large fishes, alternate with beds full of plants, confusedly piled together, it will be impossible to account for the origin of coal by subsidence or overflow of masses of vegetation *in situ.*

In a recent communication on the Falls of Niagara, Mr. Lyell has taken the opportunity of explaining the sections of the American geologists who have described them, from Mr. Amos Eaton, who first showed the order of the strata, though his comparisons with British types were erroneous, to those of Conrad and James Hall, who have successfully placed these groups in parallel with our own Silurian strata. In showing the varied alternations of the hard and soft rocks which form the Silurian system of that region, and the exact inclination of the strata, Mr. Lyell exhibits chronometers of the probable retrocession of the falls, indicating where the river has worked back more rapidly when it had to recede through soft shale and sand, and how the solid barriers of limestone have presented greater obstacles. These data are indeed only more correct and more detailed illustrations of the general phænomena advocated by Bakewell, De la Beche, and the American geologists, that the recession is chiefly due to the water undermining soft shale and sand from beneath ridges of harder rock which are successively plunged into the abyss. It is well however to observe, that, from an inspection of the country, Mr. Lyell has modified his former view, that the letting off or bursting of the Lake Erie might be the ultimate result of the retrocession of the Falls, for he now seems to incline to the belief, that owing to the nature of the strata through which they will have to work back, the final result will be the formation of long and dangerous rapids; while he justly points out how the formation of canals and the demand of water for the use of the lower country, which is passing from a state of forest to one of cultivation, will cause a gradual diminution of the upper lakes, and thus prevent a future catastrophe. But the chief point of interest in this memoir seems to me to be the inference deduced from the occurrence of beds of ancient fluviato-lacustrine shells near the top of the cliffs bounding the defile of the Niagara, and necessarily high above its present bed, that the river has worn down its channel through a tract, in which the former water-courses (probably a succession of lakes or lake rivers) flowed on a much higher level; and he gives a strong reason for believing that the river has been the chief agent in this denudation, by stating that the channel in which it flows is not in any part the scene of dislocations or faults.

MICROSCOPICAL RESEARCHES.

The microscopic examination of fossil bodies was much enhanced in value when D'Orbigny astonished us by its application to the smaller cephalopods or foraminifera of the tertiary and cretaceous rocks, and by presenting us both with valuable descriptions and enlarged drawings and models. The discoveries, however, of Ehrenberg, and the much higher magnifying powers employed by him, opened out as it were a new former world of life, when he proved that certain strata were almost if not entirely composed of *Infusoriæ* so minute, that millions were included in a cubic inch of rock. In advancing his observations, this naturalist has recently asserted that certain species of animals of this class, which are *now living in seas and estuaries, were in existence when the cretaceous rocks were formed.* This announcement cannot but fail to arouse the lively attention as well as the surprise of geologists, who, relying upon what all the other departments of palæontology had developed, had come to the belief, that no form now living was created until after the completion of what are termed the Secondary rocks. If this discovery of the illustrious Prussian be substantiated, we see in it another proof, in addition to those which I have adduced in the previous pages, of the danger of as yet attempting to establish a nomenclature founded solely on the *fauna* and *flora* of former conditions of the planet. No terminology appeared less likely to be shaken than that proposed for the tertiary rocks by Mr. Lyell, nor could more time, thought and caution have been bestowed than he gave to the consideration of the names for the subdivision of the Tertiary Series, as founded on a great philosophical view. Whatever objections some persons might entertain to the upper divisions of his system, the characters of which were made to depend on a greater or less per-centage of existing species, there could be little doubt, from the multitude of previous researches, that his term "Eocene" was at all events secure from criticism. Many practical geologists believed that the close of the Secondary period was marked by some great agent of change, which in modifying the surface was followed by the creation of new races of animals. A few only argued that such a disruption or break in the sequence of organic life must be a partial phænomenon, and that as observations extended, we should find parts of the earth where transition strata of the supra-cretaceous age would fill up the hiatus which seemed apparent between the chalk and the tertiary strata over wide tracts of Europe. Such transitions, for example, it was contended, were observed by Professor Sedgwick and myself in the Austrian Alps, but the justness of our views was then combated by Boué, a geologist of great experience and research, whilst M. de Beaumont, M. d'Orbigny, and M. Michelin have since decided against us, the first mentioned by a visit to the spot, the two others by analogies worked out in the South of France. If our adversaries should prove correct, the microscope of Ehrenberg has done more than the eyes of the geologist; for whilst in the case of Gosau* the number of tertiary-like genera, such as *Volutes, Cerithia, Mitra,* &c., and the absence of all Ammonites and Belemnites constituted our case, the discovery of the Prussian microscopist goes to prove, from *specific* forms, that the Eocene or dawn of the present fauna had its germ in rocks as old as our chalk; and thus if we should be led to adopt his views, which however we can only do after some time and with great caution, the *only* barrier line which was abruptly placed between two formations as a general phænomenon, would be shaded off so as imperceptibly to connect the Secondary and Tertiary states of organic life.

In our own country this department of the science, which is in a state of great advancement through the labours of Owen, Brown, Stokes, and other naturalists, has been cultivated with much zeal in one department by Mr. Bowerbank. Having formerly shown that the flints and cherts of the cretaceous system were originally composed (at least in great part) of *sponges,* he has lately pointed out, that the fossil bodies in question did not differ as he had supposed from the horny sponges of commerce, having recently discovered siliceous spicula in the latter. After a detailed and laborious examination of moss-agates and jaspers from Oberstein, Sicily, and Hindostan, he sees in them all the proofs, more or less distinct, of tubular fibres—of what he believes to be gemmules—and the existence of vascular structure, and hence he infers, that sponges have had a still greater share than he originally supposed in the production of the solid strata. In the Egyptian jaspers Mr. Bowerbank detects between the layers composing a specimen hundreds of *foraminiferæ,* often difficult to distinguish from species known in the calcaire grossier of Paris. Though as geologists and mineralogists we may be startled by the announcement of signs of former life in the geodes of Oberstein, because they are certainly, like our trap nodules of Scotland, inclosed in rocks of plutonic origin, I am quite prepared to admit, with Mr. Bowerbank, that in many jaspers, at all events, the microscope should develope former types of life.

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* In regard to Gosau I must in candour state, that M. d'Orbigny has discovered in the upper greensand, "Craie chloritée," at Uchaux near Vaucluse, thirty-one species of Ammonites associated with some of the same species of Corals and Univalves, which occur at Gosau, and M. Michelin had indeed previously discovered other Gosau forms of corallines supposed to be of the age of the "gault" deposits, and thus no doubt seems to remain that the myriads of *tertiary-like* shells, and the absence of Ammonites and Belemnites on which Professor Sedgwick and myself rested our chief conclusions, cannot be assumed as proofs of the age of the Gosau rocks. It still, however, remains to be ascertained, whether this peculiar development of the cretaceous system of the Alps (in which one Ammonite only has been discovered and no Belemnite) is not after all a link between what has been called Tertiary and Secondary. At all events, the sections on the flanks of the Alps at Kressenberg, &c. lead to this conclusion.

When we consider the short period which has elapsed since these, the very minutest secrets of our solid strata, have been revealed to us, and by how few inquirers they have been studied, we may well admire the results. At the same time, seeing the great difficulties attending the study of these minute bodies, and the possibility that a certain amount of error *may* arise from the examination of such of these organisms as are imperfect under very high magnifying powers, I quite coincide with your late President, that we ought not to adopt too rapidly all the conclusions of the microscopists, however we must cordially thank them for the steps they are endeavouring to establish.

PROVINCIAL GEOLOGICAL SOCIETIES.

When presiding over this Society ten years ago, I congratulated my associates on the increasing taste for our science by the rapid rise of provincial scientific institutions*. I will not now endeavour to enumerate all these Societies, since through my ignorance I may omit to mention some which are well entitled to notice; but I will simply advert to two of the most recently established of these bodies, and whose objects are exclusively the same as our own, viz. the Manchester Geological Society, and the Dudley and Midland Geological Society.

The first of these, presided over by Lord Francis Egerton, has just published the first volume of its Transactions, which contains much good local geology, from the pens of our deceased member Mr. Bowman and Mr. E. W. Binney, and valuable descriptions of fossils by Capt. T. Brown. I am glad to find that the shells delineated for the first time in this volume, and which occur in the lower red marls at Collyhurst near Manchester, are now admitted to be in beds, which are equivalents of the magnesian limestone, an opinion it will be recollected which was expressed when these fossils were first brought to our own halls by Professor Sedgwick and Mr. Phillips†, thus offering a fresh proof that with newly-discovered lithological conditions, the same formation is often found to be diversified with remains unknown to us in the rocks of the same age which preserve their ordinary mineral characters.

Of the still younger Geological Society of Dudley, I have sincere pleasure in saying, that its first anniversary festival, at which I was requested to deliver an inaugural address, was eminently successful in uniting together the gentlemen of property in the neighbourhood with practical miners and fossil collectors, and there can be no doubt that an establishment so supported, and which is founded on ground so replete with countless subterranean phænomena, must have an honourable and a useful career. I refer you to the excellent Report of the Dudley Provisional Committee, a perusal of which, whilst it acquaints you that their museum contains some unique specimens and many worthy of a visit, will convince you that it is

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* Geol. Proceedings, vol. i. p. 377. [Phil. Mag., Second Series, vol. xi. p. 378.]

† See Geol. Proceedings, vol. ii. p. 392, [Phil. Mag., Third Series, vol. viii. p. 571.] and Silurian System, p. 50.

directed by men of scientific discernment and zeal, who can well describe and appreciate the value of such a collection.

I rejoice in the formation of these provincial societies, being convinced that they will work out details of great ultimate value; and whatever may be the objections to free trade among nations, I have no hesitation in proclaiming the benefits of free trade in geology, because I know that our own volumes have risen in value, and our ranks have swelled in numbers, with the birth and growth of our younger friends and rivals.

FOREIGN GEOLOGISTS—PRUSSIAN SCHOOL.

Let us now consider the progress which our science has been recently making on the continent of Europe.

The visit of the King of Prussia to our country upon the auspicious occasion arising out of the birth of our future Sovereign, was marked by an event most gratifying to our feelings. To testify to His Majesty your sense of his gracious and warm patronage of the cultivators of geology, and "to prove that English geologists can never forget the deep obligations they owe to the land which has produced a Humboldt, a Von Buch, and an Ehrenberg*," you elected His Majesty a Fellow of the Society. The condescension with which His Majesty subscribed our obligation book, and the interest with which He examined our collections within these walls, will be remembered by us with just pride. Attended by the great philosophical traveller whose researches have opened out the widest fields to the inquirers in every department of Natural History, we who have drunk at the fountains of knowledge poured forth by Humboldt, must indeed rejoice in the day when our veteran associate appeared in our halls as the chosen friend of the Prussian monarch. Honour be to the King who has the wisdom and discernment to attach such a man to his person and his fortunes! Any effort of mine to do justice on this occasion to the eminent services which Baron A. von Humboldt has rendered to science, would be both presumptuous and misplaced; but I must seize this opportunity to assure you, that if his valuable life should be prolonged for a short term, the public will be furnished with convincing proofs that his brilliant mind can yet confer on us the choicest gifts. Let others more competent to the task dwell on the high merits of his inquiries into the distribution of terrestrial magnetism and various branches of physical science which have already appeared, or are nearly ready for the press, in a stupendous work embracing nearly all natural knowledge; be it for us, however, to estimate the skill with which he has developed, and the power with which he has applied the laws of climatology and physical geography to explain many problems in the earth's structure.

