THE

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

CONDUCTED BY

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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.

VOL. XVIII.

NEW AND UNITED SERIES OF THE PHILOSOPHICAL MAGAZINE,
ANNALS OF PHILOSOPHY, AND JOURNAL OF SCIENCE.

JANUARY—JUNE, 1841.

LONDON:

RICHARD AND JOHN E. TAYLOR, RED LION COURT, FLEET STREET,
Printers and Publishers to the University of London;

SOLD BY LONGMAN, ORME, BROWN, GREEN, AND LONGMANS; CADELL;
SIMPKIN AND MARSHALL; S. HIGHLEY; WHITTAKER AND CO.; AND
SHERWOOD, GILBERT, AND PIPER, LONDON: — BY ADAM AND
CHARLES BLACK, AND THOMAS CLARK, EDINBURGH; SMITH
AND SON, GLASGOW; HODGES AND SMITH, DUBLIN:
AND G. W. M. REYNOLDS, PARIS.

CONTENTS OF VOL. XVIII.

NUMBER CXIV.—JANUARY, 1841.

NUMBER CXV.—FEBRUARY.

NUMBER CXVI.—MARCH.

NUMBER CXVII.—APRIL.

NUMBER CXVIII.—MAY.

NUMBER CXIX.—JUNE.

NUMBER CXX.—SUPPLEMENT TO VOL. XVIII.

PLATES.

I. and II. Illustrative of Mr. Redfield's Researches on the Phænomena of Storms and Tornadoes.

III. Illustrative of Mr. Kemp's and Mr. Bowman's Papers on the Evidence of the former existence of Glaciers in Scotland.

ERRATA.

P. 37, line 11 from the bottom, for *q* read *g*.
P. 65, line 17 from the bottom, *for* Mohammed Ghizni *read* Mohammed Ghori.
P. 247, line 44, *for* non-negative *read* now negative.

A correction in Mr. Airy's paper, p. 9, line 6, will be found at p. 132.

A List of Errata in Messrs. Francis and Croft's Notices of the Results of the Labours of Continental Chemists will be found at p. 546.

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[THIRD SERIES.]

JANUARY 1841.

I. *On the Diffraction of an Annular Aperture.* By George Biddell Airy, *Esq., M.A., F.R.S., Astronomer Royal.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I HAD no wish to make a communication to you which should assume the form of a discussion with Mr. Potter, and I proposed, therefore, in adverting to the subject to which Mr. Potter has alluded in your Number of October last, rather to add something to the investigation of a point which has perhaps been passed over too lightly by writers on the Undulatory Theory, than to employ myself specially in indicating what I consider to be the failing steps in Mr. Potter's reasoning. The calculations necessary for the first of these objects were troublesome, and (with the little leisure that I can command) occupied a considerable time. Having, however, finished these, I was preparing to address you when I saw Mr. Tovey's paper in your Number for December. I am most happy to find that the very clear explanation given by Mr. Tovey has delivered me from the necessity of remarking on the reasoning in Mr. Potter's paper, and I shall now proceed with my supplementary investigation as an attempt at the extension of the received theory.

Problem.—A plane wave, whose exterior boundary is circular, is interrupted in its course by a circular plate, whose plane is parallel to the front of the wave, and whose centre coincides with the centre of the wave-boundary: it is required to find the intensity of light at any point *near* the line which is normal to the plate at its centre.

Let a be the radius of the plate, b the radius of the boundary of the wave, h the distance of the point from the circular plate, c the distance of the point from the normal line. From the point draw a normal to the plate: the distance of the foot of this normal from the centre of the plate will evidently $= c$. Take the foot of the normal as the origin of polar coordinates for the front of the wave: suppose the whole front of the wave divided into truncated sectors by radii drawn from the foot of the normal: let the angles made by two of these with the line joining the centre of the plate with the foot of the normal be θ and $\theta + \delta\theta$: and let the small truncated sector included between these be divided into small parts by arcs of circles described with the origin as centre: let the radii of two of these arcs be r and $r + \delta r$. The area included between these two arcs is ultimately $= r\delta\theta \times \delta r = \delta\theta \times r\delta r$, and the distance of this area from the point in question is ultimately $= \sqrt{(h^2+r^2)}$. If then the amplitude of the vibration which it excites at the point in question be a function of its distance, but independent or nearly independent of the angle which the line of distance makes with the front of the wave, the amplitude of the vibration produced by the small area $\delta\theta \times r\delta r$ will be represented by

$$\delta\theta \times \phi(\sqrt{h^2+r^2}) \times r\delta r,$$

and the phase being $\frac{2\pi}{\lambda}\left\{vt - \sqrt{h^2+r^2}\right\}$, the absolute disturbance in the ether at the point in question, produced by the small area $\delta\theta \times r\delta r$, at the time t, will be represented by

$$\delta\theta \times \phi(\sqrt{h^2+r^2}) \times r\delta r \times \sin\frac{2\pi}{\lambda}\left\{vt - \sqrt{h^2+r^2}\right\}$$

$$= \sin\frac{2\pi vt}{\lambda} \times \delta\theta \times \phi(\sqrt{h^2+r^2}) \,.\, \cos\frac{2\pi}{\lambda}\sqrt{h^2+r^2} \,.\, r\delta r$$

$$- \cos\frac{2\pi vt}{\lambda} \times \delta\theta \times \phi(\sqrt{h^2+r^2}) \,.\, \sin\frac{2\pi}{\lambda}\sqrt{h^2+r^2} \,.\, r\delta r.$$

And these expressions are now to be integrated with respect to r; then the limits of r, defined by the intersection of the eccentric radius with the circumferences of the two circles (and therefore functions of θ) are to be substituted, and then the whole is to be integrated with respect to θ.

The expressions involving r cannot be integrated in a finite form so long as the general symbol is retained for the function. If, however, we expand it in the form

$$\phi\left\{h + (\sqrt{h^2+r^2} - h)\right\} = \phi(h) + \phi'(h)\frac{\sqrt{h^2+r^2} - h}{1}$$
$$+ \phi''(h)\frac{(\sqrt{h^2+r^2} - h)^2}{1 \,.\, 2} + \&c.$$

they may always be integrated.

We shall, however, at present confine our attention to the case where $\phi\,(\sqrt{h^2+r^2})$ may be represented by $\dfrac{1}{\sqrt{h^2+r^2}}$. The disturbance of ether produced by the small area $\delta\theta \times r\delta r$ at the point in question, is now represented by

$$\sin\frac{2\pi v t}{\lambda} \times \delta\theta \times \cos\frac{2\pi}{\lambda}\sqrt{h^2+r^2}\,.\,\frac{r\,\delta r}{\sqrt{h^2+r^2}}$$

$$-\cos\frac{2\pi v t}{\lambda} \times \delta\theta \times \sin\frac{2\pi}{\lambda}\sqrt{h^2+r^2}\;\frac{r\,\delta r}{\sqrt{z^2+r^2}},$$

the integral of which, with respect to r, is

$$\frac{\lambda}{2\pi}\,.\,\sin\frac{2\pi v t}{\lambda} \times \delta\theta \times \sin\frac{2\pi}{\lambda}\sqrt{h^2+r^2}$$

$$+\frac{\lambda}{2\pi}\,.\,\cos\frac{2\pi v t}{\lambda} \times \delta\theta \times \cos\frac{2\pi}{\lambda}\sqrt{h^2+r^2},$$

which, taken between the limits $r_{\prime}$ and $r_{\prime\prime}$, is

$$\frac{\lambda}{2\pi}\,.\,\sin\frac{2\pi v t}{\lambda}\,.\left(\sin\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime\prime}^2}-\sin\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime}^2}\right).\delta\theta$$

$$+\frac{\lambda}{2\pi}\,.\,\cos\frac{2\pi v t}{\lambda}\,.\left(\cos\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime\prime}^2}\right.$$

$$\left.-\cos\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime}^2}\right)\delta\theta.$$

We have now to find the values of $r_{\prime}$ and $r_{\prime\prime}$ in terms of θ. For this purpose we have only to remark that the equation of the smaller circle, referred to the foot of the normal as origin, and expressed by means of rectangular coordinates (x being measured through the centre of the circle), is

$$(x-c)^2+y^2=a^2,$$
$$\text{or } (r_{\prime}\cos\theta-c)^2+(r_{\prime}\sin\theta)^2=a^2,$$
$$\text{or } r_{\prime}^2-2c\cos\theta\,.\,r_{\prime}=a^2-c^2,$$

the positive solution of which is

$$r_{\prime}=c\,.\,\cos\theta+\sqrt{a^2-c^2\sin^2\theta};$$

or supposing c so small that the first power only is worth retaining,

$$r_{\prime}=a+c\cos\theta.$$

Therefore

$$\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime}^2}=\frac{2\pi}{\lambda}\sqrt{h^2+a^2}+\frac{2\pi\,.\,a\,c\,.\,\cos\theta}{\lambda\sqrt{h^2+a^2}}$$

to the first power of c.

In forming the sine of this quantity we cannot, as before, proceed by expansion in powers of c, neglecting all after the first; and the reason for this prohibition deserves attention, as showing what extreme care is necessary, in a process of approximation, to adopt no method at any one step without due consideration of the effect of rejected terms at that step. Suppose, for instance, the terms depending on higher powers of c in the expression for $\frac{2\pi}{\lambda}\sqrt{h^2 + r_{\prime}^2}$ were $\frac{1}{1000}$th part of that depending on the first power, and suppose that $\frac{2\pi . ac . \cos\theta}{\lambda . \sqrt{h^2 + a^2}}$ were nearly equal to 10π. The rejection of the terms depending on the higher powers would only produce in this angle an error of $\frac{\pi}{100}$, which would be wholly unimportant in these investigations. But the expansion of

$$\sin\left\{\frac{2\pi}{\lambda}\sqrt{h^2 + a^2} + \frac{2\pi . ac . \cos\theta}{\lambda\sqrt{h^2 + a^2}}\right\},$$

in a series proceeding by powers of c, would give a succession of rapidly diverging terms when $\frac{2\pi ac\cos\theta}{\lambda\sqrt{h^2 + a^2}}$ had any such value as 10π; and the expression produced by retaining the first and rejecting the higher powers would not in any degree represent the whole. From this point, therefore, we must use methods which do not imply the rejection of any powers of c.

For convenience, put

$$e_{\prime} \text{ for } \frac{2\pi ac}{\lambda\sqrt{h^2 + a^2}}, \text{ and } e_{\prime\prime} \text{ for } \frac{2\pi bc}{\lambda\sqrt{h^2 + b^2}}.$$

Then

$$\frac{2\pi}{\lambda}\sqrt{h^2 + r_{\prime}^2} = \frac{2\pi}{\lambda}\sqrt{h^2 + a^2} + e_{\prime}\cos\theta,$$

and

$$\sin\frac{2\pi}{\lambda}\sqrt{h^2 + r_{\prime}^2} = \sin\frac{2\pi}{\lambda}\sqrt{h^2 + a^2} . \cos(e_{\prime}\cos\theta)$$

$$+ \cos\frac{2\pi}{\lambda}\sqrt{h^2 + a^2} . \sin(e_{\prime}\cos\theta).$$

Similarly,

$$\sin\frac{2\pi}{\lambda}\sqrt{h^2 + r_{\prime\prime}^2} = \sin\frac{2\pi}{\lambda}\sqrt{h^2 + b^2} . \cos(e_{\prime\prime}\cos$$

$$+ \cos\frac{2\pi}{\lambda}\sqrt{h^2 + b^2} . \sin(e_{\prime\prime}\cos\theta).$$

In like manner

$$\cos\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime}^2}=\cos\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\,\cos(e_{\prime}\cos\theta)$$

$$-\sin\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\,\sin(e_{\prime}\cos\theta)$$

$$\cos\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime\prime}^2}=\cos\frac{2\pi}{\lambda}\sqrt{h^2+b^2}\,.\,\cos(e_{\prime\prime}\cos\theta)$$

$$-\sin\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\,\sin(e_{\prime\prime}\cos\theta),$$

and these quantities are now to be substituted in the expression above, which multiplies $\delta\theta$, and the whole is then to be integrated with respect to θ.

If our object were to find the intensity of light produced by a portion of the annulus, these expressions could not be made more simple; but in finding the intensity produced by the whole annulus we may introduce an important simplification; for the expressions will then be integrated, with respect to θ, from $\theta = 0$ to $\theta = 2\pi$. Now it is evident that the expansion of $\sin(e_{\prime}\cos\theta)$ or $\sin(e_{\prime\prime}\cos\theta)$ will produce only odd powers of $\cos\theta$; and the integral of every one of these, from $\theta = 0$ to $\theta = 2\pi$, is zero. The terms depending on $\sin(e_{\prime}\cos\theta)$, $\sin(e_{\prime\prime}\cos\theta)$, may therefore be neglected at once; and thus we have, for this investigation,

$$\sin\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime}^2}=\sin\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\,\cos(e_{\prime}\cos\theta),$$

and so for the others; and the quantity which we have to integrate with respect to θ is

$$\frac{\lambda}{2\pi}\,.\,\sin\frac{2\pi vt}{\lambda}\,.\,\left(\sin\frac{2\pi}{\lambda}\sqrt{h^2+b^2}\,.\,\cos(e_{\prime\prime}\cos\theta)\right.$$

$$\left.-\sin\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\,\cos(e_{\prime}\cos\theta)\right)$$

$$+\frac{\lambda}{2\pi}\,.\,\cos\frac{2\pi vt}{\lambda}\,.\,\left(\cos\frac{2\pi}{\lambda}\sqrt{h^2+b^2}\,.\,\cos(e_{\prime\prime}\cos\theta)\right.$$

$$\left.-\cos\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\,\cos(e_{\prime}\cos\theta)\right).$$

Let

$$\int_{\theta}\cos(e\cos\theta)=2\pi\,\mathrm{E},\quad \int_{\theta}\cos(e_{\prime}\cos\theta)=2\pi\,\mathrm{E}_{\prime},$$

$$\int^{\theta}\cos(e_{\prime\prime}\cos\theta)=2\pi\,\mathrm{E}_{\prime\prime},$$

the integrals being taken from $\theta = 0$ to $\theta = 2\pi$. Then the coefficient of $\sin \frac{2\pi v t}{\lambda}$ is

$$\lambda \,.\left(\mathrm{E}_{\prime\prime} \,.\, \sin \frac{2}{\lambda} \sqrt{h^2 + b^2} - \mathrm{E}_{\prime} \,.\, \sin \frac{2}{\lambda} \sqrt{h^2 + a^2}\right) = \mathrm{S}.$$

The coefficient of $\cos \frac{2\pi v t}{\lambda}$ is

$$\lambda \,.\left(\mathrm{E}_{\prime\prime} \,.\, \cos \frac{2\pi}{\lambda} \sqrt{h^2 + b^2} - \mathrm{E}_{\prime} \,.\, \cos \frac{2\pi}{\lambda} \sqrt{h^2 + a^2}\right) = \mathrm{C}.$$

and if the two terms be aggregated into one of the form

$$\mathrm{P} \sin \left(\frac{2\pi v t}{\lambda} + \mathrm{Q}\right),$$

it is easily seen that P or the amplitude of vibration at the point in question is $\sqrt{\mathrm{S}^2 + \mathrm{C}^2}$; and if the intensity of light be assumed proportional to P^2, it may be represented by $\frac{\mathrm{S}^2 + \mathrm{C}^2}{\lambda^2}$, or

$$\mathrm{E}_{\prime}^2 + \mathrm{E}_{\prime\prime}^2 - 2\,\mathrm{E}_{\prime} \,.\, \mathrm{E}_{\prime\prime} \,.\, \cos \frac{2\pi}{\lambda}\left(\sqrt{h^2 + b^2} - \sqrt{h^2 + a^2}\right).$$

Our attention must now be directed to finding the numerical value of the function E for different values of e.

By the usual expansion,

$$\cos(e \cos\theta) = 1 - \frac{e^2 \cos^2\theta}{1.2} + \frac{e^4 \cos^4\theta}{1.2.3.4} - \frac{e^6 \cos^6\theta}{1.2.3.4.5.6} + \&c.,$$

a series which is always convergent, and may therefore be safely used for numerical computation.

Integrating separately each term, from $\theta = 0$ to $\theta = 2\pi$, and observing that if $\cos^{2n}\theta$ be expressed by simple powers of cosines of multiple arcs, the constant term is

$$\frac{1}{2^n} \; \frac{1.3.5.7 \ldots 2n-1}{1.2.3.4 \ldots n},$$

we obtain

$$\int_\theta \cos(e \cos\theta) = 2\pi \times \left\{1 - \frac{e^2}{(2)^2} + \frac{e^4}{(2.4)^2} - \frac{e^6}{(2.4.6)^2} + \&c.\right\}$$

and therefore

$$\mathrm{E} = 1 - \frac{e^2}{(2)^2} + \frac{e^4}{(2.4)^2} - \frac{e}{(2.4.6)^2} + \&c.$$

By means of this series I have computed the value of E for every value of e, proceeding by steps of 0·2 from 0·0 to 10·0. Although the series does in all cases ultimately converge with great rapidity, yet for large values of e it commences by diverging rapidly. Thus for $e = 10·0$ the successive terms are

1·0,
− 25·0,
+156·25,
−434·0277,
+678·168, &c.

and for this value it was necessary to proceed as far as e^{36}. The calculations have been made to six places of decimals, but I have thought it prudent to retain only four places. I may add, that the calculations have not been examined with great care, but that, from the care with which they were made and the general regularity of progression among the numbers, I am confident that there is no large error in the results.

Table of the values of $E = \frac{1}{2\pi}\int_{\theta} \cos(e \cos\theta)$, the limits of the integral being 0 and 2π.

e.	E.	E^2.	*e.*	E.	E^2.
0·0	+1·0000	1·0000	5·2	−0·1104	0·0122
0·2	+0·9900	0·9802	5·4	−0·0412	0·0017
0·4	+0·9604	0·9224	5·6	+0·0269	0·0007
0·6	+0·9120	0·8318	5·8	+0·0917	0·0084
0·8	+0·8463	0·7162	6·0	+0·1507	0·0227
1·0	+0·7652	0·5855	6·2	+0·2018	0·0407
1·2	+0·6711	0·4504	6·4	+0·2433	0·0592
1·4	+0·5669	0·3213	6·6	+0·2740	0·0751
1·6	+0·4554	0·2074	6·8	+0·2931	0·0859
1·8	+0·3400	0·1156	7·0	+0·3001	0·0900
2·0	+0·2239	0·0501	7·2	+0·2951	0·0871
2·2	+0·1104	0·0122	7·4	+0·2781	0·0773
2·4	+0·0025	0·0000	7·6	+0·2516	0·0633
2·6	−0·0968	0·0094	7·8	+0·2154	0·0464
2·8	−0·1850	0·0342	8·0	+0·1727	0·0298
3·0	−0·2601	0·0677	8·2	+0·1222	0·0149
3·2	−0·3202	0·1025	8·4	+0·0691	0·0048
3·4	−0·3643	0·1327	8·6	+0·0144	0·0002
3·6	−0·3918	0·1535	8·8	−0·0394	0·0016
3·8	−0·4026	0·1621	9·0	−0·0907	0·0082
4·0	−0·3971	0·1577	9·2	−0·1365	0·0186
4·2	−0·3766	0·1418	9·4	−0·1768	0·0313
4·4	−0·3423	0·1171	9·6	−0·2091	0·0437
4·6	−0·2961	0·0877	9·8	−0·2324	0·0540
4·8	−0·2404	0·0578	10·0	−0·2460	0·0605
5·0	−0·1776	0·0315			

It appears from this table that E vanishes for the values of

e, 2·405, 5·52, 8·64, nearly; and that the successive maximum values of E^2 correspond to the values of e, 0·00, 3·85, 7·01, 10·15, nearly; and that the corresponding maximum values of E^2 are nearly 1, $\frac{1}{6}$, $\frac{1}{11}$, $\frac{1}{16}$.

I will now proceed to point out the practical application of this table.

First, suppose the value of $\frac{2\pi}{\lambda}\left(\sqrt{h^2+b^2}-\sqrt{h^2+a^2}\right)$ to be small, or not exceeding a few multiples of π.

In this case no considerations can be introduced which tend to simplify the expression for the intensity of the light. If the light be heterogeneous, it will be necessary to form the numerical values of $e_{\prime}$, $e_{\prime\prime}$, $E_{\prime}$, $E_{\prime\prime}$, and $\frac{2\pi}{\lambda}\left(\sqrt{h^2+b^2}-\sqrt{h^2+a^2}\right)$ for each different colour, and to substitute them in the expression $E_{\prime}^2+E_{\prime\prime}^2-2\,E_{\prime}\,E_{\prime\prime}\,.\cos\frac{2\pi}{\lambda}\left(\sqrt{h^2+b^2}-\sqrt{h^2+a^2}\right)$. It is easily seen that the greatest intensity of light under any circumstances is $(E_{\prime}+E_{\prime\prime})^2$ and the least is $(E_{\prime}-E_{\prime\prime})^2$.

Secondly, suppose the value of $\frac{2\pi}{\lambda}\left(\sqrt{h^2+b^2}-\sqrt{h^2+a^2}\right)$ to amount to many multiples of π.

Whatever care be taken to make light homogeneous, it is practically impossible to make it homogeneous or even nearly so. Therefore the value of $\frac{2\pi}{\lambda}\left(\sqrt{h^2+b^2}-\sqrt{h^2+a^2}\right)$ will vary, perhaps to the amount of many multiples of π, for the different values of λ corresponding to the different kinds of light in the stream of light which is the subject of experiment. And this may happen even when the portion of the spectrum corresponding to the light used is so short that there is no conspicuous change of colour. In aggregating the effects of these, it is to be observed that, for points near to the central normal of the circular plate, the change produced in $e_{\prime}$ and $e_{\prime\prime}$ by the change of λ is very small, and the only variation which needs to be considered is that of $\cos\frac{2\pi}{\lambda}\left\{\sqrt{h^2+b^2}-\sqrt{h^2+a^2}\right\}$. As the arc included in this quantity changes its value by several multiples of π, the cosine will have equal values repeatedly positive and negative, and the sum of all may be considered $= 0$. This reduces the expression for intensity to $E_{/}^2 + E_{//}^2$, which will be easily discussed.

Example 1.—The light enters through a round hole without interruption.

Here $a = 0$, and therefore $e_{/} = 0$, and $E_{/} = 1$, and the expression becomes $1 + E_{//}^2$.

The smallest value of this quantity occurs when $E_{//} = 0$. This happens when $e_{//} = 2{\cdot}41$, or $= 5{\cdot}52$, or $= 8{\cdot}64$, &c., or when $e_{//}$ is large. For all these cases the intensity is represented by 1.

The greatest value occurs when $e_{//} = 0$, and the intensity is represented by 2; the next maximum is when $e_{//} = 3{\cdot}8$, and the intensity is 1·16; the third maximum is when $e_{/} = 7{\cdot}0$, and the intensity is 1·09; the fourth maximum is when $e_{//} = 10{\cdot}1$, and the intensity is 1·06.

Thus there will be at the centre a bright spot of double the general intensity, surrounded by rings brighter than the great expanse of light; but the excess of intensity in the rings, even the first, is so small, that it probably could not be seen. The whole diameter of the bright spot, to its very extremities, is $2\,c$, when $\dfrac{2\,\pi\,b\,c}{\lambda\,\sqrt{h^2 + b^2}} = 2{\cdot}41$, or $c = \dfrac{\lambda\,\sqrt{h^2 + b^2}}{2\,\pi\,b} \times 2{\cdot}41$; but the visible diameter would be much smaller.

Example 2.—The stream of light is unlimited externally, but an opake circle of radius a interrupts a portion of it.

Here $e_{//}$ is very great, and $E_{//} = 0$, and the expression for the intensity of the light is simply $E_{/}^2$. The intensities therefore at different distances along any radius drawn from the central normal parallel to the plane of the circle are represented by the numbers in the last column of the table above. The first maximum occurs when $e_{/} = 0$, and it is then $= 1$, or is the same as the intensity in an uninterrupted pencil; the second when $e_{/} = 3{\cdot}8$, and it is then $\frac{1}{6}$ of the former; the third when $e_{/} = 7{\cdot}0$, and it is then $\frac{1}{11}$ of the former; the fourth when $e_{/} = 10{\cdot}1$, and it is then $\frac{1}{16}$ of the former. The light absolutely disappears when $e_{/} = 2{\cdot}41$, or $= 5{\cdot}52$, or $= 8{\cdot}64$. Thus there is a bright spot whose central intensity is equal to that of uninterrupted light, surrounded by rings of much feebler light, whose intensity decreases rapidly till it becomes insensible.

The whole diameter of the bright spot is $2\,c$, when $e_{/}$ or $\dfrac{2\,\pi\,a\,c}{\lambda\,\sqrt{h^2 + a^2}} = 2{\cdot}41$, or $c = \dfrac{\lambda\,\sqrt{h^2 + a^2}}{2\,\pi\,a} \times 2{\cdot}41$; or the diameter of the spot is $\dfrac{\lambda\,\sqrt{h^2 + a^2}}{\pi\,a} \times 2{\cdot}41$.

Suppose, for instance, a circular plate 1 foot in diameter is placed to interrupt a stream of light; a screen is placed 10 feet behind it; to find the diameter of the bright spot.

Here $h = 120$ inches, $a = 6$ inches; λ may be taken for mean light $= 0{\cdot}00002$ inch. Substituting in the expression above, we find the diameter of the bright spot $= 0{\cdot}000308$ inch, or less than $\frac{1}{3000}$ inch, regarding the visibility of which in common experience we need not to disquiet ourselves.

If the diameter of the circular plate had been taken 1 inch, the distance of the screen remaining the same, the diameter of the spot would have been 0·0037 inch, a speck difficult even for a philosopher to discover under these circumstances. If the diameter of the plate were 0·1 inch, the diameter of the spot would be 0·037 inch, a very fit subject for experimental measure. It is on such spots as this that observations have been made, with the purpose of testing the undulatory theory, and of the agreement of those measures with the theory no one acquainted with both has, I believe, doubted.

With regard to any objection that may have been made, either against the undulatory theory generally or against the application of Huyghens's principle in particular, from confronting the result of a simple investigation of the intensity of light at one mathematical point, with the fact of general experience that light is not visible behind an opake screen; the inference from the preceding investigation is, I believe, that the undulatory theory in general, and the application of Huyghens's principle in particular, stand as firmly as they did before appeal was made to this comparison, perhaps even more firmly. For the future I would remark that, however much we may doubt whether one mathematician is entitled to give to another a lesson of *caution*, this principle must, I think, be allowed by every one,—that a theory standing on such varied and such extensive evidence as that which supports the undulatory theory is entitled to the same respect which is given to a private person of high character; it is not to be rashly attacked until the supposed grounds of attack have been most thoroughly examined, although if these grounds are valid, it possesses no immunities beyond other theories.

I am, Gentlemen,
Your very obedient servant,
G. B. Airy.

Royal Observatory, Greenwich, Dec. 4, 1840.

II. *Reply to* Mr. Tovey's *Remarks on a Paper on the Application of* Huyghens's *Principle in Physical Optics, inserted in the Lond. Edinb. & Dublin Phil. Mag. for October last*; vol. xvii. p. 243. *By* Richard Potter, *Esq.*, *B.A.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I BEG to return Mr. Tovey my thanks for keeping alive the discussion on the capability of the undulatory to explain facts; by such discussions we may entertain hopes that some sound progress will eventually be made in physical optics.

Mr. Tovey says I have mistaken a luminous *line* for a luminous *space*, and consequently my conclusions have, in reality, no foundation. He ought unquestionably to have given us his proof.

In the investigations for a small opake disc and a small circular aperture, it was considered, by the most eminent mathematicians who have adopted the undulatory theory of light, as quite sufficient to find the intensity along a line drawn perpendicularly through the centre of the aperture or disc, and their results were brought forward as strong proofs of the truth of the theory. When, in the same way, I have applied a more correct analysis, for all magnitudes of circular discs and apertures, it is objected to, without showing where the limit between a large and a small magnitude lies, and at what point the method becomes inaccurate.

Our knowledge of the phænomena of diffraction leads us to conclude that there can be no maxima and minima in the line before-mentioned without accompanying ones in the plane perpendicular to that line, and such has tacitly been received with respect to former investigations. With this the undulatory theory also accords. How Mr. Tovey could suppose that there is in reality in any case only a luminous line, it is not easy to account for; if he had been acquainted with the phænomena I think he would not have made the objection.

The case of the fixed stars shows that the smallness of the angular magnitude of the luminous body at the eye is no bar to light being perceived when there is adequate intensity.

The following analysis shows that the undulatory theory indicates maxima and minima in the plane perpendicular to the line, for which my former results were investigated.

Taking now a point out of the line (C B) passing perpendicularly through the centre of the circular aperture or annulus, let its distance from that line be small and equal to x.

Let θ be the angle which any radius makes with the plane passing through the perpendicular line and this point. Then $r\,\delta\theta\,\delta r$ will be an elementary area in the aperture, and for simplicity I take the case of plane waves, the other only differing by a constant.

Putting h and a the same as before, we have the displacement of the particle at the point, arising from this element.

$$= \frac{r\,\delta\theta\,\delta r}{\sqrt{r^2 + h^2 + x^2 - 2\,r\,x\cos\theta}}$$

$$\times \sin\frac{2\,\pi}{\lambda}\left(v\,t - \sqrt{r^2 + h^2 + x^2 - 2\,r\,x\cos\theta}\cdot\right)$$

Expanding and retaining the terms involving the first power of x only, we have the whole displacement

$$= a\int_r\int_\theta \frac{r}{\sqrt{r^2 + h^2}}\sin\frac{2\,\pi}{\lambda}\left(v\,t - \sqrt{r^2 + h^2}\right)$$

$$+ \frac{2\,\pi\,a\,x}{\lambda}\int_r\int_\theta \frac{r^2\cos\theta}{r^2 + h^2}\cos\frac{2\,\pi}{\lambda}\left(v\,t - \sqrt{r^2 + h^2}\right)$$

$$+ a\,x\int_r\int_\theta \frac{r^2\cos\theta}{(r^2 + h^2)^{\frac{3}{2}}}\sin\frac{2\,\pi}{\lambda}\left(v\,t - \sqrt{r^2 + h^2}\right)$$

$$= a\int_r \frac{r\,\theta}{\sqrt{r^2 + h^2}}\sin\frac{2\,\pi}{\lambda}\left(v\,t - \sqrt{r^2 + h^2}\right)$$

$$+ \frac{2\,\pi\,a\,x}{\lambda}\int \frac{r^2\sin\theta}{r^2 + h^2}\cos\frac{2\,\pi}{\lambda}\left(v\,t - \sqrt{r^2 + h^2}\right)$$

$$+ a\,x\int_r \frac{r^2\sin\theta}{(r^2 + h^2)^{\frac{3}{2}}}\sin\frac{2\,\pi}{\lambda}\left(v\,t - \sqrt{r^2 + h^2}\right) + C$$

and taken between the limits $\theta = 0$ and $\theta = 2\,\pi$

$$= 2\,\pi\,a\int_r \frac{r}{\sqrt{r^2 + h^2}}\sin\frac{2\,\pi}{\lambda}\left(v\,t - \sqrt{r^2 + h^2}\right),$$

which is the same expression as on the line itself, and indicates a maximum in the plane perpendicular to the line when there is a maximum in the line.

In examining the terms involving x^2, I shall retain only the principal term in the coefficient of that quantity, which has λ^2 in its denominator, and thus the quantity which is the correction of the last found expression is the following:

$$-\frac{2\,\pi^2\,a\,x^2}{\lambda^2}\int_r\int_\theta \frac{r^3\cos^2\theta}{(r^2 + h^2 + x^2)^{\frac{5}{2}}}\sin\frac{2\,\pi}{\lambda}\left(v\,t - \sqrt{r^2 + h^2 + x^2}\right)$$

between the limits $\theta = 0$ and $\theta = 2\,\pi$

$$= -\frac{2\pi^3 a x^2}{\lambda^2}\int_r \frac{r^3}{(r^2+h^2+x^2)^{\frac{3}{2}}} \sin\frac{2\pi}{\lambda}\left(vt - \sqrt{r^2+h^2+x^2}\right)$$

which we see is a small quantity as long as x is a quantity of the same order of magnitude as λ, and h is many multiples of r, which would still give the case of a point in the shadow of a large circular disc, the aperture being an annulus; but to take the extreme case which can be urged as an objection to my former results, let h be small compared with r, or the point deeply immersed in the shadow. The integral being separated into two we have

$$-\frac{2\pi^3 a x^2}{\lambda^2}\int_r \frac{r}{\sqrt{r^2+h^2+x^2}} \sin\frac{2\pi}{\lambda}\left(vt - \sqrt{r^2+h^2+x^2}\right)$$

$$+\frac{2\pi^3 a x^2}{\lambda^2}\int_r \frac{(h^2+x^2)\,r}{(r^2+h^2+x^2)^{\frac{3}{2}}} \sin\frac{2\pi}{\lambda}\left(vt - \sqrt{r^2+h^2+x^2}\right).$$

We now reject the latter as very much smaller than the former, and have only to perform the same integration as in my former paper, and the intensity becomes for an aperture which is an annulus and the radii r_1 and r_2,

$$=4a^2\lambda^2\left(1-\frac{\pi^2 x^2}{\lambda^2}\right)^2 \sin^2\frac{\pi}{\lambda}\left(\sqrt{r_2{}^2+h^2+x^2}-\sqrt{r_1{}^2+h^2+x^2}\right).$$

The maximum intensity on the line being $4a^2\lambda^2$, there will be a minimum equal to zero when $x = \pm\frac{\lambda}{\pi}$, and then the intensity will increase again, and apparently to a greater magnitude than before, but our having used approximate expressions prevents us pushing our conclusions further.

We see clearly how maxima and minima are indicated by the theory in the plane perpendicular to the line in which I formerly discussed the results of the same method, and how much Mr. Tovey has mistaken the question.

Queens' College, Cambridge, Dec. 8, 1840. RICHARD POTTER.

III. *The Impossibility of the Boetian System of Numerical Contractions, and the Alabaldine Notation having had a common Origin.* *By* J. O. HALLIWELL, *Esq.*, *F.R.S.* &c.

ON a further examination of the Arundel MS. No. 343, the real explanation of which has already appeared in this Magazine, and on working out all the examples in that manuscript with the tedious notation of the Boetian and Alabaldine contractions, I have met with the following, which shows more clearly than any other I have yet discovered, the accuracy of the views taken by M. Chasles and myself, on the impossibility of the Alabaldine system having commenced with the abacus. I merely give the early steps, which will be quite sufficient to show the correctness of my statement.

Required the result of dextans. siliqua. ceraces.

The reduction of dextans:

$$\text{dextans} = \frac{\text{igin. sipos}}{\text{andras. calcis}} = \frac{\text{andras. quinas}}{\text{andras. calcis}} = \frac{\text{quinas}}{\text{calcis}}$$

$$= \text{semiuncia. quicunx.}$$

The very first step of which uses the decimal scale.

IV. *On Steam considered as a Conductor of Electricity. By Dr.* CHARLES SCHAFHAEUTL*.

IN the last Number of the Philosophical Magazine, the electricity obtained from a jet of high-pressure steam was considered to be of similar origin with that obtained from the insulated and separated positive metallic disc of Volta's Electrophorus.

On this point, the first question which presents itself is, in what relation does steam or water-gas stand with the conductors or non-conductors of electricity.

It is well known that moist air is a conductor of electricity, and dry air, viz. air which contains less water-gas than it is capable of containing according to its temperature, is a non-conductor of electricity; but, besides this, I am not aware of any experiment made to ascertain the conducting power of pure steam without being in contact with water or mercury, and I therefore determined to ascertain this question by experiment.

The ends of a glass tube, about two inches long and a quarter inch interior diameter, were drawn out over a lamp to points, and bent upwards in a right angle. A thick platinum wire, with a small ring formed at each end, was then inserted into one end of the tube, and the glass melted around it air-tight. Water was then poured into the tube and boiled till only about two drops remained, when another platinum wire was inserted at the other end, and the hole quickly hermetically closed as before. The distance between the ends of the two wires in the tube was about one inch and a quarter, and the tube in this state contained of course nothing but water-gas and some liquid water.

This tube was now inserted close to the bulb of a thermometer into a small sand-bath †, and covered with the sand, excepting the two vertical ends.

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* Communicated by the Author, whose former communication on the subject appeared in our last Number; L. E. and D. Phil. Mag. vol. xvii. p. 449.

† I cannot omit to mention the kindness I experienced from the gentlemen lecturing at the Adelaide Gallery, in allowing me the use of their

One of these platinum wires was then connected with the outside of a Leyden jar, the other with an insulated discharger.

The Leyden jar, containing about 100 square inches armed surface, was now charged by means of fifty revolutions of a twelve-inch glass plate, and then discharged through the tube as usual. The glass tube acted exactly as an imperfect conductor, interrupting the conducting wire, which connects the two surfaces of the jar, like a piece of wet cotton thread, or portion of glass tube moistened inside. The jar was perfectly discharged by the first touch, with that peculiar hissing noise and reddish fascicular stream of electricity which invariably occur under similar circumstances. The temperature of the sand-bath was now gradually raised, and at every 5 degrees a similar electric discharge from the Leyden jar was passed through the tube. The same results were obtained until the thermometer reached 250 degrees. At this point, by discharging the jar, a small red spark was obtained, instead of the former fascicular stream, and the jar was found to be entirely discharged, although the noise occasioned by the spark was scarcely audible, compared with the loud clap produced by the discharge of the jar under ordinary circumstances.

After the temperature had been elevated to 405 degrees, the contents of the jar discharged with the usual brilliant spark and loud report, and at the same time the spark was seen passing through the tube. At this time no moisture could be detected in the tube, and the water-gas contained in it had entirely ceased to be a conductor of electricity, at the same time giving less resistance to the passage of the spark than common air, the striking distance having been elongated from half an inch to one inch and a quarter. When the temperature was reduced below 405 degrees, the discharge passed as before mentioned, either as a small red spark, or in a fascicular stream, according to the temperature. When above 405 degrees the spark passed as usual, until the temperature rose to 443, when the tube burst, which prevented me from ascertaining its weight with and without the water; the difference of which would of course have given me the weight of the water contained in the tube, the cubical contents of which would have been ascertained by filling it with quicksilver. If we assume that there were two drops of water in the tube when it burst, weighing together 0·73 grains, and supposing the contents of the tube to be 500 cubic lines, we have the pressure of 23·5 atmospheres therein.

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laboratory and philosophical apparatus for the purpose of making these experiments. Institutions of this description, which are to be found only in England, are invaluable to foreigners occupied in philosophical researches.

From the preceding experiments we may conclude that steam, pure and free from contact with water, has, like all other gases, the property of being a non-conductor of electricity.

The facility with which the spark passes through the water-gas seems to be worthy of attention; the striking distance of the spark having increased from half an inch to one inch and a quarter; for, according to Mr. Harris's discoveries, the striking distances are, in ordinary cases, in the inverse ratio to the density of the gas.

If we now consider the fact, that the electricity of a jet of condensed steam is, according to Mr. Armstrong's experiments, positive; that the quantity of electricity obtained from a jet of high-pressure steam is in proportion to its condensation; further, that the steam contained in the boiler presents no appearances of free electricity; and that, according to Mr. Pattinson's experiments, both water and boiler are negative, which is a necessary consequence of Mr. Armstrong's experiments;—we perceive a simultaneous development of electric polarity in opposite directions from a central or neutral point, as in the magnetic steel-bar; and this development of electric polarity can only be ascribed to the opposite changes of molecular arrangements, as well as chemical condition of the water and steam column; and we must consider both electric poles as co-existent and not separately.

Volta's electrophorus is only remarkable for the property of retaining its electricity for a lengthened period, and its action is entirely due to *induced electricity*, with which no one will confound the electricity obtained from steam. Besides, the disc of the electrophorus, from which the spark is obtained, must be a perfect conductor well insulated, and shows signs of free electricity only when it is, after close contact with the electrophoric cake and neutralization of its free electricity, removed absolutely from the inducing cake. In a boiler filled with steam and water, neither of the above-mentioned circumstances can take place, and the positive electricity of the condensed steam, and the negative electricity of the boiler are the only points ascertained by experiment. The electricity developed by evaporation, as the source of the observed free electricity, is only hypothetical.

Volta's experiment, of splashing water on ignited charcoal, can scarcely be considered as identical with the evaporation of water in a boiler; in the first instance, a mass of chemical decomposition and changes are taking place which never can occur in the latter, for even the sudden cooling of substances is sufficient to produce signs of free electricity.

DIRECTION OF WIND AT NOON OBSERVED BY W.C. REDFIELD, DEC$^{\text{R}}$. 15 1830.

DIRECTION OF WIND AT NOON OBSERVED BY W. C. REDFIELD, DEC^{R} 15 1830

If we ascribe the electricity of steam to its condensation, the circumstances under which that condensation takes place are likewise of great influence. The smallest jet of high-pressure steam developes more free electricity than 100 times greater quantity of low-pressure steam;—another condition under which electricity is produced from a jet of steam, seems therefore to be its *rapid expansion* when issuing from the boiler; or probably, as I have suggested on a former occasion, the quantity of caloric becoming latent during the expansion of high-pressure steam, has some relation to the quantity of electricity being set free. Even electricity in thunder-storms seems to be ascribable partially to the rapid currents of air whirling towards the centre of the clouds, as caloric is absorbed whilst the thunder-clouds are charging themselves.

V. *Explanation of a Map, showing the Direction of the Wind at Noon, as observed at various Places, in the Storm of December* 15, 1839. *By* W. C. REDFIELD, *Esq.*

THE arrows on the map, Plate I. denote, approximately, the direction of wind at noon at the several places of observation. The concentric lines, drawn at intervals of thirty miles, were added, not as precisely indicating the true course of the wind, but in order to afford better means of comparison for the several observations.

The assumed axis of the gale, at this time, should probably have been placed more to the westward, on a line with the position of the Morrison and Cape Cod Bay, at which points the gale was then blowing with great violence in opposite directions. The Morrison was from China, bound to New York, and I have reason to believe that her position may be safely relied on. The ship, as I am informed, was lying to at noon with bare poles, and had taken the western side of the gale suddenly at 7 A.M. This gale was severely felt in most of the region comprised in this map, excepting its northwestern and extreme northern portion, and excepting also the light winds which were found near the axis of the gale in the vicinity of Buzzard's Bay, &c. in the afternoon and evening. A very heavy fall of snow accompanied the gale in the states of Connecticut, Rhode Island, Massachusetts, New Hampshire and Maine, also in parts of the states of New York and Vermont. Some snow also fell in the western and northern parts of New York and Vermont, but attended with moderate winds, chiefly from the north and north-west.

Abbreviations—N.H. New Hampshire.—Me. Maine.—Ms. Massachusetts.—R.I. Rhode Island, State.—Ct. Connecticut.—L.I. Long Island.—N.Y. New York, State.—N. J. New Jersey. *Note.* My observations on the 15th P.M. have on a former occasion been erroneously printed N.W. by W.; *read* N.W. by N.

Schedule of the Observations on the Direction of Wind in the Storm of December 15, 1839, *as indicated on the annexed Map.*

No.*	Places of Observation.	A.M.	Noon.	P.M.	Observers and Authorities.
1.	Nantucket, Ms....	E.	S.E. at 1 P.M.	S.W.	Report of J. Mitchell, as published by Mr. Espy.
2.	Barnstable, Ms.	N.E. at 7 A.M. Gale from S.E.	(E.S.E.)	E. 2 P.M.—S.E. Sunset ... ... S.W. P.M.—Sunset, clear	Report to the Editor of the Boston Courier. Letter from Wm. H. Brown, Barnstable. I take the mean between E. and S.E. for the true direction at noon.
3.	New Bedford, Ms.	Sunrise N.E. moderate. Sunrise E. fresh.	(E. by N.)	E.N.E. 2 P.M. — S., 3½ P.M. ... E. 2 P.M.—S.S.E., Sunset.	Joseph Congdon's Meteorological Journal. Samuel Rodman's do., as published by Mr. Espy. I take E. by N. as the mean for noon.
4.	Newport, R. I.	N.E.	(N E.)	N.E.	Prof. Caswell's Meteorological Journal.
5.	Cape Cod Bay.	E.S.E.	E.S.E.	E.S.E. at 2 P.M.	Report of Capt. Slemmer, brig Columbus.
6.	Cape Cod, Provincetown, Ms.	E.S.E.	E.S.E.	E.S.E.	Marine reports in the Boston Newspapers.
7.	Providence, R. I.	N.E.	N.E.	N.E.	Prof. Caswell's Meteorological Journal.
8.	Norwich, Ct.	N.E.	N.E.	N.E.	Norwich Courier.
9.	Culloden Point, L. I.... ...	“ change to	N.W. at noon.”		Capt. Green's account, as published by Mr. Espy.
10.	Boston, Ms.	N.E. at sunrise. E. by N., A.M.	E.N.E. E.byN. (E. by N. ½ N.)	E.S.E. at sunset. E. by N. P.M.	Wm. Cranch Bond's Meteor. Journal. Robert Treat Paine.
11.	Gloucester, Cape Ann, Ms.	E.S.E.	E.S.E.	E.S.E.	Letter from Gloucester in the Boston Newspapers.
12.	Salem, Ms.	Eastward.	Eastward.	Eastward.	Salem Gazette.
13.	Waltham, Ms.	N.E.	(E.N.E.)	E.	Monthly Met. Journal by C. F., published in the Boston Daily Sentinel.
14.	Worcester, Ms.	N.E.	N.E.	N.E.	Met. Journal kept at the State Lunatic Hospital, published in the National Ægis.
15.	Nashua, N. H.	N.E.	N.E.	N.E.	Nashua Telegraph.
16.	Middletown, Ct.	N. N.	(N. by E.)	N. N.N.E.	Reported by Prof. Smith. Dr. Barratt's Met. Journal.
17.	Ship Morrison, lat. 39°·35 long. 71°·50	S.E. W.N.W.	W.N.W.	W.N.W.	Ship's Log-book, and testimony of Capt. Benson and his Officers.
18.	Portsmouth, N. H.	E.	(E.)	E.	Weekly Met. Journal, published at Portsmouth.

19.	Northampton, Ms. ... Amherst, Ms.	N.E. N. by W.	(N.N.E.)	N.E. N. by W.	W. Atwill and others, from Mr. Atwill. Prof. Snell's Met. Journal, from Professor Hitchcock.	Approximate mean assumed N.N.E.
20.	New Haven, Ct.	N. by W. N.N.E.	(Mean N. 3°. E.)	N.N.W. N.N.E. till 1½ P.M.	Report of Capt. Woolsey, Steamer Providence. Judge Darling's Met. Journal.	
21.	Concord, N. H.	North-easterly.	N.E.	N.E. & more Northly.	Letter from Concord to S. G. Arnold, from Mr. [Arnold.	
22.	Keene, N. H.	N.E.	N.E.	N.E.	Rev. Z. S. Barstow's Met. Journal.	
23.	Litchfield, Ct.	Night of 14th, N.E.	(N.N.E.)	N. at night of 15th.	Litchfield Inquirer.	
24.	Stratford, Ct...	N. by W.	N. by W.	N. by W.	Rev. J. R. Linsley's Met. Journal.	
25.	Fire Island Beach, L. I. ...	At midnight. N.E., veered by N.	. W.	N.W.	Captains Cartwright and Skiddy, employed at the Beach.	
26.	Portland, Me.	N.E. at 11 E.	(E. 6° S.)	E. by S.	Met. Report of the Keeper of Marine Observatory, published at Portland.	
27.	Kinderhook, N. Y.	N.E.	N.E.	N.E.	Silas Metcalf; Met. Journal.	
28.	West Point, N. Y.	N.	N.	N.	Met, Journal of the Medical Officer.	
29.	New York, City ... ... Fort Wood, N. Y. Harbour	N. by W.—N.N.W. N.	N.N.W. (N.N.W.)	N.W. by N. N.W.	Met. Journal of W. C. Redfield. Met. Journal of the Medical Officer.	
30.	Flatbush, L. I.	N.	(N.N.W.)	N.W.	Rev. T. M. Strong; Met. Journal.	
31.	Sandy Hook Bay, N. Y. ...	N.	N.W.	N.W.	Log-book of Bark Osceola, anchored in the Bay.	
32.	Hanover, N. H.	N.E.	(N.E.)	N.	Prof. Young's Met. Journal, from Prof. Hubbard.	
33.	Salem, Washington Co., N. Y.	N.E.	N.E.	N.E.	Wm. Brand and W. Larkin; Met. Journal.	
34.	Albany, N. Y. ... ... Lausingburgh, N. Y. ...	N.E. N.N.E.	(N. 28°. E.)	N.E. N.	T. R. Beck, M.D.; Met. Journal. E. T. Foote; Meteorological Journ.	Mean assumed N. 28°. E.
35.	Kingston, N. Y.	N.E.	N.E.	N.E.	Isaac Blauvelt; Met. Journal.	
36.	Goshen, N. Y.	N.E.	(N.N.E.)	N.	Nathaniel Webb and Dr. John S. Crane; Met. Journal.	
37.	Bark Ann Louisa, off Absecund, N. J.	W.N.W.	N.W.	N.W.	Ship's Log-book, and testimony of Capt. Wilson.	
38.	Hartwick, Otsego Co., N. Y.	N.E.	N.E.	N.E.	Rev. Geo. B. Miller and L. Sternberg; Met. Journal.	
39.	Oxford, Chenango Co., N. Y.	N.	N.	N.	Merritt G. M'Koow; Met. Journal.	
40.	Cape May, N. J.	N.W.	N.W.	N.W.	Marine Reports, and Letter in the Philadelphia Newspapers.	

New York, July 6, 1840.

* These correspond to the same numbers on the map.

VI. *Remarks relating to the Tornado which visited New Brunswick in the State of New Jersey, June* 19, 1835, *with a Plan and Schedule of the Prostrations observed on a Section of its Track. By* W. C. Redfield, *Esq*[*].

[Illustrated by Plate II.]

IN a paper printed in the American Journal of Science, in which I referred to the support given by Prof. Bache to Mr. Espy's theory of storms, at the meeting of the British Association in 1838, founded upon observations made on the New Brunswick tornado, I have stated, that in my own examinations I had observed numerous facts which appear to demonstrate the *whirling* character of this tornado, as well as the *inward* tendency of the whirling vortex at the surface of the ground; and further, that the direction of rotation was *towards the left*, as in the North Atlantic hurricanes[†]. It was due to Prof. Bache that my observations should be brought forward, a task which has been too long delayed, partly from a desire that he would revise his former conclusions. The facts now presented form part of the evidence to which I then alluded.

If the effects which I present for consideration be due to "a moving column of rarefied air without any whirling motion at or near the surface of the ground," as maintained by Prof. Bache[‡], we might expect to find a relative uniformity in these effects, on the two opposite sides or margins of the track. How far this is the case, may be seen by inspecting the observations which are found upon the annexed figure. (Plate II.)

The occurrence of these tornadoes appears to have been noticed from the earliest antiquity; and their violence has been considered as the effect of an active whirling motion in the body of the tornado; this peculiarity of action having often been supported by the testimony of eye-witnesses.

The whirling motion, however, has not been recognized by Prof. Bache, Mr. Espy [§], or Prof. Walter R. Johnson [||], in their several accounts of the New Brunswick tornado; these writers having been led to adopt or favour a theory of ascending columns in the atmosphere, founded on the supposed in-

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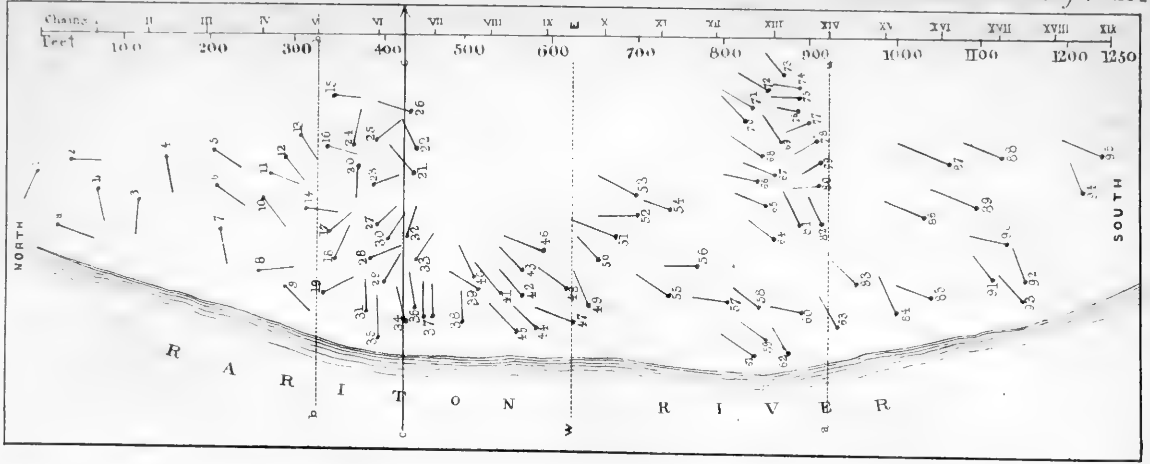
[*] [Communicated by Sir John F. W. Herschel, Bart.] This paper was intended by its author to have been read at the late meeting of the British Association in Glasgow, but was unfortunately detained.—J. F. W. H.

[†] Amer. Journ. of Science, Oct. 1838, vol. xxxv. pp. 206, 207.

[‡] Transactions of Amer. Phil. Society, vol. v. p. 417. New Series.

[§] Trans. Amer. Phil. Society, vol. v. New Series.

[||] Journ. Academy Nat. Sciences of Philadelphia, vol. vii. Part ii.



Sketch of the Prostrations caused by the Tornado of June 19th 1835 in the belt of Wood on the east bank of the Rariton, opposite the City of New-Brunswick, in New Jersey by W. E Redfield.

NOTE The bank of the river is here covered by an irregular belt of Wood, immediately east of which is a clear field, the western margin of which is equally irregular in its outline

⟵——⟶ Represents the course of the apparent axis of the Tornado — **W** West **E** East.

The dots show the root end of the trees, part of the roots, in all the cases, being fast in the ground.

Course of the Tornado, East.

J Basire lith.

fluence of calorific expansion accompanying the condensation of vapour.

It is remarkable that up to this period the evidences of the rotation, or other characteristic action of tornadoes, appear not to have been recorded, nor to have received the distinct consideration of scientific observers. We are, therefore, left to seek out the peculiarities of their action, by examining the direction of the prostrations and other effects of the wind: and from a careful induction from the effects which are thus registered as by the finger of the tornado, we may hope to arrive at satisfactory conclusions.

If the numerous prostrations of trees and other objects, which may be observed in the path of a tornado, be the effects of a violent whirlwind, it appears most reasonable to infer that this whirl had the common property which may be observed in all narrow and violent vortices, viz. *a spirally involute motion*, quickened in its gyrations as it approaches toward the centre or axis of the whirl, and there continued (in the case of the whirlwind) spirally upward, but gradually expanding again in its spiral course in ascending towards the extreme height of the revolving mass.

If we now contemplate the action of this whirling body, while in a state of rapid progression, on the several objects found in distinct portions of its path, we may expect to witness effects of much complexity, particularly as regards the lines of direction; and, also, that amid this apparent complexity some clue may be obtained that will serve to indicate or establish the true character of its action. Some of the effects which may be expected, or observed, will be here considered.

1. We may expect to find in the path of the whirlwind, strong evidence of the inward or vorticular course of the wind: the violence of which inward motion is clearly indicated by the force with which various objects, often of much weight, are carried spirally upward about the axis of the revolving body.

Now the effects of this inward vorticular motion at the surface of the ground, are clearly manifested in the cases before us; and are also well illustrated by Prof. Bache in his paper on this tornado, although referred by him to a different action *.

2. As the effects observed at various points in the track were produced at different moments of time and by forces acting in different directions, as well as of various intensities,

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* Transactions of American Philosophical Society, vol. v.

we may expect to find great diversities in the several directions of the fallen trees and other prostrated bodies: and further, as all the forces, in addition to their inward tendency, have likewise a common tendency in the direction pursued by the tornado, we may expect to find, also, full evidence of this progressive force in the direction of the fallen bodies.

These effects, I need hardly state, are distinctly observed in the case before us; and appear likewise from the observations of Prof. Bache. The results already noticed have been observed also in the tracks of other tornadoes; so that a general inclination, both inward and onward, amid the various and confused directions of the fallen bodies, is distinctly recognized by all parties to this inquiry.

3. It has been often noticed, that where two fallen trees are found lying across each other, the uppermost or last fallen points most nearly to the course pursued by the tornado.

In view of the facts above stated, much pains have been taken to establish, by induction, a central and non-whirling course in the wind of the tornado; first inward and then upward, like that resulting from a common fire in the open air. I do not propose to notice the insuperable difficulties which appear to attend this hypothesis. It is important to state, however, that all the above-mentioned effects, when theoretically considered, are, at least, equally consistent with the involute whirling action of an advancing vortex. This important consideration I have not seen recognized by the advocates of the non-whirling theory; and it seems proper, therefore, to point out, as we proceed, other and more distinguishing effects of the whirling action.

4. It has been noticed, also, that the directions given to broken limbs and other bodies, by the successive changes in the direction of the wind as the tornado passed over, have been found in opposite courses, on the two opposite margins of the track.

This fact, too, has been strongly urged as disproving a rotary motion. But, unfortunately for the objection, this effect accords fully with the rotary action of a progressive mass of atmosphere; as is well known to all who understand the *theory* of rotary storms.

In all such whirling masses the successive changes in the direction of the wind, which result solely from their progressive motion, necessarily take place in opposite directions or courses, on the two opposite sides of the advancing axis. This indication fails, therefore, as a theoretic test: and I now proceed to notice others, which are peculiar to a progressive whirling action.

5. In considering further the effects of such action, we may expect to find that the increased activity of gyration which is observed near the centre of a vortex, will be indicated by a more violent and irregular action in and near the path pursued by the axis of the whirlwind, than is found under its more outward portions.

This effect is often strikingly exhibited in the path of tornadoes; while, in the supposed ascent of a non-whirling column, it would seem that no part of the surface would be so much exempted from its action as that lying near its centre.

6. As the effect of rotation must be to produce, on one side of the advancing axis, a reversed motion which is contrary to the course of the tornado, it is evident that on this side the prostrating power will be much lessened; that the cases of prostration, therefore, will be less numerous; and that some of these, at least, will be produced in a backward direction, more or less opposite to the course of the tornado. By this criterion, not only the whirling movement, but the direction of the rotation also may be clearly ascertained.

This effect is best observed by comparing the two opposite margins of the track; and is strongly exemplified in the case before us. Here we find, that most of the trees prostrated within five chains (110 yards) from the northern or left-hand margin of the track, lie in directions which are more or less backward from the course of the tornado. The prostrations in this part of the track are also for the most part less general than on the opposite side of the axis *, a greater portion of the trees being left standing.

It sometimes happens, owing perhaps to the inward or involute motion having exceeded the progressive motion at a particular point, that some inclination backward will be found in the prostrations on the progressive side of the whirl, as seen on the sketch, Nos. 77 to 80. But these unfrequent cases by no means compare with the numerous backward and sometimes *outward* prostrations, found on the reverse side of the whirl, as illustrated by Nos. 1, 3, 4, 7, 9, 10, 12, 13, &c. on the left side of the track.

Thus we find here a satisfactory indication that this tornado was a whirlwind; and that the course of its rotation was to the *left* in front.

7. It is also apparent, that the prostrating power of a whirlwind on the side of its reversed motion as just consi-

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* There was a vacant space in the belt of wood, immediately to the right of the line *c c* or axis of the tornado, owing to which the effect mentioned does not appear so obvious in the figure.

dered, will be limited to a shorter distance than on the opposite or progressive side of its axis.

This is seen in the more limited *extent* of the prostrations on the north or left margin of the track, as compared with the extent of those which incline inward on the right side of the apparent axis.—There were many trees standing beyond the northern border of the track, but none had fallen.

8. It follows, in like manner, that on that side of a whirlwind in which the rotary motion coincides with the progressive movement, the prostrating power will not only be increased in its intensity, but will also be effective over a wider space; and that few, if any, of the prostrated bodies will be found to have been thrown backward.

In the case before us, as may be seen in the sketch, the prostrations are found to extend on the southern or right side of the apparent axis to a distance nearly twice as great as on the left side. The same general result has also been noticed in the tracks of other tornadoes which I have examined.

The facts here considered are too important to be overlooked, and seem fully to establish the course of rotation.

9. If a rotative action be exhibited, the mean directions of all the prostrations, on each of the two opposite sides, will differ greatly in their respective inclinations to the line of progress, and the mean direction of those on the reverse side will be found more backward than on the opposite side, where the rotative course coincides with the progressive action.

In the case before us, the mean direction of all the prostrations on the right side of the track is found to incline fifty-seven degrees inward from the line of progress. The course of the tornado is here taken to be east; although for the last half mile its course had been a little north of east. On the left side, the mean direction is found to be S. 6° W., or ninety-six degrees inward and backward; a difference in the mean inclination from the course on the two sides of forty-seven degrees*.

If we now take the indications afforded by the two exterior portions of the track, to the width of five chains on each side, where the effects are more distinctive in their character, we find on the right side a mean inward inclination of forty-nine degrees; the mean direction being N. 41° E.: while on the left side of the track the mean inclination is not only inward, but forty-seven degrees backward; the mean

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* The inclinations of the fallen trees from the course, on both sides the axis, are reckoned inward and backward.

direction on this side being S. 47° West. We have thus a mean difference in the inclination of the fallen trees on the two exterior portions of the track, of no less than eighty-eight degrees. These indications seem conclusive also in favour of the whirling action in the direction from right to left.

10. Although of less importance, it should be mentioned that the diminished action of the tornado which is commonly observed on the hill sides and summits over which it passes, and the greatly increased action in the bottoms of the valleys and even in deep ravines, afford a strong argument against ascribing the effects to the ascent of a non-whirling rarefied column; as the latter, it would seem, must act with greater force on the hill sides and summits than in the bottoms of valleys. The general correctness of the observation here stated cannot justly be questioned.

11. The sudden and extraordinary diminution of the atmospheric pressure which is said to take place at the points successively passed over by the tornado, causing the doors and windows of buildings to burst outwards, seems to afford strong confirmation of a violent whirling motion, for an effect of this kind is necessarily due to the centrifugal and upward force of the vorticular action. There are no other means known by which such an abstraction of pressure can be effected in the open air. An increase of calorific elasticity, if such were produced, either generally or locally, would not greatly disturb the equilibrium of pressure, being resisted by the surrounding and incumbent weight of the entire atmosphere. Besides, the immediate effect of such increased elasticity might rather be to burst *inward* the windows and doors of buildings exposed to its action.

Some of the more important indications mentioned above, appear also from Prof. Bache's observations; although the latter are not definitely located by him, as regards the extreme borders of the track. Thus, in figure 7 of Prof. Bache's paper, assuming the course of the tornado to be east, and rejecting a few observations near the centre, to avoid error, we find in twenty observations on the right side of the track, a mean inward inclination of sixty-four degrees, and for nine observations on the left side, a mean inclination, reckoned inward and backward from the course, of one hundred and four degrees, being fourteen degrees backward.

It is stated by Prof. Bache, "that the trees lying perpendicular to the track of the storm, are not those furthest from the centre of that track." This generalization accords with my own observations; but can hardly be reconciled with an inward non-whirling motion in the tornado.

It may appear to some that in the case of a whirlwind the greater portion of the prostrations on the reverse side of the axis should be found in a backward direction: and so they would undoubtedly be found, were it not for the inward and the progressive action. But the force is here so far lessened by the reverse action above noticed, that in most cases only a small portion of the trees exposed will be thus prostrated: while the greatest force of the whirlwind, on this side, is felt near its last or closing portion and towards the apparent axis, where the inward, together with the rotative and progressive forces, seem to combine their influence in the closing rush towards the heart of the receding vortex. This appears to account for the nearly opposite directions of prostration found on this side, and it is apparently by this more violent closing action, that many trees which were first overthrown in a direction nearly across the centre of the path, were again overturned inversely, and often carried forward nearly in the course of the tornado. It is proper to remark here, that an attentive examination of these effects has served to convince me that on the right and more central portions of the track the prostrations for the most part take place either at the outset or under the middle portions of the whirlwind, while on the left or reverse side, up to the line of the apparent axis, and sometimes across the latter, they occur chiefly under the closing action of the whirl, as above described. The violent effects of the latter are more clearly seen as we advance from the left-hand margin towards the centre or apparent axis of the path.

From the causes to which I have just alluded, aided by the elevating forces about the axis of the tornado, the effects are usually more violent on and near the line passed over by the axis, than in other portions of the track. This line of greatest violence is found to coincide nearly with the line which separates the inwardly inclined prostrations of the two opposite sides of the track*. The latter line or apparent axis of the track is sometimes called the line of convergence, and is indicated on the figure by the line and arrow *c c*. Along this line, from the causes just mentioned, aided also by the elevating forces about the axis, many of the trees are carried forward and left with their tops in a direction nearly parallel to the course of the tornado; forming an apparent, but not a just exception, to the more lateral direction which pertains to most of the trees prostrated by the onset of the whirlwind, near the central portions of the track. Indeed, the central or closing violence of the advancing whirl is here so great, that

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* The line of greatest violence, for the most part, is found somewhat to the right of the line of convergence.

the trees are not unfrequently torn out of the ground and carried onward to considerable distances.

It is proper to state here, that in the tracks of all the tornadoes which I have had opportunity to examine, and in some, at least, of those examined by others, the course of rotation has been found the same as in the case before us*.

In order to make a just and satisfactory examination of the effects of a tornado, it appears necessary to select portions of the track where the extension of wood or single trees, on each side, is found sufficient to mark clearly the exterior limits of the prostrating power, and where the effects on both sides of the axis are also clearly developed. Our next care should be to ascertain, as near as may be practicable, the line which separates the opposite convergence of the two sides, noticed above as the axis or line of convergence. We should then determine the general direction of this line and of the track at the place examined; which being done, we may proceed to measure the distance to which the prostrations are extended on each side, and then carefully to take the position and direction of prostration of each and all of the fallen bodies, noting with care, also, any other phænomena which may serve to aid our inquiries. We may thus obtain valuable materials for future analysis; and this course of investigation, if faithfully pursued, will, it is believed, remove all reasonable doubt of the rotative action of these tornadoes. An examination of their probable origin and the causes of their enduring activity and violence, belongs not to the present ocasion.

New York, July 20, 1840.

Schedule of Observations on the Directions of the Trees prostrated by the Tornado of June 19, 1835, *near New Brunswick, N. J. Course of the Tornado, East.*

No.	Direction of Prostration.	Inclination inward and backward.	No.	Direction of Prostration.	Inclination inward and backward.
	Table I. *Left division of the track to the line* b b: *width five chains, or* 110 *yards.*		3.	West.	180
			4.	S. 80° W.	170
			5.	S. 40 W.	130
			6.	S. 40 W.	130
a.	S. 20° W.	110	7.	S. 80 W.	170
b.	S. 80 W.	170	8.	S. 10 E.	100
1.	N. 67 W.	157	9.	S. 50 W.	140
2.	South.	90	10.	S. 50 W.	140

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* As in the tornado which passed through Allegany county, New York, July 25, 1838, described by Mr. Gaylord in the American Journal of Science, vol. xxxvii. p. 92.

No.	Direction of Prostration.	Inclination inward and backward.
11.	S. 26° W.	116°
12.	S. 50 W.	140
13.	S. 65 W.	155
14.	South.	90
	16 cases: mean direction S. 47° West, being 47 degrees backward, or 137 degrees inward and backward.	
	Table II. *Left of the centre or axis, from* b b *to* c c: *width one and a half chain, including also* 32 *and* 33, *and excluding* 34 *and* 35.	
15.	S. 2° E.	88
16.	S. 12 W.	102
17.	S. 35 E.	55
18.	S. 62 E.	28
19.	S. 25 E.	65
20.	N. 80 W.	190
23.	S. 20 E.	70
24.	S. 80 E.	10
25.	S. 45 E.	45
27.	S. 45 E.	45
28.	S. 20 E.	70
29.	S. 60 E.	30
30.	S. 60 E.	30
31.	East.	00
32.	S. 75 E.	15
33.	S. 56 E.	34
	16 cases: mean direction S. 35° E., being 55 degrees inward.	
	Mean of two divisions on the left side, 32 cases, S. 6° W., being 6 degrees backward or 96 degrees inward and backward.	
	Table III. *Doubtful or neutral cases; belonging to either side.*	
35.	East.	00
36.	N. 85° E.	05
37.	East, two cases	00
	4 cases: mean direction E. 1° N.	
	Table IV. *On the right of the axis from* c c *to* W E: *width three chains, including* 34, *and excluding* 32, 33, 36, *and* 37.	
21.	N. 56° E.	34°
22.	N. 60 E.	30
26.	N. 10 E.	80
34.	N. 80 E.	10
38.	East	00
39.	N. 30 E.	60
40.	N. 70 E.	20
41.	N. 55 E.	35
42.	N. 50 E.	40
43.	N. 78 E.	12
44.	N. 45 E.	45
45.	N. 45 E.	45
46.	N. 25 E.	65
47.	N. 35 E.	55
48	N. 40 E.	50
	15 cases: mean direction N. 51° E., being 39 degrees inward from course.	
	Table V. *On the right of axis from* W E *to* a a: *width four and a half chains.*	
49.	N. 67° E.	23
50.	N. 45 E.	45
51.	N. 22 E.	68
52.	N. 3 E.	87
53.	N. 30 E.	60
54.	N. 10 E.	80
55.	N. 35 E.	55
56.	North	90
57.	N. 10° E.	80
58.	N. 3 E.	87
59.	N. 45 E.	45
60.	N. 10 E.	80
61.	N. 35 E.	55
62.	N. 60 E.	30
64.	N. 40 E.	50
65.	N. 20 E.	70
66.	N. 10 E.	80
67.	N. 20 E.	70
68.	N. 40 E.	50
69.	N. 70 E.	20

No.	Direction of Prostration.	Inclination inward and backward.	No.	Direction of Prostration.	Inclination inward and backward.
70.	N. 50°E.	40°	84.	N. 40°E.	50
71.	N. 35 E.	55	85.	N. 70 E.	20
72.	N. 30 E.	60	86.	N. 23 E.	67
73.	N. 50 E.	40	87.	N. 31 E.	59
74.	North (two)	90	88.	N. 20 E.	70
75.	North	90	89.	N. 22 E.	68
76.	North	90	90.	N. 10 E.	80
77.	N. 20° W. (clump) ...	110	91.	N. 55 E.	35
78.	N. 35 W.	125	92.	N. 70 E.	20
79.	N. 30 W.	120	93.	N. 55 E.	35
80.	N. 10 W.	100	94.	N. 68 E.	22
81.	N. 65 E.	25	95.	N. 25 E.	65
82.	N. 70 E.	20		14 cases: mean direction, N. 41° E., being 49 degrees inward.	
	33 cases: mean direction, N.24° E., being 66 degrees inward.			Mean direction of three divisions on the right of axis; 62 cases: No. 33½° East, being an inward inclination of 57 degrees, nearly.	
	Table VI. *Right division of the track from* a a *to the south margin.*				
63.	N. 35°E.	55			
83.	N. 45 E.	45			

VII. *On a new Compound of Chlorine and Cyanogen.*
By James Stenhouse, *Esq.*[*]

THIS somewhat remarkable compound may be obtained by two processes, both of which I shall now minutely describe. The first way in which I obtained it was by decomposing an alcoholic solution of bicyanuret of mercury by dry chlorine gas. The method of proceeding is the following. Four or five ounces of bicyanuret of mercury should be finely powdered, and introduced into a tubulated retort, and the same weight of strong alcohol poured over them. It is proper to agitate the mixture for some time, and even to heat it a little, in order to saturate the alcohol completely with the salt. The retort is then to be kept as cool as possible by being placed in a vessel of cold water, a copious stream of which is to be kept falling upon it during the whole process. The chlorine to be introduced into the solution of the bicyanuret of mercury must be dried, by being passed through an intermediate vessel containing sulphuric acid: it may then be conveniently introduced into the solution by a tube passed

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[*] Communicated by the Author; having appeared in Liebig's *Annalen* for January, 1840.

through the tubulure of the retort, and reaching nearly to its bottom. The current of chlorine is to be sent through the liquid very slowly: if this is not attended to, the temperature rises very high, and the gaseous chloride of cyanogen passes off as fast as it is formed, instead of being absorbed by the alcohol, and the quantity of the compound obtained is exceedingly diminished, if indeed its formation be not wholly prevented. When this is the case heavy muriatic æther is almost the only product; but by careful cooling and cautious evolution of the chlorine this result may be easily prevented.

When the current of chlorine has been sent through for some time, abundance of crystals begin to appear in the retort, accompanied by a violent effervescence. These crystals will be found to be sal-ammoniac, the quantity of which, when there was not much alcohol in the retort, is so great as to convert the whole into a solid mass. If the chlorine is continued to be sent through the liquid after the crystals of sal-ammoniac have appeared, it forms abundance of the heavy muriatic æther, which adheres tenaciously to the chlorocyanous compound, and from which it can be separated only by repeatedly dissolving it in hot alcohol, and precipitating it by water. If any trace of this æther remains adhering to the crystals, it communicates to them its peculiar smell and greasy feel, and lowers their melting point very considerably.

If the alcoholic solution is then treated with water, the sal-ammoniac is dissolved, and the cyanogen compound falls in great abundance in long silver-white needles. If this is done by hot water the crystals form more slowly, and become therefore larger and more beautiful. When the liquid has stood some time the crystals are to be collected and washed upon a filter with cold distilled water, till every trace of acid is removed; they are then quite pure. The salt which remains in solution after the crystallization of the cyanogen compound is not corrosive sublimate, as might be expected; it is the combination of chloride of mercury and sal-ammoniac usually known as the sal-d'Alembroth; it is much more soluble than corrosive sublimate.

I shall now describe the second method, which is much more œconomical.

Strong hydrocyanic acid is first made in the usual way by sulphuric acid and prussiate of potash. It is then to be redistilled, and to be condensed in the alcohol intended to be used, until the latter is saturated with the acid. The other arrangements are precisely the same as in the first process; and the too rapid evolution of chlorine, or the heating of the solution, must be carefully guarded against. The chlorine is continued to be sent through the liquid till the crystals of sal-ammoniac begin to form, which is accompanied by the violent effervescence I have before mentioned, owing to the escape of carbonic acid gas.

The cause of these phænomena is owing to the decomposition of chloride of cyanogen, by its constituents uniting with the elements of four equivalents of water, and giving rise to sal-ammoniac and carbonic acid, as will be readily seen from the following symbols.

$$\begin{array}{l|l} Cl_2\ N_2 & C_2 \\ H_8 & O_4 \end{array}$$

or $C_2\ N_2\ Cl_2 + H_8\ O_4 = C_2\ O_4 + N_2\ H_8\ Cl_2$.

Properties of the Compound.

The compound crystallizes in long, soft, perfectly white needles, of a silvery lustre; they very much resemble sulphate of quinine: it is neutral to test paper; it is tasteless and inodorous; it melts at 140° C, by which heat it is partly sublimed. When heated to 160° C. it is decomposed, and emits a smell resembling that of benzoic æther. It burns easily with a large yellow flame, resembling that of alcohol, and emits no smoke. It is little soluble in cold water, but so much so in boiling as to be deposited in crystals on the cooling of the liquid. In alcohol and æther it is very soluble, but it may be precipitated from either solvent by water: it is deposited in crystals on their evaporation.

When heated with an aqueous solution of potash it is decomposed with the evolution of ammonia, and the solution becomes of a deep brown colour. With a solution of liquid ammonia in the cold there is no action, but it is dissolved on the application of heat, and is deposited unaltered on the cooling of the liquid. Sulphuric acid dissolves it very readily when assisted by a gentle heat; it is not in the least blackened; it does not precipitate on the cooling of the liquid, but water causes the precipitation of it in an apparently unaltered state.

The analyses conducted in the usual way gave the following results.

1st. 0·7902 of the combination gave 1·022 carbonic acid and 0·360 water.

2nd. 0·505 gave 0·658 carbonic acid and 0·227 water.

1st. 0·7925 gave by decomposition at red heat by quick lime 0·833 chloride of silver, equal to 25·930 per cent. chlorine.

2nd. 0·5245 gave 0·555 chloride of silver equal to 26·10 per cent. chlorine.

The nitrogen was determined qualitatively. The carbonic acid was to the nitrogen in the proportion of 8 : : 1 by volume, accordingly 8 : : 2 in atomic proportion.

This gives the following result.

	1.	2.
Carbon	35·760	36·02
Hydrogen	5·038	4·99
Nitrogen	10·350	10·43
Chlorine	25·930	26·10
Oxygen	22·922	22·46
	100·000	100·00

Theoretical composition.

		In 100 parts.
16 at. Carbon	1222·96	35·60
28 at. Hydrogen	174·71	5·08
4 at. Nitrogen	354·08	10·30
4 at. Chlorine	885·30	25·75
8 at. Oxygen	800·00	23·27
	3437·05	100·00

From this formula Prof. Liebig has shown that it may be considered as a combination of 3 atoms aldehyd, 2 atoms chlorcyan, and 5 atoms of water, as follows.

3 at. Aldehyde = $C_{12}\ H_{18}\ O_3$
2 at. Chloride of cyanogen = $C_4\ N_4\ Cl_4$
5 at. Water = $H_{10}\ O_5$

1 at. of the combination $C_{16}\ H_{28}\ O_8\ N_4\ Cl_4$

VIII. *On the Composition of Chelidonin and Jervin.* *By Dr.* HEINRICH WILL*.

THE organic bases employed in the following analyses were prepared and sent to me to be analysed by their discoverers; the chelidonin by Dr. Probst of Heidelberg, and the jervin by E. Simon, apothecary in Berlin.

Chelidonin †.—The chelidonin, whose properties and mode of preparation have been described by Probst, forms a pure white, soft powder, and is also obtained in a crystalline form. When heated it melts into a colourless oily fluid, which becomes brown at a stronger heat; finally it takes fire and burns away with a bright smoky flame, without leaving the least residue. The base dried in the air contains water of crystallization which disappears completely at a temperature of 100

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* From Liebig's *Annalen der Pharmacie* for July, 1840: vol. xxxv. part 1.

† See the Supplementary Number for the present month, vol. xvii. p.543.

centigrade; if heated then till it melts, which it does at 130 cent., it loses no more weight. It is soluble in alcohol and æther, but not in water; a good deal of ammonia is developed when it is melted with caustic potash. A solution of chelidonin in diluted muriatic acid, or the watery solution of the neutral muriate itself, gives with chloride of platinum a beautifully yellow precipitate, at first flaky, but afterwards granular, which may be washed out without decomposition. This precipitate, which is a double salt of muriate of chelidonin with chloride of platinum, was used for determining the atomic weight of chelidonin. This double salt was not decomposed by boiling with nitric acid.

First. 0·448 of chelidonin dried in the air lost at the temperature of 100° centigrade, 0·023 of water = 5·13 per cent.

Second. At 130°, when the chelidonin was melted, 0·172 lost 0·008 of water = 4·65 per cent.; the mean of both experiments is 4·89 per cent.

1. Chelidonin weighing 0·425 dried at 100°, gave 1·062 of carbonic acid, and 0·215 of water.

2. 0·2585 gave 0·643 of carbonic acid, and 0·1315 of water.

3. 0·321 gave 0·793 of carbonic acid, and 0·162 of water.

In a direct nitrogen determination 0·2995 yielded 30 cubic centimeters of nitrogen gas at 11° cent., and height of the barometer 27″ 10‴, which after reduction to standard temperature and pressure gave 2·88 cub. cent. = 12·19 per cent. Hence the following per cent. composition is calculated:

	1.	2.	
Carbon	69·07	68·76	68·30
Hydrogen ...	5·62	5·65	5·60
Nitrogen......	12·19	...	...
Oxygen	13·12		
	100·00		

1. 0·307 of chelidonin-chloride of platinum dried at 100° left when burnt 0·0535 of metallic platinum = 17·42 per cent.

2. 0·3125 gave 0·055 of metallic platinum = 17·60 per cent.

From these two determinations the numbers 7076·8 and 7008·5, come out as the atomic weight of the double salt, and therefore for the chelidonin 4502·89 and 4434·9, which determination, particularly the last, agrees as well with the following formula as can be expected from such weighing, where a difference of half a millegramme increases or diminishes the atomic weight 70 or 80 parts.

The foregoing determination of the water of crystallization gives exactly 2 atoms of water to 1 atom of chelidonin.