Having myself been favoured with the perusal of some pages of a work on the distant parts of the Russian empire, which will very shortly be published, I venture (however incompetent to offer an

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* The above words were spoken by the President in admitting His Majesty as a Fellow of the Geological Society.

adequate analysis of its merits) to assure you, that this work will shed fresh lustre on the head of its author and of his associates Rose and Ehrenberg, in elucidating the metamorphism of rocks, the origin of gold veins, and the epoch of formation of the gold alluvia of Siberia; whilst in expounding the great sources whence the civilized nations of antiquity derived their precious metals, Humboldt, the geographer, the geologist, the botanist, the man of universal science, will appear before you as an antiquary and etymologist, not inferior in erudition even to his late illustrious brother.

In correcting the errors which had crept into our maps respecting the direction of the great mountain-chains of Central Asia, he places before us, and on the grandest scale, a striking coincidence between the state of mineralization of various parallel *meridians*, or N. and S. chains, and happily contrasts them with the different characters of those which have an east and west direction.

These splendid generalizations, like others previously known to us, results arising from a long life of scientific research, are of so extended and diversified a character, that whilst we all applaud, few of us are capable of justly estimating their whole bearing upon the progress of science.

Although his duties to his sovereign alone prevented our conferring upon this great chieftain in science an honour commensurate with his high deserts*, as Englishmen we may always reflect with delight, that when Humboldt appeared among us he received the universal homage which is so justly his due, and which his enlightened and benevolent monarch must have been proud to acknowledge as one of the highest compliments we could offer to himself and to his people.

In explaining the motives which induced the Council to award the medal of this year to M. von Buch, I have necessarily dwelt not only upon the former great services rendered by that eminent geologist, but also to his recent palæontological works. So actively indeed is he employed, that even whilst I write he is preparing a monograph on the genus *Productus*, thus offering fresh evidence of his sagacity and indefatigable research.

Ehrenberg, to whom I have elsewhere alluded, is daily adding to his conquests over the invisible realms of nature, and Gustaf Rose has written on the metamorphism and mineral structure of the Ural with so much ability, that it will be my special business to dwell at some length on this topic on another occasion.

In the construction of improved geological maps of various parts of the Prussian dominions, Professor von Dechen still pursues his useful and meritorious career. His large and detailed map of the Rhenish provinces, in which he has been aided by Erbreich and other good geologists of the Prussian school of mines, is I believe completed. Two years ago M. von Dechen kindly furnished me with an unfinished copy, which has served as the model, from which has been taken the small map prepared for the Transac-

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* Allusion is here made to the proposed national British scientific festival in his honour, which Baron Humboldt was compelled to decline.

tions to illustrate the memoir on the Rhenish provinces by Professor Sedgwick and myself. You must not, however, Gentlemen, judge of the very high merits of the original from the reduced skeleton map which we publish, and I beg you to consult the former as one of the most valuable documents of this nature yet offered to the public, particularly in the elaborate delineation of every variety of igneous, metalliferous, and metamorphic rocks, in a region so strikingly replete with them. Silesia has also occupied much of the time of M. von Dechen, in some districts of which he has marked the existence of bands of carboniferous limestone as distinguished from the Devonian, Silurian, and older members of the palæozoic series.

Oeynhausen, the old associate of Von Dechen, and so well remembered by many of us, has recently bored in search of salt springs through upwards of 1000 feet of lias near Pyrmont, a fact which ought to teach us great caution in estimating what may be the maximum thickness of deposits. In our own country, the accurate method which Mr. De la Beche employs to test the thickness of deposits, will eventually give us, I trust, close approximations to the facts; and I learn from him that some of the ancient strata (the carboniferous for example) which have been accumulated in basins are enormously more thick than we had supposed, whilst others extending, like the Old Red Sandstone, over wide areas in lofty escarpments, will not prove to have those dimensions we had assigned to them. When, indeed, we consider that all shales, sandstones, &c. were once nothing more than the blue and black, and red mud, or sand which occupied the bottom of seas in former epochs, it seems as difficult to decide from general observations on the maximum thickness of any great deposit, as it would be to insist on the utmost depth of the ocean without the survey of the hydrographer. The borer and the field engineer must therefore combine to enable us to speak with precision on the vertical dimensions of strata.

RUSSIAN AND NORTHERN SCHOOL.

Not having yet personally visited Sweden, Norway, and Denmark, I am not prepared to say what progress our science has recently made in these states, but I may remark, that the beautiful map of Norway by Keilhau has scarcely received the attention which it merits; and we may be sure that the countries of so good geologists as himself and our associate Forchhammer, cannot be lagging behind in the general onward movement.

In regard, however, to Russia, I am enabled to speak with some confidence, after the two visits which I have paid to that country. Gratified as we were, not only by the most hospitable reception, but also the kind assistance afforded us by every Russian, from the Emperor to his humblest subject, it was a real source of delight to my associates and myself in our first visit to trace throughout the northern regions of that vast empire, the same palæozoic divisions which have been proposed as types in the British Isles. During the last summer we extended our researches to the distant Ural, the Siberian plains, and the steppes of the south; and afterwards terminated the whole of these observations by a general transverse section from the sea of Azof to the Baltic. Although we carried with us into Russia, what may be called the geological key of that great country, by which the chief subdivisions and relations of these rock masses have been established, let me say that Russia herself contains naturalists and geologists who would rank high in any land. In palæontology, Eichwald and Pander have already largely contributed to our knowledge; the first, by numerous local works, and recently by his illustrations of the Silurian strata in the Baltic provinces of Russia; the latter, by his very original researches into the fossils of the same strata, the lithological characters and detailed relations of which were first given by our own Strangways. Professor Asmus of Dörpat is about to enrich us, as I have already stated, with a most curious and elaborate work on the fishes of the Old Red or Devonian system.

The great steps, however, which Russia is now making in field geology and stratigraphical arrangement, are owing to the clear and well-defined view of this subject which has recently been adopted by the Imperial School of Mines at the suggestion of the energetic chief of their staff, General Tcheffkine, who, under the orders of the enlightened Minister, Count Cancrine, has taken the surest means of advancing practical geology, and of rendering many officers of his corps well acquainted with our subject; not only by adopting the suggestions of those qualified to judge respecting the formation of geological maps, but by so increasing the fossil collections of the Imperial School of Mines, that it is now furnished with many illustrations of the sedimentary deposits of the empire, even from the remotest parts of the Altai and the countries bordering on China. It will be my duty and pleasure very shortly to bring before your notice the names of many officers of the Russian corps of Mines, whose labours were of material use to myself and associates in our distant explorations; but I cannot resist naming at once Colonel Helmersen, the inspector of the establishment, who whether he be viewed as a physical geographer, a geologist, or as a writer, has rendered most valuable service to Russia by his luminous and attractive descriptions of the structure and outline of various parts of the empire, including the most remote tracts. I beg also to refer you to the five published volumes of the School of Mines, as works containing much excellent matter, and highly creditable both to the government which promoted their publication, and to the officers whose memoirs they contain.

In the mean time, besides what is doing on the Neva, a periodical work on Russia has appeared at Berlin under the title of 'Archiv für Wissentschaftliche Künde von Russland,' by the enterprising traveller A. Erman, of which two parts are published. Together with various memoirs on physical geography, history, language, antiquities, and physics, the editor has added a sketch of the recent advances in the geology of Russia, and illustrates his views by the publication of a small outline map of the empire. In the estimate of the geological steps in Russia which various labourers have accomplished, I rejoice to see the name of our countryman Strangways placed where it ought to be, as the first who applied the methods of modern practical geology to that empire, by the publication of his general map in the year 1822. Nevertheless it is too certain, as M. de Verneuil and myself informed you last year, that when we first visited St. Petersburgh in 1840, this map, though published in our Transactions, was, as far as we could ascertain, unknown to the men of science in that country. In the first memoir on Russia, we specially directed your attention to the merits of Strangways, and we shall have ample opportunities hereafter of reverting to them. What I have now to observe in reference to the map of M. A. Erman is, that in his account of it, the special researches and the new points which my friend M. de Verneuil and myself established, are merged with what I must consider the copies of our views. The source whence the chief materials were obtained, is sufficiently proved indeed by the words "Silurische und Devonische schichten" engraved upon the map, particularly when coupled with the fact, that M. de Verneuil, Count Keyserling, and myself are the *only* geologists who traced the older groups to the White Sea, aided materially, as we have previously acknowledged, in a part of that region, by the Baron A. de Meyendorf, and for a short time by Professor Blasius. The original observations which we made were inserted by myself on a map which was shown at Moscow and St. Petersburgh in August, and to the British Association at Glasgow, in September 1840. On this map the range of the great bands of Silurian, Devonian, and Carboniferous rocks from St. Petersburgh and Moscow to the White Sea, with a vast basin of red deposits in the governments of Vologda and the Middle Volga, were laid down, I assert, *for the first time,* and thus established the essentially distinguishing features of subdivision of the North of Russia.

After the application of this basis, Colonel Helmersen, to whom I have alluded, put together in the ensuing winter a small general map of Russia in Europe, in which he inserted the result of the labours of M. de Verneuil, the Baron A. de Meyendorf, Count Keyserling, Professor Blasius, and myself, acknowledging our services as well as those of all previous observers. The map of M. Erman which followed, was prepared by the Baron A. de Meyendorf and his companions, who extended the knowledge which they acquired with M. de Verneuil and myself to some of the central and southern parts of Russia, and thus marked a new step in the development of the structure of the empire. Since that time, the extended geological researches of the expedition in which my friends M. de Verneuil and Count Keyserling were associated with me, aided by Lieut. Koksharof, and an independent survey of Colonel Helmersen, have thrown a new light over the structure of various parts of the central, eastern, and southern regions, and have rendered necessary considerable changes in all previous maps. As a mere prelude, therefore, to what may hereafter appear, I have, with the aid of my associates, coloured a small general sketch-map of the empire, including the Ural chain, which as it will shortly appear before you in a published form, I only mention in this place to assure you that it differs very essentially from all previous maps.

Whilst on the topic of Russia, I will now state, that if on account of the preparation of this discourse and other official duties I had not been greatly occupied, I might before now have presented to you some of the results of the second visit to that country. In the mean time, however, my colleagues, M. de Verneuil and Count Keyserling, have been sedulously comparing our collections of fossils, and reducing a vast number of barometrical observations, whilst with their cooperation I have already completed a general table of superposition of Russian deposits, which, with a section across Russia, and the map above alluded to, are now nearly ready for publication. My brother geologists will feel that a general table of classification ought to be the finishing stroke in illustration of any country previously little known, and respecting which so much confusion prevailed. We offer it, however, in the persuasion that its leading divisions will be supported by the evidences hereafter to be brought forward, and we simply put forth this table (which was drawn up at Moscow after our second journey) to convey to the cultivators of our science the chief results of our inquiries, and to place them upon record as bearing date from September 1841.