The theoretical composition of the chelidonin, in the calculation of which the hydrogen was taken as agreeing very near, perhaps too near with what was found by experiment, is the following:

		In 100 parts.
40 at Carbon	3057·40	68·90
40 at Hydrogen	249·59	5·62
6 at Nitrogen	531·12	11·97
6 at Oxygen	600·00	13·51
1 atom of anhydrous chelidonin =	4438·11	100·00
2 atoms of water	224·96	4·82

1 atom of crystallized chelidonin =4663·07.

The chelidonin-chloride of platinum consists of 1 equivalent of muriate of chelidonin.

	Calculated.		Found. 1.	Found. 2.
	4893·24	69·78	70·08	69·70
1 of chloride of platinum ...	2118·80	30·22	29·92	30·21
	7012·04	100·00	100·00	100·00

or

		In 100 parts.	Obtained. 1.	Obtained. 2.
1 equivalent of chelidonin	4438·11	63·29		
1 do. platinum	1233·50	17·59	17·42	17·60
3 do. chlorine	1327·95	18·94	...	...
1 do. hydrogen	12·48	0·18	...	...
	7012·04	10·000		

The composition of the chelidonin stands in remarkably close relation to that of narcotin, if we retain for the latter as the truer the older formula given by Liebig:

Chelidonin is ... ... $C_{40}\,H_{40}\,N_6\,O_6$
Narcotin ... ... $C_{40}\,H_{40}\,N_2\,O_{12}$

It is evident that in chelidonin with an equal number of atoms of carbon and hydrogen, the oxygen of the narcotin is wholly or in part replaced by nitrogen. Experiments to change one into the other remained without result. If we dissolve narcotin in a solution of ammonia in alcohol, and keep the mixture some time near boiling, it crystallizes again, on cooling, with its properties unchanged. If chelidonin is melted with caustic potash and a little water so that it is not perfectly decomposed, it may be obtained unchanged when neutralized with an acid and precipitated by ammonia. The small quantity of the bases did not permit me to make further experiments.

For comparison I place here the per cent. composition of narcotin and chelidonin near one another.

Carbon ……	68·90	65·27
Hydrogen …	5·62	5·32
Nitrogen……	11·97	3·78
Oxygen … …	13·51	25·63

The plants which yield these two bases belong to the same family, the Papaveraceæ.

Jervin.—This vegetable base, the general properties of which and mode of preparation have already been described in vol. xxiv. p. 214 of the *Annalen der Pharmacie*, is obtained along with veratrin and sabadillen from the root of *Veratrum album*.

The analysed jervin was white and crystalline; when heated on a slip of platinum it melted to an oily fluid, almost as clear as water, which at a greater heat became brown, took fire, and was consumed with a smoky flame, but without leaving any residue. When melted with caustic potash ammonia is developed. It is almost insoluble in water, but dissolves in spirits of wine; with acetic acid it forms a salt soluble in water. The sulphate muriate and nitrate of jervin are scarcely soluble in water, and in mineral acids. The acetic solution of jervin is precipitated in copious flakes by the last-named acids and by ammonia, which redissolve only with difficulty, even in a great excess of the precipitant. Jervin bears in an oil-bath a temperature of 190° centigrade, without being decomposed, but above 200° it becomes brown and is decomposed. With chloride of platinum, jervin, like all organic bases, forms a double salt, which is best obtained by precipitating the acetic solution of jervin with an acid solution of chloride of platinum, or by replacing the alcohol solution of the acetate of jervin by chloride of platinum, and then evaporating it and washing the salt with water; the double salt precipitated from the acetate of jervin forms a beautifully bright yellow substance, easily washed without being decomposed, which was employed in the analysis given below.

1. 0·218 of jervin dried in the air lost at a temperature of 100°, 0·015 of water = 6·88 per cent.

2. 0·4575 heated to 130° lost 0·0315 of water = 6·88 per cent.

1. 0·370 jervin dried at 100° gave 0·319 water, and 1·0165 carbonic acid.

2. 0·203 gave 0·178 water and 0·555 carbonic acid; 0·398 at 17°·8 centigrade, and 27″ 6‴ of barometer gave 17·4 cubic centimeters of nitrogen gas = 16·11 centigrade, and at 0° centigrade, and 28 Rhenish inches of barometer.

This gives for the composition of jervin in 100 parts,

	I.	
Carbon	75·96	75·60
Hydrogen ...	9·57	9·74
Nitrogen......	5·38	5·38
Oxygen	9·09	9·28
	100·00	100·00

To find the atomic weight the platinum in the double salt was determined by burning. It melted, became black, and burned away with flame.

I. 0·1237 gave 0·018 of metallic platinum = 14·55 per cent. As the atomic weight we deduce for the double salt the number 8476·83; for the muriate of jervin 6358·03, and for the anhydrous base 5902·90.

II. 0·094 of the double salts gave 0·0135 of metallic platinum = 14·33 per cent. Hence the atomic weight of the double salts 8588·81; of the muriate of jervin = 6470·01, and of the jervin = 6014·88.

If we calculate after these data the formula of the jervin, we get the following theoretical composition:—

			In 100 parts.
60 at	Carbon	4586·10	76·41
90 —	Hydrogen	561·57	9·36
4 —	Nitrogen	354·08	5·89
5 —	Oxygen	500·00	8·35
1 at	Anhydrous jervin	= 6001·75	100·00
4 —	Water	= 449·92	6·97

1 at Crystallized jervin = 6451·67.

For the platinum double salt we get the following composition:—

	Calculated.		Obtained. 1.	Obtained. 2.
1 equ. muriate of jervin	6456·88	75·29	75·34	75·01
1 — chloride of platinum	2118·80	24·71	24·66	24·99
1 equ. muriate of platinum and jervin .... =	8575·68	100·00	100·00	100·00

or also

		Calculated.	Found. 1.	Found. 2.
1 equ. jervin . .	6001·75	69·98	——	——
3 — chlorine	1327·95	15·49	——	——
1 — platinum	1233·50	14·39	14·55	14·33
1 — hydrogen	12·48	0·14	——	——
1 equ. of the double salt	8575·63	100·00.		

Among all the organic bases hitherto analysed, there is none to which the jervin stands related as to its composition.

IX. *On some new and curious numerical Relations of the Solar System.* By S. M. Drach, *Esq.*[*]

In every system of bodies circulating round a comparatively much greater one possessing a rotary motion, the stability of the orbital revolutions cannot be insured unless the secondaries be placed at distances from the primary greater than the limit of equilibrium between the gravitating and centrifugal forces of the latter; otherwise the secondaries would ultimately inevitably fall on the primary, causing great devastation to both. This limiting distance, so important an element in the solar and planetary systems, seems hitherto to have been overlooked, although the distances of the planets from the sun, and of the satellites from their primary, are connected with it in a remarkable manner, as the following investigation will show.

Denoting by m, r, p, q, the relative mass, equatorial radius, period of rotation, and surface gravity of a planet with respect to the earth; and putting $Q = \frac{\text{centrifugal force}}{\text{gravity}}$ at the surface of our planet, and q = the similar quantity at the surface of another, we have

$$q = \frac{r^3}{m p^2} \cdot Q. \quad \ldots\ldots\ldots \quad (1.)$$

Supposing the planets spherical and homogeneous, and putting δ = relative density, (1.) becomes

$$q = \frac{1}{\delta p^2} \cdot Q \quad \ldots\ldots\ldots\ldots \quad (2.)$$

The *maximum possible* equatorial radius of the limiting surface $= \sqrt[3]{\frac{1}{q}}$, and the corresponding polar one was found

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[*] Communicated by the Author.

by Laplace = $\frac{2}{3}$ of the former; the planetary ellipticities are confined between $\frac{q}{2}$ and $\frac{5}{4}q + \frac{75}{14}q^2$, &c. According to M. Poisson (*Méc.* i. p. 367.), $Q = \frac{1}{288{\cdot}908}$.

With data furnished by the Ladies' Diary for 1837, the following table was constructed:

St.	q^{-1}.	$q^{-\frac{1}{3}}$.	Eq. rad. in miles.	Rel. Pol. Rad.	Limits Ell^y.
☉	48205·39	36·3942	16153281	24·2628	$\frac{1}{38564}$ & $\frac{1}{96410}$
☿	758·935	9·121	13609	6·081	$\frac{1}{606}$ & $\frac{1}{1518}$
♀	282·604	6·562	25006	4·375	$\frac{1}{224}$ & $\frac{1}{565}$
♁	289·908	6·619	26225	4·413	$\frac{1}{231}$ & $\frac{1}{580}$
♂	201·684	5·864	12380	3·909	$\frac{1}{161}$ & $\frac{1}{403}$
♃	12·476	2·319	100027	1·546	$\frac{1}{11}$ & $\frac{1}{25}$
♄	5·497	1·765	72319	1·177	$\frac{1}{6}$ & $\frac{1}{11}$
♅	$63{\cdot}22p^2$	$3{\cdot}98p^{\frac{2}{3}}$		$2{\cdot}65p^{\frac{2}{3}}$	

This table suggests the following remarks:

1. The first four planets rotating nearly in equal times, q is for them nearly as the density inversely, and $\sqrt[3]{\frac{1}{q}} \propto \delta^{\frac{1}{3}}$.

2. With the single exception of Venus, the value of the relative radius appears to decrease as we recede from the sun according to some function of the distance.

3. The oft-named limiting distance of a planet (16 millions of miles): distance of the first planet Mercury (37 millions) $::1:2.27::1:2\frac{1}{3}::3:4+3$ nearly, which is probably the true reason of 3 and 4 being the *fundamental* numbers of Bode's law, viz.

$$4 \quad , \quad 4 + 2^i . 3 \quad \text{from } i = 0 \text{ to } i = 6.$$

4. These researches have evolved to me a simpler and more universal law, embracing the comets, and secondary systems

of our planetary world. The four periodic comets have for their semi-axes major (Bowdich, *Méc. Cél.*, vol. iii.).

Encke's	2·224346	Olbers's	17·7
Biela's	3·53683	Halley's	17·98705.

Putting the distance of Jupiter from the sun = 121, I find

Limit	3·946	$= 2^2$	Juno	62·09	$= 8^2$
Mer.	9·005	$= 3^2$	Cer.	64·38	
Venus	16·33	$= 4^2$	Pal.	64·51	
Earth	23·26	$= 5^2$	Bie.C.	82·28	$= 9^3$
Mars	35·45	$= 6^2$	Jup.	121·00	$= 11^2$
En. C.	51·74	$= 7^2$	Sat.	221·9	$= 15^2$
Vesta	54·94		Ol.C.	411·8	$= 20^2$
			Hal.C.	418·8	

Hers. 446·2 $= 21^2$.

The greatest deviation from the exact *square* $= \frac{1}{11}$. Hence the periodic times are nearly as the *cubes* of the natural numbers; and the squares of the times, as also the cubes of the distances, are expressible by the *sixth* powers of the series 1, 2, 3, 4, &c. very nearly.

For Jupiter's system (Baily, Ast. Tab.),

Limit	2·319	= 25·0	or 5^2
1st Sat.	6·049	65·2	8^2
2nd	9·623	103·6	10^3
3rd	15·350	165·4	13^2
4th	26·998	291·0	17^2.

For the Saturnian system (Baily), diam. ring $= \frac{38''{\cdot}42}{16''{\cdot}20}$ = 2·3716 diam. Sat.

Limit	1·765	= 26·5	or 5^2
Ring	2·372	35·4	6^2
1st Sat.	3·351	50·0	7^2
2nd	4·300	67·2	8^2
3rd	5·284	78·9	9^2
4th	6·819	101·7	10^3
5th	9·524	142·2	12^2
6th	22·081	329·6	18^2
7th	04·359	960·5	31^2.

For the Herschelian system (Baily).

Limit (unknown).

1st Sat.	13·120	= 96·0	or 10^2
2nd	17·022	124·3	11^2
3rd	19·845	145·2	12^2
4th	22·752	166·5	13^2
5th	45·507	333·0	18^2
6th	91·008	665·9	26^2.

The period of this planet's rotation being probably between 6^{h} and 12^{h}, $\sqrt[3]{\frac{1}{q}}$ is between 1·5 and 2·5, and the corresponding square is 4^2.

5. As every body *within* the limit *must* fall on the planet, we may suppose the planetary sphere to extend to the limit, and by Kepler's law, the rotation of a body at the limit of the

Sun	= $25^{\mathrm{d}}{\cdot}78$	Saturn's limit	$8^{\mathrm{h}}{\cdot}6472$
Earth	= $1^{\mathrm{d}}{\cdot}0086$	Do. ring	$13^{\mathrm{h}}{\cdot}4588$
Jupiter	= $10^{\mathrm{h}}{\cdot}0824$	Mean of do.	$11^{\mathrm{h}}{\cdot}052$

All of which are nearly coincident with the *actual* times of rotation, and from these last the true value of q and r may be found for every moon-attended planet.

6. The greater the number of satellites, the more is the danger that from some internal explosion or external shock a moon might be precipitated upon the surface of its primary; hence the number of satellites must have influenced the relative or actual limit. The relative distance from the limit to the surface of the star, is for the

Sun	=35·3942	Jupiter	=1·319
Earth	= 5·619	Saturn	=0·765.

And our planet having one moon, Jupiter 4, and Saturn 7,

$$1{\cdot}319 \times 4 = 5{\cdot}276,\ \frac{7}{4}\,(0{\cdot}765) = 1{\cdot}338,\ 0{\cdot}765 \times 7 = 5{\cdot}355,$$

the results being nearly = the terrestrial and joval distances. If this be correct, the sun must have 35·394 ÷ 5·619 = 6 to 7 satellites (primary planets) capable of causing any *serious* damage, which is the exact number of the large or older planets.

7. If t denote the time, z the height above a planet's surface (radius = 1), the equations of motion give

$$\frac{d^2 z}{d t^2} = -\frac{g}{(1+z)^2} + g\,q\,(1+z) = 0 \text{ at limit.}$$

$$\text{Whence } \frac{d z^2}{d t^2} = \frac{2g}{1+z} + g\,q\,(\overline{1+z}^2 - 1) = g\,(3\,q^{\frac{1}{3}} - q). \quad (3.)$$

At the limit where $q\,(1+z)^3 = 1$; g being the gravity in parts of the radius, eq. (3.) expresses the velocity to send a body up from the surface of the planet, or the velocity with which a body let fall from the limit will impinge on the surface; this is per second for the Sun 1,316,500 feet, Mercury 38,000 feet, Venus 16960, Earth 17397, Mars 7858, Jupiter 144,080, and Saturn 103,800 feet.

8. The axis of the planet Venus being inclined at the singularly low angle of 15° to its orbit, its torrid zone extends

to 75° latitude from its equator, and its frigid zone from the pole to 15° latitude, so that Venus's arctic circles are nearer the equator than its tropic, and it must, therefore, have very hot summers and very cold winters, mitigated, however, by the sun's rapid change of declination, and by its year being only two-thirds of ours. Mercury's torrid zone extends only to 7° latitude, its frigid zone commences at 83° latitude, so that 76° of latitude are in the temperate zone, which must diminish the burning heat at that planet very considerably*. The tropics of the other planets being at less than 30° latitude, cause no great difference between them and the earth in this respect.

9. The sun is the principal cause of the difference of mean annual temperature between our pole and equator; therefore as this action diminishes with the planet's distance from the sun, the further the planet, the more equable is its surface-temperature, the less is the elasticity of its atmosphere disturbed, and consequently the calmer is the atmosphere, so that the diminished heat and light is compensated by the greater purity and stillness of the atmosphere, allowing those agencies to be more effective.

London, Aug, 10, 1840. S. M. DRACH.

P.S. The sun's relative limit less unity = 35·3942, which divided by 7 (the number of planets capable of causing *serious* injury) gives 5·0563. Dividing this last number by 1, 4, and 7, we nearly get the limits (less 1) of the Earth, Jupiter, and Saturn. Applying this principle to Uranus, we have

♅ and ☉, 6 : 1 :: 5·056 : 0·843
⊕, 6 : 1 :: 5·619 : 0·936
♃, 6 : 4 :: 1·319 : 0·879
♄, 6 : 7 :: 0·765 : 0·892

whereof the mean – 0·887. Hence 1·887 is nearly the relative limit of ♅ $= 3{\cdot}984\ p^{\frac{2}{3}}$ $p = 0{\cdot}32597$ sidereal day = 8 hours nearly, so that ♅ rotates on his axis in 7½ to 8 hours. Also $q = \frac{1}{6{\cdot}7}$; his ellipticity is therefore between $\frac{1}{3}$ and $\frac{1}{13}$. But the Earth's and Jupiter's actual oblateness is nearly a mean between the limits; therefore from analogy the oblateness of ♅ $= \frac{1}{5}$ nearly.

Sept. 4, 1840. S. M. D.

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* This diminution would be materially assisted by an ocean, whose great evaporation would reduce and nearly equalize the temperature.

X. *On the surprising intensity of current of the Zinc-Iron-circuit, its causes, and some allied subjects. By Prof.* J. C. Poggendorff *.

THE author read before the Berlin Academy † of the recent discovery of Mr. Martyn J. Roberts,‡ that iron, combined with zinc and dilute sulphuric acid, produces a considerably more energetic current than, under like circumstances, the far more negative copper. He in the first place communicated some observations in confirmation and extension of this fact, both interesting in a scientific point of view and important for the praxis. He showed that the superiority of the current of the *zinc-iron circuit* does not merely occur on employing dilute sulphuric acid, but also with dilute nitric acid, solutions of caustic potash and common salt, and similar fluids; and indeed not solely in reference to the *zinc-copper circuit* (even one with double copper surface), but likewise in reference to circuits of *zinc* and *silver*, or *zinc* and *platina*. It was, in fact, necessary to give to the zinc-platina circuit plates three times as large as those of the zinc-iron circuit, if its current was to be of like intensity with that of the latter. On the other hand, it was found that a Daniell circuit, in which, as is well known, the copper is placed in a solution of the sulphate of copper, and the zinc separated by membrane, developes, in acid, the size and distance of the plates being the same, a greater intensity of current than the zinc-iron circuit; while, on the other hand, that a combination constructed after Daniell's circuit, namely, a circuit in which the iron is placed in a solution of the sulphate of the protoxide of iron, produces a current of very inconsiderable energy.

The author then passes on to the explanation of these phænomena, respecting which Mr. Roberts has offered no opinion, and which, indeed, according to the views at present entertained in England on galvanism, could hardly be given in a satisfactory manner.

And yet the explanation has not far to be sought for. We have long known, says the author, that the intensity of a current of a voltaic circuit depends on two things,—the electromotive force and the resistance. We know, moreover, that it is the quotient from the division of the former by the latter.

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* Translated and communicated by Mr. William Francis, from Poggendorff's *Annalen*, vol. l. p. 255.

† This notice is taken from the Reports of the Royal Berlin Academy, and must be considered merely as a preliminary notice with reference to the main subject; a more accurate numerical determination of the elements treated of will follow in a short time.

‡ Announced by Mr. Roberts in L. and E. Phil. Mag., vol. xvi., p. 142.

Now the electromotive force between zinc and iron is, it is true, smaller than that between zinc and copper, silver or platina, which is evident from this, that when it is combined in the opposite sense with one of the last-mentioned circuits into a system, it is immediately overpowered by it; but that resistance in two circuits which are alike in all except in the nature of the plates, constitutes the sole or essentially different element; viz. the resistance of transition is, as Fechner has shown, in general likewise small in metals which are attacked by the liquid of the circuit. If now, as might hence be supposed, this resistance is smaller in iron with acids and solutions of salts than in copper, and indeed in still greater proportion smaller than the electromotive force between zinc and iron, in comparison to that of zinc and copper, it is evident that, all other circumstances being equal, the current of the zinc-iron circuit must be stronger than that of the zinc-copper circuit.

If, however, the iron circuit owes its greater intensity of current to the smallness of its resistance of transition, then its current must possess a slighter tension than that of the copper circuit, or in other words, it must be weakened by the insertion of a foreign resistance of some consideration in a greater proportion than that of the latter circuit. The testing of this circumstance must decide as to the correctness of this explanation.

The author has undertaken such an examination, it is true, in want of an apparatus of measurement, only with the help of a common galvanometer, which for quantitative determinations is an ill-suited instrument, but will however afford for the present case a sufficient approximation. The presupposition was made that the intensity of the current must be proportional to the tangent of the deviation. This presupposition is evidently false: the force increases in a greater proportion than the tangent of the angle of deviation, since the needle on rotation goes further from the convolutions of the wire. But exactly because the force increases in greater proportion than the tangent of the angle of deflection,—the stronger forces, therefore, were estimated in a greater proportion too small than the weaker,—the conclusions drawn from the measurements under that presupposition must deserve the more confidence.

The plates employed were all of equal size, 1 inch broad, dipped 2·5 inches into dilute sulphuric acid, and stood 5 lines from each other. The current of the two circuits, that of zinc-iron and that of zinc-copper, were successively measured in the above-mentioned manner under two circumstances; once when the circuit was merely closed by the 11-foot long and $\frac{1}{5}$-line thick multiplier wire; and then, when to this a wire of German silver, about 50 feet in length and of the same thickness, the resistance of which is nearly equal to that of a 50-feet long copper wire of the same thickness, was added. To render the galvanometer suited for the measurement of forces of some intensity, a similarly directed position was given to its needles.

In three experiments made at different times, the intensity of the current corresponding to the smaller resistance, which may be expressed for each by 100, sunk, by the insertion of the greater resistance, in the following proportion:

Copper-circuit.	Iron-circuit.
100 : 17·6	100 : 12·3
100 : 19·6	100 : 14·4
100 : 17·2	100 : 13·6

The greater weakening current of the iron circuit is here so evidently expressed, that no doubt can remain as to its reality, especially if we consider that it is the intensity of the current of this circuit which, as the greater, in both cases, and especially in the smaller resistance, has been estimated too low more than that of the copper circuit. It may, therefore, be regarded as good as proved by these measurements, although they are but approximations, that the ascendency of the intensity of the current of the iron-circuit is founded on the smallness of its resistance of transition.

In the just-mentioned measurements, the current of the iron circuit was more weakened than that of the copper circuit by the interposition of the 50-feet long German silver wire. It remained, nevertheless, always considerably stronger than the latter, as will be evident from the following comparison of the intensities of the current of both circuits:

Iron : Copper with smaller resistance.	Iron : Copper with greater resistance.
267 : 100	187 : 100
237 : 100	175 : 100
225 : 100	178 : 100

But it is evident that we can, by continued increase of the inserted resistance,—presupposing naturally that it be both as to size, immersion, and distance of the plates, equal for both circuits,—at last arrive so far as to make the current of the iron circuit not only as weak, but even weaker than that of the copper circuit.

The author has attempted to test the theory even on this interesting point, but he did not succeed, with the means at his disposal, with *equal* and indeed with the *stated* size of the plates, even in attaining an equality between the intensity of current of both circuits. The currents, it is true, can be weakened very easily, and in any degree, by the interposition of a long column of liquid, but they then become unmeasurable if the multiplying (multiplicatorischen) means be not increased in the same proportion. A multiplier, with 16 to 20,000-feet long copper wire, such as employed by Fechner, is an essential requisite for such inquiries.

On the other hand, it will be perceived that the ascendency of the current of the iron-circuit will sink in that measure more, when we, under otherwise like circumstances, take the plates of both circuits larger, but still remaining equal; but evidently a considerable size of the plates will be requisite to render the current of the copper circuit more powerful, or even as powerful as that of the iron circuit.

And therefore it is that the discovery of Mr. Roberts is of importance in the practical application. With moderate size of plates and moderate resistance, which may already at present be foreseen, we may in all the usual circuits and piles substitute iron with considerable advantage for copper, both with respect to œconomy as well as to action; and the more so in piles or many-membered circuits, as the ascendency of the zinc-iron combination over that of zinc-copper must increase with the number of plates.

The author, intending subsequently to communicate more accurate measurements on this as well as on allied subjects, is induced, however, to observe, that the iron circuit, although not affording a constant current, still possesses, in so far, a not inconsiderable advantage over the copper circuit, as the intensity of the current decreases with it slower than with the latter.

Lastly, the iron-circuit is therefore of much interest, for the theory, in as far as it perhaps exhibits more distinctly and decidedly than any other known phænomenon the presence and influence of the resistance to transition still doubted by so many*. There is a whole series of similar phænomena, espe-

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* Even the author was formerly inclined to suspect in the resistance to transition, at least partially, an effect of the discharge, which, indeed, actually occurs in many observations not entirely foreign to the above. But experiments made by him on the passage of the currents of Saxton's machine through fluids, in which a discharge in a perceptible degree cannot have place, convinced him subsequently, most decidedly, of the existence of such a resistance. The first interposition of a thin layer of fluid in the circuit produced a resistance at least four to five times as great as the doubling of this layer, as could be observed by means of an air-thermometer, and a peculiar, well-adapted arrangement for the accurate measurement of resistances. That, moreover, the resistance of transition with iron, even in a solution of caustic potash, a liquid which does not dissolve this metal, is smaller than with copper, evidently proves that this resistance is not always in inverse ratio to the chemical attack, which, indeed, has long

cially in the contrary sense, *i. e.* cases where weakness of current is connected with considerable magnitude of the electromotive force; but one which points to the cause so clearly as the iron circuit, might perhaps not easily be found a second time.

Hitherto the author has only succeeded in discovering one pair, and that moreover less prominent, analogous to the zinc-iron circuit. This is a circuit of amalgamated zinc and common zinc. This produces (the size and distance of the plates being as above, charged with diluted sulphuric acid, and merely closed by a multiplier-wire 11 feet long) a more powerful current, than under similar circumstances, one of amalgamated zinc and cadmium or tin. Since cadmium and tin are more negative than zinc, consequently the electromotive force of the latter circuits greater than that of the former, it is evident that the greater intensity of current of this can likewise solely be founded on its slighter resistance to transition. It is likewise conceivable that, from the smallness of the electromotive force of a circuit of amalgamated and common zinc, there can be but few combinations with the first metal as positive member which are inferior to it in intensity of current. And this is really the case. Already a circuit of amalgamated zinc and brass or copper produces, notwithstanding its greater resistance to transition, a more powerful current.

Another fact belonging here, is that the amalgamated zinc, which, from its being considerably more positive than the non-amalgamated, is usually regarded as far more effective than the latter (which has even given rise to special explanations), does in fact only produce a current of less great intensity than the non-amalgamated, when it, as this, is connected with the circuit by a negative metal, a dilute acid and a connecting wire of moderate resistance. The current from amalgamated zinc and copper is in so slight a degree stronger than that from non-amalgamated zinc and copper (all other circumstances in both circuits being alike), that a momentary removal of the plates from the acid, or any other mode of opening the latter circuit, suffices to give to this in intensity of current the ascendency over the former*. This equilibrium even follows of itself after no very long time. The advantage of employing amalgamated zinc in the construction of voltaic piles, consists, therefore, almost solely in no metal being uselessly expended: little is gained in effect by it.

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since been known from its decrease in platina, by moistening this metal with nitric acid.

* This, as well as the previously-mentioned comparisons, were made with the so-called *differential-galvanometer.*

The author remarks, in conclusion, that amalgamated iron (which is obtained without difficulty by immersing iron in a solution of the chloride of mercury, or in metallic mercury to which a dilute acid has been added), combined with zinc and acid, produces a considerably weaker current than, under similar circumstances, the non-amalgamated iron: it is, however, still much more powerful than that from a zinc-copper circuit. Amalgamated iron is less attacked than the non-amalgamated by dilute sulphuric acid, and is somewhat more *negative.* The latter circumstance, connected with the silvery lustre of the amalgamated iron, shows distinctly the insufficiency of the position lately advanced by Vorsselman de Heer, that amalgamated zinc is more positive than the non-amalgamated, from its surpassing this in lustre; an assertion which, if not already disproved in general by the well-known action of alloys, of which just as many stand *below* and *above* as *between* their constituents in the electromotive scale, would be contradicted by the fact that non-amalgamated zinc still always remains considerably negative towards the amalgamated, even when it has acquired the greatest possible degree of lustre by filing. An inquiry respecting the order of succession of various easily oxidizable metals, in the amalgamated and non-amalgamated state, gave the following result, enumerated from the positive to the negative: *amalgamated zinc, zinc, cadmium, amalgamated cadmium, amalgamated tin, amalgamated lead, lead, tin, iron, amalgamated iron.* Of these five metals, therefore, three, viz. *zinc, tin, lead* (the latter, however, but very slightly), are in the amalgamated state *more positive,* two, on the contrary, *cadmium* and *iron, more negative* in this state than in the non-amalgamated.

XI. *Additional Observations on the Voltaic Decomposition of Alcohol. By* Arthur Connel, *Esq., F.R.S.Ed., Professor of Chemistry in the United College of St. Salvator's and St. Leonard's, St. Andrew's. Read at the Meeting of the British Association in Glasgow.*[*]

IN the course of the experiments by which I endeavoured to establish, some years ago[†], that under powerful voltaic agency, the water entering into the constitution of absolute alcohol is resolved into its elements, the hydrogen being evolved at the negative pole, and the oxygen going to the positive, where it is usually employed in producing a second-

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[*] Communicated by Sir David Brewster.

[†] Transactions of the Royal Society of Edinburgh, vol. xiii. and Lond. & Edinb. Phil. Mag. 1835.

ary action, although under particular arrangements it separates and becomes visible;—I showed that the galvanic agency was greatly increased by dissolving minute quantities of potash in the alcohol, so small a quantity as $\frac{1}{10\cdot000}$th part having a decided influence; this effect being, as I conceived, due to an increased conducting power of the liquid, and to the disposing affinity of the potash for the products of the secondary action of the oxygen. Notwithstanding the very insignificant quantity of the dissolved alkali which was capable of producing this result, and the circumstance that the voltaic decomposition took place without its aid, as well as by adding saline substances which do not contain water, and that the quantity of hydrogen collected greatly exceeded that contained in the water of the dissolved potash, it was objected in more than one quarter, that the increase of effect was due to the water of the hydrate of potash employed. It afterwards occurred to me that every cavil might be obviated by employing small quantities of potassium instead of potash. It is known that when potassium is dissolved in alcohol, it is oxidated with evolution of hydrogen; and when the solution is carried to saturation, a solid compound is obtained of anhydrous potash and æther. Hence, if a minute portion of potassium is dissolved in absolute alcohol, a corresponding quantity of anhydrous potash will remain in solution; and thus we can increase the proportion of the dissolved body as we think proper without the fear of adding water. Accordingly, it was found that minute quantities of potassium had precisely the same effect as minute quantities of potash in promoting the voltaic agency. The quantities of elastic fluid collected in such experiments are necessarily small, because a considerable electric action is required; and when we carry the power beyond a certain point, the action is so violent that the liquid is thrown into ebullition, and the foils are soon left bare.

About a drachm of alcohol (sp. gr. ·7918, at 66° F.), with a quantity of potassium dissolved, equivalent to $\frac{1}{266}$ of anhydrous potash, yielded, under the action of seventy-two pairs of four-inch plates, 0·8 cubic inch of hydrogen from the negative pole in 1^{h} 5^{m}; and when the dissolved anhydrous potash was $\frac{1}{133}$, 0·99 cubic inch were obtained in the same time. In these experiments the greatest energy of the battery was not brought into action, as is done by placing the platinum foils parallel to one another. Their extremities were simply approached to one another in the alcohol. After an hour's action the evolution of gas slackened considerably; so that after two and a half hours' action, the quantity collected was about 1¼ cubic inch from the beginning. But by re-charging the battery, and adding a little more potassium to the same alcohol, we can renew the effect. Thus in an experiment where, with a quantity of potassium dissolved equivalent to $\frac{1}{266}$ of anhydrous potash, 1·15 cubic inch of hydrogen was obtained, and the discharge had much slackened, as much more potassium was added to the same alcohol, and the charge of the battery was renewed. The flow of gas then became as rigorous as ever, and in two hours from the renewal 1·04 cubic inch of additional hydrogen was obtained, or 2·19 cubic inches from the commencement of the experiment; and after some hours' longer action, $2\frac{1}{2}$ cubic inches in all were collected. It is thus evident that, by successive additions of small quantities of potassium and renewals of the charge, we might decompose any proportion of the water, entering into the constitution of the alcohol, we think proper. In all these experiments similar secondary products of oxidation were formed at the positive pole as formerly detailed.

In repeating, on a larger scale, the experiments in which a comparison was instituted between the quantity of hydrogen evolved from alcohol and that evolved by the same current from water, I found that several circumstances require to be attended to. The platinum foil poles ought not to be placed parallel to one another in the alcohol containing dissolved potash or potassium, because a portion of the hydrogen, from being produced in near proximity to the other pole, enters into the constitution of the secondary products of oxidation, and the quantity evolved is thus diminished. And even when the extremities only of the foils are approximated to one another, and the negative is kept uppermost, there still is a certain diminution of the hydrogen liberated, unless where other circumstances of powerful action are employed. Thus where $\frac{1}{266}$ of anhydrous potash was in solution, and seventy-two pair of four-inch plates were employed, the quantity of hydrogen liberated from water, acidulated with $\frac{1}{12}$ of sulphuric acid within the first hour, was usually about one-quarter greater than that from the alcohol; and if the action was longer continued, the difference continued to increase. The cause of this I conceive to be twofold; first, that a part of the hydrogen, unless when very rapidly liberated, is absorbed by the liquid and enters into the constitution of the substances formed at the positive pole; and second, that when the solution diminishes considerably in conducting power the electric action diminishes also; a result which may be imitated by passing the same current through distilled water, and water acidulated with sulphuric acid, when the hydrogen liberated from the pure water will be found to be notably less than that from the well-conducting fluid.

To obtain a satisfactory result, we ought to dissolve a somewhat larger quantity of potassium in the alcohol, and employ a still more energetic electric current, and foils approached to one another in the same plane, when the quantities of hydrogen liberated from the two fluids will be found to be very nearly equal in amount; and the comparison is best instituted during the early stages of the action, because the potash gets saturated with the secondary products of oxidation, and the conducting power diminishes, whilst the electric energy is also on the decline.

I still consider the experiments, now and formerly detailed, to afford the only direct experimental proof which we yet have of the existence of water, as such, in absolute alcohol. The oxidation of potassium in alcohol, with evolution of hydrogen, is perhaps the nearest approach of other experiments to such evidence; but still the tendency to form æther, and the affinity of potash for that substance, might give rise to the evolution of hydrogen. But it appears to be quite impossible, on electro-chemical principles, to suppose, that under electric agency a part of the hydrogen of the alcohol should go to one pole, and the remainder, with its other constituents, to the other, there being no evidence of any *one* substance at the positive pole, which could have been in combination with hydrogen as an electrolytic compound; and, besides, we have direct proof that it is oxygen which passes to the positive pole, because the effects which take place there are effects of *oxidation*, and under particular arrangements that oxygen becomes even *visible*. We have thus oxygen going to the one pole and hydrogen to the other, and the latter in the proportion in which it exists in water; and it is only a truism to say that the direct voltaic decomposition of any substance cannot take place, unless that substance previously has a distinct and substantive existence.

XII. *On the Electricity of Effluent Steam.* No. II. *By* W. G. ARMSTRONG, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN concluding my last communication on the above subject*, I alluded to experiments which had then been commenced to try the effect of insulating the boiler and entirely condensing the issuing steam. These experiments have since been completed, and many others have also been tried which subsequently suggested themselves, all of which I shall now proceed to describe.

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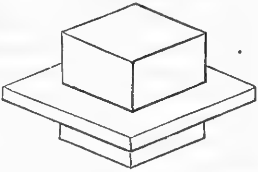
* See our last Number, vol. xvii., p. 452.

The insulation of the boiler was effected by lifting the engine with screw-jacks, until the wheels were raised about six inches above the rails, and then supporting it upon four insulators which rested upon logs of timber. Each insulator consisted of three separate pieces of baked wood, coated with pitch, and having layers of pitch and brown paper placed between them. The middle piece was made larger than the other two, so as to project beyond them, and thereby increase the surface without adding to the height of the insulator, which would have been dangerous; and the three pieces when put together formed a block, of which a representation is annexed (fig. 1.).

As soon as the engine was placed on the insulators, the boiler was filled with water, and the fire lighted, and as the pressure gradually rose in the boiler, the steam was occasionally suffered to escape.

Fig. 1.



The engine indicated no electricity whatever so long as the steam was confined in the boiler, but became negatively electrified as soon as any escape was permitted. A very trifling escape proved sufficient to render the negative electricity of the boiler sensible, and when the steam was very freely discharged the negative development became exceedingly powerful. The sparks never much exceeded an inch in length, but were very large and brilliant, and, owing no doubt to the magnitude of the body from which they were drawn, they produced effects fully equal to those obtained by the use of an average-sized Leyden jar. Some idea of their potency may be formed from the fact, that when not more than half an inch in length, they easily ignited a piece of cotton wool filled with powdered resin.

The greatest care was taken to ascertain whether the extent of the electrical development was at all dependent upon the *density* of the steam in the boiler; and it was found that when the discharge from the valve was so regulated in the different trials as to render the actual quantity or *weight* of steam ejected in a given time as uniform as possible, the negative electricity of the boiler increased *a little* with the pressure, but the positive electricity, drawn by the pointed conductor from the issuing steam, increased *enormously* as the density of the steam was augmented. After the fire had been extinguished at the close of the experiments, and the pressure had subsided to six or eight pounds on the square inch, sparks could no longer be obtained from the conductor held in the steam, but the negative electricity of the boiler continued to produce sparks until the steam was entirely exhausted, or as nearly so as possible. These results appear to indicate that a jet of high-pressure steam is not in reality much more electrical than one of low-pressure steam, but merely that the electricity of the high-pressure jet is more easily collected.

The insulation of the boiler undoubtedly had the effect of diminishing the positive electricity obtained from the steam, but not to such an extent as might have been anticipated.

A glass tube A (fig. 2), with a cock C affixed to it, having been inserted in the boiler in the manner described in my last letter, another glass tube B, about four feet long, was attached to the cock, and supported at the end furthest from the boiler by a glass rod fixed in the insulating stool. A small brass cylinder D having a number of pointed wires projecting from one end into the inside, as shown in the section (fig. 3),

Fig. 2.

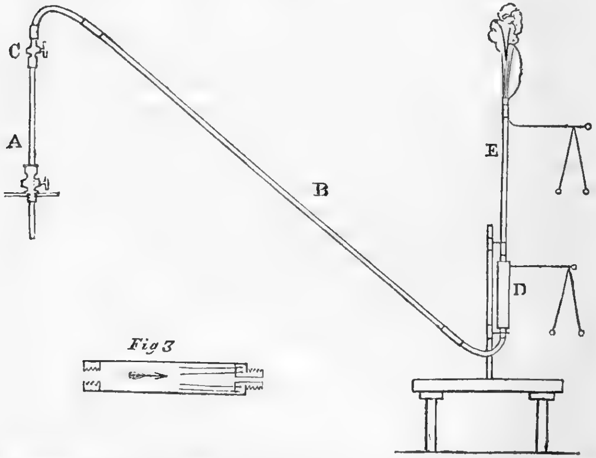


Fig 3

was then joined to the glass tube B, and to this cylinder was added a third glass tube E, so as to extend the channel through which the steam was to be conveyed. A wire with forked points was affixed to the top of the tube E for the purpose of collecting the electricity of the jet, and from this wire a pair of pith-balls was suspended. Another pair of pith-balls was in like manner suspended from a wire screwed into the brass cylinder, all which arrangements will be clearly understood by reference to the figure. The cylinder, and a great part of each of the tubes, were enveloped in flannel, so as to prevent condensation as much as possible. The chief object of this apparatus was to test the electrical condition of the steam before it issued into the air, both when the boiler was insulated and when it was connected by a conductor with the earth. An iron bar was then placed against the engine to establish a communication between it and the ground, and the cock being opened, a current of steam mixed with water issued from the tube E. Under these circumstances both pairs of balls remained stationary, the electricity being probably carried off by the water which escaped with the steam. As the tubes became heated the quantity of water discharged with the steam was reduced to a mere spray, and both pairs of balls then slightly diverged with positive electricity, the upper pair expanding rather more than the lower pair; but upon partially closing the cock both pairs of balls expanded fully *three times as much* as they had done when the cock was fully open; and then gradually converged, arriving at their original position after the lapse of about a minute.

I was at first exceedingly puzzled how to account for this singular effect of attenuating the steam, but I now think it may be explained as follows. It is probable that the moisture of the steam when the cock is fully open is sufficient to enable it to conduct great part of the electricity of the jet back again to the boiler; but that as soon as the temperature of the steam is reduced by the attenuation below the temperature which the tubes have previously acquired from the high-pressure steam, its dryness is so far increased as to prevent the transmission of electricity from the jet to the boiler, but not from the jet to the cylinder, which is a much nearer object. As the tubes cool down to an equality with the steam the dampness returns, and the electricity is again in a great measure carried back to the boiler.

The iron bar was then removed from its contact with the boiler so as to restore the insulation, and the engine was rendered intensely negative by a copious emission of steam from the safety-valve. The cock being again fully opened, both pairs of balls diverged strongly with *negative* electricity; and by diminishing the escape from the valve to a certain point, and then partly closing the cock, the upper balls indicated *positive*, and the lower ones *negative* electricity. When the escape from the valve was entirely prevented and the cock fully opened, both pairs of balls remained in a collapsed state, but on partly opening the cock they both diverged for a short time with positive electricity to much the same extent as they had previously done while the boiler communicated with the earth by means of the iron bar.

It is important to state that when the balls were repelled with *negative* electricity, they collapsed very considerably when the cock was touched by the hand, but when they diverged with *positive* electricity no effect was produced by touching the cock, which strongly favours the supposition that the *positive* electricity manifested by the lower pith-balls, was conducted to the cylinder from the jet.

A coil of lead pipe, immersed in a glas jar filled with wet snow, was then placed upon the insulating stool and connected with the tube B, after the brass cylinder had been detached; and the iron bar was again placed against the boiler to suspend the insulation. Upon opening the cock to the full extent, little or no electricity could be discovered in the coil, but when the cock was partly closed, positive electricity appeared for a short time and then faded away exactly as in the experiments with the brass cylinder. Upon removing the connexion between the boiler and the earth and raising the valve the coil became highly negative, but upon closing the valve the negative electricity vanished.

I have little doubt that the predominance of the *negative* over the *positive* electricity in the above experiment is attributable to the conducting power of the steam causing more negative electricity to be conveyed to the coil from the boiler, than the coil would acquire if the steam were a non-conductor. When negative electricity was in like manner so strongly manifested by the pith-balls in the experiments with the brass cylinder, it was noticed, that after closing the cock, while the interior of the tubes were bedewed with moisture, scarcely any negative electricity was transmitted to the balls, so that the conducting power must have been in the steam, and not in the mere dampness on the glass.

A vertical glass tube, about an inch in diameter, and between two and three feet long, was then screwed on to the cock in substitution of the tube B. The lower part of this tube contained a number of pointed wires for the purpose of abstracting and imparting to the cock any electricity which the steam might possess on entering the tube. When the cock was now fully opened, flashes of light began to dart through the whole length of the tube, from the cloud above it to the cock, and continued to do so as long as the cock remained open, both when the boiler was insulated, and when it was connected with the earth. The steam in the tube was perfectly transparent, and no moisture could be seen on the inner surface of the glass.

The visible transmission of electricity from the jet to the cock, in this experiment, furnishes convincing proof that the positive electricity which we find in the jet is not developed until the steam assumes the form of a visible vapour; and it shows also that the steam, even in its transparent state, is, as I have already surmised, a tolerable conductor of electricity.

I will now venture to offer such an explanation of the electrical phænomena of effluent steam, as I conceive to be most consonant with the experiments I have described in this and my preceding communication.

Independently of the experimental proofs which have been adduced of the neutral state of the steam up to the instant of its transformation into an opake vapour, the water and the steam must be so thoroughly intermixed in the boiler of a locomotive engine, as to render it impossible for the steam to become electrified with positive electricity without immediately imparting it to the water.

I assume, then, as a fact, established both by reason and experiment, that the steam is in a neutral state in the boiler; and I think I am equally supported in saying, that it does not exhibit positive electricity, after leaving the boiler, so long as it retains its aeriform nature.

We learn from experiment that a development of negative electricity in the boiler accompanies the emission of the steam; and since the negative development in the boiler is obviously independent of the subsequent condensation of the steam, it follows, that if the effluxion of the steam could be effected without permitting any condensation to take place, we should then have a development of *negative* electricity in the boiler without any simultaneous development of *positive* electricity; and in like manner if the ejected steam were subsequently condensed into water, we should then have a liberation of *positive* electricity without a concomitant liberation of *negative* electricity.

These conclusions are entirely incompatible with the theory of two electric fluids, but are quite reconcileable with the hypothesis of a single fluid. It seems perfectly rational and consistent with analogy to suppose, that the immense augmentation of volume which takes place when water expands into steam of any ordinary density, should occasion a greatly increased capacity for electricity; and consequently, that the quantity of electricity which suffices to produce a neutral or saturated state in water, should be quite inadequate to sustain that condition when the water is converted into steam. Upon this supposition the steam as it forms in the boiler will absorb electricity from the adjacent conductors in order to acquire a *neutral* or *saturated* state, and when by condensation it again becomes water, the electricity thus absorbed will necessarily be set free; and hence the positive electricity which we find in the jet.

Upon the same principle, if the boiler be insulated, the water, the boiler, and the uncondensed steam, will all be rendered negative, provided the steam be permitted to escape, but not otherwise; for if the steam be confined within the limits of the boiler, the evaporation will not be attended with any *increase of volume*, and the absorption of electricity will in consequence be prevented. In all these respects the theory exactly accords with observation.

I am bound, however, to admit that the explanation which I have here advanced, involves certain conditions which appear somewhat at variance with experiment. In the first place the condensation of a given weight of *low-pressure* steam ought, if the absorption of electricity depends upon increase of volume, to liberate more positive electricity than the condensation of an equal weight of *high-pressure steam*; and in the second place, the expansion of steam from one degree of density to another should, on the same principle, be accompanied by a development of negative electricity. Notwithstanding, however, what has been advanced in favour of high-pressure steam containing more electricity than steam of low density, I think it quite possible that the reverse of this may be the fact; for it is not at all improbable that a low-pressure jet may conduct so large a portion of its electricity back again to the boiler as to make it appear to liberate less electricity than a high-pressure jet, while in reality it may develope a great deal more. It is quite reasonable to suppose that the conducting power of steam should be increased by rarefaction; and besides, in order to make a fair comparison between the quantities of electricity liberated by two jets of steam, it is indispensable that they should discharge equal *weights* in equal times, and to effect this condition it is necessary to discharge a much larger *volume* when the steam is of low pressure, than when it is of great density, and by so doing we unavoidably increase the quantity of unvaporized water swept out of the boiler in conjunction with the steam, and thus improve the communication between the jet and the boiler, and at the same time cause a considerable dispersion of the electricity.

With regard to expansion producing negative electricity, I have certainly never detected such a result; but I much question whether in any of the above experiments expansion was effected without an aqueous condensation sufficient to produce a countervailing effect; or the insulation was ever sufficiently perfect to prevent feeble negative electricity being carried off, or overwhelmed by the positive electricity developed in the jet.

Mr. Nicholson has zealously cooperated with me in the experiments I have described, and it is our intention to make a further attempt to clear up the difficulties which still embarrass the subject.

The limits of this letter will not permit me at present to enter upon any discussion on Dr. Schafhaeutl's interesting paper which appeared in your last publicatlon.

Newcastle-upon-Tyne, WM. GEO. ARMSTRONG.
Dec. 19, 1840,

XIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Anniversary Address of the Marquis of Northampton, *President, November* 30, 1840.

GENTLEMEN,

IN addressing you at the termination of this, the second year that I have had the honour of presiding over your Society, my first duty is to return my thanks to those gentlemen whom you have nominated to be my Council. They have rendered an onerous duty comparatively light and easy by their unremitting attendance and zeal; and have, as I trust, prevented your affairs from suffering from any incompetency on my part. In making my report of the transactions of the last year, I know of nothing to regret, except the loss of some of our valued Associates, who between the end of last November and the present time have paid the debt of nature.

Among the new members enrolled in our body, it will perhaps be right to mention the name of the Bishop of Norwich, as being President of the Linnean Society, one of the oldest branches that may be considered as thrown off from our parent stem. Also that of the Duke of Richmond, as President of the two newest Associations founded for the promotion of science, the Royal Agricultural and Botanical Societies. We have one new member of still higher rank, who has honoured us by becoming one of our Fellows, H. R. H. Prince Albert, the consort of our beloved Queen and Patroness. As your organ, Gentlemen, I will venture to say that you duly appreciate the honour conferred on yourselves; at the same time while His Royal Highness gratifies us by joining our body, we entertain no doubt that he does so from the just conviction that the patronage and advancement of science are national objects of the deepest importance.

The Antarctic Expedition, to whose departure I adverted in my last year's Address, is, I trust, now successfully pursuing its career of scientific research. Already a portion of the fruits of its labours has reached us, and promises an abundant and valuable harvest. The fixed Magnetic Observatories on the territories of Her Majesty are now also in full operation; while foreign powers have given us the assistance of observations made at the same instant of time, and on the same system as our own. I have the satisfaction to state, that these observatories exceed forty in number *.

While these researches are being made by our own and other states, private enterprise is not idle. Mr. Enderby, to whom geography is already indebted, has sent out a vessel for the purposes of discovery in the Antarctic seas, with the object of approaching as near as may be to the Southern pole. His ship is navigated by Mr. Mapleton, an officer who had been selected by Her Majesty's Government to take a part in Captain Ross's expedition; but he had not returned to England before that Expedition had sailed. We may well hope that he will merit this double confidence, and that he will, if his life be spared, add another wreath to the laurels of England, won by a Parry and a Ross, a Cooke and a Vancouver.

You are aware, Gentlemen, that previously to the departure of the late expedition, the Council of the Royal Society was requested by the government to draw up a statement of the most desirable objects in science to which the attention of the officers employed might be directed. With this request, as you might also know, the Council immediately complied; and in the execution of the duty thus devolved on us, the assistance of the Scientific Committees was our principal means of success. Since that time it has occurred to us, that the same recommendations, in rather a different shape, might be of great use to other scientific travellers. We have accordingly taken considerable pains in perfecting these suggestions, which we have caused to be printed for the public in general †. We have already furnished copies to the Commander of the Expedition to the Niger, and I hope that, in addition to his higher objects, he may be enabled to promote our acquaintance with the details of the geography and natural history of those imperfectly known parts of the globe.

I have the satisfaction to inform you, that by desire of the Council Mr. Shuckard has completed a Catalogue of the valuable manuscript letters in our library, among which are many from Ray, Willoughby, Newton, Boyle, Hook, and other eminent men, which we trust may serve as a useful aid to those Fellows who may wish to consult documents of so much interest and value.

You will also be glad to know that Mr. Halliwell, one of our Fellows, has undertaken and executed the task of making a catalogue of the miscellaneous manuscripts in our library; a labour for which I am sure you will feel much indebted to the author. In this collection the Society possesses one most valuable manuscript, the *Principia* in the hand-writing of the immortal Newton.

The Royal Society, Gentlemen, was founded for the advancement of natural knowledge, not for any purposes of private advantage or vain glory. It must, therefore, always hail the foundation and

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[* See L. & E. Phil. Mag. vol. xiv. p. 137; also vol. xv. p. 224; for an account of the instrument and mode of observation to be employed in the magnetic observatories founded by the British government: also vol. xvi. p. 598, xvii. pp. 144, 418.—Edit.]

[† A portion of these recommendations, relating to physics and meteorology, will be found in L. & E. Phil. Mag. vol. xv. p. 177.—Edit.]

prosperity of new bodies of scientific men, brought together by the same object in particular branches of science, either in the abstract, or connected with important arts and manufactures. It cannot but rejoice, therefore, at the continued prosperity of the British Association, and in the formation of new societies for Microscopical Research*, and for the improvement of Botany and Agriculture.

With respect to the last of these, we must look with satisfaction to every effort to carry the torch of science, to light up the intricacies and doubts of an art, the first probably in age, and certainly the first in importance to civilized man. As to the Botanical Society, we cannot but be glad that the want of a public and national Garden should be in some measure supplied, and we may indulge a hope that the example may, at a future time, lead our Government to provide an establishment commensurate with the wealth of Great Britain, with the magnitude of the metropolis, and with the extensive colonial empire from which it might be supplied with the productions of the most varied climates.

With the continued and increasing prosperity of the British Association, I may the more boldly call on you to sympathize, as the greater part of its most active and talented members are also among the most valued of our own Fellows. It had this year a great additional merit in the very numerous attendance of the scientific natives of other countries, and it gave to those among us who were present at Glasgow the opportunity of becoming personally acquainted with some of our Foreign Members; with an Encke, and an Agassiz. The latter gentleman, having fortunately extended his visit to England, we have now the pleasure of seeing him at our Anniversary; a pleasure that we should have probably lost, had he not been attracted by the meeting of the British Association.

During the last year we have had more than one occasion of testifying our grateful and loyal attachment to our Royal Patroness. Another now presents itself, which I am sure you will gladly seize, to express our joy at the birth of a Princess, our gratitude to Heaven for our Sovereign's safety, and our fervent prayers for her long and prosperous reign over the hearts as well as persons of her subjects.

Since we last assembled our room of meeting has been ornamented, as you will see, Gentlemen, by the portrait of our late Royal President, from the pencil of Mr. Phillips. I am sure that you will be gratified by this addition to the likenesses of the far greater number of my predecessors. Future successors of the present Fellows of the Royal Society will look with satisfaction at the striking representation of a Prince, who has added to the many favours received by us from his illustrious house, that of deigning to preside over our Meetings and our Councils.

Of the papers that have been read at our Meetings, it is not necessary for me to speak. Of their merits you will be enabled to judge in a considerable degree by the Abstracts contained in our printed Proceedings; while those that have appeared to the Council,

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[* The foundation of the Microscopical Society is noticed in L. & E. Phil. Mag. vol. xv. p. 549.]

assisted by the Scientific Committees, to be of the most importance, will be found in the Philosophical Transactions. As a new proof of which I cannot, however, refrain from adverting for a moment to one topic,—the discovery of Photography, for which we are indebted to a Neipce, a Daguerre, and a Talbot*; and which not only promises important results to art, but valuable assistance also, by the application of an Ibbotson and others, to those branches of knowledge which are connected with organic matter. This is, however, not all. Through the interesting observations of Sir J. Herschel and Mr. Hunt, followed up, as they will doubtless be, by other philosophers, a vista seems to open into hitherto unexplored regions of science†.

I have the honour, Gentlemen, to inform you that the Council have awarded the Royal Medals for the present year to Sir John F. W. Herschel and Professor Wheatstone; one Copley Medal to Professor Liebig, and another to M. Sturm; and the Rumford Medal to M. Biot.

SIR JOHN LUBBOCK.

It is to me a cause of the highest satisfaction that I have to present through you this Royal Medal to Sir John F. W. Herschel, being the fourth Medal that has been awarded to him by the decision of different Councils.

It is to myself and to the other Members of the Council a most gratifying circumstance, that in an invention of great interest to art, he has found an instrument capable of making us acquainted with most curious laws regulating the chemical action of the different rays of the spectrum on the same substances. They have an additional interest to us as well as to you, in reminding us of researches made by the honoured parent whose example of scientific zeal he so well emulates. The observations that Sir J. Herschel has made in his recent communication, appear likely to lead to new discoveries in optical as well as chemical science; and we trust that the papers already before us may be the forerunners of still more striking and important results.

PROFESSOR WHEATSTONE.

It is with great pleasure that I address you, and present to you one of those Medals that our Royal Patroness has placed at the disposal of the Council of our Society. Your valuable experiments, and the ingenious methods by which you have solved the difficult question of Double Vision ‡, form the immediate, and certainly sufficient motive, for their adjudication. It is, however, still more satisfactory, as it gives me, as their organ, the opportunity to mark their sense of the value of your other discoveries, of the science and ingenuity by which you have measured Electrical Velocity, and by

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[* See L. & E. Phil. Mag. vol. xiv. p. 196.—EDIT.]

[† An Abstract of Sir John Herschel's paper appeared in L. & E. Phil. Mag. vol. xvi. p. 331; and of a paper by Mr. Hunt in vol. xvii. p. 381; in which volume also, p. 202, appears Mr. Hunt's last paper, referring to others before inserted.]

[‡ Noticed in vol. xiii. p. 416; see also vol. xvi. p. 599.]

which you have also turned your acquaintance with Galvanism to the most important practical purposes.

PROFESSOR DANIELL.

In confiding to your charge, as our Foreign Secretary, the Rumford Medal which the Council has awarded to M. Biot, I am sure that every cultivator of the higher and more abstruse branches of science will feel that it is bestowed on a philosopher, whose optical researches on the curious and interesting subject of Polarized Light are of the highest value. You may, at the same time that you forward the Medal, assure M. Biot of the anxious wish of the Royal Society that he may be long enabled to carry on inquiries so honourable to himself, and so important to more than one branch of science.

Professor Daniell, I hold in my hand, and deliver to you one of the Copley Medals, which has been awarded by us to Professor Liebig. My principal difficulty, in the present exercise of this the most agreeable part of my official duty, is to know whether to consider M. Liebig's inquiries as most important in a chemical or in a physiological light. However that may be, he has a double claim on the scientific world, enhanced by the practical and useful ends to which he has turned his discoveries. I hope that he may long be able to follow at the same time the paths of scientific research and practical utility.

Professor Daniell, I have again to call on you, in your official capacity, to transmit a Medal to the Continent. The gentleman to whom we have adjudged it is M. Sturm, for his valuable mathematical labours, the fruits of which must be important, not only to mathematics, but also to those other high and abstruse sciences to whose advancement algebraical analysis is a necessary instrument. In his solution, therefore, of a problem which has baffled some of the greatest mathematicians that the world has produced, he has well earned the gratitude of every lover of natural knowledge.

You will, Gentlemen, hear read an account of the eminent men connected with our Society whom we have had the misfortune to lose since last November. Having confided the task of enumerating them to one of your Members, more able than myself to do justice to their merits, I shall not further touch upon the subject than to express my deep regret at the decease of one who had been my predecessor in this Chair, and to whose counsel I might have looked for aid in any conjuncture of difficulty, with full reliance on his good sense and ability, and also on his zeal in any matter in which the interests of the Royal Society were at stake. I may also be permitted to express the condolence of all the Members of the Royal Society with the domestic affliction of our valued Treasurer by the decease of his father, who was also one of our Fellows. I will now desire Dr. Roget to read the account of those whom we now miss from our ranks.

The first name in the list of our deceased Fellows, which it is my melancholy duty to notice, is one which cannot be mentioned in this room, without feelings of deep regret for the loss of his services and of affectionate respect for his many virtues: it is hardly necessary for me to add that I refer to our late associate and former President, Mr. DAVIES GILBERT*.

He was the only surviving son of the Rev. Edward Giddy, of St. Erth in Cornwall, and his mother, whose maiden name was Davies, was the representative of several ancient and distinguished families in that county, and the heiress likewise of very considerable property. His early education, which was almost entirely domestic, was chiefly superintended by his father, who was an accomplished classical scholar; and in 1785 he became a gentleman commoner of Pembroke College, Oxford, where he attended the lectures of Dr. Beddoes on chemistry, Dr. Sibthorp on botany, and Mr. Hornby on geometry and astronomy, and devoted himself with very unusual diligence to the study of mathematics and the natural sciences. He used to boast in after life, with very becoming pride, that he was the first student of his class in the University of Oxford, who had ever read the Principia of Newton.

In 1791, he was elected a Fellow of this Society, and became associated from that time forward with the most eminent men of science in the metropolis. He had, in very early life, appreciated the extraordinary combination of poetical and philosophical genius in his friend and fellow-countryman Humphry Davy, at that time in a very humble capacity; and by recommending him, first, as an assistant to Dr. Beddoes in his experiments on the medical effects of gaseous inspirations, and, secondly, to the Royal Institution, he had the merit and good fortune of contributing to rescue from obscurity one of the greatest discoverers in modern chemistry†. In the year 1804, he became Member of Parliament for Helston, and in 1806 for Bodmin, a borough in his own immediate neighbourhood, which he continued to represent until the era of Parliamentary Reform in 1832. He was emphatically the representative of scientific interests in the House of Commons, and contributed by his exertions to carry many very important projects, including amongst them the great breakwater at Plymouth and the bill for the revision of weights and measures; a bill founded upon the report of a commission of which he was a member, in conjunction with Captain Kater, Dr. Young and Dr. Wollaston.

Mr. Davies Gilbert was the author of Papers in our Transactions "On the Mathematical Theory of Suspension Bridges‡," with particular reference to the Menai bridge, which was at that time in progress, and the curvature of which was considerably modified in conformity with the results of his calculations:—"On the Progressive Improvements made in the Efficiency of Steam-engines in Cornwall, with investigations of the methods best adapted for imparting great

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[* Another obituary notice of Mr. Davies Gilbert appears in the Supplementary Number for the present month, vol. xvii. p. 531. A third will be given shortly.—EDIT.]

[† See L. & E. Phil. Mag. N. S. iii. p. 57, for Mr. Gilbert's own statement of these circumstances.]

‡ For 1826, Part III. p. 202.

Angular Velocities*," in which he first distinctly defined and made known to men of science what is termed the *duty* of steam-engines, from the correct observation of which so many important practical improvements have followed:—"On the Nature of Negative and Imaginary Quantities†," which contains many ingenious views, although they have been in a great measure superseded by later speculations on this subject‡. Mr. Gilbert was a mathematician of the old school; but the papers to which we have just referred are very creditable specimens of the clearness with which he apprehended the bearing of some simple theoretical truths upon very important practical questions.

He became President of this Society in 1828, on the resignation of Sir Humphry Davy; a situation from which he retired in 1831. He continued, however, for the remainder of his life, to take a prominent part in the concerns of the Society; and there are few of my brother-Fellows, whom I have now the honour of addressing, who have not had opportunities of observing and appreciating his constant zeal for the interests of science, the variety and philosophical character of his conversation, the simple and unaffected eloquence of his public addresses§, and, above all, that sweetness of temper and kindness of heart which beamed forth in the expression of those truly classical and benevolent features, which one of the most accomplished of our artists (himself a brother-Fellow) has so happily perpetuated in the portrait which adorns these walls. The very absence of that inflexibility of purpose and of opinion which some might consider essential to the perfection of the character of a philosopher, seemed, in his case, the proper developement of that natural benevolence and humanity which made him so justly beloved in every relation of life, whether as a husband, a father, and a brother,—as a master, a landlord, and a friend.

Mr. Gilbert was the author and editor of several antiquarian and other works relating to his native county, whose interests he always laboured to promote with more than common zeal and patriotism. He was President of the Cornish Geological Society from the period of its first establishment in 1814, and he never omitted attending its meetings, though on the last occasion he was so weak as to be compelled to resign the chair to his friend and countryman Sir Charles Lemon. In 1808, he married Miss Gilbert, and assumed her name in 1817, on succeeding to a very large property in Sussex. The same simple and unaffected character which distinguished him in public life was still more conspicuous in his domestic relations. He died on the 24th of December last, and his body was

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* For 1830, Part I. p. 121.

† For 1831, Part II. p. 341.

[‡ Abstracts of the two papers here referred to will be found in Phil. Mag. and Annals, N. S., vol. vii. p. 449, vol. ix. p. 37. Mr. Gilbert communicated a paper on the Regular or Platonic Solids, in Phil. Mag. and Annals, N. S. vol. iii. p. 161; and in vol. ix. p. 200, appears his original statement respecting the legacy of the late Earl of Bridgewater.]

[§ Addresses delivered to the Royal Society by Mr. Gilbert, will be found in Phil. Mag. and Annals, N. S. vol. iii. p. 50, vol. vii. p. 33, and vol. ix. p. 39.—Edit.]

borne to the grave by his own labourers, and followed by his widow and family in that primitive and unostentatious form which best suited the simplicity and natural humility of his own character.

Dr. SAMUEL BUTLER, Bishop of Lichfield, was born in 1774, at Kenilworth, which was likewise the birth-place of two other contemporary prelates of our church. He was educated at Rugby, and became afterwards a member of St. John's College, Cambridge, where he gained the highest classical honours which the University could confer. In 1798 he was made Head Master of Shrewsbury School, over which he continued to preside during a period of thirty-eight years. His great acquirements as a scholar, his eminent skill as a teacher, his active interest in the welfare of his pupils, and the tact and knowledge of character which he showed in their management, all contributed to raise the school to the highest reputation, and to give it, during many years, the pre-eminence over every other school in the kingdom in the number and rank of the academical honours which were gained by his scholars. The date of his elevation to the Bench was nearly contemporaneous with the appearance of that fatal disease which, after three years of the most depressing sufferings, borne with most exemplary patience and resignation, brought him to the grave. He was a man of great cheerfulness of temper and disposition, kind, affectionate and generous in every relation of life, and justly the object of the grateful attachment and love of his numerous pupils.

Dr. Butler was the author of an elaborate edition of Æschylus, with the notes and text of Stanly, and of several educational and other works. He formed a very extensive library; and his collection of Aldines, which is unhappily now dispersed, was perhaps the most complete in Europe. One of his last works was an interesting memoir of Dr. John Johnstone, of Birmingham, with whom he had long been connected by the bonds of the most affectionate friendship.

Mr. JAMES PRINSEP, whose brilliant career of research and discovery has been closed by a premature death in the flower of his age, was Principal Assay Master, first of the Mint at Benares, and secondly of that of Calcutta, where he succeeded Professor Wilson in 1833; he was a young man of great energy of character, of the most indefatigable industry, and of very extraordinary accomplishments; he was an excellent assayist and analytical chemist, and well acquainted with almost every department of physical science; a draughtsman, an engraver, an architect, and an engineer; a good Oriental scholar, and one of the most profound and learned Oriental medallists of his age.

In 1828 he communicated to our Society a paper "On the Measurement of High Temperatures," in which he described, amongst other ingenious contrivances for ascertaining the order, though not the degree, of high temperatures, an air-thermometer applicable for this purpose, and determined by means of it, probably much more accurately than heretofore, the temperature at which silver enters into fusion*.

[* See Phil. Mag. and Annals, N. S. vol. iii. p. 129, and vol. x. p. 356, note.—EDIT.]

His activity whilst resident at Benares has more the air of romance than reality. He designed and built a mint and other edifices; he repaired the minarets of the great mosque of Aurengzebe, which threatened destruction to the neighbouring houses; he drained the city and made a statistical survey of it, and illustrated by his own beautiful drawings and lithographs the most remarkable objects which the city and its neighbourhood contains; he made a series of experimental researches on the depression of the wet-bulb hygrometer; he determined from his own experiments the values of the principal coins of the East, and formed tables of Indian metrology and numismatics, and of the chronology of the Indian systems and of the genealogies of Indian dynasties, which possess the highest authority and value.

When transferred to Calcutta, he became the projector and editor of the "Journal of the Asiatic Society of Bengal," a very voluminous publication, to which he contributed more than one hundred articles on a vast variety of subjects, but more particularly on Indian coins and Indian Palæography. He first succeeded in deciphering the legends which appear on the reverses of the Greek Bactrian coins, on the ancient coins of Surat, and on those of the Hindoo princes of Lahore and their Mahomedan successors, and formed alphabets of them, by which they can now be readily perused. He traced the varieties of the Devanagari alphabet of Sanscrit on the temples and columns of Upper India to a date anterior to the third century before Christ, and was enabled to read on the rocks of Cuttock and Gujarat the names of Antïochus and Ptolemy, and the record of the intercourse of an Indian monarch with the neighbouring princes of Persia and Egypt; he ascertained that, at the period of Alexander's conquests, India was under the sway of Boudhist sovereigns and Boudhist institutions, and that the earliest monarchs of India are not associated with a Brahminical creed or dynasty. These discoveries, which throw a perfectly new and unexpected light upon Indian history and chronology, and which furnish, in fact, a satisfactory outline of the history of India, from the invasion of Alexander to that of Mohammed Ghizni, a period of fifteen centuries, are only second in interest and importance, and we may add likewise in difficulty, to those of Champollion with respect to the succession of dynasties in ancient Egypt.

These severe and incessant labours, in the enervating climate of India, though borne for many years with little apparent inconvenience or effect, finally undermined his constitution; and he was at last compelled to relinquish all his occupations, and to seek for the restoration of his health in rest and a change of scene. He arrived in England on the 9th of January last; but the powers both of his body and his mind seemed to have been altogether worn out and exhausted; and after lingering for a few months, he died on the 22nd of April last, in the forty-first year of his age. The cause of literature and archæology in the East could not have sustained a severer loss.

Sir Anthony Carlisle was born at Stillington, in the county of Durham, in the year 1768. After commencing his professional education at York, under the care of his uncle, he became a student at the Hunterian School of Anatomy, in Windmill Street, under Dr. Baillie and Mr. Cruikshank, where he attracted the particular notice of John Hunter. He subsequently became a resident pupil of Mr. Henry Watson, one of the most eminent surgeons in the metropolis, whom he succeeded at the Westminster Hospital in 1793. In 1800 he communicated to our Transactions a paper "On a Peculiarity in the Distribution of the Arteries sent to the Limbs of Slow-moving Animals." This was followed by many others on various points of comparative and human anatomy, including his papers "On Muscular Motion," and "On the Arrangement and Mechanical Action of the Muscles of Fish," which formed the Croonian Lectures for 1804 and 1806. He was the author likewise of many communications in the Transactions of the Linnean and Horticultural Societies and in other contemporary journals, on different branches of natural history and physical science*.

"An Essay on the Connexion between Anatomy and the Fine Arts," led to his appointment, in 1808, to the Professorship of Anatomy to the Royal Academy, a situation which he filled with great advantage to the students during a period of sixteen years.

Sir Anthony Carlisle was not less distinguished for his knowledge of anatomy, physiology, and natural history, than for his professional merits, and for his patience and skill as an instructor of medical students. As a practitioner, he was invariably kind and attentive to those who were entrusted to his care, and eminently liberal in devoting his professional services to those who had no adequate means of repaying them.

Mr. NICHOLAS AYLWARD VIGORS was born in 1787, at Old Leighlin, in the county of Carlow, where his family had long resided. After the usual preparatory education, he proceeded to the University of Oxford, where he became a very diligent and successful student. On quitting the University, he purchased a commission in the Guards, and distinguished himself highly at the battle of Barossa, by continuing to bear the colours of his regiment after he was severely wounded. On his return from the Peninsula, he was prevailed upon, by the earnest entreaties of his family, to quit the army; and he devoted himself afterwards, with characteristic ardour, to scientific and literary pursuits.

Mr. Vigors was one of the founders and the first Secretary of the Zoological Society, to whose museum he gave his very valuable collections of ornithology and entomology, which were the two branches of natural history he had most carefully studied. He was the author of a very elaborate paper in the Linnean Transactions†, "On the Natural Affinities which connect the Orders and Families of Birds," in which he attempted to apply in detail the same principles of arrangement that Mr. MacLeay had previously

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[* In 1832, Sir A. Carlisle communicated to us some official documents respecting the health of workmen employed in the sewers of Westminster and the existence of cholera among them, which were inserted in L. and E. Phil. Mag. vol. i. p. 354.—EDIT.]

† Linnean Transactions, vol. xiv.

sketched out in his *Horæ Entomologicæ*, in a more general way, as applicable to the whole animal kingdom. He afterwards published, in conjunction with Dr. Horsfield, another very valuable memoir* on the Birds of Australia, grounded upon a rich collection from that country, in the possession of the Linnean Society, which they described and arranged according to their natural affinities. He was likewise the principal editor, during several years, of the "Zoological Journal," in which he wrote many memoirs, chiefly devoted to the further exposition of his views with respect to the affinities of birds, but some of them descriptive of new or rare Mammalia, or new forms of exotic insects or birds †.

Mr. Vigors was a man of very considerable attainments as a scholar as well as a naturalist ‡, and made a liberal use of an ample private fortune in the promotion of those sciences which he cultivated: he was the representative in Parliament, for some years before his death, first of the city, and lastly of the county of Carlow.

Mr. RICKMAN was born in 1771, and educated at Westminster School, from whence he proceeded as a student to Christ Church, Oxford. Early in life he was recommended by Dean Jackson as Secretary to Mr. Abbott, the Speaker of the House of Commons, and was chosen to examine and digest the Parliamentary returns under the first Population Act in 1800, a duty which he continued to perform at the three succeeding decennial periods, and was preparing to discharge it for the fifth time during the present year, when he was attacked by the disease which terminated in his death. He was appointed second Clerk Assistant to the House of Commons in 1815, and subsequently Clerk Assistant, an office which he continued to hold for the remainder of his life.

The introductions to the "Population Returns," of which he was the author, are remarkable for the very able analysis which they contain of the general condition, changes and prospects of all classes of the population §.

Mr. Rickman was an excellent classical scholar, and was, in addition to many other attainments, extremely well acquainted with many branches of engineering and practical mechanics. He was the intimate friend, and after his death the executor, of the late Mr. Telford, whose autobiography he published, with a preface and an atlas of engravings descriptive of his principal works, which is in every way worthy of the fame of that great engineer.

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* Linnean Transactions, vol. xv.

[† Mr. Vigors's name has very frequently appeared in our pages, from the time when he first became known as a naturalist to a very recent period. Abstracts of many papers by him occur in the Proceedings of the Zoological Society, beginning in Phil. Mag. and Annals, N. S., vol. ix. p. 54, and continued through that and the present series.—EDIT.]

[‡ Mr. Vigors was the author of "An Inquiry into the Nature and Extent of the Poetic Licence," of which a second edition appeared in 1813, Lond. 8vo.—EDIT.]

[§ A review of the Introductions to the Population Returns (from the pen, we may now mention, of the late George Harvey, F.R.S.), was inserted in L. & E. Phil. Mag. vol. i. p. 213.—EDIT.]

Sir Jeffrey Wyattville, member of the Royal Academy and a distinguished architect, was a member of a family which has long been honourably connected with the arts. He was born in 1766, and acquired a knowledge of his profession under the instructions of his father and uncle, and was subsequently employed, during many years, in the somewhat ambiguous capacity of architect and builder in the execution of many considerable works. In 1824 he was selected by George IV., to whom he had been formerly known, to design and superintend the magnificent alterations and additions to Windsor Castle, a truly royal and national work, in which he succeeded in combining uncommon external grandeur and strict architectural propriety with great convenience and splendour of internal arrangements. Sir Jeffrey Wyattville, besides many important original works, made very extensive additions to the principal mansions of our nobility, including Chatsworth, Longleat, Woburn, Badminton, and Ashridge. He was a man of sound judgment and great integrity, and was very generally beloved for the remarkable simplicity and frankness of his manners, his great kindness of heart, and cheerful and unaffected good humour. He died in February last, and was buried in St. George's Chapel, at Windsor, in a vault which he had himself prepared for the reception of the remains of a beloved daughter, who died in the flower of her age.

Captain Charles Phillipps, of the Royal Navy, was the author of several inventions of great value in navigation, and in the equipment and management of ships: such are his methods of suspending compasses so as to avoid concussions in time of action; his improvement of the pump-dale of ships, and more particularly the capstan, which bears his name, and which is in general use in the Navy. He was an active and enterprising officer, who had seen much service during the last war, had been eminently successful in rescuing slaves off the coast of Africa, and had nearly fallen a victim, in common with the greatest part of his crew, to that pestilential climate.

Sir Robert Seppings received his education as a shipwright under the late Sir John Henslow, Surveyor of the Navy, and continued in connexion with the important service of our dock-yards during a period of fifty years. He was the author of many important improvements in our naval architecture, including his system of diagonal bracing and trussing, which formed the subject of two memorable Papers in our Transactions in the years 1814* and 1818†, and which attracted an unusual amount of public attention. The great principle of this method was such an arrangement of the principal timbers as would oppose a powerful mechanical action to every change of position of the ribs and other timbers in every part of the ship; thus firmly compacting together the entire fabric, and

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[*] On a New Principle of Constructing His Majesty's Ships of War.—Phil. Trans. 1814, p. 28. [See Phil. Mag. First Series, vol. xliii. pp. 228. 305; vol. xlv. p. 374.

[†] On the great strength given to Ships of War by the application of Diagonal Braces.—Phil. Trans. 1818, p. 1.

preventing that perpetual racking of beams and working of joints, which, in the ancient system of ship-building, produced hogging, creaking, leakage, and rapid decay; and filling up likewise every vacuity between the timbers, which were occasionally the unavoidable receptacles for foul air, filth, vermin, and various other sources of rottenness and disease.

These important improvements, though opposed to the inveterate prejudices of the older shipwrights, a body of men who have not sufficiently valued and understood, in this country at least, the just principles of mechanical action, in the practical operation of ship-building, were universally adopted in the Navy under the enlightened administration of Mr. Charles York, and the powerful advocacy of Sir John Barrow*: and the merit of their author was acknowledged by his appointment as Surveyor of the Navy, and by the award of the Copley Medal of this Society.

This was not the only important improvement which Sir Robert Seppings introduced into our system of naval architecture. The Admiralty presented him with £1000 as a reward for his simple yet most useful invention of an improved block for supporting vessels, by which their keels and lower timbers were much more easily and promptly examined and repaired. His plan for lifting masts out of the steps, which superseded the employment of sheer hulks for that purpose, has been the means of saving much expense and labour. His new mode of framing ships has led to a much more extensive use of short and small timbers, which were formerly of little value; but the most valuable of all the reforms of construction for which the Navy of England is indebted to him, was the substitution of round for flat sterns, which afford increased strength to the framework of the ship, greater protection against *pooping* in heavy seas, an almost equal power of anchoring by the stern and by the bow, a more secure and effective position for the rudder, and a stout platform for a powerful battery, embracing a sweep of more than 180°. This capital improvement was strenuously opposed by many distinguished naval officers, who regretted the loss of those magnificent cabins, which were better suited for purposes of state than of service; but the good sense of less prejudiced judges happily prevailed, and secured for our ships of war an additional claim upon the respect of our enemies.

Foreign nations have not been tardy to acknowledge the value of these important improvements, and their author received many substantial proofs of their sense of his merits; and we may safely affirm, that in the national record of the great benefactors of their country, there are few names which will deserve, and, we trust, continue to receive, a more grateful commemoration than that of Sir Robert Seppings.

It has long been the practice of the Royal Society to associate with its body those persons in our country who are most eminent for their high rank or their commanding talents, for their distinguished public services, for their accomplishments in the arts,

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* In very able articles in the 24th and 43rd Numbers of the Quarterly Review.

for their attainments in literature, for the important influence which their virtues or labours may have exercised upon the character and prospects of society, or upon the general interests of humanity; wisely judging that science will gain both in the enlargement of its objects and in the dignity and estimation of its cultivators, by being thus united with whatever is best entitled to command and to receive the admiration and respect of mankind: it is amongst this class of our Members that I have to notice several losses of more than ordinary importance.

The EARL OF MANSFIELD was a nobleman of illustrious family, who, in addition to many other accomplishments, was one of the most elegant and effective parliamentary orators of his day.

LORD HOLLAND was Chancellor of the Duchy of Lancaster, and a nobleman who was remarkable for his profound knowledge of the constitutional history of his country, and for the extent and variety of his literary attainments*. It was the remark of a well-known philosophical author and writer, "that there was something so sweet in the blood of the Foxes, that no one could approach them without feeling the fascination of their social powers:" and there was probably no man of his age who was the object of more enthusiastic love and admiration of his friends, private and political, than Lord Holland.

Sir WILLIAM SYDNEY SMITH was a hero in the most chivalrous period of our naval history, the scenes of whose early triumphs have so recently been rendered illustrious by others of an equally memorable character.

Sir JOHN LUBBOCK was one of those persons engaged in trade whose extensive transactions and liberal views give dignity to the operations of commerce: it is not one of the least distinctions of such a father, that his name and honours have been inherited by one whose profound acquirements in the most difficult branches of science have merited and received the highest honours which this Society is able to confer.

In our foreign list we have to lament the loss of three of our most illustrious members, Blumenbach, Olbers, and Poisson.

JOHN FRIEDRICH BLUMENBACH was born on the 11th of May, 1752, at Gotha, where his father was Prorector of the Gymnasium. He was accustomed to attribute the formation of his taste for literary history and the study of the natural sciences to the instructions and encouragement of Menz and Christ, two professors of Leipsig, who were friends and fellow-townsmen of his father. After studying for some time at Jena, he removed to Göttingen, for the purpose of completing his medical course, where he was very favourably noticed by Heyne and Michaelis, and more particularly by Büttner, Professor of Natural History, a great linguist, and a man of very extraordinary acquirements, whose museum of medals and natural history, when afterwards purchased by the University, he was employed to arrange. The skill and diligence which he showed

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* He was the author of a most elegant account of the life and writings of Lope de Vega, accompanied by some beautiful translations of his more remarkable poems.

in this employment, and the reputation of his professional and other attainments, secured him the appointment of Extraordinary Professor of Medicine in 1776, and of Ordinary Professor in 1778, a situation which he continued to hold for nearly sixty years.

His lectures comprehended Natural History, Comparative Anatomy, Physiology and Pathology, on all which subjects he published many valuable memoirs and other works, more particularly his admirable *Manuals*, which have long enjoyed an extraordinary popularity, and which have been translated into nearly every great European language.

The first of this series of publications was the "Handbuch der Naturgeschichte," which appeared in 1779. In his "Institutiones Physiologicæ," a work equally remarkable for the originality, precision and clearness of its statements, which was published in 1787, he made known his views on the "bildungs trieb," or "Nisus formativus," which he had before announced in the Göttingen Transactions for 1785, and which he made the subject of a special work in 1789*. His "Specimens of the Physiology of Warm- and Cold-blooded Animals," appeared in 1789. In 1794 he published in our Transactions, "Observations on some Egyptian Mummies opened in London in 1792," with especial reference to the three distinct varieties of national physiognomy which appear amongst them. His "Handbuch der vergleichenden Anatomie" appeared in 1805, and showed how fully he already appreciated the important views of Cuvier, which elevated Comparative Anatomy from a merely descriptive science to one which was capable of the most instructive generalizations, and affording the means of distinguishing types and laws of formation, as well for different organs as for different classes of animals.

The term *nisus formativus* was employed by Blumenbach to denote that *vital power* which is innate in all living organized bodies, and in active operation during the whole period of their vital existence, by which they are controlled and modified with reference to a specified end; it is that power by which the organizable matter of every individual being assumes, at its conception, its allotted form; which form is also capable of successive modifications by nutrition, according to the purpose for which it is destined by the Author of Nature, as well as of the reparation (within prescribed limits) of the injuries which it may have received. The announcement of this principle was received with extraordinary favour by physiologists, though it differed in little more than in name from the *vis essentialis* of the celebrated Wolff. It will be found to have formed the basis of some of his important speculations.

Blumenbach's well-known collection of the crania of the different races of mankind was made with a view to their more accurate classification, and gave rise to some of his more celebrated publications†. According to his ultimate views, he would make the

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* Ueber den Bildungs trieb.

† Collectio Decad. vi. craniorum diversarum gentium tabulis 60 æneis illustrata: 1790—1820. De generis humani varietate nativâ: 1795.

Caucasian race the primary stem, from which all the others have degenerated to the Mongol at one extremity, and the Æthiopic at the other, interposing the American variety between the Caucasian and the Mongol, and the Malay between the Caucasian and the Æthiopic: it is difficult, however, to arrive at very correct general conclusions on this very interesting subject, without reference to those which are founded on the analogies of language, as has been done by Cuvier and Prichard.

It is quite impossible, within the short compass to which this notice is necessarily confined, to convey more than a very general impression of the vast variety of the labours of this distinguished philosopher. We find him applying his knowledge of natural history in illustration of the arts and poetry of antiquity*; he was also one of the first naturalists who appreciated the importance of a knowledge of fossils in determining the relative ages of the strata of the earth†. He had cultivated archæology and literary history‡ from his earliest years with more than common interest and zeal. There were, in fact, few departments of knowledge and literature, however remotely connected with the natural sciences, which he has not illustrated by his writings: it was when thus travelling into provinces of knowledge which were somewhat foreign to his own, that he was accustomed to quote the adage of Seneca: "*Soleo et in aliena castra transire, non tanquam transfuga, sed tanquam explorator.*"

Blumenbach had long been considered as the patriarch of the University of Göttingen, and was allowed the full privileges attached to his distinguished reputation, to the memory of his long services, and to the respect due to his venerable old age; he retained his usual cheerfulness, his memory, and much of his ancient activity, until nearly the close of his life. He died on the 22nd of January last, in the 88th year of his age, a memorable proof that the tranquil pursuits of science and the gentle stimulus of constant though not laborious employments are equally favourable to contentment of mind and length of days.

The name of the venerable Dr. Olbers, of Bremen, must be for ever memorable in the annals of astronomy, as the discoverer of two planets in our system. He was a member of that remarkable association of twenty-four astronomers which the indefatigable Baron de Zach of Gotha had formed towards the close of the last century, who undertook the vigilant observation of as many zones of the heavens, with a

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* Specimen historiæ naturalis, antiquæ artis operibus illustratæ eaque vicissim illustrantis: 1803. Com. Acad. Gott., tom. xvi.

Specimen historiæ naturalis ex auctoribus classicis, præsertim poetis, illustratæ eosque vicissim illustrantis: 1815. Com. recent. Acad. Gott., tom. cxi.

† Beiträge zur Naturgeschichte der Vorwelt: 1790. Specimen archæologice telluris terrarumque imprimis Hannoveranarum: 1801. Also Comment. Acad. Gott., tom. xv. p. 132—156. Com. recent. Acad. Gott., tom. cxi. pp. 3—24.

‡ His "Introductio in Historiam Medicinæ Literariam," published in 1786, is a most instructive specimen of scientific bibliography.

general view of discovering new comets and planets, and of recording any remarkable phænomena that might occur. Their zeal in the prosecution of these researches had been stimulated by the recent discovery of Herschel, as well as by the revival of a suggestion made by Kepler of the probable existence of a planet between Mars and Jupiter, in conformity with one of those mystical analogies, which might have been treated as the visionary dreams of an enthusiast, if they had not been so intimately connected with the discovery of the great laws forming the true basis of all correct knowledge of the system of the universe. The absence likewise of a planet at the distance from the sun, represented by 28, that of the earth being 10, interfered with the completeness of an empirical law which Bode of Berlin had suggested, and was not without its influence in confirming their faith in these extraordinary anticipations. The labours of this Association had been hardly organized, when the remarkable discovery of Ceres by Piazzi on the first day of the present century, in almost the precise position which Bode's singular law had assigned to it, seemed at once to convert their dreams into realities. Dr. Olbers calculated a circular, and Gauss an elliptic orbit for the same planet; and so wonderful was the accuracy of the first approximation to the elements which the latter had made, that they enabled Olbers to re-discover it on the 1st of January 1802, exactly one year after it had been first observed. It was in consequence of having formed a configuration of stars in the geocentric route of this planet, with a view to its being more readily found, that he discovered Pallas on the 25th of March of the same year*, at nearly the same distance from the sun†, though moving in an orbit more than three times as much inclined to the plane of the ecliptic. The discovery of two planets, in the position where one of them had been so anxiously sought for ‡, induced Dr. Olbers to conjecture that they were fragments of a larger planet, which had been scattered by some great catastrophe, and that many others probably existed at nearly the same distance from the sun, and possessing common nodes: he therefore earnestly recommended astronomers to observe most carefully those spaces of the heavens in which the nodes of these planets are placed; a practice which he himself observed for many years. His exemplary diligence was rewarded by the discovery of Vesta on the 29th of March, 1807, nearly in the precise position in which he had conjectured that it was most likely to be found§. This was the last of those remarkable discoveries whose history illustrates in so striking a manner that union of profound, yet somewhat visionary speculation, with uncon-

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* "Ueber einen neuen von Dr. Olbers in Bremen endeckten höchst sonderbaren cometen." Zach's Monatliche Correspondenz for May, 1802.

† If the distance of the earth from the sun be 1, that of Ceres is 2·7674, and that of Pallas 2·7676: the difference is less therefore than 19,000 miles.

‡ Their essays on this subject were generally headed, "On the long-expected Planet between Jupiter and Mars."

§ The longitude of the ascending node of Pallas is 172° 32′ 35″; that of Vesta is 171° 6′ 37″.

querable perseverance, which is so characteristic of the German nation.

His well-known method of calculating the orbits of comets, which has been so generally used by German astronomers, was published at Weimar in 1797*, with a commendatory preface by his zealous friend the Baron de Zach. This memoir, independently of its other merits, is sufficient to show that its author was a mathematician of very considerable powers, and perfectly acquainted with the works of contemporary astronomers.

Dr. Olbers was a diligent observer of comets; and there are few astronomers who have contributed so much to our knowledge of these singular bodies. He was the discoverer of several comets, including the celebrated comet of long period of 1815; and we are indebted to him, not merely for very important suggestions and observations respecting the celebrated comet of Encke, but still more for having developed the taste for astronomical calculations and observations of that great astronomer, who for many years served him in the capacity of assistant in his observatory.

The Baron de Zach visited this observatory in September, 1800 †, and has described the simple apparatus which enabled him to make so many important discoveries. It was placed in the upper part of his house in the midst of the town of Bremen, and afforded openings or platforms sufficient to afford a command of nearly every point of the heavens. His instruments were an excellent five-foot Dollond of $3\frac{3}{4}$ inches aperture, with a circular micrometer (which he used in the observation of the small planets), a five-foot reflecting telescope by Schröter, a quadrant by Bird, an admirable sextant by Troughton, and a clock by Castens of Bremen. He possessed no transit instrument or fixed instruments of any kind; yet he speedily availed himself of the circumstances of his locality to determine his time with great accuracy, as well as nearly every element which the peculiar character of his observations rendered necessary; so fertile are the resources of genius and enterprize to overcome difficulties, which by ordinary men would be abandoned as altogether insuperable.

Simeon Denis Poisson, one of the most illustrious men of science that Europe has produced, was born at Pithiviers on the 21st of June, 1781, of very humble parentage, and was placed, at the age of fourteen, under the care of his uncle, M. L'Enfant, surgeon, at Fontainebleau, with a view to the study of his profession. It was at the central school of this place that he was introduced to the notice of M. Billy, a mathematician of some eminence, who speedily discovered and fostered his extraordinary capacity for mathematical studies. In 1793 he was elected a pupil of the École Polytechnique, which was then at the summit of its reputation, counting amongst its professors Laplace, Lagrange, Fourier, Monge, Prony, Berthollet, Fourcroy, Vauquelin, Guyton Morveau, and Chaptal. The pro-

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* Abhandlung ueber die lechteste und bequemste methode die Bahn eines cometen aus einigen beobachtungen zu berechnen.

† Monatliche Correspondenz for Feb. 1801.

gress which he made at this celebrated school surpassed the most sanguine expectations of his kind patron, M. Billy, and secured him the steady friendship and support of the most distinguished of his teachers.

In the year 1800, he presented to the Institute a memoir "Sur le nombre d'intégrales complètes dont les équations aux différences finies sont susceptibles," which cleared up a very difficult and obscure point of analysis. It was printed on the recommendation of Laplace and Lagrange in the *Mémoires des Savans Etrangers*, an unexampled honour to be conferred on so young a man.

Stimulated by this first success, we find him presenting a succession of memoirs to the Institute on the most important points of analysis, and rapidly assuming the rank of one of the first geometers of his age. He was successively made Répétiteur and then Professor of the Polytechnic School, Professor at the Collège de France and the Faculté des Sciences, Member of the Bureau des Longitudes, and finally, in 1812, Member of the Institute.

His celebrated memoir on the *invariability* of the major axes of the planetary orbits, which received the emphatic approbation of Laplace, and secured him throughout his life the zealous patronage of that great philosopher, was presented to the Institute in the year 1808. Laplace had shown that the periodicity of the changes of the other elements, such as the eccentricity and inclination, depends on the periodicity of the changes of the major axis; a condition, therefore, which constitutes the true basis of the proof of the stability and permanence of the system of the universe. Lagrange had considered this great problem in the Berlin Memoirs for 1776, and had shown that, by neglecting certain quantities which might possibly modify the result, the expression for the major axis involved periodical inequalities only, and that they were consequently incapable of indefinite increase or diminution. It was reserved to Poisson to demonstrate *à priori* that the non-periodic terms of the order which he considered would mutually destroy each other; a most important conclusion, which removed the principal objection that existed to the validity of the demonstration of Lagrange*.

This brilliant success of Poisson in one of the most difficult problems of physical astronomy, would appear to have influenced him in devoting himself thenceforward almost exclusively to the application of mathematics to physical science; and the vast number of memoirs and works (amounting to more than 300 in number) which he pub-

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* The publication of this memoir recalled the attention of this illustrious mathematician to a subject which he had long neglected, and gave rise to three of his noblest memoirs. Poisson, in his "Mémoire sur le Mouvement de la Lune autour de la Terre," has not satisfactorily shown that the major axis of the moon's orbit contains no argument of long period amongst the terms which involve lower powers of a certain quantity m, which denotes the ratio of the sun's mean motion to that of the moon, than the fourth; a demonstration of this most important proposition has been given by Sir John Lubbock in the Philosophical Magazine for the present year. [L. E. & D. Phil. Mag. vol. xvii. p. 338.]

lished during the last thirty years of his life, made this department of mathematical science, and more particularly whatever related to the action of molecular forces, pre-eminently his own. They comprehend the theory of waves and of the vibrations of elastic substances, the laws of the distribution of electricity and magnetism, the propagation of heat, the theory of capillary attraction, the attraction of spheroids, the local magnetic attraction of ships, important problems on chances, and a multitude of other subjects, which the time allowed for this notice will not permit me to mention. His well-known treatise on Mechanics is incomparably superior to every similar publication in the clear and decided exposition of principles and methods, and in the happy and luminous combination of the most general theories with their particular and most instructive applications.

Poisson was not a philosopher who courted the credit of propounding original views which did not arise naturally out of the immediate subjects of his researches; and he was more disposed to extend and perfect the application of known methods of analysis to important physical problems, than to indulge in speculations on the invention or transformation of formulæ, which, however new and elegant, appeared to give him no obvious increase of mathematical power in the prosecution of his inquiries. His delight was to grapple with difficulties which had embarrassed the greatest of his predecessors, and to bring to bear upon them those vast resources of analysis, and those clear views of mechanical and physical principles in their most refined and difficult applications, which have secured him the most brilliant triumphs in nearly every department of physical science.

The confidence which he was accustomed to feel in the results of his analysis—the natural result of his own clear perception of the necessary dependence of the several steps by which they were deduced—led him sometimes to accept conclusions of a somewhat startling character: such were his views of the constitution and finite extent of the earth's atmosphere, which some distinguished philosophers have ventured to defend*. It is not in mathematical reasonings only that we are sometimes disposed to forget that the conclusions which we make general are not dependent upon our assumed premises alone, but are modified by concurrent or collateral causes, which neither our analysis nor our reasonings are competent to comprehend.

The habits of life of this great mathematician were of the most simple and laborious kind; though he never missed a meeting of the Institute, or a lecture, or an examination, or any other public engagement, yet on all other occasions, at least in his later years, he denied access to all visitors, and remained in his study from an early hour in the morning until six o'clock at night, when he joined his family at dinner, and spent the evening in social converse, or in amusements of the lightest and least absorbing character, carefully avoiding every topic which might recall the severity of

[* Noticed by Mr. (now Sir John W.) Lubbock, in L. E. & D. Phil. Mag. vol. xvii. p. 469.—Edit.]

his morning occupations. The wear and tear, however, of a life devoted to such constant study, and the total neglect of exercise and healthy recreations, finally undermined his naturally vigorous constitution, and in the autumn of 1838 the alarming discovery was made that he was labouring under the fatal disease of water in the chest. The efforts of his physicians contributed for a time to mitigate the more serious symptoms of his malady; but every relaxation of his sufferings led to the resumption of his labours; and to the earnest remonstrances of his friends, and the entreaties of his family, he was accustomed to reply, that to him *la vie c'était le travail*; nay, he even undertook to conduct the usual examinations of the École Polytechnique, which occupied him for nearly ten hours a day for the greatest part of a month. This last imprudent effort ended in an attack of paralysis, attended by loss of memory and the rapid obscuration of all his faculties; he continued to struggle, amidst alternations of hope and despondency, for a considerable period, and died on the 25th of April last, in the fifty-ninth year of his age.

Poisson was eminently a deductive philosopher, and one of the most illustrious of his class; his profound knowledge of the labours of his predecessors, his perfect command of analysis, and his extraordinary sagacity and tact in applying it, his clearness and precision in the enunciation of his problems, and the general elegance of form which pervaded his investigations, must long continue to give to his works that classical character, which has hitherto been almost exclusively appropriated to the productions of Lagrange, Laplace, and Euler. If he was inferior to Fourier or to Fresnel in the largeness and pregnancy of his philosophical views, he was incomparably superior to them in mathematical power: if some of his contemporaries rivalled or surpassed him in particular departments of his own favourite studies, he has left no one to equal him, either in France or in Europe at large, in the extent, variety, and intrinsic value of his labours.

The last work on which he was engaged was a treatise on the theory of light, with particular reference to the recent researches of Cauchy: nearly two hundred pages of this work are printed, which are altogether confined to generalities, whose applications were destined to form the subject of a second and concluding section: those who are acquainted with the other works of Poisson will be best able to appreciate the irreparable loss which optical science has sustained in the non-completion of such a work from the hands of such a master.

XIV. *Intelligence and Miscellaneous Articles.*

LEPIDOMELANE—A NEW MINERAL.

THIS mineral comes from Pirsberg in Wermeland, Sweden, and appears to be a species of mica differing from others. It has been analysed by M. Soltman in M. Wohler's laboratory.

This mineral is a granular and schistose aggregate of small crystals in scales, which seldom exceed half a line in diameter; their form is irregular, but in some cases it has the appearance of six-sided tables, which approach a regular form; these scales are raven-black, and reflect a very bright green, and yield a green powder. Separately examined they are flat and shining, their lustre is vitreous like the diamond, with very brilliant aggregated faces. They are not translucent, but when very small they are transparent. It is difficult to state their cleavage and flexibility; their specific gravity is 3·; hardness on Mohs's scale = 3. It is therefore harder than the mica of two axes, but less so than the pearly mica; it is rather brittle; it is rather hard to the touch, but less than pearly mica, Heated to redness by the blowpipe, it becomes of a tombac brown colour, with the metallic lustre of magnetic iron ore. It gives a bottle-green colour to borax, dissolves readily in nitric and hydrochloric acid, and the silica remains in soft pearly scales and crystalline form, like the mica of one axis. It yielded by analysis

Silica	37·40
Alumina	11·60
Oxide of iron	27·66
Protoxide of iron	12·43
Magnesia and lime	0·60
Potash	9·20
Water	0·60
	99·49

In some of its characters this mineral resembles a species of mica which M. Breithaupt has described under the name of Siderischer Felsglimmer, or Raben Glimmer; but as the analysis of this has not been given, it is difficult to ascertain whether these two minerals belong to the same mineral species.—*L'Institut*, No. 352.

HYDROTELLURIC ÆTHER. BY M. F. WOHLER.

The existence of this compound is interesting, as affording a fresh proof of the remarkable analogy which exists between sulphur and tellurium, and as showing that the latter substance may, like sulphur, enter into the composition of organic compounds, and replace oxygen in them.

This æther is readily prepared by the double decomposition of sulphovinate of barytes and telluret of sodium; it is sufficient for this purpose to submit solutions of these two substances in water to distillation. The telluret is prepared by the calcination of tellurium, or the native telluret of bismuth with carbonate of soda, mixed with charcoal; and, in order to avoid oxidation, the rough product is immediately conveyed to the retort which contains the boiling solution of sulphovinate. The hydrotelluric æther distils with the water, yielding, at the beginning, much froth in the retort.

This substance is a liquid of a reddish-yellow odour, approaching the colour of bromine, but lighter; it is heavier than water, and this dissolves only a trace of it; its odour is disagreeable, extremely penetrating, and remains for a long time; it partakes of the smell both of hydrosulphuric æther and hydrotelluric acid, and it appears to be very poisonous. Its boiling point is below 212°; it readily takes fire and burns with a brilliant white flame, terminated by a blue which is quite peculiar, and it yields a very thick white vapour of telluric acid. This æther, when exposed to the air, is soon covered with a pellicle, which is at first yellowish, becomes afterwards white, and is eventually converted into tellurous acid. If it be exposed in a vessel to the direct rays of the sun, it immediately exhales vapour, and its oxidation goes on much more rapidly than under the action of diffused light; the action even of pure oxygen gas is, however, never sufficiently energetic to inflame the æther. Nitric acid attacks it rapidly, and dissolves it with the disengagement of nitric oxide gas; if hydrochloric acid be added to the solution, a heavy colourless liquid separates in oily drops; this has not been examined.

According to calculation, hydrotelluric æther should contain 68·53 of tellurium; analysis gave 68·75; in fact, 0·560 of this æther were dissolved in nitric acid, and hydrochloric acid was added; it was then boiled for a long time, after which the addition of sulphurous acid separated 0·385 of metallic tellurium dried *in vacuo*. This product is therefore simple hydrotelluric æther $C^4 H^5 Te$, and is composed of

Hydrogen	5·33
Carbon	26·14
Tellurium	68·53
	100·

The product, corresponding to mercaptan, would contain 81 per cent. of tellurium.—*An. de Ch. et de Phys.*, 75, 215.

METEOROLOGICAL OBSERVATIONS FOR NOV. 1840.

Chiswick.—November 1. Foggy: rain. 2. Showery: fine. 3. Foggy. 4. Dense fog: clear and fine. 5. Rain: fine: rain at night. 6, 7. Rain. 8. Clear and fine. 9. Boisterous. 10. Cloudy and fine. 11. Hazy: rain. 12. Clear. 13. Boisterous with rain; barometer very low. 14. Clear. 15. Fine. 16. Cloudy and windy. 17. Stormy with heavy rain. 18. Heavy rain. 19. Rain: fine. 20. Very fine. 21. Stormy with rain. 22—24. Fine. 25. Dense fog. 26. Sharp frost. 27, 28. Very dense fog. 29. Foggy: fine. 30. Overcast.

Boston.—Nov. 1. Fine: lightning P.M. 2. Stormy. 3, 4. Cloudy. 5. Rain: rain early A.M. 6. Cloudy: rain A.M. 7. Cloudy. 8. Fine. 9. Cloudy: rain early A.M.: rain A.M. 10, 11. Fine. 12. Cloudy. 13. Rain and stormy: rain early A.M. 14. Cloudy. 15. Fine. 16. Cloudy: rain early A.M. 17. Cloudy: rain A.M. and P.M. 18. Fine. 19. Cloudy: rain A.M. and P.M. 20. Fine. 21. Rain and stormy. 22. Cloudy: rain early A.M. 23. Rain: rain early A.M. 24—30. Fine.

Applegarth Manse, Dumfries-shire.—Nov. 1. Dull, cloudy, and moist. 2. Showery all day. 3. Very fine. 4. Wet nearly all day. 5, 6. Occasional showers. 7. Wet A.M.: cleared up. 8. Slight showers. 9. Rain A.M.: cloudy all day. 10. Rain A.M.: moist P.M. 11. Thick fog all day. 12. Cloudy and watery. 13. Wet and stormy. 14. Fair after rain during night. 15. Fair and fine: frosty morning. 16. Rain and high wind. 17. Fair but dull and threatening. 18, 19. Frost: clear. 20. Thaw: cloudy. 21. Heavy rain A.M.: cleared. 22. Fair all day. 23. Rain early A.M. 24. Fair throughout and warm. 25. Drizzling day. 26. Frost. 27. Frost: very hard. 28. Frost: threatened change. 29. Cloudy: drizzling. 30. Moist without rain.

Meteorological Observations made at the Apartments of the Royal Society 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, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1840. Nov.	Barometer.						Thermometer.									Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dum-fries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8¾ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·644	29·592	29·337	29·25	29·52	29·40	47·2	53·7	44·9	56	46	46	52½	42½	E.	NE.	calm	E by S.	...	·30	...	...	47
☽ 2.	29·538	29·515	29·455	28·95	29·16	29·41	51·2	53·3	46·5	57	45	47	50	45	SE.	S.	S.	E.	·050	...	...	...	46
3.	29·432	29·404	29·343	29·04	29·44	29·30	51·5	56·4	47·6	56	40	49	53½	43	NE.	E.	calm	E.	...	·02	...	...	48
4.	29·288	29·339	29·200	28·80	29·09	29·04	50·3	55·6	49·0	56	46	49	48	43	S.	W.	calm	E by S.	·052	·16	...	...	49
5.	29·202	29·232	29·152	28·69	29·01	29·04	50·3	55·8	48·8	55	41	50	46½	42	S.	S.	E.	E by S.	·302	·23	·15	...	49
6.	29·056	29·073	28·921	28·72	29·00	28·60	47·3	54·4	45·8	51	41	45	45	40	SE.	SW.	NW.	E by S.	·133	·48	·11	...	45
7.	29·138	29·154	29·083	28·63	28·74	28·98	48·8	51·7	48·0	58	41	49·5	49½	43	SSW.	SW.	NW.	S.	·286	·10	·09	1·00	47
8.	29·360	29·336	29·299	28·85	29·05	29·06	47·0	55·3	44·7	55	41	42	47½	35	S.	SW.	calm	S.	·083	·20	...	...	45
○ 9.	29·106	29·406	29·077	28·64	28·90	28·91	48·8	53·8	46·0	53	41	47	45½	42	S.	SW.	W.	NW.	·091	·10	·13	...	46
10.	29·074	29·263	29·046	28·63	28·92	29·05	44·5	53·5	44·2	51	37	39	45	39½	SSW.	NW.	calm	WNW.	·105	·01	·07	...	45
11.	29·332	29·308	29·207	28·88	29·10	29·20	44·2	50·3	42·2	49	36	38	44	31½	S.	SW.	calm	E by S.	...	·30	...	..	43
12.	29·528	29·516	29·316	29·08	29·50	29·32	41·3	49·8	40·6	54	39	36	44	34	S.	S.	calm	E.	·114	·35	...	..	40
13.	28·828	28·818	28·669	28·55	28·90	28·46	48·7	51·0	41·7	55	44	44	43	37	S.	S.	S.	E.	·272	·22	·44	...	45
14.	29·044	29·287	28·992	28·41	28·69	29·09	46·2	53·4	45·0	50	28	44	44½	41	S.	W.	W.	WNW.	·230	...	·24	0·77	44
15.	29·512	29·509	29·463	29·05	29·28	29·30	39·3	50·6	38·6	56	38	39	43½	31	S.	SW.	calm	WSW.	...	·11	...	...	39
☾ 16.	29·084	29·234	29·059	28·47	28·74	28·71	55·5	57·9	38·5	62	44	55	49	32	SW. var.	SW.	S.	SW.	·083	...	·27	...	49
17.	29·600	29·547	29·138	29·04	29·33	29·38	48·2	60·6	46·7	56	38	47·5	48	40	S. var.	S.	W.	ENE.	...	·40	·04	...	47
18.	29·814	29·789	29·605	29·39	29·69	29·75	41·2	54·6	41·4	44	34	35	38	28½	NW.	NE.	calm	NENW	·447	·41	·23	...	43
19.	29·800	29·939	29·771	29·46	29·89	30·00	39·8	42·4	37·2	44	30	40	38½	30½	N.	NE.	NW.	N.	·450	·01	·04	...	41
20.	30·112	30·078	30·061	29·70	29·85	29·88	35·7	44·7	35·0	43	37	33	44	25½	NW.	W.	calm	WNW.	...	·02	·02	...	33
21.	29·526	29·604	29·261	29·04	29·02	29·46	43·3	44·3	35·0	46	39	42	45	40	S.	SW.	W.	NNW.	·033	·16	·02	1·05	41
22.	29·794	30·033	29·778	29·42	29·89	29·92	40·3	47·3	40·0	43	30	41	43½	34	NNW.	N.	N.	NW.	·211	·01	·11	...	38
23.	30·070	30·056	29·980	29·61	29·74	29·79	38·4	43·5	35·6	44	39	36·5	52½	34½	S.	SW.	S.	W.	...	...	·07	...	33
● 24.	30·140	30·242	30·088	29·68	30·00	30·19	50·0	51·2	38·2	56	28	45	51	45	W.	NE.	calm	SW.	...	...	...	...	43
25.	30·424	30·400	30·372	30·00	30·24	30·28	41·0	54·3	38·3	46	24	35	46	38½	N.	SE.	calm	SW.	...	...	...	...	40
26.	30·460	30·433	30·382	30·05	30·30	30·31	37·4	47·0	36·0	44	22	32	41	33	E.	E.	calm	E.	...	...	...	...	35
27.	30·430	30·400	30·368	30·02	30·30	30·28	34·5	44·0	34·3	34	23	30	35½	24	N.	NE.	calm	SE.	...	...	...	...	35
28.	30·406	30·395	30·328	30·04	30·20	30·17	32·8	44·5	32·8	35	22	27·5	41	23½	NE.	NE.	calm	SE.	...	...	...	0·44	35
29.	30·336	30·326	30·185	29·94	30·02	29·79	30·8	39·3	30·3	36	30	31·5	48½	39	NE.	E.	calm	SW.	...	...	...	...	32
30.	30·084	30·066	29·932	29·58	29·72	29·78	46·7	47·7	30·3	54	50	45	51	47	S.	SW.	calm	SW.	...	...	...	...	35
Mean.	29·639	29·676	29·528	29·18	29·434	29·459	44·1	50·7	40·8	49·96	36·46	41·3	45·8	37·4					Sum. 2·942	3·59	2·03	3·26	Mean. 42

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

[THIRD SERIES.]

FERRUARY 1841.

XV. *On the Combination of Hydrated (the Hydrate of) Sulphuric Acid with Nitric Oxide. By* M. ADOLPH ROSE*.

THERE has frequently occurred of late in commerce, a sulphuric acid which is more or less contaminated by one of the oxides of nitrogen, and which, treated with a solution of copperas, (*eisenvitriol*) gives at times a dark red, at times a blackish-brown colouring. It is usually considered as contaminated by nitric acid, which is said to originate, partially by the decomposition of the white crystals, which are frequently, or almost always formed in larger or smaller quantities in the leaden chambers†, partially by the oxidation of the nitrous acid in the chambers. Those crystals consist, according to Gaultier de Claubry and Henry, of sulphuric acid, nitrous acid and water, and are stated to be decomposed on solution in water, into sulphuric acid, nitric acid, and evolving nitric oxide.

This, even though slight, amount of nitric acid, in English sulphuric acid of commerce, is very injurious in the preparation of hydrochloric acid, as on the employment of such an acid a chloriferous hydrochloric acid is constantly obtained.

To purify the sulphuric acid from the nitric acid and sulphate of lead, it is usually subjected to distillation. On the distillation of a sulphuric acid which gave a strong colouring with copperas, I changed the receiver, as in this, according to almost all works on chemistry‡, the water and nitric acid

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* From Poggendorff's *Annalen*, vol. l. p. 161. Translated and communicated by Mr. W. Francis.

† [A paper on the composition of these crystals and on the relations of their formation in the manufacture of sulphuric acid, by Dr. S. L. Dana, appeared in Lond. and Edinb. Phil. Mag., vol. iii. p. 116.—EDIT.]

‡ Geiger's *Pharmacie*, latest edition, p. 273.—Mitscherlich's *Chemie*, p. 480.

are contained. To my astonishment, however, I found both this, as well as the next portion distilled, free from every trace of nitric acid, or any other oxide of nitrogen; on the contrary, the residuum in the retort was considerably more contaminated by one of these oxides, a fact which I subsequently found Barruel* had already drawn attention to, and which had been confirmed by Wackenroder†. On a second distillation, for which I employed 4 pounds of the same acid, I obtained the same result, somewhat above 1½ pound of perfectly pure acid having passed over.

Upon this I mixed 4 pounds of pure sulphuric acid with 4 ounces of nitric acid of 1·4 spec. gr., and subjected the mixture to distillation, taking the precaution to change the receivers frequently. The first three ounces distilled were very watery, and contained much nitric acid and little sulphuric acid; the succeeding two and three ounces consisted almost solely of sulphuric acid, which contained but little nitric acid, the next two ounces still contained a trace of nitric acid, after which one pound and some ounces of perfectly pure sulphuric acid distilled over. An acid then passed over, containing traces of one of the oxides of nitrogen, upon which the distillation was discontinued.

The residuum in the retort was coloured yellow, and disengaged, when mixed with water, nitric oxide, which changed, on coming into contact with atmospheric air, into nitrous acid. To observe this more distinctly, some of the acid was conveyed into a cylinder filled with distilled water, which then became filled with a colourless gas that on the addition of air became red, and was therefore nitric oxide. The residuum behaved towards reagents exactly in the same manner as the solution of the anhydrous sulphate of nitric oxide of H. Rose‡, in sulphuric acid, which led me to suppose that it might perhaps be a similar combination, a view which seems to be fully confirmed by subsequent experiments.

To be certain that nitric oxide, and not nitrous acid, and some nitric acid, which might likewise have been the case, is contained in the residuum from the distillation, I diluted half an ounce of it with water until no evolution of nitric oxide any longer occurred, then divided the liquid into two equal portions, added to the one half one drop of nitric acid, and boiled both portions the same length of time. The portion to which nitric acid had been added, treated with a solution of copperas and pure sulphuric acid, exhibited a very

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* Centralblatt, 1836, p. 314.

† *Annalen der Pharmacie,* vol. xviii. p. 153.

‡ Poggendorff's *Annalen,* vol. xlvii. p. 605.

strong dark colouring; the other portion, treated in the same manner, did not exhibit this at all. Had the residue contained nitrous acid, nitric acid must have formed on dilution with water, and I must have obtained in the boiled liquid a reaction of nitric acid. I have subsequently always tested in this way, and have found it to be the best, when I wished to learn whether nitric oxide or nitrous acid was combined with the sulphuric acid.

The sulphate of the protoxide of iron is a most excellent reagent for the slightest traces of nitric oxide, nitrous and nitric acids; but it is absolutely essential always to add a considerable quantity of pure sulphuric acid, as otherwise small traces might easily be overlooked.

Nitric oxide and nitrous acid may be very well distinguished in concentrated sulphuric acid from nitric acid, by the addition of a dilute solution of the bichromate of potash, for both reduce the chromic acid to oxide, while they themselves are converted into nitric acid. The liquid is thereby coloured green; it is, however, requisite to add the dilute solution of the bichromate of potash in drops, otherwise the green colour might easily be concealed by the excess of the reagent. Nitric acid cannot naturally act in a reducing manner on the chromic acid of the bichromate of potash. A solution of the permanganate of potash cannot be employed, as this is altered by powerful acids; but it is a most excellent test, if the sulphuric acid has been previously diluted with about six parts of water. If sulphuric acid be mixed with pure distilled nitric acid, diluted with six parts of water, and, after it has perfectly cooled, some drops of a solution of the permanganate of potash be added to it, the colour is not in the least affected; but if the residue from the distillation, which, as I have shown to be probable, consists of a solution of the sulphate of nitric oxide in sulphuric acid, be diluted with six parts of water, the solution of the permanganate of potash is decolorated by it. If I heat the diluted liquid for a moment, so that the solution of the protosulphate of iron with sulphuric acid still indicates the presence of nitric oxide, the solution of the permanganate of potash is likewise decolorated. If I continue to heat the solution of the protosulphate of iron with sulphuric acid, it no longer occasions any colouring, and a drop of the solution of permanganate of potash is no longer decolorated.

Wackenroder* advised the addition to the concentrated or diluted sulphuric acid of a solution of the sulphate of the deut-

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* *Annalen der Pharmacie*, vol. xviii. p. 154.

oxide of manganese, which, on the presence of nitric acid is not, but on that of nitric oxide and nitrous acid is, immediately decolorated; however, the solution of the hypermanganate of potash is far more sensitive.

A portion of the residuum from the distillation of the sulphuric acid with nitric acid, was distilled in a small retort over an alcohol lamp, with the precaution that the passing product of distillation was frequently removed and tested. At first a sulphuric acid passed over, which contained very little nitric oxide; however, the amount of this latter increased in the succeeding portions more and more; and the last was a concentrated solution of the sulphate of nitric oxide in sulphuric acid, which acted towards reagents exactly like the solution of the sulphate of nitric oxide in sulphuric acid presently to be mentioned. Only a trace of the sulphate of lead remained in the retort. The last product was white, became yellow on heating, and on diluting it with water, green, blue, and lastly, colourless, under the evolution of much nitric oxide.

I hereupon attempted to prepare direct a combination of nitric oxide with hydrated sulphuric acid. I passed nitric oxide dried over the chloride of calcium, into distilled English sulphuric acid which was placed in a large vessel, but so that the entrance of air was prevented. A rapid absorption took place when the disengagement was not effected too violently, and no gas could be observed to escape; only when the evolution was too violent did nitric oxide escape. At the same time, a crystalline crust was formed on the sides of the vessel, which, however, disappeared again when the glass was shaken, it being dissolved by the acid. After passing the nitric oxide through for some time the liquid became lilac, then slightly blue, and at last, beautiful dark blue, without any increase of temperature being perceptible. The liquid became thicker and thicker, and on shaking the vessel ran down its sides like a thick syrup; on shaking it was converted into a white frothy mass, which, however, on being left to stand, again changed into the blue syrup. By continually passing nitric oxide through it, it was at last converted into a solid white crystalline mass, which melted on being slightly heated without decomposition, and on cooling, again solidified.

If I passed nitric oxide not dried over the chloride of calcium, into English sulphuric acid, a slight increase of temperature occurred; nevertheless, the crystals then appeared to form more rapidly, so that it seems that a little aqueous vapour favours their formation.

If this crystalline saline mass was treated in a dry glass gradually with water, nitric oxide was evolved, and there was formed, according to the quantity of water, a green, blue, and lastly, a colourless liquid. The crystalline mass dissolves in concentrated sulphuric acid without decomposition; and, if the solution be subjected to distillation, the superfluous sulphuric acid, contaminated by some sulphate of nitric oxide, first passes over, and then the concentrated solution of the sulphate of nitric oxide in sulphuric acid, which can be distilled over several times without decomposition. This solution, as well as the crystalline mass, was tested for nitric and nitrous acids in the manner previously described, without, however, a trace of them being discovered. As a counter-experiment here likewise, as always subsequently, a drop of nitric acid was added to the one half of the diluted solution, and both portions were boiled for an equal length of time. In the portion to which nitric acid had been added, I naturally obtained with sulphuric acid, and a solution of copperas, a deep brownish-black colóuring; in the other portion not the slightest trace of it.

The sulphate of nitric oxide is, it is true, decomposed as rapidly by water as its solution in sulphuric acid; yet the decomposition does not seem to be effected perfectly in the cold; if, however, a great quantity of water be employed for dilution, and it be boiled somewhat longer, every trace of nitric oxide at last escapes. But so long as one of the oxides of nitrogen is detected in the solution, by means of sulphuric acid and copperas, the solution of the permanganate of potash is decolorated by it.

The combination of nitric oxide with anhydrous sulphuric acid, prepared by Prof. H. Rose, acts, according to the experiments I have made with it, in exactly the same manner. On dilution with water the same play of colours occurs under disengagement of nitric oxide; and if the diluted solution be boiled for some minutes, all the nitric oxide escapes, and the solution of copperas with sulphuric acid does not indicate any nitric acid in the boiled liquid. Likewise, in this case, the sulphate of the nitric oxide is more completely decomposed, English sulphuric acid being present. If the crystalline mass was diluted with much water, and heated only for a moment without sulphuric acid, it was, it is true, coloured by copperas and sulphuric acid, but then the cooled liquid also still discoloured a dilute solution of the permanganate of potash; as soon as this, after longer boiling of the cooled liquid, was no longer discoloured, did copperas and sulphuric acid likewise cease to react.

Since these crystals acted exactly in the same manner as those which are produced in the preparation of English sulphuric acid, as also those which originate on passing nitrous acid into the hydrate of sulphuric acid, and which, as already mentioned, are stated to consist not of nitric oxide, sulphuric acid and water, but of nitrous acid, sulphuric acid and water, I was induced to prepare them.

For this purpose I conducted into a spacious bottle, containing one ounce of distilled oil of vitriol, and which was connected air-tight with a pneumatic trough, nitrous acid which I obtained by boiling 1 part of starch with 10 parts of nitric acid of 1·3 spec. gr. (according to Liebig's method). The air in the flask became immediately coloured dark red, and the escaping gas was caught; it consisted, however, for the greater part, of carbonic acid which had been produced by the action of the nitric acid on the starch contemporaneously with the nitrous acid. Since my expectation that oxygen would be evolved was disappointed, I terminated the operation after an hour. A yellowish green liquid had formed. If one part was diluted with much water, a great deal of nitric oxide was evolved; the diluted fluid, to one part of which sulphuric acid was added, to the other not, was boiled for some length of time, the evaporated water being from time to time replaced; but, nevertheless, I constantly obtained, on the addition of pure sulphuric acid and solution of copperas, a strong brown colouring, while the solution of the hypermanganate of potash was not in the least decolorated by it.

The well-closed liquid solidified after some hours on shaking, and separated into a white crystalline mass and into a supernatant yellowish liquid, which was separated from the crystalline mass by means of a funnel and glass rod; the crystalline mass was dried on clay-stone (*Thonstein*), over sulphuric acid. The filtered liquid contained much sulphate of nitric oxide, dissolved in sulphuric acid and nitric acid; when diluted with much water and boiled for any length of time, the evaporated water being from time to time restored, I obtained with a solution of copperas and sulphuric acid a strong deep brown colouring, while a dilute solution of the permanganate of potash was not discoloured. The crystalline dried mass gave on dilution with water under evolution of nitric oxide the frequently-mentioned changes of colour, behaved like the sulphate of nitric oxide, only that in the boiled diluted aqueous solution traces of nitric acid were constantly detected; this slight trace undoubtedly arose solely from the adherent mother-liquor, from which it cannot be separated. The nitrous acid seems to have been decomposed on its passing through sulphuric acid into nitric oxide and nitric acid, the former combining with the sulphuric acid, while the latter is present in great quantity in the mother-liquor. It is hence evident, that the crystals formed in this manner are contaminated either by nitric acid or by the nitrate of nitric oxide.

I now prepared these crystals by passing sulphurous acid and nitric oxide into a large vessel filled with atmospheric air, into which, by means of a glass tube, I could spurt water or blow air. With the presence of some water and an excess of nitric oxide, the crystals formed immediately, and covered partly the inner sides of the vessel, partly appeared in the centre in crystalline flakes, quite similar to snow. Decomposed by water, nitric oxide was abundantly evolved; but if this solution was boiled with sulphuric acid and water, and then solution of copperas and pure sulphuric acid added to it, it evinced no reaction of nitric acid, but this must necessarily have been the case if the crystalline mass consisted of nitrous acid, sulphuric acid and water. I likewise in this case divided the solution into two parts, and added to the one portion a trace of nitric acid, boiled this portion even somewhat longer, and yet obtained a distinct reaction of nitric acid in the portion to which nitric acid had been added, while the other portion showed not the least trace. Nor did I omit to add to the diluted boiled solution, after cooling, a solution of hypermanganate of potash, and by that means firmly convinced myself that no nitric acid had been formed by diluting with water, and that these crystals therefore did not consist of nitrous acid and sulphuric acid, but of a combination of sulphuric acid, nitric oxide and water.

But if, after the operation is terminated and the vessel remains filled with nitric oxide, atmospheric air be so long blown into the vessel that it again becomes colourless, then indeed a slight reaction of nitric acid is obtained after boiling, which however certainly arises from the water in the vessel dissolving the nitrous acid formed. The solution was tested as above.

These, then, are the crystals which are formed in the lead chambers in the preparation of English sulphuric acid, and always will be formed when an excess of nitric oxide in comparison to atmospheric air and sulphurous acid is present, a portion of the nitric oxide then changing only into nitrous acid, which oxidizes the sulphurous to sulphuric acid, and this then immediately enters into combination with the nitrous oxide; nay, they will be formed even with an excess of nitrous acid and atmospheric air, the sulphuric acid formed decomposing the nitrous acid into nitric acid and nitric oxide, with which it combines. It is therefore requisite, in order to avoid the production of these crystals in the preparation of sulphuric acid, to see that sulphurous acid is constantly present in sufficient quantity in the chamber.

The quantitative analysis of these crystals, as well as of those which originate on passing nitric oxide into sulphuric acid, and which is connected with some difficulties, I intend communicating in a subsequent paper.

The solution of the sulphate of nitric oxide in sulphuric acid is so analogous to the red fuming nitric acid, that it is not improbable that the latter may be with more reason regarded as a solution of the nitrate of nitric oxide in nitric acid, in favour of which, moreover, is the fact noticed by Gay-Lussac, that crystals are formed on mixing the red fuming nitric acid with fuming sulphuric acid, which are undoubtedly the sulphate of nitric oxide. If the solution of the sulphate of nitric oxide in sulphuric acid be distilled, the superfluous sulphuric acid as above-mentioned first passes over, and then this concentrated solution, which can be distilled several times without decomposition. When heated, the concentrated solution becomes yellow; diluted with water it is changed, under evolution of nitric oxide, according to the quantity of water, into a green, blue, and colourless liquid, just as is the case with fuming nitric acid. Both this, as well as the fuming nitric acid, cannot be completely decomposed by mere dilution with water, for if the diluted solutions of both be kept for any length of time, they still decolorate the solution of the permanganate of potash. But by heating, nitric oxide is expelled from both, only in fuming nitric acid quicker. Both can be prepared by merely passing nitric oxide into sulphuric acid, or into nitric acid.

Berzelius[*] considers it quite as probable that the fuming nitric acid is a solution of a combination of nitric oxide with nitric acid, as that it is a combination of nitrous acid with nitric acid in excess of nitric acid; and the first view undoubtedly acquires greater probability from the existence of an analogous combination with sulphuric acid.

It now appeared to me still of interest to learn how it is that at present the sulphuric acid is so frequently contaminated with the sulphate of nitric oxide, for contamination with nitric acid perhaps seldom occurs. I diluted pure sulphuric acid with water so long till it had a spec. gr. of 1·2 (of which concentration it is generally drawn off from the lead chambers), and treated a portion of it with the sulphate of nitric oxide, a portion with a solution of the same in sulphuric acid, a portion with pure nitric acid, and another portion with fuming nitric acid. Upon this I heated each portion separately in a retort un-

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[*] Berzelius's *Lehrbuch der Chemie.*

til sulphuric acid passed over, always obtaining pure sulphuric acid as the residuum; I had, however, especially with the addition of nitric acid, to continue the heating so long, that the remaining sulphuric acid had a spec. gr. of 1·84. Even when concentrated sulphuric acid is mixed with nitric acid, and the mixture exposed to a very gentle heat, sulphuric acid, almost pure, is obtained as the residue. If the concentrated acids be mixed together without any change of temperature taking place, and the mixture be left to stand for several weeks, no decomposition appears to occur; if large quantities be quickly mixed together, a trace of the sulphate of nitric oxide is formed, which probably arises from a disengagement of heat. But if the mixture be quickly heated in a retort, a decomposition ensues, the neck of the retort is filled with red fumes, and at first a sulphuric acid, containing nitric acid, distils over, then pure sulphuric acid, and in the residue remains sulphuric acid, containing the sulphate of nitric oxide dissolved. When the sulphuric acid is coloured by organic substances, and it is decolorated by warming and adding drop by drop nitric acid to it, it becomes contaminated with sulphate of nitric oxide.

According to this the sulphuric acid ought to occur in commerce free from every oxide of nitrogen, when it has a specific gravity of 1·84, of which strength it however rarely occurs, and only rendered impure by the sulphate of nitric oxide when it has been discoloured by nitric acid. Of late the sulphuric acid is concentrated in vessels of platina, which are so constructed that dilute sulphuric acid continually flows into the more concentrated, and this is probably the cause that the sulphate of nitric oxide is formed which then does not distil over.

Barruel* has proposed to digest the impure sulphuric acid over sulphur at 200° cent. in order to destroy the acids of nitrogen, and then to distil. But if the acid be distilled, then this is superfluous, for even did the sulphuric acid contain nitric acid, a pure acid would be obtained by distillation, which is even, as I have stated, obtained when one ounce of nitric acid is added to a pound of sulphuric acid, a contamination which occurs seldom if ever in commerce; it is merely requisite to change the receiver frequently. If the sulphuric acid contains the sulphate of nitric oxide, then a pure acid passes over immediately.

To procure a pure sulphuric acid for the preparation of hydrochloric acid, it is only necessary to mix it, (it matters not whether it be contaminated by the sulphate of nitric oxide or nitric acid, with two parts of water) and then to heat

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* Centralblatt, 1836. p. 315.

it in a retort until vapours of sulphuric acid pass over, whereby there is at the same time the advantage of having an acid of 1·85 sp. gr.

In the distillation of sulphuric acid several precautions have been proposed to avoid the sudden jerks (*stossweise*) in the boiling of the acid, yet most of them are by cautious equal heating, even the platina wire, unnecessary, and I have at last even distilled without this over an open fire, without ever meeting with an accident. But it is very necessary that the neck of the retort is not too long and is as broad as possible, and that the receiver does not lie immediately on the neck of the retort, but is separated from it by a platina wire, and that an equal fire be kept up, which is best attained by using charcoal. The retort is filled two-thirds with sulphuric acid, placed with the usual precautions in the sand bath, and at first a good fire kept up until strong vapours ascend from the acid; the fire is then diminished, and the acid comes to boil gently. Care should be taken, by keeping up an equal fire, that the acid does not cease boiling; should this happen, it is not necessary to interrupt the distillation, but the fire must be cautiously increased, that the acid may not enter suddenly into a too violent ebullition.

XVI. *A new Method of analysing the Ores of Iron, and Crude Iron. By Professor* Fuchs*.

THIS process is founded upon the fact, that hydrochloric acid is incapable of dissolving metallic copper, provided the air be excluded; but if a piece of copper be put into a solution of a persalt of iron in hydrochloric acid, the persalt is converted into a protosalt, and a quantity of copper is dissolved, forming a protosalt of copper exactly equivalent to the quantity of peroxide of iron contained in the solution.

Into a solution of peroxide of iron in hydrochloric acid put a piece of copper, whose weight is accurately known; boil it well until no more copper is dissolved; weigh the undissolved copper after having cleaned and dried it, and thus ascertain how much copper has been dissolved. The quantity of peroxide of iron contained in the solution is to that of the copper dissolved as their respective equivalents, viz. as 40 to 31·7. Therefore if 31·7 copper be dissolved, it indicates 40 as the quantity of peroxide of iron originally contained in solution.

When the peroxide and protoxide are both present, two experiments are necessary to determine their respective quan-

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* Extracted from the Transactions of the Royal Bavarian Academy.

tities. The quantity of peroxide is ascertained in the manner just indicated. To determine the quantity of protoxide the whole of the iron must be brought into the state of peroxide, and from the amount of the peroxide thus obtained must be deducted the quantity of peroxide in the first experiment, and the remainder reduced to the protoxide by calculation.

The following precautions must be observed in order to obtain accurate results. The copper must be in a state of purity. The hydrochloric acid must be also pure, tolerably concentrated, and employed in excess. Nitric acid should not be used for bringing the iron to the state of peroxide, but either a stream of chlorine, or what is preferable, chlorate of potash. Upon adding the piece of copper to the solution it must immediately be brought to the boiling point, and kept there in order to prevent access of air. Previously to removing the undissolved copper from the solution, hot water is to be added till the vessel is quite full; this is to be poured off and fresh hot water to be added: the copper, which is generally covered with a brownish coating, is then carefully washed in cold water, dried at a gentle heat, and then weighed. The foregoing process has this advantage; that the substances ordinarily met with in iron ores,—such as silica, alumina, magnesia, lime, oxide of titanium, oxides of manganese, the phosphoric and sulphuric acids, &c.—do not in any way interfere with the accuracy of the results. An iron ore containing arsenic, however, cannot be analysed on this principle, as blackish gray scales of arseniuret of copper are deposited on the metallic copper.

This process is applicable to the examination of cast iron, or iron of other kinds, and also for the comparison of one sort of iron with another.

Among many experiments which were made, M. Fuchs relates the following.

Exp. 1. 50 grains of very soft English iron were dissolved in hydrochloric acid, and brought to the state of peroxide by chlorate of potash; 85·8 grains of pure copper were boiled in this solution, of which 56·2 grains were dissolved. Therefore as 31·7 : 28 :: 56·2 : 49·46, which is equal to 98·92 per cent. of pure iron; a repetition of this experiment gave 99·19 per cent.

Exp. 2. 50 grains of pianoforte wires were examined, which indicated 98·75 per cent. of pure iron. The iron in this experiment was peroxidized by a current of chlorine; a considerable deposit of carbon was formed during the process of solution in hydrochloric acid, which, however, disappeared upon passing chlorine through the solution.

Exp. 3. 50 grains of gray and white cast iron from Bergen were examined, and gave 94·33 per cent. of pure iron: the impurities, on examination, were found to consist of

Carbon ...	3·43
Silica	1·75
Phosphorus	0·37
Sulphur ...	0·12.
	—— 5·67

Exp. 4. 70 grains of crystallized carbonate of iron from Lobenstein, examined, gave equal to 56·9 per cent. of protoxide, equivalent to 91·68 per cent. of carbonate of the protoxide; the other constituents consisted of carbonate of the protoxide of manganese and carbonate of lime and magnesia.

Exp. 5. 70 grains of specular iron ore from Gleissing in the Fichtelgebirge, gave upon examination a quantity equal to 92·30 per cent. of peroxide of iron, and 7·40 per cent of silica, leaving only a loss of 0·3 per cent.

Exp. 6. Crystallized magnetic iron, containing both protoxide and peroxide, was next examined. 50 grains were dissolved in hydrochloric acid, to which chlorate of potash was added to bring the whole of the iron to the state of peroxide. 40·71 grains of copper were dissolved, which is equivalent to 51·36 of peroxide of iron, or 102·72 per cent. Another portion of 50 grains was dissolved without the addition of chlorate of potash; the copper taken up was only 27·1 grains, equivalent to 34·2 grains of peroxide of iron, or 68·4 per cent.; this deducted from 102·72 indicated by the first 50 grains, gives 34·32, corresponding to 30·88 protoxide of iron: consequently this mineral gave 68·40 peroxide of iron

68·40	peroxide of iron
30·88	protoxide of iron
0·72	loss.
100·00	

This process is applicable in many instances to the determination of the quantity of copper contained in ores of that metal.

The ore of copper is to be dissolved in hydrochloric acid, care being taken that the whole of the copper is converted into oxide or chloride; the solution is then to be boiled with copper (the necessary precautions having been first duly observed), until it assumes a pale olive-green tint, and becomes colourless on being diluted with water. If no oxide of iron be present, precisely the same quantity of copper will be transferred to the solution as was originally contained therein; the quantity of copper remaining is to be subtracted from the quantity of reguline copper employed in the experiment, in order to ascertain the amount of copper contained in the copper ore examined. 100 grains of pure malachite were examined in this manner, and gave equal to 57·5 of pure copper, which is very nearly the quantity that should be obtained; had the quantity of copper dissolved proved considerably less, it would have shown that the malachite was not in a state of purity.

XVII. *On the Electricity of Steam.* *By* John Williams, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

EARLY in November last, I read in your Journal a communication from Mr. Armstrong, of Newcastle-upon-Tyne, on the subject of the electricity observed in a jet of steam issuing from the boiler of a high-pressure steam engine*. I felt much interested on reading this account, as I doubt not that it will eventually lead to some valuable discoveries relative to meteorology and atmospherical electricity, subjects which have occupied much of my attention for more than forty years past.

Volta's experiment of dropping a red-hot coal into a vessel of water placed on the insulated cap of an electrometer, was repeated by me in various ways and on a large scale; and from the results, I came to a conclusion that the positive electricity observed in the steam-cloud on its *immediate formation* was simply the effect of evaporation, and not, as some electricians have supposed, of a decomposition of the water into its primitive elements. And I imagine, that when steam under great pressure, and, consequently, of a high temperature, is liberated, and in the act of expanding and mixing with atmospheric air, it has its capacity for electricity increased; and that a similar process is continually in action in the ordinary course of evaporation from the surfaces of water, leaves of vegetables, and moist soil, with this difference; that in the latter natural processes the resulting electricity of the rising vapour is of low intensity, and not discoverable by our ordinary electrometers.

These reflections led me to the consideration of the cause of volcanic lightning, and particularly of the shape generally assumed by the enormous cloud of smoke, as described by Sir William Hamilton on viewing an eruption of Vesuvius.

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* [Papers on this subject have appeared in our Numbers for November, December, and January; vol. xvii. p. 370, 375, 449, 452, and 457; present volume, p. 14, 50, and 95.—Edit.]

With a view to find out the cause of volcanic lightning, I made some experiments with great care, by insulating a small portable furnace; and by doubling the electrical signs, by means of condensers, I found the insulated furnace to be in a negative state when the fire burnt with a strong flame; these electrical signs, however, were strongest after the addition of fresh fuel, and when *a dense smoke arose a few seconds before it burnt into flame**.

After the locomotive high-pressure engines came into use as public conveyances for passengers, I visited Manchester and Liverpool purposely to inspect the machinery and observe the steam- and hot-air cloud when issuing from the funnel; the weather being cold, and consequently the steam visible, I noticed the rapid divergence of the particles of vapour, and thought this might be caused by its electrical state, which I resolved to prove by experiment, but which I was not enabled to do till November 1838, when, having provided myself with a small close-covered boiler, made with strong sheet-copper, and furnished with a safety valve loaded with a weight, it was placed on a portable furnace, and insulated on a stool with glass legs, placed under a large spreading willow-tree in my garden, where I never could observe any signs of atmospherical electricity. The day was nearly calm. The following is a copy of the marginal note I entered in my meteorological journal:

"November 13th, 1838.—To-day I repeated a former experiment†, which fully confirmed my pre-conceived ideas, that when steam, confined under pressure in a boiler, is suddenly allowed to escape into the open air, the liberated steam has its capacity for electricity increased, consequently it leaves an insulated boiler in a negative state, which proved to be the case; for on raising the safety valve by means of a silk string and a piece of sealing-wax, *the gold leaves of the electrometer* opened one quarter of an inch with *negative electricity*."

I am, Gentlemen,
Your obedient servant,

Pitmaston, Worcester, 15th December, 1840. JOHN WILLIAMS.

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* [It must be remembered, that the cloud, apparently of smoke, which issues from the crater of a volcano in eruption, is not really smoke, but *steam*, (mingled with finely-divided solid matter—volcanic sand,—consisting almost entirely of di-electrics,) issuing from an orifice in non-conducting matter. Volcanic lightning, therefore, is precisely analogous to the electric sparks given by effluent steam.—E. W. B.]

† The former experiment here alluded to was made by me many years ago, by opening a steam-cock on a large Watt's low-pressure engine boiler, and I found by my electrometer that the expanding steam-cloud was in a state of *positive* electricity.

XVIII. *On the Circumstances under which Steam developes Positive Electricity. By Dr.* Charles Schafhaeutl*.

IN a conversation with Mr. Bradley of the Adelaide Gallery, he informed me that steam issuing under a pressure of about 40 atmospheres, from the boilers of Mr. Perkins's steam-gun, was able only slightly to move the gold leaves of a voltaic condenser. On my expressing an opinion that the electricity of the issuing steam had some relation to the formation of the deposit or incrustation in the boiler, he invited me to make some experiments at the Adelaide Gallery, in order, if possible, to elucidate this point.

As Mr. Perkins's boiler is constructed so as to prevent any incrustation forming, I selected for my experiments a common Marcet boiler, consisting of a globular iron vessel about 5 inches diameter, in the vertical axis of which is inserted a mercurial gauge, and at an angle of 45 degrees from it, a thermometer on one side, and on the other side a jet, with a stop-cock. The lower part of the boiler was of course occupied by quicksilver, on the top of which distilled water was poured to the height of $1\frac{1}{4}$ inch. In the direction of the jet a glass bell was suspended of 9 inches diameter and 5 inches in depth, so that the distance of the top of the bell from the orifice of the jet was about 9 inches. A bundle of copper wire was then attached at one end to the disc of a voltaic condenser, and the other end was spread out and inserted in the glass bell. The water in the boiler was then brought to a state of ebullition, and when the mercurial column had risen to 31 inches, the stop-cock of the jet was opened, the issuing steam condensed in the glass bell in great quantities, and the gold leaves of the voltaic condenser after the first few seconds separated instantaneously to their full extent, even under a pressure of 23 inches. This was likewise the case when the condensing plate was removed from the electroscope, and the gold leaves were always found positive electrified.

The steam under the same pressure directed against the copper wires without the bell was found to exhibit no trace of electricity.

The distilled water, which had assumed a red colour from the hydrate of oxide of iron derived from the inside of the boiler, was now changed for a saturated solution of common salt, which occupied a space of about three quarters of an inch on the top of the quicksilver; the other part of the experiment

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* Communicated by the Author, whose former communications on the subject will be found in vol. xvii. p. 449, and in our last Number, pres. vol. p. 14.

was then repeated as before, but no trace of electricity could be discovered.

After the greater part of the water had been evaporated the boiler was opened, the deposited salt removed, and after being well washed filled again with distilled water. After the steam had reached a pressure of 31 inches, as in the first instance, the stop-cock was again opened as before, and the jet of steam was directed against the inside of the glass bell in all its varied positions, but the gold leaves of the condenser remained motionless, or when positively electrified had a tendency to collapse.

I was obliged to fill the boiler again, and now took care to have the same quantity of water as in the first experiment. On the second trial the gold leaves separated again. On the third trial the jet of steam touched the outside as well as the inside of the glass bell. The fourth succeeded as in the first experiment, and afterwards no effect could be produced.

The boiler was again filled, and as I thought I had observed that the gold leaves separated only when the steam issued from the jet with a peculiar rushing fluttering noise, quite different from the hissing sound which generally accompanies the escape of pure steam, I therefore directed my particular attention to it in this experiment.

The water was at the height of about 1½ inch, and as soon as the mercurial column had reached 32 inches I opened the stop-cock with one hand, bringing with the other hand the axis of the glass bell in the direction of the jet of steam. The rushing sound was immediately observable, and the condensed water streaming from the edges of the bell, the gold leaves separating at the same moment to their extreme angle. After I had discharged the disc of the electroscope the leaves again opened several degrees, which happened likewise the third and fourth time, as often as the vibrating bell came into contact with the wires. The wetted interior of the bell had, therefore, not only obtained, but also retained, a small charge long after the steam had ceased to flow, and I was now able to produce the desired effect as often as I pleased, the success depending entirely on that state of the escaping steam which causes the rushing or fluttering noise which happened only when the boiler was filled at about 1½ inch, and ceased when the boiling sheet was reduced, the escape of steam without that peculiar sound, even under a pressure of 32 inches, causing the separated gold leaves to collapse.

The temperature of the glass bell seemed to be of little consequence, as the experiment succeeded on the first trial when the glass was quite cold, as well as when the bell had become warm after repeated trials; the only requisite being that peculiar condition of the steam which produces the above-mentioned rushing noise. Now this noise appears to be caused solely by the sudden boiling of the water, and the conversion of a portion of it into a fine spray, as the steam issuing under those circumstances and splashing against the interior of the bell deposits a large quantity of water running down in drops, and often in streams, from the edge of the bell.

The boiler was insulated and the stop-cock was opened with a dry folded silk handkerchief, but the omission of all these precautions did not in the slightest degree affect the results; a proof that the electricity of the steam developed in the glass bell could not be contained in the steam during its passage through the 3-inch long metallic jet of the boiler, as all electricity would have been deposited in this narrow metallic passage. But the condensation of steam even into the form of mist, seems not to be sufficient to produce electricity; the condensation into liquid water seems to be indispensable, at least in the present experiment, where the electricity appears to be due to the deposit of liquid water in the bell solely, or perhaps in relation to the separation of this liquid water from the steam.

The circumstance of steam in a state of mist not being capable of exhibiting traces of free electricity, seems to afford us some clue to the elucidation of a hitherto mysterious phænomenon, viz. that only certain clouds are capable of producing thunder storms. A common cloud consisting only of moisture seems to be analogous to a pure jet of steam in the glass bell, both consisting of minute hollow water globules or bubbles, leaving only a small deposit of moisture in the glass bell, or in the air, which finally collects into small drops of rain. But when the steam deposits rapidly a great quantity of liquid water which in a thunder-cloud produces those well-known heavy showers, electricity is set free in great quantities, so that a jet of issuing steam from Marcet's boiler in three seconds produced the same effects upon the gold leaves of the electroscope as a feeble spark from an electric machine with a glass plate of 9 inches, produced in damp weather. I have here only to observe, that the sudden separation of water in drops in thunder-clouds seems to be caused, as I expressed on a former occasion, by a sort of sudden compression and cooling caused by the vehement currents of air towards the centre of the thunder-cloud; because I found when I was immersed in a thunder-cloud, that the hygroscope during the increase of the wind rose to the highest degree of wet; the thermometer at the same time falling, which was followed immediately by a separation of water and a flash of lightning, either at the same moment, or following each other at short intervals. It therefore appears that the column of vapour rising from the crater of a volcano seems to be in a similar condition to the fluttering steam issuing from the jet of Marcet's boiler, and the flashes of lightning attributable to the separation of the liquid water from steam and smoke, as I hinted in my first communication on this subject, before I was enabled to make these experiments.

In continuing the above-mentioned experiments, I found that the bundle of wires connected with the electroscope for the absorption of the supposed electricity of the steam, might be dispensed with, and nothing more was necessary than to hold the glass bell against the jet of issuing steam; and as soon as the peculiar rushing noise commenced, the jet of steam at the same time changing from transparency to a milk-white opacity, the interior of the bell became immediately charged with electricity, whether the bell was 9 or 18 inches distant from the metallic jet. If the inside of the bell be brought whilst in this state into contact with the knob of the electroscope, the gold leaves immediately diverge very widely, and by repeating this experiment several times, a spark may easily be obtained.

A copper wire inserted through the jet into the boiler, forming the axis of the pencil of issuing steam, such wire approaching even within an inch of the glass bell, did not in the slightest degree affect the results described; a further proof that the free electricity which manifested itself was not contained in the issuing steam, but was developed during condensation in the glass bell. A tinfoil coating on the outside of the bell, reaching within an inch and a quarter of the edge, very considerably diminished the quantity of electricity. When an inch and a half of this coating was removed, the bell acted the same as when entirely uncovered, which may, perhaps, be simply ascribed to the steam on rushing out of the bell coming into contact with the foil when it is so near the edge.

If the water in the boiler was saturated with common salt or with sulphate of lime, and even a slight excess of sulphuric acid, the angle between the diverging gold leaves remained the same as if distilled water was used.

From these experiments we may safely infer that the observed free positive electricity in this case was solely attributable to the sudden condensation and separation of water from steam, caused in my experiment by its coming into contact with the internal surface of the glass bell, on the superficial contents of which the quantity of electricity developed greatly depends. On a larger scale, as in a steam-engine, the air in which the cloud originates has the same function as the glass surface. In thunder storms the currents of air rushing towards the centre of the nascent cloud, produce the peculiar sudden condensation and separation of water-gas similar to the pencil of steam issuing with the fluttering noise already mentioned from the jet of the boiler.

I must here particularly impress on the reader, that as the pencil of steam developes electricity only under certain circumstances, that is when steam is mixed with minutely-divided water before its expansion, so the thunder-cloud must likewise be in a similar state in respect to its water-gas and minutely-divided water, quite different to the state of a rainy atmosphere, where the condensation of the watery vapour into the shape of clouds commences only in the highest and coldest regions, uniformly and gradually, and where during their descent at certain intervals, from the highest to the lowest regions, the atmosphere begins gradually and uniformly to discharge moisture until the hygrometer indicates the point of saturation at which the formation of drops commences.

The electricity developed by evaporation, &c. could never manifest itself in a metallic boiler, and is under the most favourable circumstances so feeble, that its existence was long denied by the most eminent philosophers, until Dr. Harris succeeded unequivocally in demonstrating its presence. This electricity is likewise much influenced by chemical actions ensuing at the same time, as M. Pouillet has distinctly proved, which was not the case in my experiments.

With the positive electricity obtained from a jet of condensed steam, negative electricity is at the same time developed in the boiler and water, and as far as I was able to judge, of the same intensity. Negative electricity derived from a locomotive engine must necessarily be affected by the chemical process of combustion going on at the same time on a very extensive scale, as well as from various other causes, such as imperfect insulation, points, and sharp edges, and I was not able to detect in my small boiler any traces of negative electricity, if at the same time positive electricity did not appear in the glass bell.

XIX. *On the Phænomena of the Electricity of Steam, observed by* Mr. Pattinson *and* Mr. Armstrong. *By* M. Peltier*.

MY experimental and meteorological pursuits† having led me to study the circumstances which impart electricity to vapours, I am enabled to add some facts calculated to afford the explanation of the phænomenon of Cramlington.

When a thick piece of platina slightly convexo-concave (embouti), is made red-hot, and water poured upon it, a part of the phænomena which take place is known; therefore I shall only mention them, although the true explanation of these movements has not, in my opinion, been yet given; I propose to revert to this subject at another time.

If the bottom upon which the drop of water rests is flat or very little convexo-concave, it takes the figure of an ellipsis, as has been very correctly observed by M. Auguste Laurent (*Annales de Chimie et de Physique*, vol. lxii. p. 327). This ellipsis returning upon itself, there is formed from it another which is perpendicular to it: this one being depressed in its turn, the first one re-appears, and so afterwards. After some moments the elongation of the ellipses diminishes; but then they are multiplied: others are formed from them at 45 degrees from the first, making four, marching two by two. The temperature diminishing, the sallies grow feeble; a greater number occur, but they are small, no longer possessing regular and successive movements. These tumultuous movements resolve themselves into a gyratory movement which has its axis vertical, round which is seen a crenelated crown, the vestiges of the ellipsoidal elongations which are disappearing. A little later this movement diminishes in velocity, then it stops, and the drop seems to be in a state of repose, and vividly reflects the light. The form of the collective movements of these elongations is in part produced by exterior resistances themselves depending on the curvature of the crucible, and on the size of the drop projected.

On examining the drop of water with a lens during its rotation, there are seen in the interior several kinds of movements. When the rotation stops, there is a moment of suspense in the resultant, which must arise from interior movements; another rotation is then produced, the axis of which is horizontal, and which leaves the superficies

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* *Ann. de Chim. et de Phys.*, Nov. 1840, p. 330. See the preceding articles.

[† M. Peltier has lately published an interesting meteorological treatise, "*Observations et Recherches sur la Formation des Trombes*; Paris, 1840;" in which he ascribes to water-spouts an electrical origin.—Ed.]

perfectly even. This collective movement lasts but a short time; several others established themselves in the interior; we see them jostle one another, press on each other in different directions, move in array again together once more, then again to divide themselves. At last the movement slackens: the drop, which until then had retained its globular form and had not moistened the platina, flattens, wets it, and evaporates all at once, without any electricity having been produced, if distilled water and a clean crucible have been used.

If instead of pure water a weak solution of sea salt be used, the effect will be the same the first time; but the salt left by the water having formed a slight layer on the platina, is again taken up by the water used for the succeeding experiment: the strength of the solution thus increases, and when the drop is diminished two-thirds it is become almost opake; a multitude of minute bodies are seen swimming in the inside, and soon after some decrepitations are heard, accompanied with saline projections. At this moment the needle of the electrometer indicates a negative tension. If the platina has become cool enough to admit of moistening, the decrepitation ceases, the drop spreads, and it is immediately transformed into vapour; the electricity which had just been developed during the decrepitation, instead of being doubled by this sudden change into vapour, disappears with the vapour, which carries it off with it. The saline layer being increased, the effect at the third experiment is greater; the decrepitation is stronger, and the needle is projected to a distance. Thus, before and after the decrepitation there is no electricity produced; the instrument remains mute, whatever be the quantity of vapour produced.

From the preceding observation, it was natural to suppose that the salt, being substituted for the salt water, would produce the same effect; this is what the experiment has verified. The chloride of sodium decrepitating without aqueous fusion, the water interposed plays the part of saturated solution, and sets in motion the needle of the electrometer. If it be a salt containing water of crystallization, as the nitrate of ammonia so easy of decomposition, there is at first the aqueous fusion, a great evaporation without producing electricity, then at last comes the decrepitation, and the needle is strongly projected by negative electricity.

It is then at the moment of the separation of the combined molecules of water that electricity is produced; it is at the moment that a chemical decomposition takes place, and not during the separation of the excess of water. The application of these experiments to the phænomenon observed at Cramlington, is sufficiently obvious: it appears by the accounts given, that it was necessary for the water to be saturated to such a degree as to have a deposit formed on the side of the boiler; that a high temperature is requisite as the machine is a high-pressure one; that the electricity increases with the deposit, and that it varies with the temperature.

This phænomenon only appearing when there is a saline coat deposited, and its tension increasing with the thickness of the layer, it will serve to make known the degree of internal incrustation and the sudden variations of temperature.

XX. *On a new Fat Acid in the Butter of Nutmegs.* By LYON PLAYFAIR, *Ph. D.*[*]

THE butter of nutmegs has been the subject of several examinations; for the best account of its properties, however, we are indebted to Schrader. This chemist has shown that it is a compound of three oils, two of them being solid, the other volatile and liquid. He has also examined the proportional quantities of these, and described the methods by which they might be separated from one another.

Lecanu[†] observed that this butter possessed different properties from other vegetable fats, and approached more nearly in character to those of animals. He remarked likewise its partial solubility in æther, which had formerly been pointed out by Schrader as a distinguishing mark.

Pelouze and Boudet[‡] described a method by which *margarine* could be procured in a state of purity, and mentioned that the same margarine existed in the butter of nutmegs; but they have given no experiments in proof of this, nor did they state analyses of the products obtained.

None of the chemists now mentioned have published the numerical results of their examinations. Hence it was uncertain whether the acid existing in the butter of nutmegs was really margaric acid, or some other acid resembling it in properties. It was interesting to ascertain the exact composition of this *margarine*, and for this purpose the following examination was undertaken.

When the butter of nutmegs is digested with alcohol of the common strength, it is decomposed; the alcohol dissolves a coloured fat, acquires a wine-red colour; and yields by

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[*] Communicated by the Author; having been read before the British Association at the late meeting at Glasgow.

[†] *Journal de Pharmacie*, t. xx. p. 339.

[‡] *Annales de Chimie et de Physique*, t. lxix. p. 47.

evaporation a red, soft, semi-fluid fat of an agreeable odour of the nutmeg. Part of the butter remains undissolved; a small portion of it does dissolve, but is again precipitated upon cooling. When the alcohol is very strong, the butter dissolves in four times its weight (Schrader).

The fat, which remains undissolved, is very impure, and, even after several digestions, still retains the odour of the butter. It must therefore be subjected to strong pressure within folds of bibulous paper, first by treating it with alcohol, and afterwards with æther, and renewing the pressure after each treatment. The solution in æther must be filtered whilst hot, in order to get rid of the impurities. When the fat has attained a constant melting point of 31° C., it may be considered pure.

Care must be taken in the selection of the butter, for that sold in commerce under the name of "butter of nutmegs," sometimes consists of animal fat boiled with powdered nutmeg, and coloured with sassafras. The specimen may be relied on as pretty pure, if it dissolves in four times its weight of strong boiling alcohol, or half that quantity of æther.

The fat obtained as described above is oxide of glyceril in combination with a fat acid, which, as far as I am aware, has never hitherto been described. It has a beautiful white silky appearance. From this property (which is shared also by the acid derived from it) I propose to call it *Sericine* (from the Latin word *serica*), as I am desirous at present to give it no name exclusively applying to its origin; for researches now in progress of being made by another, appear to show that it is not confined to this butter, but also exists in others.

Sericic Acid.—The acid to which this name is applied, may be obtained by the saponification of *sericine*. The soap must be washed with cold water, in order to free it from the alkali employed in its saponification. It is now to be dissolved in boiling water, and muriatic acid added until the liquid possesses an acid reaction. The sericic acid now separates as a colourless oil, which solidifies to a crystalline fat on cooling. It must be washed with water, in order to free it from muriatic acid, and then repeatedly melted in fresh portions of pure distilled water.

Thus obtained, it possesses a snow-white colour and crystalline appearance. It is very soluble in hot alcohol, from which it is partly deposited in small crystals on cooling; the remainder may be obtained by further evaporation. In hot æther it dissolves in considerable quantity, but separates almost entirely on cooling. When allowed to crystallize slowly from alcohol by spontaneous evaporation, it is semi-transparent and highly crystalline. Its melting point is between $48\frac{1}{2}°$ and 49° C.

The atomic weight of the anhydrous acid obtained by the analysis of the salt of silver, is 2733·27; the mean of two analyses of the salt of barytes gave the atomic weight 2732·54. The formula of the anhydrous acid deduced from the analyses of its salts is $C_{28}\,H_{27}\,O_3$.

The formula for œnanthic acid is $C_{14}\,H_{13}\,O_2$; sericic acid may, therefore, be considered to have the same composition as œnanthic acid, in which one equivalent of oxygen is replaced by one of hydrogen.

2 atoms of œnanthic acid $= C_{28}\,H_{26}\,O_4$
1 atom of sericic acid $= C_{28}\,H_{27}\,O_3$

It may be compared, in this respect, to benzoic acid and the hydruret of benzule.

The anhydrous acid has not been procured in an isolated state; the acid procured by the decomposition of sericate of potash by muriatic acid, is the hydrate, and contains one atom of water. The following are the results of the analyses of this hydrate.

I. 0·351 gramme of substance, burned with oxide of copper, gave 0·389 gramme water, and 0·941 gramme carbonic acid.

II. 0·309 gramme of substance gave 0·342 water, and 0·829 carbonic acid.

III. 0·412 gramme of substance, burned with chromate of lead, gave 1·101 carbonic acid, and 0·454 water.

IV. 0·250 gramme of substance gave 0·670 carbonic acid, and 0·276 water.

V. 0·278 gramme of substance gave 0·744 carbonic acid, and 0·309 water.

	I.	II.	III.	IV.	V.*
Carbon ...	74·12	74·06	73·89	74·10	74·00
Hydrogen	12·31	12·29	12·24	12·26	12·02
Oxygen ...	13·57	13·65	13·87	13·64	13·98

These numbers approach closely to the formula $C_{28}\,H_{28}\,O_4$.

Atoms.		In 100 parts.
28 Carbon	2140·18	74·06
28 Hydrogen ...	349·42	12·09
4 Oxygen	400·00	13·85
	2889·60	100·00

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* The analyses Nos. I. II. IV. were made upon sericic acid repeatedly crystallized from alcohol; IV. was kindly executed by Mr. Miller, assistant to Professor Daniell; III. V. were made upon the acid obtained by decomposing sericate of soda with muriatic acid, with precautions formerly described.

The formula for the anhydrous acid is $C_{28}\ H_{27}\ O_3$; hence the formula for the hydrate is $C_{28}\ H_{27}\ O_3 + HO$.

There are several points besides its composition, which distinguish this hydrate from that of margaric acid, for which it has hitherto been mistaken. Its melting point is very different from that of margaric acid, and it is soluble in almost every proportion in hot alcohol. The soaps which it forms with potash or soda dissolve with greater facility than the corresponding soaps of margaric acid; they are also more crystalline in their appearance. Sericic acid does not appear capable of forming two classes of salts with the alkalies, that is, its salts are always neutral, and the soaps may be treated with water without passing into supersalts; a character possessed neither by stearic nor margaric acid.

The solution of sericic acid in alcohol strongly reddens litmus paper. When it is boiled with nitric acid diluted with half its weight of water, a violent action ensues, and peroxide of nitrogen is evolved in considerable quantity. The products of the decomposition appear to be soluble in water, for the acid remaining after the action had ceased was found unchanged; at least the salts of barytes and silver made from it possessed a composition precisely similar to that of the original acid; the melting point also remained the same. The subject was not more nearly examined.

Sericine.—The process for obtaining this substance has been described at the commencement of the paper; it is the solid part of the butter of nutmegs, and has been described by MM. Pelouze and Boudet as *margarine* (margarate of the oxide of glyceril).

Sericine is a very beautiful fat (when crystallized from æther), having a snow-white colour, and silky lustre. It is soluble, in all proportions, in hot æther; but the greater part crystallizes on cooling: in water it is quite insoluble. It is not easily saponified by caustic alkaline ley of the common strength, a property which distinguishes it from margarine; but it is easily converted into a beautiful white soap, by melting it with hydrate of potash and a very small quantity of water.

For the purpose of determining with what base sericic acid is combined in sericine, the following process was adopted. A quantity of sericine was boiled for several days with basic acetate of lead. An insoluble salt of lead was thus formed with the acid: the base must, therefore, have been separated. A stream of hydrosulphuric acid was now passed through the liquid filtered from the salt, until all the lead was precipitated. Hence the liquid could now only contain the base with which the acid had been combined, together with acetic and hydrosulphuric acids. During the evaporation of the liquid, the two latter were expelled, and a thick straw-coloured liquor of a syrupy consistence and sweet taste remained, which possessed all the common characters of the oxide of glyceril.

The following are the results of the analyses of sericine.