Among these results I will now merely allude to the first announcement of some of them, in a letter of the above date, addressed to Dr. Fischer de Waldheim at Moscow, in which the two points most dwelt upon were the discovery of a large central dome or axis of Devonian rocks, which separates Russia in Europe into two great north and south basins of very dissimilar characters; and the classification of certain cupriferous deposits of sand, marl, limestone, &c. under the term of "Permian system." As the explanation of the reasons which led to the suggestion of this name will be shortly offered to you in full detail, I should not now occupy your time by alluding to it, had not the mention of the word already called forth from M. A. Erman the remark, that these deposits have been long known to other observers. I admit that they were mineralogically known, but I deny that their geological position had been determined by any competent geologist previous to the researches of myself and friends; and I contend that there was no Russian formation concerning whose age so many contradictory opinions had been expressed. As a proof of this, I may state that the illustrious Humboldt himself assured me in the spring of last year, that it was the great point to which he hoped our labours would be directed. So strongly indeed was the difficulty of placing these strata in their correct geological horizon felt by Russian observers, that Major Wangenheim von Qualen, who had long and patiently studied them *in situ*, and Dr. Fischer, who had ably described many of their fossil contents, at once abandoned the field to my associates and myself, and put us in possession of all their knowledge, avowing their inability to arrive at a satisfactory geological conclusion. I was, therefore, surprised to read the premature criticism of M. A. Erman; the more so, as that author has called a large portion of the great limestone of Russia, *Jurassic*, which we have ascertained to be carboniferous, and to form the support of the hitherto anomalous system, which we shall endeavour to place in parallel with its equivalents in Germany and the British Isles, by showing its place in the order of superposition, and by describing the fauna and flora by which it is characterized as a distinct type intermediate between the Carboniferous and Triassic systems.

FRENCH SCHOOL.

From the northern parts of Europe let us now pass to the consideration of the chief points of progress which our opposite neighbours are making. The publication of the splendid geological map of France, executed by Messieurs Elie de Beaumont and Dufrénoy, is indeed a subject of gratulation for the scientific men of all countries. Commenced in 1827, the map would have appeared five or six years earlier, had not the engraving of it led to unexpected delays. The part surveyed by each author is easily defined. France was divided by a line, proceeding from Hâvre, through Alençon, Avallon, Lyons, and Marseilles, to the Mediterranean. The western part was assigned to M. Dufrénoy, and the eastern to M. de Beaumont; but each was empowered to extend his observations, not only beyond the line of division, but also into those parts of the neighbouring countries which are included within the limits of the map.

The authors pursued their researches separately for several years, but as soon as they had settled the bases of classification they united to survey those points which required their conjoint examination, and by this means they finally established a perfect agreement in all the parts of their great undertaking. During the last five or six years, since the main features of the map were completed, the results have been communicated to every geologist who sought information, as I myself have experienced in my visits to Paris; and the authors, accepting in the mean time the contributions of others, have brought the map to its present degree of perfection.

Wishing to popularize geology in France, and to give their labours an extended sphere of usefulness, Messieurs de Beaumont and Dufrénoy have published, with the first volume of explanations which accompanies the large map, one on a reduced scale, giving an exact idea of the disposition of the mineral masses, and facilitating the comprehension of the admirable descriptive memoirs contained in the volume.

A desire has been often expressed, as you know, that all geologists might come to an understanding on the choice of colours, so that geological maps might be a sort of book written in a universal language. This idea, as our own great geological geographer Mr. Greenough has found, is more plausible in theory than practicable. In the selection of their colours, I confess, I regret that our foreign associates have not employed the normal colours used in the map of England, but then we must recollect that the principle of their colouring was decided and put into execution long before the publication of Mr. Greenough. The authors of the French work have however done well in giving one colour only to each great natural division of rocks, and they have distinguished the subdivisions by conventional signs, in a similar manner to that employed in the map of the Silurian region and Mr. Greenough's map of England. The advantage of this certain method of showing the relations which exist between the different parts of the same formation, is now thoroughly recognised.

Under the modest title of explanation of the map, the authors will publish three quarto volumes, of which the first only has yet appeared, and judging from this specimen we have a right to conclude that they will form one of the most splendid and useful works ever executed on the geology of a great country. In the introductory chapter of the published volume the general principles of the science are admirably given, and the succeeding chapters are occupied by descriptions of the "*Massif central de la France*: *Presqu'ile de Bretagne, Ardennes*: *Vosges*: *Montagnes littorales du Departement de Var*: *Terrains Houillers.*" The authors have divided their descriptions into great geographical regions, beginning with the most ancient formations; and I cannot resist expressing how much pleasure it has given me to see that these eminent men have adopted the divisions and nomenclature which have been proposed for the palæozoic rocks of England. In the other volumes the authors will describe the more recent formations, reserving for the conclusion, the account of those parts of France where the elevated and dislocated sedimentary deposits present problems most difficult of solution, and which continue to raise doubts in the minds of the best and most experienced observers. In their description of the rocks, the authors, faithful to what may be called the "natural method," have classed together all those which appear to have a common origin, such as granites, porphyries, basalts, trachytes, &c.

In short, the geological map of France, and the volumes of explanation to accompany it, will form one of the finest monuments raised to science in our æra, and must be constantly consulted by those who wish to understand the spirit of that school of geology, which has cast such a brilliant light over France and throughout Europe. Doubly grateful indeed is the production of the work to ourselves, for in presenting it to this Society its authors have assured us that it was in our own islands they first acquired that knowledge of classification which led them to attempt the great enterprise, the completion of which so well sustains the high reputation they enjoy. Further, when we recollect that the knowledge of our foreign associates was one of the first fruits of the general peace, well may we now view the noble structure they have reared upon such a basis, as a convincing proof of the advantages conferred on science by the friendly intercourse of nations, which now rival each other only in advancing science and art!

Another most important work undertaken in France during the last year, is 'La Paléontologie Française' of M. Alcide d'Orbigny. Early initiated into the study of organic bodies and the anatomy of mollusks, this naturalist has acquired, during his extended travels, a good knowledge of positive geology; and he is therefore peculiarly qualified to carry into effect his arduous enterprize of describing the fossils of France in the order of the formations. He has commenced this vast undertaking by publishing during last year 139 plates, and upwards of 500 pages of text, on the Cephalopods of the Chalk. It is only necessary to glance over the figures, to perceive the care with which the different parts of the fossils are delineated. I particularly recommend to your notice the new genera, named by M. d'Orbigny "*Ancyloceras*" and "*Toxoceras*," and which added to the "*Crioceras*," recently introduced into the science, increase that infinite variety of forms in which the great Ammonite family expanded, previously to its total disappearance from the living world.

The Cephalopods, very rare in the upper beds of the chalk, occur in such prodigious quantities in the lower parts, and particularly in the "Neocomian" group, as defined by continental geologists, that they occupy all the Numbers hitherto published of the 'Paléontologie Française.' The Ammonites have been the object of especial study to M. d'Orbigny, and have led him to conclusions of the highest interest, both zoological and geological. In the former respect, his observations on the external characters of Ammonites, and on the limits of their natural and accidental varieties; of the differences of sex, and particularly of age, are entirely original. Following these remains through the period of their development, he describes the transformations they undergo, and investigates the laws of such changes. The chambers, or the internal characteristics of Ammonites, the importance of which was long ago indicated by Von Buch, have presented new features to M. d'Orbigny, which are easily applied to the purposes of classification. I speak of the distinction of the "*selles et lobes en parties paires et impaires*," according as they are cloven at the extremity, or terminate in a conical point. Combining this characteristic with that of the length of the dorsal lobe, and with those afforded by the exterior ornaments of the shell, the form of the back and mouth, between which there is almost always a coincidence, M. d'Orbigny has made twenty-one natural groups, of which eleven had been already established by Von Buch, and ten are new. Of these twenty-one groups, seven are peculiar to the Jurassic or Oolitic formations, ten to the cretaceous, and four contain species common to both.

M. d'Orbigny points out the modifications of species through time and space, and shows the relation that exists between certain forms and the beds which contain them. He recognizes three new creations or replacements of the species of Ammonites during the cretaceous period, and thus establishes, on zoological data, three divisions of natural groups;—first the Neocomian*, second, the

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* We have to learn why the very well-defined British formation, the Lower Greensand, seems to be suppressed and merged by our opposite neighbours in the "Système Néocomien." Cannot the Lower Greensand be preserved and the Neocomian be considered as a marine equivalent of our Wealden?

Gault, and third, the Upper Greensand (Craie chloritée), and the White Chalk; and he estimates that in this triple succession of deposits, the Ammonites gradually decrease according to the numbers seventy-five, forty-two, and twenty-seven, to disappear finally with the uppermost chalk or Maestricht beds, and before the tertiary epoch.

The total number of determined species of Ammonites in the great cretaceous system of France is 144, according to M. d'Orbigny, and with the exception of three, which are common to the Gault and the Upper Greensand, all the other species are divisible into groups, each of which is peculiar to one of the three great divisions of this system, and may be considered characteristic of it. Although the species have been thus replaced several times during the cretaceous period, there exists, however, among them a certain affinity of forms which differs sufficiently from the general characteristics of the Jurassic Ammonites to constitute the beds containing them a truly distinct and separate series. We may congratulate M. d'Orbigny on having begun his 'Palæontology' with the fossils of this period: for whilst the labours of the English, particularly the admirable general views and detailed descriptions of Dr. Fitton, and the works of Dr. Mantell, have contributed to a good acquaintance with the northern chalk and greensand, it must be confessed that there is ample room for research in the southern type.

In consequence of the numbers of fossils sent to M. d'Orbigny from all parts of France, and which I had the pleasure last spring of seeing on his tables, a new light may be thrown by the 'Paléontologie Française' on the classification of the sedimentary masses of the Alps and Apennines; the limestones of Greece, Turkey in Europe, Palestine, the coasts of Africa and in fact of the whole circuit of the Mediterranean, the chief formations of which are at present arranged in the cretaceous epoch.

I might now notice the recent labours of M. Rozet, M. Leymerie, M. Rolland du Roquand, M. Duval and others, whose memoirs have been partly published in the volumes of the Geological Society of France, but such duties pertain to the office of the President of the French Society, and doubtless, the eminent man* who is now at the head of it will do ample justice to these authors.

BELGIAN SCHOOL.

In Belgium, the most important works that claim our attention for the year 1841, are,—1st, the completion of the field survey of the Geological Map of Belgium by M. Dumont, which was begun more than four years ago, and has been pursued with the zeal and ability manifested by the author in his first publication, and which we had so much pleasure in rewarding with our Wollaston Medal. I learn, however, that the appearance of the map may be delayed in consequence of the time necessary to complete its engraving: 2ndly, I may notice a great palæontological work, undertaken by M. de Koninck, several plates of which I examined last spring in Paris.

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* M. Cordier.

This young naturalist, already known by his works on Conchology, is about to give us, in fifty or sixty plates with descriptions, an account of all the fossils of Belgium, from the Lower Silurian to the coal-fields inclusive, whether published or not. This work, of which the first Numbers have appeared, will doubtless be of great assistance in completing the classification of the palæozoic rocks of Belgium, the lithological and mineral characters and lines of demarcation among which had been so faithfully and clearly described by Mons. d'Omalius d'Halloy, and Professor Dumont.

OTHER FOREIGN COUNTRIES.

From France and Belgium I shall, in the sequel, direct your attention to certain works which have appeared in relation to the Alps, where the glacial theory more particularly is at present the great subject of debate. I would now carry your attention to the southern parts of Germany and to Italy, but with the exception of an able memoir by Professor Sismonda of Turin, 'Osservazioni Geologiche sulle Alpi marittime,' &c., and a Monograph of the fossil *Murex* by Michelotti, I have not been able to make myself acquainted with the recent literature of our science in those tracts, though I have no doubt that they have been illustrated by good workmen, of whose labours I may be enabled to speak at our next Anniversary. On that occasion, I will further endeavour to take a view of the last advances which geology has made in the other quarters of the globe, whether in the numerous British Colonies, or in the United States, or in those parts of Asia and Africa which have been recently explored.