I. 0·3045 gramme of substance yielded 0·344 gramme of water, and 0·832 gramme carbonic acid.

II. 0·406 gramme of substance yielded 0·452 gramme of water, and 1·104 gramme of carbonic acid.

III. 0·310 gramme of substance yielded 0·341 gramme of water, and 0·847 gramme of carbonic acid.

	I.	II.	III.
Carbon	75·55	75·19	75·55
Hydrogen ...	12·18	12·36	12·22
Oxygen	12·27	12·45	12·23
	100·00	100·00	100·00

Lecanu has endeavoured to show that stearin (the stearate of the oxide of glyceril) is composed of two atoms of stearic acid, and one atom of a peculiar oxide of glyceril represented by the formula $C_6\,H_6\,O_4$. But Pelouze has proved that the common oxide of glyceril should be represented by the formula $C_6\,H_7\,O_5$. Meyer, in his researches upon elaidic acid, has also shown that Lecanu's formula for glyceril is correct in some combinations of the fat acids; although it may reasonably be questioned whether it possesses this composition in stearin; for Liebig has shown it to possess the formula $2\,\underline{St} + Gy\,O + 2\,HO$. Liebig does not deny that other oxides of glyceril may exist besides that expressed by the formula $C_6\,H_7\,O_5$; on the contrary, he suggests that there may be several, which may unite with one, two, or three atoms of anhydrous acid, just as there are acids which unite with one, two, or three atoms of a base. But very little is known regarding the nature of glyceril.

Sericine may be an example of such a combination, containing an oxide of glyceril, $C_6\,H_5\,O_3$, capable of uniting with four atoms of an anhydrous fat acid. These different glycerins would be formed by the removal of one or two atoms of water from the common oxide of glyceril. The hydrogen shown by the analysis is a little too high to countenance this idea; but this may be partly accounted for by the æther with which it is prepared, and which adheres to it with much obstinacy. The formula would be $4\,(C_{28}\,H_{27}\,O_3) + (C_6\,H_5\,O_3)$.

	Atoms.		By calculation.
Carbon	118	9019·33	75·65
Hydrogen ...	226	1285·37	11·82
Oxygen	15	1500·00	12·53
		11804·70	100·00

Sericate of the Oxide of Ethyle.—This compound may be formed by sending a stream of muriatic acid gas through a concentrated alcoholic solution of sericic acid. The solution must be kept boiling, in order to ensure the complete decomposition of the chloride of ethyle by the fat acid. After some time, sericic æther collects upon the surface of the alcohol as a colourless oil, which may be purified by simple agitation with distilled water. The agitation with water must be continued until no further smell of muriatic æther can be perceived. It may be rendered still purer, by digesting it with a solution of carbonate of soda, in order to remove any excess of sericic acid; but in this case a considerable portion of the æther is lost. If the stream of muriatic acid be continued sufficiently long, such a precaution is unnecessary. The æther cannot be purified by distillation, as part of it, in this case, appears to be decomposed.

When sericic æther is obtained in the manner now described, it is an oily transparent fluid at common temperatures, destitute both of colour and smell; but when the muriatic æther has not been carefully removed, it possesses a pale straw-yellow colour, and a faint odour. It is lighter than water, having a specific gravity of 0·8641. It may be obtained in beautiful white crystals, by surrounding the liquid with a freezing mixture.

Sericic æther is insoluble in water, but soluble both in alcohol and æther: when boiled for a considerable time with an alcoholic solution of caustic potash, it is decomposed. The following are the results of its analysis, which was performed in the same way as that of other liquid bodies of small volatility.

I. 0·243 gramme of substance, burned with oxide of copper, yielded 0·653 gramme carbonic acid, and 0·273 gramme water.

II. 0·199 gramme of substance, burned with oxide of copper, yielded 0·535 gramme carbonic acid, and 0·221 gramme water.

	I.	II.	Atoms.	By calculation.
Carbon ...	74·30	74·34	60 = 458·61	74·75
Hydrogen	12·48	12·34	60 = 748·77	12·20
Oxygen...	13·22	13·32	8 = 800·00	13·05
	100·00	100·00	6134·87	100·00

Prof. Redtenbacher has shown* that stearic æther is a compound of stearate of the oxide of ethyle with the hydrate of stearic acid, and represented by the formula $2\,\underline{\mathrm{St}}\,\mathrm{Ae\,O} + 3\,\mathrm{HO}$. As stearic acid is a bibasic acid, four atoms of base are combined with two of acid. Sericic æther is a body of an analogous composition, and forms a true double salt—sericate of oxide of ethyle and sericate of water. Its formula therefore, is $(\mathrm{Se} + \underline{\mathrm{Ae}}\,\mathrm{O}) + (\mathrm{Se} + \underline{\mathrm{H}}\,\mathrm{O})$.

Two atoms of sericic acid ... = C_{56}, H_{54}, O_{6}.
One atom of oxide of ethyle = C_{4}, H_{5}, O_{1}.
One atom of water ... ... = H_{1}, O_{1}.

Sericate of the oxide of ethyle = C_{60}, H_{60}, O_{8}.

The formation of sericic æther may be easily explained; the muriatic æther, formed by passing muriatic acid gas through a solution of sericic acid in alcohol, is decomposed by the fat acid; sericic æther is thus produced, and attaches itself to an equivalent of the undecomposed hydrate.

Sericate of Barytes.—This salt may be prepared from sericate of potash, by adding an alcoholic solution of the latter to a pure salt of barytes. A bulky white precipitate is thus obtained, which must be thrown on a filter, and well washed. It is slightly soluble both in water and in alcohol; in the former it possesses nearly the same degree of solubility as gypsum.

I. 0·797 gramme of the salt left, after ignition, 0·266 gramme carbonate of barytes; and 0·858 gramme, burned with chromate of lead, yielded 0·691 gramme water, and 1·702 gramme carbonic acid.

II. 0·481 gramme of the salt left, after ignition, 0·161 gramme carbonate of barytes; and 0·319 gramme, burned with oxide of copper, yielded 0·257 gramme water, and 0·634 gramme carbonic acid.

	I.	II.	Atoms.		By calculation.
Carbon ...	56·91	57·09	28	2140·18	57·32
Hydrogen	8·94	8·95	27	336·94	9·02
Oxygen...	8·26	8·09	3	300·00	8·04
Barytes...	25·89	25·97	1	956·88	25·62
	100·00	100·00		3734·00	100·00

The absolute quantity of carbon obtained in the analysis was under that stated: in the first 54·85 is the quantity shown by the analysis, but 2·06 per cent. must be added for the carbonic acid retained by 25·89 of barytes, as carbonate of ba-

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* *Annalen der Chemie und Pharmacie*, xxxv. 1.

rytes. In the second also 54·95 is expressed by the analysis, but 2·14 being added for the carbon retained by 25·97 barytes, gives the number stated above.

The mean of the two analyses makes the quantity of barytes contained in the salt 25·93 per cent.: the quantity found by calculation is 25·62.

Sericate of Silver.—This salt may be prepared by double decomposition in the same manner as sericate of barytes.

When it is newly precipitated, it is a bulky white powder, which speedily acquires a lilac colour by exposure to light. To avoid this it must be washed in the dark. It is insoluble in water, but very soluble in a solution of caustic ammonia, from which it may be obtained, by spontaneous evaporation, in large transparent colourless crystals. It melts upon the application of heat, but at the same time undergoes decomposition.

I. 0·361 gramme of substance, burned with oxide of copper, gave 0·267 water, and 0·646 carbonic acid.

II. 0·340 gramme of substance, burned with oxide of copper, gave 0·610 carbonic acid, and 0·243 water.

III. 0·553 gramme of substance, burned with oxide of copper, gave 0·992 carbonic acid, and 0·4015 water.

0·704 gramme of salt left, after ignition, 0·277 metallic silver.

	I.	II.	III.
Carbon	49·48	49·61	49·60
Hydrogen	8·03	7·94	8·06
Oxygen	7·82	7·78	7·67
Oxide of silver	34·67	34·67	34·67
	100·00	100·00	100·00

If we suppose the salt to be composed like that of barytes of one atom of sericic acid and one atom of oxide of silver, then the numbers given below should represent the analysis.

			By calculation.
28	Carbon	2140·18	50·61
27	Hydrogen	336·94	7·94
3	Oxygen	300·00	7·13
1	Oxide of silver	1451·61	34·32
		4228·73	100·00

The difference between the carbon found by analysis and calculation is very considerable, but the close correspondence between the hydrogen and the oxide of silver leave little doubt that the formula is the same as that of the salt of barytes. At the same time it is proper to mention that the close correspondence between the analyses themselves, which were executed on salts prepared at different times, and with scrupulous attention to their purity, lead us to question whether some other formula should not be adopted for it. Salts of silver are generally anhydrous, but this does not always appear to be the case; for Crasso has lately described* a salt of silver which contains water. Sericate of silver may be an example of a similar compound, corresponding to Johnston's sulphate of lime, in which two atoms of salt are united with one atom of water. This is merely brought forward as a conjecture, want of material having prevented my making any further experiments on this subject. The calculated result, however, agrees pretty closely with the analysis.

		By calculation.
56 Carbon	4280·36	49·94
55 Hydrogen......	686·37	8·00
7 Oxygen	700·00	8·19
2 Oxide of silver	2903·22	33·87
	8569·95	100·00

Hence the formula would be $2\,\underline{\mathrm{Se}}\,\mathrm{Ag}\,\mathrm{O} + \mathrm{H}\,\mathrm{O}$.

Sericate of Potash.—This compound may be prepared by melting pure carbonate of potash and sericic acid with a small quantity of water. The mixture must then be gently heated, in order to effect the complete saponification of the acid, and afterwards evaporated to dryness on the water-bath. The residue is now to be digested with absolute alcohol, which dissolves the sericate of potash, but leaves the carbonate of potash undissolved.

Sericate of potash is very soluble both in hot and cold alcohol and water. When it is dissolved in a hot solution of alcohol, it is partly deposited on cooling in the form of beautiful white crystalline scales. It is insoluble in æther.

We possess no data for determining how we should estimate the carbonic acid in the analysis of a salt of potash formed by an organic acid. Liebig states, in his treatise on Organic Analysis, that the potash remains after the combustion as a carbonate; and consequently that an atom of carbon should be added to the result of our analysis. But experiments made on this subject by Prof. Redtenbacher, Dr. Varrentrapp and myself, show that this cannot be relied on, when the salt is burned with oxide of copper. Dr. Varrentrapp mixed the carbonates of potash and soda with oxide of copper, and exposed them to the action of heat in a common tube of com-

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* Liebig's *Annalen*, Band xxxiv. 1. 79.

bustion. As the result of his experiments, he ascertained that a quantity of carbonic acid always passed into the apparatus, containing potash. This gain was generally about one-third of the potash employed, two-thirds still remaining in combination with the alkali. By subtracting this from the weight of the carbonic acid obtained in the experiment, and calculating the potash as a neutral carbonate, we cannot be far from the truth. This has been done by Redtenbacher * in his researches upon stearic acid, and is the only one at present which can be adopted.

I. 0·354 gramme of substance gave 0·324 water, and 0·797 carbonic acid.

II. 0·324 gramme of substance gave 0·296 water, and 0·727 carbonic acid.

0·404 gramme gave 0·130 sulphate of potash.

The first is equal to 62·25 per cent., the second 62·04 per cent. carbonic acid; but on the supposition that the potash remains after the combustion, as a carbonate, then 2·25 per cent. of carbon must have remained with it. Only two-thirds of this, however, should be added to the carbon actually procured in the analysis; hence the result is as follows:

	I.	II.	Atoms.		
Carbon ...	63·75	63·54	28	2140·18 ...	63·56
Hydrogen	10·16	10·15	27	336·94 ...	10·00
Oxygen...	8·70	8·92	3	300·00 ...	8·92
Potash ...	17·39	17·39	1	589·91 ...	17·52
	100·00	100·00		3367·03	100·00

The formula of the salt is $\underline{Se}+KO$. Before sericate of potash is subjected to analysis, it must be repeatedly dissolved in water and evaporated to dryness, for the alcohol employed in its preparation adheres to it with so much tenacity that it cannot be expelled, unless this process is adopted.

Sericate of Soda.—This salt may be prepared in the same manner as the last. It is soluble in water and alcohol, but insoluble in æther. For the reasons already stated under "sericate of potash," the salt of soda was not analysed, as its analysis could lead to no satisfactory results.

Sericate of Lead.—This salt was obtained by digesting sericine for several days with basic acetate of lead $(\underline{Ac}+6\,PbO)$. It is a dense white powder, insoluble in water, and insoluble or very slightly soluble in alcohol. After digesting for a considerable time, the salt must be thrown on a filter, and washed very carefully with water. The lead was estimated

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* *Annalen der Chemie und Pharmacie,* xxxv. 1.

by igniting the salt, ascertaining the weight of the lead, and oxide of lead, which remained; washing the mixture with weak acetic acid by decantation, and calculating the loss as oxide of lead.

It has been shown* that both benzoate and margarate of lead contain acetic acid in chemical combination, and not as a mere accidental constituent arising from imperfect washing. Hence it was necessary to ascertain whether or not sericate of lead contained this acid. For this purpose a few grains of the salt were moistened with alcohol and sulphuric acid, and subjected to distillation. Acetic æther passed over and was easily recognized by its well-known properties. The quantity of acetic acid contained in the salt, appeared to be very small.

0·985 gramme of salt left, after ignition, 0·421 gramme of lead and oxide of lead; of this 0·55 gramme was oxide of lead, and 0·366 gramme metallic lead: both together are equal to 45·58 per cent. of oxide of lead.

0·456 gramme, burned with oxide of copper, gave 0·684 gramme carbonic acid, and 0·273 gramme water.

		Atoms.		By calculation.
Carbon	41·48	116	8866·46	41·21
Hydrogen...	6·65	111	1385·22	6·48
Oxygen	6·29	15	1500·00	6·94
Oxide of lead	45·58	7	9761·50	45·58
	100·00		21513·18	100·00

We have already seen that sericine contained four atoms of sericic acid, and one atom of oxide of glyceril. In the sericate of lead made from sericine, the oxide of glyceril appears to be displaced by one atom of basic acetate of lead $(\underline{\mathrm{Ac}}+3\,\mathrm{Pb\,O})$.

4 atoms of neutral sericate of lead $= C_{112}+H_{108}+O_{12}+\mathrm{Pb}\,O_4$
1 atom of basic acetate of lead $= C_{4}+H_{3}+O_{3}+\mathrm{Pb}\,O_3$

1 atom of sericate of lead $= C_{116}+H_{111}+O_{15}+\mathrm{Pb}\,O_7$

The formula would therefore be $4\,(C_{28}\,H_{27}\,O_3+\mathrm{Pb}\,O) + (C_4\,H_3\,O_3+\mathrm{Pb}\,O_3)$.

Another salt of sericic acid and oxide of lead may be obtained by adding acetate of lead to a solution of sericate of potash. But from the great disposition of oxide of lead to form basic salts, a mixture of salts appears, in this case, to be

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* *Annalen der Chemie und Pharmacie*, xxxv. 1.

formed; at least the results of the analysis of a salt thus obtained showed no definite composition*.

Before concluding this paper it may be interesting to mention a few facts connected with the butter of nutmegs. It has been already stated, that when this butter is digested with alcohol of the common strength, a soft fat of a red colour is dissolved: when this fat is distilled with a large quantity of water, a colourless oil passes over. This has an agreeable pungent odour, and is probably the same as that described by John and others.

But if this red fat be subjected to dry distillation, several interesting compounds are formed, which, however, have not been examined more closely. The same oil passes over as that obtained by distillation with water, but as the heat increases it is accompanied by a white crystalline fat, which after being purified presents the characters of paraffin. A black matter like humus remains in the retort, and may be easily saponified by continued digestion with caustic potash; when the soap thus formed is dissolved in water, and decomposed by muriatic acid, a mixture of fat acids is separated. By dissolving this in weak alcohol, and allowing it to evaporate, a black fat exactly similar in appearance to humus is deposited. Upon further evaporation a white fat is likewise separated, which may be purified by repeated solutions in alcohol, and digestion with animal charcoal. The black fat is soluble in alcohol and æther; with the latter it forms a solution of a syrupy consistence. This solubility in æther shows that it is not humic acid. The colour of the acid does not appear to be merely accidental; but the examination of these substances was not proceeded with, for being products of decomposition and not at all crystalline, there were no means of ascertaining when they were sufficiently pure for analysis.

St. Andrews, Oct. 28, 1840.

XXI. *Abstract of recent Researches on the Constitution of the fatty Substances, made by* MM. REDTENBACHER, VARRENTRAPP, MAYER *and* BROMEIS[†].

FROM the original analyses of Chevreul, the composition of hydrated stearic acid appeared to be

$$\left.\begin{array}{lr} \text{Carbon} \ldots\ldots & 77\cdot42 \\ \text{Hydrogen} \ldots & 12\cdot43 \\ \text{Oxygen} \ldots\ldots & 10\cdot15 \end{array}\right\} 100.$$

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[*] Sericate of copper may be obtained by double decomposition; it is of a green colour, insoluble in water, and contains water in chemical combination; the sericates of zinc, lime, cobalt, &c. may be procured in a similar manner, but their examination was quite unnecessary.

[†] From the *Annalen der Pharmacie*, July, August, and September, 1840.

From which results the formula $C_{70}\,H_{69}\,O_7$, which was adopted by Berzelius and most other chemists; and the acid being bibasic, the formula for the anhydrous acid was $C_{70}\,H_{67}\,O_5$. As, however, in organic analyses the hydrogen is valued a little too high, Liebig proposed lately the formula $C_{70}\,H_{66}\,O_5$. In order to decide upon the constitution of these bodies, M. Redtenbacher commenced a complete investigation of them at Liebig's request.

The result was that the stearic acid was found, by a careful series of analyses executed on it in its hydrated condition, and on a great variety of its salts, to be represented in its anhydrous condition by the formula $C_{68}\,H_{66}\,O_5$, and when hydrated by $C_{68}\,H_{66}\,O_5 + 2\,\mathrm{Aq}$; the latter is composed of

$$\left.\begin{array}{ll} \text{Carbon} \;.\;.\;.\;.\;. & 77{\cdot}04 \\ \text{Hydrogen} \;.\;.\;.\;. & 12{\cdot}58 \\ \text{Oxygen} \;.\;.\;.\;.\;. & 10{\cdot}38 \end{array}\right\} 100{\cdot}0$$

with which the numerous experimental results perfectly agreed.

Among other collateral evidence was the analysis of stearic æther, which was found to consist of $C_{72}\,H_{72}\,O_7$, being produced by

$$\left.\begin{array}{lr} \text{1 atom of Stearic acid} & C_{68}\,H_{66}\,O_5 \\ \text{1 atom of Æther}\;\;.\;. & C_4\;\,H_5\;\,O\; \\ \text{1 atom of Water}\;\;.\;. & H\;\;\,O\; \end{array}\right\} C_{72}\,H_{72}\,O_7.$$

The substance described by Lassaigne as stearic æther, could not be formed.

When stearic acid is distilled, there is generated a large quantity of margaric acid and other products, with which Chevreul did not occupy himself (margarone, &c.), and the first full explanation of the process is due to M. Redtenbacher.

The formula of hydrated margaric acid hitherto generally received was $C_{70}\,H_{66}\,O_8$, which was such that this acid might be looked upon as a compound of

$$\left.\begin{array}{lr} \text{1 atom of Stearic acid} & C_{70}\,H_{68}\,O_7 \\ \text{1 atom of Oleic acid} & C_{70}\,H_{62}\,O_7 \\ \text{2 atoms of Water}\;.\;.\;. & H_2\;\,O_2 \end{array}\right\} \frac{C_{140}\,H_{132}\,O_{16}}{2}.$$

Berzelius suggested also that a carbo-hydrogen, $C_{78}\,H_{67}$, may be supposed to be the common radical of the stearic and the margaric acid, which would then differ only in having different degrees of oxidation. The composition of the margaric acid has, however, been determined afresh by Varrentrapp, with the following results.

Margaric acid, whether prepared directly from human fat or by the destructive distillation of tallow, lard, oleic acid or olive oil, is identical in constitution, being when hydrated expressed by $C_{34} H_{34} O_4$, or when anhydrous, by $C_{34} H_{33} O_3$.

The margaric æther consists of an atom of margaric acid and one of æther, $C_{34} H_{33} O_3 + C_4 H_5 O$.

Varrentrapp analysed the margarone which is produced when margaric acid is distilled with lime. Its formula is $C_{33} H_{33} O$, or it is formed by the loss of the elements of one atom of carbonic acid.

From these results, however, it follows equally, as from the older, that the carbon and hydrogen is the same in the stearic and the margaric acids, which differ only in the proportion of oxygen,

1 equivalent of Stearic acid = $C_{68} H_{66} O_5$
2 equivalents of Margaric acid = $C_{68} H_{66} O_6$

and hence Berzelius's idea is still quite true, that if we express $C_{34} H_{33}$ by R, then

Margaric acid = $R + O_3$, like sulphuric acid $S + O_3$
Stearic acid = $R_2 + O_5$, like hyposulphuric acid $S_2 + O_5$.

But it still remains to be shown how destructive distillation, which is in general not an oxidating process, changes both the stearic and oleic acids into margaric acid. This has been explained by Redtenbacher. Besides margaric acid there are produced a small quantity of water and of carbonic acid, and margarone and a liquid polymeric with olefiant gas in large proportion, so that the process consists in four equivalents of stearic acid producing

6 equiv. of hydrated margaric acid = $C_{204} H_{204} O_{24}$
1 —— water . . . . = $H \; O$
1 —— margarone . . . . . . = $C_{33} H_{33} O$
1 —— carbonic acid . . . . $C \; O_2$
Polymeric carburetted hydrogen $C_{34} H_{34}$

$C_{68} H_{68} O_7 \times 4$ = $C_{272} H_{272} O_{28}$

The action of nitric acid on stearic acid has been accurately studied by Bromeis, and has produced extremely interesting results. In the first instance an equivalent of anhydrous stearic acid is converted into two equivalents of anhydrous margaric acid, by the absorption of an equivalent of oxygen, $C_{68} H_{66} O_5$, and O producing twice $C_{34} H_{33} O_3$.

When the action of the nitric acid is continued, suberic acid is produced in large quantity, the formula of it being $C_8 H_6 O_3 + Aq$.

The mother liquors from which the suberic acid had separated contains a large quantity of succinic acid, the formula of which is, in the crystallized form, $C_4 H_3 O_4$.

The oleic acid, when treated with nitric acid, produces also margaric and suberic acid. The mother liquor from which these have separated contain the pimelic and adipic acids discovered by Laurent. For the first, M. Bromeis verified Laurent's formula, $C_7\ H_5\ O_3 + Aq$, but for the second he obtained a different result, viz. $C_{14}\ H_9\ O_7 + 2\ Aq$.

Laurent had supposed that œnanthic acid was produced in this process, but Bromeis could not obtain any satisfactory evidence of its formation, nor has he as yet been able to verify the history of the lipic and azoleic acids as given by that chemist.

The drying oleic acid from linseed oil gave with nitric acid neither pimelic acid nor adipic acid, but a large quantity of oxalic acid, of which the proper oleic acid yields none.

It has been long known that when olive oil is mixed with a solution of proto-nitrate of mercury it becomes solid, and this property, which was once made use of to determine the purity of olive oil, has been found to belong to almost all fatty bodies in a greater or less degree. Boudet proved that the action of the mercurial solution depended exclusively on the hyponitrous acid in the liquor, and that the same effect was produced by hyponitrous or nitrous acid fumes alone. He also pointed out that the solid mass produced was a compound of glycerine with a peculiar acid, which he termed the *elaidic*, as he gave the name of *elaidine* to this artificial fat.

The only numerical results obtained of the constitution of this acid were given by Laurent. From his analyses the formula for the crystallized elaidic acid should be $C_{35}\ H_{33}\ O_3 + Aq$, and for the elaidic æther the formula $C_{35}\ H_{33}\ O_3 + C_4\ H_5\ O$.

From this constitution, however, no insight into its origin could be obtained, and Mayer undertook in Liebig's laboratory the complete revision of its history.

M. Mayer found the modes of preparation prescribed and followed by Boudet and by Laurent quite insufficient to obtain a product of absolute purity. He arrived ultimately at the result that it is only from the oleic acid that the elaidic acid is produced, and hence to form it he had recourse to absolutely pure oleic acid prepared by the method of Varrentrapp, which will be described by and by. It may also be prepared nearly, but not so absolutely pure, by passing a stream of nitrous acid vapour through oil of sweet almonds. Nitric oxide gas passes off, and the oil, when exposed to cold, gives a copious crop of crystals of elaidic acid.

Elaidic acid is of a brilliant white colour, crystallizes in large tables, and melts at 112° Fahrenheit. It is easily soluble in alcohol, from which it separates in crystals like benzoic acid. It is also soluble in æther. These solutions react acid.

When elaidic acid is treated with a great excess of caustic potash it is completely decomposed, and a new acid formed identical with that which Varrentrapp had found oleic acid to give under the same circumstances: with carbonated alkalies it produces, however, soaps from which it separates on the addition of a stronger acid unchanged.

The elaidates of silver, lead, and barytes are voluminous white precipitates; that of silver may be obtained crystallized from an ammoniacal solution.?

Numerous analyses of crystallized elaidic acid gave for its composition as follows:

72	atoms of Carbon ...	5503·3	78·04
68	—— Hydrogen	848·6	12·03
7	—— Oxygen...	700·0	9·93
		7051·9	100·00

The elaidate of silver consisted of

72	atoms of Carbon	5503·3	56·56
66	—— Hydrogen......	823·6	8·46
5	—— Oxygen	500·0	5·14
2	—— Oxide of silver	2903·2	29·84
		9730·1	100·00

Its formula is therefore $C_{72} H_{66} O_5 + 2 Ag O$; and the crystallized acid is expressed by $C_{72} H_{66} O_5 + 2 Aq$, it being a bibasic acid.

The composition of the elaidic æther agrees also with these results; it consists of

76	atoms of Carbon ...... =	5809·1	78·42
72	—— Hydrogen ...	898·5	12·13
7	—— Oxygen	700·0	9·45
		7407·6	100·00

Its formula is therefore $C_{72} H_{66} O_5 + C_4 H_5 O + Aq$.

These results differ totally from those given by Laurent, who evidently worked upon an impure substance.

Sebacic acid.—This acid is produced whenever any fatty substances, as the fat oils, lard, &c. are distilled. Its composition has been ascertained by Redtenbacher, who confirmed fully the result long since given by Dumas, that the formula of the crystallized acid is $C_{10} H_8 O_3 + Aq$.

Redtenbacher has, however, established the interesting fact, that in this distillation it is only the oleic acid which produces, by its decomposition, the sebacic acid. Neither glycerine, nor the stearin, nor the margaric acids yield any trace of sebacic acid when distilled. Its origin must therefore depend upon the constitution of the oleic acid.

Oleic acid.—This acid, one of the most important of the series of fatty bodies, is still but imperfectly known. Since its original discovery by Chevreul it has been examined only by Laurent; but he distilled the acid he employed, and as in this process it is almost totally decomposed, the results at which he arrived were very incorrect.

The chemical history of oleic acid has been re-examined by Varrentrapp. He has found the purest source of it to be oil of sweet almonds, which contains no stearic acid, and but a trace of margaric acid. The almond oil is to be saponified by means of a solution of caustic potash, and this decomposed by dilute sulphuric acid. The impure oleic acid thus obtained is to be digested at 212° for many hours with oxide of lead, and then the mass produced macerated in the cold with æther. The oleate of lead dissolves readily in æther, but the margarate is insoluble therein, and by this means the former is obtained completely pure. The filtered liquor is to be then mixed with its own volume of water, and as much dilute muriatic acid to be added as converts the oleate into chloride of lead. This deposits itself completely in the water below, and the oleic acid remains dissolved in the æther. When this is distilled off the oleic acid remains with the properties described by Chevreul.

In the same manner the oleic acid of tallow or lard may be obtained pure.

A series of ten analyses, which agreed very well with one another, gave for the oleic acid the following composition:

44	atoms of Carbon	3363·1	77·10
40	—— Hydrogen ...	499·2	11·44
5	—— Oxygen	500·0	11·46
		4362·3	100·00

The composition of oleate of barytes was found to be

44	atoms of Carbon	= 3363·1	64·59
39	—— Hydrogen	= 486·7	9·35
4	—— Oxygen	= 400·0	7·68
1	—— Barytes	= 956·9	18·38
		5206·7	100·00

Its formula is therefore $C_{44}\,H_{39}\,O_4 + Ba\,O$, and that of the acid analysed is $C_{44}\,H_{39}\,O_4 + Aq$.

The oleic æther was $C_{44}\,H_{39}\,O_4 + C_4\,H_5\,O$.

When oleic acid is distilled, there is produced a small quantity of carbonic acid, sebacic acid, a large proportion of a liquid having the composition of olefiant gas, and some charcoal remains: thus

$$\left.\begin{array}{lll} \text{1 atom of} & \text{Sebacic acid} & C_{10}\,H_9\,O_4 \\ \text{3 atoms of} & \text{Carbonic acid} & C_3\qquad O_6 \\ \text{4 ——} & \text{Carbon} \ldots\ldots & C_4 \\ \text{Carburet of} & \text{Hydrogen} \ldots & C_{71}\,H_{71} \end{array}\right\}\text{are formed from}$$

$$\text{2 atoms Oleic acid} = C_{88}\,H_{80}\,O_{10}.$$

As no other fatty acid produces sebacic acid when distilled, its formation is a test of the presence of oleic acid. Hence we may know if wax or spermaceti, or stearic acid, have been adulterated with tallow by submitting a small quantity to distillation.

When the oleic acid is warmed with an excess of caustic potash and a few drops of water, until it be completely saponified, and then more potash being added, the temperature be raised to the melting point of the latter, the mass, which should be constantly stirred, does not become brown, but disengages a quantity of hydrogen gas. When the mass so produced is dissolved in water and decomposed by an acid, a white solid separates in large crystalline grains like stearine. Elaidic acid treated in the same way yields an identical product.

When crystallized from its alcoholic solution this new acid fuses at 143° Fahrenheit; when analysed it gave the following result:

32	atoms of	Carbon...	2445·9	75·69
31	——	Hydrogen	386·0	11·97
4	——	Oxygen...	400·0	12·34
			3231·9	100·00

The silver salt gave as follows:

32	atoms of	Carbon	2445·9	53·54
30	——	Hydrogen...	374·4	8·20
3	——	Oxygen......	300·0	6·50
1	——	Oxide of silver	1451·6	31·76
			4571·9	100·00

The formulæ are, therefore, for

the silver salt .... $C_{32}\,H_{30}\,O_3 + Ag\,O$

the crystallized acid $C_{32}\,H_{30}\,O_3 + Aq$.

The liquor from which this new acid is precipitated contains abundance of acetic acid formed at the same time. The reaction may be easily explained as follows:

$$\left.\begin{array}{ll} \text{1 atom of oleic acid} & C_{44}H_{39}O_{4} \\ \text{and 8 atoms of water . . .} & \quad H_{8}\ O_{8} \end{array}\right\} C_{44}H_{47}O_{12},$$

may produce

$$\left.\begin{array}{ll} \text{1 atom of the new acid} & C_{32}H_{30}O_{3} \\ \text{3 atoms of acetic acid} & C_{12}H_{9}\ O_{9} \\ \text{and 8 atoms of hydrogen} & \quad H_{8} \end{array}\right\} C_{44}H_{47}O_{12};$$

or from elaidic acid

$$\begin{array}{ll} \text{1 atom of elaidic acid} & C_{72}H_{66}O_{5} \\ \text{7 atoms of water . . .} & \quad H_{7}\ O_{7} \\ \cline{2-2} & C_{72}H_{73}O_{12} \end{array}$$

may produce

$$\begin{array}{ll} \text{2 atoms of the new acid} & C_{64}H_{60}O_{6} \\ \text{2 atoms of acetic acid} & C_{8}\ H_{6}\ O_{6} \\ \text{and 7 atoms of hydrogen . .} & \quad H_{7} \\ \cline{2-2} & C_{72}H_{73}O_{12} \end{array}$$

Some traces of carbonic and oxalic acids which appear are not here introduced.

The production of elaidic acid from oleic acid cannot as yet be explained. There are also other products, particularly one, a red or orange-coloured liquid, the formation of which is the cause of the citrine tinge in ordinary elaidine. This substance could not be got in a state fit for analysis.

XXII. *Mineralogical Notices from Foreign Journals*[*].

ANTIGORITE. BY MM. WISER AND SCHWEIZER.

[From Poggendorff's *Annalen*, vol. xlix. Part 4.]

THE characters assigned by M. Wiser to this mineral are as follows:—not crystalline; hardness = 2·5 (scratches gypsum, is scratched by calcspar); specific gravity 2·622, slightly lustrous; in thin plates semi-transparent; in very thin leaves transparent; colour by reflected light blackish green; by transmitted light leek-green; it does not affect the magnetic needle. Two analyses by M. Schweizer gave

Silica	46·22	46·18
Protoxide of iron	13·05	12·68
Alumina	2·08	1·89
Magnesia	34·39	35·19
Water	3·70	—
	99·44	99·64

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[*] Communicated by Mr. W. Francis.

Its constitution may be expressed by the formula

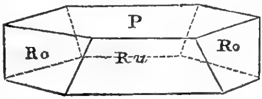
$$\left.\begin{matrix}\dot{M}g^3\\ Fe^3\end{matrix}\right\}\dddot{S}i^2+\dot{M}g\,\underline{\dot{H}}.$$

which at the same time indicates its near relation to serpentine.

PENNINE. BY PROF. J. FRÖBEL AND M. SCHWEIZER.

[From Poggendorff's *Annalen*, vol. l. Part 3.]

Under this name (derived from the Pennine Alps, in which district, near Zermatt, it occurs) Prof. J. Fröbel has described a mineral bearing great resemblance to the chlorite from Achmatowsk and the Zillerthal. Hexagonal, with very distinct basal structure. The well-developed crystals generally present at times the tabular, at times the columnar combination of the faces of a rhombohedron with the horizontal pair of faces, as shown in the annexed wood-cut.



The values P on Ro = 99°, of Ro on Ru = 118°. Hardness on the basal faces somewhat above that of gypsum, on the rhombohedral faces not quite equal to that of calcspar. Thin leaves flexible, but not elastic. By reflected light, black-green; by transmitted light, in the direction of the principal axis between emerald-green and leek-green, at right angles to the principal axis brown to hyacinth-red. Lustre vitreous; when not too thick, perfectly transparent. The results of two analyses by M. Schweizer gave

Silica	33·82	33·07
Protoxide of iron	11·30	11·36
Alumina	9·32	9·69
Magnesia	33·04	32·34
Water	11·50	12·58
	98·98	99·04

leading to the formula

$$\left.\begin{matrix}\dot{M}g^3\\ \dot{F}e^3\end{matrix}\right\}\dddot{S}i^2+\underline{\dddot{A}l}\,\dddot{S}i^3+7\,\dot{M}g\,\underline{\dot{H}},$$

which is constructed according to the same principle employed by M. Varrentrapp for chlorite.

CHLOROSPINELLE AND XANTHOPHYLLITE. BY PROF. G. ROSE.

[From Poggendorff's *Annalen*, vol. l. Part 4.]

Chlorospinelle occurs crystallized in octahedrons generally, but of from one to two lines in size; colour grass-green; transparent at the margins; lustre vitreous; of the hardness of topaz; specific gravity 3·594. According to two analyses performed by Prof. H. Rose, it consists of

Magnesia	26·77	27·49
Lime	0·27	—
Peroxide of copper	0·27	0·62
Alumina	64·13	57·34
Peroxide of iron	8·70	14·77
	100·14	100·22

resembling, therefore, spinelle and zeylanite, and belonging with these, since it likewise coincides in its crystalline form, to the same genus. Locality, Slatoust.

Xanthophyllite, so named from its wax-yellow colour and laminar structure, constitutes a new genus. In thin leaves transparent; on the surface of fracture it has a strongly nacreo-vitreous lustre. Hardness that of felspar; specific gravity =3·044. Before the blow-pipe it proved to contain alumina, lime, soda, peroxide of iron and silica, but no fluoric acid, magnesia or potash. It occurs near Slatoust.

PIKROPHYLL. BY A. F. SVANBERG.

[From Poggendorff's *Annalen*, vol. l. Part 4.]

This new mineral occurs near Sala. Structure foliated; hardness between that of mica and calcspar; specific gravity 2·73; colour, very dark green. Analysis gave

		Oxygen.	
Silica	49·80	25·88	26·40
Alumina	1·11	0·52	
Lime	0·78	0·22	13·43
Magnesia	30·10	11·65	
Protoxide of iron	6·86	1·56	
Protoxide of manganese	a trace		
Water	9·83		
	98·48	8·73	

whence the mineralogical formula $3\ MS^2 + 2\ Aq$.

XXIII. *On Artificial Oil of Ants.* *By* J. STENHOUSE, *Esq.**

IT was first observed by Dœbereiner, when preparing formic acid by the action of sulphuric acid and oxide of manganese on sugar, that the liquor which distilled over contained

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* Communicated by the Author.

a small quantity of oily matter which rendered it milky. To this he gave the somewhat fanciful name of "artificial oil of ants." From the extremely small quantity in which it is produced by this process, he did not succeed in collecting it, or in determining its properties.

In repeating Professor Emmet's process for formic acid, which consists in distilling grain of various kinds with sulphuric acid, without any oxide of manganese, I observed that under certain circumstances the liquid in the receiver contained considerable quantities of this oil. After repeated trials I succeeded in pretty easily preparing it. It is a constant product, though in very different quantities, of the action of sulphuric acid on vegetable substances, and it may be conveniently procured from grain of any kind, saw-dust, husks of corn, &c.

The process which I found to succeed best is the following. Take equal weights of oatmeal or saw-dust and sulphuric acid, diluted with its own bulk of water, and introduce the mixture into a large copper still. As there is no oxide of manganese present there is no frothing up of the liquid, so that the still may be safely filled rather more than half-full; when the mixture has boiled some time and the meal is quite charred, pour back the liquid which has distilled, adding the same quantity of water as at first, and push the distillation nearly to dryness. The liquid which passes over into the receiver is slightly milky, owing to the presence of the oil, and at the same time strongly acid from the sulphurous and formic acid which it contains. It is to be neutralized with slaked lime, when it becomes of a deep yellow colour, and is again to be slowly distilled till about a fourth or fifth part of it has passed over. This is to be mixed with a considerable quantity of fused chloride of calcium, and subjected to a second distillation. The chloride of calcium retains a great part of the water, in which the oil is pretty soluble: if too much water still passes over after a second rectification, a new quantity of chloride of calcium must be added, and the distillation repeated a third time. The oil comes over mixed with a little water, and sinks to the bottom of the receiver; about twelve pounds of meal yielded nearly one ounce of oil, so that it is by no means an abundant product.

Properties.—The anhydrous oil is transparent when newly prepared, nearly colourless, having only a greenish-yellow tinge, but when kept some time it becomes of a brownish-red, from the formation of a resinous matter. Its taste and smell are very pungent and aromatic, resembling oil of Cassia. It stains paper, but not permanently. It catches fire very easily, and burns with a strong yellow flame, emitting much smoke. It boils at 168° C. Its specific gravity is 1·1006 at 27° C. It is pretty soluble in water, and much more so in alcohol and æther. Potassium effervesces strongly when placed in it, but neither the alcoholic nor aqueous solution of potash has any effect upon it. Solid potash, with the assistance of heat, converts it into a brown resin. It dissolves iodine very easily, but without any violent action even when heated. In the cold muriatic acid gives it a fine red colour, and with the assistance of heat turns it into a dark brown resin. Nitric acid first reddens, then chars it. Sulphuric acid has a similar effect.

The results of its analyses are as follows:

I. 0·330 substance gave 0·747 carbonic acid, and 0·130 water.

II. 0·324 gave 0·725 carbonic acid, and 0·1275 water.

III. 0·3185 gave 0·7205 carbonic acid, and 0·128 water.

	Found.			Calculated.
	I.	II.	III.	
Carbon ...	62·59	61·87	62·55	62·94
Hydrogen	4·37	4·37	4·46	4·11
Oxygen...	33·04	33·76	32·990	32·94
	100·00	100·00	100·00	99·99

This gives the formula:

Carbon 5 atoms ...... =382·175
Hydrogen 4 atoms ...... = 24·959
Oxygen 2 atoms ...... =200·000

Atomic weight 607·134

It is evident that this oil has a very different composition from oils in general, as it contains oxygen and hydrogen only in the proportions to form water, while all other known oils contain a great excess of hydrogen.

XXIV. *Facts and Observations relating to the Science of Phonetics.* By R. G. LATHAM, *Fellow of King's College, Cambridge.*[*]

PART I.—*Upon the Aspirates (real and accredited) of* K *and* G.

THE object of the present paper is to show that the real aspirates of K and G are not the sounds that are generally considered as such; in other words, that they are

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[*] Communicated by the Author.

not the powers of the German *ch* and Scotch *gh*, but that they are sounds perfectly distinct from either the one or the other of those articulations.

The question in point, besides its value in etymology, has a value in acoustics; at least in that particular part of them, that deals with the affinities and analogies of articulate sounds.

This part of acoustics may conveniently be called the province of phonetics.

How far the articulate sounds are systematically related to each other; how far and in what cases they run into each other; how far the chain of relationship is lineal, and how far it is circular; how far vowels and consonants, mutes and liquids differ in kind from each other; these questions, and questions similar to these, constitute the province of phonetics.

These investigations must be distinguished, on the one hand, from those of physiology, and on the other hand, from those of etymology.

The science of phonetics determines how two given articulations are related; the science of physiology inquires how they are produced.

Phonetics tell us that between two articulations an immutable and essential relation exists; whilst etymology observes that under certain circumstances one articulation is changed into another. Very often etymology does more; it assumes the alliance from the change.

Now it does not follow, in etymology, that because in a given language one sound changes to another, those two sounds are, therefore, naturally allied to each other; although such is *often* (far, however, from always) the case.

Nor yet has it been proved in physiology that the sounds which to the ear sound alike, are produced by a like disposition of the parts of the mouth or larynx; although that such a correspondence exists is highly probable.

The truth is, that the special study of phonetics, instead of being promoted by the grammarian and anatomist, has been retarded by them. Etymological and physiological tests, etymological and physiological classifications, have been applied where acoustic principles alone ought to have been recognized. A necessary correspondence, moreover, between the three kinds of sciences, which in grammar may be proved non-existent, and in physiology has not been proved at all, has for the most part been gratuitously assumed; and more than this, in those cases where the three tests have not coincided, the disposition has been to sacrifice the phonetic test to the other two.

The Phonetic test stands thus—Two sounds are allied, because, to the ear, they sound alike.

The physiological test thus—Two sounds are allied, because, in the mouth and larynx, they are formed alike.

The etymological test thus—Two sounds are allied, because, under certain circumstances, one is changed to the other.

Expressed in Latin axioms, the etymological test is, *Propter fortunas articulatio est id quod est*; the physiological one, *Propter formationem articulatio est id quod est*; and, finally, the phonetic one is, *Propter sonum articulatio est id quod est*. The coincidence of the three is (in the present state of our knowledge) to be considered as an accident.

Phonetic science considers sounds irrespective of the signs by which they are expressed, and irrespective of the names by which they are called. The sound of the *b* (in *bat*) is the same, whether it be expressed by the sign *b* or *β*; it is also the same whether it be called *bee* or *beta*. A sound is not double because it is spelt with two letters, nor yet single because it is expressed by one. The *th* in *thin* is a simple sound, irregularly expressed: the *x* in *box* is a double one compendiously spelt. In questions of the kind in point, the eye has so often misled the ear that the above given truisms are scarcely to be considered superfluous. In points of acoustics the blindest guides are the best.

To guard against the influence of names and signs it is convenient to express the relations of certain sounds, arithmetically; and besides this, in the case of such simple single sounds as have no simple single sign (or letter) to denote them, to coin or borrow appropriate signs as the occasion requires.

The subjoined scales of relationship being those that are generally recognized, it is unnecessary to go beyond the mere exhibition of them.

The sound of *b* (in *bin*) is in a certain relation to the sound of *p* (in *pin*). Now,

v : f :: b : p . and
d : t :: v : f . and
ð[1] : þ[2] :: đ : t . and
g : k :: ð : þ . and
z : s :: g : k . and
ζ[3] : σ[4] :: z : s .

[1] The *th* in *thine*. [2] The *th* in *thin*. [3] The *z* in *azure*. [4] *sh* in *shin*.

Again, the sound of *f* (as in *fa*) is in a certain relation to the sound of *p* (as in *pa*); or, expressed more loosely, *f* is in a certain relation to *p*. Now

v : b :: f : p . and
þ : t :: v : b . and
ð : d :: þ : t . and
σ : s :: ð : d . and
ζ : z :: σ : s .

To these two classes of relation the names Hard and Soft, Lene and Aspirate may (for want of better) be allowed to stand; so that the double relationship is expressed in the following table:—

Lene.		Aspirate.	
Hard.	Soft.	Hard.	Soft.
p	*b*	*f*	*v*
t	*d*	þ	ð
k	*g*	?	?
s	*z*	σ	ζ

or

Hard.		Soft.	
Lene.	Aspirate.	Lene.	Aspirate.
p	*f*	*b*	*v*
t	þ	*d*	ð
k	?	*g*	?
s	σ	*z*	ζ

Here we have (so to say) four parallel lines of double relationship. The sequence, however, although perfect as far as it goes, is incomplete. We miss the sounds that are to *k* and *g*, as *f* is to þ and *v* to *b*, &c. &c. Were it not for this deficiency we should have a mute-system of sixteen allied articulations, square and symmetrical, each articulation being to the other doubly related, *i.e.* hard or soft, lene or aspirate. As it has been said before, it is the object of the present paper to fill up the deficiency in question, and, subordinately to the main end, to show that the sounds that are considered to be the aspirates of *k* and *g* are not so in reality. The sounds that in all the works (philological or physiological) with which I am acquainted, have been supposed to hold the relationship in point, are the German *ch* (as in *auch*) and the Gaelic *gh* (as in *lough*) respectively. Let these two sounds be expressed by *kh* and *gh* respectively, and let the undetermined aspirates of *k* and *g* be expressed by K_x and G_x respectively.

The affinities which invalidate the current opinion, are, first, the affinity which exists, and which is recognized, between the mute sounds of *b*, and *v* and the semivowel sound of *w*; and secondly, the affinity (existing and recognized) between the semivowel sounds of *y* (as in *ya*) and the mute sound of *g* (as in *ga*).

The sound of *w* (as in *wa*) is to the sound of *v* (as in *va*) in a certain degree of relation; and, through this, it is allied to *b* (as in *ba*), in a manner determinable by the relation of *v* and *b*: and, again, *y* (as in *ya*) is to *g* (as in *ga*) as *w* is to *v*. Hence it arises that the sound which shall be to *g* as *v* is to *b*, shall be to *g* and *y* as *v* is to *b* and *w*. Expressed formally, the fact stands thus:

$$G_x : g \text{ and } y :: v : b \text{ and } w.$$

In a short tract, published in 1835, I stated that the sound in point must be on the "*y*-side of *g*," and that, although a ludicrous mode of speaking, expressed my meaning fully.

The conditions then *of* G_x are, that it must be in a certain relation to *g* and *y*; in which relation I am satisfied that *gh* is not.

It may not be thought superfluous to remark, that the two last assertions, that is, the *assumptions* of the question in hand, are made irrespective of any previous reasoning. I consider that they rest upon the evidence of our sense of hearing. Expressed formally, the facts and inference are as follows:

$$G_x : g \text{ and } y :: v : b \text{ and } w \text{. which } gh \text{ is not.}$$

* * * * * *

What *gh* is, is another question. Unless the number of mutes is to be reduced to four, sounds like *b*, *p*, *f*, *v*, &c. &c. must be considered not as *varieties* of a given typical sound, but as sounds *specifically distinct*: and, again, unless the number of mutes is to be indefinitely increased, sounds like the *t* and *d cerebral* of the Hindoos (I speak in the way of illustration) must be considered, not as sounds specifically distinct, but as *varieties* of a given typical sound.

To come, then, under the necessary conditions, the sound of G_x must be specifically different from that of *G*. Now *Gh* (as may be shown at another time) is no *specific* sound, but only a *variety*.

The force of the words *specific* and *variational* depends upon their definitions. To say that þ is a variety of *t* is true or false, according to the definition of the term *variety*; and the assertion is objectionable only on the score of inconvenience. To say, however, that þ is a variety of *s* (as is often said by those who are strangers to the sound) involves something more than an inconvenient definition. It is an error in respect to the fact.

Having (to my own satisfaction at least) inferred that *Gh* was not G_x, I was left to consider what really was G_x, and whether such a sound either actually existed or was capable of existing.

I drew the distinction between the actual and the possible existence of a sound for the following reason. It is very evident that although an articulation may be capable of being both formed by the larynx and distinguished by the ear, it may not exist in any spoken language; in other words, it does not follow that because a sound exists *in posse* it exists *in esse* also. If the French were the universal language, the sound of the English *th* would exist only in the capabilities of the ear and larynx; it would be a sound *in posse*, or a latent sound.

Now against G_x being a possible sound, I knew of no reason except the *accident* of its not being found in any of the well-known languages; whilst in favour of its being one were the analogies of the other mutes. This latter fact having convinced me that the sound in point was within the compass of the vocal organs, I conceived it possible that (two facts being ascertained) it might be formed *à priori*, that is, independent of imitation. These two facts were, first, the determination of the difference between an aspirate and its lene in respect to their sounds; secondly, the determination of the same in respect to the disposition of the parts of the mouth and larynx. In the midst of my investigations an accident intervened, that solved all the difficulties. I found, in Rask's Laplandic Grammar, the description of a rare and peculiar sound. It was not *Gh*; nor yet was it either G or Y. *It was something between the two.* This threw a light upon the question; so that all that now remained for me to do, was to hear the sound. In the town where I was residing there was a Laplander from Norwegian Finmark, whom the Norse government were instructing in reading and writing. Having sent for this person, and having set before him the words that Rask had given as examples, I found that his pronunciation of the sound in point coincided with the description of Rask, with my own previous notions as to its nature, and, consequently, with the conditions of G_x; in other words, the Lappish ʒ (so expressed in Rask's Grammar) was G_x.

Now, in the foregoing notices, nothing has been said concerning K and K_x. Nothing has been said concerning them, because it is so evident that they follow the analogies of G and G_x, that any mention of them would have been superfluous. The existence *in posse* of K_x I consider to be all but demonstrated; of its existence *in esse* I am less certain. It is probable that in some of the Greek dialects *k* is pronounced as K_x.

In certain dialects of the same language I imagine that $\gamma = G_x$. The letter *g* in the mouth of a native of Berlin, is pronounced (as I am told) intermediately to *G* and *Y*.* If this be true, it is in all probability G_x. It is probable that in certain dialects, or in certain stages of the Anglo-Saxon, ȝ has been sounded as G_x.

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K_x then being represented by $\varkappa$, and G_x by γ, we have, as an exhibition of the system formed by the sixteen mutes, the following table of double relations.

Lene.		Aspirate.	
Hard.	Soft.	Hard.	Soft.
p	*b*	*f*	*v*
t	*d*	þ	ð
k	*g*	$\varkappa$	γ
s	*z*	σ	ζ

I consider that these relations are immutable and essential, independent of the physiological arrangement, in their formation, of the parts of the mouth and larynx, and independent of their interchanges in the grammars of particular languages. I consider, moreover, that the number of specifically distinct mutes, independent of the fact of their being found in languages, is neither more nor less than sixteen, and that, in the present state of our knowledge, there is no more convenient mode of distinguishing mutes from other articulations, than by saying that every mute is one of four sounds, each of which forms a part of a system of four, and each of which is to some other hard or soft, and lene or aspirate.

XXV. *Reply to Mr.* Airy's *Remarks on Professor* Challis's *Investigation of the Motion of a small Sphere vibrating in a resisting Medium.* *By the Rev.* J. Challis, *Plumian Professor of Astronomy in the University of Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

ALLOW me, through the medium of your Journal, to express my thanks to Mr. Airy for calling my attention, by his letter in your Supplementary Number, to a step in my solution of the problem of the resistance to a sphere vibrating in an elastic medium, which I had left unexplained. I have certainly considered it possible that the velocity of the fluid at a given distance from the centre to or from which it is directed may, at a given instant, be different in different directions from the centre, provided there be no *abrupt* variation.

[* The letter *g*, at least when final, is precisely so pronounced by the Swedes.—Edit.]

For in the case of motion under consideration, if $v_{\prime}$ be the velocity impressed on the fluid by the vibrating sphere at the extremity of the radius drawn in the direction of its motion, the velocity impressed at the same instant at any point of the surface, the radius to which makes an angle θ with that direction, is *in fact* $v_{\prime} \cos \theta$, the sphere being supposed perfectly smooth; also the ratio of the two velocities, which is $\cos \theta$, does not vary with the time. It is, however, undoubtedly true, as Mr. Airy contends, that if the motion be of this kind, the lateral pressures, namely, those which take place at the surface of the sphere in directions perpendicular to its radii, should be taken into account; and the explanation which Mr. Airy desires will, I suppose, be given, if it be shown, as I am about to do, that these pressures *are* taken into account in my solution of the problem under discussion.

It will be admitted that if the same accelerative force be impressed at every instant on the sphere and on all points of the fluid surrounding it, in the same direction, no motion of the fluid along the surface of the sphere and no alteration of density will be thereby produced. Conceive to be impressed on the sphere at each instant its own accelerative force in a direction contrary to that in which it takes place, and the same accelerative force to be impressed on all points of the fluid in the same direction. The sphere will thus be reduced to rest, and the case of motion will become that of a variable stream impinging on a stationary sphere. The effective accelerative force at any point of the surface of the sphere will be the resultant of the above-mentioned impressed force, and of the force impressed at that point by the sphere in motion. This resultant is in the direction of a tangent to the surface, and if $\frac{dv}{dt}$ be the accelerative force of the sphere at the time t, is equal to $\frac{dv}{dt} \sin \theta$, the angle θ being that which has already been defined. Now by a known theorem of hydro-dynamics, if p be the pressure, ϱ the density, and f the effective accelerative force at any point s of a line drawn always in the direction of the motion of the particles through which it passes,

$$\frac{dp}{\varrho\, ds} + f = 0.$$

In the instance before us the line s is along the surface of the sphere, $ds = r\, d\theta$ (r being the radius of the sphere), and if $v = m \sin bt$, $f = mb \sin \theta \cos bt$. Hence by substituting and integrating,

$$p = p_{\prime} + mbr \cos \theta \cos bt.$$

The pressure $p_{\prime}$ corresponding to $\theta = \frac{\pi}{2}$ is that part which is common to every point of the surface of the sphere. Hence the pressure which tends to put the sphere in motion, or $p - p_{\prime}$, is

$$b\,m\,r \cos\theta \cos b\,t.$$

This, by the remark made at the commencement of the reasoning, is the same pressure as when the fluid is stationary and the sphere in motion, and is identical, if very small quantities be neglected, with the value obtained by a very different process at the bottom of p. 465 of my communication to the December Number. The consideration of the lateral pressure does not therefore lead to any variation from the former result.

The force of this explanation will, perhaps, be more clearly seen by the following remark. Supposing the sphere at rest and the fluid in motion, the velocity at the position corresponding to $\theta = 0$ is nothing. But as the lines drawn at any instant in the direction of the motion of the particles through which they pass nowhere intersect the surface of the sphere, it follows that the particles about that position are successively carried along the surface till at a position corresponding to $\theta = \frac{\pi}{2}$ they acquire the velocity of the *stream.* The lateral pressure is concerned in effecting this transfer of the individual particles, while the law of its variation at any instant along the surface of the sphere is that which is given by the preceding investigation. When the fluid is at rest and the sphere in motion the velocity impressed on any particle at the position $\theta = 0$ is reduced to nothing when it arrives at the position $\theta = \frac{\pi}{2}$ by the same pressure.

I am, Gentlemen, yours, &c.

Cambridge Observatory, Jan. 18, 1841. J. CHALLIS.

XXVI. *Correction in a paper published in the Philosophical Magazine for January. By* G. B. AIRY, *Esq., M.A., F.R.S., Astronomer Royal.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I AM indebted to the kindness of Mr. Tovey for pointing out an omission in my investigation published in your Number for January; arising, as will frequently happen, from the difficulty of attending to every small step when hastily writing out an investigation with whose results we are familiar.

I request you to insert in your February Number the following correction:

Philosophical Magazine, January 1841, page 9, line 6, *read* as follows:

" Here $a = 0$, and the approximate investigation for $r_{,}$ at the bottom of page 3, does not apply. Remarking, however, that $r_{,}$ is now $= 0$, the expression for $\frac{2\pi}{\lambda} \sqrt{h^2 + r_{,}^2}$ is reduced to $\frac{2\pi}{\lambda} h + 0$: the term corresponding to $\cos (e_{,} \cos \theta)$ is cos 0 or 1: and the term corresponding to $\frac{1}{2\pi} \int_{\theta} (\cos e_{,} \cdot \cos \theta)$ (from 0 to 2π) is $\frac{1}{2\pi} \times 2\pi = 1$. The expression for the intensity therefore becomes $1 + E^2_{,,}$."

No alteration is required after line 7, and the results of the investigation are in no way affected.

I am, Gentlemen, yours, &c.

Royal Observatory, Greenwich,
Jan. 20, 1841.

G. B. AIRY.

XXVII. *On the Electricity of expanding Air, as connected with the Electrical Phænomena of Effluent Steam.* By WM. GEO. ARMSTRONG, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN connexion with the experiments which I have recently published on the electricity of effluent steam, it occurred to me that it would be well to inquire whether similar effects to those I have described, could be produced by compressing common air in a receiver, and then suffering it to escape in a jet, in the same manner as steam had been discharged from a boiler in the experiments alluded to. With this view, therefore, I condensed about eight atmospheres into a strong vessel, the capacity of which was nearly six quarts; I then insulated the vessel, and discharged the air through a glass tube which I had previously inserted for the purpose.

On the first trial I obtained no indications of electricity whatever, but upon repeating the experiment on a subsequent day, the insulated vessel became so highly electrified, when the air was discharged, as to yield sparks fully a quarter of an inch long. I afterwards tried the same experimenta great number of times, and, strange to say, the electricity of the vessel, though generally *negative*, occasionally proved to be *positive*. Sometimes the electricity was very strong, and sometimes very weak, and frequently I could get no electricity at all. By means of an insulated conductor terminating in a number of points, I also obtained electricity from the ejected air, and found it to be *positive* every time I tried it.

I more frequently succeeded in producing an electrical development when the receiver was cold, and contained a little moisture, than when it was warm and dry, so that it is not improbable that evaporation may even here be the source of the electricity. I am by no means sure, however, that my better success when the receiver was cold and damp, was not mere chance, and I only mention the circumstance as a suggestion to persons who may think proper to repeat the experiment.

Newcastle-upon-Tyne, WM. GEO. ARMSTRONG.
Jan. 23, 1841.

XXVIII. *On Irish Tin Ore.* By AQUILA SMITH, *Esq., M.D., M.R.I.A.*[*]

THE question has been often asked, is tin ore found in Ireland? and I believe the only reply which could be given is, that it was said to have been fouud in the county of Wicklow, about the year 1796, when the gold mines were worked on account of the Government.

In the first report on the working of the gold mines, drawn up by Messrs. Mills, King, and Weaver, dated 1st August, 1801, and published in the second volume of the Transactions of the Dublin Society, the directors state, that " in every instance, where the gold has been found, there have been also found fragments of *magnetic iron ore*, and quartz containing *chlorite*, *iron ochre*, and *martial pyrites*, attended, more particularly at the works of Ballinvally (on the north-east declivity of Croaghan mountain), with *specula iron ore*, *brown* and *red ironstone*, *tin-stone crystals*, *wolfram*, and gray ore of *manganese*" (Part ii. p. 147.).

Here we find the existence of " tin-stone crystals," announced for the first time in Ireland; and, strange to say, its discovery does not appear to have attracted any attention in a commercial point of view, nor even to have been sought for as a curiosity by the mineralogists of that time; for although many minerals newly discovered in Ireland are briefly de-

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[*] Read before the Geological Society of Dublin, Wednesday, December 9th, 1840; and now communicated by the Author.

scribed in the catalogue of the minerals in Trinity College Museum, drawn up in 1807 by the late Rev. Walter Stephens, no specimen of Irish tin-stone is mentioned; and even at a later period, in 1818, when my respected friend, Dr. Whitley Stokes, published his catalogue, Irish tin-stone is not to be found in the list.

These circumstances would almost induce us to suspect that some other substance must have been mistaken for tin-stone by the directors of the works at the gold mine.

The next notice of this metal being found in Ireland is in the catalogue of Irish minerals in the museum of the Royal Dublin Society, published in 1832 by the late Sir Charles Giesecke. No. 213 is thus described: "tin-stone of a hair-brown colour, accompanies frequently, in small grains, the native gold found in streams at Croaghan mountain, county Wicklow."

Ever since I have turned my attention to mineralogy, I have anxiously sought for a specimen of Irish tin ore, in our public museums, as well as in several private collections of Irish minerals, but was unsuccessful in discovering any such specimen.

About six weeks ago I received, through my friend Mr. Robert Mallet, a small paper of washed sand from the gold works at present carrying on at Croaghan mountain, under the superintendence of Mr. Suter, who sent me the sand for the purpose of having it examined.

I found in it the following minerals:—

1. Several minute particles of native gold.
2. Magnetic iron ore consisting chiefly of very minute and brilliant octahedral crystals, readily separated by the magnet.
3. Fine-grained specula iron ore, giving a red streak when scratched with a knife, and becoming magnetic when heated.
4. Numerous minute garnets of a pale red colour, some of them exhibiting the planes of a rhombic dodecahedron, and fusing readily without intumescence into a brilliant black globule.
5. Many fragments of an earthy looking mineral, some of them presenting crystalline planes, abraded by friction; the fracture was shining, and the hardness about = 6·0 or that of felspar. Not being certain as to what this mineral was, I proceeded to examine it more particularly. In the forceps when heated it became yellow, and in a strong heat deposited a white sublimate on the points of the forceps, but it did not fuse. I then tried it on charcoal, but from the difficulty of keeping so small an assay steadily fixed under the flame, the

only result was that it became yellow when heated. I next tried it with borax, in which it dissolved very slowly, and did not colour the glass. I was still at a loss to say what it was, but suspecting that it contained some metal, which was indicated by the white sublimate, I tried it with carbonate of soda on charcoal, and it speedily yielded brilliant metallic globules, which tarnished rapidly after cooling; they were malleable, and when flattened out presented the appearance of tin.

No doubt now remained as to its nature; and I have only to add, in corroboration of the assertion of Messrs. Mills, King, and Weaver, that "native oxide of tin" exists in the county Wicklow.

XXIX. *Introduction to an Essay on the Amount and Distribution of the Multiplicity of the Roots of an Algebraic Equation.*

By J. J. SYLVESTER, *F.R.S. &c., Professor of Natural Philosophy in University College, London*[*].

I USE the word *multiplicity* to denote a number, and distinguish between the total and partial multiplicities of the roots of an algebraic equation.

There may be r different roots repeated respectively $h_1\ h_2 \dots h^r$ times.

r is the index of distribution.

$h_1\ h_2 \dots h_r$ are the partial mutiplicities, and if $h = h_1 + h_2 + \dots + h$,

h is the *total* multiplicity.

The total multiplicity it is clear may be defined as the difference between the index of the equation and the number of its roots distinguishable from one another.

In this Introduction, I propose merely to consider how existing methods may be applied to determine the amount and distribution of multiplicity in a given equation, and conversely, how equations of condition can be formed which shall imply a *given* distribution and amount.

Let the greatest common factor between $f x$ (the argument of the proposed equation) and $\frac{d f x}{d x}$ be called $f_1 x$.

And in like manner, let the greatest common factor of $f_1 x$ and $\frac{d f_1 x}{d x}$ be called $f_2 x$. and so on, till in the end we come to $f_r . x$, which has no common factor with $\frac{d f_r . x}{d x}$.

Let $k_1\ k_2 \dots k_r$ denote the degrees in x of $f x\ f_1 x \dots f_r . x$ respectively.

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[*] Communicated by the Author.

It is easy to see that

$k_1 - k_2$, partial multiplicities, are less than 2, *i. e.* are each units.

$k_2 - k_3$, partial multiplicities, will be less than 3, and therefore either 1 or 2 in value respectively, and so on till we come to

$k_{r-1} - k_r$ which will severally be between zero and $r-1$, and $k_r \quad - 0$ of values intermediate between zero and r.

Hence there will be

$k_1 - 2\,k_2 + k_3$ multiplicities each of the value 1.
$k_2 - 2\,k_3 + k_4$ ... ... 2.
... ...
$k_{r-1} - 2\,k_r$ ... of the value $r-1$.
and k_r ...... of the value r.

In place of $f x$ with $\frac{d_1 f x}{d x}$ we might employ $\frac{d f x}{d x}$ with $\frac{d^2 f x}{d x^2}$ and so on for the rest; the values of $k_2\,k_3 \ldots k_r$ will remain unaffected by this change; but the former method would be more expeditious in practice.

The total multiplicity is, of course, $= k_1$.

Suppose now that we propose to ourselves the converse problem to determine the conditions that an algebraic equation may have a given amount of multiplicity distributed in a given manner.

If $h_1\,h_2\,h_3 \ldots b_r$ be used to denote the given number of partial multiplicities which are respectively of the values 1 2 3 ... r, it is easy to see that the quantities derived above by $k_1\,k_2 \ldots k_r$ are respectively equal to

$$\begin{array}{l} h_1 + 2\,h_2 + \ldots\ldots + r\,h_r \\ h_2 + 2\,h_3 + \ldots\ldots + r\,h_{r-1} \\ h_3 + 2\,h_4 + \ldots\ldots + r\,h_{r-2} \\ \ldots \quad \ldots \quad \ldots \quad \ldots \quad \ldots \\ h_r\,. \end{array}$$

Now from $\frac{d f x}{d x}$ having a factor of the degree k_1 common with $f x$ we obtain k_1 conditions from $\frac{d f_1 x}{d x}$ having a factor of the degree k_2 common with $f_1\,x$ we obtain k_2 more, and so on. So that altogether we obtain in this way

$$k_1 + k_2 + \ldots\ldots\ k_r \text{ conditions.}$$

But it may easily be seen that the total multiplicity being k, the number of conditions *need* never to exceed k_1 in number, no matter what its distribution may be. Hence, besides the enormous labour of the process, and the extreme complexity of the results, we obtain by this method more equations by far than are necessary, and it requires some caution to know which to reject.

In my forthcoming paper (to appear in Phil. Mag. of next month) I shall show, by a most simple means, how without the use of derived or other subsidiary functions, to obtain the simplest equations of condition which correspond to a given distribution of a given amount of multiplicity.

The total multiplicity, say m, being given in as many ways as that number can be broken into parts, so many different systems of m equations can be formed differing each from the other in the dimensions of the terms.

These systems may be arranged in order so that each in the series shall imply all those that follow it, and be implied in all those that go before, without the converse being satisfied.

The subject of the unreciprocal implication of systems of equations is a very curious one, upon which the limits assigned to me prevent me from enlarging at present. It is closely connected with a part of the theory of elimination, which, as far as I am aware, has either been overlooked, or has not met with the attention which it deserves; I mean the theory of *Special Factors.*

An *example* may make what I mean by these clear.

Let C be a function (if my reader please) void of x, which equivalent to zero implies two given equations in x having a common root.

Let C be rid of all irrelevant factors, *i. e.* let C be the simplest form of the determinant, when the coefficients of the two equations are perfectly independent qualities. Now suppose, as is *quite possible in a variety of ways*, that such relations are instituted between the coefficients alluded to as make C split up into factors, so that $C = L \times M \times N = 0$.

Only one of the factors, L, M, N will satisfy the condition of the co-existence of the two given equations: the others are clearly, however, not to be confounded with factors of solution, or irrelevant factors, as they are termed, but are of quite a different nature, and enjoy remarkable properties, which point to an enlarged theory of elimination, and constitute what I call special or singular factors.

I shall feel much obliged to any of the readers of your widely circulated Journal, interested in the subject of this paper, who would do me the honour of communicating with me upon it, and especially if they would (between now and the next coming out of the Magazine) inform me whether any thing, and if so how much, different from what is here stated has been done in the matter of determining the relations between the coefficients of an equation corresponding to a given amount and distribution of multiplicity in its roots.

I ought to add, that my method enables me not merely to determine the conditions of multiplicity, but also to decompose the equations containing multiple roots into others free of multiplicity, *i. e.* to find, *à priori*, the values of the several quantities

$$\frac{fx \cdot f_2 \cdot x}{(f_1 \cdot x)^2}, \frac{f_1 \cdot x \cdot f_3 \cdot x}{(f_2 \cdot x)^2}, \ldots\ldots, \frac{(f_{r-1} \cdot x}{(f_r \cdot x)^2}, f_r \cdot x.$$

Moreover, other decompositions, not necessary to be enlarged upon in this place, may be obtained with equal facility.

University College, London, or 58 Doughty Street.

[To be continued.]

XXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xvii. p. 387.]

Nov. 19, 1840. A PAPER was read, entitled, Supplement to a paper "On the Theoretical Explanation of an apparent new Polarity in Light;" by George B. Airy, Esq., M.A., F.R.S., Astronomer Royal.

In a paper published in the second part of the Philosophical Transactions for 1840*, the author explained, on the undulatory theory of light, the phænomena observed by Sir David Brewster, and apparently indicating a new polarity in light. That explanation was founded on the assumption that the spectrum was viewed out of focus; an assumption which corresponded with the observation of the author and of other persons. But the author having, since the publication of that memoir, been assured by Sir David Brewster that the phænomenon was most certainly observed with great distinctness when the spectrum was viewed so accurately in focus that many of Frauenhofer's finer lines could be seen, he has continued the theoretical investigation for that case, which had been omitted in the former memoir, namely, when the spectrum is viewed in focus; and he has arrived at a result, which appears completely to reconcile the seemingly conflicting statements, and to dispel the obscurity in which the subject had hitherto been enveloped.

Nov. 26, 1840.—Description of the Electro-magnetic Clock. By C. Wheatstone, Esq., F.R.S.

The object of the apparatus forming the subject of this communication, is stated by the author to be that of enabling a single clock to indicate exactly the same time in as many different places, distant from each other, as may be required. Thus, in an astronomical

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* An abstract of which appeared in L. E. & D. Phil. Mag., vol. xvii. p. 381.—Edit.

observatory, every room may be furnished with an instrument, simple in its construction, and therefore little liable to derangement, and of trifling cost, which shall indicate the time, and beat dead seconds audibly, with the same precision as the standard astronomical clock with which it is connected; thus obviating the necessity of having several clocks, and diminishing the trouble of winding up and regulating them separately. In like manner, in public offices and large establishments, one good clock will serve the purpose of indicating the precise time in every part of the building where it may be required, and an accuracy ensured which it would be difficult to obtain by independent clocks, even putting the difference of cost out of consideration. Other cases in which the invention might be advantageously employed were also mentioned. In the electro-magnetic clock, which was exhibited in action in the Apartments of the Society, all the parts employed in a clock for maintaining and regulating the power are entirely dispensed with. It consists simply of a face with its second, minute and hour hands, and of a train of wheels which communicate motion from the arbor of the second's hand to that of the hour hand, in the same manner as in an ordinary clock train; a small electro-magnet is caused to act upon a peculiarly constructed wheel (scarcely capable of being described without a figure) placed on the second's arbor, in such manner that whenever the temporary magnetism is either produced or destroyed, the wheel, and consequently the second's hand, advances a sixtieth part of its revolution. It is obvious, then, that if an electric current can be alternately established and arrested, each resumption and cessation lasting for a second, the instrument now described, although unprovided with any internal maintaining or regulating power, would perform all the usual functions of a perfect clock. The manner in which hi s apparatus is applied to the clocks, so that the movements of the hands of both may be perfectly simultaneous, is the following. On the axis which carries the scape-wheel of the primary clock a small disc of brass is fixed, which is first divided on its circumference into sixty equal parts; each alternate division is then cut out and filled with a piece of wood, so that the circumference consists of thirty regular alternations of wood and metal. An extremely light brass spring, which is screwed to a block of ivory or hard wood, and which has no connexion with the metallic parts of the clock, rests by its free end on the circumference of the disc. A copper wire is fastened to the fixed end of the spring, and proceeds to one end of the wire of the electro-magnet; while another wire attached to the clock-frame is continued until it joins the other end of that of the same electro-magnet. A constant voltaic battery, consisting of a few elements of very small dimensions, is interposed in any part of the circuit. By this arrangement the circuit is periodically made and broken, in consequence of the spring resting for one second on a metal division, and the next second on a wooden division. The circuit may be extended to any length; and any number of electro-magnetic instruments may be thus brought into sympathetic action with the standard clock. It is only necessary to observe, that the force of the battery and the proportion between the resistances of the electro-magnetic coils and those of the other parts of the circuit, must, in order to produce the maximum effect with the least expenditure of power, be varied to suit each particular case.

In the concluding part of the paper the author points out several other and very different methods of effecting the same purpose; and in particular one in which Faraday's magneto-electric currents are employed, instead of the current produced by a voltaic battery: he also describes a modification of the sympathetic instrument, calculated to enable it to act at great distances with a weaker electric current than if it were constructed on the plan first described.

ROYAL ASTRONOMICAL SOCIETY.

February 14, 1840. *Annual Meeting.*

Extracts from the Report of the Council.

"Twenty years have now elapsed since this Society was instituted: during which period, the advances in Astronomy, both theoretical and practical, must be evident to the most ordinary observer. For, not only have public and private observatories been multiplied beyond any former example, but a great impulse has, generally, been given to every department of the science. There is now scarcely a single branch of astronomy that is not pursued in detail by one or other of the many active astronomers of the present day; whether requiring the laborious exertions of the observatory, or the equally arduous investigations of the closet. In the construction and manipulation of instruments also, great and important improvements have been made, which have introduced a system of accuracy and minuteness unprecedented in former times. If this Society has not, indeed, been the originator of this happy state of things, it has at least cordially assisted in the impulse: and let us hope that the next twenty years will be distinguished by similar exertions, and crowned with equal success.

"Amongst the losses by death, during the past year, the Council have to announce the decease of his Majesty the King of Denmark, one of the honorary members of the Society, and a great patron of the science of Astronomy. In the year 1832, his Majesty founded a gold medal, for the first discovery of a telescopic comet not previously known; and in November 1835, the conditions on which the medal was to be awarded were read at the meeting of this Society, and published in the Monthly Notices. It is somewhat remarkable, that from that period till within a few hours of his Majesty's decease, when the recent comet was observed by M. Galle at Berlin, no discovery of the kind had been made; although, some years previously, a considerable number of comets had made their appearance: and his Majesty's liberal intention had, consequently, been nearly frustrated. It will be gratifying to the Society to know that his Majesty's successor, the present King of Denmark, is also a patron of our science; and it is, therefore, with much pleasure, that the Council propose that he be elected an honorary member, in the place of his late predecessor*.

"The other losses that the Council have to deplore, are those of Mr. Davies Gilbert, Professor Rigaud, and Mr. Epps.

"Mr. Davies Gilbert was born on the 6th of March, 1767, in the parish of St. Erth, in the west of Cornwall. His father was the Rev. Edward Giddy, his mother Catherine Davies, a descendant of William Noye, attorney-general in the reign of Charles the First. The subject of this memoir was reared with great care and attention as a child of promise but not of robust health. His early education was conducted at home by his father, who was well qualified for the task as a scholar, and as a man of acknowledged ability and attainments; but his pupil's taste soon led him to prefer the study of the severer sciences to the elegancies of classical literature; and these studies were pursued with an ardour not to be repressed. As he grew up it was thought expedient to place him in the grammar-school of Penzance (of which the Rev. James Parkin was then the master); and for that purpose, his parents removed for about eighteen months to that town. In the year 1782 they removed to Bristol, where their son's studies were assisted, for a time, by Mr. Benjamin Donne. In the year 1786 he was matriculated at Oxford, after having been entered as a gentleman-commoner of Pembroke College. He had already made himself master of considerable mathematical and physical knowledge, and had acquired the greater portion of it by almost unassisted application. His efforts had been guided and aided, indeed, by a very friendly intercourse with the Rev. Malachy Hitchins, vicar of St. Hilary (whose connexion with the late Dr. Maskelyne and the 'Nautical Almanac' is well known), from whom, as he has himself recorded, he obtained, whenever it was asked, information 'in those sciences which afforded him uninterrupted entertainment and delight throughout the whole continuance of a protracted life.'

"During his residence at Oxford, he was a regular attendant at the lectures on anatomy and mineralogy of Dr. Thomson, at Christ Church. He likewise attended with assiduity the lectures on chemistry and botany of Drs. Beddoes and Sibthorp, and formed with them alliances of reciprocal friendship, which terminated only with their lives. His society was, in fact, courted by his seniors, and included a long list of the most eminent among the professors and other distinguished men of the University.

"He took the honorary degree of M.A., and continued to reside much in his college, until, in 1793, he returned to Cornwall to serve the office of sheriff, where his time was divided between the cultivation of science and literature, and the duties of a magistrate in a populous and busy county.

"Soon after this, we find him brought into closer contact with the world, and enabled to display, upon the wider stage of the me-

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* A resolution to this effect was proposed at the meeting, and unanimously carried.

tropolis, those powers of mind which had been hitherto confined to the narrower sphere of his native county, and a few private admirers and friends. He represented Helston in parliament in the year 1804; and at the general election in 1806, he was chosen to represent Bodmin, and continued to sit for that borough, till, in December 1832, he ceased to be a member of the legislative body.

" On the 8th of April, 1808, he married Mary Ann Gilbert, only niece of Charles Gilbert of Eastbourn, in Sussex, under whose will he came into possession of considerable estates in that county; and, in compliance with its injunctions, assumed the name and arms of Gilbert.

" Mr. Gilbert's exertions to promote the objects of science in general, and to forward the views of the different scientific Societies of London and other parts of the United Kingdom, with which he was connected, both in the House of Commons and with the leading men of the state, are well known, and will be long held in remembrance. He had been early admitted into the Royal Society, of which he never ceased to be an active and distinguished member; and, in 1827, was elected, without opposition, their President, on the resignation of Sir Humphry Davy, whose friend and patron he had been in the early life of that illustrious chemist and philosopher. He resigned that chair in November 1830, and was succeeded in it by H.R.H. the Duke of Sussex. He filled the office of President of the Royal Geological Society of Cornwall, from its origin till his death.

" Mr. Gilbert, as a member of the legislature, and a distinguished cultivator of science, appeared to government a proper assistant in all public inquiries for which station in society, and a competent knowledge of mathematics, formed necessary or useful qualifications; and he was accordingly appointed a member of the Board of Longitude, a Commissioner of Weights and Measures, and a Commissioner to settle the Boundaries of Boroughs under the first Reform Bill. He was also Chairman of the Committee appointed to consider the subject of the Measurement of the Tonnage of Shipping; whose plan (proposed by one of our Fellows, Mr. Riddle) was passed into a law. He was, indeed, at all times willing, when called upon, to devote his time and acquirements either to the public service of his country, or to the private aid of those who appealed to him for assistance or advice.

" After his retirement from Parliament and the Chair of the Royal Society, he did not resign himself entirely to repose; but continued, notwithstanding his increasing infirmities of body, actively to co-operate with his fellow-labourers in the great field of science. He accepted a commission of investigation into the Stannary Laws of Cornwall, and frequented the Societies of which he was a member with undiminished alacrity and zeal, always prepared to obey the calls of duty or friendship with readiness of purpose and gentleness of manners.

" Mr. Davies Gilbert was not a very large contributor to the Philosophical Transactions. In 1826, he gave a paper on Suspension Bridges, accompanied by tables, which is the most complete account yet published of the points on which it treats. In 1827, he read a paper on the expediency of assigning specific names to all such functions of simple elements as represent definite physical properties; and in 1830, a description of the progressive improvements made in Cornish steam-engines. Very different from these, in the subject treated, is a paper on the nature of negative and imaginary quantities, read in 1830. His last contribution, a continuation of the tables relative to suspension bridges, was made on May 19, 1831.

"He had for some years exhibited symptoms of decay. What were the causes of the atrophy so visible in his form and countenance is unknown; but in 1839, he became weaker in strength and spirits; and though he made a journey to Durham, and afterwards into Cornwall, where he presided for the last time at the Anniversary of the Royal Geological Society of Penzance, and attended that of the Polytechnic Society of Falmouth, he was evidently unequal to the task he had imposed upon himself, though his powers of mind were clear, quick, and apparently unimpaired. His last visit was to Oxford, which had, some years before, conferred on him one of the highest titles in her power to bestow. From that period he never went into public, but took his last journey from London to his house at Eastbourn, on the 7th November. He expired in the presence of his family, on the 24th of December, 1839; and his memory will be long cherished as that of an ardent lover of science, a liberal patron of merit, a kind friend, a mild and accomplished gentleman.

"Mr. Rigaud* was descended from a French family of consideration, who, on the revocation of the Edict of Nantes, resigned their property, and fled to a foreign land for conscience sake. His maternal grandfather and father had each successively filled the office of Observer to the King, at Kew; an office which was afterwards graciously conferred on Mr. Rigaud himself. Mr. Rigaud was born in the year 1774; and in 1791, when little more than sixteen years of age, was matriculated at Exeter College, Oxford. He was elected a Fellow of that Society before he was of sufficient standing to take the degree of B.A.; and, as soon as his age permitted, was appointed one of the College tutors. On the death of Dr. Hornsby, in 1810, Mr. Rigaud was appointed Savilian Professor of Geometry, and Reader on Experimental Philosophy; the duties of which latter office he had for some time before been discharging for Dr. Hornsby. In 1827, Mr. Rigaud succeeded Dr. Robertson in the office of Savilian Professor of Astronomy and Radcliffe Observer, thereby relinquishing the Professorship of Geometry. As an astronomer, Mr. Rigaud's attention was principally directed to the literary and historical department of the science, for which his extensive learning, and accuracy of research, especially qualified him. In 1831, he published in a 4to volume, "The Miscellaneous Works and Correspondence of the Rev. James Bradley," to which he pre-

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[* See L. and E. Phil. Mag., vol. xv. p. 524.—Edit.]

fixed copious memoirs of Bradley himself. This work contains, besides several other unedited papers of Bradley, his original observations for the determination of the constants of aberration and nutation. These observations had been lost sight of for upwards of seventy years; and, but for Mr. Rigaud's zeal and exertion, would, in all probability, never have been recovered. Fortunately, however, it came to his knowledge that several MSS. of Bradley still existed among the papers of Dr. Hornsby, then in the possession of his family, by whom they were readily given up on application from the University. Mr. Rigaud was requested to undertake the office of editor; a task which was not an easy one, owing to the confused state of the materials from which he had to derive his information. He succeeded, however, in producing a work which will ever be regarded as a most valuable record in the history of our science. So highly was it esteemed on the Continent, that in the very next year after its publication, the Royal Academy of Sciences of Copenhagen made the reduction of the observations for aberration and nutation the subject of their prize; which was adjudged to Dr. Busch of the Kœnigsberg Observatory, the assistant of that illustrious astronomer to whom the reputation of Bradley is so deeply indebted. As connected with this subject, it may be mentioned, that it was through the instrumentality of Mr. Rigaud, that his late Majesty, King William IV., was pleased to cause a monument to be erected at Kew, to mark the spot where Bradley made the observations which led to his great discoveries. In 1835, Mr. Rigaud published a small pamphlet, containing an account of the 'Astronomiæ Cometicæ Synopsis' of Halley, and, in 1838, the last work he lived to complete, 'An Historical Essay on the first publication of Sir Isaac Newton's Principia;' which exhibits, in every page, the author's minute acquaintance with the events of that important period. Besides these works, Mr. Rigaud was the author of many papers read before the Ashmolean Society in Oxford; and also of one 'On the Principal Instruments at Greenwich in the time of Dr. Halley,' which is inserted in the ninth volume of our Memoirs.

"In 1827, Mr. Rigaud met with a severe domestic affliction in the loss of his wife; an event which left him sole guardian of a large family of young children; to the superintendence of whose education much of the attention of the latter years of his life was devoted. The affection and solicitude with which he discharged this duty was rewarded by his being spared to witness the academic distinction of his eldest son, who is now a Fellow of our Society; and the Council are happy in being able to state that he is completing the publication of a collection of letters from scientific men in the beginning of the last century, upon which his father was engaged at the time of his death; and the original documents of which formerly belonged to Mr. Jones, the father of Sir William Jones, but now in the possession of the Earl of Macclesfield.

"Many members of this Society have had opportunities of observing the kindness and unaffected simplicity of manner which marked Mr. Rigaud's intercourse in private life; and some of them, the more sterling qualities of his character. For many years past, however, he had entered but little into society. His almost constant residence, for nearly half a century, was Oxford; and there he has left a large circle of friends, who had abundant opportunities of knowing his virtues, and who will long regret his removal. In him the University has lost a most devoted son; and it is now a consolation to remember that he was ever foremost among those whom she delighted to honour.

"Mr. James Epps was appointed Assistant-Secretary of this Society in 1830, and during the eight years he officiated in that capacity, he not only merited the approbation of the Council by the ability and zeal with which he discharged the duties of his office, but also rendered himself acceptable to the Fellows at large by his uniform urbanity, his cheerful disposition, and his readiness to oblige on all occasions. Although he had not the advantages of a regular education, and the occupations of his early life left but little leisure for the cultivation of the sciences, he had acquired, nevertheless, very considerable knowledge both of theoretical and practical astronomy; and he had also much skill and experience in astronomical computation. He was the author of several papers printed in our Memoirs; namely, one in the fourth volume, accompanied by some useful tables for computing the azimuthal deviations of a transit instrument from the observed passages of two stars through the vertical it describes; another, in the same volume, on the errors of the same instrument occasioned by the inclination of the axis to the horizon; one, in the sixth volume, on the method of ascertaining the comparative rates of chronometers; and one, in the ninth volume, on the investigation of formulæ for reducing observations made with the annular micrometer. He likewise recently contributed another paper on the errors that may be produced in determining differences of longitude by observations of moon-culminating stars, when there are no corresponding observations; accompanied by a table of results deduced from comparisons of such observations, with the places given in the 'Nautical Almanac,' which has been ordered by the Council to be printed in the forthcoming volume.

"In 1838, Mr. Epps resigned his office in the Society, and removed to Hartwell to superintend the private observatory of our excellent Treasurer, Dr. Lee. For this appointment he was eminently well qualified. He entered on its duties with his usual ardour; thus meriting the friendship and esteem of his patron, which he continued to enjoy without interruption to the hour of his death. On his removal to Hartwell, he was elected a Fellow of this Society.

"Mr. Epps was a man of varied accomplishments and extensive general information; and knew well how to turn theoretical knowledge to practical account. He was born in 1773, of humble but respectable parents residing in Kent; and died at Hartwell on the 10th of August last, regretted by all who knew him.

"In the Report of the Council to the Society in the year 1838, it was stated that the pendulum observations made by the late lamented Lieut. Murphy in Asia, had been received, and placed in the hands of Mr. Baily, for examination and reduction. Since that period, the pendulums themselves have arrived, and Mr. Baily has repeated his experiments on them, for the purpose of comparing the results before and after the voyage, with those made by Lieut. Murphy. A Report on the whole of these experiments, and on their general result, will be made by Mr. Baily, and read to the Society at one of the evening meetings. These pendulums are now entrusted to Capt. James Clark Ross, as already mentioned, for the purpose of making further experiments at such places as he may find it convenient, during his present scientific voyage: and they are thus, for the third time, placed in active operation.

" As connected with this subject, it may be mentioned that when Mr. Maclear departed for the Cape of Good Hope, to take the superintendence of the Observatory there, he took with him one of Kater's invariable pendulums, that had been previously swung in this country by Mr. Baily. That pendulum has recently been returned to this country, together with a detail of Mr. Maclear's experiments. The whole have been placed in the hands of Mr. Baily, who will, in this case also, report upon the general result.

" In alluding to the labours of Mr. Maclear at the Cape of Good Hope, the Council may mention also his intention of remeasuring the arc of meridian formerly measured by Lacaille. Already have the stations of Lacaille been satisfactorily identified, and the latitudes of the extreme stations been observed, as we have seen by the paper recently read to this Society: and the requisite apparatus has been sent out for finishing what has been so auspiciously begun. Under the direction of this able and zealous astronomer, there is every reason to expect a satisfactory result to so important an operation.

" The Council have great pleasure in stating that the eleventh volume of the Memoirs of the Society is now in the press, and that considerable progress has been made in the printing. Amongst the papers that will appear therein, is a very valuable catalogue of all the stars that were observed by Mr. Airy during the time that he had the superintendence of the Observatory at Cambridge. Partial lists of those stars had from time to time been printed in the several volumes which annually proceeded from that Observatory; subject however to a slight correction for reducing them to one and the same equinox. In the catalogue about to appear in our Memoirs, they are all uniformly reduced to the epoch 1830, with the annual precessions annexed; and are thus immediately available for occasional reference and application. This catalogue will be found to fill up many *lacunæ,* and tend to rectify many errors in former catalogues, arising either from imperfect observation, or from mistakes in transcribing or computing authentic records. It has been found of great assistance in perfecting and enlarging the catalogue which goes under the name of the Catalogue of this Society; as it contains several stars that had not been observed since the original observations by Hevelius, Flamsteed, and Bradley. With a view to extend its utility still further, the Council have directed that an additional number of copies should be printed, and presented to the Rev. Mr. Challis, the director of the Cambridge Observatory, with a request that they might be distributed with the forthcoming volume of the 'Cambridge Observations:' a request with which he has readily complied.

"It is also with much pleasure that the Council can state that the extension of the Society's 'Catalogue of Stars,' just alluded to, and which has been undertaken at the suggestion and at the expense of the British Association, is in great progress, and will probably be completed before the next anniversary. It is intended that this enlarged catalogue shall contain not only every known star in the catalogues of Hevelius, Flamsteed, Bradley, Mayer, Lacaille, and Zach, but also every star, in any of the more modern catalogues, of the sixth magnitude, in whatever part of the heavens it may be situated, and every star in such catalogues not less than the seventh magnitude, within 10° of the ecliptic; together with every other star that, from its peculiar position, suspected proper motion, or other extraordinary circumstance, may be deserving of being thus recorded, and pointed out for further observation.

"In the execution of this work, considerable assistance has been afforded by the four several catalogues published by Mr. Thomas Glanville Taylor, at Madras. The second of those catalogues contains nearly all the stars in the Society's catalogue, visible in that latitude; and the last two exhaust nearly the whole of Piazzi's celebrated catalogue. The total number of stars contained in these four volumes is upwards of 8800; most of which have been observed more than once, and many of them more than five times. The whole have been of essential advantage in completing and perfecting the extension of the Society's catalogue above mentioned: since it has enabled the computers not only to verify the positions of nearly all the stars, but also, in most cases, to deduce the proper motion (if any) that belongs to each of them respectively. The establishment of this observatory is highly honourable to the East India Company, and the fruits which it has produced reflect great credit on the zeal and assiduity of Mr. Taylor, the active superintendent.

"Another subject also undertaken by the British Association, is the reduction of the stars in the '*Histoire Céleste*' (a work containing about 50,000 observations), together with the annual precession annexed to each star. About one-half of this work is already executed; and, when completed, it will afford a ready and convenient reference to almost all the stars (not circumpolar) that are visible in this latitude with an ordinary telescope. The positions of the stars are reduced to the epoch 1800, by means of the very convenient tables of M. Schumacher; a work which renders it scarcely necessary that the computations should be done in duplicate; for, as every star will be referred to its original authority in the printed work, the astronomer will have an easy and ready mode of verifying any suspected result, and of rectifying any error that may be discovered.

" To the British Association also, astronomers are indebted for another work of a similar kind: namely, the reduction of *all* the observations of stars made by Lacaille at the Cape of Good Hope. It is well known that only 1942 of those stars were reduced by Lacaille himself, and formed into a catalogue, which is printed at the end of his '*Cœlum Australe Stelliferum*:' but the great mass of his observations, consisting of upwards of 10,000, have never yet been reduced, although they are of equal authority with those in the published catalogue. The execution of this work is proceeding under the direction of Professor Henderson of Edinburgh, who has been kind enough to supply the elements for the reduction, and to superintend the process of the computations: and there is every reason to believe that Lacaille's new and enlarged catalogue will, as far as the stars in the old catalogue are concerned, be more entitled to confidence (since it is founded on more accurate elements) than the original catalogue published by Lacaille himself. It will, moreover, contain a great number of other stars of equal authority, unknown to astronomers till the appearance of the recent catalogue of Sir Thomas Brisbane.

" Connected with this subject is another work of great interest, likewise proposed by the British Association; namely, a revision of the nomenclature of the stars, and of their division into constellations. It is well known that much confusion at present exists in the notation that has gradually crept into practice: a notation now without order, system, regularity, or uniformity. Hevelius was the first to break in upon the arrangement of the constellations as propounded by Ptolemy; and Flamsteed (or his editors), although he did not disturb the order or number of the constellations in the catalogue of Hevelius, yet introduced some confusion by inserting stars in one constellation that had previously been considered as belonging to another: Bayer's mode of indicating the relative magnitudes of the stars in each constellation was in a great measure lost sight of; and thus the way was led for that mass of confusion which is contained in Bode's large catalogue of 17,240 stars. In the southern hemisphere, we find Halley and Lacaille introducing totally new constellations, frequently overlapping each other, and the stars themselves indicated by such a profusion of letters (many of them precisely similar and several times repeated in the same constellation), that it is often difficult and not always possible to identify them. With the view of applying a remedy to this species of scientific annoyance, the British Association has appointed a Committee, and placed funds at their disposal, for a new arrangement and classification of the stars; preserving as much as possible the old constellations, and Flamsteed's system of numerical order; but correcting gross errors in such arrangement, and confining the adopted constellations to known and definite limits. The present time appears peculiarly favourable for such an undertaking, when so many catalogues are about to be formed into one uniform system. Those only, who have had much experience in such matters, can fairly estimate the convenience and advantage to be gained by such a reform. It is to be hoped that one of the leading members of the Committee will favour this Society with his views and proposals on this subject, in order that it may have the opportunity and benefit of a free discussion, prior to the adoption of so important an alteration.

" From amongst the several distinguished names that were proposed for the Medal this year, the Council have selected that of M. Plana, for his elaborate treatise entitled '*Théorie du Mouvement de la Lune*:' and they trust that this award will meet the approbation of the meeting. The medal will be delivered in the usual manner at the close of the meeting; and the President will, in his address, explain the grounds on which the Council have formed their decision."

The President (Sir J. F. W. Herschel, Bart.) afterwards addressed the Meeting on the subject of the award of the Medal, as follows:—

Gentlemen,—The Report of the Council to which we have just listened (with the painful exception of the losses the Society has sustained in the persons of those respected and lamented members who have been so ably commemorated in that Report) is one calculated to afford most lively satisfaction; and it is so full and complete on every point as to have left me nothing to say, except on that one subject on which, by ancient usage, it has been considered right for the Chairman of this meeting to add a few words of explanation—I mean the award of the Medal for the year.

The award of our medal for this year, gentlemen, to Signor Plana is an act, as it may at first sight appear, of somewhat tardy justice. Those great works on the lunar theory (for which that award is made), and on the perturbations of the planets, especially of Jupiter and Saturn, have now been so long before the public, that it may almost appear as if, in the dearth of matter of sufficient interest of later date, your Council had been ransacking the annals of modern astronomy to find something on which they might rely in a kind of inglorious safety for a justification of their award.

This would be a very erroneous view, indeed, to take of this subject. So far from experiencing a lack of matter to choose from —so far from a deficiency of interest in the subjects which have shared the consideration of the Council in coming to the conclusion they have done—there have been, in fact, on probably no occasion, such powerful countervailing claims—and so far from seeking, in this award, a merely safe and justifiable course—it has required no common share of boldness and decision in your judges to put aside those claims, in favour of M. Plana's—of that boldness I mean which is based on justice and a long-sighted view of public utility.

Before I proceed, therefore, to state the reasons which have weighed with the Council to take the step they have done, it will be right for me to mention, at least in general terms, two of the subjects which have chiefly divided their attention on the occasion; and this I am fortunately enabled to do, infinitely better than I could pretend to do it on my own knowledge and reading, by the aid of most excellent reports on those subjects laid before the Council by Professor Airy and Mr. Main—the one on the subject of

Professor Hansen's general researches in physical astronomy, the other on Professor Bessel's and Mr. Henderson's observations on the parallax of those remarkable double stars, 61 *Cygni* and *α Centauri* —observations which it would appear, beyond question, have brought us to the very threshold of that long-sought portal which is to open to us a measurable pathway into regions where the wings of fancy have hitherto been overborne by the weight or baffled by the vagueness of the illimitable and the infinite.

M. Hansen's researches on the lunar and planetary theories are every way most remarkable, and seem likely to lead to results of the utmost generality and importance. He has attacked the great problem of three bodies (extended, in the conception and application of his methods, to the mutual perturbations of *four*) by a method entirely novel in its idea, although based on and starting from Lagrange's idea of the variation of the elements. Of this method, it would not be easy, in words unaided by symbolic expression, to give any distinct account; but its principle may be stated in general terms, as assuming not the elliptic *elements,* but the elliptic *time,* to be subject to perturbation; or, in other words, as considering the perturbed co-ordinates, each to arise from the combination of invariable *elements* with a varied or perturbed *time,* the amount of whose variation shall exactly account for all that the variation of the elements accounts for in Lagrange's method. The mere mention of this refined and abstruse mode of conceiving the problem must suffice to show, that, to carry it into effect, must require at every step a contention of mind, a degree of intellectual effort, far surpassing what is required for the mere management of algebraic symbols and developments, however intricate.

Whatever be the skill and dexterity, however, exhibited by the author of this truly original conception, and whatever promise it must be considered as holding out for the future advancement of our knowledge in this intricate research, it can hardly yet be regarded as having attained that extent of development which it will require to supersede in the construction of tables, and the actual calculation of the lunar and planetary perturbations, the methods already in use, which the researches of Clairaut, Laplace, Lagrange, Poisson, Damoiseau, and Plana, have wrought up to such a pitch of practical perfection. Hansen's theory appears to afford what, in the actual state of our knowledge, must be regarded as most precious—a new handle by which to seize this refractory problem—one of universal applicability and gigantic power and *purchase,* but of which the management is not yet fully reduced to practice, and of which even the author himself can scarcely yet be said to have acquired the entire mastery. In the theory of Jupiter and Saturn, indeed, the final numerical results are obtained, and tables calculated; but in the lunar theory, which (in the words of Mr. Airy) "must be considered as the ground of his chief analytical triumph, there exist at present only what may be termed the *foundations* for such a theory." "No man living" (I continue to use the words of the eminent geometer last-mentioned), "No man living, probably, except M. Hansen himself, could work it into a complete lunar theory; and the exhibition of numerical results is here, therefore, still distant." Let us hope that he will not long leave them so.

On the other subject to which I alluded—the parallax of the fixed stars—it would be doing an injustice to the valuable report of Mr. Main, which, as a beautiful specimen of astronomical history, I hope to see ere long adorning our Transactions, if I were to avail myself more largely of it on this occasion than is absolutely necessary. It has long been understood by astronomers, that the research of parallax ought not to be confined to the largest stars, but that, in order to determine our choice of stars for this research, other *primâ facie* grounds for suspecting a proximity to our system ought to be taken into consideration; such as great proper motion, or, in the case of a double star, great apparent dimensions of the orbits described about each other. In the case of the double star 61 *Cygni*, both these indications combine to point it out as deserving inquiry. In that of *α Centauri* they also conspire; for it is well known that this fine double star has a considerable proper motion; and my own observations prove, that the mutual orbit described by its individuals about each other, is of unusually large angular dimension. The great brilliancy of the star also, and its situation in a region of the heavens in which the stars, generally speaking, seem to be less remote than in others, all favour the expectation of a measurable parallax being detected in it: and such Mr. Henderson, from his own observations, assigns to it. I am not about to criticise this result; on the contrary, I am disposed to attribute much weight to his conclusion; but it is only on a very long series of observations of *absolute places*, affected as they are by instrumental error and uncertainty of refraction, that any conclusion of this kind can rest with security.

Bessel has attacked the question in a different way, by measuring at all times of the year the angular distance of the stars composing the double star 61 *Cygni* from two small stars visible in the same field of view, and within limits adapted to secure micrometrical measurement. The method is unexceptionable, the measurements conducted with consummate skill, and their reduction executed with all possible regard to every thing likely to influence the result. And that result is, to assign a minute, it is true, but perfectly unequivocal amount of parallax, in a way so striking as hardly to allow a doubt of its reality. Such is the impression on merely reading the numerical statement; but, put in the light in which Mr. Main has placed it, by the graphical projection of the measures, the conclusion seems quite irresistible:

> "Segnius irritant animos demissa per aures,
> Quam quæ sunt oculis subjecta fidelibus, atque
> Ipse sibi tradit spectator."

It may now be reasonably asked, If all this be so, why have your Council hesitated to mark this grand discovery with that distinct stamp of their conviction and applause, which the award of their annual medal would confer? A problem of this difficulty and importance solved, so long the cynosure of every astronomer's wishes —the ultimate test of every observer's accuracy—the great land-mark and *ne plus ultra* of our progress, thus at once rooted up and cast aside, as it were, by a *tour de force,* ought surely to have commanded all suffrages. It is understood, however, that we have not yet all M. Bessel's observations before us. There is a second series, equally unequivocal (as we are given to understand) in the tenour, and leading to almost exactly the same numerical value of the parallax, and not yet communicated to the public. Under these circumstances, it became the duty of your Council to suspend their decision. But, should the evidence finally placed before them at a future opportunity justify their coming to such a conclusion, it must not be doubted that they will seize with gladness the occasion to crown, with such laurels as they have it in their power to extend, the greatest triumph of modern practical astronomy.

M. Plana is well known to the astronomical world as the director of the Observatory at Turin, from which have emanated some valuable series of observations. In conjunction with M. Carlini, he also carried on that extensive and important triangulation of the Savoy Alps, which have made his name celebrated as a geodesist. His works, too, on many other subjects, both astronomical and purely analytical, are of great importance; particularly his investigations on the subject of refraction prefixed to the 'Turin Observations,' from 1822 to 1825, published in 1828; those on the motion of a pendulum in a resisting medium, &c. But it is of his researches on the lunar theory for which our medal has been actually awarded; and of these it behoves me now to speak; and I cannot do so in more clear, concise, and discriminating terms, than those used by Mr. Airy in his Report already alluded to:—

"The method pursued by Plana, in his '*Théorie de la Lune,*' is slightly, but not importantly, different (I mean in the fundamental equations) from those of his predecessors, Clairaut, Laplace, and Damoiseau. He first starts with the method of variation of elements, and pursues it to such an extent as to ascertain generally the form of the expressions connecting the longitude, the latitude, and the time. He then reverts to Clairaut's equations; and, as these equations require for the successive substitutions an approximate expression for time in terms of longitude, he adopts a peculiar form (suggested by the variation of elements) for the principal part of it, and attaches to that principal part a subordinate part marked with the prefix δ. The same thing is done for the latitude. The process then is tolerably direct, and is almost similar to that of antecedent writers. In the fundamental algebra, therefore, there is no very great originality in the plan; but the mode followed in the detail of the work is beyond all praise. In the whole of the analytical combinations of this immense work, every part arising from the combination of any one term (however small) with any other term, is given separately, in such a form as to leave no difficulty in the detection of error to any careful examiner. The terms of peculiar difficulty (as, for instance, that depending on twice the distance between the node and the perigee) are made the subject of special discussion; and, in some instances, the origin of discordance between the author's results and those of Laplace is investigated with the same clearness which prevails through the other operations.

"In one respect, the plan of investigation differs much from those of his predecessors, as well as from Hansen's. The investigation is wholly symbolical: no numerical value is introduced, and no consideration of relation of values entertained, till the final substitutions are made. As an example of theory, there can be no doubt of the beauty of this process. As a subject for practical accuracy, it may not be so certain whether it is advisable. The convergence of the series is sometimes extremely slow. As far as I can observe, the accuracy of this method is exactly and properly that of successive substitution: but, in various parts of the lunar theory (in all places where the terms rise two orders by integration), the method of successive substitution is not sufficient; in fact, it is necessary to assume a term in order to find its correct value. Adopting this method, however, the author has pushed it as far as, probably, it will ever be carried. The whole is worked to the fifth order, and some parts to the seventh order.

"Finally, the author has determined from observations the principal constants which require to be substituted in the symbolical expressions, and has substituted them, and has thus produced a set of numerical expressions which may immediately be used for the formation of lunar tables.

"In terminating the remarks on the works of these two authors, Plana and Hansen, I must again express my very great admiration for both. But their merits are of very different kinds. The theory of Hansen is undoubtedly of the higher order, but it can hardly yet be said to be practical (at least in the lunar theory): many years will yet elapse before it will influence the lunar tables. The theory of Plana is very good, and probably adequate in all respects: it is eminently practical in form: it has already influenced the investigations of other writers, and will probably soon influence the tables."

There is but one thing more to add to this clear and powerful summary, and I will supply it by a quotation from the work itself:—

"Je n'ai pu me faire aider par personne; j'ai du traverser *seul* cette longue chaîne des calculs, et il n'est pas étonnant si par inadvertence j'ai omis quelques termes qu'il fallait introduire pour me conformer à la rigueur de mes propres principes." When we look at the work itself there seems something almost awful in this announcement.

A very important memoir of M. Plana, on the theory of the planetary perturbations, has adorned the Transactions of this Society. The points of which it treats are miscellaneous, and some of them, perhaps, not of the highest importance, except in one point of view, and that, perhaps, the most important of all. Every one who is at all conversant with these researches must be impressed with the enormous interval which separates—I will not say the mere differential equations of the planetary motions—but their integrals after much and intricate development—from the final numerical results on which their tables are to be constructed; that is to say, the computed values of the coefficients of terms having the same argument, when assembled from all the points whence they arise in the algebraic processes and amalgamated together. M. Plana appears to have proposed to himself the gigantic task of revising and correcting not only those algebraic developments, but the actual numerical calculations of the whole '*Mécanique Céleste*;' and this paper contains many examples sufficiently proving the necessity of such revision, and leading the way to those further and more elevated researches on the theory of Jupiter and Saturn, to which the latter part of this memoir must be considered as having given occasion; and which are further developed in several other memoirs published in various academical and other collections.

Neither the time nor the nature of this occasion would allow of my entering into any history of the controversy to which the revision thus set on foot, and the discordant results arrived at in this memoir, gave occasion. Suffice it to say, that errors—venial, no doubt, and such as it would be miraculous did they not exist—were discovered on all sides, and the absolute necessity established not merely of a thorough revision of every part of these immense computations, but of printing and publishing the steps in that regular and methodical form, which alone can put it in the power of subsequent calculators to lay their finger on the precise point where error shall have crept in; and to resume the calculations from that point without sacrificing the whole of what precedes.

It is this methodical clearness—this letting in of the light on every dark corner of every intricate combination and heart-breaking numerical calculation, which may be regarded as marking from this time a new era almost in the planetary theory itself. In the '*Mécanique Céleste*,' we admire the elegance displayed in the alternate interlinking and development of the formulæ, and exult in the power of the analytical methods used; but when we come to the statement of numerical results, we quail before the vast task of filling-in those distant steps, and while cloud rolls on after cloud in majesty and darkness, we feel our dependence on the conclusions attained rather to partake of superstitious trust, or of amicable confidence, than of clear and demonstrative conviction. Let me not be misunderstood as by these expressions casting any reflection on the conduct of that immortal work. The surest proof of its titles to such immortality which can be given, is that microscopic examination subsequently lavished on every point embraced in its immense outline. It is no disparagement to the agriculturist, whose energies have extirpated the wilderness, and established in its place cultivation and wealth, that a period shall arrive when his furrows shall, in their turn, be replaced by the garden, and his system of culture, by a measured and calculated succession. Neither would I be understood to lay the sole stress of our applause of M. Plana's researches on the luminousness of their statement. His analysis is always graceful, his combinations well considered, and his conceptions of the ultimate results to be expected from them perfectly just, and justified by the results when obtained.

It cannot but be agreeable to this meeting to know that our award is duly appreciated by M. Plana himself, and regarded by him in the light which it is ever most desirable it should be,—as a stimulus to fresh researches and further exertions of his powerful talents in the same line where they have already reaped so rich a harvest. No sooner had the Council decided on their award, than, as in private regard no less than in public duty bound, I communicated to him the result; and his reply, which breathes the warmest spirit of attachment to the Astronomical Society, and of undiminished zeal in his own peculiar line of research, is now before me. In the absence of any personal friend to receive it for him, I shall now, therefore, present our medal to Mr. Rothman (in the absence of our Foreign Secretary, Captain Smyth), in his name, and request him to forward it to him, with our best wishes for his health and happiness.

XXXI. *Intelligence and Miscellaneous Articles.*

ANALYSIS OF CHYLE AND LYMPH. BY G. O. REES, M.D., F.G.S., ETC.[*]

THE following analyses of chyle and lymph have lately been made by Dr. Rees, with the view of ascertaining what those ingredients of chyle are which disappear from the lymph, and which are consequently of most service for the nourishment of the body. The fluids were both obtained from the same animal immediately after death. The subject of experiment was a young ass, which was killed by a blow on the head about seven hours after taking a full meal of oats and beans. The chyle was taken from the lacteal vessels before entering the thoracic duct, and was therefore free from lymph; the lymph was from the absorbents of the lower extremity. Analysis yielded the following results:—

	Chyle.	Lymph.
Water	90·237	96·536
Albuminous matter	3·516	1·200
Fibrinous matter	0·370	0·120
Alcoholic extractive	0·332	0·240
Aqueous extractive	1·233	1·319
Fatty matter	3·601	a trace only.
Salts { alkaline, chloride, sulphate and carbonate, traces of phosphate, oxide of iron.. }	0·711	0·585
	100·000	100·000

On these analyses Dr. Rees makes the following observations:—

The albuminous matter, as obtained from the chyle, contained a substance in admixture which gave it a dead white colour, and which Dr. Rees is inclined to believe is identical with a substance existing in the saliva. It may be obtained from both the chyle and saliva by agitating them with æther, when the peculiar principle appears floating above the inferior stratum of fluid. Iron was found in considerable quantity in the aqueous extractive of chyle; and Dr. Rees

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[*] Abstract from the Med. Gaz. for Jan. 1st, 1841.

throws out the suggestion, whether this peculiarity in the locus of iron may not in some way relate to the absence of the red colouring matter of the blood. The colour of chyle has been generally supposed attributable to the fatty matter contained in it; Dr. Rees, however, is of opinion that the white matter mentioned above as analogous to that existing in the saliva, has a considerable share in the production of the opake milky character of the chyle.

NEW MINERAL FROM LANGBANSHYTTA, NEAR FAHLUN; DESCRIBED AND ANALYSED BY PROFESSOR O. B. KÜHN OF LEIPZIG.

This mineral lies immediately on a grayish-black mass, of a metallic lustre, which contains oxide of iron, but no trace of arsenic; and this mass itself lies on fine-grained bitter-spar.

The colour of the mineral is in some places like pale honey, in others dirty white; but there are no definite limits between two differently-coloured parts. It is of a waxen lustre. Its specific gravity, determined in water, is 2·52; its hardness between 5 and 6; it is brittle and easily pulverized; only in one direction could I perceive foliated cleavage; in that one the fragments are tolerably even, but in all other directions it splits unevenly.

Heated by itself with the blow-pipe it becomes gray, but does not melt even at the edges; heated in a glass tube it does not yield the smallest quantity of water; with borax and phosphoric salt it effervesces, and a smell resembling that of arsenic is evolved; both glasses are, when melted on platina, transparent, and in every respect nearly colourless; with soda there is, in like manner, effervescence; and indications of manganese are more or less strongly marked in different specimens.

In nitric acid it is perfectly soluble, with greater or less effervescence, in different specimens, though it is in all but trifling: in the fluid we can detect lime, magnesia, some manganese, besides a trace of iron and much arsenic acid: also a trace of chlorine. Fluorine was not to be found in the mineral.

Three analyses have given the following results expressed in numbers:—

Lime	23·22	21·31	20·96
Magnesia	15·68	17·07	15·61
Protoxide of manganese	2·13		4·26
Iron, only a trace			
Arsenic acid	58·52		56·56
Loss by ignition..........	0·30		2·95
Insoluble matter..........			0·23
	99·85		99·57

Though the last analysis was even made with the greatest possible care, still the great difference between the quantities of lime and manganese cannot be accounted for; still more the quantity of manganese varies in different parts, which, indeed, might be expected from its external appearance; and then, according to the law of isomorphism, it replaces an equivalent quantity of lime. We may assume, with certainty, that it is carbonic acid which is lost by ignition, but we cannot determine with which base it had been combined; most probably with all three, for the three sorts of carbonates are found in almost immediate contact with each other. Now if we calculate on the last values obtained, being the more carefully determined, and if we assume that the three bases are combined with the quantity of carbonic acid in the same proportions in which they are found throughout, then the lime becomes 1·21; magnesia 0·90; and protoxide of manganese 0·25. After subtracting these carbonates and the insoluble matter, the remaining constituents give the following proportions, to which we shall add the twofold calculation from the atomic weights, and from the quantity of oxygen present in each body.

CaO....	7	199·5	20·90	20·81	5·84	12·81	3
MgO....	7	144·2	15·11	15·50	6·02		
MnO....	1	35·7	3·74	4·22	0·95		
$AsO_{2\frac{1}{2}}$..	10	575·0	60·25	59·47		20·69	5
		954·4	100·00	100·00			

The formula hence derived is

$$\left(\begin{array}{l}1\tfrac{1}{2}\,CaO + AsO_{2\frac{1}{2}}\\ \tfrac{1}{15}\,MnO.\end{array}\right) + \left(\begin{array}{l}1\tfrac{1}{2}\,MgO + AsO_{2\frac{1}{2}}\\ \tfrac{1}{15}\,MnO\end{array}\right)$$

or, after the mode of Berzelius,

$$\left.\begin{array}{l}3\,CaO\\ 3\,MgO\\ 3\,MnO\end{array}\right\} As_2O_5.$$

This mineral requires a particular name as a new combination. Neither the composition nor the external properties are sufficiently marked to be expressed by it. I shall therefore make use of a customary resource in mineralogy, and call the substance after one who has advanced the science; and as no one doubts that Berzelius, who, through the invention of the doctrine of proportions and the formation of formulæ, as well as through his innumerable most accurate analyses of mineral bodies, has done so much true service to mineralogy, should be awarded as much honour as those whose names are only brought into the science through the friendship or partiality of some mineralogist, I do not hesitate to propose calling this new compound, first found in Sweden, by the name of Berzeliit.

REMARKABLE SOLAR BOW.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

The following account of a meteorological phænomenon of, I believe, rather a rare character, may not be uninteresting to you.

Being at St. Day with J. S. Enys, Esq., on the afternoon of December 24th, 1840, my attention was called by that gentleman to a solar arc in the west. Around the western horizon were broken masses of cumuli, between and above which the sky was deeply blue, and with the exception of a few light cirri, quite free of clouds. The sun was not more than ten degrees above the horizon; and, at an angle of not more than 12 degrees between the hills to the west of St. Day, appeared the limb of a solar bow, strongly painted, but deficient of the rays of greatest refrangibility, or rather from the singular crimson character of the portion occupied by the red ray, it appeared as if the spectrum was doubled on itself, and the violet mingled with the less refracted rays. The very remarkable appearance and situation of the bow necessarily attracted the attention of many persons, and some who saw it from Falmouth harbour, described it to me as being nearly in a line with the sun.

It is necessary to notice, that no rain was falling: the day was unusually fine for the season; and the temperature, which had during the day been above the freezing point, was then certainly below it. Over the hill on which this phænomenon appeared, was slowly spreading the lightest possible veil of vapour, so thin that a casual observer would have stated that the rays were painted on the most transparent air. It has been suggested, that the phænomenon was produced by an image of the sun reflected from some of the clouds behind the orb itself, and it certainly appears to me highly probable that such was the case; and from its similarity to some of the phænomena I have observed in my experiments on polarized light, I am almost inclined to look upon it as an instance of atmospheric polarization.

I expect the frost bows, of which some accounts have recently been given, are of analogous character to the bow I have described.

I remain, Gentlemen, yours, &c.
Robert Hunt,
Falmouth, Jan. 2, 1841. Secretary Royal Corn. Polytechnic Society.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1840.

Chiswick.—Dec. 1. Hazy: overcast and mild. 2. Very fine. 3. Frosty: fine. 4. Sharp frost: overcast. 5, 6. Hazy. 7. Overcast. 8. Rain. 9. Frosty: clear. 10. Thawing: hazy. 11. Hazy. 12, 13. Overcast and cold. 14. Sharp frost. 15. Dry frosty air. 16. Overcast: snowing. 17. Snowing: cloudy: severe frost at night. 18. Frosty: overcast and cold. 19. Hazy: rain. 20, 21. Overcast and cold. 22—24. Severe frost. 25. Intense frost: Dense fog. 26. Thick hoar frost. 27—29. Foggy. 30. Clear: cloudy: rain at night. 31. Cloudy: clear and fine.

The mean temperature of this month was lower than that of any December within at least the last forty years.

Boston.—Dec. 1. Cloudy: rain early A.M. 2—4. Fine. 5—7. Cloudy. 8. Rain: rain early A.M. 9. Fine. 10—13. Cloudy. 14. Cloudy: snow P.M. 15. Cloudy: snow A.M. 16. Snow. 17, 18, Cloudy. 19. Rain. 20, 21. Cloudy. 22—24. Fine. 25—29. Cloudy. 30. Fine: rain P.M. 31. Fine.

This is the coldest December since 1829.

Applegarth Manse, Dumfries-shire.—Dec. 1. Raw but fair. 2, 3. Fine and fair. 4. Slight showers. 5, 6. Drizzling. 7. Wet and stormy. 8. Fair, but cloudy. 9, 10. Fair, but wet preceding night. 11—13. Fair throughout. 14—16. Hard frost. 17. Thaw, with slight drizzle. 18. Frost again. 19. Slight frost A.M.: drizzle. 20. Frost: Aurora Borealis. 21—24. Frost. 25. Frost A.M.: thaw P.M. 26. Frost again, but cloudy. 27. Thaw A.M.: cloudy and dark. 28. Frost but slight. 29. Frost—moderate. 30. Thaw and snow. 31. Raw and drizzly.

Sun shone out 21 days. Rain fell 9 days. Snow 1 day. Frost 14 days.

Wind north 1 day. North-north-east 1 day. North-east 7½ days. East-north-east 1 day. East 4 days. East-south-east 3 days. South-east 3 days. South-south-east 1 day. South 1 day. South-west 3½ days. West-south-west 1 day. West 2 days. North-west 2 days.

Calm 12 days. Moderate 8 days. Brisk 6 days. Strong breeze 1 day. Boisterous 4 days.

Meteorological Observations made at the Apartments of the Royal Society 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, and by* Mr. DUNBAR *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1840. Dec.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dum-fries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·960	29·998	29·924	29·44	29·68	29·88	51·7	52·8	45·8	56	38	51	48	44	SE. var.	W.	calm	SW.	...	...	·05	...	45
☽ 2.	30·188	30·311	30·137	29·69	30·05	30·33	44·6	55·5	44·7	51	30	39	42	35½	WNW.	N.	calm	NW.	...	...	...	...	42
3.	30·538	30·493	30·493	30·11	30·35	30·32	36·3	47·4	35·7	43	22	31	41	25½	NW.	NE.	calm	WSW.	...	...	...	...	36
4.	30·480	30·465	30·375	30·02	30·22	30·18	32·8	42·3	32·5	41	29	33	44½	40	SW.	SW.	calm	SW.	...	...	...	...	33
5.	30·466	30·353	30·250	29·96	30·16	30·10	39·4	41·0	32·7	42	37	36	46½	34½	SW.	W.	calm	SE.	...	...	...	...	34
6.	30·186	30·171	30·018	29·79	29·90	29·69	40·8	43·5	38·6	42	25	39	46	42	E.	SW.	calm	S.	...	...	...	...	35
7.	29·808	29·817	29·435	29·43	29·33	29·10	39·2	43·7	35·2	41	33	37·5	46	42	E.	S.	S.	SE.	...	...	...	0·50	35
8.	29·230	29·384	29·229	28·95	28·93	29·22	39·7	40·3	34·2	43	26	35	42	38	E.	S.	S.	ESE.	...	·10	·05	...	34
○ 9.	29·738	29·923	29·692	29·36	29·48	29·65	36·3	44·3	36·2	45	26	33·5	41½	33½	S.	SW.	calm	ESE.	·066	...	·26	...	35
10.	29·970	29·910	29·892	29·60	29·66	29·83	40·8	42·0	36·2	44	36	36	46	37	E.	SE.	calm	SE.	...	...	...	...	38
11.	29·958	29·947	29·897	29·65	29·93	29·99	37·6	45·0	35·2	37	32	36	42½	34	ENE.	E.	calm	ESE.	...	...	...	...	36
12.	30·124	30·195	30·086	29·80	30·10	30·22	38·5	39·2	36·0	38	32	38	36½	32½	N.	NE.	calm	ENE.	...	...	...	...	34
13.	30·282	30·257	30·248	30·00	30·28	30·31	33·8	39·7	33·7	37	24	34	37	33	N.	NE.	calm	E.	...	...	...	...	33
14.	30·236	30·227	30·207	29·96	30·25	30·26	27·3	35·4	27·3	32	23	31	34	24	N.	NE.	E.	E.	...	...	...	0·30	25
☾ 15.	30·276	30·245	30·202	29·98	30·29	30·32	27·0	31·3	27·3	26	18	28	34	26½	N.	NE.	calm	NNE.	...	...	...	...	25
16.	30·132	30·154	29·818	29·86	30·11	29·94	28·3	28·5	24·0	33	28	28·5	35½	27	NW.	NW.	W.	NW.	...	...	·20	...	25
17.	29·772	29·762	29·758	29·50	29·82	29·85	31·4	34·6	27·7	28	17	35	36	34	NE.	NW.	calm	NE.	...	...	...	...	28
18.	29·614	29·600	29·500	29·46	29·81	29·74	28·3	31·7	21·2	33	28	35	35½	33	N.	NE.	E.	NE.	...	...	...	...	25
19.	29·588	29·644	29·579	29·39	29·68	29·82	31·3	35·5	29·2	34	32	36	36½	34	N.	NE.	calm	NE.	...	·19	...	...	27
20.	29·840	30·108	30·826	29·65	30·01	30·26	35·8	36·3	29·3	36	34	36·5	38½	33	N.	SE.	calm	N.	...	...	·02	...	29
21.	30·262	30·303	30·247	30·00	30·34	30·38	35·6	37·2	35·0	36	30	37	35½	28½	NE.	NE.	calm	NE.	...	...	...	0·00	30
22.	30·344	30·329	30·255	30·10	30·36	30·25	32·2	37·3	32·3	33	19	32	36½	28	ENE.	NE.	E.	E.	...	...	...	...	28
● 23.	30·176	30·179	30·055	29·90	30·13	30·14	27·5	32·8	26·9	32	21	28	45	26½	NNW.	NE.	calm	NE.	...	...	...	...	25
24.	30·100	30·162	30·088	29·85	30·15	30·33	32·3	34·4	27·0	36	16	31	38½	23	NNW.	NE.	calm	NE.	...	...	...	...	28
25.	30·328	30·366	30·308	30·03	30·34	30·35	30·0	36·7	28·9	28	15	33·5	36½	32	N.	E.	calm	NE.	...	...	...	...	28
26.	30·562	30·595	30·553	30·20	30·42	30·59	29·0	33·4	25·0	31	27	32	38½	33	NW.	N.	calm	SW. NE	...	...	...	...	25
27.	30·656	30·644	30·532	30·30	30·58	30·50	32·3	32·6	29·2	33	29	32	36½	32	NW.	NE.	calm	E.	...	...	...	...	26
28.	30·408	30·400	30·275	30·10	30·30	30·23	30·3	35·3	29·4	32	18	31	37½	31½	E.	E.	calm	SSE.	...	...	...	0·12	28
29.	30·256	30·306	30·248	29·91	30·17	30·34	26·4	33·0	24·2	36	19	28	37½	31	S.	W.	calm	W.	...	...	...	...	26
30.	30·406	30·412	30·164	30·05	30·26	29·77	27·8	37·6	26·2	40	31	27·5	51½	19½	S.	W.	calm	SW.	...	·16	...	...	27
☽ 31.	29·886	29·990	29·873	29·49	29·72	29·83	41·3	42·0	28·5	45	25	41	45	39	W.	NW.	NW.	W.	·347	...	·27	...	30
Mean.	30·121	30·150	30·069	29·78	30·026	30·055	34·4	38·8	31·5	37·55	26·45	34·2	40·2	32·4					Sum. ·413	·45	·85	0·92	Mean. 31

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

MARCH 1481.

XXXII. *On certain points in the Theory of Undulations, related to the Hypothesis of the Symmetrical Arrangement of the Molecules. By the Rev.* BADEN POWELL, *M.A., F.R.S., &c., Savilian Professor of Geometry, Oxford**.

(1.) AMONG the numerous and important accessions which have of late years been made to our knowledge of the theory of light, and the chief of which have from time to time found a place in the pages of this Journal, not the least valuable have arisen out of the free discussion of points of difficulty or objection, whether affecting the stability of the theory itself, or referring to the structure of some of its subordinate parts.

Among such topics, my present remarks relate to one which some time since struck me as of peculiar importance; originating in the first instance in the researches of Mr. Tovey on elliptic polarization†, and further illustrated by several communications from Sir J. Lubbock ‡, yet involved in some difficulties, from which I do not think anything hitherto published has completely relieved it.

(2.) More precisely: Mr. Tovey's investigation § involves an important principle, viz. the existence of a *relation* between the peculiar supposed *arrangement of the molecules* of the ætherial medium in space, and the *nature of the vibration as elliptic or rectilinear.* The character of such arrangement is indicated mathematically by the presence or evanescence of certain terms in the differential equations. These terms had

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* Communicated by the Author.

† L. & E. Phil. Mag. vol. xii. No. 71. Jan. 1838.

‡ Vol. xi. No. 69. Nov. 1837; Vol. xii. No. 71. Jan. 1838; Vol. xv. No. 97. Nov. 1839.

§ While this sheet was passing through the press I have been informed by a friend, that Fresnel adverts to a similar principle, but the time does not allow of my referring to it now more particularly.

been regarded as *evanescent* by M. Cauchy and others: and this as a consequence derived from the hypothesis of a *uniform* distribution of the ætherial molecules in space. Mr. Tovey's object in the paper referred to is to show that when this is *not the case*, elliptic polarization is the result.

Highly valuing this investigation, but conceiving that the remarkable conclusion was not so fully explained as its importance seemed to render desirable, in a paper inserted in the Phil. Trans. for 1838, part ii.*, I endeavoured to establish and further elucidate the conclusion by what seemed to me a more direct and perspicuous method.

The intimate connexion between these theoretical views and the important points discussed in the brief but masterly papers of Sir J. Lubbock, was soon rendered evident. The direct object of these papers was chiefly the illustration of Fresnel's views respecting the axes of elasticity and the wave surface; and there appeared at first sight some degree of contradiction between these deductions and the views just referred to. I endeavoured to draw attention to the difficulty in a short communication to the British Association at Birmingham, 1839†.

In consequence of some correspondence and further discussion, I drew up a supplement to my last-named paper, which was inserted in the Phil. Trans. 1840, part i., in which I pointed out the modifications which the former conclusions would receive in connexion with the principles thus elucidated.

This supplement, however, was very brief: other points also appeared to call for further notice, and the whole investigation seemed to require re-casting. My present object is therefore to offer in the first instance a general review of the main investigation in such a form as I trust may meet all the difficulties which have been urged; and further, to point out its bearing on the several topics with which it stands connected.

(3.) To express undulations we shall suppose the common formulas adopted for the vibration of a single molecule of æther, the assemblage of which expressed by the summation Σ, represents a ray of light: and as in all the investigations on this subject, if $\xi\ \eta\ \zeta$ be the displacement of a molecule in the direction of the three rectangular axes of $x\ y\ z$, and further if we take the ray as coinciding with the axis x, then on the principle of transverse vibrations, we have

$$\begin{aligned} \xi &= 0 \\ \eta &= \Sigma\,[\alpha \sin (n\,t - k\,x)] \\ \zeta &= \Sigma\,[\beta \sin (n\,t - k\,x)] \end{aligned} \qquad (1.)$$

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[*] Of which an abstract appeared in L. & E. Phil. Mag. vol. xiii. p. 222. —Edit.]

† See Sectional Proceedings, p. 3.

where t is the time from the commencement of the disturbance, while the velocity of the propagation of the wave is expressed by $v = \frac{n}{k} = \frac{1}{\mu}$ (2.) and $k = \frac{2\pi}{\lambda}$ (3.) λ being the wave-length in the medium.

In common or unpolarized light the vibrations are in all possible azimuths round x; hence the coefficients α and β are wholly arbitrary and independent.

In plane polarized light we have, on Fresnel's principles,

$$\alpha = \text{A} \cos i \qquad \beta = \text{A} \sin i \qquad (4.)$$

whence i is the angle formed by the plane of vibration with another plane, which he terms the plane of polarization: also the squares of the amplitudes expressing the intensities of light,

$$\alpha^2 + \beta^2 = \text{A}^2.$$

The formulas in this case represent *two* rays polarized in planes at right angles.

If we consider only one ray wholly polarized in either plane, it is equivalent to supposing either $i = 0$, or $i = \frac{\pi}{2}$, or that one of the formulas disappears. (5.)

M. Cauchy, Professor Maccullagh, Mr. Tovey, and other mathematicians term the plane of polarization that *in* which the vibration is performed. M. Fresnel uses the same term to signify the plane *perpendicular* to this. This difference in terms, however, involves consequences which affect the subsequent applications of the theory*. But this will not influence our present investigation.

In the case of elliptical vibrations we have to consider *not*, as in the other cases, a *rectilinear* displacement and its resolved parts, but a *curvilinear* displacement, which is the result of two virtual rectilinear displacements at right angles to each other, and in a plane perpendicular to the ray, and one of which is *retarded* behind the other by an interval b. Thus the expression will be

$$\begin{aligned} \eta &= \Sigma\,[\alpha \sin (n\,t - k\,x)] \\ \zeta &= \Sigma\,[\beta \sin (n\,t - k\,x - b)], \end{aligned} \qquad (6.)$$

in which, taking a single term, and substituting, we readily find the equation to the ellipse described by a vibrating molecule, the origin being at the centre, the conjugate axes parallel to the coordinate axes of y and z, and their values being

$$\alpha \sin b = \tfrac{1}{2} \text{ axis} \qquad \beta \sin b = \tfrac{1}{2} \text{ conj. axis} \qquad (7.)$$

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[*] See L. & E. Phil. Mag. and Journal of Science, vol. xii. No. 74, p. 259.

This constitutes elliptically polarized light: if $\alpha = \beta$, it is circularly polarized.

The retardation b is constant for the whole ray; and if we suppose that for all the ellipses the values of α and of β respectively are equal, these quantities will become constant coefficients in the summation, and we may write

$$\Sigma\,\alpha = h\,\alpha \qquad \Sigma\,\beta = h\,\beta \tag{8.}$$

And the formulas (6.) will become

$$\begin{aligned}\eta &= \alpha\,\Sigma\,(\sin\,(n\,t - k\,x)) \\ \zeta &= \beta\,\Sigma\,(\sin\,(n\,t - k\,x - b)).\end{aligned} \tag{9.}$$

(4.) In order to obtain the equations which express the motions of a system of molecules, connected by their attractive and repulsive forces so as to form an elastic medium, (which is the idea originally pursued by M. Navier and since by M. Cauchy and others,) I will suppose the same method followed as in all these investigations, and which it will not be necessary here to repeat; these equations in their general form will be found in several papers in this Journal, as in my abstract of M. Cauchy* (though in a different form); in Mr. Tovey's paper†; or again in my paper in the Phil. Trans. 1838, part ii. eq. (13.), and in Mr. Kelland's memoir, Camb. Trans., vol. vi. p. 158.

When we adopt the supposition of the ray coinciding with x, as above, the equations in question are

$$\frac{d^2\,\eta^2}{d\,t^2} = m\,\Sigma\,[\phi\,(r)\,\Delta\eta + \psi\,(r)\,\Delta y\,(\Delta\,y\,\Delta\,\eta + \Delta\,z\,\Delta\,\zeta)] \tag{10.}$$

$$\frac{d^2\,\zeta}{d\,t^2} = m\,\Sigma\,[\phi\,(r)\,\Delta\zeta + \psi\,(r)\,\Delta\,z\,(\Delta\,z\,\Delta\,\zeta + \Delta\,y\,\Delta\,\eta)], \tag{11.}$$

where m is the unit of force, r the distance from the molecule first agitated, $\Delta\,y\;\Delta\,z$ the differences of coordinates of the molecules from the first, $\Delta\,\eta\;\Delta\,\zeta$ the corresponding differences of the displacements.

It is also material to observe, that these equations have been obtained *without any particular supposition being made as to the arrangement of the molecules in space: they consequently apply if we imagine the molecules distributed in the most irregular or unsymmetrical manner.*

(5.) If we consider the two component displacements $\eta\;\zeta$ which enter the above equation as related in the way expressed by the formula (6.),

$$\begin{aligned}\eta &= \Sigma\,[\alpha\,\sin\,(n\,t - k\,x)] \\ \zeta &= \Sigma\,[\beta\,\sin\,(n\,t - k\,x - b)]\end{aligned}$$

(in which, if $\alpha\,\beta$ and b are assumed as before explained, we

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* L. & E. Phil. Mag. and Journal of Science, vol. vi. No. 31, p. 25.

† Ibid. vol. viii. No. 43.

can express the varieties of elliptically or circularly polarized light,) it is easy to show this formula is the solution of the differential equations in the form just given (10.) (11.), if n have a certain value, which we proceed to determine.

Taking the increments of these expressions, we have

$$\Delta\eta = \Sigma \left\{ \begin{aligned} &\Big[-2\sin^2 \frac{k\,\Delta\,x}{2} \sin\,(n\,t - k\,x) \\ &\quad - \sin k\,\Delta\,x \cos\,(n\,t - kx) \Big] \end{aligned} \right\} \tag{12.}$$

$$\Delta\zeta = \left\{ \begin{aligned} &\Sigma \Big\{ \beta\cos b \Big[-2\sin^2 \frac{k\,\Delta\,x}{2} \sin\,(n\,t - k\,x) \\ &\qquad\qquad - \sin k\,\Delta\,x \cos\,(n\,t - k\,x) \Big] \Big\} \\ &+ \Sigma \Big\{ \beta\sin b \Big[\quad \sin k\,\Delta\,x \sin\,(n\,t - k\,x) \\ &\qquad\qquad -2\sin^2 \frac{k\,\Delta\,x}{2} \cos\,(n\,t - k\,x) \Big] \Big\}. \end{aligned} \right\} \tag{13.}$$

Also differentiating them, we find

$$\frac{d^2\,\eta}{d\,t^2} = -\,n^2\,\Sigma\,\alpha \sin\,(n\,t - k\,x) \tag{14.}$$

$$\frac{d^2\,\zeta}{d\,t^2} = -n^2\,\Sigma\,\{\beta\cos b \sin\,(nt - k\,x) - \beta\sin b \cos\,(nt - k\,x)\} \tag{15.}$$

Now for brevity writing

$$\left. \begin{aligned} p \;&= \phi\,(r) + \psi\,(r)\,\Delta\,y^2 \\ p' \;&= \phi\,(r) + \psi\,(r)\,\Delta\,z^2 \\ q \;&= \psi\,(r)\;\Delta\,y\,\Delta\,z \\ 2\,\theta &= k\,\Delta\,x \end{aligned} \right\} \tag{16.}$$

the equations (10.) and (11.) will be expressed by

$$\frac{d^2\,\eta}{d\,t^2} = m\,[\Sigma\,(\,p\,\Delta\,\eta) + \Sigma\,(q\,\Delta\,\zeta)\,] \tag{17.}$$

$$\frac{d^2\,\zeta}{d\,t^2} = m\,[\Sigma\,(p'\Delta\,\zeta) + \Sigma\,(q\,\Delta\eta\,)]. \tag{18.}$$

And here substituting the above values of $\Delta\,\eta\,\Delta\,\zeta$, and arranging the terms, these equations become

$$\frac{d^2\,\eta}{d\,t^2} = m \left\{ \begin{aligned} &\left. \begin{aligned} &+ \sin\,b\,\Sigma\,[\beta\,q \quad \sin\,2\,\theta] \\ &- \cos b\,\Sigma\,[\beta\,q\;2\,\sin^2.\,\theta] \\ &- \qquad\;\; \Sigma\,[\alpha\,p\;2\,\sin^2.\,\theta] \end{aligned} \right\} \sin\,(n\,t - k\,x) \\ &\left. \begin{aligned} &- \qquad\;\; \Sigma\,[\alpha\,p \quad \sin\,2\,\theta] \\ &- \cos b\,\Sigma\,[\beta\,q \quad \sin 2\,\theta] \\ &- \sin\,b\,\Sigma\,[\beta\,q\;2\,\sin^2\;\;\theta] \end{aligned} \right\} \cos\,(n\,t - k\,x) \end{aligned} \right. \tag{19.}$$

$$\frac{d^2\zeta}{dt^2} = m\left\{\begin{array}{l} \left.\begin{array}{l} +\sin b\,\Sigma\,[\beta p' \ \sin 2\theta] \\ -\cos b\,\Sigma\,[\beta p' \ 2\sin^2\theta] \\ -\qquad\ \ \Sigma\,[\alpha q \ 2\sin^2\theta] \end{array}\right\}\sin(nt-kx) \\ \left.\begin{array}{l} -\qquad\ \ \Sigma\,[\alpha q \ \sin 2\theta] \\ -\cos b\,\Sigma\,[\beta p' \ \sin 2\theta] \\ -\sin b\,\Sigma\,[\beta p' \ 2\sin^2\theta] \end{array}\right\}\cos(nt-kx). \end{array}\right. \qquad (20.)$$

On comparing these expressions with those for the same functions (14.) (15.), which must be identical, and equating the respective coefficients of sin $(nt-kx)$ and of cos $(nt-kx)$, since they must hold good for all values of those terms, we have the following equations:

$$-n^2\Sigma\,\alpha = m\left\{\begin{array}{l} +\sin b\,\Sigma\,[\beta q \ \sin 2\theta] \\ -\cos b\,\Sigma\,[\beta q \ 2\sin^2\theta] \\ -\qquad\ \ \Sigma\,[\alpha p \ 2\sin^2\theta] \end{array}\right. \qquad (21.)$$

$$0 = m\left\{\begin{array}{l} -\qquad\ \ \Sigma\,[\alpha p \ \sin 2\theta] \\ -\cos b\,\Sigma\,[\beta q \ \sin 2\theta] \\ -\sin b\,\Sigma\,[\beta q \ 2\sin^2\theta] \end{array}\right. \qquad (22.)$$

$$-n^2\cos b\,\Sigma\,\beta = m\left\{\begin{array}{l} +\sin b\,\Sigma\,[\beta p' \ \sin 2\theta] \\ -\cos b\,\Sigma\,[\beta p' \ 2\sin^2\theta] \\ -\qquad\ \ \Sigma\,[\alpha q \ 2\sin^2\theta] \end{array}\right. \qquad (23.)$$

$$-n^2\sin b\,\Sigma\,\beta = m\left\{\begin{array}{l} -\qquad\ \ \Sigma\,[\alpha q \ \sin 2\theta] \\ -\cos b\,\Sigma\,[\beta p' \ \sin 2\theta] \\ -\sin b\,\Sigma\,[\beta p' \ 2\sin^2\theta] \end{array}\right. \qquad (24.)$$

From the two last forms (23.) (24.), by multiplication and addition, we obtain

$$-n^2\Sigma\,\beta = m\left\{\begin{array}{l} -\qquad\ \ \Sigma\,[\beta p' \ 2\sin^2\theta] \\ -\cos b\,\Sigma\,[\alpha q \ 2\sin^2\theta] \\ -\sin b\,\Sigma\,[\alpha q \ \ \sin^2\theta] \end{array}\right. \qquad (25.)$$

In the case of elliptic polarization from the conditions before stated (7, 8, 9), we can obtain from the forms (21.) and (25.),

$$n^2 = \frac{m}{h(\alpha^2+\beta^2)}\left\{\begin{array}{l} \quad\beta^2 \qquad\qquad \Sigma\,[p' \ 2\sin^2\theta] \\ +\ \alpha^2 \qquad\qquad \Sigma\,[p \ 2\sin^2\theta] \\ +2\cos b\,\alpha\beta\,\Sigma\,[q \ 2\sin^2\theta] \end{array}\right. \qquad (26.)$$

Also the form (24.) gives, on transposing,

$$\sin b = \frac{m\,[\alpha\,\Sigma\,(q \sin 2\theta) + \beta\cos b\,\Sigma\,(p' \sin 2\theta)]}{n^2 h\,\beta - m\,\beta\,\Sigma\,(p'\,2\sin^2\theta]} \qquad (27.)$$

Upon the whole, then, we see that the formula (9.) for elliptically polarized light, involving the above value of n, is the solution of the differential equations (10.) (11.) for the motion of a system of molecules constituted as at first supposed.

It is important to bear in mind that this solution has been obtained solely from the conditions of elliptic polarization, the original equation *being in the forms* (10.) (11.) *retaining all its terms.*

(6.) If instead of the formula (9.) we had taken the expressions for the component displacements η ζ, as not involving such a relation, but simply as in the original expressions (1.), or supposing $b = 0$ in the forms (6.), we might still pursue steps analogous to those above exhibited, though with different values. To trace these results we have only to alter these formulas agreeably to the new conditions. Thus, for unpolarized light, on making $\sin b = 0$ $\cos b = 1$, we have

$$\text{from (21.)}\quad 0 = \left\{\begin{array}{l} \left\{\begin{array}{l} n^2\,\Sigma\,\alpha \\ -m\,\Sigma\,[\alpha\,p\,2\,\sin^2\theta] \end{array}\right\} \\ -m\,\Sigma\,[\beta\,q\,2\,\sin^2\theta] \end{array}\right. \qquad (28.)$$

$$\text{from (22.)}\quad 0 = \left\{\begin{array}{l} -\;\Sigma\,[\alpha\,p\,\sin 2\,\theta] \\ -\;\Sigma\,[\beta\,q\,\sin 2\,\theta] \end{array}\right. \qquad (29.)$$

$$\text{from (24.)}\quad 0 = \left\{\begin{array}{l} -\;\Sigma\,[\beta\,p'\,\sin 2\,\theta] \\ -\;\Sigma\,[\alpha\,q\,\sin 2\,\theta] \end{array}\right. \qquad (30.)$$

$$\text{from (23.)}\quad 0 = \left\{\begin{array}{l} \left\{\begin{array}{l} n^2\,\Sigma\,\beta \\ -m\,\Sigma\,[\beta\,p'\,2\,\sin^2\theta] \end{array}\right\} \\ -m\,\Sigma\,[\alpha\,q\,2\,\sin^2\theta] \end{array}\right. \qquad (31.)$$

Now in all these equations it is evident that since α and β are by the original condition *wholly arbitrary and independent* both of each other and of the other quantities, these equations can only hold good for *all values whatever*, of α and β, if each of the terms involving respectively α and β are *separately* $= 0$, that is, we must have

$$\text{from (29.)}\quad \left\{\begin{array}{ll} 0 = \Sigma\,[\alpha\,p\,\sin 2\,\theta] & (32.) \\ 0 = \Sigma\,[\beta\,q\,\sin 2\,\theta] & (33.) \end{array}\right.$$

$$\text{from (30.)}\quad \left\{\begin{array}{ll} 0 = \Sigma\,[\beta\,p'\,\sin 2\,\theta] & (34.) \\ 0 = \Sigma\,[\alpha\,q\,\sin 2\,\theta] & (35.) \end{array}\right.$$

$$\text{from (28.)}\quad 0 = \Sigma\,[\beta\,q\,2\,\sin^2\theta] \qquad (36.)$$

$$\text{from (31.)}\quad 0 = \Sigma\,[\alpha\,q\,2\,\sin^2\theta] \qquad (37.)$$

$$\text{from (28.)}\quad 0 = n^2\,\Sigma\,\alpha - m\,\Sigma\,[\alpha\,p\,2\,\sin^2\theta] \qquad (38.)$$

$$\text{from (31.)}\quad 0 = n^2\,\Sigma\,\beta - m\,\Sigma\,[\beta\,p'\,2\,\sin^2\theta]. \qquad (39.)$$

Hence the formula for unpolarized light is *only* a solution, provided those conditions are fulfilled in the original equations.

From the last two forms (38.) (39.) we have

$$n^2 = \frac{m}{\Sigma\,\alpha}\,\Sigma\,[\alpha\,p\;2\sin^2\theta]. \tag{40.}$$

$$n^2 = \frac{m}{\Sigma\,\beta}\,\Sigma\,[\beta\,p'\;2\sin^2\theta]. \tag{41.}$$

For plane polarized light from the conditions (5.) let $\beta = 0$, and the form (22.) will become

$$0 = \Sigma\,[\alpha\,p\sin 2\,\theta]. \tag{42.}$$

In like manner (23.) will give

$$0 = \Sigma\,[\alpha\,q\;2\sin^2\theta] \tag{43.}$$

and (24.) $$0 = \Sigma\,[\alpha\,q\sin 2\,\theta]\,; \tag{44.}$$

while from (21.) we find

$$n^2 = \frac{m}{\Sigma\,\alpha}\,\Sigma\,[\alpha\,p\;2\sin^2\theta]. \tag{45.}$$

Hence the formula for plane polarized light is *only* a solution, provided these conditions are fulfilled in the original equations.

Since $\Delta\,\eta$ and $\Delta\,\zeta$ are both of the form

$$\text{M}\sin 2\,\theta + \text{N}\,2\sin^2\theta,$$

the above conditions give

$$\Sigma\,(q\,\Delta\,\eta) = 0 \tag{46.}$$

$$\Sigma\,(q\,\Delta\,\zeta) = 0. \tag{47.}$$

Thus when they are fulfilled in the terms of the original equations, those equations become, for unpolarized light,

$$\frac{d^2\eta}{d\,t^2} = \Sigma\,[p\,\Delta\,\eta] \tag{48.}$$

$$\frac{d^2\,\zeta}{d\,t^2} = \Sigma\,[p'\Delta\,\zeta], \tag{49.}$$

while for plane polarized light the second of these forms disappears.

(7.) Now recurring to the supposed constitution of the medium, to examine the conditions under which these terms can vanish, we may first observe, that since none of the factors can separately become = 0, the terms can only become nothing by sums with opposite signs being equal and destroying each other.

That this may happen depends on an hypothesis respecting the arrangement of the *ætherial molecules* in spaces, viz. that they are *distributed uniformly*. This is the supposition adopted by M. Cauchy and other writers.

On this hypothesis the axis x passing through the first molecule m in any direction, the sums of the corresponding distances of all the other molecules on each side of it, whether in the plane of y or z, will be equal for all positions of x in the medium.

It is easily seen that the respective sums of products

$$\phi\, r \sin 2\,\theta \qquad \psi\, r\, \Delta\, y^2 \sin 2\,\theta \qquad \psi\, r\, \Delta\, z^2 \sin 2\,\theta$$
$$\psi\, r\, \Delta\, y\, \Delta\, z \sin 2\,\theta$$

with opposite signs will be equal. Thus we shall always have

$$\left.\begin{array}{l} \Sigma\, [\alpha\, q \sin 2\,\theta] = 0 \\ \Sigma\, [\alpha\, p \sin 2\,\theta] = 0 \\ \Sigma\, [\alpha\, p' \sin 2\,\theta] = 0 \end{array}\right\}. \qquad (50.)$$

Whenever these terms are evanescent, it is easy to show that we always have also

$$\Sigma\, [\alpha\, q\, 2 \sin^2 \theta] = 0\,; \qquad (51.)$$

and similarly for the like terms involving β, by a simple transformation of coordinates, as explained by Sir J. Lubbock in his valuable paper*. That paper indeed relates to the more general views of the subject, to which I shall refer in the sequel; but the particular process in question is independent of these views.

Thus the hypothesis of symmetrical distribution gives

$$\Sigma\, [q\, \Delta\, \eta] = 0 \qquad \Sigma\, [q\, \Delta\, \zeta] = 0 \qquad (52.)$$

and the original equations are reduced to

$$\frac{d^2\,\eta}{d\,t^2} = \Sigma\, [p\, \Delta\, \eta] \qquad (53.)$$

$$\frac{d^2\,\zeta}{d\,t^2} = \Sigma\, [p'\, \Delta\, \zeta]\,; \qquad (54.)$$

or restoring the original values,

$$\frac{d^2\,\eta}{d\,t^2} = \Sigma\, [(\phi\,(r) + \psi\,(r)\, \Delta\, y^2)\, \Delta\, \eta] \qquad (55.)$$

$$\frac{d^2\,\zeta}{d\,t^2} = \Sigma\, [(\phi\,(r) + \psi\,(r)\, \Delta\, z^2)\, \Delta\, \zeta]. \qquad (56.)$$

That is, the equations are reduced to precisely the same form by the hypothesis of uniform distribution, as they are on the hypothesis of unsymmetrical distribution, by the conditions of plane polarized and unpolarized light.

Thus the formulas for *plane polarized and unpolarized* light are *only* solutions of the original equations when in the same form, to which they are reduced by *symmetrical distribution.*

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* L. & E. Phil. Mag. and Journal of Science, vol. xv. November, 1839.

For elliptically polarized light, on the hypothesis of *symmetrical* distribution, we can follow out results analogous to those above obtained. We should have instead of the form (21.),

$$n^2 h = m \Sigma \left[p\, 2 \sin^2 \theta\right]; \qquad (57.)$$

and instead of (25.),

$$n^2 h = m \Sigma \left[p'\, 2 \sin^2 \theta\right], \qquad (58.)$$

which is identical with the former, whence we have $p = p'$; also,

$$n^2 = \frac{m}{h} \Sigma \left[p\, 2 \sin^2 \theta\right]. \qquad (59.)$$

The formula (27.) is thus reduced to

$$\sin b - \tfrac{0}{0} \qquad (60.)$$

Thus (although with altered values) the formula for *elliptic polarization* is a solution of the original equation *equally* in the form (10.) (11.), and when reduced to the form (55.) (56.) by the hypothesis of *symmetrical distribution.* In other words, of the equations, in the form (10.) (11.), the formula for elliptic polarization is the only solution: in the form (55.) (56.) the formulas for elliptic vibrations or rectilinear indifferently are solutions.

(8.) Thus it follows, that if we suppose the ætherial molecules *unsymmetrically* distributed, then *elliptic polarization alone* is the result. *Æther so constituted cannot admit rectilinear vibrations.* Light, therefore, entering such a portion of æther necessarily becomes elliptically polarized.

If we suppose the molecules symmetrically distributed, this is compatible with either elliptic or rectilinear vibrations indifferently. Either therefore will be propagated according to the condition of the intromitted ray.

Thus elliptic polarization is traced to its cause in the simple consideration that the vibrations which constitute it are necessarily produced when waves are propagated through any portion of æther in which a symmetrical arrangement of the molecules does not subsist.

The investigation conducted by Mr. Tovey's method*, is directed to showing by the equations (4.) of his paper, that when the sums involving the odd powers of the differences are *not* evanescent, the quantities b and ρ (the ratio of the semiaxes of the ellipse) are *determinate*; or, in other words, the expressions must belong to ellipses, or in a medium so constituted as to make those sums finite, elliptical polarization will result. When

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* L. & E. Phil. Mag. and Journal of Science, vol. xii. p. 10.

the sums just mentioned vanish, then it is seen that those quantities are altogether arbitrary, and the movements will be the same as those expressed by the author's formulas in his other paper *; or such a medium will propagate elliptic or rectilinear vibrations indifferently†.

(9.) Mr. Whewell ‡ had observed the difficulty of conceiving any mechanical conditions for the production of elliptic polarization, and that not even a plausible hypothesis had been proposed so as to give a physical interpretation to the language of analysis, especially as conveyed in the equations obtained by Prof. Maccullagh.

From what has just been stated, this difficulty appears now to be, at least in a general way, overcome by the conclusion of Mr. Tovey.

It is easy to conceive the physical possibility of a portion of the æther possessing an unsymmetrical arrangement of its molecules. For example, at the bounding surface of a medium and of vacuum, or generally of two media of different densities, we can hardly suppose the change of density in the æther to take place abruptly; but must from all analogy imagine a thin stratum on either side, within which there is a gradual alteration in the arrangement of the molecules; and this more considerable as the difference of the refractive powers is greater. It is conceivable that this variation may in some instances be of sufficiently great amount to give the requisite conditions of unsymmetrical distribution within this stratum, though on either side of it the symmetrical arrangement may subsist.

(10.) With regard to the value of n, experiment shows that the state of the same ray as to polarization produces no difference in the magnitude of its refractive index. Hence it follows that in all the preceding different cases in which the value of n has been expressed (whether on the hypothesis of unsymmetrical or of symmetrical distribution) the terms involved must vary in magnitude, so that the whole expression shall remain constant, or the values (26. 40. 41. 45. 59.) all equal. Thus, in general, writing h for the sum of the arbitrary terms α and β, we express the value of n by the formula

$$n^2 = \frac{m}{h} \Sigma \left[\alpha p \, 2 \sin^2 \theta\right]. \qquad (61.)$$

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* L. & E. Phil. Mag. and Journal of Science, vol. viii. p. 426 and p. 502.

† See also the same author's paper on the nature of the vibrations in quartz, vol. xiv. 1839, pp. 169 and 323,

‡ Hist. of Ind. Science, vol. ii. p. 448: 1837.

If we return to the values (2.) (3.),

$$k = \frac{2\,\pi}{\lambda} \qquad \theta = \frac{\pi\,\Delta\,x}{\lambda}, \tag{62.}$$

we have obviously

$$\frac{1}{\mu^2} = \frac{n^2}{\left(\frac{2\,\pi}{\lambda}\right)^2} = \frac{1}{4}\,\frac{n^2\,\Delta\,x^2}{\left(\frac{\pi\,\Delta\,x}{\lambda}\right)^2} = \frac{1}{4}\,\frac{n^2\,\Delta\,x^2}{\theta^2}; \tag{63.}$$

and on substituting in the form (61.) we obtain

$$\frac{1}{\mu^2} = \frac{m}{2h}\,\Sigma\left\{\alpha\,(\phi\,(r) + \psi(r)\,\Delta y^2)\,\Delta x^2\left(\frac{\sin^2\left(\frac{\pi\,\Delta\,x}{\lambda}\right)}{\left(\frac{\pi\,\Delta\,x}{\lambda}\right)^2}\right)\right\} \tag{64.}$$

which for abridgement may be expressed by

$$\frac{1}{\mu^2} = \Sigma\left\{\mathrm{H}^2\,\frac{\sin^2\left(\frac{\pi\,\Delta\,x}{\lambda}\right)}{\left(\frac{\pi\,\Delta\,x}{\lambda}\right)^2}\right\} \tag{65.}$$

the formula for the dispersion.

(11.) I have here confined my remarks strictly to the illustration of the one primary question of the criterion of elliptic polarization; but in all that has been said relative to the evanescence of the terms on the hypothesis of symmetrical arrangement, by the transference to new axes, &c., I have touched upon the far more extensive relations between our immediate subject and that of the axes of elasticity and the wave-surface: some remarks on these must be reserved to a future communication; meanwhile I will venture to hope that the particular subject of the preceding observations has now been placed in such a light as to free it from the ambiguity and doubt in which it seems to have been involved.

XXXIII. *On Lightning Conductors, and on Experiments relating to the Defence of Shipping from Lightning.* By W. Snow Harris, *Esq., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

You will greatly oblige me, by allowing the following communication to appear in the pages of your Journal, being the concluding observations I have to offer on Mr. Sturgeon's

Memoir on Marine Lightning Conductors, to which I have alluded in former papers *.

Had the question been one of private, rather than of public interest, I should certainly have followed the course pursued by others, who have been honoured with Mr. Sturgeon's censure, and have shown by my silence how little I care to deprive him of any advantage which he may consider he derives by his coarse treatment of me. Such, however, is not the case: my method of defending shipping from lightning by permanent conductors of electricity fixed in their masts, being still carrying out in several of H.M. ships, I feel myself called on to afford a clear elucidation of certain circumstances intimately associated with this highly important question, which have been lately much misrepresented; moreover, the subject is one of increasing interest, and intimately connected with an important department of physical science.

In the year 1780, the French began to turn their attention to the more effectual defence of their buildings and shipping from the effects of lightning.

Mons. Le Roi was sent to visit Brest and the various seaports of France for that purpose. His memoir in the *Histoire de l'Académie des Sciences* for the year 1790, will be read with advantage by every one interested in the subject.

Impressed with the necessity of adopting some more permanent security than was derived from the use of chains, temporarily applied in the rigging, he endeavoured to place, he says, such conductors in ships as might be deemed "fixed and durable." With this view long linked rods of small dimensions were led from a point at the mast-head along a part of the rigging, and fixed to plates of metal at the ship's side communicating with the sea. These, however, he admits did not stand the working of the rigging, and were broken in a storm; so that he was induced to change them for similar linked rods of small size led in divided stages along the masts.

This form of conductor was fixed in the French ships of war Etoile, Resolution, Experiment, Boussole, Astrolabe, and in some ships destined for America. Although Mons. Le Roi had reason to believe that it was sufficiently secure from the effects of mechanical violence, yet it was eventually abandoned in the French service, probably from its still being found inconvenient and ill calculated to meet the violent forces incidental to a ship's mast and rigging. The only kind

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* L. & E. Phil. Mag., December 1839, February 1840, May 1840.

of conductor ever generally used by the English, consists of links of small copper rods under the form of a flexible chain triced up, as occasion required, to the mast-head and allowed to touch the sea, a contrivance recommended by the learned Dr. Watson in the year 1762, in a letter to Lord Anson, then First Lord of the Admiralty.

The damage, however, which, notwithstanding every suggestion of the kind, continued to occur to shipping from atmospheric electricity, and especially to our navy, showed that such methods had not fully met all the circumstances of the case; hence the necessity of resorting to some system of defence, differing essentially from those hitherto tried. Now that system which experience indicated evidently embraced the following principles.

1. The conductor should be permanently fixed, so as to be always in place, and at all times ready to meet the most unexpected danger.

2. It should be continuous and of considerable electrical capacity, so as to admit of a rapid equalization of the electrical action.

3. It should in no way interfere with the standing or running rigging; should admit of the motion of the sliding masts without deranging its existing state; and in case of either being removed by accident or design, the remaining portion of the conductor should be still as efficient as before.

4. It should be independent of the crew of the ship, and not be left to them to put in place or not, as may be deemed requisite; or require any handling, to their great annoyance, —under many circumstances to their imminent peril.

5. It should be so applied that a discharge of lightning falling on the vessel, could not enter into any circuit of which it did not form a part.

The desirableness of such a plan as that which in the year 1820 I proposed to the Admiralty, embracing, as it did, every required condition, was admitted. The doubts, however, existing at the time were, whether it was fully borne out in all its details by a sufficient number of facts in science; and whether it could be carried out so perfectly as to meet the many variable circumstances in which the general fabric in all its casualties might become placed; but no doubt existed of the propriety of adopting such a form of conductor, were these questions satisfactorily disposed of.

Your readers are no doubt aware, that the method I proposed, was to make the masts themselves perfect conductors of electricity, by incorporating with them in a peculiar way two laminæ of copper sheet of considerable thickness and extension; uniting them with the metallic masses in the hull by other laminæ, and giving the whole a perfectly free communication with the sea in all directions. The reasonableness of this as an abstract question in electricity is quite evident; since all the damage found to occur on ship-board from lightning arises principally from the effects of expansion, where the electrical discharge passes over or through bad conducting matter. If we could conceive for an instant that the vessel, sails, rigging, &c. were metallic throughout, the possibility of its receiving any damage from lightning would be inconceivable.

This, in fact, has been shown by the perfect way in which the iron steam-vessels in Lander's last voyage met discharges of lightning whilst the timber vessels were damaged. The nearer, therefore, we can approximate to this condition, the more perfect the security.

I satisfied the Navy Board, together with many eminent scientific men of that day, of the completeness of my views, and of their practical application to the variable conditions of a ship's mast; and the celebrated Dr. Wollaston, in a letter to the Comptroller of the Navy, gave my plan his "decided approval." Under these circumstances it was at last ordered to be carried out in ten of H.M. ships, including line-of-battle ships, frigates and corvettes; a small brig of 10 guns was subsequently added to the list; and thus the conductors were tried in the navy from the largest to nearly the smallest vessel in it. After a period of ten years, during which time these vessels had been exposed to several storms of lightning in all parts of the world, the Lords Commissioners of the Admiralty appointed, under the countenance of the House of Commons, a commission of nautical and scientific men to investigate and report on this momentous topic. The members of the commission were all named without any reference to myself, and were all men of unquestionable integrity and ability. When the names of Admiral Griffith, Sir James Gordon, K.C.B., Captain J. C. Ross, Professor Daniell, Mr. Fincham, and Mr. Clifton of the Admiralty, are mentioned, no one of the least character in society would question the fairness of any investigation placed under their direction.