In respect to American geology, I have, however, to notice two short communications to ourselves by Mr. Henwood; the first on the beds near Lockport and at Rochester, in which he sustains, by aid of a series of organic remains presented to the Society, the views respecting those strata entertained by American geologists; and the second on parts of New Brunswick, particularly the coal-measures which extend over a wide area, and rest in some places upon granite and in others upon schistose rocks; and he shows that though granite veins penetrate the slate, not one is to be found in the coal-measures: hence he infers, that the schists are the oldest rocks of the country, and the coal-measures the newest.

THE GLACIAL THEORY.

The last subject I will advert to is that of glacial action, which has recently occupied the thoughts of many geologists. From a study of the Alps, where Venetz and Charpentier led the way in showing that a connexion existed between the erratic blocks and the advance of glaciers, Professor Agassiz has deduced a glacial theory, and has endeavoured to generalize and apply it even to our own countries, in which effort he has been supported by my predecessor in the Chair. In the following observations, I will endeavour to point out what new materials have been brought forward, abroad and at home, to enable us to reason correctly on this difficult question, and I will then suggest some essential modifications of the new hypothesis.

As propounded by Agassiz, the glacial theory, even in its application to the Alps, has met with an opponent in the person of Professor Necker de Saussure. In the first volume of a work which he is now publishing, M. Necker treats, in great detail, the whole subject of superficial detritus connected with the northern and western watershed of the Alps, and gives us the fruits of many years of observation. Adding very considerably to the list of phænomena of transported materials collected by M. A. de Luc, he takes his own illustrious ancestor, De Saussure, as his model, and following in the track of the historian of the Alps, he endeavours to enlarge and improve upon that great observer's suggestions. Pointing out the distinctions between two classes of detritus, viz. one of high antiquity and another of modern date, M. Necker contends that the enormous masses of the ancient drift or diluvial detritus have a direct connexion with the actual configuration of the surface, because the *chief* part of them has been derived from the centre of the chain, the flanking and lower mountains, and even the strata on which it rests, having contributed comparatively little to the great advancing body. Examining the high valleys about Chamouni and the foot of Mont Blanc, and finding massive walls from 300 to near 600 feet in height, composed of this ancient diluvium in its coarsest form, near the extremities of certain glaciers, he concludes that they were once the moraines of glaciers which melted away and retired from them. He then goes on to suppose that when the recession of the glaciers took place (an effect which he refers to the same cause as De Saussure) such transversal moraines formed dykes standing out at some distance from the mountain and barred-up lakes formed by the melting of the snow and ice. These lakes, at length swollen to excess, are supposed to have burst through the moraine barrier, and to have drifted the materials of which it was composed into the lower countries. M. Necker believes that when these ancient glaciers existed, the Alps were considerably higher than at present, and he judges that such was the case, because the "aiguilles" of Mont Blanc have been lowered very considerably in our own times. Arguing that great blocks are never found at the foot of mountain chains which have not permanent glaciers, of what De Saussure called the "first class," he cites many negative examples, and brings forward the Pyrenees, where no true erratic blocks are seen, as a proof that the minor or second class glaciers, which there occur, never advanced sufficiently far to dam up water-courses, and thus to form those great lakes, to the letting off of which and to the destruction of vast moraines, he attributes the presence of large boulders in the Alps.

I must, however, remind M. Necker, that if he assumes that all great erratic blocks are to be referred to some *neighbouring* chain, now the seat of glaciers, he forgets the cases in Scotland and England, and indeed many others, far removed from mountain ranges, and which must be classed, as I shall presently show, with submarine deposits. Indeed by far the widest spread of erratic blocks with which we are acquainted, extending over the plains of Germany and

Russia, must have taken place (as I believe at least) when those flat regions were beneath the sea, for recent observations have shown, that the blocks constitute the uppermost or last surface deposit in tracts which exhibit, here and there, proofs of having been an ancient bottom of a sea. But without extending his theory to other parts of the world, it does not appear to me, even when confined to the Alps, that M. Necker explains satisfactorily how the granite blocks of Mont Blanc should lie upon the Jura, by any reference to subaërial debacle; for if we are to imagine the deep hollow of the lake of Geneva, filled up with gravel, sand and mud, and forming an inclined talus from the centre to the flanks of the chain, the subsequent scooping out of this enormous mass of materials involves an intensity of degradation as difficult to believe in as the former extreme climate of Agassiz, by which thousands of feet of snow and ice are supposed to have occupied the same deep valley. I ought not to omit to state that one of the chief elements introduced by Agassiz into this question, the polished and striated surfaces of the rocks, has not yet been alluded to by this author, but will be treated of in his second volume.

In the mean time, however he may fail to account satisfactorily for the transport of the very distant great blocks, we have to thank M. Necker for the additional materials, which seem to establish one fundamental fact in reference to the Alpine case, viz. when this detritus was cast off, the gorges and flanks of the chain had nearly the same reference to the central crest as that which now prevails. If this be proved, the theory which depends chiefly upon the supposition, that a great elevation of the centre of the chain broke off the ice and dislodged the glaciers, is deprived of its chief basis. In what manner Professor Agassiz can account for the Alps being a great centre of dispersion *when at a lower level*, is indeed a part of his theory which is not easily comprehended. On the other hand, whatever we may think of M. Necker's hypothesis, it must be admitted that the facts adduced by him support one essential point of the glacialists, by connecting the presence of blocks with the existence of glaciers in the Alps, the former being, as he states, invariably found both in the southern and northern watersheds of those mountains, and at the mouths of the great transverse ravines which lead up to the regions of perpetual snow, and in all such cases he allows that the condition of the blocks is highly indicative of their having once formed part of the "moraines" produced by former glaciers.

But the important point, that the glacier is the chief source of the origin of erratic blocks, is entirely denied by another antagonist to the theory of Agassiz, who has appeared in the person of M. Godeffroy[*].

After the observations of two summers in the Alps, this author has become convinced that the materials of the so-called moraines have not been derived simply by the glacier from the solid rock in the higher mountains, but are the re-arranged portions only of a

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[*] Notice sur les Glaciers, les Moraines et les Blocs Erratiques, 1840.

great pre-existing diluvial deposit, which had been accumulated in the radiating valleys during a period of great disturbance, anterior to the existence of glaciers in that latitude. Describing (like M. Necker) one of these "trainées" as having a continuous length of fifteen leagues, he infers that such a mass could never have been deposited by a glacier proceeding from mountains of no greater altitude than the Alps. Arguing that glaciers are merely the condensed or central portions of vast accumulations of snow, forced downwards into the gorges by increasing volume from above, the chief novelty of M. Godeffroy's work is contained in the opinion, that in advancing, these bodies of ice cut through the ancient diluvium or drift, just as a plough-share cleaves the soil ("presso tellus consurgit aratro" being his motto), and threw up some portions into lateral moraines, as well as pressed before them others to form terminal moraines. To the crystalline and mechanical changes which the snow has undergone in its passage into solid ice, is attributed much of the confusion and irregularity of outline so visible in the "aiguilles" and other icy masses of the Alps; and to the same disturbing action is referred the rounded and worn exterior of the boulders in moraines, as contrasted with comparatively angular blocks of the pre-existing drift which have not been in contact with the glacier. I refer you to the work of M. Godeffroy for the explanation of the manner in which he supposes the surface of the advancing or retreating glacier was subjected to lateral overflows or "écroulemens" of stones, gravel, and earth, and also for his theory of medial moraines; but I now bring to your notice his ingenious effort to solve one of the very difficult climatological problems in the Alps. Having shown how the lower valleys must, from year to year, become more and more encumbered with detritus, he seizes this fact to explain by it alone, both the well-known retreat of the glaciers and the fact brought forward by Venetz and other observers; viz. that roads which existed in certain former passes of the High Alps are now quite choked up with snow and ice—a fact which has been supposed to indicate a sensible decrease of temperature within the historic æra. M. Godeffroy contends, that in ancient times, when the gorges were more open, and the heaps of detritus at the entrance into the lower valleys were less in size and fewer in number, and when consequently the glaciers easily extended to greater distances, the continual and unrestricted supply of snow and ice from many affluents more than countervailed the loss through atmospheric action; but that as the obstacles increased at some distance above the terminal moraine, the lower ends of the glaciers not being so fed as to regain in one season the melting losses of the previous year, the inevitable result was a successive shrinkage and retrocession of the mass. The increase of snow and ice in the upper passes, and the blocking up of the roads, are explained by the same agency; for as soon as the descent of the glacier from the higher to the lower Alps was impeded, it would follow, that the frozen matter of the higher regions, deprived of its previous exit, must find its way into the adjacent upper depressions, and there form those *mers de glace* which have obstructed the road-ways or passes of our ancestors. Thus is the supposed anomaly explained without recurring to any change of climate*.

In that part of our own country to which the glacial theory has been applied, Mr. Charles Maclaren, already known to you by excellent geological treatises, has recently published a well-condensed, small work explaining the views of Agassiz. The phænomena of glaciers and the general doctrines derived from their study being explained, Mr. Maclaren proceeds to analyze those cases of transported detritus in the neighbourhood of Edinburgh to which the theory had been supposed to apply.

A year and a half only has elapsed since Professor Agassiz and Dr. Buckland seemed to think, that this district was as rich in proofs of the action of glaciers as many other parts of Scotland which they visited, and as I happened to witness the efforts of my predecessor in this Chair to attach Mr. Maclaren to his views, I must be permitted to direct your attention to the practical results at which this gentleman has arrived, in some prominent cases.

Observing blocks of greenstone on Arthur's Seat, which, from their peculiar structure, must have been transported from Salisbury Craigs, a *lower* hill, and separated from the former by an abrupt valley, Mr. Maclaren infers, that if the present surface of the land be argued upon (and in all questions of glaciers this is a postulate), neither glacier, nor iceberg, nor current will explain the fact. It is unnecessary that I should here examine this author's hypothesis, by which in order to solve the local problem, he restores the inclined stratified masses of Salisbury Craigs to such an extent as to give them an altitude in ancient times superior to that of Arthur's Seat; for whether we adopt his ingenious view, involving a mighty subsequent denudation, or suppose that in the oscillations of this plutonic tract the former low and high points of land have been relatively depressed and elevated, it is obvious, from the very structure of the rocks, that in both cases a subaqueous, and not a subaërial condition is called for to explain the appearances, and this too, be it recollected, on the summits of the highest hills in the immediate vicinity of the Scottish metropolis, in and around which the action of glaciers has been supposed to be visible at much lower levels!