The Commission, in carrying out this measure, called in evidence the officers who had commanded the ships in which my conductors were tried, and obtained valuable information from various others who had witnessed the effects of lightning at sea. They also invited the opinions of men acknowledged to be the best acquainted with this department of science; and they examined various other forms of lightning conductors for ships. But upon the whole evidence, they conclude with an earnest recommendation to the Board to carry out the plan fully in the Royal Navy*.

No sooner had this report been laid on the table of the House of Commons, than several persons interested in superseding my plan, endeavoured in various ways, notwithstanding its ten years' trial, to get it set aside; hence arose what Mr. Sturgeon calls Experimental and Theoretical Researches in Electricity, Fourth Memoir on Lightning Conductors; especially addressed to the British Association, and to all learned societies in Europe and America,—a list of which is ostentatiously displayed at the head of the memoir.

Mr. Sturgeon's production having been thus paraded forth, we should of course expect to find it contain new and important facts in science, commensurate with so loud an announcement, and with its pretensions, to arrest the attention of all the various learned bodies above mentioned, and as he states in another place, " the ablest electricians which the world can produce." After so great an advancing shadow, we should naturally expect something like an adequate substance.

The following brief outline of the memoir is sufficient to show how little such expectations are realized.

The first five pages, consist of unhandsome insinuations of a want of honesty and ability on the part of the members of the Commission, of pretensions on his own part to put " the question in a proper light," and of deceptive statements of the nature and objects of my experiments, called by him an

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* " Having now completed our remarks on the several points to which their Lordships' instructions directed our attention, we trust we have shown, from the evidence of facts derived from the experience of many years, as well as by the opinions, not only of scientific but professional men, the efficacy of Mr. Harris's lightning conductors; and considering the number of lives which have been lost by lightning, the immense amount of property which has been destroyed, as shown by Mr. Harris, and is still exposed without adequate protection, the inconvenience which has arisen, and is still liable to arise from the loss of the services of ships at moments of great critical importance, the *difficulty of procuring new spars in times of war on foreign stations* (not to mention the great expense of wages and victuals for the crews of ships while rendered useless till repaired),—we again beg to state our *unanimous opinion* of the great advantages possessed by Mr. Harris's conductors *above every other plan,* affording permanent security at all times, and under all circumstances, against the injurious effects of lightning, effecting this protection without any nautical inconvenience or scientific objection whatever; and we therefore most earnestly recommend their general adoption in the Royal Navy."

"examination." The next eight pages consist of notices of the damage done to the Rodney, copied from the newspapers of the day, of a few extracts from the statements made by naval officers who had witnessed the protecting effects of my conductors, and of a few desultory remarks on those statements; here then are thirteen pages to begin with, containing no kind of scientific research whatever. The next three pages and a half contain a few ill-contrived repetitions of Priestley and Cavallo's experiments, made above fifty years since, on the residual electricity of a Leyden jar, and which Mr. Sturgeon, by a most unhappy blunder, has taken as a lateral discharge produced at the instant of the primary discharge *. "This residual electricity (observes Cavallo) should be carefully considered in performing delicate experiments †".

The remaining 15 pages contain merely ill-digested remarks, resting on the above fallacy; designing appeals to the fears of the uninformed; an account of a few common-place and inevitable results of ordinary electrical action on a kite; and a proposal to place copper rods in various ways about the rigging of a ship as a defence against lightning, in open defiance of his own previous admissions; with a few considerations of the expense attendant on it.

How such a paper as this can fairly come under the denomination of "Theoretical and Experimental Researches in Electricity," when not above two out of thirty-two pages, or about one sixteenth of the whole, contain any experiment at all,—the experiments not original, and the reasoning upon them a fallacy,—I am really at a loss to determine.

In my former communications, which appeared in this Journal in December, 1839, and February and May, 1840, I have given a complete exposition of these and other points contained in this memoir; so that but little more remains to notice than the deceptive colouring in which Mr. Sturgeon has thought fit to disguise my experiments, and his illiberal dealing with every one holding opinions adverse to himself.

The simplicity and convenience of my fixed conductors having, as I have already stated, been in the year 1820 fully admitted, the Navy Board at that time thought proper to call upon me to illustrate and investigate experimentally, so far as possible, their probable operation through the ship.

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* Lond. and Edinb. Phil. Mag. for December, 1839.

† Mr. Sturgeon appears to be well aware of the great mistake he has made on this point. In no place, so far as I know, has he attempted to defend it.

I was called upon, for instance, to show what had been the general laws of the action of lightning on ship-board; that the connexion of my conductors with the sea through the metallic masses in the hull was in no way detrimental to their action, or liable to objection; that, in short, electrical discharges might find their way and become as safely dispersed in the sea by the metallic bolts driven through the keel and keelson, and other parts of the vessel, as by a chain in the rigging.

I was further called upon to show, so far as possible, how a system, such as I proposed, would operate in this way under various positions of the masts, and that the circumstance of their motion upon each other would in no way interfere with the action of the conductor. In order to meet the views of the officers of the Board, I naturally enough resorted to such practical illustrations as were within my reach. By way of showing the operation of my conductors through the bolts in the hull, strong charges from twenty-five square feet of coated glass were passed over a vessel's mast fitted with the conductors and floating in the sea, the negative side of the battery being connected with an interrupted circuit passing over a small gun placed in a boat (in some instances) forty fathoms from the vessel. Percussion powder was placed over the joints of the conductor on the mast, and in an interrupted circuit at the mast head. The sliding masts were placed in various positions, and occasionally were put in motion at the time of the experiment; but in all cases at the instant of completing the circuit, the whole charge, equivalent to fuse fifteen feet of small iron wire, passed freely through the conductor and the sea in all positions of the masts without igniting the powder placed over the joints, but firing the powder at the mast-head, and the gun, instantaneously, thereby showing that the charge had reached from the mast head to the water.

Any one will, I think, perceive that these experiments illustrated, so far as they went, the points in question, viz.

1st. The operation of the conductor through the hull.

2nd. Its perfect continuity on the masts.

3rd. Its complete operation under every possible position, &c. of the masts.

4th. The reception of the charge by the sea.

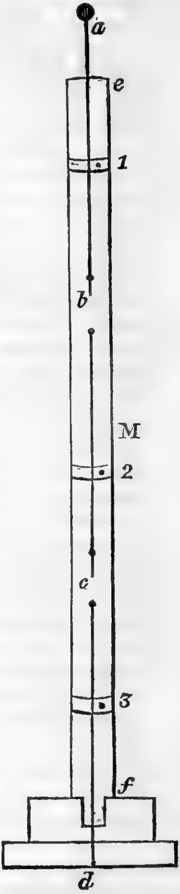
Now Mr. Sturgeon fairly shuts his eyes to the unpretending nature and object of these experiments, and perverting their meaning, tells his readers that they prove nothing peculiar to my system, and serve only to show that copper is a conductor of electricity, and that detonating powder can be ignited by the electrical spark. This he calls a *fair* and *candid explanation* of my experiments before the Navy Board at Plymouth.

As he gives every one who in any way treated this question with fairness, a most liberal credit for ignorance, he thinks I ought to have explained to the Board that a *wire could be made red-hot by electricity*, and if then brought into contact with *gunpowder would ignite it*; together with some other truisms of a similar form. Now, however ignorant Mr. Sturgeon may suppose the officers of the Board to have been, they certainly understood the question much better than he appears to do; they entered very completely into its merits, and left no point unexplored; they required of me information relative to the conducting power of different metals; their respective resistance to fusion by electricity; the ratio in which they became heated, either by the same or by different quantities, the relative quantities required to heat wires of different diameters to the same degree, &c. &c.; for the perfect elucidation of which new experiments and apparatus were invented, and the results exhibited in a way not before done.

Among other points to which the Board directed my attention, was the electrical effect of the incorporation of the conductor with the mast and hull, and the certainty of its confining the course of the discharge to certain lines and to the surface of the masts without being productive of any lateral effect upon the masses of the metal, such as iron hoops, bolts, &c. which either entered into the body of the mast, or were otherwise connected with it; points which Mr. Sturgeon, in the happy consciousness of his own superior sagacity, says were either "not known or unaccountably neglected."

My first illustrations were confined to small models about six inches in length, which could be splintered by artificial discharges, and defended by small lines of metallic leaf placed along them, in imitation of the continuous conductor. But being desirous to exemplify how completely the conductor would direct and confine the discharge upon itself, I placed the experiment under new and very delicate circumstances; and as I consider this experiment an important one, as bearing on the theory of lightning conductors, and directly applicable to all the objections made to them on account of lateral discharges, I may, I hope, be excused for giving a particular account of it.

A model of a mast, M, about ten feet in length, was made in parts, and an interrupted line of metal, *a*, *b*, *c*, *d*, placed in the heart of it. Percussion powder, which it is well known inflames with the least spark of electricity, was placed between these interruptions. On its surface M was placed a continuous conductor *e* M *f*, and the interior and exterior lines of metal connected with common points of junction at *a* and *d*. To make the experiment more complete, bands of metallic leaf, &c. were made here and there to surround the mast, together with other metallic bodies, which could enter into the mast itself and touch the metallic line within, as at 1, 2, 3.



An intense electrical accumulation from four jars of five square feet each, highly charged, was now allowed to fall upon a ball at *a*, with a view of discovering (since the electric matter had a choice of two lines) whether it would pass upon the metal within in preference to that without, or upon both, or whether it could in passing down the exterior conductor cause a lateral discharge to enter the mast, or affect the interior by any other lateral result.

Now the extreme facility with which percussion powder becomes inflamed by the most minute spark, rendered it a very severe test of the presence of passing electricity, so that if the least action had taken place upon the metallic bodies adjacent to the conductor, it would have been immediately shown by an explosion within the substance of the model. Almost every one must I think perceive that this was a fair experiment, and as good a one as could have been resorted to under the circumstances of the case. Even Mr. Sturgeon himself must allow that it completely meets the very points he has insisted on, and which he thinks I had so much overlooked. Thus in sect. 202 of his memoir, mistaking the residual electricity of a discharged surface for what he calls a lateral discharge produced by the passing shock, he says, "this kind of lateral discharge will always take place when the vicinal bodies are sufficiently capacious, and *near to the principal conductor which carries the primitive discharge*, or *to any of its metallic appendages*;" and in sects. 198 and 199, he says he can produce lateral discharges *half an inch long*, though the jar be of the capacity of a quart only, &c. &c., and "at a distance of fifty feet from the direct discharge;" and in sect. 200, he admits that a discharge from such a jar would "*imitate a flash of lightning striking a similar conductor on the mast.*"

Here then is an experiment which at once brings to the bar Mr. Sturgeon's own position; and I appeal to any one, if what he has thus advanced has any foundation, whether "an electrical accumulation not merely from a jar of a quart capacity, but from twenty-five square feet of coated glass highly charged, and equivalent to destroy fifteen or twenty feet of small iron wire, should not have ignited the powder and caused the mast to be blown in pieces. Yet such was not the case, and never can be so long as the continuous conductor remains on its surface. When the exterior conductor, however, was removed and a similar charge thrown on the model, then the mast was blown in pieces, proving, that if the discharge had under any form pervaded the interior, this effect would have resulted in the first case.

Now Mr. Sturgeon roundly asserts, that "this experiment happens to have no bearing on the subject *whatever*," notwithstanding that in sect. 200 he actually refers to the passing of electricity over conductors placed in given directions, and says, "that the discharge of a jar of only a quart capacity would imitate a flash of lightning striking a similar conductor on the mast:" this is very one-sided reasoning indeed.

In a subsequent communication he inquires if "I mean to be considered as a philosopher or necromancer by endeavouring to persuade the British Association that blowing asunder two pieces of wood by gunpowder was a true representation of the effects of lightning on a ship's mast."

This may be all very well as the best means Mr. Sturgeon had of parrying the direct bearing of the experiment in question on his own position; he must, however, necessarily feel how immediately it involves all the conditions he has himself pointed out as essential to the exhibition of his imaginary results, by which he pretends to have illustrated and proved the effects of lateral discharges of lightning.

How does it happen, if his views have the slightest foundation, that the heaviest "primitive discharges," to use his own phraseology, can be passed along the exterior conductor of the mast without in any way affecting the detonating powder within, even though connected with the interior metals by short metallic nails? it surely should do so, if what he says in sect. 198 be true, viz. "that by this kind of lateral discharge a dense spark may be produced when the bodies are half an inch apart, though the jar be only of a quart capacity that chemical decompositions are easily performed by it, and every other class of electrical phænomena, exhibited with a sufficiently large apparatus, as decidedly as by the primitive discharge itself." Now the plain truth is, there is no such effect as he describes produced by a passing charge of electricity. It is altogether a fallacy, arising out of his being very ill informed on the subject, and from his having mistaken the common result of the residual accumulation for a lateral explosion, as I think I have fully shown; Cavallo and the old writers have taken great pains to guard the *inexperienced manipulator* against this source of error.

In short, if Mr. Sturgeon will look at Cavallo, Priestley and other writers of above half a century since, he will find his theoretical and experimental researches very *perfectly imitated*; by a most extraordinary coincidence, the experiments are virtually the same; the deductions, however, from them are widely different. With such careless manipulation, and such a great lack of knowledge of his subject, it is not surprising that Mr. Sturgeon should deem others ignorant of it.

These experiments were not the only experiments which were made matters of discussion by the Board. Thus, to show that in the case of the sliding masts being entirely, or partially lowered, that part of the conductor below the caps would be the same in respect of the discharge as if it did not exist, strips of gold-leaf were laid on paper in the same relative positions as the conductors would assume on the masts, and powerful discharges sufficient to disperse the gold passed over them.

These strips were affected in certain parts only, showing how completely the discharge was confined to such parts, and the total absence of all lateral explosion.

Experiments on the expansive effects of electricity on bodies, the fusion and heating of metals, &c. &c. were also entered upon. In short, the series was as complete as could well be desired. The experiments were all original, or otherwise new, of their kind, and many of them embraced points not before considered; such experiments had for the most part never been tried before; and they were considered by Dr. Wollaston and others who investigated them to be of great interest, and to have an important bearing on the question of lightning conductors in ships.

I suppose it will be admitted that Dr. Wollaston had at least as clear a comprehension of this subject as Mr. Sturgeon, and was just as likely to have detected the "necromancy" to which Mr. Sturgeon alludes, had such existed.

This explanation of the practical nature of the experiments, and which, amongst other considerations, led the Navy Board to try my plan for defending ships from lightning in the navy, I have, for the reasons already assigned, thought proper to offer; they at the same time show the scrupulous care which the Board exhibited on the occasion, and how much they were alive to the question.

[To be continued.]

XXXIV. *Remarks on* Prof. Whewell's *Paper on the Mean Level of the Sea. By* RICHARD THOMAS, *Civil Engineer*[*].

I OBSERVE that Mr. Whewell has in the Philosophical Magazine for November honoured with his notice my observations relative to the mean level of the sea, which were inserted in the Magazine for August. I should not have thought it needful to say another word about it, but leave the matter to be determined, as I think it ought to be, by extending the levels westward to the Land's-end, connecting these levels with others which might be taken at convenient places across from the north-west coast to the south-east of Cornwall; but it appears that Mr. Whewell has mistaken the drift of my argument, by which I endeavoured to show, that owing to the tides, the sea at Axmouth might be kept up to a higher mean level than on the coast of Cornwall. I do not draw such inference from the mere narrowing of the English Channel by the projection of Cape la Hogue, but by that projection in combination with the deeply embayed form of the opposite coast of England between the Start and the Bill of Portland, giving such directions to the tide-currents as would continually, both on the flood and ebb, tend to set them over against that part of the coast on which Axmouth is situated, and consequently tend to keep up the level of the water there; for it must be kept in mind that at the time of high water at Axmouth, the tide is strong flood in the Channel, and at low water the ebb tide is strong; and there appears to be strong evidence that the flood sets in the way I state, from the velocity with which it passes along by the back of Portland Island and occasions the " Race " off the Bill.

Mr. Whewell says, " If the surface of low water were a level surface, as Mr. Thomas supposes," &c.; and in reference to a comparison of the tides of Plymouth and Axmouth, that " Mr. Thomas has no authority either from theory or observation for assuming the high waters to coincide," &c. &c. The best answer I can give to these remarks, is a reference to my paper as printed in your Magazine.

However, as Mr. Whewell agrees with me in the propriety

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[*] Communicated by the Author.

of settling the question by extending the levels as above stated, I need not say more, but leave the question to be decided, as I hope it soon will be, by such extension of the levels.

I do not mean to dispute Mr. Whewell's *theory* as to the mean levels of the tides, supposing there were no obstructions to their free action; but I do conceive that it is very possible that circumstances may operate to prevent the mean level of the tides from being at the same elevation at every part of the coast.

My object is to prevent any general conclusion with respect to tide-levels from being acted on until the fact be placed beyond doubt; and whatever may be the result of the inquiry, the levels which may be determined will be available for establishing the elevations of certain fixed points or *bench-marks*, which will always be much more useful for reference than any determination depending on the tides.

Falmouth, Jan. 4, 1841.

XXXV. *Analysis of the Oils of Elemi and Olibanum.*

By John Stenhouse, *Esq.*[*]

MOST of the resins contain, as is well known, volatile oils, to which they owe their peculiar odours. A considerable number of these oils have been carefully examined, but there are several, and among the rest those of elemi and olibanum, which, as far as I am aware, have not previously been subjected to analysis.

I was induced therefore to prepare a quantity of each of them for this purpose.

Elemi Oil.—Preparation. A quantity of pulverized elemi resin was distilled with water in the usual way. The larger portion of the oil came over at an early period of the distillation, and collected on the surface of the water in the receiver, from which it was easily separated by means of a sucker. It was then allowed to remain some days over fused chloride of calcium to render it anhydrous, and afterwards rectified. It is stated in most systems of chemistry, on the authority of Bonastre, that elemi resin yields $12\frac{1}{2}$ per cent. of volatile oil, but though I operated on an apparently fresh and unadulterated specimen, I did not succeed in obtaining more than $3\frac{1}{2}$ per cent.

Properties.—Elemi oil is a transparent colourless liquid of an agreeable smell, similar to that of the resin, and of a pungent taste. Its specific gravity is 0·852 at +24° C. It boils at 140° C. It burns with a bright smoky flame, like oil of

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[*] Communicated by the Author.

turpentine. It does not dissolve in water. It is very little soluble in weak spirits, but it dissolves very rapidly in alcohol and æther. Potassium remains unaltered in it, but solid potash, with the assistance of heat, converts it into a brown resin. Iodine acts on it with energy; considerable heat is evolved; part of the iodine is driven off into vapour, and the oil is converted into a thick red magma. Elemi oil absorbs muriatic acid gas very readily, and becomes of a dark-brown colour, but I did not succeed in obtaining an artificial camphor. Nitric acid in the cold quickly changes it to a brownish yellow; when assisted by heat it explodes, evolving deutoxide of azote, and converting the oil into a resinous matter. Sulphuric acid in the cold gives it a fine red colour, but when heated it chars it.

The following are the results of its analysis:

I. 0·4305 gave 1·369 carbonic acid, and 0·453 water.
II. 0·197 gave 0·625 carbonic acid, and 0·208 water.

	Found.		Calculated.
	I.	II.	
Carbon......	87·93	87·72	88·46 = 5 at.
Hydrogen...	11·69	11·73	11·54 = 8 —
	99·62	99·45	100·00

From this it is evident, that elemi oil has a similar constitution with oil of turpentine, oil of lemons, copaiba balsam oil, and several others, too numerous to be mentioned.

Oil of Olibanum.—Olibanum, the frankincense of the ancients, yields an oil which is prepared in the same way as that of elemi. The quantity I could obtain was 4 per cent. of the resin employed; it is a transparent, colourless and very limpid oil. Its smell resembles that of oil of turpentine, but it is much more agreeable. Its specific gravity is 0·886 at + 24° C., and its boiling point is 162°. It burns with a strong bright flame, and emits much smoke. The action of reagents upon it is almost identical with that of the oil of elemi, so that it is unnecessary to enter into details. The following are the results of its analysis:

I. 0·3255 gave 1·0015 carbonic acid, and 0·330 water.
II. 0·3465 gave 1·061 carbonic acid, and 0·351 water,
III. 0·302 gave 0·931 carbonic acid, and 0·307 water.

	Found.			Calculated.
	I.	II.	III.	
Carbon ...	85·07	84·66	85·23	85·61
Hydrogen	11·26	11·25	11·29	11·18
Oxygen...	3·67	4·09	3·48	3·23
	100·00	100·00	100·00	100·00

This gives the formula

Carbon	35 =	2675·225
Hydrogen	56 =	349·426
Oxygen	1 =	100·000
		3124·651.

It is somewhat singular that this is precisely the composition which Dr. Kane found for the oil of *Mentha viridis.*

XXXVI. *Chemical Examination of Palm Oil and Cacao Butter.* *By* JOHN STENHOUSE, *Esq.**

Palm Oil.

THIS oil has of late years become rather an important article of commerce, from the great extent to which it is employed in the manufacture of soap. It is chiefly imported from the coast of Africa, and is derived, according to some botanists, from the *Cocus Butyracea*, according to others from the *Avoira Elais*. It is extracted by boiling the bruised fruit in water; the oil collects as a cake on the surface, from which it is easily removed. It has a butyraceous consistence, is of a reddish yellow colour, and has an agreeable aromatic odour. When long kept it becomes readily rancid, and whitens at the same time, especially if exposed to the influence of the light and air. Soap made with the unbleached oil is of a yellow colour, but white soap may be made from it if the oil has been previously bleached. This may be done in various ways; by melting the oil in hot water and treating it with peroxide of manganese and sulphuric acid, or by keeping it melted, spread out in thin layers on iron plates. It has also been proposed to bleach it with hypochlorite of lime and dilute sulphuric acid; and bichromate of potash and muriatic acid have more recently been employed for the same purpose.

It was first observed by Zier, and the observation has been since confirmed by Messrs. Pelouze and Boudet, that palm oil is strongly acid, and that its acidity increases with the age of the oil. The quantity of free acid it contains is very great, often amounting to nearly a third of its weight. It is owing to this circumstance that palm oil is so remarkably easily saponifiable. Indeed, a very tolerable soap may be made from it by boiling it for a short time with the alkaline carbonates. Soap made from palm oil and soda is of a firm consistence, has an aromatic odour, and forms an excellent and agreeable detergent. The

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* Communicated by the Author.

greater portion of the common brown soap made in Great Britain contains no tallow at all, but consists of a mixture of palm oil and common resin. The melting point of the specimen of palm oil I examined was 99° Fahr. It appeared to be pretty old. Palm oil was formerly supposed to contain margaric and oleic acids; but Mr. Fremy has lately subjected it to examination, and found it to contain a new acid, to which he has given the name of the palmitic. I am happy in being able to confirm the truth of his observations.

The mode of proceeding to examine the constituents of palm oil is to saponify it with caustic potash or soda, and then to decompose the soap with muriatic or tartaric acid. The mixture of palmitic and oleic acids thus obtained, is to be dissolved in boiling spirits of wine, in which it is very soluble, and allowed to crystallize. The crystals are to be collected and strongly pressed between folds of blotting paper, and again dissolved in alcohol. These operations are to be repeated eight or nine times till the palmitic acid crystallizes quite free from the oleic acid, which remains dissolved in the mother liquors. Before being subjected to analysis it was once more saponified, and the soap decomposed by muriatic acid. The palmitic acid had then a melting point of 140° Fahr. The following are the results of its analyses.

	gramme.						
I.	0·3025	gave	0·827	carbonic acid,	and	0·338	water.
II.	0·3147	,,	0·860	,,	,,	0·3545	,,
III.	0·2950	,,	0·8076	,,	,,	0·3305	,,
IV.	0·2662	,,	0·7262	,,	,,	0·3007	,,

This is per cent.

	I.	II.	III.	IV.
Carbon ...	75·48	75·56	75·69	75·46
Hydrogen	12·41	12·51	12·48	12·51
Oxygen...	12·11	11·93	11·83	11·83
	100·00	100·00	100·00	100·00

In order to determine the atomic weight of the acid the silver salt was prepared by precipitating an alcoholic solution of the soda soap with nitrate of silver.

	gramme.					Oxide of silver.	
I.	0·4992	salt	gave	0·1456	silver	= 31·31	p. cent. oxide.
II.	0·8204	,,	,,	0·2414	,,	= 31·59	,,
III.	0·6228	,,	,,	0·183	,,	= 31·55	,,
IV.	0·5375	,,	,,	0·1585	,,	= 31·43	,,
V.	0·5385	,,	,,	0·1575	,,	= 31·41	,,

Combustion with oxide of copper gave the following results:

I. 0·3205 salt gave 0·621 carbonic acid and 0·249 water.
II. 0·3305 „ „ 0·6395 „ „ 0·257 „
III. 0·2885 „ „ 0·559 „ „ 0·222 „

	I.	II.	III.
Carbon …………	53·57	53·50	53·58
Hydrogen………	8·63	8·64	8·54
Oxygen…………	6·35	6·41	6·43
Oxide of silver…	31·45	31·45	31·45
	100·00	100·00	100·00

The analyses of the hydrate agree best with the following formula:

			Per cent.
66	atoms of Carbon ……	5044	75·67
128	„ Hydrogen ‥	824	12·33
8	„ Oxygen ……	800	12·00
		6668	100·00

From the determination of the quantity of oxide of silver in the silver salt, however, I am disposed to prefer Mr. Fremy's formula for the hydrate as the more probable. It is

			Per cent.
32	atoms of Carbon ……	2446	75·37
64	„ Hydrogen…	399	12·40
4	„ Oxygen……	400	12·23
		3245	100·00

The formula $C_{32} H_{62} O_3 + Ag O$ gives the following numbers for the silver salt:

			Per cent.
32	atoms of Carbon………	2446	53·35
62	„ Hydrogen……	387	8·44
3	„ Oxygen………	300	6·54
1	„ Oxide of silver	1452	31·67
		4585	100·00

The hypothetical composition of the anhydrous acid is consequently as follows:

			Per cent.
32	atoms of Carbon……	2446	78·08
62	„ Hydrogen‥	387	12·35
3	„ Oxygen ….	300	9·57
		3133	100·00

The number 3133 agrees very nearly with the mean of five determinations of the silver salt, which gave 3165 for the atomic weight of the anhydrous acid. The baryta salt was prepared in the usual way by precipitating an alcoholic solution of the soda soap with chloride of barium.

			Per cent. baryta.
I.	0·5080 salt gave	0·150 carb. of baryta	= 22·90
II.	0·4947 ,, ,,	0·1462 ,, ,,	= 22·92

The calculated quantity is 23·39 per cent. baryta. The atomic weight 3221, which this gives for the anhydrous acid, does not differ essentially from that found by the silver salt.

The following is the mode of preparing Palmitin, the compound of palmitic acid and glycerine. Palm oil is to be subjected to pressure between folds of linen, to separate the more fluid portion. The solid part is to be treated six or seven times with boiling spirits of wine to remove the palmitic and oleic acids, which are very soluble in that liquid. Palmitin, which, on the contrary, is nearly insoluble in hot spirits of wine, is to be then dissolved in hot æther and filtered, to separate it from the impurities which accompany it, and which remain on the filter. On the cooling of the solution the palmitin is deposited in very small crystals. These are to be pressed between folds of blotting-paper and recrystallized out of æther. These operations are to be repeated six or seven times till the palmitin is free from every trace of olein. It is then to be kept melted on the water-bath till the æther is driven off. Pure palmitin melts at 120° Fahr., and when saponified yields palmitic acid with the ordinary melting point of 140° Fahr. Palmitin is quite neutral; on being melted and not allowed to cool it does not crystallize, but forms a half transparent mass like white wax, but which differs from wax in being easily reducible to powder. It is almost insoluble in ordinary spirits; but absolute alcohol dissolves a little of it at a boiling temperature, which precipitates in white flocks on the cooling of the liquid. It is soluble in æther in every proportion. In its appearance and properties generally it very much resembles stearin; palm oil contains only a few parts per cent. of palmitin.

The following are the results of its analysis.

I.	0·3235 gave	0·896 carbonic acid, and	0·3485 water.
II.	0·3384 ,,	0·9397 ,, ,,	0·3744 ,,
III.	0·3317 ,,	0·9195 ,, ,,	0·3665 ,,

or per cent.

	I.	II.	III.
Carbon	76·58	76·78	76·65
Hydrogen	11·99	12·29	12·27
Oxygen	11·43	10·93	11·08
	100·00	100·00	100·00

The calculated numbers are,

35 atoms of	Carbon	2675	76·73
66 „	Hydrogen ...	412	11·80
4 „	Oxygen	400	11·47
		3487	100·00

and consequently palmitin must be regarded as consisting of

1 atom of Palmitic Acid = $C_{32} H_{62} O_3$
1 „ Glycerine = $C_3 H_4 O$
1 atom of Palmitin = $C_{35} H_{66} O_4$

It is evident that this gives a very different formula for glycerine from the ordinary one, $C_6 H_{14} O_5$. This, however, gives a much simpler view of its constitution, proving it to belong to the class of simple organic oxides, like those of ethule and methule. This formula also affords a very simple explanation of the decomposition which glycerine undergoes when treated with peroxide of manganese and sulphuric acid. It is then, as is well known, converted into formic and carbonic acids, as under:

$$C_3 H_4 O + O_5 = C_2 H_2 O_3 + H_2 O + C O_2.$$

Palmitin when distilled gave acrolein, but no sebacic acid; palm oil, on the contrary, when subjected to distillation, yielded sebacic acid in abundance. This shows that the other acid which palm oil contains is the oleic, as it is the only fat acid known to yield sebacic acid by distillation.

Messrs. Pelouze and Boudet discovered that palm oil contains free glycerine, which can be obtained from it by treating the oil with hot water and filtering. I have also succeeded in obtaining it by this process. The acid which accompanied it was neutralized with carbonate of soda, and the glycerine extracted by alcohol. Its quantity was very inconsiderable. The presence of free glycerine in palm oil is what might have been expected from its containing so large a quantity of free palmitic and oleic acids.

Palm oil also contains small quantities of a blueish-green colouring matter. It is heavier than the oil, and forms a thinnish layer, which adheres to the lower part of the cake which palm oil forms when it is melted in water and allowed to cool; I thought at first it was owing to the palm oil having been prepared in copper vessels, but on examination I found that it was wholly of a vegetable nature. Hydrosulphate of ammonia had no effect upon it; caustic alkalies rendered it colourless; muriatic and nitric acid had a similar effect, but sulphuric acid blackened and apparently charred it.

Butter of Cacao.—Cacao butter is obtained from chocolate beans, the fruit of the *Theombra Cacao*; either by subjecting the bruised beans to pressure between hot iron plates, or by boiling them in water and scumming off the oil as it collects on the surface. One pound of beans usually yields from three to four ounces of the oil. When first prepared it has usually a yellowish colour, but this can be easily removed by boiling it with water, or still more completely by treating it with hot alcohol. The taste of cacao butter is mild and agreeable; and its smell, which resembles that of the beans, is owing to the presence of an oily matter, which may be easily removed by one or two digestions with alcohol. With caustic soda cacao butter forms an excellent soap; but it is by no means very easily saponified. The melting point of cacao butter is stated by most chemical authors at 122° Fahr. I found that of the specimen I examined only 86° Fahr. MM. Pelouze and Boudet state it at 85° Fahr. Cacao butter is remarkable for the length of time it may be kept without becoming rancid.

In order to subject the acids which cacao butter contains to examination, it was saponified by caustic potash and the soap decomposed by muriatic acid. The melting point of the mixed acid was 125° Fahr.; they were then dissolved in hot spirits of wine and allowed to crystallize, when the crystals were collected and carefully dried by pressure. These solutions and crystallizations were repeated nine or ten times to free them from the less crystallizable acids, which remained dissolved in the alcoholic mother liquors. The crystals had then a melting point of 157° Fahr., and on analysis they proved to be stearic acid. The following are the results.

I.	0·318	gave	0·8835	carbonic acid,	and	0·3622 water.
II.	0·2595	,,	0·719	,,	,,	0·300 ,,
III.	0·299	,,	0·832	,,	,,	0·347 ,,

or per cent.

	I.	II.	III.
Carbon	76·82	76·61	76·85
Hydrogen ...	12·65	12·84	12·86
Oxygen	10·52	10·55	10·29
	100·00	100·00	100·00

This gives the following formula for the hydrous acid:

				Per cent.
68	atoms of	Carbon	5197·6	77·04
132	,,	Hydrogen...	848·6	12·58
7	,,	Oxygen......	700·0	10·38
			6746·2	100·00

In order to determine the atomic weight of the acid the æther was prepared in the usual way, by dissolving a portion of stearic acid in alcohol and saturating it with muriatic acid gas.

The æther is a semi-transparent crystalline body, very like stearin. It was repeatedly washed with hot water to free it from muriatic acid. It was then carefully dried and subjected to analysis; the following are the results.

I. 0·2715 gave 0·7595 carbonic acid, and 0·3155 water.
II. 0·240 „ 0·671 „ „ 0·2805 „

or per cent.

	I.	II.
Carbon …………	77·35	77·30
Hydrogen………	12·91	12·94
Oxygen…………	9·79	9·76
	100·00	100·00

The formula deducible from these analyses, $C_{72}\ H_{144}\ O_7$, is equivalent to 1 atom acid, 1 atom æther, and 1 atom water.

			Per cent.
72 atoms	of Carbon …	5503·3	77·49
144 „	Hydrogen	898·5	12·62
7 „	Oxygen…	700·0	9·86
		7101·8	100·00

When one atom of æther and one of water are subtracted from the above,

$$\begin{array}{r} C_{72}\ H_{144}\ O_7 \\ -C_4\ \ H_{12}\ \ O_2 \\ \hline C_{68}\ H_{132}\ O_5 \end{array}$$

we obtain the numbers $C_{68}\ H_{132}\ O_5$ as the formula of anhydrous stearic acid:

			Per cent.
68 atoms of	Carbon ………	5197·7	79·70
132 „	Hydrogen……	823·6	12·58
5 „	Oxygen ……	500·0	10·38
			100·00

When cacao butter is distilled it yields no appreciable quantity of sebacic acid, but when the uncrystallizable acid which remains in the alcohol out of which the stearic acid has crystallized is subjected to distillation, traces of sebacic acid are readily obtained. This indicates that the quantity of oleic acid contained in cacao butter is by no means very considerable.

I have every reason to believe that besides the stearic and oleic acids, cacao butter contains a third acid, which has a melting point of about 140° Fahr., and which may probably be the margaric.? Its nature, however, must be the subject of some future investigation.

XXXVII. *Examination of a Fourth Experiment adduced by* Professor Faraday *in support of* M. de la Rive's *Theory, and regarded by* Dr. Fusinieri *to be demonstrative. By Dr.* Stephen Marianini, *Acting Professor of Particular and Experimental Physics in the University of Modena, &c. &c.*[*]

> "........ senza il corredo dell'esperienza non siamo sicuri che la natura sia con noi."—*Romagnosi, Dell'indole e dei fattori dell'incivilmente*, p. 3.

WHEN in the summer of 1835 I undertook the researches which form the subject of the preceding memoir 4th[†] upon the theory of electromotors, I intended to treat of only two of the experiments adduced by Professor Faraday in support of the theory of M. de la Rive; namely, of the electric currents obtained by the immersion of platina and zinc in sulphuric acid, not placed in contact with each other in those parts not immersed in the liquid; and of the spark obtained by bringing the copper wire attached to a large copper plate immersed in acid, in contact with the zinc wire attached to a zinc plate immersed in the same liquid. The third fact brought forward by Professor Faraday is the positive electrization of copper, when, joined with iron, it is immersed in a solution of sulphuret of potassa. I did not wish to speak of this as being at all analogous to the other facts already adduced by M. de la Rive, and which I had already shown in no way favoured the new theory. But having seen that Dr. Fusinieri laid great stress upon this same fact, I began to examine it, and with the assistance of clear and easy experiments, I think I have shown that this also was not more favourable than the others to the theory of the learned Genevan.

I did not write of the fourth experiment pointed out by Professor Faraday, because, not only did it appear to me less conclusive than the others, but I also thought that it must appear so to whoever is acquainted with what I have already published on electromotors. I now see that I partly deceived myself, for Dr. Fusinieri in the discussion into which he enters in some pages of the *Annale delle Scienze del Regno Lombardo-Veneto*, relative to my aforesaid memoir, attaching not a little import-

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[*] From the author's *Memoria de' Fisica Sperimentale scritta dopo il* 1836. *Anno secundo*, 1838. Modena, 1838, 8vo.

[†] The memoir here mentioned is inserted in volume 21 of the Memoirs of the Italian Society of Sciences of Modena. Of the other three upon the same subject, the first, read at the Athenæum at Venice on the 22nd of May, 1828, was afterwards published in the 20th volume of those Memoirs; the second was printed in Venice by Alvisopoli in 1830, and in the 45th volume of the *Annales de Chimie et de Physique*; and the third, in the first *bimestre* for 1836 of the Scientific Annals of the Lombardo-Venetian Kingdom.

ance to this experiment, declaring it of itself demonstrative. And as after such a judgement it may be believed by some that I am of the same opinion, and that for this reason I have been silent on the subject, I think it incumbent on me to discuss it in this article, which may therefore be considered as a continuation of the foregoing memoir 4th.

The experiment now under consideration is that in which, by agitating with a feather the liquid in which are immersed the zinc and the copper of a voltaic pair, the electric current is strengthened. And to explain such increase of strength, Professor Faraday observes that the neutralized acid stratum adhering to the zinc is removed, the acid stratum in contact with it is renewed, and thence the chemical action acquires new force.

By stirring with a feather the liquid in which the voltaic pair is immersed, the neutralized acid stratum adhering to the zinc is removed when the electromotor has been a short time in action; but when the electric current operates for some time, half an hour, for example, the greater part of the neutralized acid stratum remains still adhering to the zinc, notwithstanding the agitation of the liquid. I will suppose, then, that an experiment is here treated of in which the circuit is completed only for a few minutes, in which case, by this agitation, the neutralized acid stratum covering the zinc is in great part removed.

By means of the above-mentioned operation, if it is performed when the electric circuit is interrupted, the current is considerably strengthened when the circuit is again completed; but whenever the circuit is completed many times, and especially when the liquid is water, acidulated or strongly saline, the liquid may be agitated with a feather as much as may be desired, since the electric current does not acquire more strength, as is shown by the constantly decreasing deviation of the galvanometer. And this is very natural, since if on the one hand, the agitation of the liquid removes some of the neutralized acid stratum from the surface of the zinc, the electric current causes it to adhere again, and probably in greater quantity than the agitation of the liquid subtracts. It is otherwise, if, instead of stirring the liquid with a feather, the surface of the zinc is lightly scraped with the angle of a plate of glass; since, in that case, detaching from the metal more neutralized acid than that which the current causes to adhere to it in the same time, the electric current is in a degree visibly strengthened. But here it is said that the current is strengthened by agitating the liquid with a feather; we will therefore suppose that such an operation must be made with the circuit interrupted.

When the force of an electromotor becomes weakened by keeping the circuit complete for some time, we know that by suspending the communication between the poles it recovers by itself its primitive force, either entirely or in part, without the liquid being agitated in the smallest degree*. In order to estimate the true value which in the present question ought to be attached to this experiment, it will be useful to know how much of the strength is owing to the agitation of the liquid, and how much to the repose of the electromotor.

For this purpose I procured forty plates of zinc, and as many of copper; I tried five pairs, one after the other, taken at hazard, and all produced an equal deviation of the galvanometer; all in similar circumstances electrized to an equal tension the electrometer furnished with a condenser. I made these first trials to be certain that the other pairs also would produce the same effects under similar circumstances, and thus to be able to make use of new plates in every experiment, in order the better to obtain the necessary similarity of circumstances.

I have made other preliminary experiments to ascertain what force such pairs would lose when kept in action for a given time, and how much of it they would regain by being left in repose for a like given time. These things being premised, I came to the examination of the experiment.

II. I took therefore a plate of copper and one of zinc connected by means of the wire of the galvanometer, and plunged them two thirds of their height into the acidulated water. The galvanometer deviated quickly full eighty degrees, but after three minutes the deviation was only twenty-one degrees. Having interrupted the circuit, and left it so for twenty minutes without agitating the liquid, the deviation which followed on first re-completing it was thirty-two degrees, and it stopped at last at twenty-one. After three minutes the needle was nearly stationary at nineteen. I interrupted the circuit for twenty seconds, and agitated the liquid, and then completing the circuit, the first deviation was of thirty-eight degrees, and when the needle no longer vibrated it pointed to about twenty-three.

In another experiment I left the circuit closed during an hour and five minutes. The needle of the galvanometer pointed thirteen degrees; I opened the circuit, and after five

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* See Memoir upon the loss of tension, &c. in the *Annales de Chimie et de Physique*, August, 1828.

minutes of repose I closed it; the needle pointed exactly to thirty-five, and stopped at the eleventh degree. The circle being again interrupted, and the liquid stirred with a feather, the needle deviated to thirty-nine, and finally stopped at about thirteen degrees.

From these and other similar experiments I have seen that the agitation of the liquid, after the electromotor has acted for some time, produces an increase of strength in the current independently of the repose. Since in the first, for example, of the above experiments, by the inaction only the pair would have recovered strength to make the needle deviate thirty-two degrees, whilst with the repose and the agitation of the liquid combined the force was such as to make it deviate thirty-eight degrees. Therefore six degrees of deviation above the thirty-two, or the degree of strength expressed by them, was procured by the agitation of the liquid. In the second experiment, then, the force recovered by the agitation of the liquid is expressed by four degrees of deviation above the thirty-five.

III. I convinced myself in another way also of this advantage, which agitating the liquid with a feather confers upon the current, by observing the time required for the needle of the galvanometer to recede a given number of degrees from the point where it had ceased to oscillate; since when the liquid was agitated the time was generally greater than when it was not agitated, and especially if the liquid itself was in any degree a conductor, as for example, spring water, or water rendered slightly saline, which also shows that with the agitation of the liquid we put an obstacle to the cause which tends continually to slacken the current when the circuit is complete.

It is true that this increase of strength caused by agitating the liquid is but trifling when compared with that which is caused by repose. At all events the proposition of Faraday is not the less true, although he may not have remarked that the greater part of the increase of strength is owing to the repose, and the less to the agitation of the liquid: and so much the more founded is the proposition of the learned Englishman, that these two causes of increase of strength resolve themselves into one; since even simple repose causes the voltaic pair to acquire force, probably because the liquid freeing the neutralized acid detaches it from the zinc.

IV. The liquid being agitated, the chemical action acquires new vigour, because a new stratum of acid comes into contact with the zinc. When indeed this experiment is made by immersing the pair in distilled water (in which case the effects are the same, except that they are very slow and small) it is not a new acid stratum that comes to act upon the zinc; yet it will be a stratum of something which will take its place; that is to say, which causes the chemical action exercised by the liquid upon the zinc to take new vigour. I limit myself, therefore, very willingly to the experiment esteemed by Professor Faraday "of itself convincing," in which there is no doubt that when the liquid is agitated in the said circumstances these two facts follow,—the restoration of the chemical action, and the strengthening of the current.

But from both these phænomena arising when the liquid is agitated, does it follow immediately, as a necessary consequence, that the second is occasioned by the first? To admit this is to acknowledge that the chemical theory may be proved independently of this experiment. Then this experiment cannot be proclaimed "of itself convincing," because some other is necessary to prove the truth of the theory of M. de la Rive.

V. If it is not of itself sufficient to demonstrate the truth of the said theory, let us see if at least the aforesaid experiment may serve to confirm it, on the hypothesis that it may already be otherwise proved.

The Delarivian theory being therefore supposed true, that is, it being supposed that the current in the voltaic pair becomes stronger, by the chemical action exercised by the liquid upon the zinc, than upon the copper; it is easily understood, it is true, how by renewing the acid stratum upon the surface of the zinc, the current may be rendered more powerful. But together with the zinc, the copper also is immersed in this liquid, and this metal is attacked itself also by the liquid; and if proofs of this were wanted, the same chemical theory would immediately furnish them; because when copper is joined with gold, platina, charcoal, it becomes more highly electrical. Then by stirring that liquid the chemical action upon the copper is also increased, and this, if the theory of M. de la Rive be just, must be to the injury of the current.

Is it not possible that the trifling increase of power observed on agitation, depends on the chemical action on the copper being also increased in power, in consequence of which the effect that is apparent is only that which is due to the excess of the increase of action on the zinc above that of the increase of action on the copper? Had this been the case it would have been very easy to see it in the experiment I am about to relate.

VI. Instead of placing the two plates of the pair in the same cup, I put the copper into one and the zinc into another;

in each cup was contained the usual acidulated water, and in each was immersed one end of the galvanometrical wire, which was of copper. The circuit being complete, making the two plates to communicate with each other metallically, after the needle was quiet (it pointed to twenty-five degrees) I stirred the liquid in the cup where the zinc was, but I did not perceive any increase in the deviation. I interrupted the circuit, and agitated the liquid, but after it was closed the needle showed no greater deviation, or at least the increase was not perceptible. Then at least the increase of power was less than when the liquid was agitated whilst the two plates were in the same cup; and still, according to the theory of M. de la Rive, in this last case, the increase of power ought, on the contrary, to be greater, because the chemical action upon the zinc was increased, while that on the copper remained unaltered.

But again, if the experiment is repeated, leaving the liquid in which the zinc is immersed always quiet and agitating that in which the copper is immersed, almost the same increase in the deviation is observed which was obtained in the experiments in which the plates were in the same cup; yet according to the new theory, by strengthening the chemical action upon the copper and not upon the zinc, the copper ought to become less negative, and thence the current become weakened instead of strengthened; therefore, according to the results of the examination hitherto made, the experiment treated of contradicts the chemical theory, instead of confirming it.

VII. Shall, then, this little increase in the force of the current which follows upon the agitation of the liquid, or upon such a cleaning (*pulimento*, Fr. *décapé*) of the plates, remain without explanation? I believe it will, so long as no other effect is required to take place from that agitation than the increase of the chemical action of the liquid on the metal.

Since by this action either electricity is not developed, or even if it is developed it is not that which circulates in the electromotors, at least in ordinary cases, as I have shown, with many experiments in my second memoir upon this subject *: thus the strengthening of this action on either metal causes, of itself, no change in the current, as the preceding experiments may show. But if from this agitation of the liquid, besides the strengthened chemical action of the liquid itself upon the metals, we see result also an increase in the con-

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* Memoir upon the chemical theory of Voltaic Electromotors, simple and compound. Venice, 1830.—*Annales de Chimie et de Physique*, September and October, 1830.

ductibility of the apparatus, an increase observed and noted by M. de la Rive, and by myself many years ago*, the explanation of the phænomenon will become easy and natural; and whoever wishes to see this verified has only to repeat the experiment, which I now proceed to describe.

Having plunged a voltaic pair into two cups of acidulated water, as in the experiment of the preceding paragraph, I put the liquid of the cup in which the copper was plunged in communication with a third cup, containing some of the same liquid, by means of an arc formed by a plate of copper; and in this cup was plunged another plate of copper, which formed one of the extremities of the galvanometric wire; whilst the other was dipped, by means of a plate of copper, into the cup containing the zinc plate of the pair. All being thus arranged, I closed the circuit, and the needle deviated nearly to the fortieth degree, and then stopped at the eighth; and after a minute, I interrupted the circuit and held it open for half a minute; after which time the needle was stationary at zero, and having completed the circuit again the needle deviated quickly to thirty degrees, and then stopped again at the eighth degree. Hitherto the liquid was not agitated; but a minute after the needle became stationary, I opened the circuit, I stirred the liquid in the third additional cup, that namely, in which was plunged one end of the arc of copper joined to the pair, and one plate of copper attached to one end of the wire of the galvanometer, and after half a minute, the circuit being completed, the needle deviated to the thirty-fifth degree, and stopped afterwards upon the ninth.

If instead of stirring the liquid the plates immersed in it are slightly touched with a feather, the increase of strength is observed without interrupting the circuit. In one of the experiments made in this manner, the needle was stationary at the sixth degree, and after brushing the plate of copper annexed to one end of the galvanometric wire and immersed in the third cup, the needle moved to nine degrees, whence it turned quickly to six. The same thing happened upon brushing gently the plate which served as an arc in the part immersed in the cup containing the zinc plate of the pair.

Now in such experiments those plates of copper served only to convey the electricity put in motion by the voltaic pair; then the agitation of the liquid or that light friction upon the plates of copper, strengthens the current in proportion as the power of conducting or transmitting the electricity is strengthened in the apparatus.

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* Memoir upon the loss of tension which electromotors undergo, &c. Section 15.—*Annales de Chimie et de Physique*, August, 1828.

VIII. We may conclude from what has been said, that the fourth experiment of Professor Faraday, which, according to Signor Fusinieri, is itself demonstrative of the truth of M. de la Rive's theory, is at least inconclusive.

And here it may be asked, whence then comes that alteration in the conductibility of the apparatus? And M. de la Rive will reply that the agitation of the liquid or the cleansing of the plates increases the transmissibility for the electricity of the same; and I shall add that the alteration in the relative electromotive faculty, or, which is the same thing, in the electrotism of the plates, also concurs in this. By the current, indeed, the electrotism of those plates in which the electricity enters, becomes positive, and those from which it proceeds to pass into the liquid negative, and that alteration proceeds from the matter which the current collects upon the plates themselves. Now by the agitation of the liquid, and brushing the plates gently with a feather, all or a part of this matter which alters the electrotism of them, and hence necessarily strengthens the voltaic current, is removed. I was convinced that this was connected with the phænomenon, by observing that in the second experiment of the preceding paragraph, if any one of the plates in which the electrotism becomes negative is brushed with a feather, there is an increase of the current much greater than when one of those in which the electrotism become positive is brushed; and we know that the changes in electrotism to positive are, *cæteris paribus*, more trifling than the changes to negative*.

IX. The above remarks relative to the experiments in question are those which are most obvious, and which suffice to show the little or no importance which it has relatively to the theory of the electromotors. It may, however, assist those who desire to repeat that experiment and to study it, briefly to bring under their notice some other phænomena which may present themselves, and which do not escape the attentive observer.

Sometimes the feather is scarcely introduced into the liquid when the force of the current is increased, and at other times it is lowered. At times the said force decreases when the feather is scarcely withdrawn from the liquid, at others it increases.

In two cases the force of the current is seen to increase at the moment when the feather is introduced into the trough or cup in which are plunged the plates of the voltaic pair.

1st. When the vessel being small, the level of the liquid increases by the immersion of the feather, and this is not in-

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* See the Memoir upon the alterations produced by the voltaic currents in the electrotism of the principal metals, 1837, p. 140.

troduced between the two plates. In order that such increase of level might not be sensible, I made use for the experiments above described, of rather large cups, so that the two plates of the pair remained distant from each other four centimetres.

2ndly. In the second place the force of the current is increased on the first insertion of the feather, when this being wet, one of the plates is touched with it; because it is as if of these plates another part was immersed in the liquid besides that which is already immersed; and in this case if it is the plate of copper which is touched by the wet feather, the increase of force is much greater than when the plate of zinc is touched; and this is very intelligible after what we know relative to the voltaic pair with unequal plates*.

The deviation of the galvanometer which indicates the force of the current, decreases in some degree when the feather is introduced between the two plates, and parallel to them, or but little inclined; for in this case the feather intercepts part of the current, or, to speak more correctly, obliges part of the current to run through a greater distance before arriving at the other plate.

Whenever the feather is in the liquid, but not between the plates, or if it is between them, remains there so as not to intercept the current in any great measure; in this case, the feather being withdrawn, the current itself slowly diminishes in some little degree.

Finally, when the feather has been for some minutes immersed between the plates, and so as to cause the deviation of the galvanometer to diminish in some degree, and has been left there without agitating the liquid, upon withdrawing the feather, or even only drawing it aside, the deviation is immediately seen to increase an equal number of degrees, and the needle to stop above that to which it pointed before the feather was placed between the plates.

In one of such experiments, in which the liquid conductor was water very slightly saline, the circuit was scarcely closed, when the needle deviated eighty degrees; after an hour the needle was for several minutes stationary at twenty-eight; the feather being introduced between the plates, the needle itself moved to twenty-five. After a space of three minutes I removed the feather, and the needle oscillated to thirty-three and then stopped at thirty. This is an experiment at first sight very singular, and for which it would be difficult to assign a reason, unless it be remembered that when an electromotor has lost force by the circuit remaining complete for some time,

* Essay before quoted, §§. 129 and following.—*Annalen, &c.*, Oct. 1826.

not only does it regain its strength when the circuit is interrupted, but it recovers a part of it also, when instead of interrupting the circuit the current itself is only slackened*.

[To be continued.]

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

To Richard Taylor, Esq.

Dear Sir,

We have the honour to forward to you for insertion in your valuable Journal, the accompanying notices of the researches of Continental chemists. We trust that, however brief and imperfect, they may prove acceptable to English readers, as but little of what is done abroad, especially in Germany, seems to find its way into England, or, at least, until after the lapse of some years. As instances of this, we need only refer to some papers by English chemists recently published in the Philosophical Magazine, which in a great measure have been but repetitions of what has been done long since on the Continent. Thus Professor Apjohn would have been spared a great deal of labour had he been earlier acquainted with the works of Liebig and Pelouze, and, still more, with those of Mulder. Professor Johnston might have been enabled to shorten his experiments on the Benzoe resins, and probably would have modified his results, if he had known of the researches of Van der Vloet on the same subject, with whose formulæ some of Professor Johnston's results agree. Our next communication shall furnish some comparison of these two excellent investigations. The double oxalates, or at least several of them, which have been so well described by Professor Graham in his celebrated paper on the constitution of the Melates, &c. had been described already by Professor Mitscherlich. The fact that aldehyd is contained in nitrous æther, as lately stated by Dr. Golding Bird, was known years before in Germany from the labours of Heinrich Rose.

It is, however, needless to multiply instances: our object is to assist in preventing their occurrence, and to save English chemists some little unnecessary trouble. It would, it is true, be better to give full translations of the papers, but our own studies do not allow us time for this:—the extracts, which are made in our few leisure hours, are intended to give an

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* See §. 11 of the Memoir already cited, upon the loss of tension which electromotors undergo when the circle remains closed, and upon the regaining of the primitive tension when the communication between the poles is suspended.

idea, not of what has been done, but merely of what is actually going on in the Continental chemical world.

Should our attempt meet your approval and that of your readers, we shall with pleasure continue our communication monthly. We have the honour to remain,

Dear Sir, yours truly,
William Francis.
Henry Croft.

Berlin, Dec. 13, 1840.

P.S. The Journals consulted are Poggendorff's *Annalen der Physik und Chemie*; Liebig's *Annalen der Chemie und Pharmacie*; Erdmann's *Journal für Praktische Chemie*; *Archiv der Pharmacie*; *Annales de Chimie et de Physique*; *Journal de Pharmacie*; Berzelius's *Arsberattelse*, and the German translation by Wöhler.

MM. Dumas and Stas have published a second treatise on chemical types *. The principal subject of this treatise is the action of potassa on the various kinds of alcohol, both those which are evidently similar to the wine alcohol, and also on those substances which have been considered as analogous to alcohol, as for instance, glycerin, &c. In many cases it is possible to convert an alcohol into its equivalent acid; but yet chemists have not been able to convert an acid into its corresponding alcohol. The discovery and examination of these alcohols is much more important than that of any acid; for the acid is only interesting in as far as its combinations with inorganic bases are concerned, while an alcohol is the head of a numerous family. A new alcohol may be compared to a new metal. The formula of the alcohol of any acid is found by substituting H^2 for O^2 in the hydrated acid.

In the experiments on the action of hydrate of potassa, it was found convenient to employ a mixture of equal parts of fused potassa and well-burnt lime. This substance affected the glass vessels much less than potassa alone; it may, for shortness' sake, be called lime-potassa.

If this substance be moistened with alcohol and subjected to a gentle heat, a gas is developed which consists chiefly of hydrogen, and a little carburetted hydrogen and carbonic oxide. The residue contained a quantity of acetic acid, proportional to the quantity of alcohol employed. If the heat is increased, carbonate of potassa remains behind, and marsh gas (light carburetted hydrogen) is given off; for under the influence of alkalies, acetic acid is decomposed into carbonic acid and marsh gas, $C^4 H^4 O^4 = C^2 O^4 + C^2 H^4$. In the usual preparation of acetic acid, two atoms of hydrogen are

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* *Annalen der Chemie und Pharmacie*, xxxv. p. 129, from *Annales de Chimie et de Physique*, vol. lxvi. p. 113.

replaced by two atoms of oxygen; by the action of alkalies chloracetic acid $C^4HCl^3O^4$ is decomposed into carbonic acid and chloroform $C^2O^4 + C^2HCl^3$; acetic acid is decomposed into $C^2O^4 + C^2H^4$, or $C^2O^4 + C^2H^3H$. Alcohol may therefore be considered as composed of two bodies representing carbonic acid, one the methylic aldehyd, the other marsh gas, $C^4H^6O^2 = C^2H^3 + C^2H^2O^2 =$[*] $C^2O^4 + C^2O^4$. In the usual method of forming acetic acid $C^2H^2O^2$ loses H^2, and takes up O^2, there then remains $C^2O^2O^2 + C^2H^4 = C^4H^4O^4$, or acetic acid. By the action of alkalies H^4 is given off; the water of the hydrated alkali is decomposed, H^2 is developed and O^2 assimilated. If the hydruret of potassium be treated with dilute sulphuric acid, the hydrogen of the hydruret is developed, and also that of an atom of water, the oxygen of which combines with the potassium to form potassa. So it is with the body $C^2H^2O^2$, $C^2H^2O^2 - H^2 + O^2 = C^2O^4$, we then have $C^2O^4 + C^2H^4 =$ acetic acid, as before. With pyroxylic spirit it is the same; two atoms of water are decomposed, $C^2H^4O^2 - H^2 + O^2$ (from H^2O^2) gives $C^2H^2O^4 + H^4$. In the residue, however, is found a large quantity of oxalic acid. Peligot has shown that by the action of alkalies formic acid is converted into oxalic acid. Hydrogen is thereby developed, $C^2H^2O^4 - H = C^2HO^4$; the oxalic acid in its turn may lose H, and become C^2O^4, or carbonic acid.

One part of ethal treated with 5–6 parts of lime-potassa, gave at 210° to 220° C. a considerable quantity of hydrogen. The residue, treated with hydrochloric acid, gave a flocky precipitate: this was combined by boiling with hydrate of baryta, and the salt thus formed freed from excess of ethal by boiling alcohol, and then decomposed by means of hydrochloric acid. The precipitate is ethalic acid. The mixture for preparing it must not be heated above 220° C., and must be kept at that temperature for five or six hours. Ethalic acid is colourless, tasteless, inodorous, lighter than water, easily fusible, solidifies at 55° C., and forms fine shining needles; insoluble in water, soluble in boiling alcohol and æther; volatile. Formula $C^{32}H^{32}O^4$. The salts, excepting those of the alkalies, are insoluble in water and alcohol. $\dddot{Fe}^2\,\overline{Eth}$ is dark yellow, $\dot{Co}\,\overline{Eth}$ is rose-coloured, $\dot{Cu}\,\overline{Eth}$ light green, $\dot{Ag}\,\overline{Eth}$ easily decomposed. The potash salt is formed by fusing ethalic acid with carbonate of potassa, and extraction with alcohol; it is white, with a pearly lustre, decomposed by excess of water;

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[*] The sign here should represent *equivalency,* but not at all *equality.*—R. K.

insoluble in æther; formula $C^{32} H^{31} O^3 + \dot{K}$. Soda salt crystallizes in large tabular crystals.

One part of potatoe spirit oil treated at 200° with ten parts of lime-potassa, gave hydrogen and valerianic aldehyd; this is further decomposed into valeric acid, $C^{10} H^{12} O^2 - H^2 = C^{10} H^{10} O^2 . \dot{K} - H + O = C^{10} H^9 O^3 . \dot{K}$. The residue is distilled with sulphuric acid, the vapours condensed in a solution of carbonate of soda; out of this the pure valerianic acid may be procured by distillation with phosphoric acid. Specific gravity of the hydrated acid at 16°·5 C. = 0·937; boils at 175°, is not solid at —15°; decomposed by chlorine, not by iodine and bromine; by anhydrous phosphoric acid is decomposed into inflammable gases and valeron, &c. &c. Formula $C^{10} H^{10} O^4 = C^{10} H^9 O^3 + H\,O$; density of vapour = 3·55; found by experiment = 3·67. There is also a hydrate with three atoms of water. Many salts do not crystallize. The silver salt is anhydrous.

Chlorovalerisic acid is formed when dry chlorine is passed into valerianic acid without the aid of sunshine: the reaction is violent. The acid is semifluid, transparent, heavier than water, inodorous, burning taste; at —18° still fluid; decomposed at 100–120°, with development of hydrochloric acid; combines with water; the impure acid must therefore not be washed. Formula $C^{10} H^7 Cl^3 O^4$.

Chlorovalerosic acid. Formed by passing chlorine through valerianic acid in the sunshine: the chlorine is driven out by a stream of carbonic acid. Semifluid, inodorous, burning bitter taste, heavier than water; not solid at —18°; decomposed above 150°. With water forms a hydrate. Aqueous solutions not precipitated by acid nitrate of silver, but the solutions in alcohol and æther are after a short time. Formula $C^{10} H^6 Cl^4 O^4$. Potash salt is similar to the valerianate. The acid is decomposed by an excess of alkali. The silver salt decomposes in the dark; chloride of silver and a peculiar body $C^{10} H^6 Cl^3 O^4$ are formed. Formula of the silver salt $C^{10} H^5 Cl^4 O^3 . \dot{Ag}$. The oily hydrate contains three atoms of water. The boiling point of valerianic acid varies with the quantity of water it contains; all three acids form hydrates with three atoms of water, and are herein similar to the triacetate and triphocenate of lead. By continued distillation of potatoe-oil with nitric acid the valerianic aldehyd is formed, also by acting on it with sulphuric acid and chromate of potassa, or binoxide of manganese. The aldehyd has the formula $C^{10} H^{10} O^2$.

Glycerin treated with lime-potassa in the same manner gave hydrogen, and in the residue formic and acetic acids; a glycerinic acid could not be obtained.

Aceton is decomposed into carbonic acid and marsh gas, as Persoz has already stated.

By passing the vapours of aldehyd over the heated lime-potassa, hydrogen is developed, and the residue contains a large proportion of acetic acid, $C^4 H^4 O^2$ lose H and takes $O = C^4 H^3 O^3$; aldehyd therefore resembles the oil of bitter almonds.

Oxalic æther treated with potash gives hydrogen and acetic and carbonic acids; oxalate of potash and alcohol are first formed, out of these are produced carbonate of potash, acetic acid, and free hydrogen. Acetic æther gives hydrogen and acetic acid; benzoic æther gives hydrogen, benzoic and acetic acids; iodide of ethyl gives iodide of potassium and olefiant gas, $C^4 H^5 I^2 + \dot{K} = C^4 H^4 + \dot{K} I^2 + H O$*. This æther forms with chlorine; chloride of ethyl and iodine is separated, ${}^{4}_{10}E F$ is exactly similar to R F. Chloride of æthyl gives water, olefiant gas, and chloride of potassium; the vapour treated with ammonia gives chloride of ammonium and olefiant gas, $C^4 H^5 Cl^2 + N H^3 = C^4 H^4 + N H^4 Cl$. Common æther gives carbonic acid, marsh gas, and hydrogen; treated with chromate of potash and sulphuric acid it forms acetic acid.

Acetate of mæthyl-oxide gives hydrogen, formic and acetic acids; chloride of mæthyl gives hydrogen, chloride of potassium and formic acid, which is afterwards decomposed, $C^2 H^3 Cl + K O + H^2 O^2 = C^2 H O^3 + KCl + H^4$. Oxide of mæthyl gives hydrogen and formic acid. When the vapour of alcohol is passed over heated baryta, the following decompositions take place: first, hydrate of baryta and olefiant gas are formed; in the second stage, the produced hydrate decomposes the alcohol into acetic acid and hydrogen; and in the third, the acetate of baryta forms carbonate and marsh gas. The gas given off contains olefiant gas, marsh gas, and hydrogen, and perhaps some carbonic oxide.

It appears, therefore, that several alcohols are capable of producing their own peculiar acid. According to the researches of the late M. Delalande, camphor when treated with

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* I do not know whether this method of writing organic bodies is known in England. Berzelius proposes to write the carbon above, the hydrogen below, the nitrogen in the middle; the oxygen to be expressed by dots, as before. Tartaric acid is ${}^{4}_{4}\overset{\cdot\cdot\cdot\cdot\cdot}{T}$, Benzoic ${}^{14}_{10}\overset{\cdot\cdot\cdot}{Bz}$, &c., &c.—H. C.

potash forms a peculiar acid, = camphor + water. $C^{20}H^{16}O^{2}$ forms $C^{20}H^{18}O^{4}$.

A tabular view of several acids, with their hypothetical types, which here follow, we have not thought necessary to add; the above short extract of this very important paper is only an outline, those who wish for more information must refer to the original paper.

The oil obtained by distilling the fresh plants of *Ruta graveolens* is yellowish-green, smells like the plants, tastes bitter and aromatic; sp. gr. = 0·837 at 18° C. If allowed to stand some time over chloride of calcium and then distilled, it boils at 218°, and the boiling point rose to 245°. The sp. gr. of the colourless product varies from 0·831 to 0·838. Rational formula $C^{28}H^{28} + O^{3}$. Specific gravity of the vapour was found to be 7·892, ought to be 7·690; therefore the atom is equal to four volumes. The oil is soluble in sulphuric acid, and is precipitated by water; hydrochloric acid gas has no action; by chlorine and nitric acid it is decomposed. These experiments have been made by Dr. H. Will in Giessen. (*Annalen der Pharmacie*, xxxv. 235.)

Erdmann has examined the action of chlorine and bromine on indigo. The indigo-blue used for these experiments contained a little indigo-red, which is, however, of no consequence. Dumas's formula for indigo-blue is $C^{16}H^{5}NO^{2}$; Erdmann's is $C^{32}H^{10}N^{2}O^{3}$. When chlorine is passed into indigo-blue suspended in water, the blue colour changes to a grayish-green, and then to yellow. Hydrochloric acid remains in solution. It is best to employ a low temperature. When this mass is distilled with water a volatile substance passes over; it is called chlorindopten, or chloride of indopten; it is formed in very minute quantities; it may be purified by repeated distillations with water and sublimation; it is then obtained in the form of fine needles of a peculiar unpleasant smell; fuses to a colourless oil; is, not very volatile; soluble in hot water, but very little in cold; soluble in alcohol. Formula $C^{8}H^{9}Cl^{2}O$. It is, however, a compound of two substances. When heated with caustic potassa or its carbonate the disagreeable smell disappears, a substance passes over similar in appearance to chloride of indopten, but differing from it in not giving a sour solution with water. Erdmann calls it chlorindatmit (!), from *ἀτμός*. It is formed in very small quantities; its formula is $C^{12}H^{4}Cl^{3}O^{2}$. The residue, after distilling off the chlorindatmit, is crystalline, soluble in water, less so in alcohol; it is allowed to stand in the air and then extracted with alcohol. Out of this solution acids precipitate chlorindoptenic acid. This body has a disagreeable smell, behaves similar to chlorindopten, reacts acid, gives with a salt of silver a voluminous yellow precipitate, with lead a white one. Formula of silver salt is $C^{12} H^{2} Cl^{3} . \dot{A}g$; formula of free acid $C^{12} H^{3} Cl^{3} O$.

After the chloride of indopten has been distilled off, the residue is boiled with a large quantity of water, a resinous matter remains behind, and the filtered solution deposits a a red body; on evaporation more is obtained, but less pure. This substance is dissolved in boiling alcohol; on cooling, reddish-yellow or brown crystals are deposited; these are chloride of isatin, with a little bichloride of isatin and resin: by repeated crystallizations it may be obtained pure. It is inodorous, bitter, may be heated to 160° C. without decomposition, above that temperature it is partly decomposed and partly sublimed; 100 parts of alcohol of 0·83 dissolve 0·453 parts of it. It is neutral, not altered by hydrochloric or sulphuric acids; is decomposed by nitric acid. Formula $C^{16} H^{4} N Cl O^{3}$. Treated with caustic potassa it is dissolved with a yellow colour; it takes up an atom of water and forms chlorisatinic acid. The potash salt crystallizes easily, is purified by solution in alcohol, &c.; the acid cannot be separated when precipitated by an acid out of the potash salt; it decomposes and forms chloride of isatin, which body may in this manner be obtained perfectly pure. Potash salt explodes. Formula $C^{16} H^{5} N Cl O^{4} . K$. Silver salt is $C^{16} H^{5} N Cl O^{4} . \dot{A}g$. There are two salts with baryta, one with one atom of water, the other with three. The lead salt is at first yellow and gelatinous, but becomes flocculent and of a splendid red colour. The copper salt is at first yellowish-brown, but becomes granular and blood-red, &c., &c., &c. Bichloride of isatin remains in the alcoholic solution out of which the chloride has been obtained; out of this it may be obtained: of course the latter crystallizations are the freest from the protochloride. These two bodies are extremely similar; the bichloride differs from the other in being more soluble in alcohol, and the bichlorisatinate of potash gives with lead salts a yellow precipitate, which does *not* become red like the chlorisatinate. Bichloride of isatin is, when obtained from its aqueous solution, a yellowish-red granular powder; out of alcohol it crystallizes in red shining needles and tables; sometimes four-sided prisms may be recognised; it may be partly sublimed, but the greater part is decomposed; more soluble in water than the protochloride. 100 parts alcohol of 0·83 dissolve at 14° 3·40 parts. Formula

$C^{16} H^4 N Cl^2 O^3$. Treated with potash it forms a bichlorisatinic acid, which may be precipitated from its solution in potash by means of an acid, in form of a yellow powder; this acid is tolerably soluble in water; it is easily decomposed; its aqueous solution decomposes at 60°, or even in the cold; it contains one atom water of crystallization. (*Journal für Praktische Chemie.* Erdmann and Marchand, xix. 321).

The potash salt crystallizes in needles and leaves. Formula $C^{16} H^5 N Cl^2 O^4 . \dot{K}$; the crystallized salt contains two atoms of water. The baryta salt crystallizes in the same form, and contains two atoms of water. The copper salt is at first of the colour of hydrated oxide of iron, but becomes carmine red and granular; when pressed assumes a golden lustre. The silver salt is partly soluble in water, and may be crystallized; it is anhydrous.

The more of the resinous substance is formed the more chloride of indopten is produced; it is not formed by the action of chlorine on the proto- or bichloride of isatin. The resin examined was not quite pure, for the product formed by the action of chlorine on indigo-red was always mixed with it; it is obtained in small quantities. The formula appears to be $C^{20} H^8 Cl^2 N^{\frac{1}{2}} O^5$, or $C^{40} H^{16} Cl^4 N O^{10}$.

It is probable that the ammonia resin, and chloride of indopten are unimportant and variable products.

By the action of bromine similar substances are produced, bromide of indopten; and by potassa, bromindatmit and bromindoptenic acid. They have apparently the same formula as the chlorine compounds; also bromide of isatin and bromisatinic acid, bibromide of isatin and bibromisatinic acid: they have the same composition, and are in other respects very similar to the chlorine compounds.

In a second paper Professor Erdmann promises to make known the action of certain reagents on the bodies described above.

Formic æther may be prepared, according to Wœhler, by distilling a mixture of ten parts of starch, thirty-seven parts finely powdered binoxide of manganese, thirty parts of sulphuric acid, fifteen parts of water, and fifteen of mere alcohol. In the product of the distillation chloride of calcium is dissolved, and the æther distilled off in a water bath; from the solution of chloride of calcium in the æther, small yellow crystals are sometimes deposited: they have not been further examined. (*Annalen der Chemie und Pharmacie,* xxxv. 238.)

According to Wœhler telluret of ethyl may be formed by distilling baryto-sulphate of ethyl oxide (sulphovinate of baryta) with telluret of sodium; the telluret, as obtained by fusing tellurium or telluret of bismuth with carbonate of soda and charcoal, is thrown into the baryta solution in a solid state. (*Poggendorff's Annalen*, l. 404.)

The æther is a yellowish-red fluid, like bromine, heavier than water, in which it is but little soluble; has a strong disagreeable smell, similar to sulphuret of ethyl and to telluretted hydrogen; appears to be poisonous; boils under 100°; inflammable, burns with a white flame with blue margin, in the air changes into tellurous acid. When acted on by the nitric and hydrochloric acids, an oily body is formed: contains 68·75 per cent. tellurium; it should contain 68·53. Formula $C^4\ H\ \ Te$.

Mylius has found that *Helix pomatia*, *nemoralis* and *hortensis* (but no species of *Limnæa* or *Planorbis*) contain pure uric acid in a free state; it occurs in a glandular organ situated immediately beneath the shell,—which is therefore without doubt the urinary vesicle,—secreted in a solid form, so that it shines through the covering membrane. To obtain it, it is merely necessary to cut open the organ and squeeze out the white paste into a glass. When a sufficient quantity has been collected, it is shaken frequently with water, by which the slime is suspended and can be poured off, while the uric acid is deposited at the bottom of the vessel. This simple and mechanical operation suffices to obtain a perfectly pure product: it has a pulverescent appearance, and contains no crystalline particles. The quantity obtained from each garden snail amounts to about one and three-quarter grains. (*Journal für praktische Chemie*, xx. p. 509.)

Brucin is considered by Dr. Fuss of Schönebeck to be merely a mixture of strychnia with a resin, from which it may be separated by a peculiar method. Erdmann has made some experiments with the products obtained by Dr. Fuss. (*Journal für praktische Chemie*, xix. 510.)

For preparing the chlorate of barytes, Duflos dissolves thirteen and a half parts of chlorate of soda in double the quantity of water, to which he adds a solution of ten parts of tartaric acid in as much water, and pours the whole into a glass containing a double volume of alcohol. It is then left to stand for twenty-four hours, filtered, the acid liquid neutralized with pure carbonate of barytes, which has been previously mixed with water to a milk, the spirit left to evaporate, then filtrated anew and evaporated to crystallization. The precipitate effected by the alcohol is the bitartrate of soda, which may again be employed for preparing chlorate of soda by adding it to a hot solution of sixteen parts of chlorate of potash in four times the quantity of water, leaving the mixture to cool, filtrating and evaporating the solution to forty parts. (*Arch. der Pharm.*, xxiii. p. 306.)

After continued rain, a heap of coals which lay exposed to the weather was observed to smoke, and to be on fire in the interior; when separated it was found in the middle to be in full combustion. In the neighbourhood of this part the coals were half-roasted, forming an almost compact mass, and between them was found a bright yellow, soft, saltlike substance. This proved on examination to be sulphate of magnesia; the ashes of coals contain magnesia, the burning pyrites afforded the sulphuric acid. Dr. Mohr compares it to the formation of cyanide of potassium at the Clyde iron-works*. How the sulphate of magnesia became collected into masses is not so easily explained. (*Annalen der Pharmacie*, xxxv. 239.)

Kersten has employed with advantage a mixture of hydrochloric acid and finely powdered chlorate of potash, added in small portions, as proposed by Berzelius for oxidizing and dissolving metals and metallic sulphurets, even for the analysis of the native metallic sulphurets, the sulphur being quickly and easily converted into sulphuric acid. M. Kersten, moreover, observed that finely powdered iron pyrites was perfectly decomposed by boiling with chlorate of potash, without any addition of acid, the sulphur being converted into sulphuric acid, and the iron remaining undissolved in the form of peroxide. (*Berzelius's Jahresbericht*, xix. p. 288.)

The difficulty of obtaining the hyposulphite of soda in good crystals, according to the usual methods, has induced C. F. Capaun to make known the following modified plan, by which in the shortest time considerable quantities of the salt can be prepared, and of a kind which leaves nothing further to be desired. A solution of caustic soda is boiled with sulphur so long as any of this is dissolved, the liquid poured from the undissolved sulphur put aside to cool, and then a current of sulphurous acid gas passed through it, until a test filtered from the separated sulphur has still a wine-yellow colour—it contains therefore still undecomposed sulphuret of sodium,—and by no means appears colourless. The liquid is now filtered and evaporated in a porcelain basin over a

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[* Described by Dr. Clark in L. & E. Phil. Mag. vol. x. p. 329.—Edit.]

quick fire to the consistency of a syrup. Although during evaporation the atmospheric air is not excluded, there is no fear of the hyposulphite of soda being more highly oxidized, as the oxygen of the atmosphere must first exert its oxidizing action on the sulphuret of sodium. The liquid thus evaporated is filtered, if necessary, and mixed with half its volume of alcohol and well shaken; in a few minutes the liquid separates into two layers; the upper or alcoholic one, which has taken up all the sulphuret, is of a gold-yellow colour, the lower one containing the hyposulphite of soda dissolved in water, colourless. The vessel is now placed aside, that the hyposulphite may crystallize under the covering of the alcoholic solution of the sulphuret, which happens in the course of twelve hours. The crystals obtained, when quantities not too small have been operated with, are of considerable size, and without any yellow tinge; and when dissolved in water and treated with an acid, let fall a considerable quantity of sulphur with disengagement of sulphurous acid. (*Journal für praktische Chemie*, xxi. pp. 310–313.)

Marchand prepares the dioxide of copper in the following manner: very thick wires of pure Russian copper were brought in a Gay-Lussac's furnace immediately to a violent white heat, which having continued half an hour, was diminished to a dark red heat: the oxide was taken out after some hours; it formed black crystalline masses, which were in part hollow, and gave a beautiful purple-red powder. In the greater part there was still metallic wire inside, around which the dioxide had arranged itself in a crystalline state. Another time the dark red heat was maintained during eight or nine hours. Both products were reduced by hydrogen; the first portion contained 88·6 copper and 11·4 oxygen, the second 88·65 Cn. and 11·35 O. (*Journal für praktische Chemie*, xx. p. 505.)

[To be continued.]

XXXIX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xvii. p. 542.]

Feb. 26, 1840. (*Continued.*) A PAPER was then read, "On the characters of the fossil trees lately discovered near Manchester, on the line of the Manchester and Bolton railway; and on the formation of Coal by gradual subsidence;" by John Eddowes Bowman, Esq., F.L.S., communicated by the President.

The paper commences with a few preliminary remarks on the theory of repeated subsidences of the land during the carboniferous æra; and on the drift theory, the author being of opinion that the former receives much support from the phænomena presented by the fossil trees found near Manchester, and that it affords in return great assistance in explaining the peculiarities of their position. Mr. Bowman does not deny that plants may have been carried into the water from neighbouring lands, as in the instances of fern-fronds and other remains scattered through the sandstones and shales; but he conceives it is difficult to understand whence the vast masses of vegetables necessary to form thick seams of coal could have been derived, if drifted; and how they could have been sunk to the bottom, without being intermixed with the earthy sediment which was slowly deposited upon them. He is of opinion also, that without a superincumbent layer of mud or sand, to retain the hydrogen during the process of bituminization, ordinary caking coal could not have been formed. Another difficulty, connected with the drift theory, Mr. Bowman says, is the uniformity of the distribution of the vegetable matter, throughout such great areas as those occupied by the seams of coal, extending in the instance of the lower main seam of the great northern coal field, over at least 200 square miles; and in that of a thin seam below the gannister, or rabbit coal, in a linear direction of thirty-five miles from Whaley Bridge to Blackburn. On the contrary, he believes, that it is much more rational to suppose, that the coal has been formed from plants, which grew on the areas now occupied by the seams,—that each successive race of vegetation was gradually submerged beneath the level of the water, and covered up by sediment, which accumulated till it formed another dry surface for the growth of another series of trees and plants,—and that these submergences and accumulations took place as many times as there are seams of coal. He also explains the thinning out of the seams and other strata of the coal measures, by irregularities in the mode or extent of the depressions.

Mr. Bowman then proceeds to the examination of the phænomena presented by the fossil trees discovered on the line of the Manchester and Bolton railway, and described by Mr. Hawkshaw in his paper read on the 5th of June, 1839, (see L. & E. Phil. Mag. vol. xv. p. 539.) and in the preceding communication (see vol. xvii. p. 541.): it will be necessary to notice therefore only those points which did not claim that gentleman's more particular attention. Mr. Hawkshaw describes generally the markings on the internal casts of the trees; but as it is difficult to convey a correct notion of their waved and anastomosing characters either verbally or by reduced drawings, Mr. Bowman applied paper to the surface of the stems and carefully traced the grooves or furrows by following them exactly with an instrument. The only indications of scars, which he could find after a long and close search, were at one point near the base of the largest tree, and though indistinct, his practised eye recognized them to be those of a Sigillaria. He detected also in some parts, on the ribs of the same tree, the fine wavy lines so often visible on decorticated specimens of that family. In describing the second tree, he alludes to a deep wedge-shaped rift on the south-east side, which had been coated with coal, and is strongly marked with wavy lines, like those on the surface of the alburnum of a gnarled oak. On the fifth tree, he discovered a longitudinal concavity on the north side, and he states that it resembles the impression which would be left in a dicotyledonous tree, by the pressure of a parasitic plant. The characters of the roots are also detailed at considerable length, particularly their mode of bifurcation, and position with respect to the horizon.

From a careful consideration of the phænomena presented by the fossils, Mr. Bowman is convinced that they stand where they originally flourished; that they were not succulent, but dicotyledonous, hard-wooded forest trees; and that their gigantic roots were manifestly adapted for taking firm hold of the soil, and in conjunction with the swollen base of the trunks to support a solid tree of large dimensions with a spreading top.

Towards the close of 1838, in forming the railway tunnel at Claycross, five miles south of Chesterfield, a number of fossil trees were found, standing at right angles to the plane of the strata. The tunnel passes through the middle portion of the Derbyshire coal measures, which there dip about 8° to a little north of east. The bases of the trees rested upon a seam of coal fifteen inches thick. The exterior of the stems consisted of a thin film of bright coal, furrowed and marked like the *Sigillaria reniformis;* and the interior consisted of a fine-grained sandstone. Mr. Conway, who supplied Mr. Bowman with an account of the discovery, infers, from the information which he obtained, that there must have been at least forty trees found, and judging by the area excavated, he is of opinion that they could not have stood more than three or four feet apart. There were no traces of roots, the stems disappearing at the point of contact with the coal. Several specimens of *Stigmaria ficoides* were also noticed by Mr. Conway, lying horizontally, and about three feet in length.

With reference to fossil trees in general, and especially to those near Manchester, Mr. Bowman proceeds to show still further; 1st, that they were solid, hard-wooded, timber trees, in opposition to the common opinion that they were soft or hollow; 2nd, that they originally grew and died where they have been found, and consequently were not drifted from distant lands; and, 3rd, that they became hollow, by the decay of their wood, from natural causes, similar to those still in operation in tropical climates, and were afterwards filled with inorganic matter, precipitated from water.

1. In stating his reasons for believing that the coal measures' casts were solid timber trees, Mr. Bowman alludes to the rifting of the bark of modern forest trees, in consequence of the expansion caused by the annual addition of a layer of wood between the bark and the alburnum; and to the thickening or swelling of the base of the trunk and main roots, and the apparent lifting up of the latter out of the soil, in old trees, by the greater annual increase of the upper part or that nearest to light and heat. These phænomena in vegetation were illustrated by a diagram, which exhibited the form of the base of the stem and the root of a sapling, and of a full-grown tree. The author, in applying these characters to the fossils of the Manchester and Bolton railway, alludes to the irregular, longitudinal and disconti-

nuous or anastomosing furrows on their surface, to the swelling out at the base of their stems, and to the divergence as well as the angle of dip or downward direction of their roots. These characters, he says, are not observable in soft monocotyledonous trees, their stems never expanding laterally, and being as thick when only a few years old and a foot high, as when they attain the height of 60 or 100 feet. Their roots also, instead of being massive and forking, generally present a dense assemblage of straight succulent fibres, like those of an onion or hyacinth. Analogy, therefore, as far as outward shape and habit are concerned, he adds, is strongly in favour of the fossils having been solid timber trees.

Mr. Bowman then combats the view, generally entertained, that fossil stems with perpendicular furrows, as in the Sigillaria, were succulent or hollow plants*. He states, that good specimens of decorticated Sigillariæ exhibit fine straight, and curled or gnarled striæ, similar to those on the alburnum of many modern forest trees; and that this character, in conjunction with others, renders it almost certain, that the fossils had a separate bark,—a feature which is considered in vegetable physiology to be a proof of a woody structure. He also alludes to the existence in many of the decorticated parts of these fossil trees of little prominences like those in barked timber; likewise to the scars left by the disarticulation of leaves; and he accounts for the general absence of the latter on large and old trunks, by their having been obliterated, in consequence of irregular expansion from the deposition of new layers of wood: he notices moreover the absence in small Sigillariæ of the irregular furrows observed on large specimens, and due in his opinion to the unequal expansion by the addition of new layers of wood. In support of these proofs of the original solid nature of the trees, Mr. Bowman exhibited polished slices mounted upon glass of portions of a similar fossil tree discovered in sinking a shaft 300 or 400 yards N.W. of those found on the line of the railway. The slices were made from a portion which exhibited within the carbonized bark, a patch browner, heavier, and more compact than the rest. In these slices, made under Mr. R. Brown's direction, that gentleman discovered in the transverse section, the uniformity of vascularity which is evidence of coniferous structure; and in the longitudinal section parallel to the medullary rays, the existence of these rays. The slices therefore exhibit proofs of dicotyledonous structure, and considerable probability of that structure being coniferous. The important evidence however of coniferous structure deducible from discs in sections parallel to the rays, was not obtained, the vessels having apparently undergone some alteration.

2. With respect to the second point, that the trees grew and died

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* Specimens of recent dicotyledonous wood from New Zealand, lent to the author by Mr. R. Brown, were exhibited on the table of the Meeting Room. They displayed both upon the bark and the naked wood, longitudinal ribs and intermediate furrows as regular as those on Sigillariæ; and therefore prove that these characters are not incompatible with a dicotyledonous structure.

on the spots where they are now found, and that they were not drifted from distant lands, Mr. Bowman says, the arguments in favour of the formation of beds of coal by a series of subsidences of the surface on which the vegetables that produced the coal grew, naturally lead to the inference that the trees associated with the coal also flourished on the same spots. In opposition to the opinion that trees would naturally float in an upright position in consequence of the greater specific gravity of the base and roots, he asserts, that the trees would maintain that position only as long as they floated, and that they would fall and lie prostrate when grounded on shoals or cast ashore. He agrees with Mr. Hawkshaw in the opinion, that it is more difficult to account for a number of great trunks being deposited in the position of the fossils in the Manchester railway, than to imagine that they grew on the surface of the bed on which they now stand. Their position on a bed of coal is another proof, Mr. Bowman conceives, that the trees were not drifted, for if they had been transported by currents of water they might equally have been imbedded in the alternating shales or sandstones. If beds of coal are the accumulated remains of many generations of a luxuriant vegetation, the rich compost thus formed, Mr. Bowman argues, would be well suited for the growth of trees. Again, the angle at which the roots of the fossil trees, particularly of that distinguished by him as No. 2, dip towards the bed of coal, is considered by the author evidence of the trees being in their original position, because, had they been drifted, the roots would have been bent upwards, by the downward pressure of the trunk, when the water had left them. The appearance of the roots being cut off, where in contact with the coal, he is of opinion, may be explained by the fermentative process having dissolved the vegetable texture below the surface. The stems and upper portions of the roots standing above the coal, he explains by reference to similar phænomena in peat marshes, in which the bases of the trunks of ancient forest trees stand with the roots exposed, owing to the shrinking of the surrounding peat.

3. In discussing the third point, that the trees became hollow from the decay of their wood, and were filled with sedimentary matter after their immersion, Mr. Bowman refers to the facts recorded in the preceding paper by Mr. Hawkshaw (see vol. xvii. p. 541); and in confirmation of them states, that Mr. Schomburgk during his four years' travels in Surinam repeatedly observed similar phænomena. Mr. Bowman then proceeds to explain the processes by which he conceives the fossil trees were gradually submerged—their upper branches torn off—their interior removed by natural decay,—their bark converted into coal,—their central cavities filled with sediment; and the whole buried beneath the stratum of shale or sandstone in which the trees were discovered. He afterwards applies the phænomena which he believes these processes produced to the condition and position of the trees and the arrangement of the surrounding sedimentary matter. The author then enters into the inquiries, 1st, the time which the trees may have required to attain their dimensions; and consequently the minimum of years requisite for the accu-

mulation of the vegetable matter; and, 2ndly, what thickness of vegetable matter was necessary to form the stratum of coal nine inches thick, over which the trees stand. Mr. Schomburgk is of opinion that a dicotyledonous tree which would require in temperate climates one hundred years to attain a certain diameter, would arrive at the same dimensions within the tropics in sixty or eighty years. The largest of the fossil trees forming the immediate subject of the paper is equal in circumference to an oak of 130 years growth in this climate, or about 100 for a climate equal in temperature to that of the tropics. Allowing therefore that some time elapsed after the commencement of vegetation on the surface of the then dry land before the trees began to grow, Mr. Bowman infers, that 100 years must be the minimum of time which would be required for the production of the vegetable matter out of which the nine inches of coal were produced. With respect to the depth of the stratum of vegetable matter from which it was formed, Mr. Bowman takes for his data, the thickness of the bed of coal, nine inches; the distance between the top of the seam and the bottom of the trunk under the arch formed by the roots, fifteen inches; and for the distance to the surface of the ground, four inches, or in all twenty-eight inches; whereby he infers that the thickness of the solid coal is equal to about one-third that of the vegetable matter out of which it was produced.

A paper was lastly read, "On the character of the beds of clay lying immediately below the coal seams of South Wales; and on the occurrence of coal-boulders in the Pennant grit of that district;" by William Edmond Logan, Esq., F.G.S.

Immediately below every regular seam of coal, in South Wales, (and nearly 100 are known to exist) is constantly found a bed of clay, varying in thickness from six inches to more than ten feet, and called the underclay, undercliff, understone, pouncer, or bottom stone. It is so well known to the collier, that he considers it an essential accompaniment of the coal; and only where it ceases, does he give up his expectation of finding coal. Seams which have thinned out in one portion of a work, have been recovered in another by following this bed.

The underclay is always more or less argillaceous, but it is never without a considerable admixture of sand; and in most cases it yields a very good fire-clay; which, though generally tough when freshly cut, yet crumbles on exposure into a mass of a grey colour. Occasionally it is quite black, in consequence of the presence of carbonaceous matter, and it then sometimes resists the effects of the weather. Under a part of the lowest seams of coal between Swansea and the Bury river, it is a hard, durable, finely grained, siliceous stone. It is however by containing innumerable specimens of *Stigmaria ficoides*, that these beds are most strongly marked, other portions of the coal measures presenting the same mineral composition. The stems of the Stigmaria, which are usually of considerable length, always lie parallel to the plane of the bed, and nearer to the top than the bottom; and they are occasionally compressed, their diameter varying from two to six inches. Their long slender processes, covered with a pellicle of carbonaceous matter, form an entangled mass, and traverse the beds in every direction, vertically, horizontally, and obliquely; but Mr. Logan has never been able to trace them to their termination, though he has followed single processes for considerable distances. Portions of the stem of the Stigmaria are found in other parts of the coal measures, but it is only in the underclay that the fibrous processes are attached to the stem or associated with it. Mr. Logan, however, states, that if such specimens exist in other strata, they are not so likely to be exposed, as those beds are less worked than the underclay.

In some instances, the Stigmaria, with its processes, is found equally abundant in the roof as in the floor of a coal pit, but in such cases the roof has been ascertained to be the underclay of an immediately overlying bed of coal.

Mr. Logan then quotes at length, Steinhauer's account of the Stigmaria, as it gives the best explanation he has seen of the external botanical character of the plant, as well as of its position in the beds in which it occurs; the only point in which his experience induces him to differ from Steinhauer, being the vertical extent to which the fibres range. Mr. Logan has never traced them in that direction more than seven or eight feet from the stem, though he admits they may have an horizontal range of twenty or more feet. (American Phil. Trans. New Series, vol. i. p. 265, 1818.)

When it is considered, that over so considerable an area as the coal field of South Wales, not a seam has been discovered without an underclay, abounding in Stigmaria, Mr. Logan says, it is impossible to avoid the inference, that there is some essential and necessary connexion between the existence of the Stigmaria and the production of the coal. To account for their unfailing combination by drift, seems to him unsatisfactory; but whatever may be the mutual dependence of the phænomena, he is of opinion, that it affords reasonable grounds to suppose, that the *Stigmaria ficoides* is the plant to which may be mainly ascribed the vast stores of fossil fuel.

In the second part of the paper, Mr. Logan gives an account of boulders or rounded fragments of coal, contained in the coal measures themselves.

The thickness of the coal deposit of South Wales, he says is equal in the deepest part to 12,000 feet, and that consequently a long period must have been required for the accumulation of the materials, and that any fact which may assist in ascertaining its length, cannot fail to possess some interest. The occurrence of these boulders he is of opinion bears upon the subject.

From a layer of indurated clay, two inches thick, lying on the top of a seam of common bituminous coal, and covered by hard sandstone, at Penclawdd on the Bury river, he obtained, in the spring of 1839, a worn, rounded mass of cannel coal, six inches long, four inches wide, and two inches thick. The discovery of this singular specimen having excited attention to the subject, it was ascertained that in the quarries of the enormous mass of sandstone forming

Cilfay hill and the Town-hill range from Swansea to the Bury river, there occur many irregular conglomerate beds, formed of innumerable pebbles and small boulders of coal, sometimes four inches in diameter, mingled with sand and pebbles of ironstone; and there have been also found in them small boulders of granite and mica-slate. Many impressions, coated with coal, of Sigillariæ and other plants, occur in the mass; and the difference of age between this coal and that of the pebbles, he says, is beautifully illustrated in numerous cases, where the softer coal of the plants has been pressed down upon the harder coal of a layer of the pebbles, by the cleavage of the former, however distorted the plant may be, presenting an uniform parallelism, while the cleavage of the coal forming the pebbles is parallel with the sides of the pebbles, which are inclined in all possible directions.

The pebbles consist principally of the common bituminous coal of the neighbourhood, but two have been found composed of cannel coal, the only seams of which, existing in the lower measures, occur about 2000 feet below the conglomerate bed.

The Cilfay sandstones and the measures at Penclawdd, in which the first-mentioned pebble was found, form part of the Pennant grit; and there is reason to believe, that throughout the whole of this great mass of sandstone, about 3000 feet thick, occasional beds of coal pebbles are to be met with: but Mr. Logan has not seen any associated with the lower measures.

March 11, 1840.—A paper was first read, "On the Rocks which form the West shore of the Bay of Loch Ryan in Wigtonshire, N.B.," by John Carrick Moore, Esq. F.G.S.

The peninsula of the Ryans extends about thirty miles from N. to S., and is about seven miles across at its greatest breadth, or from Stranraer to Port Patrick. In the geological maps of M. Necker, Dr. Macculloch, and Mr. John Phillips, it is coloured as part of the great graywacke chain, stretching from the Irish sea to St. Abb's Head; and the chief part of the rocks composing the peninsula, Mr. Moore says, undoubtedly belongs to that epoch; but he has ascertained from an examination of the district during the summer of 1839, that others of a more recent date also exist.

The portion of the peninsula particularly described by the author, extends about eleven miles from north to south, and about five from east to west; and is bounded on the W. and N. by the Irish sea, and on the E. by the Bay of Loch Ryan. The formations of which it consists are—1. Graywacke, 2. Trap rocks, 3. Coal measures, and, 4. a red breccia.

1. The graywacke constitutes the greater part of the district, the beds being nearly vertical and the prevailing strike E. by N. At the northern extremity, near the Corsewall Lighthouse, are beds of conglomerate composed of rounded masses of granite, with pebbles of serpentine and other rocks. In the little bay of Sloughnagarry, at the most southern point, where the graywacke shows itself, Mr. Moore found in a slaty rock alternating with compact beds, an abundance of fossils, determined by Mr. Lyell to be graptolites.

2. The trap rocks occur at two points, one near the northern extremity of the peninsula, on the farm of Balscallock, constituting a dyke of amygdaloid greenstone which cuts through the graywacke and is lost in the sea; and the other is near Loch Connell, where a mass of greenstone extends in a westerly direction for nearly two miles. At both localities, the trap intersects the graywacke; but at neither point could the author find it in contact with the coal or overlying breccia.

3. Coal Measures. A deposit consisting of beds of red and white sandstones, clays and micaceous shale, similar to those of the coal-field of Ayr, has been long known to exist in the district, and has led to several fruitless researches for coal. The deposit may be traced for about nine miles, forming a narrow band parallel to Loch Ryan. The beds are in general moderately inclined to the E. or S.E. In a quarry on the farm of Clachan, Mr. Moore found remains of *Stigmaria ficoides*, and in another, on the farm of Challock, an abundance of Calamites.

4. Red Breccia. This rock extends from the bay of Sloughnagarry to the farm of Dumlae, a distance of eight miles, forming a ridge from 200 to 300 feet high, between the coal measures and the shore of Loch Ryan. It consists entirely of irregular fragments of graywacke cemented by a red clayey sand, but in some places it passes into laminæ of red sandstone. The beds are nearly horizontal or dip slightly to the S.E., and rest on the coal measures. As Mr. Moore did not detect any organic remains in the breccia, nor find any rock overlying it, he does not offer an opinion respecting the period of its formation.

A paper was afterwards read, "On the Siliceous Bodies of the Chalk, Greensand and Oolites;" by Mr. Bowerbank, F.G.S.

The author commences by stating, that naturalists and geologists have long considered the form of tuberous masses of flint found in the upper chalk to be due to alcyonia or sponges, but that he is not aware of this opinion having been proved to be correct. It was Professor Ehrenberg's observations on siliceous bodies which first induced him to obtain thin slices of flint with the intention of procuring specimens of Xanthidium. In the examination of these slices, he was struck with the frequent occurrence of patches of brown, reticulated tissue, spicula and Foraminifera, and he was induced to infer, that the patches of tissue were the remains of the organized body, possibly a sponge, to which the flint owed its form. With this belief, he commenced his inquiries by examining thin slices of flints obtained from various localities, and he found in all of them, a perfect accordance in the structure and proportion of reticulated tissue, in the number of spicula, and in the occurrence of Xanthidia and Foraminifera. The following are the general appearances which the slices of flint exhibit when mounted upon glass.

With a power of about 120 linear, the slice presents the appearance of a stratum of a turbid solution of decomposed vegetable or animal matter containing Foraminifera, spicula, Xanthidia, and frequently fragments of the brown tissue. In a specimen from North-fleet the mass of the spongeous portion exhibited numerous cylindrical contorted canals, which from their uniformity and minuteness of diameter, Mr. Bowerbank considered to be the incurrent canals of the sponge; and other orifices of greater diameter, to be the excurrent. Very frequently, when little of the reticulated substance of the sponge remains, its former presence, the author says, is indicated by the siliceous matter resembling a congeries of gelatinous globules, moulded by the tissue amid which it was deposited; and the globules, when traced to the edges of the patches of spongeous texture, were found to agree in size and form with the orifices of the supposed incurrent canals. In cases where no traces of the sponge can be detected, Mr. Bowerbank thinks, that the mode in which the spicula, Foraminifera and other extraneous matters are dispersed equally in all parts, and not precipitated to one portion of the flint, indicates that the organized tissue in which they were entangled, retained its form and texture sufficiently long to allow of the fossilization of these remains in their original places; and that the nature and position of these bodies strongly indicates the former spongeous nature of the flint.

When the chalk is carefully washed from the exterior of a flint, and a portion examined as an opake object with a power of about fifty linear, it exhibits a peculiar saccharine appearance, with deep circular excavations, having fragments of extraneous matters partly imbedded or adhering to them. If the surface be further cleansed by immersion in diluted muriatic acid, till effervescence ceases, spicula may be detected on the sides of the deep circular cavities; and if, again, a piece a quarter of an inch in diameter, presenting the roughest aspect, be examined under a power of 120 linear, illuminated by a Lieberkuhn, the surface, under favourable circumstances, will present a complex mass of small, contorted tubuli, occasionally furnished at the apex with a minute perforation.

The structure and other characters of the tabular flints are stated to accord perfectly with those of the nodular masses, except that the under surface has a still more marked spongeous aspect, and that spicula and Foraminifera are more abundant. The absence of any apparent base or point of attachment in the great mass of nodular chalk flints, the author says (considering them undoubtedly of spongeous origin), may be accounted for by supposing that the gemmule was originally attached to some minute fragment of a shell or other substance, and that its further development took place while recumbent on the mud or silt.

The perpendicular and oblique veins of flint between Brighton and Rottingdean, are reported to present exactly the same internal characters as the tabular and nodular flints, and to agree externally with the former. The occasional existence of a fissure filled with chalk, in the centre of the vertical layers, Mr. Bowerbank conceives, may indicate that the sponge had grown from the two sides of the crevices, but had not in all places been able to unite. The sides of these flint veins are not studded with Foraminifera in a manner similar to that of the tabular horizontal layers.

Mr. Bowerbank next examined the flint with which Echinites and shells of the chalk are often entirely or partially filled and enveloped, and he states, that the results were the same, both with reference to the exterior and the interior of the flint. In those cases in which the Echinite is only partially filled, he infers that the portion so occupied was originally a sponge, because its surface is uneven; for had the flint been deposited in an empty shell or Echinite, it would present an uniformly flat surface. Again, he states, that the projecting of the flint through the two openings of the Echinite, with an extension to a greater or less distance, is owing to the sponge having grown outwards through these orifices; and the envelopment of an organic body by a tubular mass of flint, he explains by reference to the habit of recent sponges to invest testacea or other marine bodies. In some cases, he has found minute but deep depressions on the surface of flints filling Galerites, and immediately opposite to the ambulacral pores; and he ascribes the origin of the depressions to streams of water drawn in through the orifices to supply the wants of the living sponge.

Mr. Bowerbank was afterwards induced to extend his examination to the flints which invest the zoophytic bodies of the Wiltshire chalk. By carefully cleaning the interior of some of these flints, he discovered spicula projecting from all parts, however different the character of the inclosed body; and the spicula appeared to have no reference to it, none of them being found on its surface. Under the microscope, the investing flint presented in every respect the same appearance as that exhibited on the lower surface of the tabular flints, having fragments of minute corals and small shells attached to the inner surface. A thin slice exhibited the usual organic contents of the common flint. He, therefore, infers that the tubular flint which incloses the zoophytes, owed its origin also to a sponge which invested the organic nucleus.

A comparison of the characters presented by the spongeous remains of the flint, with a collection of recent sponges, has induced Mr. Bowerbank to conclude that the fossils cannot be referred to any of the established divisions of existing sponges.

On examining the cherts of the greensand of Fovant in Wiltshire in the same manner, he found that the only differences between them and chalk flints, existed in the coarser texture of the spongeous fibre, the greater size of the interstices of the network, and the larger dimensions of the imbedded extraneous bodies. The cherty nodules of the upper greensand of Shaftesbury afforded similar appearances. A black, semi-transparent nodule, with an outer coat resembling agglutinated sand, was found under the microscope to contain numerous contorted canals of various sizes, and a considerable number of beautiful green spicula. Two chert casts of Spatangi from Shaftesbury afforded results analogous to those obtained from chalk Echinites.

Slices from a great variety of the greensand cherts of Lyme Regis presented characters which agreed with the cherts of Fovant. A specimen of flint from the Portland stone of Tisbury, and another from Portland, gave a greater quantity of cellular structure than any of the previously noticed cases, and the texture bore a greater affinity to that of the freshwater sponge, than is exhibited in the flints of the chalk or the cherts of the green sand.

With respect to the causes of the deposition of the flint, Mr. Bowerbank objects to the supposition, that it was influenced by the siliceous spicula of the sponges, because the flint is in no case limited or determined by their immediate presence, but is, in all instances, bounded by the extent of the animal matter of the sponge. He has frequently observed that the large excurrent canals in the chalk-flint spongites are not filled with silex, and that the spicula projecting into them have not the slightest incrustation of siliceous matter upon their surface; while on the contrary, wherever a single tube or a thin layer of tubes has been projected from the mass into the chalk, the silex has been attracted to it. He conceives also, that the retention of the spicula and extraneous matters in all parts of the flint, may be accounted for, by supposing that the animal matter was the attractive agent, acting equally throughout the whole body of the sponge. In support of his argument he adduces the siliceous shells of Blackdown, and the siliceous corals of the Tisbury oolite and the mountain limestone, which contain no spicula, and in which it cannot be supposed that previously existing siliceous matter was the attractive agent. Lastly, the pyritous fossils of the London, Kimmeridge, Oxford and other clays, are also mentioned as examples of animal and vegetable substances having exercised an attractive influence.

March 25, 1840.—A paper was first read "On the Age of the Limestones of South Devon;" by W. Lonsdale, F.G.S.

The object of this communication is to show the nature and limits of the author's claim to having been the first to infer from zoological evidence that the limestones of South Devon would prove to be of the age of the old red sandstone; and it was drawn up at the request of Mr. Murchison, in consequence of the subsequent adoption and extension of the proposed classification by Professor Sedgwick and that gentleman; and at the request likewise of Dr. Fitton, in consequence of the same views having been applied to some of the infra-carboniferous formations of Belgium and the Boulonnais. The paper commences with a summary of the opinions previously entertained respecting the age of the limestones. The authors quoted are, Woodward, 1722; Da Costa, Maton, Playfair, Berger, L. A. Necker, De Luc, T. Thomson, Kidd, W. Smith, Brande, W. Phillips, Hennah, Greenough, Sedgwick, W. Conybeare, J. J. Conybeare, Buckland, Dufrénoy, Elie de Beaumont, De la Beche, Prideaux, Boase, J. Phillips, Austen, Murchison, Bakewell and J. de Carle Sowerby.

By these geologists the limestones are placed in the primary, transition or graywacke and carboniferous series; Mr. Prideaux being the only author who ascribes them in part, and on mineral characters, to the old red sandstone; and Mr. J. Phillips, in his article on geology in the Encyclopædia Metropolitana, hesitating to place them in a definite position, in consequence of the resemblance of many of the shells to species found in the mountain limestone. Mr. De la Beche, in his memoir on Tor and Babacomb Bays, also states that the limestones of that district rest on old red sandstone; and in his Report on Cornwall and Devonshire (1839), he says, "that those who rely very exclusively on the character of organic remains would probably feel disposed to consider the Torbay and Plymouth beds as equivalent to some such rock as the old red sandstone." The author of the paper refrains from all reference to the memoirs of the Rev. David Williams and Mr. Weaver, because his attention is more particularly confined to the limestones of South Devon. In allusion to the diversity of opinions which have been entertained respecting these rocks, even on some occasions by the same geologist, he is of opinion that it must be ascribed to the want, at the time the memoirs were written, of that preponderating weight of evidence which enables the mind to rest steadily on its own decisions; and that if a better result be now attainable, it must be ascribed to the mass of evidence, which has been recently accumulated in various parts of the kingdom. Until the organic remains of the mountain limestone and Silurian system had been determined, the former overlying and the latter underlying the old red sandstone, and shown by Mr. Murchison to graduate regularly into that formation, and to contain perfectly distinct suites of fossils, it was impossible to determine the age of a series of beds, the fossils of which were in part new, and in part closely allied to carboniferous shells; and procured from a region but partially examined, without a base line, beset with faults, and traversed by igneous rocks.

Mr. Lonsdale then proceeds to show, what was the zoological evidence on which he ventured in December, 1837, to conclude that the South Devon limestones would prove to be of the age of the old red sandstone. Previously, he had examined in part the corals of the Silurian region and South Devon, and ascertained that some of the species are common to both; he had also examined with Mr. J. Sowerby, Mr. Hennah's valuable collection of fossils from the neighbourhood of Plymouth, and had become aware, from the decisions of Mr. Sowerby, that certain of the shells could with difficulty, if at all, be distinguished from mountain-limestone species; and that some were distinct. In December, 1837, he examined with Mr. Austen a portion of that gentleman's collection of Newton Bushell fossils, and though he ventured to differ from some of the identifications with mountain-limestone species pointed out to him, yet these shells agreed so much in aspect with testacea of the carboniferous fauna, that he could not doubt there was a connexion between the beds from which they had been obtained and the mountain limestone system: the same collection also proved that, associated with these shells were corals of Silurian species. He had also been informed by Mr. Austen, that in beds connected with the limestone, the *Calceola sandalina* had been found. It was therefore by combining the amount of the above evidence, the presence in the same strata of shells, identical, or nearly identical, with mountain-limestone species, of Silurian corals, the *Calceola sandalina*, and a numerous distinct testacea, that he suggested the South Devon limestones would prove to be of an age intermediate between the carboniferous and Silurian systems, and consequently of that of the old red sandstone. In alluding to Professor Sedgwick and Mr. Murchison's adoption of the suggestion in 1839, and their bold application of it to all the older sedimentary rocks of Devon and Cornwall, the author states, that the fullest testimony is borne in the papers, containing their present views of the structure of those counties, of the source whence the suggestion was derived.

Appended to the notice was a list of fossils, necessarily very incomplete, from the limited nature of the materials at the author's command. It consisted of sixty-three species; twelve considered common to the Carboniferous and Devonian limestones, forty-two peculiar to the Devonian strata; and nine, seven of which are corals, common to the Devonian and Silurian formations; doubts were, however, entertained respecting the identification of the two species of shells. The author then observes,—should it be urged that it was unjustifiable to assume, from organic remains alone, the age of the Devonshire limestone, it may be replied, that in a district of which little in 1837 was positively known, which is cut off by the granite of Dartmoor from the only base line of the country, the culm measures of central Devon, proved in 1836 by Prof. Sedgwick and Mr. Murchison to be the representative of the true coal measures, organic remains were the only test by which the age of strata so situated could be determined; and in support of his argument, he advanced the recent establishment in Cutch and the Desert to the east of it, from the examination of suites of fossils brought to England by Capt. Smee and Capt. Grant, and others procured by Colonel Pottinger at the request of Colonel Sykes, of a series of beds unquestionably of the age of the oolites of England, the fossils agreeing in their general characters with those of that geological epoch in this country, and being in many instances specifically undistinguishable. In this case, mineral characters and order of superposition would have been valueless guides, for the rocks are totally different in character from those of the same age in England; and there was no predetermined series of beds from which an order of superposition could be derived. Another instance was noticed of the value of organic remains, if rightly applied, in determining the relative age of a distant region, and in this case of one inaccessible to Europeans, in the Ammonites obtained from the Tartar side of the Himalayan mountains. These fossils prove the existence in that unexplored country, of rocks of the secondary epoch, by possessing that peculiar character in the sutures, which is not found in Ammonites of any other epoch; they are moreover accompanied by Belemnites.

In advocating the value of fossils, the author, however, begs it may be clearly understood, that he would not expunge from the geologist's consideration, the aid to be derived from order of superposition, and under a right control, from the use of mineral composition and lithological structure; and he would advise the observer not to depend upon his own limited sources of knowledge, but to seek the aid of the philosophical zoologist, who can teach him to reason justly on the distribution of animal life,—the accidents to which it is liable,—the changes which such accidents may produce, or the means provided by nature to resist them,—and on the effects which a permanent alteration in the inhabiting medium may work in the form and size of a shell or coral.

Of the importance of organic remains in identifying districts less widely separated, the two following instances were noticed. In M. Dumont's work on the geology of the province of Liège, published in 1832*, and justly valued for unravelling the structure of a most intricate country, the strata immediately beneath the mountain limestone are divided into three systems, but without any definite comparison with the formations which underlie that deposit in England. At the meeting of the Geological Society of France, at Mezières, in September, 1835, Dr. Buckland proposed the following first comparison between the systems of M. Dumont and the subdivisions of the Silurian system established by Mr. Murchison:—

Système calcareux supérieur	Mountain limestone.
	(Old red sandstone wanting.)
Syst. quartzo-schisteux supérieur	The Ludlow rocks.
Syst. calcareux inférieur	The Dudley and Plymouth limestone.
Syst. quartzo-schisteux inférieur	The Caradoc sandstone.
	(Builth and Llandeilo flags wanting.)
Terrain Ardoisier.	

This comparison was principally founded on the resemblance of the corals with those obtained at Dudley and Wenlock. M. Constant Prevost pointed out the resemblance of the *calcaire bleu* of the *système calcareux inférieur* of M. Dumont with the Plymouth limestone, and of the marble of Heer, subordinate to the *système quartzo-schisteux supérieur*, with the limestones of Babacombe. Mr. Greenough, however, doubted the identity of the Plymouth and Dudley limestones, and he stated that he had remarked the total absence of the Dudley Trilobites in the *système calcareux inférieur*. During the Mezières meeting, Dr. Buckland identified certain beds beneath the mountain limestone near Namur, Dinant, and Huy, and at Engis, with the old red sandstone†; and at an ordinary meeting of the Geological Society of France, in December, 1837, M. Rozet repeated his belief, that the old red sandstone is well developed between Dinant and Namur; and M. Constant Prevost stated, that he had also during the Mezières meeting, determined its existence in those districts. In 1838, M. Dumont visited England for the purpose of examining the

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[* An Abstract of M. Dumont's memoir appeared in L. & E. Phil. Mag. vol. xv.—Edit.]

† In the "Outlines of England and Wales" (1822), the Rev. W. D. Conybeare places all the Belgian beds between the carboniferous limestone and the transition slates in the old red sandstone.—Note, 468.

Silurian region; and on his return to Belgium, he laid before the Royal Academy of Bruxelles a table, differing from that of Dr. Buckland only in drawing more closely the terms of comparison, and in identifying the two upper divisions of the Terrain Ardoisier with the Cambrian system. He stated also, in a report which accompanied the table, that the old red sandstone was most probably wanting in Belgium, or, if it exist, that it must be considered as a great development of the superior part of the Upper Ludlow Rock. In M. Dumont's work, before mentioned, lists are given of the fossils from each system; and on examining them, for the purpose of determining how far the comparison of the Belgian and Silurian systems could be established by organic remains, the author of this notice ascertained, that out of twenty-two species, only four could be considered as peculiar to the Silurian system; and of these he believes two may be erroneous identifications; that five species are common to the Belgian beds and the mountain limestone, and thirteen to the Belgian and Devonian systems. These lists, Mr. Lonsdale states, are small, but, he adds, they bear internal evidence of having been carefully drawn up without any preconceived theory; and he conceives that they afford sufficient proof that the beds from which they were obtained do not belong to the Silurian system, but partake of the same intermediate character as the Devonian limestones. The other case, alluded to in the paper, refers to the older beds of the Bas Boulonnais. These strata were identified by M. de Verneuil in 1838, with the Silurian series of England, particularly a bed of limestone containing corals and other fossils with the Wenlock limestone; and M. Dumont, who examined the country with M. de Verneuil, states in his report to the Bruxelles Academy, that his four systems occur in the Boulonnais. The above bed of limestone, M. Rozet had also, in 1828, placed below the old red sandstone; but in a subsequent memoir, published in the *Annales des Sciences Naturelles* (xix. p. 145. 1830), he assigns it to the old red sandstone. At the Meeting of the Geological Society of France at Boulogne, in Sept. 1839, and at which some of the Fellows of the Geological Society of London assisted, the identification of the Boulonnais beds with the Silurian system was fully admitted. When, however, doubts were recently thrown out respecting the age of the formations in the Liège districts on account of the nature of their fossils, Mr. Murchison, who was present at the Boulogne Meeting, stated to the author of this notice, that if the Liège country had been wrongly identified, the older beds of the Boulonnais had been wrongly identified also. To determine the question, as far as fossils would assist, Mr. Murchison procured, by the kind assistance of M. Dutertre Yvart, a collection of specimens in the Museum at Boulogne. An examination of these specimens with published lists, proved that the inference was just, and that there exists in the Bas Boulonnais, the same mixed assemblage of mountain limestone, Silurian and Devonian, or peculiar fossils, as in the province of Liège and in Devonshire.

A note "On the Bone Caves of Devonshire," by R. A. C. Austen, Esq., F.G.S., was then read.

Mr. Austen commences by noticing the two theories which have been proposed to account for the introduction of the bones of animals into caves—one, which accounts for their presence on the belief that they were dragged in by hyænas or bears inhabiting the caves; the other, which supposes that the bones were drifted in by diluvial waters. He then proceeds to give his own explanation of the phænomena presented by Kent's Cave and Yealmpton Cavern; but he says it is not his intention, by doing so, to propose a general theory for ossiferous caves.

In the Devonshire caverns, mentioned above, remains of the Elephant, Hog, Rhinoceros, Horse, Ox, Bear, Hyæna, and Cat, generally bearing marks of teeth, are intermingled. With reference to the means by which they were collected, Mr. Austen observes, the habits of the Hyæna are now better known than formerly, and there is little in them to warrant the conclusion that the fossil bones were collected by that animal. He says, on the authority of Cuvier, that hyænas "se tiennent solitaires dans les parties montagneuses," (last Edit. Oss. Foss.) least of all do they inhabit caves; that they have not the courage to attack any formidable animal, preferring the putrid flesh and bones, which they find in their nightly prowlings: that they never drag away their prey, but devour it greedily on the spot: and he adds, on the authority of M. Marcel de Serres, who has observed the habits of the Hyæna in Africa, "that its gluttony is equalled only by its cowardice."

The Lion, on the other hand, seeks solely for living prey, which it prostrates at one spring, and then conveys to its lair. The African lion has been known to carry off a bullock, and its constant abode is in chasms, caves, or on overhanging ledges of rock.

Mr. Austen is therefore induced to believe that the cavern bones were in the first instance the prey of the larger feline animals, and that during their absence the hyænas visited the caves to feed upon the fragments of the partially consumed prey; and in support of this view he quotes the passage from Johnson's Field Sports, given in the *Reliquiæ Diluvianæ* (p. 22): "they feed on small animals and carrion, and often come in for the prey left by tigers and leopards after their appetites have been satiated."

What the large feline animals were, Mr. Austen says, is not important, as they resemble each other in their habits. The remains of a Felis as large or larger than any now known, have been found in the Plymouth and Hutton caves, and the canine and molar figured by Dr. Buckland from Kirkdale, are said by Cuvier to differ in no respect from those of a lion. (Oss. Foss. IV. 151.)

The remains of a fossil lion have been also found in the caves of Gailenreuth, the province of Liège, Mialet and Jobertas (Dép. du Gard), Lunel-Viel, Joyeuse, Ardêche, Fouvent, Fausan (Dép. de l'Herault), and in Kent's Cavern.

It is known that the Lions of the present day will attack every one of the animals, the remains of which are found in Kent's Hole, and other caves; and if it should be urged that the most powerful lion could not carry off the bodies of the great Pachyderms, Mr. Austen says, that an examination of a very large proportion of the remains taken from Kent's Hole has proved that the bones and teeth of the Elephant belonged to young animals; and he quotes Dr. Buckland's statement, that the ten elephants' teeth discovered in Kirkdale Cave belonged to extremely young animals. (Rel. Dil. p. 18.)

The conclusions, therefore, which Mr. Austen wishes to draw are, 1st, that the carcases were dragged into the bone caves by powerful feline animals; and 2ndly, that hyænas picked and gnawed the bones after those animals had satisfied their hunger, and while they were absent. He also objects to the belief that some of the German caves are filled with the animal matter of countless generations of bears, as the decomposition of one carcase, he says, would have driven the living bears from the cave; but he believes the prevailing fossil remains in each locality indicate only what animals were most abundant in the district, and consequently most frequently fell a prey to the powerful Felidæ: thus in the low grounds about Yealmpton, Kent's Hole or Kirkdale, herbivora may have been most abundant, and bears in the region of the Hartz*.

April 8th.—A paper was first read, "On the Great Fault, called the Horse, in the Forest of Dean Coal Field;" by John Buddle, Esq., F.G.S.

The term fault is used in this paper in the miner's signification, or for any interruption in the regular deposition or range of a bed. The Horse Fault, therefore, is not a displacement of one part of the stratum by a dislocation, but a local thinning out of a bed of coal, and a substitution of sandstone for it.

The Horse has been traced in the Coleford High Delf seam, the 23rd in the descending series, or the 3rd from the bottom, and the only one in which it is clearly developed, for about two miles; and its known breadth varies from 170 to 340 yards. The only point at which it has been tunneled through in a transverse direction, is under Barn Hill enclosure, between Brixslade and Howler's Slade valleys, and its width is there about 200 yards. The upper surface of the seam of coal, to a considerable distance on each side of the Horse, undulates considerably, producing depressions called "lows," and great varieties in the thickness of the bed; but the pavement composed of the ordinary argillaceous deposits, which accompany the seam throughout the basin, preserves nearly its ordinary regularity.

The roof of the seam consists of the strong sandstone which usually reposes upon the Coleford High Delf, but a layer of black slaty substance is sometimes interposed between it and the coal. This sandstone extends to the surface, varying in thickness according to the undulations of the ground, but at one point over the Horse, the thickness is 94 yards. The sandstone sometimes passes into a conglomerate, containing fragments of coal, ironstone, and vege-

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[* See Phil. Mag., First Series, vol. lxvi. p. 307.—Edit.]

table remains; also quartz pebbles, similar to those which abound in "the pudding-stone," a deposit between the carboniferous limestone and the old red sandstone, and which attains a considerable elevation in the adjoining hills. The sandstone also encloses concretions of highly indurated, ferruginous sandstone, scattered irregularly throughout its mass; and angular fragments of obliterated casts of vegetable remains, formed likewise of highly indurated sandstone.

The coal under the lows is generally mixed with particles of the sandstone of the roof; but it contains no boulders, angular fragments, or pebbles, as asserted by some observers; the supposed boulders and fragments being, Mr. Buddle observes, the concretions and vegetable remains of the roof, alluded to in the preceding paragraph.

The fall of the Horse conforms to that of the strata or S. 31° E., but whether the "fault" rises with the seam of coal to the outcrop on the S.E. side of the basin, remains to be proved. In the transverse section, the bed of the Horse is nearly horizontal.

There are no indications on the surface by which the Horse can be traced beyond the limits explored under ground; and whether it produces any change in the overlying seams, can be determined only by future works. Mr. Buddle infers, that it does not descend any lower than the Coleford High Delf seam, in consequence of the evenness of the floor, and the entire absence in it of sandstone.

In its underground characters, the Horse is similar to the "washes" or aqueous deposits in many coal-fields, but it differs in not underlying a river bed, or being in the bottom of a valley, and in not extending to the surface. In the Newcastle coal-field all the "washes" cut through the whole of the strata, from the surface to that on which the wash reposes.

In the workings of the Park End Colliery in Park End High Delf seam, which is situated 50 fathoms higher in the series than the Coleford High Delf, and two miles to the S.E. of the point to which the Horse has been traced, a great succession of "lows" has been found in crossing the line of the Horse, but no fault corresponding with the Horse. The coal is deteriorated in the same manner as in the Coleford seam. This colliery is situated beyond the centre of the basin, and where the strata rise in the opposite direction. Future workings alone can determine if there be any connexion between the Horse and these "lows."

In the direction of the Horse there is also an extraordinary oval depression of the Coleford High Delf, the centre of the seam being 20 feet below the ordinary level; and it remains to be proved if the Horse presents the same characters under the depression as elsewhere.

From the phænomena exhibited by the Horse and the adjacent coal-seam, Mr. Buddle is of opinion, that the fault and seam occupy the site of a lake, which existed during the deposition of the latter, and that the carbonaceous matter, which forms the seam, was accumulated while the water was deep and tranquil; that the undulations on the surface of the coal were occasioned by the action of the water when the lake was discharged; and that the Horse occupies the bed of the stream by which the complete drainage of the lake was effected. The sandstone of the roof, and that which fills the lows, he conceives, on account of the fineness of the grain, were tranquilly deposited.

A paper was then read, entitled, "Remarks on the Structure of the Royal George, and on the Condition of the Timber, Iron, Copper, &c., recovered during the operations of Col. Pasley, in the Summer of 1839;" by Mr. Creuze, of Her Majesty's Dock Yard, Portsmouth, and communicated by Captain Basil Hall, R.N., F.G.S.

The Royal George was accidentally sunk at Spithead on the 29th of August, 1782, and as the specimens described in the paper were recovered during the summer of 1839, they had consequently been immersed in a tide-way of salt water fifty-seven years. She was the first ship of war built on the principles recommended by the committee appointed to inquire into the superiority of the vessels in the Spanish and French navies; and was commenced at Woolwich in 1746, and launched the 1st of February, 1756. The Royal George was consequently, when sunk (1782), twenty-six years old.

The great agent in the work of destruction of the timbers had been "the worm." This insect had gradually, by its innumerable perforations on every exposed portion of the wreck, destroyed the fibrous tenacity of the wood, and reduced it to such a state as to permit the wash of the tide to remove the surface layer by layer. The quantity which had been thus destroyed, Mr. Creuze considers, from the parts recovered, to have been almost the whole of the upper portion of the ship, including the topsides above the line of the middle deck ports; and he is of opinion, that in another half century the same agents might have destroyed every part of the hull above the surface of the mud, if Col. Pasley's operations had not been undertaken.

The timbers which had been protected by the mud, were found to be solid and firm; but the only exposed wood which has escaped the ravages of "the worm" is the ash of which the dead-eyes were made. A portion of one of these timbers, which accompanied the paper, and had formed a part of the exterior surface covered with mud, bore no marks of "the worm." The copper sheathing appears to have suffered very slightly, several whole sheets having been found to be of the average weight per square foot of that now used. This state of preservation Mr. Creuze assigns to galvanic action. The copper nails are also nearly perfect. The cast-iron guns which have been recovered, were so soft when first brought to the surface, that they were easily abraded by the finger nail to the depth of at least $\frac{1}{16}$th, and in some parts of $\frac{1}{8}$th of an inch; but they have gradually hardened on exposure to the atmosphere. The brass guns were apparently as sharp in their ornamental castings, and as sound, as at the period of their immersion. A fragment of tarred rope-yarn exhibited a remarkable instance of durability. It is supposed to have formed part of the sea-store of the Royal George, or of one of the cables used by Mr. Tracey in an attempt to raise the ship soon after she was lost. A piece of $2\frac{1}{2}$-inch cable-laid cordage, made from the yarns of this junk, bore 21 cwt. 3 qrs. 7 lbs.; a piece of similar cable, from yarn spun in 1830, bore only 20 cwt. 1 qr. 7 lbs.; but another manufactured from yarn spun in 1838, bore 23 cwt. 1 qr. 7 lbs.

The paper contained various details respecting the construction and measurement of the Royal George, and of the different descriptions of timber used in building the ship. Appended to the Memoir was also a catalogue of twenty-three specimens sent for exhibition, a portion of each of which was presented to the Society's Museum.

A letter, dated November, 1839, was afterwards read, addressed to Dr. Mantell by Mr. C. Hullmandel, "On the Subsidence of the Coast near Puzzuoli."

In 1813, Mr. Hullmandel resided during four months in the Capuchin Convent, which is situated at the entrance of Puzzuoli, and on the seaward side of the road towards Naples. The oldest friar, styled *il molto reverende*, then ninety-three years of age, informed Mr. Hullmandel, that when he was a young man, the road towards Naples passed between the convent and the sea, but that from the gradual subsiding of the soil it had been obliged to be changed to its present course. During Mr. Hullmandel's residence, the refectory and the entrance-gate were from six to twelve inches under water, whenever strong westerly winds prevailed. Thirty years previously such an occurrence never took place. The small wharf at Puzzuoli was also constantly under water during westerly winds. Mr. Hullmandel therefore infers, that as it is not probable the architect of the convent would have so placed the ground-floor as to expose it to inundations, or the builder of the wharf would have so constructed a landing-place as to render it liable to be overflowed; —a gradual subsidence of the soil has been going on for many years, and that this change tends to corroborate the opinion respecting the differences of relative level which have taken place in the Temple of Jupiter Serapis.

A notice was next read, "On part of Borneo Proper;" by G. Tradescant Lay, Esq. Communicated by the President.

The country visible in the background, on approaching the estuary into which the river of Borneo flows, is of variable, though nowhere of considerable elevation. Towards the east, however, is a remarkable range of mountainous ridges, rising one above another like steps, and trending, the author supposes, towards Kinibalu, the most lofty point in the island.

Borneo Proper consists, as far as Mr. Lay's observations extended, of sandstone; but near the mouth of the river is a little island on which coal is found, and called by the natives Pulu-cheomin, or Mirror island, in allusion, it is supposed, to the brightness of the coal.

Mr. Lay says, if he understood his informant rightly, a large supply of fuel might be obtained from the island. Lignite is also found by the natives in sandstone in a deep valley or ravine, not far from Borneo city, and believed by the author to be that called Kianggi. The bed extends obliquely from one side of the ravine to the other, forming an angle of about 45°, with the direction of a rivulet, which flows through the valley; and it is stated to be more than two yards in breadth. The valley is accessible by a path called Jalan-subrek, and conspicuous from the palace of the Sultan, but it is steep, rugged, and narrow. The distance from the water-edge is less than two miles. The whole of the peninsula lying on one side of the river is formed of very steep hills, which gradually become more lofty towards the south-west. Upon the main land, or opposite side of the river, the ridges are supposed to range at right angles to the mountains. They are composed, generally, of a soft sandstone, alternating with clay; but on the summit of one of the hills, Mr. Lay noticed the outcrop of a hard red sandstone, formed of round and angular masses of quartz, particles of black mica, and a ferruginous cement.

A paper was last read, "On some Geological Specimens from Syria;" by Mr. W. C. Williamson.

The specimens were sent to England by Mr. Heugh, to whom the author states, he is indebted for a few notes respecting the localities, whence they were obtained. The chief districts are the vicinity of Beyroot, especially Mount Gebeel Suneen, which forms the part of the Lebanon range immediately above Beyroot. The triangular tongue of land, on which that town is built, is about four miles in extent from the mountains to the coast, and it presents an undulating surface, some of the higher points attaining 500 feet above the level of the sea. The formation of which it is composed, is a hard cream-coloured limestone, which exhibits in the cliffs along the sea-shore numerous veins of flint; and it is in one part of the coast overlaid by a soft calcareous rock, occasionally 100 feet thick. The latter stone is easily wrought, and is employed as a building material, being better able to resist the effects of the earthquakes than the harder and more compact rock.

On ascending Gebeel Suneen from the flat plain, which extends along its foot, and is 400 feet above the level of the sea, the following rocks are passed over:—

Compact limestone..............................	1200 to 1500 feet.
Coarse siliceous conglomerate, containing thin seams of lignite, and fragments of siliceous wood................................	800
Compact limestone..............................	2000
A very ferruginous rock, composed of minute grains of sand, thickly coated with hydrated oxide of iron	50
A seam of oysters, which may be traced completely around the mountain	

Compact limestone, forming the summit of the mountain, about	100 feet.

In a break in the side of Gebeel Suneen, and extending for some distance along the upper part of the lower conglomerate, is a basaltic dyke, which shoots upwards into the compact limestone. It is about 100 yards wide, and begins, as well as terminates, very abruptly. Except a small hillock near the sea of Tiberias, this is stated to be the only trap seen by Mr. Heugh or his friends throughout the whole of Syria.

The fossils from the middle bed of limestone are generally casts, but are assigned by the author to the genera *Dolium*, *Buccinum*, *Nerinæa*, *Turritella*, *Venus*, *Crassatella ?*, *Hippurites*, *Trigonia*, *Cardium*, *Lucina*, *Nucula*, and *Spatangus*.

In a soft limestone at the village of Ba-abda, and also on the banks of the Zamies, are found large drusy geodes of quartz, and sometimes of chalcedony.

Among the other fossils contained in the collection, are specimens of *Clupea brevissima* (Agassiz, Tab. LXI., f. 6–9.). They occur in great numbers a little above Tripoli, on the way to the Cedars, and about thirty miles north of Beyroot.

None of the fossils, except the fishes, having been identified with described species, Mr. Williamson does not venture to determine the precise age of the beds from which they were obtained; but he is of opinion that the fossils are more nearly allied to the organic remains of the cretaceous series than to any other. The *Dolium*, he says, bears a strong resemblance to the *D. nodosum* of the English chalk, and a species of *Venus* to the *V. angulata* of the green sand. *Nerinæa*, he states, on the authority of Mr. Daniel Sharpe, are found near Lisbon associated with *Hippurites*.

LONDON INSTITUTION.—CONVERSAZIONI.

Jan. 20.—Mr. Grove on a powerful Voltaic Combination.

In reviewing the history of the voltaic battery from its first discovery to the present day, Mr. Grove showed how the chemical theory had led to all the improvements of that instrument. The original pile was defective, as, from the small intervening stratum of liquid, chemical action was soon exhausted; hence the troughs of Cruickshank and Wollaston. A second defect was the waste of local action, now remedied by amalgamated zinc, the inactivity of which is explained by the chemical theory. A third defect was the reaction occasioned by the precipitation of cations upon the negative metal, remedied by Mr. Daniell's introduction of sulphate of copper. However, as the chemical theory supposes that the power of this voltaic combination is as the affinity of oxygen or chlorine for zinc, minus its affinity for copper, a wide field was still open for the increase of power; thus solutions of silver, gold or platina, would be efficient did not their expense preclude their practical employment; the nitric, iodic, chloric and bromic acids, whose elements are united by a very feeble affinity, are still more effectual than any metallic solution, and fortunately the nitric acid, being commercially manufactured, is sufficiently cheap for the purpose.

The battery excited on this occasion was probably the most powerful ever seen, although of very small size; it consisted of 100 pairs of zinc and platina plates two inches by four, with interposed burned pipe clay diaphragms, and was charged with concentrated nitric and dilute sulphuric acid; its superficial relation to the great battery of Davy was as 1 : 40.

The arc of light was from three to four inches long and of great volume; its prismatic spectrum was of extreme brilliancy, and it was polarized and dipolarized by Mr. Grove, showing its identity with solar light.

[We understand that Mr. Grove has received the appointment of Professor of Experimental Philosophy in the London Institution.—Edit.]

XL. *Intelligence and Miscellaneous Articles.*

ON THE FOSSIL WAX OF GALLICIA. BY M. P. WALTER.

SEVERAL years since a fossil wax was discovered at Truskawietz, in Gallicia, or Austrian Poland, in beds of grit and bituminous clay, at a depth of two or three metres. I procured this substance, but want of leisure has prevented me from examining it: it is with regret that I feel it necessary to publish the little that I know respecting it; and I have decided to do so only in the hope that I shall hereafter be enabled to examine it completely.

This fossil is of a brownish black colour; its smell is penetrating and bituminous; it is slightly soluble in alcohol and æther; it fuses at 156° Fahr. When heated in a retort by means of an oil-bath, it first fuses, and at 212° it loses a little water; at 572° nothing whatever distils; above this, ebullition begins to occur, and continues up to 662°: oils first appear, afterwards a yellow-coloured substance, which forms the greater portion of the distilled product. This matter, freed from the empyreumatic oils by pressure through cloth, and dissolved in boiling æther, precipitates on cooling in the state of a beautiful white pearly substance.

It yielded by analysis,

Carbon	85·85
Hydrogen	14·28

This therefore is the composition of bicarburetted hydrogen and paraffin; and what induces me to believe that it is actually paraffin is, that sulphuric acid appears not to exert any action upon it. The examination of oils, formed by the distillation of fossil wax, may throw great light on the formation of naphtha and analogous compounds, which are probably derived from the decomposition of this kind of bodies.—*Ann. de Chim. et de Phys.*, 75, 212.

PHOSPHATE OF COPPER FROM HIRSCHBERG, ON THE SAALE, IN RUSSIAN VOIGTLAND. ANALYSED BY KÜHN.

This mineral is of a nodular appearance; the internal structure is concentrically fibrous, with conchoidal fracture; in colour it resembles malachite. Its specific gravity cannot be determined on account of the impurity of the material. The matrix is a brown iron stone, interspersed with veins of quartz, and containing here and there groups of quartz crystals.

The analysis was gone through in the following manner: some of the purest pieces, amounting to 0·407 gramme, gave 0·031 loss by ignition. After its solution in dilute nitric acid, there remained 0·030. The whole loss from ignition amounted, therefore, to 7·62 per cent.; and the foreign matter to 7·37. Now since 1·147 of pure matrix, by ignition, gave 1·031 of residue, so must 7·37 of matrix, brought to a red heat, have been obtained from 8·20, which was unignited. Thus, in the total loss from ignition, 0·83, which resulted from the matrix, are reckoned. The salt of copper had, consequently, from 91·80 of its first condition, lost 6·79 of water = 7·40 per cent. The oxide of copper, precipitated by boiling caustic potash, amounted to 0·268, or 71·73 per cent.; and since no other acid, besides phosphoric acid, could be found, this last, including what was lost, must have amounted to 20·87. Another portion, which weighed 1·946, was dissolved in dilute nitric acid, and precipitated by sulphuretted hydrogen; the sulphuret of copper was then converted into oxide of copper, which weighed 1·354 = 69·58 per cent.; the liquor, filtered from the sulphuret of copper, remained, on the addition of caustic ammonia, colourless and clear.

From a third unweighed portion, oxide of copper weighing 0·374, and lead precipitate weighing 0·567, were obtained. If, in the last, 0·100 of phosphoric acid must be assumed, so, in the first experiment, where 71·73 per cent. of oxide of copper were obtained, 19·21 of phosphoric acid were contained. The water, besides loss, amounted to 9·06.

The result was, consequently,

Oxide of copper	71·73	69·58	71·73
Phosphoric acid	(20·87)		19·21
Water	7·40		(9·06)

The simplest proportion of the formula is $3\,\mathrm{Cu\,O} + \mathrm{P\,O}_{2\frac{1}{2}} + 1\frac{1}{2}\,\mathrm{H\,O}$, and of the quantity of oxygen 6 : 5 : 3. From experiment the following formula may be calculated:

$$1\tfrac{1}{2}\,(\mathrm{Cu\,O} + \mathrm{H\,O}) + (1\tfrac{1}{2}\,\mathrm{Cu\,O} + \mathrm{P\,O}_{2\frac{1}{2}}):$$

or, after Berzelius's method and numbers,

$$3\,(\mathrm{Cu\,O} + \mathrm{H\,O}) + (3\,\mathrm{Cu\,O} + \mathrm{P_2\,O_5}).$$

Some additional grounds for this formula, as well as some analogues, shall be published hereafter.—*Annalen der Pharmacie,* Vol. xxxiv. No. 2.

ON MINIUM. BY M. LEVOL.

Although minium has been an object of research by several chemists, opinions are still divided, not only as to its analysis, but also as to the manner in which its true constitution ought to be represented.

M. Levol states that, having had occasion to examine several samples with respect to the commercial value, his experiments tend to confirm the views generally entertained with respect to this substance, namely, that it is not a peculiar oxide, but a definite compound of protoxide and peroxide.

M. Levol agrees with M. Dumas, and it may be added also, with the previous determination of Dalton, that the composition of minium is represented by $PbO^2 + 2PbO$, and he finds that this is also its composition obtained by the two following new processes:

The first consists in calcining, in a platina or silver crucible, a mixture of 100 parts of protoxide of lead, prepared by calcining the carbonate, 25 of chlorate of potash, and 200 of nitrate of potash; this last salt is employed for the purpose of rendering the mixture fluid, without occasioning loss of chlorate of potash.

In operating in this way, the action of the oxygen upon the oxide of lead is so effectual, that it is converted into peroxide; and this oxide may be thus procured with the greatest readiness. If the operation be carried on till the mixture becomes nearly red-hot, the swelling diminishes, the mass thickens, and minium is formed. It is sufficient to boil the residue in a solution of potash or soda, and to wash it well, in order to obtain pure minium of the composition above stated. The product is in a state of minute division, of a fine red colour, with a shade of orange, like the finest minium obtained in commerce.

Minium may also be obtained in the moist way, by boiling for an hour or two a solution of an alkaline plombate with binoxide of lead in fine powder; the colour of the binoxide becomes gradually lighter, and eventually an ochre-red powder is obtained; this is merely minium, mixed with a little binoxide, which has escaped the action of the plombate; it is easily got rid of by digestion in a solution of oxalic acid, which decomposes the peroxide without acting upon the compound, and the oxalate of lead is afterwards separated by potash. The product thus obtained has always a deeper red tint than that of the minium prepared in the dry way; but it becomes brighter, and more nearly resembles the latter when it is triturated with water; their composition is similar, and the difference of colour appears to be entirely owing to texture; there is, in fact, some appearance of crystallization in the minium obtained in the moist way.

M. Levol's analyses were performed by putting the minium into excess of nitric acid of sp. gr. 1·114, and agitating the mixture frequently during twenty-four hours; the temperature must not be raised by the action, or otherwise a portion of the binoxide is decomposed, and a small quantity is even dissolved, which gives the solution a violet colour. It remains after this operation merely to weigh the residual binoxide, and to determine that it was totally soluble in protonitrate of mercury, which does not act upon minium.

The following considerations induced M. Levol to believe that minium is a compound of two oxides. In supposing that minium is a peculiar oxide intermediate between these oxides, it is inexplicable how it happens that minium cannot be converted into binoxide by calcination with chlorate of potash, an effect which it readily produces with the protoxide of lead.

Oxalic acid constantly converts binoxide of lead into protoxide, but does not alter minium; and this is at once a method of purifying, and a good characteristic of minium. As this acid, as well as protonitrate of mercury and sulphurous acid, reduce the binoxide of lead to protoxide, and has no action on minium, it may be inferred, not only that minium is a compound of the two oxides, but that it is a compound of remarkable stability.—*Ann. de Chim. et de Phys.*, 75, 108.

ACTION OF ANHYDROUS PHOSPHORIC ACID UPON ANHYDROUS CAMPHORIC ACID. BY M. P. WALTER.

The action excited by anhydrous phosphoric acid upon anhydrous camphoric acid, in nowise resembles that of Nordhausen sulphuric acid upon anhydrous camphoric acid: the anhydrous sulphuric acid acts, so to speak, in a less destructive manner; it re-acts according to the rules of substitutions. Instead of taking carbon from the camphoric acid, it adds the elements of sulphurous acid, and from this re-action a new acid arises; whereas, in causing anhydrous phosphoric acid to act upon camphoric acid, it attacks it in all its molecules and gives rise to several compounds. If several layers of anhydrous phosphoric acid, and anhydrous camphoric acid, be arranged in a tubulated retort, with a tubulated receiver, having a bent tube adapted to it, and immersed in mercury, on the cautious application of heat to the retort, a considerable and continuous disengagement of gas takes place; when this ceases, a liquid substance runs down the neck of the retort into the receiver; this has a slight yellow colour, and a penetrating, but not disagreeable odour; when repeatedly rectified from anhydrous phosphoric acid it is rendered perfectly colourless. In the bottom of the retort there remains a black matter, which is strongly acid.

The gas which is formed in this re-action is of a compound nature; it is a mixture of carbonic acid and oxide of carbon, but they are in indefinite proportions. Several experiments proved that, for one volume of carbonic acid, there were four of oxide of carbon. The liquid is a carburetted hydrogen: two analyses, performed with two different products, gave the same quantity of carbon, namely, 88·4 and 88·2 per cent.; but the quantity of hydrogen varied half per cent.; in one analysis 11·6, and in the other 11·07 were obtained. If the first analysis be correct, the carburetted hydrogen has the same centesimal composition as oil of turpentine; but I do not think that it is oil of turpentine, though its formation may be explained by means of the formula of anhydrous camphoric acid; I am more inclined to believe that the [carburetted] hydrogen in question is a species of naphtha which contains 89 per cent. of carbon, and that the smaller quantity of carbon, found in my analyses, is due to the presence of a little phosphuretted hydrogen, which it is difficult to separate from the carburetted hydrogen. In the latter case, in order to explain the formation of this carburetted hydrogen by means of the formula for anhydrous camphoric acid, the formation of water must be admitted in the re-action. I should have decided this question long since, if the quantity of carburetted hydrogen, obtained during the re-action, had been more considerable than it actually was.—*Ann. de Chim. et de Phys.*, 75, 212.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1841.

Chiswick.—Jan. 1. Hazy: fine with clouds. 2. Rain: clear and fine: hurricanes at night. 3. Thunder-storm about 7 A.M., accompanied with large and vivid flashes of lightning, rain, hail and sleet, and high wind, which soon after subsided into a perfect calm. 4. Sharp frost: slight fall of snow: clear at night. 5. Densely overcast: snow: large lunar halo in the evening. 6. Hazy. 7. Intense frost. 8. Dense fog: severe frost. 9. Intense frost. 10. Overcast: slight haze: rain at night. 11. Overcast. 12. Cloudy: clear. 13. Foggy: rain: fall of snow. 14. Cold haze: rain: sleet and snow. 15. Rain. 16. Thawing rapidly: occasioning inundations, the frozen crust preventing the water from sinking into the earth. 17. Continued thaw. 18. Rain. 19. Overcast. 20. Cloudy and cold: sharp frost at night. 21. Frosty: fine. 22. Frosty: rain at night. 23. Clear. 24. Boisterous: cold and dry. 25. Clear and frosty. 26. Overcast and fine. 27. Very fine. 28. Cloudy. 29. Fine. 30. Hazy. 31. Foggy: rain.

Previously to the thaw, in the beginning of the month, the frost had penetrated in some soils to a depth of 12 inches.

Boston.—Jan. 1. Cloudy. 2. Fine. 3. Cloudy: stormy with lightning and rain early A.M. 4. Cloudy: snow early A.M.: stormy with rain P.M. 5. Stormy. 6, 7. Fine. 8. Fine: thermometer 17°·0 three o'clock P.M. 9. Fine: thermometer 28°·0 three o'clock P.M. 10. Cloudy: large fall of snow early A.M. 11. Cloudy: snow early A.M. 12. Cloudy. 13. Fine: rain P.M. 14, 15. Cloudy. 16. Cloudy: snow early A.M.: rain P.M. 17. Fine. 18. Cloudy. 19. Cloudy: rain early A.M. 20. Snow: snow P.M. 21. Cloudy: snow early A.M. 22, 23. Fine. 24. Stormy: heavy snow-storm P.M. 25, 26. Fine. 27. Fine: beautiful morning. 28. Cloudy. 29. Fine. 30. Cloudy. 31. Rain: rain early A.M.: snow-storm P.M. N.B. The 8th of this month was the coldest day since Jan. 1, 1820.

Applegarth Manse, Dumfries-shire.—Jan. 1. Slight showers. 2. Slight showers: frost in the morning. 3. Snow-storm. 4. Snow-storm and frost. 5. Snow-storm. 6. Fair: snow lying. 7. Snow-fall: frost very keen. 8. Snow-fall slightly: frost keen. 9. Thaw, with slight snow. 10. Snow and frost again. 11. Fair: snow lying: thaw P.M. 12. Fair: but freezing hard. 13. Fair: freezing. 14, 15. Fair. 16. Storm of snow, sleet and rain. 17. Thaw: heavy rain P.M. 18. Frost again: clear. 19. Frost again. 20. Frost again: Aurora borealis. 21. Thaw: drizzling rain. 22. Wet and boisterous. 23. Wet and boisterous: slight snow-fall. 24. Fair: frosty: slight snow-fall. 25. Frost A.M.: drizzle P.M. 26. Thaw and thick fog. 27. Shower in afternoon. 28. Fair and fine: snow melting. 29. Drizzling. 30. Thick fog all day. 31. Clear and cold: moist P.M.

Sun shone out 25 days. Rain fell 10 days. Snow 8 days. Frost 16 days. Fog 2.

Wind north 2 days. North-east 5½ days. East 2 days. East-south-east 3½ days. South-east 1½ day. South-west 4 days. West-south-west 1 day. West 4 days. West-north-west 2½ days. North-west 3 days. North-north-west 2 days.

Calm 8 days. Moderate 8 days. Brisk 3 days. Strong breeze 7 days. Boisterous 4 days. Stormy 1 day.

Mean temperature of the month...........	31°·45
Mean temperature of January, 1840......	37 ·80
Mean temperature of spring-water	42 ·00
Mean temperature of spring-water, 1840..	43 ·30

Meteorological Observations made at the Apartments of the Royal Society 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, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. Jan.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dum-fries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·962	29·942	29·882	29·50	29·72	29·76	40·3	44·2	37·5	45	34	39	47	38	W.	W.	calm	W.	·016	...	...	...	34
2.	30·096	30·086	29·816	29·70	29·95	29·61	39·7	45·5	40·0	45	32	35	43	33	W.	W.	calm	W.	...	·15	...	...	35
3.	29·304	29·302	29·084	28·86	29·01	28·74	35·8	44·8	36·0	39	21	37	44	30	S.	NW.	W.	WNW.	·166	...	·13	...	35
4.	29·002	29.185	28·978	28·75	29·20	29·46	30·4	38·7	29·8	35	29	28·5	35	29	S.	NW.	calm	NE.	·061	...	...	...	32
5.	29·308	29·376	29·316	29·10	29·53	29·60	31·8	36·3	30·0	33	22	33	35	27½	NW.	N.	NW.	NE.	·036	...	·47	...	29
6.	29·528	22·631	29·518	29·28	29·55	29·61	27·7	34·4	28·3	30	12	27	29	19	N.	SE.	calm	E.	...	...	...	...	27
○ 7.	29·696	29·760	29·698	29·45	29·58	29·58	19·7	29·8	20·7	27	6	13·5	19	1	NW.	NE.	calm	NE.	...	...	...	0·00	21
8.	29·884	29·893	29·866	29·70	29·60	29·67	19·7	27·3	19·5	20	6	9·5	30½	8½	SSW.	NW.	calm	SE.	...	...	...	...	24
9.	29·706	29·738	29·441	29·47	29·37	29·07	21·2	21·4	14·9	33	27	15·5	34	18	SW.	S.	calm	ESE.	...	...	...	...	18
10.	29·230	29·247	29·089	29·04	29·00	28·94	32·4	32·8	22·2	39	31	27	33½	27	SE.	SE.	calm	ESE.	...	·40	...	...	25
11.	28·864	29·064	28·864	28·66	28·90	29·05	34·3	37·7	32·8	39	33	32·5	35½	23½	S.	SW.	calm	ESE.	·205	·02	...	...	28
12.	29·366	29·578	29·364	29·02	29·19	29·38	35·2	37·2	33·0	39	22	34	34½	28	W.	SW.	calm	NE.	...	...	...	...	30
13.	29·550	29·563	29·324	29·27	29·38	29·38	35·2	38·0	32·8	38	32	27	33½	10	S.	E.	calm	NW.	...	·29	...	...	32
☾ 14.	29·370	29·398	29·176	29·16	29·40	29·46	35·2	39·2	34·0	36	32	35	29½	14	NE.	NE.	calm	NNW.	·352	·80	·11	0·00	32
15.	29·578	29·687	29·559	29·29	29·55	29·57	34·8	37·2	33·3	39	31	36	31	15½	NW.	SE.	calm	NE.	·700	·15	...	...	33
16.	29·654	29·631	29·365	29·39	29·40	28·93	37·4	38·2	33·6	52	40	33·5	37½	30	E.	SW.	calm	E.	·088	·16	...	...	35
17.	29·632	29·729	29·605	29·20	29·18	29·34	47·4	51·8	37·0	52	45	42	40½	35½	SE.	SW.	calm	WSW.	·166	·02	·38	...	41
18.	29·736	29·725	29·685	29·29	29·52	29·70	46·8	52·3	46·7	47	33	43·5	35	31½	S.	W.	calm	N.	...	·24	...	...	42
19.	29·812	29·918	29·778	29·45	29·80	29·91	35·5	48·7	35·0	36	28	35	34	24	NNW.	N.	calm	NNW.	·227	·02	·05	...	37
20.	30·050	30·182	30·019	29·73	30·03	30·14	34·3	37·8	32·6	34	22	30	32½	23	NW.	N.	N.	N.	·022	...	...	...	32
21.	30·410	30·505	30·374	30·00	30·19	30·20	29·8	34·7	28·4	38	25	29	39	23½	W.	NW.	calm	NW.	...	...	...	...	29
● 22.	30·396	30·303	30·157	29·96	29·97	29·70	34·5	39·4	30·2	44	32	36	42	35½	S.	SW.	calm	SW.	...	·10	...	2·35	34
23.	30·084	30·108	30·001	29·65	29·85	29·75	39·3	44·7	34·3	43	32	36	38	31	NW.	NW.	N.	WNW.	·100	...	...	...	37
24.	29·796	30·161	29·796	29·41	29·77	30·13	36·7	42·0	36·0	38	26	34	34	26½	NW.	N.	N.	NW.	...	·02	...	...	33
25.	30·350	30·336	30·195	30·00	30·10	29·88	31·5	38·0	31·0	40	32	28·5	35	21½	SW.	W.	calm	SW.	·033	·01	·15	...	31
26.	30·000	30·000	29·988	29·59	29·70	29·70	42·7	43·6	31·0	49	43	38	42	36	WSW.	SW.	calm	SW.	...	...	...	...	35
27.	30·050	30·225	30·015	29·58	29·77	29·97	47·3	48·6	43·0	53	34	47	48	38	SSW.	W.	calm	W.	...	...	...	...	40
28.	30·322	30·300	30·250	29·83	30·12	30·14	37·8	52·8	36·0	44	28	41	45½	37	SW.	W.	calm	WNW.	...	...	...	...	36
29.	30·202	30·208	30·176	29·80	30·10	30·11	37·7	44·8	36·0	46	32	36·5	42½	41	NW.	N.	calm	W.	...	·02	...	0·55	37
☽ 30.	30·282	30·251	30·212	29·90	30·08	30·13	40·2	44·2	37·9	40	37	37	43½	38½	S.	W.	calm	S.	...	·08	...	...	37
31.	30·210	30·190	30·165	29·82	30·20	30·35	40·8	41·7	40·2	42	28	39	42	34	S.	E.	calm	NE.SE.	·052	·12	·26	...	38
Mean.	29·788	29·813	29·701	29·44	29·635	29·647	35·3	40·3	32·7	39·90	28·61	32·7	37·0	26·7					Sum. 2·224	2·60	1·55	0·92	Mean. 33

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

[THIRD SERIES.]

APRIL 1841.

XLI. *On the Voltaic Decomposition of Aqueous and Alcoholic Solutions.* *By* Arthur Connel, *Esq., F.R.S.Ed., Professor of Chemistry in the United College of St. Salvator's and St. Leonard's, St. Andrew's*[*].

HAVING been engaged at intervals for several years in researches on the nature of the changes which take place in solutions in the principal solvents under voltaic agency, and having, I conceive, attained some general results, I propose at present to give a condensed view of these experiments and conclusions, the more detailed account of them being contained in two papers already published in the Transactions of the Royal Society of Edinburgh[†], and a third in the course of publication. The principal objects which I have in view at present, are to suggest one or two general rules relating to the secondary voltaic decomposition of dissolved combinations of elementary bodies; to illustrate the state in which the haloid salts are dissolved; and to offer some elucidations of the conducting power of solutions.

I. *Aqueous Solutions.*—It is well known that by the employment of the voltameter Mr. Faraday was enabled to show that in many instances of voltaic action on aqueous solutions, the changes produced on the dissolved substance were the results not of the direct action of the electric current, but of the secondary agency of the products of the direct decomposition of water[‡]. This method, however, is not capable of resolving all cases, many of its results being explicable equally on the idea of a primary as of a secondary decomposition of the

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[*] Communicated by Sir David Brewster.

[†] Vol. xiii. and xiv.

[‡] Experimental Researches, Seventh Series. [L. and E. Phil. Mag., vol. v.]

dissolved body; for the dissolved substance is often equally capable, with water, in so far as respects composition and atomic constitution, of yielding a definite proportion, whether of oxygen or of hydrogen; and the non-appearance of one or other of these elements at either pole is often equally explicable on the idea of the dissolved substance not containing such element, and on that of its being derived from water, and subsequently entering into union with the elements of the dissolved substance. Many cases, however, occur, in which, from peculiarity of atomic constitution, taken in conjunction with the proportion of gases evolved at the poles, we can have little hesitation in pronouncing in favour of a secondary action. Such cases, I conceive, are those of nitric, sulphurous and the organic acids, and of ammonia, as exemplified in Mr. Faraday's experiments.

Accordingly, although in such instances as these Mr. Faraday decided with reason in favour of such an action, yet he was so far from laying down as a general rule, that in all cases of solution of acids, alkalies and other primary combinations of elements, the dissolved body was never directly decomposed by galvanic agency, that he distinctly gave his opinion that the hydracids are directly decomposed*, and inclined to the view also that the haloid salts were in the same situation.

By adopting other methods, with the occasional aid of the principle of the voltameter, I conceive that I have been enabled to extend the rule of secondary action to all cases of aqueous solution of primary combinations of elementary bodies.

The usual method followed, was to endeavour to separate some one of the constituents of the dissolved body, on the assumption that that body was directly decomposed, and to exhibit it in a distinct shape. The solution, contained in a small tube of the capacity of about one and a half drachm, was connected with another tube of the same capacity filled with distilled water, the connexion being made by a short and thick bundle of asbestus previously well washed with dilute acid and then with distilled water, with which latter fluid it was still moist when the connexion was made, and the level of the solution being a fraction of a line lower than that of the distilled water.

The water was then connected with the side of the battery, to which the constituent of the dissolved substance would naturally pass if that substance suffered direct decomposition,

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* *Ubi sup.* §. 763–4.

and the solution itself with the other side, as in fig. 1. In this way the constituent must necessarily be exhibited in the water, either separately or in a state of combination with one or other of the elements of the water, which new combination would in its turn suffer decomposition, whether primary or secondary, if admitting it. The readiest and simplest illustration of the efficacy of this method, is to connect the solution of any ordinary salt, such as sulphate of soda, with two tubes containing distilled water, and one of the latter with the positive and the other with the negative side of the battery, as in fig. 2, where B contains the saline solution, and A and C distilled water; when by the passing of acid into C and of alkali into A, the constituents will be exhibited in a separate state.

Fig. 1.

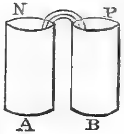
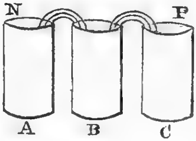


Fig. 2.



The first case which shall be adverted to, is that of a simple binary combination having neither acid nor alkaline qualities. If an aqueous solution of bromide of iodine is mixed with a solution of starch and acted on voltaically, iodine immediately separates at the negative pole, forming the usual blue combination with starch. Apparently, therefore, the bromide is resolved by the action of the current into its constituents, iodine going to the negative, and bromine to the positive. It is easy, however, to show that this effect is entirely a secondary one: the mixed solution of the bromide of iodine and of starch was placed in the tube B, fig. 1, and the starch solution in A, the asbestus being moistened with the starch solution, B being made positive, and A negative by fifty pairs of two-inch plates[*]. Effervescence speedily ensued from both poles, but after forty minutes' action not a trace of any blue colour was observed in either tube. The battery was then reversed, the platinum foil in B being now connected with the negative side, and that in A with the positive side. Within two minutes the blue combination appeared round the negative foil; the effervescence ceasing at that pole, but continuing at the positive. Had the bromide been directly decomposed, iodine

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[*] All the batteries employed in these experiments were on Cruickshank's construction.

ought to have been liberated, and the blue colour produced in one or other of the tubes before reversed; but as this change did not occur till after reversal, the effect was due to nascent hydrogen at the negative pole. This hydrogen must have combined with bromine if the bromide is dissolved as such, as is usually held, and is most probable; or with oxygen if the whole or a part of it decomposes water, forming hydrobromic and iodic acids *. The appearances of effervescence lead to the same conclusion. The oxygen liberated at the positive pole in B before reversal must have come from water, and implies an equivalent quantity of hydrogen passing into A, and uniting with the oxygen of water in the course of decomposition in A. Now as this hydrogen did not appear on reversal of the battery, it must have been employed in liberating iodine.

The next case to be considered is that of the oxyacids. When we observe the radical of such an acid solution separating at the negative pole and oxygen at the positive, we are at first sight inclined to suppose that the acid itself has been the subject of direct voltaic decomposition; and accordingly it has been often thought that sulphur and phosphorus, when they are observed in such circumstances, are the result of direct decomposition of the corresponding acids. In like manner we might conclude that the iodine which instantly separates at the negative pole when a solution of iodic acid is acted on, is the result of primary decomposition; but a simple experiment at once shows the action to be secondary. A mixed solution of iodic acid and of starch was placed in the tube B, fig. 1, and a solution of starch in A, the connexion being by asbestus moistened with the latter solution, and B made positive and A negative by fifty pairs of two-inch plates. Effervescence ensued from both poles, but after half an hour's action no formation of blue was observed anywhere in either tube. The battery was then reversed in the manner above described. In two minutes blue matter was formed on the now negative foil in the acid solution with little or no effervescence at that pole, but effervescence from the positive foil. Thus in the first position of the battery, as there was no direct decomposition of the acid, iodine did not pass towards the positive pole; whilst after reversal, iodine immediately appeared by the reducing action of hydrogen derived from directly decomposed water. The analogy of this ex-

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* When both poles are plunged into the solution, and the battery is in fresh action, there is effervescence at both poles, a part of the hydrogen only being engaged in producing the secondary action.

periment may evidently be extended to sulphuric, phosphoric and other oxyacids.

This result may be further illustrated on the principle of the voltameter. A current of the same power as above was passed through a solution of iodic acid, and one of sulphuric acid diluted with twelve parts of water. Iodine without any elastic fluid separated from the negative pole of the iodic solution, and from the other poles the following quantities of gas were collected.