Among the examples of the scratched and polished surfaces of rocks near Edinburgh, I do not perceive that the glacialists have grappled with certain appearances on which Dr. Buckland formerly dwelt with so much pleasure, viz. the grooved or channeled

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* I hoped to have been able to quote the opinions of Professor J. Forbes on this *vexata quæstio*, because it is well known that he was a companion of Professor Agassiz in the Alps during the last summer, but this distinguished cultivator of physical science has not yet published his views on the action of glaciers as affecting the surface of the earth, though he has given to the public a very ingenious sketch, descriptive of a peculiar parallel striation in the solid ice of glaciers.—Edinburgh New Philosophical Journal, January, 1842.

surfaces of the Braid Hills, first pointed out by Sir James Hall, and which the great chemical geologist attributed to a powerful rush of waters. When I visited the low ridge in question with Dr. Buckland and other friends*, my conviction was that these grooves, though then attributed by Dr. Buckland to glacial action, are due neither to that agency, nor to any rush of waters, but are simply the result of the changes which the mass of the rock underwent, when it passed from its former molten or pasty condition into a solid state. These appearances differ essentially from ordinary glacial scratches or scorings†. They are, in fact, broad undulations or furrows, and instead of trending *from* the higher grounds to the Firth of Forth, as would naturally be the case if they were due to the expansion and descent of glaciers, they rise up to the very *summit* of the low ridge in a direction transverse to its bearing, and with no neighbouring point of ground higher than that on which they occur. On clearing away the thin turf which barely covered the rock, some of these undulations in the surface appeared wide enough to contain the body of a man, and though observing a rude sort of parallelism, their forms were often devious. As their surface was smooth, not much unlike the usual aspect of the so called "moutonnés" rocks, the glacialists of our party at first seemed to be proving their case, when suddenly a discovery destroyed, at least in my opinion, their theory; for in the adjacent quarries of the same hill, at a much lower level, and upon beds just uncovered by the workmen from beneath much solid stone, other sets of undulations or grooves were detected, so like to those upon the summit of the hill, that a little atmospheric influence alone was required to complete their identity. My belief therefore is, that the undulations were caused by the action which took place when the stone was solidified.

Phænomena of a similar nature to the Scottish have been since observed in Wales by our late Fellow, Mr. Bowman. Captivated by the glacial theory, and having himself endeavoured to show that it could even be as successfully applied to the South as to the North of Scotland, he examined the highest region of Wales, with the geological structure of which he was previously familiar, half convinced, *à priori*, that he would naturally find in those mountainous tracts some proof in support of the new views which he had adopted. He, however, quitted that country without having been able to observe any evidence whatever in favour of the Alpine theory, though his journey enabled him to detect several examples of striated rocks, which in unskilful hands might have been mistaken for the effects of glacial action; and these he holds up as warning beacons. After stating that there are, in his opinion, no terraces which any follower of Agassiz can construe into "moraines," whether terminal, medial, or lateral, on the flanks of the mountains of Snowdon, the Arenigs, or the Berwyns, he describes three distinct and differently

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* Dr. Graham and Mr. Maclaren were of the party, in Oct. 1840.

† Plaster casts of these exist in the Geological Society.

formed sets of parallel markings which he observed in the newly uncovered surfaces of the schistose Silurian rocks, and shows satisfactorily how such appearances, as well as the tops of the joints, might be mistaken by cursory observers for scratches, although they are in fact due to structure.

Unlike Mr. Bowman, Dr. Buckland has not confined his views of the action of glaciers to Scotland, but applies them largely to the North of England and to Wales. He has recently endeavoured to satisfy us, that the rocks on the sides of the chief valleys in the latter country which open out from a common centre of elevation are striated, worn and polished in the direction of the present water-courses, and these he conceives to be evidences of former *glaciers*, which filled up all the valleys radiating from Snowdon to a distance of many miles from a common centre. I confess I see almost insurmountable objections to this view. Apart from other evidence, the very physical geography of this tract is at variance with the construction of such an hypothesis. In the Alps, and indeed in every other part of the world in which they have been observed, the length of glaciers is in ratio to the height of the mountains from which they advance, or, to use the words of Agassiz, from which they *expand*. Now whilst in the present days, a small glacier hangs to the sides of a mighty giant like Mont Blanc, having the altitude of 15,000 feet, our Welch hills, having a height only of 4000 feet, had glaciers, by the showing of Dr. Buckland, of a length of many miles. Again, in the same memoir, which fills so large a portion of the principality with glaciers, the author comments upon certain facts already well known to us, viz. the existence upon Moel Tryfane and the adjacent Welch mountains of sea shells of existing species, at heights of 1500 and 1700 feet above the sea, where they are associated with mixed detritus of rocks transported from afar, all of which have travelled from the North, the hard chalk and flints of the North of Ireland being included. How are we to reconcile these facts with the theory that the greater part of the country in question was frozen up under the *atmosphere* in some part of the same modern period? Unable otherwise to explain how marine shells should be found on mountains which are supposed to have been previously and during the same great period occupied by terrestrial glaciers the accumulation of ages, Dr. Buckland invokes anew the aid of the old hypothesis of a great *wave*. This wave, rolling from the north, must have dashed over the mountains to a height of near 2000 feet, depositing as it went gravel, boulders and fragments, derived from places 200 miles distant, and transporting also marine shells in its passage. But is it not more natural and accordant with all the data upon which our science has been reared, to suppose that when such shells were deposited, the parts of the mountains so affected were permanently beneath the sea, than to call into play the assumption of the passage of so mighty a wave? At one moment the argument used is, that scratches and polishings of rock must have been done by ice, because in existing nature it has been found that ice can produce such effects; and in the same breath we are told that beds of shells have been placed on a mountain by an agency which is truly supernatural.

In fact, the "glacier" theory, as *extended* by its author in proving too much, may be said to destroy itself. Let it be limited to such effects as are fairly deducible from the Alpine phænomena so clearly described by Agassiz, and we must all admire in it a *vera causa* of exceeding interest; but once pass the bounds of legitimate induction from that *vera causa*, and try to force the many and highly diversified superficial phænomena of the surface of the globe, into direct agreement with evidences of the action of ice under the atmosphere, and you will be driven forward, like the ingenious author of the theory, so to apply it to vast tracts of the globe, as in the end to conduct you to the belief, that not only both Northern and Southern hemispheres, but even *quasi* tropical regions, were shut up during a long period in an icy mantle. Once grant to Agassiz that his deepest valleys of Switzerland, such as the enormous chasm of the lake of Geneva, were formerly filled with solid snow and ice, and I see no stopping-place. From that hypothesis you may proceed to fill the Baltic and Northern Seas, cover Southern England, and half of Germany and Russia with similar icy sheets, on the surfaces of which all the northern boulders might have been shot off. But even were such hypotheses granted, without we also build up former mountains of infinitely greater altitude than any which now exist, we have no adequate centres for the construction of enormous glaciers which imagination must create in many regions to account for the phænomena. The very idea which records the existence of these vast former sheets of ice is at variance with all that is most valuable in the works of Charpentier, Venetz, and Agassiz, whose data, as carefully eliminated from Alpine phænomena alone, would naturally teach us never to extend their application when those conditions are absent, viz. the mountain chain, by the very presence of which the phænomena are explained.

But though the Alpine glacial theory be new, the scratches and polished surfaces of rocks are by no means of recent observation. Many Swedish miners, from the days of Tilas and Bergman, failed not to remark how their mountain sides were furrowed, and in our own times, Sefström* of Sweden, and Böhtlingk of Russia, have not only narrowly traced them over wide regions, but have endeavoured to account for them. The first of these authors remarked that nearly all the hard rocks of this country had a "worn or weather side," and a highly escarped or "lee side," the former being exposed to the North and the latter to the South; and having further shown that the detritus had generally been carried from N. to S., he called the worn face the "weather side," and the higher and jagged extremity of such ridges the "lee side." Extending his observations to many hundred places, he divided these scratches into what he calls normal and side furrows, showing that in the latter there are frequent aberrations from the persistent courses of the former. Although he had

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* See Taylor's Scientific Memoirs, vol. iii. p. 81.

been at first disposed to think, from the data in a given country around Falun, that the normal lines were invariably from N. to S., he afterwards discovered that in large tracts of the South of Sweden the direction was from N.W. to S.E., and in others, particularly along the coasts of Norway, from N.E. to S.W.; all these facts being recorded on a map, which is a most valuable document.

Since Sefström's work was published, M. Böhtlingk, a young Russian naturalist of great promise, but, alas! prematurely carried to the grave, extended his researches to the northern territories of Russia. Observing that the dominant direction of the scratches in parts of the governments of Olonetz and Archangel was from N. to S., and that along the edges of the Bothnian Gulf their course was from W. to E., he passed the summit level of Russian Lapland, and found that there the drift had no longer been transported from N. to S., or from N.W. to S.E., but, on the contrary, from S.E. to N.W.; or, in other words, that the blocks of Lapland had been carried northwards into the shores of the Polar Sea. In a recent letter to Mr. Lyell, read before this Society, Professor Nordenskiöld has accurately recorded the phænomena of this class observed by him in Finland, and he shows that there the blocks and striæ proceed from N.N.W. to S.S.E.

The theory of Sefström and his followers is, that a great flood, transporting gravel, sand and boulders, was impelled from the north over pre-existing land, and that the deviations from the N. and S. direction are due only to various promontories by which the flood was deflected. So convinced was this author that with local aberrations all the transport throughout the whole of Europe had taken place from north to south, that he not only travelled over the whole of Germany and saw nothing except materials streaming in the same direction, but even carried with him his northern drift into the Austrian and Bavarian Alps. I will not waste your time by pointing out the errors into which his hypothesis, though founded on data good within a limited radius, led this author. Every one who has studied the Alps (and the facts were well known before the days of glacial theories), is perfectly aware that the detritus on their flanks has been shot off eccentrically from the higher central masses. The observations indeed of Böhtlingk give the same result upon a very grand scale in the North, and explain what Sefström, with all his valuable labour, had left unknown, viz. that the Scandinavian mountains, as a whole, had produced exactly the same detrital result as the Alps, having poured off their detritus in all directions *from a common centre*, the northern chain differing only from that of central Europe by the much wider range to which its blocks and boulders were transmitted.

My own belief, Gentlemen, as you know, has been, that by far the greatest quantity of boulders, gravel, and clay distributed over our plains and occupying the sides of our estuaries and river banks was accumulated *beneath the waters* of former days. Throughout large tracts of England we can demonstrate this to have been the case by the collocation of marine shells of existing species with far-transported materials. It was the association of these testacea with foreign blocks in the central counties of England which first led me to attach a new and substantial value to that view of glacial action which had been so well advocated by Mr. Lyell before Professor Agassiz came forward with his great terrestrial and general theory. I am bound to say that wide researches during the last two years have strongly confirmed my early views*. I could not travel in the autumn of the year 1840 around the shores of the highlands of Scotland, without being convinced that the terrace upon terrace, presented on the sides of some of the great valleys, and often high upon the sea-ward hills of the bays opening out to the ocean, were nothing more than the bottoms of former seas and estuaries which had been successively desiccated.

I coincide, therefore, entirely with Mr. C. Darwin in his very ingenious explanation of the probable formation of the parallel roads of Glen Roy (Phil. Trans., 1839, p. 39). Since then that excellent observer has borne out similar views in a paper read before our own Society. In this memoir, estimating the different changes of the sea and land, and showing to what extent the solid strata were depressed, whose relative histories he thus reads off, he traces the shingle beds from the edge of the sea, where they are in process of formation, to considerable heights inland; and estimating how blocks were transported from the great Cordillera within, or not long before the period of existing sea shells, he explains the far-transported boulders by their being carried to the spots where they lie in vessels of ice. The melting of these icebergs he conceives to have been the chief agent in forming such masses of clay, gravel, and boulders, as constitute the "till" of Scotland, whilst the confusion and contortion of their imperfect strata is considered by him to be necessarily due to the grounding of icebergs in the manner formerly suggested by Mr. Lyell. To the same powerfully disturbing agent he attributes the general absence of organic remains in these deposits; and, lastly, he infers that it is much more probable that the great boulders were transported in icebergs detached from glaciers on the coast, than imbedded in masses of ice produced by the freezing of the sea.

M. de Verneuil and myself had previously brought before you some new results, arising from our first expedition to Russia. We endeavoured to show the utter inapplicability of the Alpine glacial theory to vast regions of Northern Russia, though the surfaces of the rocks are scored and polished, and far-travelled blocks occur throughout a wide area in isolated groups, because much of this detritus has travelled over extensive tracts of low country, from which it has ascended to levels higher than the sources of its origin. Hence we inferred, that the onward persistent march (in many parts up-hill) of a body of glaciers, having a front of many hundred miles in extent, is irreconcileable with any imaginable subaërial action. On the other hand, it was proved, by the presence of

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* See Silurian System, p. 536.

sea shells of an arctic character, that the "terra firma" to which some of the blocks had been transported, had been the bed of the Northern or glacial Sea at the period of this transport. We then attempted to explain how the parallel striæ and polishing of the surface of rocks of unequal altitudes was reconcileable with the *submarine* action of ice, by supposing that ice floes and their detritus might be set in motion by the elevation of the Scandinavian continent, and the consequent breaking up of great glaciers on the northern shores of a sea which then covered all the flat regions of Russia; and we further stated our belief, that the bottoms of these icebergs, extending to great depths, must have every here and there stranded upon the highest and most uneven points of the bottom of the sea into which they floated; that where the bottom was hard rock, the lower surface of the iceberg, like the lower surface of a glacier, would grate along and score and polish the subjacent mass; that where the bottom consisted of tenacious mud or clay, the iceberg once fairly stranded would be retained till it melted away, entirely or in part, whilst it would be more frequently borne over sand-banks, on account of their less resistance. In this manner, we endeavoured to explain not only the scratches and polish of hard submarine rocks, but also why large blocks are often found on former submarine hills, and why (in Russia at least) such blocks are more frequently associated with clay than sand. These views were indeed first expressed at the Glasgow meeting of the British Association, when I strove to reduce a large portion of the Alpine glacial theory to considerations depending upon the fact, that during the æra of the dispersion of the large blocks, by far the greater portion of our continents were *beneath the sea.*

Mr. Maclaren, to whom I have already adverted, has recently improved this view, by showing how the parallel scratches and grooves ranging from N.N.W. to S.S.E., and the dispersion of blocks in that direction, are reconcileable with the union of currents from the N., set in action, as above supposed, by a great polar elevation which acted as a "centre of dispersion;" but, as this author adds, a broad current would also set continually *eastward* along the immersed regions included in the temperate zone; and hence, he says, that when the icebergs were drifting southwards from the poles, they would naturally be carried to the S.E. by a stream compounded of the two currents. After reasoning upon the wide application to which the view of floating iceberg action is capable, and how many of our present terrestrial appearances it will explain, Mr. Maclaren adds, "Mr. Murchison's hypothesis, if adopted, does not exclude that of Agassiz. On the contrary, it may be assumed, that while the glacial condition (which caused the great accumulation of ice in the northern regions) continued, every mountain chain, which *then* had an elevation of 2000 or 3000 feet above the sea, would be encrusted with ice, perhaps as far south as the latitude of 40°. Each of these would be on a small scale what the polar nucleus was on a great scale, a centre of dispersion."

In the memoir upon Russia by M. de Verneuil and myself, one observation, however, occurs which has not found its way into the abstracts, and which, therefore, I may advert to, as explaining why the rough detritus of mud, sand, clay and boulders so very seldom contains marine remains. Such heaps are made up of materials which we consider to have been imbedded in a true terrestrial glacier, and therefore, though detached, and floated to a distance, they never could afford more than *terrestrial* detritus; and if to this be added the consideration of how the stranding of such masses would destroy animals in the vicinity, as suggested by Darwin, we may rationally conceive why so few shells have been discovered in this coarse detritus, whilst we readily perceive why the stones impacted in it should be scored and striated, and often polished.

Besides the great advancement of our knowledge of terrestrial magnetism, which at some future day may be connected with our labours, the Antarctic expedition, under the distinguished navigator Captain James Ross, has, as might have been expected, thrown considerable light upon the glacial theory[*]. A few years only have passed since the existence of an enormous mass of ice-clad land in the antarctic region, was announced by an American squadron of geographical research. This great icy tract, which was described as exhibiting hills and valleys, and even rocks upon its surface, has entirely disappeared in the short intervening time; for Captain Ross has sailed completely through the parallels of latitude and in the same longitude which it was said to occupy. As we cannot suppose that the American navigators were deceived by atmospheric phænomena, so must we believe that what they took for solid land, was one of the enormous accumulations of ice called "packs," the great source of those numerous ice islands, which periodically encumber the Southern Seas.

Continuing his progress towards the South Pole in almost open sea, Captain J. Ross discovered, as he proudly says, "for the honour of England," the southernmost known land, which he named Victoria, and which he coasted for more than eight degrees of latitude. This land rises in lofty mountain peaks, from 9000 to 12,000 feet in height, perfectly covered with eternal snow, from which glaciers descend, and project many miles into the ocean, terminating in perpendicular lofty cliffs. The rocks which could be examined were of igneous origin, and near the extreme south point of his exploration, or in S. lat. 77° 32′, long. 167° E., a magnificent volcano was seen in full action, emitting flame and smoke at an altitude of 12,400 feet. Further progress to the southward was then impeded by an enormous barrier of ice, or glaciers 150 feet high, which stretched from W.N.W. to E.S.E., and which the bold seaman traced in continuity for 300 miles, to long. E. 191° 23′, and lat. S. 78°. That this barrier was a true glacier was inferred from the existence of a very lofty chain of mountains behind it, the tops of which, as seen from the mast-heads, were estimated to be a degree of latitude to the south of the sea-face of this great wall of ice, at not more

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[* Capt. Ross's report of the discoveries made by the Antarctic Expedition will be found in Phil. Mag., Third series, vol. xx. p. 141.]

than half a mile from which the soundings were at 318 fathoms deep, and upon a bed of blue soft mud. Here then the geologist is presented with abundant matter for speculation. Volcanos in the midst of eternal polar snow and glaciers, with seaward faces as wide as some of the continental tracts, which, from the striæ and polish on their surface, and the wide dispersion of blocks and detritus, are supposed to have been affected by former terrestrial glacial action. Whilst, however, we have here the proof that existing glaciers advance some few miles into the sea, we are also informed that the ice ceases suddenly against an ocean 2000 feet deep, and thus we are led to conclude that many glaciers, which may formerly have extended themselves into the sea, had a length, the extent of which, whether like this antarctic example, or those which have been measured in the Alps, was proportioned to the altitude of the ancient mountains against which they rested. By the same reasoning we may infer that the striæ and polish of rocks, or accumulation of coarse detritus, and large blocks which are only to be observed in places far beyond the limits that are now established between mountains and their dependent masses of ice, cannot be due to the advance of former solid glaciers, but must rather be referred, as I have argued, to the floating away of vast packs and icebergs liberated from *centres of congelation.*

But besides the submarine operations now in action, and which may serve to explain most of our ancient phænomena, it has been shown that in Russia and other cold countries there are several actual subaërial processes, by which large blocks are accumulated at different heights by the expansion of the ice of rivers, or have been piled up by the glacial action of former lakes, when at much higher levels [*], leaving lines of coarse angular blocks.

I desist, however, in this place from entering further into the many features under which the existing agency of ice may be viewed apart from the results of the movements of glaciers. More than enough has indeed already been said: for so long as the greater number of practical geologists of Europe are opposed to the wide extension of a terrestrial glacial theory, there can be little risk that such doctrine should take too deep a hold of the mind. But whilst we may have no fear of this sort in Europe, I have lately read with regret certain passages in the Anniversary Discourse of Professor Hitchcock of the United States. In North America, striated, scored, and polished surfaces of rocks, proceeding from N. to S. for vast distances, occupy, it appears, at intervals a breadth of 2000 miles, and are seen on hard rocks at all levels from the sea-shore to heights of 3000 and 4000 feet. Professor Hitchcock tells us, that these phænomena and the accumulations of gravel and blocks had always been inexplicable, until the work of Agassiz unexpectedly threw a flood of light upon his mind [†]. If Professor Hitchcock could de-

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[*] Geological Proceedings, Murchison and De Verneuil on Russia, vol. iii. p. 406. [Phil. Mag., vol. xix., p. 489.]

[†] Anniversary Address. Philadelphia, April 1841, p. 24. I must be excused for stating that Professor Hitchcock has entirely misconceived my

monstrate what he now seems to believe, that the great mass of the continent of North America was formerly covered with ice, he must first prove that it was not at that period below the level of the sea; but as yet no facts are before us to lead us to doubt that the great accumulation of detritus and the transport of blocks did take place beneath the waters in that country. In justice, however, to this author, it must be said, that in expounding the glacial theory he ingenuously acknowledges the great difficulty of believing that solid masses of ice 3000 to 4000 feet thick, covered the whole region; that no action of a glacier will explain the persistent striation of the surface of an entire *continent* from N. to S., and that the direction of the boulders and the striæ is to a great extent up-hill. When these and many other difficulties shall have been carefully weighed, our transatlantic friends may be disposed to modify their views, particularly when they find that the existence of glaciers in Scotland and England (I mean in the Alpine sense) are not yet, at all events, established to the satisfaction of what I believe to be by far the greater number of British geologists.

The presence of Mr. Lyell at this time in North America, is indeed, most opportune, for whatever changes his mind may have recently undergone, no geologist has more strenuously laboured to make himself master of all its bearings, or more systematically enlarged our knowledge of this disputed subject. Possessing as he now does the advantage of observation on a vast scale, I have little doubt that he will account for the wide dispersion of blocks in America from N. to S. by referring to a cause quite as general and quite as aqueous as that by which he originally sought to explain the phænomenon in Europe*.

Although the consideration of this subject has already carried me beyond the limits I had prescribed to myself, yet I cannot quit it without reminding you, that the greatest geological authorities on the continent, led on by Von Buch who has so long studied these phænomena in his native land, are opponents to the views of Agassiz. Even whilst I write, I find that M. de Beaumont has just communicated to the Institute of France, a report on the results of a journey through Lapland, Finland, and the north of Europe, by his countryman M. Durocher, in which grouping the facts with great perspicuity, he handles the whole subject with his usual master's hand, and points out the value of the previous observations of Von Buch, Brongniart, and other writers. M. Durocher conceives that the phænomenon of the transport of erratic matters has proceeded from two successive and distinct operations: the first a great current from the pole, to which the striæ and polish of rocks, and the deposits

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views, when he places my name among those who had espoused the Alpine glacial theory. My efforts have been invariably directed towards its limitation, nay, to its entire rejection as applicable to by far the largest portions of the surface of the globe.

* See Principles of Geology, 2nd ed. vol. i. p. 342; and Elements of Geology, 1st ed. p. 136.

called Osars are referred; the second, the transport of the distant blocks by vessels of ice, when all that part of Europe which they cover was subjected to the immersion of an icy sea. He does not agree with M. Böhtlingk, that the point of departure of the current can be placed in Lapland, but supposes it to have proceeded directly athwart those regions from the pole*. But the point to which I now specially advert is, that in his skilful analysis of this memoir our eminent foreign associate admits floating ice as a *vera causa* to explain the drift of blocks, just in the same manner as in common with Lyell, Darwin, and others, I have been endeavouring to explain the phænomenon during the last three years, and thus the inference which was drawn from plain facts is admitted, viz. that the chief tracts covered by erratic blocks were *under the sea* at the period of their dispersion. (Sil. Syst. p. 536.)

Thus far had I written, Gentlemen,—in short I had, as I thought, exhausted the glacial subject at all events for this year,—when two most important documents were put into my hands. The first of these is the discourse of my predecessor, who has so modified his first views, that I cannot but heartily congratulate the Society on the results at which he has now arrived. I rejoice in the prudence of my friend, who has not permitted the arguments of the able advocate to appear as the sober judgment of so distinguished a President of the Geological Society†. In fact, it is now plain that Dr.

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* M. Durocher has made two valuable observations in showing us that the striated and polished surface of the hard rocks is sometimes covered by accumulations of sand and detritus; and that although proceeding in a general sense from the north, the furthest transported blocks are so distributed as to indicate *radiation* from certain mineralogical centres, much in the same way as our blocks of Shap granite have, on a less scale, been scattered from one point of distribution. In stating, however, that in the progress of these transported masses to the south, granitic blocks always constitute the outermost zone, it appears to me that M. Durocher has generalized beyond the field of his own observation. In Russia, for example, M. de Verneuil and myself traced greenstone blocks to the same southerly latitudes as granites. The blocks between Jurievitz and Nijny Novogorod are composed of quartz rock and of the peculiar trappæan breccia known in Russia as "Solomenskoi-kamen," the parent rocks of which we examined *in situ* near Petrazowodsk (Geol. Proceedings, vol. iii. p. 405), [Phil. Mag., Third series, vol. xix., p. 497], whilst the extreme boundary of these boulders extends to Garbàtof on the Okka, S.W. of Nijny Novogorod, and consequently very far beyond Kostroma, the limit assigned to them by M. Durocher. Again, if M. Durocher prolongs the northern drift to the flanks of the Ural Mountains he is decidedly in error, for there is no coarse detritus whatever on the flanks of that chain, whether derived from the north or from itself. Of the *Tchornoi-Zem*, or black earth of the central regions of Russia, to which, quoting Baron A. de Meyendorf, M. de Beaumont refers in a long note, I will now only say, that having studied the nature and extent of this singular deposit over very wide regions, I intend, with the help of my fellow-travellers M. de Verneuil and Count Keyserling, to lay before the public very shortly a sketch of its relations to the northern drift and other superficial deposits of Europe.

[† The portion of Dr. Buckland's Address which relates to this subject will be found at p. 515 of the present volume.]

Buckland abandons, to a great extent, the theory of Agassiz, and admits fully the effects of water as well as of ice, to account for many of the long-disputed phænomena. Whilst this admission involves the concession for which we have been contending, viz. that the great surfaces of our continents were *immersed*, and not above the waters when by far the greater number of the phænomena on the surface of rocks was produced, I reject for those who entertain the same opinions as myself, the simple division into "glacialists" and "diluvialists," into which Dr. Buckland has divided the combatants on this question; for to whatever extent the former title has been won by Agassiz and himself, we who have contended for the submarine action of ice in former times, analogous to that which we believe is going on at present, can never be merged with those who, under the name of diluvialists, have contended for the rush of mighty waves and waters over continents. Besides glacialists and diluvialists, my friend must therefore permit me to call for a third class, the designation of which I leave to him, in which some of us desire to be enrolled who have advocated that modified view to which the general opinion is now tending.

The other point to which I allude, and bearing at once on this view, is a discovery which our Librarian has just made without quitting the apartments which he so truly adorns. In the American Journal of Science for the year 1826, Mr. Lonsdale has detected a short, clear, and modest statement, entitled "Remarks on Boulders, by Peter Dobson," which, though little more than one page in length, contains the essence of the modified glacial theory at which we have arrived after so much debate. First describing in a few lines the manner in which large boulders, weighing from ten cwt. to fifteen tons, were dug out in clay and gravel, when making the foundations for his own cotton factory at Vernon, and seeing that it was not uncommon to find them worn, abraded, and scratched on the lower side, "*as if done* (to use his own expression) *by their having been dragged over rocks and gravelly earth in one steady position*," he adds this most remarkable sentence:—"*I think we cannot account for these appearances, unless we call in the aid of ice as well as water, and that they have been worn by being suspended and carried in ice over rocks and earth under water.*" To show also that he had read much and thought deeply on this subject, Mr. Dobson quotes British authorities to prove, that as ice-floes constantly carry huge masses of stone, and deposit them at great distances from their original situation, so may they explain the transportation of foreign boulders to our continents.

Apologising therefore for having detained you long, and for having previously too much extended a similar mode of reasoning, I take leave of the glacial theory in congratulating American science in having possessed the original author of the best glacial theory, though his name had escaped notice; and in recommending to you the terse argument of Peter Dobson, a previous acquaintance with which might have saved volumes of disputation on both sides of the Atlantic.

In the mean time, however we may attempt to account for the transport of boulders, the striation and polish of rocks, and the accumulation of superficial detritus, we cannot quit the glacial subject without avowing our obligations to Venetz, Charpentier, and Agassiz, and above all to the last, for having brought the agency of ice more directly into consideration as a *vera causa*, to explain many phænomena on the surface. Even we who differ from Agassiz in his generalizations, and have not examined the Alps since the theory was propounded, should not hastily adopt opinions which may be modified after a study of the glaciers *in situ*. "Come and see" is the bold challenge of the Professor of Neuchatel to all who oppose him, and sanguine as to the correctness of his opinions, he is certain that many will be converted if they would but observe the phænomena on which his views are based. Truly we must acknowledge, that he was the first person who roused our attention to the effects produced by the bottom of an advancing glacier, and if geologists should eventually be led to believe, that certain parallel scratches and striæ on the rocks were in some instances due to glaciers moving *overland*, but in many other cases were produced by icebergs, we must remember that the fertile mind of Agassiz has afforded us the chief means of experimentally solving the problem.

In conclusion, Gentlemen, it is gratifying to reflect, that notwithstanding the vibrations of opinion which have been caused by the introduction of glacial action among geological dynamics, the fundamental principles of our science remain entirely unaffected. Conspicuous as it may appear through the attractive descriptions of Agassiz, or the eloquence of Buckland, the glacial theory must be considered an episode only in the records we are labouring to prepare of the grand changes of the planet. Let not, therefore, geology be decried as a science without fixed principles, because her cultivators have recently differed upon a point which, though connected in theory with the science, has no bearing whatever on its uses nor upon the many fundamental points which it had previously established.

Your labours, Gentlemen, and those of your foreign associates, have already afforded proofs of the regular succession of the strata, and have traced their chronology; you have accurately marked the revolutions which have interrupted the sequence of by-gone races; you have explained the origin and position of various mineral substances essential to mankind, the dependence of geographical and agricultural products upon geological laws, and have shown how antagonist forces proceeding from the interior have modified the earth's outline, and been the cause of mineral wealth,—in a word, by your patient study of the masses you have acquired a true knowledge of the structure of the surface of the globe.

By these achievements the geologist has earned his best trophies, and has shown that the principles of his science are based upon the unerring laws of nature. Let then the shortness of his bright career incite us to renewed exertions, so that if at the close of life our vast subject should still present some unexplained phænomena, we may at all events have won the race in our own generation by establishing new landmarks in the rapidly increasing delta of natural knowledge.

ROYAL IRISH ACADEMY.

[Continued from p. 440.]

Feb. 22, 1841.—The Secretary read the following "Collection of Notes on the early History of Science in Ireland." By James Orchard Halliwell, Esq., F.R.S., F.S.A., F.R.A.S., &c.

"The following scraps on a subject which has never yet been treated of by any writer with whose works I am acquainted, although unfolding no views of any great importance, will, it is believed, form a subject of discussion interesting to all natives of Ireland, who would think favourably of the intellectual character and resources of their countrymen.

"The earliest remnant of Irish science that I have met with, is contained in MS. Arundel, 333, in the British Museum, which contains several medical and astrological tracts in the Irish language of the thirteenth century, together with similar tracts of the fourteenth and fifteenth centuries. These tracts are of a similar nature with contemporary manuscripts written in England and on the continent. For instance, at fol. 27, is an extract translated from the treatise of the venerable Bede, *De Divisionibus Temporis*; at fol. 35, is a short tract on the months of the year and their several durations; at fol. 76, is a scrap on the four seasons of the year, and on the planets which govern them.

"The whole volume contains astrology, mixed with the sciences of medicine and astronomy. Medical manuscripts in Irish of this early period are more numerous than others; and the Egerton collection in the British Museum contains several; one dated in the year 1303, and written on the continent *.

"Some writers say, that Johannes de Sacro Bosco, the contemporary of Roger Bacon, and who shines so conspicuously in the history of the mathematical sciences of the thirteenth century, was a native of Ireland; but, whatever obscurity may hang over the actual place of his birth, it is certain that he resided nearly the whole of his life in England and France, and there is nothing to show that his writings were ever circulated in that country.

"Be this as it may, yet it appears from MS. Egerton, No. 90, that the Arabic numerals usually, though erroneously†, ascribed to Roger Bacon, were well known and understood in Ireland at the commencement of the fourteenth century. The document contained in this volume is very valuable evidence, in the absence of any other as early. The MS. referred to contains an astronomical and ecclesiastical calendar, together with a table of ecclesiastical computation, all in the Irish character, and the numerals are written in identically the same form as they appear in foreign documents of the same period‡.

"The introduction of the zero is a proof that the Arabic notation was fully understood by the writer of the manuscript. It may be added, that there follows, immediately after the documents just mentioned, a table of the twelve signs of the zodiac, with their different

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* MS. Egerton, No. 89.

† See my Rara Mathematica, p. 114.

[‡ These numerals are given in the Proceedings of the Academy, No. 28.]

astrological influences, viz. Aries = good; Taurus = evil; Gemini = evil; Leo = evil; Virgo = evil; Libra = good; Sagittarius = good; Capricornus = evil; Aquarius = good. The others are said to neutralize their influences.

"In the Philosophical Transactions of the Royal Society of London*, Dr. Ward has given an account of a date in Arabic numerals found on a stone in Ireland, which he considered to belong to the twelfth century. Professor Peacock, however, in his History of Arithmetic, has ably confuted this conjecture.

"The Liber Niger of Christ Church, Dublin, is said to contain 'a curious treatise on arithmetic, exhibiting the state of that science before the introduction of Arabic numerals †.' I much question the accuracy of this statement, and should be rather inclined to think that it is merely an account of the numbers of algorism, so common in manuscripts of this class. The same volume also contains a transcript of the French poetical treatise entitled *Imago Mundi*, one of the most curious unpublished scientific tracts of the middle ages. This latter treatise is now in the progress of publication, by the Historical Society of Science.

"But by far the most curious document that I have met with relating to the early science of Ireland, is a manuscript in the possession of C. Wright, Esq. of Cambridge, who has kindly allowed me to make use of it, and has also furnished me with a translation of the greater part, which has been of great assistance to me. This MS. consists of six folio leaves on vellum, slightly injured by damp, apparently belonging to the early part of the fifteenth century, and containing the following articles:—

"1. A brief treatise on arithmetic.

"This unfortunately commences imperfectly in the account of the rule of duplation; 'In duplation only one order of figures is necessary: in the three preceding kinds, we commenced from the right and from a smaller figure; but in this and the following kinds, we commence from the left and from a larger figure. For if you wish to double from the first figure, it happens that you must double it twice. And if you can in any other manner commence from the right-hand, the operation and construction will be much more difficult. If, therefore, you wish to double any number, that number must be written by its differences, and the last number must be doubled. From that duplation, therefore, either results a digit, an article, or a composite. If a digit, it must be written in the place of the other blotted out. If an article, a 0 must be written in the place of the other blotted out, and the article must be removed towards the left-hand. If a composite number, the digit which is a part of that composite must be written in the place of the other blotted out, and the article be removed to the left-hand. This being done, the last figure must be doubled, and whatever thence arises must be dealt with as before; but if a cipher turns up, it must be left untouched. We prove duplation by means of mediation.'

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* For the year 1745, p. 283.

† Report on the Public Records of Ireland, p. 307.

"This extract will be sufficient to give an idea of the whole tract. After this rule, follow those of multiplication, division, and progression in their proper order. For the comprehension of the uninitiated in the old arithmetic, it may be necessary to state, that a digit is any number below ten, an article is ten, or any multiple of ten, and that all other numbers are composites, or composed of an article and some digit. My friend Mr. Wright gives it as his opinion, that this tract is a translation from the Latin or French.

"2. *Tractatus de Geometria.*

"This is an Irish tract with a Latin title, and consists of only one page, containing the simplest rules of geometrical measurement, applied to one example of finding the height of a tower. No mention occurs of any of the old geometers.

"3. A treatise on the signs of the zodiac.

"An astrological tract with very curious drawings of the various signs. Messabalah, the famous Arabic astronomer, is mentioned at the commencement, and this tract is very probably translated from one of that author's works.

"4. A treatise on the length of the days in the year.

"5. A fragment (one half page).

"This terminates the contents of this manuscript, and is written in Latin. It appears to relate to abacal arithmetic, but as I confess myself unable to understand its meaning, I give it here entire, in the hopes that some other may be more fortunate in attempting to decipher its meaning.

"'Intervalla autem in quibus distribuuntur. dicimus sedes horum numerorum. qui in abaci regula secundum geometricam habitudinem sic proportionaliter ordinati continentur. ut juxta numerum novem caracterum nonis termis alternati distinctis terminis. secundum propor. * * *'

"I have pointed this exactly as in the original manuscript, but the fragment appears to be altogether unconnected.

"In addition to the above, I may mention, that in the library of Trinity College, Cambridge, under the press mark R. xiv. 48, is preserved a short poem in the Irish language on astronomy, of the early part of the thirteenth century*. And in the Bodleian Library, MS. Rawlinson, B. 490, is a translation of the *Secreta Secretorum* of Aristotle, by James Yonge, on vellum, of the early part of the fifteenth century. This work of Young is not mentioned by Sir James Ware, nor does it appear to be at all known to Irish writers. It is almost unnecessary to observe, that this latter work has no relation with science, but its rarity is a sufficient excuse for mentioning it here.

"It will now be necessary to pass over nearly two centuries before we meet with any traces of scientific progress. Some time about the year 1600, William Farmer, 'Chirurgian and Practitioner in the Mathematicall Artes,' dwelt at Dublin; and among the manuscripts of Archbishop Tenison, at Lambeth Palace, No. 816, is

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* This I learn from Mr. Wright. In the printed catalogue it is said to be in Saxon characters.

an autograph MS. by him, entitled 'A Prognosticall Almanack for this Bissextile yere, 1612, composed with a three fould Kallender generally calculated for this Kingdom of Ireland, and will also serve very well for alle the Northe and Northweste partes of England.' William Bourne also, who flourished at the same time, and greatly distinguished himself by his mechanical inventions, was a native of Ireland. To these two we may add Nathaniel Carpenter, an Englishman by birth, but who resided in Dublin early in the seventeenth century, and left behind him treatises on geography and optics. A copy of this latter work is still preserved in MS. in the Library of University College, Oxford*.

"With Molyneux, in more recent times, the science of Ireland rose to a level with that of surrounding nations, and the names Ponce, Boyle, Petty, and Ashe†, serve to fill the complement of the seventeenth century. In January 1684, Molyneux suceeded in forming a Philosophical Society at Dublin, on the plan of the Royal Society of London. The first meeting of the Society took place on the 28th of January 1684, when Sir William Petty was chosen President, Dr. Charles Willoughby Director, and Molyneux undertook the combined offices of Secretary and Treasurer. November 1st, All Saints' day, was chosen for the anniversary of the Society. On the 1st of November, 1684, Sir William Petty was re-elected President, Molyneux as Secretary, and William Pleydell, Esq., Treasurer. On the 2nd of November, 1685, Lord Viscount Mountjoy was elected President, George Tollet, Esq., Treasurer, and St. George Ashe Secretary. In this year Molyneux retired from actual office, but retained his place on the Council of the Society. On the 1st of November, 1686, Lord Viscount Mountjoy was re-elected President, George Tollet, Esq., Treasurer, and Edward Smyth, Secretary.

"The preceding particulars are taken from the original Minute-book of the Society preserved in the British Museum, MS. Addit. 4811‡. The last entry in this book is the account of the General Meeting of 1686, and this would lead us to suppose that the Society was dissolved at this period, although Dr. Hutton assures us, that it was not broken up till 1688§.

"From MS. Addit. 4812, it appears that in the year 1707, an attempt was made to re-establish the Society, but its success was not of any long duration, and this MS. contains a register of the philosophical papers read before the Society, from August 15th, 1707, to March 11th, 1708. The Earl of Pembroke, then Lord Lieutenant of Ireland, presided over the Society at this revival.

"In 1686, Molyneux printed at Dublin his *Sciothericum Telescopium*, containing a description of the structure and use of a telescopic dial invented by him. In the British Museum is preserved the author's own copy of this volume, enriched with numerous MS. notes

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* Under the press mark L. 14. See Bernard's Catalogue, 1697, p. 5.

† Archbishop Usher was the author of some treatises on sciences and their history, more especially astronomy.

‡ The same volume likewise contains copies of numerous letters and papers on scientific subjects, addressed for the most part to Molyneux.

§ Mathematical Dictionary, vol. ii. p. 117.

and observations, and what is particularly worthy of being noticed, an analysis of its history."

March 16, 1841 (Stated Meeting).—Mr. J. M. Ferrall drew the attention of the Academy to several drawings, and a preparation, exhibiting a new and beautiful mechanism belonging to the human eye, and discovered by him in April last, while engaged in researches on certain diseases of the orbit, which the received anatomy of those parts did not appear to him to explain.

The new structures consisted of a distinct fibrous tunic, investing the globe of the eye, facilitating its movements, and separating it from all the surrounding tissues.

The anatomy of the schools, and of the best authors, from the earliest time to the present, teaches that the ball of the eye is in contact with its muscles, and, between them, with a quantity of adipose substance on which it was supposed to be cushioned. It was difficult to conceive, however, why the eye did not manifest any of the symptoms incidental to pressure suddenly endured, whenever those muscles were brought into action, since there appeared to be no provision for its protection. That pressure, suddenly made on the globe of the eye, produces the sensation of a spark or flash of light, is familiarly known as the consequence of a slight blow on the eye.

The act of sneezing is frequently followed by a similar phænomenon, and Sir Charles Bell has shown, in a paper published in the Philosophical Transactions, that it is occasioned by the sudden pressure on the ball of the eye, by the orbicularis palpebrarum or principal muscle of the eyelids, which is suddenly brought into action by the respiratory nerves. The four recti muscles, which move the eye in different directions, being favourably placed (according to the received anatomy) for exercising such a pressure, it might have been expected that a similar phænomenon would have resulted; but no such coruscations have ever been observed to follow their most rapid actions.

The discovery of this tunic, which Mr. Ferrall has ventured to term the TUNICA VAGINALIS OCULI, at once explains the absence of those phænomena, by showing that a protective provision has always existed to prevent them.

Mr. Ferrall went on to state, that the most beautiful portion of this mechanism remained to be described. It was one of those exquisite manifestations of design which abound in the animal frame.

In the anterior portion of this tunic were to be found six different well-defined openings, through which the tendons of the muscles pass to their insertion in the sclerotic coat of the eye, and over which they play as over pulleys in their progress.

Wood engravings, executed from original drawings made in April 1840, for Mr. Ferrall's clinical lectures in St. Vincent's Hospital, and displaying this conformation faithfully, are given in No. 28 of the Proceedings of the Academy.

The first shows the tendon of the internal rectus emerging from behind the tunic, and passing through its pulley to be inserted in the eyeball.

The second figure represents the eyeball drawn downwards, in order to expose the exit of the tendons of the superior rectus and superior oblique muscles; the superior rectus plays over its pulley, and the superior oblique passes beneath the former to reach its insertion in the sclerotic coat.

The presence of some such contrivance as is here exhibited might have been inferred from its necessity, and yet it has never been suspected to exist. The four recti muscles running from the bottom or apex of the orbit, forward to grasp the eye, must, without it, have had the power of retracting the ball of the eye, and yet no such retraction has ever been observed in the human eye. Retraction is certainly performed in some of the lower classes of animals; but *they* are provided with a strong retractor muscle, independent of the four recti muscles. Again, the rotatory movements of the human eye, which enlarge the sphere of vision, and contribute to expression, are not easily accounted for by the received anatomy of the orbit, because the course of the muscles from the bottom of the orbit forwards, manifestly gives them a power of retracting rather than of rotating the eye upon its centre. Thus, then, there appeared to be no provision for the rotatory movements of the ball of the eye, which are of *constant occurrence*, and nothing to prevent retraction, which we knew *did not take place*. A knowledge of the existence of this new and beautiful mechanism reconciles and explains these anatomical and physiological contradictions.

Mr. Ferrall said, he had found these structures in several of the lower animals, in whom they appear to enable the recti to antagonize the proper retractor muscles.

Several phænomena in diseases of those parts, formerly obscure, are now easily understood; but Mr. Ferrall refrained, on this occasion, from discussing questions of a practical nature.

The following gentlemen were elected Officers and Council for the year 1841–1842:—

President.—Sir Wm. Rowan Hamilton, LL.D. *Committee of Science.*—Rev. Franc Sadleir, D.D., Provost; Rev. Humphrey Lloyd, D.D.; James Apjohn, M.D.; James MacCullagh, Esq., LL.D.; Rev. William D. Sadleir, A.M.; Robert Ball, Esq.; Robert Kane, M.D. *Committee of Polite Literature.*—His Grace the Archbishop of Dublin; Rev. Joseph H. Singer, D.D.; Samuel Litton, M.D.; Rev. William H. Drummond, D.D.; Rev. Charles R. Elrington, D.D.; Rev. Charles W. Wall, D.D.; Rev. Thomas H. Porter, D.D. *Committee of Antiquities.*—Thomas H. Orpen, M.D.; George Petrie, Esq., R.H.A.; Rev. Cæsar Otway; Rev. James H. Todd, D.D.; Henry J. Monk Mason, Esq., LL.D.; Aquila Smith, M.D.; Samuel Ferguson, Esq. *Officers.*—*Treasurer*: Thomas H. Orpen, M.D. *Secretary to the Academy*: Rev. Joseph H. Singer, D.D. *Secretary to the Council*: J. MacCullagh, Esq., LL.D. *Secretary of Foreign Correspondence*: Rev. Humphrey Lloyd, D.D. *Librarian*: Rev. William H. Drummond, D.D. *Clerk and Assistant Librarian*: Edward Clibborn.

The President appointed the following Vice-Presidents:—

His Grace the Archbishop of Dublin; the Provost of Trinity College; the Rev. Humphrey Lloyd, D.D.; the Rev. J. H. Todd, D.D.

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END OF THE TWENTIETH VOLUME.

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