Negative of sulphuric solution	·33	cubic inch.
Positive of do. do.	·12	,,
Positive of iodic solution	·14	,,

This result can scarcely be explained except on the idea of the direct decomposition of water only; for similar quantities of oxygen were liberated from both solutions, notwithstanding the difference of atomic constitution of the two acids; and whilst a corresponding quantity of hydrogen was evolved from the sulphuric solution, that from the iodic did not appear, and was plainly occupied in reducing iodine.

In like manner when the same current was passed through dilute sulphuric acid and a solution of boracic acid, it was found that very nearly the same relative quantities of oxygen and hydrogen were evolved from both solutions in the same time, notwithstanding the difference in the atomic constitution of the two acids. There was therefore little doubt that in both solutions the water and not the acid had suffered decomposition.

We turn now to the hydracids. When a solution of muriatic or hydriodic acid, not too weak, is acted on under ordinary circumstances, there is an immediate liberation of chlorine or iodine without oxygen at the positive pole, and of hydrogen at the negative. The first view which strikes one, is that the hydracid is directly resolved into its elements; and this is the view which Mr. Faraday has adopted. It is not, however, difficult to show that the decomposition is a secondary one. A moderately strong solution of muriatic acid was placed in the tube A, fig. 1, and distilled water in B, the connecting asbestus being moistened with the latter fluid, A being made negative, and B positive by a power of fifty pairs of two-inch plates. There was speedy effervescence from both poles, and in about a quarter of an hour a trace of acid was found in the positive tube, which went on increasing; but it was not until after nine hours' action that a feeble and doubtful odour of chlorine could be detected in that tube. When the conducting power of the water in B was improved by adding a few drops of sulphuric acid, a brisker effervescence than before was observed at the poles, but still in ten minutes no trace of chlorine was observed in either tube by the smell or by test paper. The battery was then reversed, when in less than two minutes a decided smell of chlorine was observed in B, and in ten minutes test paper was bleached when plunged into it. It was also ascertained that no gas but chlorine was liberated from the positive pole subsequent to reversal. When the power was seventy pairs of four-inch plates and the liquid in B distilled water, acid was detected at the positive pole in three or four minutes; but after half an hour's action no bleaching could be observed in either tube, although a feeble odour of chlorine was latterly observed. The battery was then reversed, when instantly a pungent odour of chlorine arose and test paper was bleached.

When a moderately strong solution of hydriodic acid was placed in A and distilled water in B, A being negative and B positive, and the power fifty pairs of two-inch plates, a slight effervescence ensued from both poles, and during the first ten minutes there was no discoloration of the liquid in either tube. A slight yellow tint then appeared in the positive liquid and no change in the negative, and shortly after a just perceptible acid reaction was observed in the positive liquid. In a quarter of an hour the battery was reversed. Instantly, notwithstanding the diminished action of the battery, the liquid around the positive foil became brown from liberated iodine, without the least effervescence from that pole. When the level of the hydriodic solution was about a line lower than that of the water, no discoloration took place in either tube in a quarter of an hour, but it was instantly observed on reversal. When the power was seventy pairs of four-inch plates, the discoloration began in B in about five minutes, with acid reaction. On reversal there was instant discoloration in A, and no gas.

In these experiments, it seems obvious that the non-appearance of chlorine for so long a period, and its speedy appearance after reversal with cessation of oxygen, show its secondary origin from the reducing action of oxygen on muriatic acid. If before reversal a trace of it ultimately appeared, its origin was plainly due to the acid which had been some time before drawn over to the positive side. In the case of hydriodic acid the iodine was in like manner due to the reducing action of oxygen; and if it appeared sooner before reversal than chlorine, that circumstance was due to the circumstance of hydriodic acid being a much weaker combination, and more readily reducible than muriatic acid. Even if we suppose that chlorine or iodine in passing by direct voltaic agency towards the positive side reacted with the hydrogen of the water in B, it is plain that the hydracid so formed should have been immediately decomposed again, and the chlorine or iodine should have speedily appeared at the pole in a separate form, and *that* before acid appeared; whereas in the case of muriatic acid, the chlorine did not appear till considerably after acid had passed into B, and much later than iodine appeared in the case of hydriodic acid, although the constituents of the latter are less decidedly opposed to one another in electric nature than those of muriatic acid, and therefore ought reasonably to be of more difficult voltaic decomposition. The quantity of chlorine or iodine which appears is not in proportion to the absolute quantity of acid which passes over, but to the facility with which that acid is reduced. The fair and natural interpretation of the whole phænomena, is evidently afforded on the view of a secondary action on the hydracid.

The experiment with hydriodic acid was varied by connecting two tubes, B and C, fig. 2, each filled with distilled water, with the tube A, containing the hydriodic solution, the power being seventy pairs of four-inch plates. Slight effervescence ensued at both poles, but during forty minutes' action no discoloration appeared in either tube. Soon after a brown tint appeared in C, with acid reaction at the positive pole. In ten minutes more the battery was reversed, when iodine without any gas instantly appeared in A. All these appearances are quite in conformity with the above views.

In regard to metallic oxides it is not easy to obtain such direct experimental evidence, because the metals of such as are soluble in water react on the solvent; but we may take the decomposition of metallic oxides as contained in soluble salts, during which the metal frequently appears at the negative pole, as illustrating the action on solutions of such oxides as are dissolved by pure water. It is now pretty generally admitted, that when metal appears in solutions of metallic salts at the negative pole, it is due to the reducing action of nascent hydrogen; and this opinion I have verified directly by finding, that when solutions of sulphate of copper, or muriate of zinc or nitrate of silver, were made positive by fifty pairs of two-inch plates, and connected by asbestus with distilled water which was made negative, neither metal nor oxide was carried towards the negative pole during half an hour's action; whilst on reversal of the battery, the non-negative foil in a quarter of an hour was found to be more or less covered with reduced metal. Thus the circumstance that in the one position nascent hydrogen was produced in the metallic solution, and in the other in the distilled water, evidently was the cause of the difference of the result. The experiment with muriate of zinc and nitrate of silver was varied by placing the solution in a bent tube and pouring distilled water over it, the negative wire being alternately plunged in the water and in the solution. The results before and subsequent to reversal of the battery were the same as in the experiments with asbestus; the only difference being, that in the experiment with zinc solution, a little oxide of zinc appeared to be deposited at the confines between the water and the solution.

I am aware of the circumstance mentioned by Davy, that the connecting asbestus in an experiment with nitrate of silver acquired a film of silver, water being on the negative side; but without doubting for a moment the accuracy of the statement, I do not hesitate to conclude, from a consideration of my own results, that the silver had resulted in the single instance mentioned from the reduction of silver by the secondary action either of light, or of the hydrogen evolved, by the powerful battery employed, in the negative liquid.

I thought that a negative pole of tellurium in the metallic solution might, by combining with hydrogen, have prevented the formation of metal at that pole: but I found that copper and zinc were deposited even in those circumstances; showing that nascent hydrogen, where the metal held in solution is easily reducible, rather unites with its oxygen than with the tellurium.

In regard to haloid salts, I shall afterwards endeavour to show that they are not dissolved by water as such, but as hydracid salts. In the mean time I shall mention, that proof of exactly the same description as that which has been detailed in regard to the hydracids, was obtained in regard to solutions of chlorides and iodides of potassium, showing that the chlorine and iodine which appear when they are acted on voltaically has a secondary origin.

There is also a very simple experiment of a different description which leads to the same conclusion. When an aqueous solution of iodide of potassium was acted on, using a positive pole of zinc, by fifty pairs of two-inch plates, instead of iodine separating at that pole, as is the case when one of platinum is employed, there was a speedy and copious deposition of oxide of zinc from the *positive* pole, with only a bubble or two of elastic fluid, but with brisk effervescence from the negative; appearances which can only be explained on the idea of the secondary origin of the iodine, and are conformable to the view of the direct decomposition of water, the oxygen of which combines with the zinc when the pole is of that metal, or with hydrogen of the acid of the salt when platinum is used. With chlorides of potassium and calcium, there was also a deposit of oxide of zinc at the positive pole, and sometimes a part of the oxide of zinc was taken up by the acid of the hydracid salt and carried to the negative side.

I conceive that a sufficient number of cases has been investigated to warrant the general conclusion, "that when aqueous solutions of primary combinations of elementary bodies are submitted to voltaic agency, the dissolved substance is not decomposed by the current, but only the solvent." The rule of course does not embrace combinations of the second order, such as oxysalts, which as every one knows are resolved into their constituent acid and alkali under voltaic agency.

[To be continued.]

XLII. *A new and more general Theory of Multiple Roots. By* J. J. SYLVESTER, *F.R.S. &c., Professor of Natural Philosophy in University College, London**.

I SHALL begin with developing the theory of polynomials containing perfect *square factors*, one or more.

First, let us proceed to determine the relations which must exist between the coefficients of such polynomials, and afterwards show how they may be broken up into others of an inferior degree.

A parallelogram filled with letters standing in *one* row is intended to express the product of the squared difference of the quantities contained. Thus $\left(\overline{\underline{\,a\ b\,}}\right)$ indicates $(a-b)^2$ $\left(\overline{\underline{\,a\ b\ c\,}}\right)$ is used to indicate $(a-b^2) \cdot (a-c)^2 \cdot (b-c)^2$, and so forth.

Suppose now that two of the roots $e_1\ e_2 \dots e_n$ belonging to the $= {}^n f x = 0$ are equal to one another, it is clear that $\left(\overline{\underline{\,e_1\ e_2 \dots e_n\,}}\right) = 0$; and moreover is a symmetric function, and can be calculated in terms of the coefficients of $f\,x$.

Next let us suppose that we have two couples of equals, (as for instance a and b, two of the roots equal, as also c and d two others) it is clear, that on leaving any one of the roots out, the $(n-1)$ that are left will still contain one equality, and therefore we have

$$\left(\overline{\underline{\,e_2\,e_3 \dots e_n\,}}\right) = 0 \quad \left(\overline{\underline{\,e_1\ e_3 \dots e_n\,}}\right) = 0 \dots \left(\overline{\underline{\,e_1\ e_2 \dots e_{n-1}\,}}\right) = 0.$$

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* Communicated by the Author.

None of the parallelogrammatic functions above taken *singly*, are symmetric functions of the coefficients, but their sum is; so also is the sum of the product of each into the quantity left out.

Now in general, suppose that the polynomial fx contains r perfect square factors, so that we have r couples of equal roots belonging to the equation $fx = 0$, it is clear that

$\left(\overline{\underline{e_r\, e_{r+1} \cdots e_n}}\right)$ and all the other $\dfrac{n(n-1)\ldots(n-r+2)}{1\,.\,2\,\ldots\,(r-1)}$ functions of which it is the type are severally zero. Moreover, the sum of these or the sum of the products of each by *any* symmetrical function of the $(r-1)$ letters left out will be a symmetrical function of the coefficients of the powers of x in fx. To express now the *affirmative** conditions corresponding to the case of there being r pairs of equal roots, we *might* employ the r equations,

$$\begin{aligned} &\left(\overline{\underline{e_1\, e_2 \cdots e_n}}\right) = 0 \\ \Sigma &\left(\overline{\underline{e_2\, e_1 \cdots e_n}}\right) = 0 \\ \Sigma &\left(\overline{\underline{e_3 \cdots e_n}}\right) = 0 \\ &\cdots\cdots\cdots\cdots\cdots\cdots \\ \Sigma &\left(\overline{\underline{e_r\, r_{r+1} \cdots e_n}}\right) = 0. \end{aligned}$$

But these, except the last, are not the *simplest* that can be employed; that is to say, we can write down r others, the terms of which shall be of lower dimensions in respect to the roots.

Let f_μ denote that any rational symmetrical function of the μth degree is to be taken of the quantities which it precedes.

Then the r equations in question are all contained in the general equation

$$\Sigma\left\{\int_\mu (e_1\, e_2 \ldots e_{r-1}) \times \left(\overline{\underline{e_r\, e_{r+1} \cdots e_n}}\right)\right\} = 0;$$

μ being taken from 0 up to $(r-1)$ we obtain r equations, which in respect to the roots are respectively of all degrees between $\dfrac{n\,.\,(n-1)\,\ldots\,(n-r+2)}{1\,.\,2\,\ldots\,(r-1)}$ and $\dfrac{n\,.\,(n-1)\ldots(n-r+2)}{1\,.\,2\,\ldots\,(r-1)} + (r-1)$ reckoned inclusively.

Now at this stage it is important to remark that the above r equations, although *necessary*, are not *sufficient*; and indeed, no more affirmations of equality can be sufficient to ensure there being r pairs of equal roots.

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* The importance of the restriction hinted at by the use of the word affirmative will appear hereafter.

To make this manifest, suppose $r = 2$. Then in order that an equation *may* have two pairs of equal roots, we must have by the above formula

$$\Sigma\left(\overline{e_2\, e_3 \dots e_n}\right) = 0 \quad \Sigma\left\{e_1\left(\overline{e_2\, e_3 \dots e_n}\right)\right\} = 0.$$

But if instead of there being two perfect square factors there be one perfect *cube* factor in $f\,x$, it may be shown by the same reasoning as above, that the very same two equations apply. In fact, it may be shown in general that no such equations as those given above can be *affirmed* in consequence of there being an amount r of multiplicity consisting of unit parts which may not be affirmed with equal truth as necessary consequences of the same amount distributed *in any other manner* whatever. How to obtain affirmative equations sufficient as well as necessary (under certain limitations) will appear at the close of this present paper.

It is worthy of being remarked, that if we make f_μ denote the sum of the products of the quantities to which it is prefixed, taken μ and μ together, the equations of affirmation become identical with those obtained by eliminating between $f\,x$ and $\frac{f\,x}{d\,nd}$[*].

It can scarcely be doubted that the illustrious Lagrange, had he chosen to perfect the incomplete theory of equal roots given in the *Résolution Numérique*, by applying to it his own favourite engine of symmetric functions, could scarcely have failed of stumbling by a back passage upon Sturm's memorable theorem.

Let us now proceed to show how a polynomical known to contain one or more perfect square factors may be decomposed.

Let us begin with supposing that it contains but one such factor; so that $f\,x = \phi\,x\,.\,(x-a)^2$.

I shall show how to obtain the equations $\mathrm{C}\,(x-a) = 0$, $\mathrm{D}\,\phi\,x\,.\,(x-a) = 0$, $\mathrm{E}\,(x-a)^2 = 0$, $\mathrm{F}\,.\,(\phi\,x) = 0$, each in its lowest terms.

1. To form the equation $\mathrm{L}\,x + \mathrm{M} = 0$, where $x = a$, it is easy to see that if we write down in general the expression $(x-e_1)\left(\overline{e_2\, e_3 \dots e_n}\right)$ this will become zero whenever the root e_1 left out is not one of the equal roots (a): so that in fact (calling the two equal roots e_1 e_2 respectively)

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[*] See my Note on Sturm's Theorem, Phil. Mag., December, 1839. [L. and E. Phil. Mag. vol. xv. p. 434.]

$$\Sigma\left\{(x-e_1)\times\left(\overline{\underline{e_2\,e_3\ldots e_n}}\right)\right\} = (x-e_1)\times\left(\overline{\underline{e_2\,e_3\ldots e_n}}\right)$$
$$+\,(x-e_2)\times\left(\overline{\underline{e_1\,e_3\ldots e_n}}\right).$$

or simply $\qquad = 2\,(x-a)\,.\left(\overline{\underline{e_2\,e_3\ldots e_n}}\right)$

Hence by making

$$x\,\Sigma\left(\overline{\underline{e_2\,e_3\ldots e_n}}\right) - \Sigma\left\{e_1\times\left(\overline{\underline{e_2\,e_3\ldots e_n}}\right)\right\} = 0,$$

we have an equation for finding the equal roots $e_1\,e_2$.

Again, it is easily seen upon the same hypothesis, that

$$\Sigma\left\{(x-e_2)\,(x-e_3)\,(x-e_4)\ldots(x-e_n)\times\left(\overline{\underline{e_2\,e_3\ldots e_n}}\right)\right\}$$
$$= 2\,(x-e_2)\,(x-e_3)\ldots(x-e_n)\times\left(\overline{\underline{e_2\,e_3\ldots e_n}}\right).$$

Hence, to form the equation having the same roots as $(x-a)\,\phi\,x$, we have only to make

$$x^{n-1}\Sigma\left(\overline{\underline{e_2\,e_3\ldots e_n}}\right) - x^{n-2}\,\Sigma\left\{\overline{e_2+e_3+\ldots e_n}\times\left(\overline{\underline{e_2\,e_3\ldots e_n}}\right)\right\}$$
$$\ldots\ldots\ \pm\,\Sigma\left\{\overline{e_2\,e_3\ldots e_n}\times\left(\overline{\underline{e_2\,e_3\ldots e_n}}\right)\right\} = 0.$$

Suppose now in general that we have (r) perfect square factors, so that $f\,x = \phi\,x\,.\,(x-a_1)^2\,(x-a_2)^2\ldots(x-a_r)^2$.

To form the equation $C\,(x-a_1)'(x-a_2)\ldots(x-a^r) = 0$, we have only to make

$$\Sigma\left\{\overline{x-e_1}\,.\,\overline{x-e_2}\ldots\overline{x-e_r}\times\left(\overline{\underline{e_{r+1}\,e_{r+2}\ldots e_n}}\right)\right\} = 0$$

And to obtain $D\phi\,x\,\times(x-a_1)\,(x-a_2)\ldots(x-a_r) = 0$ we must make

$$\Sigma\left\{(x-e_{r+1})\,(x-e_{r+2})\ldots(x-e_n)\times\left(\overline{\underline{e_{r+1}\,e_{r+2}\ldots e_n}}\right)\right\} = 0.$$

The theory of perfect square factors is not yet complete until it has been shown how to obtain constructively $\phi\,x$, and as analogy suggests the complementary part $D'(x-a_1)^2\,.\,(x-a_2)^2\ldots(x-a_r)^2$ each in its lowest terms. To effect the latter it might be said that it is only necessary to take the square of $C\,(x-a_1)\,(x-a_2)\ldots(x-a_r)$. It is true the polynomial so formed would contain every pair of equal factors, but not in the lowest terms as regards the coefficients (as we shall presently show).

To solve this last part of the problem, let it be agreed that two rows of letters inclosed in a parenthesis shall indicate the product of the squares of the differences got by subtracting each in the row from each in the other, so that $\begin{pmatrix} a \\ b \end{pmatrix}$ $= (a-b)^2$ $\quad\begin{pmatrix} a & \\ b & c \end{pmatrix} = (a-b)^2\,(a-c)^2$ $\quad\begin{pmatrix} a & b \\ c & d \end{pmatrix} = (a-c)^2.(a-d)^2$ $(b-c)^2\,.\,(b-d)^2$.

Let us begin with supposing that fx has one pair only of equal roots; to form the simplest quadratic equation containing this pair, write down

$$(x-e_1)(x-e_2)\times\left(\overline{\underline{e_3 e_4 \dots e_n}}\right)\times\left(\begin{matrix}e_1 e_2 \\ e_3 e_4 \dots e_n\end{matrix}\right).$$

Now if e_1 and e_2 are the two equal roots in question the multipliers of $(x-e_1)(x-e_2)$, neither of them vanish.

If e_1 and e_2 are neither of them equal roots $\left(\overline{\underline{e_3 e_4 \dots e_n}}\right)=0.$

If one of the two only belong to the pair of equal roots

$$\left(\begin{matrix}e_1 e_2 \\ e_3 e_4 \dots e_n\end{matrix}\right)=0.$$

Hence it is clear that

$$\Sigma\left((x-e_1)(x-e_2)\times\left(\overline{\underline{e_3 e_4 \dots e_n}}\right)\times\left(\begin{matrix}e_1 e_2 \\ e_3 e_4 \dots e_n\end{matrix}\right)\right)=0$$

is the equation desired.

In like manner if there be (r) pairs of equal roots the equation of the $(2r)$th degree which contains them all may be written

$$\Sigma\left\{(x-e_1)(x-e_2)\dots(x-e_{2r})\times\left(\overline{\underline{e_{2r+1}\dots e_n}}\right)\times\left(\begin{matrix}e_1 e_2 \dots e_{2r} \\ e_{2r+1}\dots e_n\end{matrix}\right)\right\}=0.$$

The coefficient of x^{2r} in this equation is clearly of $(n-2r)(n-2r-1)+4r(n-2r)$ *i.e.* of $(n+2r-1)(n-2r)$ dimensions. The coefficient of x^r in the equation which contains the r equal roots unyoked together is of $(n-r)(n-r-1)$ dimensions, and consequently the coefficient of x^{2r} in the square of this equation would be of $2(n-r)(n-r-1)$ dimensions, *i. e.* would be $n^2+6r^2-(4r+1)n$ dimensions higher than needful.

Finally, to obtain an equation clear of *simple* as well as double appearances of the equal roots, we have only to write the complementary form

$$\Sigma\left\{(x-e_{2r+1})(x-e_{2r+2})\dots\dots(x-e_n)\times\left(\overline{\underline{e_{2r+1}\dots e_n}}\right)\right.$$
$$\left.\times\left(\begin{matrix}e_1 e_2 \dots e_{2r} \\ e_{2+1}\dots e_n\end{matrix}\right)\right\}=0.$$

Let us, now that we are more familiarized with the notation essential to this method, revert to the question with which we set out, and endeavour to obtain r such equations as shall imply *unambiguously* the existence of r pairs of equal roots.

The existence of r such pairs enables us to assert the following disjunctive proposition, which cannot be asserted when the *same amount* of multiplicity is distributed in any other way.

To wit, on selecting any r roots out of the entire number, either these r will all be found again in those that are left, *or* those that are left will contain *inter se*, one repetition at least; so that except on the latter supposition any $(r-1)$ may be absolutely sunk out of those thatare left, and therewill still be *one* root common to the $(n-2r+1)$ remaining, and to the r originally selected to be left out.

Wherefore calling the roots $e_1\, e_2 \ldots e_n$, and giving μ any value whatever, we have

$$\Sigma\left\{\int_\mu (e_1 e_2 \ldots e_r)\times\left(\overline{\underline{e_{r+1}\, e_{r+2}\, \ldots e^n}}\right)\times\Sigma\begin{pmatrix} e_1 & e_2 & \ldots & e_r \\ e_{2r} & e_{2r+1} & \ldots & e_n \end{pmatrix}\right\}=0.$$

Hence the simplest distinctive equations indicative of the existence of r pairs of equal roots are to be found by putting μ equal in succession to all values from 0 up to $(r-1)$.

For instance, if we require that an equation of the seventh degree shall have three pairs of equal roots, we need only to call the seven roots respectively $a\, b\, c\, d\, e\, f\, g$, and then our type equation becomes

$$\Sigma\left\{\int_\mu (a\, b\, c)\times\left(\overline{\underline{d\, e\, f\, g}}\right)\times\left\{\begin{matrix}\begin{pmatrix} d\,e \\ a\,b\,c\end{pmatrix}+\begin{pmatrix} d\,f \\ a\,b\,c\end{pmatrix}+\begin{pmatrix} d\,g \\ a\,b\,c\end{pmatrix} \\ +\begin{pmatrix} e\,f \\ a\,b\,c\end{pmatrix}+\begin{pmatrix} e\,g \\ a\,b\,c\end{pmatrix}+\begin{pmatrix} f\,g \\ a\,b\,c\end{pmatrix}\end{matrix}\right\}\right\}=0.$$

From this it appears that the r distinctive equations for r pairs of equal roots are of different dimensions from the r general or overlying ones corresponding to the multiples r, anyhow distributed; the lowest of the latter being of $(n-r+1)\,(n-r)$, the lowest of the former of $(n-r).(n-r-1) + 2r(n-2r+1)$ *i. e.* of $n.(n-1)-3r(n-1)$ dimensions. In general we shall find that the more unequally distributed the multiplicity may be the lower are the dimensions of the distinctive equations, and are accordingly lowest when the multiplicity is absolutely undistributed*.

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* It must not, however, be overlooked, that the equations above given, although decisive as to the existence of r pairs of equal roots *when* the multiplicity is known to be not greater than r, do not enable us to affirm with certainty their existence when this limitation is absent: for should the multiplicity exceed r, then inevitably (no matter how it may be distributed) $\left(\overline{\underline{e_{r+1}\, e_{r+2}\, \ldots e_n}}\right)$ is always zero, and consequently nullifies each term of every one of the equations in question. In fact (repugnant as it may appear to be to the ordinary assumptions of analytical reasoning), it is not possible to express with *absolute* unambiguity the conditions of there being a multiplicity (r) distributed in any assigned manner by means of r *affirmative equations alone.*

[To be continued.]

XLIII. *On the Atomic Volume and Crystalline Condition of Bodies, and on the Change of Crystalline Form by means of Heat.* *By* Dr. HERMANN KOPP[*].

IN a former paper (Poggendorff, *Ann.* xlvii.) I have endeavoured to show that a relation subsists between the volumes in which bodies unite in forming chemical compounds, in addition to that which is known to exist with respect to their weights. Gay-Lussac had already shown that gaseous compounds combine with one another according to volumes; but this observation had not been extended to solid or liquid compounds. Hence the object of my inquiries was to examine the *atomic volumes* in which the latter combine, in contradistinction to their *atomic weights*; to which attention had hitherto been exclusively directed in examining their chemical composition.

The numbers representing the atomic volumes of bodies are proportional, just as the atomic weights are; and the atomic volume of any body may be deduced from its atomic weight, merely by dividing the latter by its specific gravity. But it is evident that these numbers must vary according to the system of atomic weights adopted; hence in the following paper, that scale of equivalents in which oxygen is equal to 100 is adopted, and not what is still retained by a few chemists, in which hydrogen is regarded as unity. The formulas of the compounds examined are generally those of Berzelius.

It is not sufficient to affirm that bodies unite according to their respective atomic weights and volumes: we must also comprehend clearly what is meant by the application of these terms. Nor is this difficult; for the idea of *mass* is represented by atomic *weight*; whilst a regular and definite *volume* is evidently represented by a *crystal*.

The doctrine of isomorphism shows us that there are many bodies which possess an analogous constitution and the same crystalline form. Our idea of the *volume* (or in other words of the crystalline form) of these bodies must, therefore, be the same. From this it follows that their specific weight is dependent upon our idea of *mass* (that is of atomic weight), whilst our idea of specific weight is connected with the mass contained in the same volume. From these considerations the following law may be deduced:

The specific weight of isomorphous bodies is proportional to their atomic weight; or, isomorphous bodies possess the same atomic volume.

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[*] Communicated by the Author.

This law may be proved from facts already known. The following presents a tabular view of all the isomorphous groups, the specific gravities of which have been determined with accuracy. In the first column the chemical formulas of the compounds are stated; in the second their specific gravities, with the authorities from which they are taken; in the third the atomic weights adopted by Berzelius; and in the fourth the atomic volumes deduced from the observations.

Gold and Silver.

Au	19·258	Brisson	1243·0	64·54
Ag	10·428	Karsten.	1351·6 675·8	129·61 64·80

Berzelius has given the number 1351 as the atomic weight for silver, but Regnault has lately proved that 675 is more correct.

Potassium and Sodium.

K	0·865	Gay-Lussac and Thénard	489·92	566·39
Na	0·972	„ „ „	290·90 581·80	299·27 598·54

Dr. Clarke, influenced by several theoretical considerations, was induced a few years since to propose that the atomic weight of sodium should be raised twice as high as it is considered by Berzelius to be; and it is remarkable, that we obtain an equal atomic volume between potassium and sodium, if we adopt the view of Dr. Clarke.

Oxide of Tin and Titanic Acid.

SnO_2	6·960	Mohs	935·20	134·38
TiO_2	4·202	Breithaupt	503·69	119·87
	4·254			118·40
	4·249	Mohs		118·54
	3·759	Breithaupt		134·00
	3·826	Mohs		131·65

The first three numbers for the specific gravity of titanic acid are deduced from the mineral *rutile*, which is always rendered impure by substances specifically heavier; its atomic volume must, therefore, be smaller than it really is. The two last are drawn for *anatase*.

Alumina, Peroxide of Iron, Oxide of Chromium and Ilmenite.

Al_2O_3	3·909	Mohs	642·33	164·32
	3·979			161·43
	3·995	Breithaupt		160·78
	4·023			159·67
	3·562	Musschenbroek		180·33
	3·531	Brisson		181·93
Fe_2O_3	5·225	Boullay	978·43	187·26
	5·24	Leonhard		186·72
	5·30			184·61
	5·251	Mohs		186·33

$Cr_2\,O_3$	5·21	Wöhler	1003·6	192·63
$(Fe+Ti)\,O_3$	4·729	Breithaupt	942·90	199·39
	4·793			196·72
	4·75	Kupffer		198·51
	4·78			197·26

Spinel, Gahnite, Chrome-iron ore, Franklinite and Magnetic-iron-ore.

$Mg\,O,\ Al_2\,O_3$	3·48	Breithaupt	900·68	258·82
	3.62			248·81
$\frac{1}{2}\left(\begin{matrix}Zn\,O,\ Al_2\,O.\\ Fe\,O,\ Al_2\,O_3\end{matrix}\right)$	4·232	Mohs	1113·6	263·12
$\frac{1}{2}\left(\begin{matrix}Mg\,O,\ Al_2\,O_3\\ Fe\,O,\ Cr_2\,O_3\end{matrix}\right)$	4·410	Abich	1171·8	265·71
	4·439			263·97
$\frac{1}{2}\left(\begin{matrix}Zn\,O,\ Fe_2\,O_3\\ Mn\,O,\ Fe_2\,O_3\end{matrix}\right)$	5·091	Mohs	1453·0	285·41
$Fe\,O,\ Fe_2\,O_3$	5·094	Mohs	1417·6	278·28

Copper-glance and Silver-copper-glance.

$Cu_2\,S$	5·695	Mohs	992·56	174·28
	5·735			173·07
$(Cu+Ag)\,S$	6·255	Stromeyer	1272·7	203·47

The atomic weight of silver is supposed to be half that adopted by Berzelius.

Antimony-glance and Orpiment.

$Sb_2\,S_3$	4·620	Mohs	2216·4	479·75
	4·626	Breithaupt		479·11
	4·850	Musschenbroek		456·99
$As_2\,S_3$	3·313	Musschenbroek	1543·6	465·91
	3·480	Mohs		443·55
	3·459	Karsten		446·24

Cobalt-glance and Nickel-glance.

$Co\,S_2+Co\,As_2$	6·298	Mohs	2080·4	330·32
$Ni\,S_2+Ni\,As_2$	6·238	Breithaupt	2081·8	333·72
	6·331			328·83

Arsenic- and Antimony-ruby-blende.

$Ag_3\,S_3+As_2\,S_3$	5·531	Breithaupt	6201·9	1121·3
	5·592			1109·1
	5·524	Mohs		1122·7
$Ag_3\,S_3+Sb_2\,S_3$	5·787	Breithaupt	6874·7	1187·9
	5·844			1176·4
	5·831	Mohs		1179·0

Tennantite and black Copper.

$\begin{matrix}Cu_6\,S_3\,As_2\,S_3\\ +2\,(Cu_6\,S_3\,As_2\,S_3)\end{matrix}$	4·375	Phillips	13563·7	3100·3
$\begin{matrix}Cu_6\,S_3,\ Sb_2\,S_3\\ +2\,(Pb_3\,S_3,\ Sb_2\,S_3)\end{matrix}$	5·763	Mohs	18600·9	3227·6

Lead-glance and Seleniuret of Lead.

$Pb\,S$……	7·220	Musschenbroek	1495·7	207·16
	7·587	Brisson		197·13
$Pb\,Se$……	8·20	Leonhard	1789·1	218·18
	8·80			203·30

The Carbonates of Zinc and Magnesia; Mesitine; the Carbonates of Iron and Manganese; Dolomite and Calcareous spar.

$Zn\,O, C\,O_2$	4·442	Mohs	779·67	175·52
	4·4	Naumann		177·20
	4·5			173·26
$Mg\,O, C\,O_2$	2·208	Breithaupt	534·79	190·45
	3·001	Mohs		178·20
	3·112			171·85
	2·88	Naumann		185·69
	2·97			180·06
$\frac{1}{2}\left\{\begin{matrix}Mg\,O, CO_2\\Fe\,O, C\,O_2\end{matrix}\right)$	3·350	Mohs	625·22	186·62
	3·363			185·90
$Fe\,O, C\,O_2$	3·829	Mohs	715·65	186·90
	3·872	Naumann		184·82
	3·6	Naumann		198·79
	3·9			183·50
$Mn\,O, C\,O_2$	3·550	Mohs	722·34	203·48
	3·592			201·10
$\frac{1}{2}\left(\begin{matrix}Mg\,O, CO_2\\Ca\,O, C\,O_2\end{matrix}\right)$	2·884	Mohs	583·62	202·36
$Ca\,O, C\,O_2$	2·721	Mohs	632·46	232·43
	2·750	Naumann		229·98

Arragonite, Strontianite, Witherite and Carbonate of Lead.

$Ca\,O, C\,O_2$	2·931	Mohs	632·46	215·78
	2·995	Breithaupt		211·17
$Sr\,O, C\,O_2$	3·605	Mohs	923·73	256·24
	3·625	Karsten		254·82
$Ba\,O, C\,O_2$	4·302	Karsten	1233·3	286·68
	4·301	Mohs		286·75
$Pb\,O, C\,O_2$	6·465	Mohs	1670·9	258·46
	6·428	Karsten		259·94

The Sulphates of Barya, Strontia and Lead.

$Ba\,O, S\,O_3$	4·200	Karsten	1458·1	347·15
	4·446	Mohs		327·95
$Sr\,O, S\,O_3$	3·588	Karsten	1148·5	320·08
	3·953	Breithaupt		290·52
$Pb\,O, S\,O_3$	6·169	Karsten	1895·7	307·29
	6·298	Mohs		301·00

The Nitrates of the same Oxides.

$Ba\,O, N_2\,O_5$	2·915	Hassenfratz	1633·9	560·51
	3·185	Karsten		513·00

SrO, N_2O_5	2·890	Karsten	1324·3	458·25
	3·006	Hassenfratz		440·57
PbO, N_2O_5	4·400	Karsten	2071·5	470·80
	4·769	Breithaupt		434·37

The Chlorides of Barium, Strontium and Lead.

$BaCl_2$	3·860	Boullay	1299·5	336·66
	3·704	Karsten		350·84
$SrCl_2$	2·803	Karsten	989·9	353·18
$PbCl_2$	5·802	Karsten	1737·2	299·40
	5·682			305·72
	5·238	Monro		331·60

The Nitrates of Potash and Ammonia.

KO, N_2O_5	1·933	Watson	1267·0	655·47
	2·101	Karsten		603·04
	2·058	Kopp		615·64
N_2H_8O, N_2O_5	1·707	Kopp	1004·0	588·16
	1·579	Hassenfratz		635·84

The Chlorides of Potassium and Ammonium.

KCl_2	1·915	Karsten	932·57	486·99
	1·945	Kopp		479·47
$N_2H_8Cl_2$	1·450	Watson	669·61	461·81
	1·528	Mohs		438·24
	1·500	Kopp		446·41

The Carbonates of Soda and Silver.

NaO, CO_2	2·466	Karsten	667·34	276·86
AgO, CO_2	6·077	Karsten	172·81	284·36

The Sulphates of the same.

NaO, SO_3	2·631	Karsten	892·06	339·05
	2·462	Mohs		362·33
AgO, SO_3	5·341	Karsten	1952·8	365·65

The Nitrates of the same.

NaO, N_2O_5	2·256	Karsten	1067·9	473·35
	2·188	Marx		488·07
	2·096	Klaproth		509·50
	2·200	Kopp		485·43
AgO, N_2O_5	4·355	Karsten	2128·7	488·79

The Chlorides of Sodium and Silver.

$NaCl_2$	2·078	Karsten	733·55	353·01
	2·150	Kopp		341·18
$AgCl_2$	5·501	Karsten	1794·3	326·18
	5·458			328·75
	5·130	Herapath		349·50

The Molybdate of Lead, the Tungstates of Lead and Lime.

$Pb\,O, Mo\,O_3$	6·7 6·698 6·760	Gmelin Leonhard Mohs	2293·0	342·25 342·35 339·20
$Pb\,O, W\,O_3$	8·10 8·0	Leonhard Gmelin	2877·7	355·27 359·72
$Ca\,O, W\,O_3$	6·040 5·800 6·028 5·576	Karsten Meissner	1839·2	304·50 317·10 305·11 329·85

Olivenite and Libethenite.

$Cu_4\,O_4, As_2\,O_5+2\,H_2\,O$	4·281	Bournon	3647·8	852·08
$Cu_4\,O_4, P_2\,O_5+2\,H_2\,O$	3·6 3·8	Mohs	3100·1	861·14 815·82

The crystallized Sulphates of Zinc, Magnesia and Nickel.

$Zn\,O, S\,O_3+7\,H_2\,O$	2·036	Mohs	1791·8	880·06
$Mg\,O, S\,O_3+7\,H_2\,O$	1·751 1·674	Mohs Kopp	1546·9	883·42 924·06
$Ni\,O, S\,O_3+7\,H_2\,O$	2·037	Kopp	1758·2	863·14

The crystallized Sulphates of Copper and Manganese.

$Cu\,O, S\,O_3+5\,H_2\,O$	2·274 2·2	Kopp Gmelin	1569·3	690·10 713·32
$Mn\,O, S\,O_3+5\,H_2\,O$	2·095 2·087 1·834	Kopp Gmelin	1509·5	720·53 723·28 823·06

The Sulphate and Chromate of Potash.

$K\,O, SO_3$	2·623 2·636 2·662	Karsten Watson Kopp	1091·1	415·97 413·91 409·87
$K\,O, Cr\,O_3$	2·612 2·640 2·705	Thomson Karsten Kopp	1241·7	475·39 470·57 459·04

Diopside, Hypersthene and Hedenbergite.

$\left(\begin{matrix}3\,Ca\,O, Si_2\,O_3\\3\,Mg\,O, Si_2\,O_3\end{matrix}\right)$	3·006 3·127	Mohs	4408·0	1466·7 1409·7
$\left(\begin{matrix}3\,Mg\,O, Si_2\,O_3\\3\,Fe\,O, Si_2\,O_3\end{matrix}\right)$	3·389	Mohs	4657·6	1374·3
$\left(\begin{matrix}3\,Fe\,O, Si_2\,O_3\\3\,Ca\,O, Si_2\,O_3\end{matrix}\right)$	3·582	Breithaupt	4950·6	1382·1

The crystallized double Sulphates of Potash or Ammonia and Alumina, Peroxide of Iron or Oxide of Chromium.

$\left.\begin{matrix}K\,O, S\,O_3\\Al_2\,O, 3\,S\,O_3\end{matrix}\right)+24\,H_2\,O$	1·724	Kopp	5936·5	3443·4

$\left.\begin{matrix}N_2\,H_8\,O, S\,O_3\\ Al_2\,O_3, 3\,S\,O_3\end{matrix}\right)+24\,H_2\,O$	1·626 1·625	Kopp	5673·6	3489·2 3491·4
$\left.\begin{matrix}N_2\,H\;\,O, S\,O_3\\ Fe_2\,O_3, 3\,S\,O_3\end{matrix}\right)+24\,H_2\,O$	1·712	Kopp	6009·7	3510·2
$\left.\begin{matrix}K\,O, S\,O_3\\ Cr_2\,O_3, 3\,S\,O_3\end{matrix}\right)+24\,H_2\,O$	1·848	Kopp	6295·8	3406·8

The crystallized double Sulphates of Potash or Ammonia and Magnesia, Copper, Iron, Manganese, Cobalt, Zinc, Cadmium or Nickel.

$\left.\begin{matrix}Mg\,O, S\,O_3\\ N_2\,H_8\,O, S\,O_3\end{matrix}\right)+6\,H_2\,O$	1·721 1·696	Thomson Gmelin	2262·5	1314·7 1334·0
$\left.\begin{matrix}Cu\,O, S\,O_3\\ K\,O, S\,O_3\end{matrix}\right)+6\,H_2\,O$	2·137	Kopp	2762·8	1292·8
$\left.\begin{matrix}Cu\,O, S\,O_3\\ N_2\,H_8\,O, S\,O_3\end{matrix}\right)+6\,H_2\,O$	1·757 1·756	Kopp	2499·9	1422·8 1423·7
$\left.\begin{matrix}Mn\,O, S\,O_3\\ N_2\,H_8\,O, S\,O_3\end{matrix}\right)+6\,H_2\,O$	1·930	Thomson	2450·1	1269·2
$\left.\begin{matrix}Zn\,O, S\,O_3\\ K\,O, S\,O_3\end{matrix}\right)+6\,H_2\,O$	2·153	Kopp	2770·4	1286·8
$\left.\begin{matrix}Ni\,O, S\,O_3\\ K\,O, S\,O_3\end{matrix}\right)+6\,H_2\,O$	2·111 2·136	Kopp	2736·8	1296·4 1281·3
$\left.\begin{matrix}Ni\,O, S\,O_3\\ N_2\,H_8\,O, S\,O_3\end{matrix}\right)+6\,H_2\,O$	1·801 1·783 1·915 1·921	Thomson Kopp	2473·9	1373·6 1387·4 1291·8 1287·8

Apatite, Green and Brown Lead Ore.

$Ca\,Cl_2+3(Ca_3\,O_3, P_2\,O_5)$	3·15 3·25 3·225	Naumann Mohs	6879·8	2184·1 2116·9 2133·3
$Pb\,Cl_2+3(Pb_3\,O_3, P_2\,O_5)$	7·0 7·050	Naumann Karsten	16964·6	2423·5 2406·3
$Pb\,Cl_2+3(Pb_3\,O_3, As_2\,O_5)$	7·2 7·208	Naumann Mohs	18607·9	2584·5 2581·6

By reference to the table now given, it will be seen that our law has received ample confirmation; but it still remains to be explained why the atomic volume in several of the isomorphous groups is not absolutely equal.

The law can obviously hold only for those bodies which are perfectly isomorphous. But this is seldom the case, for those bodies termed isomorphous are generally only approximately so, for we find the angles of their crystals deviating several degrees from one another; neither are the relations between the axes of bodies thus denominated isomorphous perfectly equal. But it is evident, that the more nearly the crystalline forms of isomorphous substances are identical, the more nearly will their atomic volumes be the same. The crystalline condition and atomic volume stand, however, in a certain dependence on one another, which I will endeavour to prove by several groups which have been more closely examined. It may be proper to remark, that the crystallographic notation employed in the following remarks is that of M. Naumann.

It is said that witherite, strontianite, carbonate of lead, and arragonite are isomorphous. They all crystallize in rhomboids, but the dimensions of their angles respectively are not perfectly equal. If we consider the correct atomic volume from each of these bodies as that deduced from the mean of the views already stated, and compare these atomic volumes with the proportions between the axes, the following is found to be the case:—

		Atomic volume.
$Ba\,O, C\,O_2$: a : b : c =	0·7413 : 1 : 0·5950......	286·72
$Sr\,O, C\,O_2$	0·7237 : 1 : 0·6096......	255·53
$Pb\,O, C\,O_2$	0·7236 : 1 : 0·6100......	259·50
$Ca\,O, C\,O_2$	0·7205 : 1 : 0·6215......	213·48

Or we may also compare the atomic volumes with the individual angles, for example with the declinations ∞P and $\breve{P} \infty$:

	∞P =	$\breve{P} \infty$ =	Atomic volume.
$Ba\,O, C\,O_2$:	118° 30′		286·72
$Sr\,O, C\,O_2$:	117 16	108° 12′	255·53
$Pb\,O, C\,O_2$:	117 14	108 13	259·50
$Ca\,O, C\,O_2$:	116 16	108 27	213·48

From these comparisons it is shown that the carbonates of lead and strontia are perfectly isomorphous: their atomic volumes also approximate very closely. Again, it is seen that the two other bodies are neither isomorphous with one another nor to the former minerals: hence we find that their atomic volumes also differ.

But it is also apparent that an increase of atomic volume for this group must be followed by an increase of the axis *a*, and a decrease of the axis *c*; or according to the augmentation in the atomic volume the declination ∞P must become more, and the declination $\breve{P} \infty$ less obtuse.

If one of these crystals (of arragonite for example) be heated, its density becomes less, and in consequence of this its atomic volume greater. The consequence of this must be that the declination ∞P must be more and that of $\breve{P} \infty$ less obtuse; and this has been long since observed to be really the case.

But the relation between the crystalline form and the atomic volume may be more correctly estimated in another class of carbonates.

The carbonates of zinc and magnesia, mesitine, the carbonates of iron and manganese, dolomite and calcareous spar, are a group of bodies which possess both an analogous constitution and an equivalent crystalline form. The crystalline form of all of them is that of a rhombohedron, but the axes a in them are unequal and the angle R different. If we adopt the mean of the observations already given as the atomic volume of each of these bodies, we find that the axis a of the rhombohedron increases, that the angle R diminishes, whilst the atomic volume increases.

	Axis a.	Angle R.	Atomic volume.
Carbonate of zinc	0·80708	107° 40′	175·33
Carbonate of magnesia	0·81165	107 25	181·25
Mesitine.................	0·81498	107 14	186·26
Carbonate of iron	0·81962	107 0	188·50
Carbon. of manganese	0·82182	106 51	202·29
Dolomite	0·83312	106 15	202·36
Calcareous spar.........	0·85440	105 15	231·20

We wish to establish a connexion between the length of the axis a and the atomic volume of a body; for this purpose it is natural to assume the atomic volume as the volume or the cubic capacity of its fundamental form. Accordingly, in the group now considered, the atomic volume (V) must be proportional to the length of the axis a; hence we have

$$a = y\,\text{V}.$$

But another number is found for y in every body, and the cause of this is obvious. It is supposed in this formula, that the filling up of the space in the rhombohedral crystals is equal on all sides; but it is known by the optical characters of these crystals that this is not the case. In order to discover a relation between a and V, let us suppose

$$a^x = y\,\text{V}.$$

We seek x and y according to the method of the least squares from the estimations of a and V of the seven different substances, and we find

$$x = 4{\cdot}739 \qquad y = 0{\cdot}0020417.$$

The relation between a and V is therefore

$$a^{4{\cdot}739} = 0{\cdot}0020417\ \text{V}.$$

This formula coincides very exactly with the observations. And as the greater part of the substances in their natural condition are accompanied by impurities, the formula may be employed to calculate their specific weight from their crystalline form. If we suppose that the axes of the bodies already examined are given, we deduce the atomic volume, and from it the specific gravity.

	Atomic volume.	Spec. gravity.
Carbonate of zinc.........	177·37	4·3956
Carbonate of magnesia...	182·18	2·9355
Mesitine	185·75	3·3658
Carbonate of iron.........	190·41	3·7585
Carbonate of manganese	193·26	3·7377
Dolomite	210·28	2·7755
Calcareous spar	232·36	2·7220

From the differences occurring between the specific weight observed and calculated, we can conclude whether the substances found in their natural condition are rendered impure by substances specifically heavier or lighter; but with those minerals which occur in a state of purity, the specific gravities ascertained by observation and calculation approximate very closely. In the case of dolomite the specific weight obtained by experiment is always greater than it is found to be by calculation; but analysis shows that it is always rendered impure by the oxides of iron and manganese. The carbonate of manganese is, on the other hand, less than the calculated result, but it is always mixed with carbonate of lime.

From what has now been brought forward, it must be evident that an increase of atomic volume is dependent upon an increase of the axis *a*. The application of heat to one of these crystals must decrease its density, and the axis *a* must be enlarged, whilst the angle R will be rendered less obtuse.

This has long since been discovered by Mitscherlich. This chemist has accurately determined the diminution of density on the application of heat to calcareous spar. He found that by a heat of 100° C. (180° F.) its specific gravity was decreased in the proportion of $1 : \frac{1}{1·001961}$. We find above the specific weight of calcareous spar, when its axis $a = 0·85440$ and its angle $R = 105° 5'$ is 2·7220. By heating it for 100° C., therefore, it will be equal to 2·71675, or its atomic volume passes from 232·36 to 232·80. If we determine the length of the axis *a* by means of the formula already given, we find it $= 0·85672$, corresponding to an angle R of 104° 57′ 22″. According to this calculation the change in the angle R by $100° \text{ C} = 7' 37''$, a result which coincides sufficiently with that found directly by Mitscherlich (8′ 34″), when we consider the difficulties which necessarily accompany the direct measurement of the dilatation and change of the angles.

XLIV. *Further Remarks on some of the Circumstances under which Steam developes Electricity.* *By Dr.* CHARLES SCHAFHAEUTL*.

BY repeating the experiments described in the February Number of the Philosophical Magazine, p. 95, of holding the glass bell against the jet of wet steam, it frequently happened that the water dropping down from the edges of the bell flowed in a continuous stream, and served as a conductor for the electricity developed within it.

In order to obviate this, I substituted for the short metallic jet (the opening of which must never be smaller than one-sixteenth of an inch in diameter) a flexible leaden tube about ten inches long, with its upper end bent downwards at an angle of about 80°. Now in consequence of this arrangement I was enabled to place the glass bell, with its mouth upwards, simply upon a small ring on the table under the opening of the jet, and the bell served not only as a receiver for the steam, but likewise for the separating water.

By this arrangement an objection is removed, which might have arisen in respect to my former experiments, viz. *Whether the observed electricity did not arise from the friction of the escaping minutely divided water during its striking against the glass bell?*

Now in this experiment the development of electricity is the same, whether the issuing steam jet strikes against the glass, or touches only the surface of the water, which is collecting in it during repeated experiments; neither the distance of the mouth of the jet from the glass or the water surface made the slightest difference in the quantity of the liberated electricity, a proof not only that the cause of the electricity is not to be ascribed to friction, but likewise that a sheet of water exerts the same condensing power in respect to wet steam as the solid glass.

The fact already mentioned, that the inside of the glass bell, when discharged, begins slightly, and often repeatedly, to recharge itself again after the steam has ceased to flow, excludes all possibility of explaining the phænomenon by means of friction, but the electricity observed in this way might be ascribed possibly simply to evaporation still going on in the bell in some degree after the steam has been stopped. I therefore frequently poured boiling water into the glass bell, and brought it into connexion with the electroscope; but neither the water nor the steam ascending in clouds from the surface

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* Communicated by the Author.

of the water showed the slightest traces of electricity. It appears, therefore, that steam, the caloric of which is derived from the water from which it separates, developes no electricity.

The temperature, for example, of the laboratory during one experiment was 45° Fahr. The temperature of the issuing wet steam at a distance of one-eighth of an inch from the mouth of the jet was 150°; at three inches further the temperature of the steam sunk to 120°; in the bell itself it was 110°; and the collected and condensed water in the bell measured 100°, the steam escaping from this water showing only between 60° and 70°.

The temperature of the steam and the water in the boiler was under a pressure of thirty-two inches, of course about 253°. After opening the stop-cock, the water released partially from its pressure was converted, till the stop-cock was shut again at a pressure of about twenty-three inches, 62·3 of its bulk further into steam, converting the mass of water into an agglomeration of air-bubbles*, which, mixed with the steam of the upper part of the boiler, escaped through the metallic tube. During its expansion in the air the free caloric of the steam became latent, which was immediately replaced by the free caloric of the minutely divided water, mixed with the steam, so that the temperature of this jet of steam must be of course, notwithstanding its expansion, far higher than that of pure steam escaping under an equal pressure; and this higher degree of heat retained in wet steam, may be one cause of its retaining the admixed water in a state in which it is easily and at once separable by striking against a resisting or condensing medium, as glass, water, or even air, at which moment electricity begins to become developed.

The steam separating afterwards from the condensed water, has at the same time its own quantity of caloric derived from the heat of the boiler; and instead of having it absorbed from the hot water, which must be the case when boiling water is poured into the glass bell, it has rather a tendency to impart some of its caloric to the water.

The greatest difficulty is to explain in which state or form the water exists mixed with the issuing steam; whether it is suspended in it as minute *solid* water globules, or as minute *hollow* ones, as Saussure considered them to exist in clouds, perhaps similar to a soap bubble? I am inclined to adopt Saussure's opinion, particularly as all liquids when agitated with gases assume finally a form resembling bubbles filled with gas. When water-gas is viewed through a microscope

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* A similar effect may be witnessed daily when the cork of a soda-water bottle is drawn.

the minute vesicles may be very easily distinguished from water globules passing at the same time through the field of the microscope, the former disappearing when coming in contact with each other, the latter uniting in one larger globule.

M. Fresnel considered the theory of Saussure as inadmissible, because the air surrounding the small vesicles must be in a state near saturation, and therefore he thought it was scarcely possible that the envelope of these little globules could be of an equal or lesser density than the air in which the clouds are suspended. Further, he considered the swiftness of their movement entirely irreconcilable with their vesicular state, as the friction caused by such movement through the air would soon set them at rest. Finally, he explains, as the air in the vesicles must be in a state of condensation, as it has to resist the tendency of the water molecules to unite themselves in a drop, the globule would finally disappear, as a soap bubble will swiftly diminish and disappear when the tube on which it is suspended is removed from the mouth. But this latter case only takes place when the film forming the soap bubble is very thick, viz. in the beginning of its formation, and can *possibly* only occur when the bubble is joined to a *tube*, which with the vesicles in the cloud is certainly never the case; but when the bubble is once formed, every child knows that it will stand till evaporation or concussion causes it to burst.

The rapid movement of the small vesicles of water is for the greatest part attributable to currents of air; and as therefore their velocity is equal to that of the molecules of the air, no possible friction and loss of velocity can arise from this circumstance.

In further considering the equilibrium of the cloud, we must not only bear in mind the single vesicles as such, but the single vesicle surrounded of course by an atmosphere of air and water-gas united to one substantial body, viz. the *cloud*, and forming itself by exerting forces within, as molecules of water when at liberty form and arrange themselves into a shape where their mutual attractive forces are in a state of perfect equilibrium. Besides, the surrounding air, as long as the cloud swims in it, can only be in a state of saturation on one part and in contact with the cloud. When the entire air between the earth and the clouds is near saturation, the clouds begin to decompose and discharge water.

Fresnel assumes that clouds consist of minute solid globules of water, and as the air, in their intervals and therefore in contact with the globules, is easier to be heated by radiation than air not in contact with solid or liquid bodies, he explains, that this air between the water globules is of a much higher temperature than the surrounding air, and that this heated and rarefied air, together with the water globules, constitutes a body of equal or less density than an equal mass of the surrounding air. But if we take into consideration the slight expansion of gaseous bodies for every degree of the thermometer, and compare the very great difference in density between air and water, and therefore the mass of heat required to keep only a small quantity of water in suspension for such a considerable time; further, if we ask, why by such an elevation of the temperature of the air in contact with the water globules the capacity for saturation of this air may not increase with the temperature, and the minute water globules be not converted into water gas again;—all these circumstances present difficulties almost insurmountable in adopting this theory.

M. Saigey finally declares, that the equilibrium of the clouds is only *apparent*; that the minute globules of water in the cloud were indeed *constantly falling*, but immediately as they left the cloud were again converted into water-gas by touching the surrounding non-saturated air, which rising and cooled down again, restored again its water to the cloud, so that all these clouds were constantly losing water on one and receiving it on the other side. This explanation can only be correct under certain circumstances, that is when the cloud is near its decomposition. The sharp definite outlines and the circumscribed forms of the clouds swimming in a clear and dry sky, prove that they are not further connected with the cause of their origin; besides, their swimming so long in the air unaltered in form during very widely different degrees of temperature, and their movement often in different directions coming in contact with each other, and separating again unaltered in shape, all proves too well their substantiality, or rather their *individuality*.

XLV. *A Suggestion relative to* Barrett's *Method of computing the Values of Life Contingencies. By* A. De Morgan, *Esq. Professor of Mathematics in University College*[*].

THE method of calculating the value of life contingencies, which is called after Mr. Barrett, its inventor, has been rapidly coming into use of late years, and is well known by those who are used to it, to save much the greater part of the time and labour required by the common methods. The use of this method will be much extended by the copious tables published by Mr. Jones in the "Library of Useful Know-

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[*] Communicated by the Author.

ledge," where, for the first time, we see tables for the problems which involve two lives.

An improvement in the method of calculating tables for two lives has suggested itself to me, which I have no doubt will be adopted as soon as proposed, and will be carried into effect in future tables.

If a_m be the number living at the age m, and v the value of 1*l.* due in a year, the construction of Barrett's tables (in the improved form suggested by Mr. Griffith Davies) is as follows:—

$$D_m = a_m v^m, \ N_m = D_{m+1} + D_{m+2} + \ldots\ldots$$

with other results, not here necessary to be specified. When we come to construct tables for two lives it is necessary that the column answering to D_m, say $D_{m,n}$, should be of the form $a_m a_n v^k$, where k is a function of m and n, which increases by a unit when m and n are both increased by a unit. In Mr. Jones's tables, k is the greater of the two, m and n; I propose that it should be the half sum of the two, thus:

$$D_{m,n} = a_m a_n v^{\frac{1}{2}(m+n)}, \ N_{m,n} = D_{m+1,n+1} + D_{m+2,n+2} + \ldots$$

In problems in which the order of survivorship does not enter, I do not see any particular advantage in this change; but in all such problems there is a decided and obvious gain. At present two formulæ must be used, accordingly as the life which is to survive the other is elder or younger of the two; while with tables calculated in the manner proposed by me, one only would be necessary. And even supposing that no new tables were calculated, it would still be better to teach the method in the manner I propose, while two very simple transformations would reduce any result to those which the present tables require.

For example, the present value of 1*l.* payable on the decease of a life aged y, on condition that a life aged x is then subsisting, according to the method I propose, is in all cases

$$\frac{N_{x-1,y-1} v - N_{x,y} + (N_{x,y-1} - N_{x-1,y}) \sqrt{v}}{D_{x,y}}.$$

To convert this into the common method (if it can yet be called common, as far as *two* lives are concerned) the rule is, divide every term of the form $N_{x,y}$ or $D_{x,y}$ by $v^{\frac{1}{2}(x-y)}$ or $v^{\frac{1}{2}(y-x)}$, according as x or y is the greater. If then $x - 1$ be greater than, or equal to y, the preceding result, so treated and reduced, becomes

$$\frac{(N_{x-1,y-1} - N_{x-1,y})\,v + N_{x,y-1} - N_{x,y}}{D_{x,y}}$$

while if $y-1$ be greater than, or equal to x, it becomes

$$\frac{(N_{x-1,y-1} + N_{x,y-1})\,v - (N_{x-1,y} + N_{x,y})}{D_{x,y}}.$$

These results agree with those of Mr. Jones (p. 176), if in the formulæ cited $N_{x,y}$ be substituted for its equivalent $N_{x-1,y-1} - D_{x,y}$. If ever the time should arrive when tables of this sort are calculated for three lives, it should be by the formula

$$D_{m,n,p} = a_m\,a_n\,a_p\,v^{\frac{1}{3}(m+n+p)}$$

and so on. In such a case, the method now in use would become intolerable, from the large number of cases into which different orders of survivorship would require formulæ to be subdivided.

XLVI. *Note to a Paper "On certain points in the Undulatory Theory, &c." in the last Number of the Philosophical Magazine. By the Rev.* Baden Powell, *M.A., F.R.S., &c., Savilian Professor of Geometry, Oxford**.

IN referring to the establishment of the principle of a relation between the symmetrical or unsymmetrical arrangement of the ætherial molecules, and the rectilinear or elliptic nature of the vibrations, I was led at the suggestion of a friend to add a note (p. 161) while the paper was in the press, with respect to the share which M. Fresnel might be supposed to have in disclosing that principle.

Having since looked more closely into the matter, I think it right to take this opportunity of stating the result.

Fresnel devotes a section of his admirable Memoir on double refraction to the consideration of the mechanical theory of ætherial vibrations. But this, though characterized y all the profound ability of the author, is discussed almost entirely in general terms, and the results are not connected with any mathematical development of equations of motion.

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* Communicated by the Author.

He has considered the conditions presented by different cases of *elasticity* of the æther within crystallized and uncrystallized media, and, in particular, traces the consequences of a supposition of this kind in modifying the formula for waves so as to give rise to elliptic or circular polarization in *rock crystal*, and refers to a peculiar arrangement of the vibrating molecules *in that substance* as influencing the right or left-handed direction of the rotation*. But I cannot find that he took up such an idea upon any more general grounds: in other words, I think he cannot be said to have shown *generally* a connexion between the arrangement of the molecules of æther in space,—the forms assumed by the differential equations accordingly,—and the consequent rectilinear or elliptic character of the vibrations. I am still of opinion, therefore, that the credit of first establishing such a general relation is due entirely to Mr. Tovey.

Oxford, March 10, 1841.

XLVII. *Report on the Memoir on Electric Currents in Warm-blooded Animals, by* Prof. Zantedeschi *and* Dr. Favio, *presented to the Royal Academy of Sciences and Belles Lettres of Brussels, on the 4th of April,* 1840. *By* M. Cantraine†.

THE memoir which is addressed to us relates to electro-physiological doctrines, and the object of the experiments which these gentlemen have made, is to ascertain whether electric currents exist in warm-blooded animals; to endeavour to find their connexion with life; their intensity, their direction, their number, and in what way they may be discovered. Electricity, whether considered as the cause of the phænomena of life, or as the special and intimate effect of life, is in some measure a new subject: few persons have attended to it; the difficulties also which surround such researches are numerous, and we must acknowledge with the authors, that in this case the concurrence of natural philosophy and the medical sciences is indispensable, as much on account of the apparatus which should be used, as for the perspicacity necessary to penetrate into the mysteries of the vital organism.

These gentlemen set out with the principle enunciated by Pucinotti and Pacinotti, the learned Professors of Pisa, which is based upon the experiments and the observations also made by those Professors. It maintains that *in living animals there exists an electro-vital or neuro-electric current, the character of*

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* *Mém. Instit.*, vii. p. 74.

† From the *Bulletin de l'Academie Royale des Sciences et Belles Lettres* of Brussels, 1840, No. 8. p. 43; *Séance du* 1er *Août.*

which is that it is the effect of a vital reaction, whether anatomical or voluntary, and that it is, if not the cause of life, at least that immediate and special effect which life alone can prôduce and maintain; that *this current may be discovered* (sondé) *and obtained in a manner more or less evident, according to the nature of the metals and the form of the instruments*; that *it proceeds in the direction of from nerve to muscle*; that *it is intimately connected with the energy and with the physiological changes of life*; that *it has nothing in common with the electro-chemical and thermo-electrical currents*; that *the convulsive movements of animals augment it, whilst, on the contrary, pain weakens it*; that *the same difference exists with it as between animal life and organic life, following thus, in the first case, the phases of animal life and dying with it; and in the second case, still existing so long as organic life can endure, although animal life has ceased.* The current which is obtained from organic life was named by the Professors of the University of Pisa, the *cardiac current.*

From this corollary, we might be led to see in this electro-physiological doctrine much analogy with that of the *electro-vitalists.* The difference, however, is very great; the authors of this memoir, recognizing the vital force as a primitive force, by which matter, obeying the common laws of nature by its intrinsic virtue, changes into animated matter.

The authors having proved that the force of the neuro-electric current depends upon the nature of the metals which are employed, made use of iron, then of silver instruments; they propose however trying those of platina. The results which they give are obtained then with iron and silver instruments.

The apparatus necessary for the experiment consists of a galvanometer,—of two metallic stylets, which are also called sounds or réophores (whether they are of iron, of silver or of platina), soldered metallically to the two extremities of the conducting wire of the galvanometer,—and of a living animal.

The galvanometer is placed in an isolated spot and far from anything composed of metal, particularly of iron; it is fixed where it neither undergoes jogs or shocks. At a little distance from this galvanometer should be a kind of small coffer rather shallow, made entirely of wood and without an atom of iron; a living animal is slightly fastened in this coffer, which is brought into the circuit of the galvanometer in the following manner:—

One of the metallic stylets which were just mentioned, and which should be furnished with an insulating handle, is suddenly plunged (*plongé*) into some part of the living animal, whilst into another part of the same animal is plunged (*enfoncé*) another stylet formed like the preceding one, and like it furnished with an insulating handle. It is understood that by means of the interposition of the animal, the circuit of the galvanometer is thus complete.

At the moment when the two stylets are plunged (*enfoncé*) into the two parts of the animal, there is disengaged from the torrent of animal life, by the effect of a vital reaction, an electric current, which, diverted from its natural conductors by the presence of the metallic body which is plunged into the organic substance, directs itself along the stylets and the wire conductors which are soldered to them, so as to complete the circuit by the same wires and in the animal parts which are interposed: thus in this circuit the current having to pass over the multiplicator of the galvanometer, the deviations which take place in the magnetic needle of this instrument indicate its intensity and direction.

This is the explanation which the authors have thought proper to give as well of the operation as of the very simple apparatus which serves for it. This, however, is not all which should concur to ensure the result of the experiments; there are still some circumstances which exert a great influence upon it, such as the perfection of the galvanometer, the nature of the metal of which the instruments are made, the homogeneity of the stylets, every intervention of chemical substances, the nature of the season, the state of the atmosphere, the painful preparations of the animal, its organic development as well as its age, and lastly, its greater or less degree of sensibility, &c.; conditions which might appear too numerous to allow us to hope anything from similar researches, if the results already obtained did not show the contrary.

The authors, considering the existence of electric currents in warm-blooded animals as sufficiently proved, ask if they should call them vital, and supposing that this name should be given them, what should be understood by *vital current*? They adopt the whole definition which MM. Pucinotti and Pacinotti have given of it, and which I quoted above; they also adopt for the time being the denomination of *electro-vital* current.

Besides this *electro-vital* current, they have verified in the same animals two other currents which had been mentioned by the Professors of Pisa; the one is the *common electro-chemical* current; the second the *thermo-electric*. The first is divided into the *common electro-chemical current*, properly so called, and the *vital electro-chemical current*; this is the current which is supposed to proceed from the intimate chemism (*chimisme*) of the organic and animal life which resides in the substance of the organs and tissues.

In order to distinguish these currents, they apprise us that we should not take as the electro-chemical current in relation with life, that which is obtained by sounding (*sondant*) the different products and the different secretions of the organs; such a current is named the *common electro-chemical*, whilst the vital electro-chemical current is that which is developed in the internal animal chemism, which is not subject to the common chemical laws; a current which they are not willing to suppose to be the cause of life, but which may be regarded as the special effect which life alone can produce.

By this it is evident that the common electro-chemical current is obtained by simply placing the stylets in contact with the secretions, or with the surfaces of the organs moistened with heterogeneous products.

The second, on the contrary, is only obtained by penetrating with stylets even into the intimate assemblage of the organic tissues, and by exciting in them a strong reaction by means of this deep perturbation.

Consequently, if we look upon this last current either as drawing its origin from the chemical and proper action of life on matter, or as the index and the measure of this active principle, agent of life, whether it be considered chemically in the first case, whether it be considered physiologically in the second, we must always conclude that the *vital electro-chemical* current either is the same thing as that which has been called *neuro-electric*, or *electro-vital*; or even if a difference should exist between them, in the present state of physical and physiological knowledge, we cannot point out with certainty any character which distinguishes them.

The third kind of current arises from the different degree of temperature of the different parts of the animal which is subjected to the operation. It is for this reason that the name of *thermo-electric* current has been given to it. It is derived from nothing else but a disturbance of calorific equilibrium; on this account it is reckoned amongst the phænomena of matter, and has no essential connexion with life. Besides, this current is, so to say, null or of very small importance, since physicists have proved that the temperature of animals is nearly equal in every part of their system.

The authors again inform us that the deviations of the needle of the galvanometer indicating the currents, were always measured by them, reckoning from the first excursion which the needle makes at the moment when the second stylet is thrust in, that is to say, at the moment when the circuit is completed, which being closed, the needle never returned to its first position, but, on the contrary, it always kept itself at a distance all the time that the experiment and the deviation lasted, according to a fixed or certain mark, sometimes of three, sometimes of four, of seven, and even of fifteen degrees. Twenty-seven experiments are described in this memoir: the authors sum up the results of them in five propositions, which are the following:—

Prop. 1. *In warm-blooded animals there is an electro-vital or neuro-electric current, which we will call external or cutaneous, which is found in the cutaneous tissue, and directs itself constantly from the extremities to the cerebro-spinal axis by means of the galvanometer. The intensity of this current, according to the experiments which have been made, is generally greater with stylets of iron than with those of silver.*

Six experiments support this proposition.

Prop. 2. *In warm-blooded animals there is an electro-vital current, which goes from the cerebro-spinal axis to the internal organs situated beneath the skin; for this reason we will call it electro-vital internal current. By means of the galvanometer we see that it is constantly directed from the cerebro-spinal axis to the other viscera, or, if you will, from the nerve to the muscle. The intensity of the internal current is in general greater with the iron stylets than with those of silver.*

Eight experiments served to establish this proposition.

Prop. 3. *The electro-vital current in warm-blooded animals grows weaker in proportion as it comes less from life: death having taken place, it proceeds in an opposite direction to that in which it was directed during life.*

This proposition rests upon eight experiments.

Prop. 4. *Pain weakens or suspends the electro-vital current; it even changes the direction of it if it is very great. The voluntary or convulsive automatic movements give, on the contrary, a very strong current, which may be named discharge of current.*

A single experiment appeared sufficient to the authors to establish this proposition.

Prop. 5. *The electro-vital current either cannot be discovered or measured, or does not really exist in the different parts of the same viscus: it is very feeble, and perhaps it is even null from viscus to viscus.*

Four experiments support this proposition.

The experiments which have served to establish each of the five propositions are followed by summary annotations of the authors. Although they appear to me to be very interesting for one who is occupied in such researches, I did not think it right to translate them, in order to avoid making the report as long as the memoir itself.

Such are the conjectures which the authors have thought they might publish, and which new experiments will come to destroy or corroborate. They have described them without any sectarian spirit, and only as physiological hypotheses, which, if they are found not unworthy of mature examination, may serve as guides for new experiments.

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

[Continued from p. 212.]

Black Substance from Sulphuric Acid and Alcohol.

THE nature of the peculiar black substance originating from the action of sulphuric acid on alcohol was first pointed out by O. Z. Erdmann, who concluded from his experiments that it was composed of a substance nearly approaching the formula $C^{50} H^{45} O^4 \dddot{S}$, and of an incombustible residue consisting of sulphatic salts. Subsequently, L. Lose, who had obtained this substance whilst preparing the isæthionic salts, was induced to examine it, and obtained somewhat different results, leading to the formula $27 C\ 21 H\ 6 O\ \dddot{S}$. The difference between these results and those of Prof. Erdmann, M. Lose attributes to a portion of the substance being subtracted as fixed salts, thus giving rise to a smaller amount of sulphur and a larger amount of carbon and hydrogen. Recently M. Erdmann has made known some new researches on this subject; and since, as far as we are aware, no notice has appeared in England of the former inquiries, we shall make our present extract rather full.

It was prepared in the first instance by heating six parts of fuming sulphuric acid, which left but a slight residue on evaporation, with one of alcohol of 0·83 sp. gr. in a retort till the evolved gas was no longer combustible; the residue then frequently treated with boiling water of ammonia, and at last washed with boiling water till the filtrated liquid no longer evinced the presence of sulphuric acid. Dried in pieces it has the appearance of coal, and is difficult to pulverize. On combustion it left behind a very small residuum, which dissolved easily in nitric acid, and gave a slight precipitate with the chloride of barium. The substance dried by exposure to the air gave off much water on being heated. Desiccation was effected *in vacuo* at 130°–150° C. It does not decompose even at 170° C. Hess's apparatus was employed for the analysis. The substance was placed in a weighed glass capsule of the form of a boat, this fixed in the tube between two coils of fine platinum wire to prevent its coming into contact with the oxide of copper, so that the weight of the incombustible residue might be determined. Between the tube containing the chloride of calcium and the apparatus for the potassa, a tube with oxide of lead was inserted, but no sulphurous acid appeared to be formed. To determine the sulphuric acid the substance was burnt with a mixture of nitre and carbonate of soda. Mean of the experiments C 66·11, H 3·78, S 8·92, O 21·19. It was then prepared according to the method employed by M. Lose, by passing alcoholic vapours into heated fuming sulphuric acid, and purified as above. The product was similar, and likewise left an incombustible residue, which was determined in the same manner. The analysis gave C 64·30, H 3·33, S 7·49, O 24·88. It was further prepared by boiling pure distilled sulphuric acid with alcohol; when the mass began to thicken from the formation of the coaly substance, the experiment was interrupted and a portion was removed, and cleaned by boiling with ammonia and water; the second portion was so long exposed to a temperature of 180° C. till the evolved gas was no longer combustible, and appeared to consist solely of sulphurous and carbonic acids; it was then washed and dried. Both products had the same appearance, and were subjected to analysis, the mean of which = C 65·58, H 4·55, S 8·38, O 21·49.

On treating the substances which had been employed in the preceding experiments with a solution of caustic potash, they all evolved such considerable quantities of ammonia that this could only be regarded as adhering mechanically to them. The treatment with potassa was continued at the boiling point till no smell of ammonia was perceptible, and the residues were then washed with boiling water. On combustion they now left considerable quantities of sulphate of potash. The black substance can therefore combine with bases, and the residue found by Erdmann in his previous analyses of the substance, which had been treated with potash to remove the sulphuric acid, was evidently sulphate of potash. Although the analyses communicated were made with ammoniaferous products, and cannot consequently give the correct composition of the black acid, yet as they had all been treated in the same way with an excess of ammonia they should evince a coincidence did the black substances possess a constant composition. But this is not the case. The products obtained by boiling sulphuric acid with alcohol, appear to differ according to the duration of the action and temperature: thus one product washed solely with water gave 63·8 C, and 3·3 H; another gave 64·3 C, and 2·4 H. At last it was obtained from three preparations of the same composition, by boiling eight to ten parts concentrated sulphuric acid with one of absolute alcohol in a sand bath at 180° C. till the mixture had lost all fluidity. The mass was then washed with water till the filtered liquid gave no precipitate with chloride of barium. The free acid, which most reddens litmus paper, as well as the potash salt, were analysed. Acid = C 67·66, H 3·34, S 6·65, O 22·35. Potash salt = C 59·78, H 2·91, S 5·70, O 20·26, Ka 11·35. Treatment with ammonia, acetate of lead, chloride of barium, gave no definite salts. Erdmann calls it thiomelanic acid, and observes that the coincidence between the found and calculated results gives to the formula advanced ($C^{80} H^{48} S^{3} O^{20}$) a high degree of probability, but that no especial value can be placed in it, as, with the exception of the agreement in composition of the substance from three different preparations, there is no surety for its purity. On employing a higher temperature than the one stated, and on heating the mass till it began to solidify, a product was obtained, the composition of which was essentially different.

For the sake of comparison the black substance was examined, which is formed from the action of sulphuric acid on pyroxylic spirit, but the composition of the substance obtained proved to be very different from that of the thiomelanic acid, viz. 67·14 C, 1·73 H, 1·40 S, 29·73 O. (*Journal für praktische Chemie*, vol. xv. p. 1.—*Poggendorff's Annalen*, vol. xlvii. p. 619.)

Action of Chlorine on Animal Substances.

M. Mulder has continued his researches on the action of chlorine on animal substances. Albumen (from eggs) was mixed with water, filtered, and submitted to a current of chlorine; very shortly white flocks were formed, which gradually increased; the precipitate was put into a filter and edulcorated; it appeared to be somewhat soluble in water. It was dried at 80°, at which water was driven off, and it shrunk together; became white with a trace of yellow; it was then dried at 100° C. Formula $C^{40} H^{62} N^{10} O^{12}, \underline{\overset{\cdots}{Cl}}$. The same compound was obtained by the action of chlorine on ammoniacal solutions of casein and fibrin. Mulder calls it proteinchlorous acid. The fluid from which this new substance has been separated contains hydrochloric and chlorous acids;

hence water must have been decomposed; the chlorous acid produced combined direct with the protein. The fluid contains a little of a brown matter, probably humin, but in very small quantity. Proteinchlorous acid is straw-yellow, has a greasy feel, like proteinsulphuric acid, insoluble in alcohol and æther, almost so in water; soluble in, without colouring, strong sulphuric acid; by water white flocks are precipitated. Nitric acid dissolves it at ordinary temperatures in the course of some days and forms xanthoproteinic acid. Soluble in cold hydrochloric acid; does not form humin as protein does. Soluble in baryta water; the solution gives precipitates with nitrate of copper and zinc. Soluble in ammonia, with evolution of nitrogen; when evaporated leaves a residue soluble in hot water; from this solution alcohol precipitates a new organic matter, which is soluble in cold water; it must be washed with alcohol and dried at 130°; it is called oxyprotein. It is a yellow powder, and, although often treated with boiling alcohol, generally contains a little chlorine. Formula $C^{40} H^{64} N^{10} O^{16}$. Oxyprotein is easily pulverizable; of an amber-yellow colour; heavier than water; soluble therein; almost insoluble in alcohol, quite so in æther; soluble in hot sulphuric and hydrochloric acids; changed by nitric acid into xanthoproteinic acid; soluble in potash, soda, ammonia and baryta water, and the solutions give no precipitate with ferrocyanuret of potassium; sulphuric acid gives a white one, soluble in the heated solution. Solution of galls gives a precipitate: nitrate of silver, acetate of lead and copper, and chloride of iron are precipitated by it. The copper salt is $C^{80} H^{126} N^{20} O^{31}. Cu O$, probably $(C^{40} H^{62} N^{10} O^{15}, Cu O) + (C^{40} H^{62} N^{10} O^{15} + H^2 O)$. Proteinchlorous acid forms a peculiar salt with baryta: the precipitates produced in metallic solutions by this salt have been analysed, but their composition is as yet doubtful. Mulder calls them chloroxyproteates. (*Annalen der Chemie und Pharmacie*, xxxvi. p. 68.)

When hæmatin is treated with chlorine it becomes white; all iron is extracted and remains in the solution with hydrochloric acid and a little organic matter.

The white flocks, when dried at 100°, still smell of chlorous acid, their formula is $C^{44} H^{44} N^6 O^{24} Cl^{12} = C^{44} \quad H N^6 O^6 + 6 (Cl^2 O^3)$, or one atom of hæmatin and six of chlorous acid.

The new compound is light yellow, insoluble in water, soluble in alcohol and æther. Dissolved by strong sulphuric acid, decomposed by heating, and sulphurous acid is evolved. Soluble in ammonia with evolution of nitrogen; soluble in potash and soda with reddish colour.

Xanthoproteinic acid is prepared by acting on several protein compounds with nitric acid; the dried product must be often treated with water and boiling alcohol. It was dissolved in ammonia, evaporated, dissolved again in water, and the solution treated with chlorine: the red colour gradually disappeared, and a whitish or yellowish flocculent precipitate was produced, which when washed and dried at 100° is lemon-yellow coloured. Formula $C^{68} H^{100} N^{16} O^{26} \underline{\overset{\cdots}{Cl}}$, or $2 (C^{34} H^{48} N^{8} O^{12} + H^{2} O) + Cl^{2} O^{3}$. Heated above 100° it loses water and some chlorous acid. With ammonia it evolves nitrogen; if the fluid be evaporated and the sal-ammoniac extracted with alcohol, the yellow acid remains.

When xanthoproteinate of ammonia is heated to 140° the ammonia is driven off, and the hydrated acid remains. Formula $C^{34} H^{50} N^{8} O^{13} = C^{34} H^{48} N^{8} O^{12} + H^{2} O$.

Action of Potash and Soda on Indigo—Anilin.

M. Fritzsche has found, that when finely-powdered indigo is thrown into a hot and very concentrated solution of caustic potassa or soda contained in a retort, the blue colour is destroyed and a reddish-brown mass produced, containing a peculiar acid; when this is still further heated, a volatile body is given off, which condenses in the neck of the retort into oily drops, and ammoniacal water distills over. By re-distillation this oil may be obtained colourless; its quantity is about 18 or 20 per cent. of the employed indigo; he calls it *anilin*. Specific gravity 1·028; refracts light powerfully, and has a strong aromatic but at the same time disagreeable smell; is but little soluble in water; mixes in all proportions with alcohol and æther. Exposed to air anilin becomes yellow, and at length is metamorphosed into a resin. Boils at 228° C. It dissolves sulphur. Anilin contains no oxygen; its formula is $C^{12} H^{14} N^{2}$. Atomic weight 1181·6. It forms salts with acids; with the oxyacids it combines and takes up one atom of water; with the hydracids it unites directly; in this respect it is similar to ammonia. Oxalate of anilin is formed when alcoholic solutions of anilin and of oxalic acid are mixed together; it is a white powder, which is to be washed with alcohol, and then dissolved in hot water, from which it separates on cooling in long crystals. Formula $C^{12} H^{14} N^{2} + C^{2} O^{3} + H^{2} O$. (*Annalen der Chemie und Pharmacie*, xxxvi. p. 84.)

Hydrochlorate of anilin is obtained by mixing anilin with hydrochloric acid and re-crystallization of the salt, which is easily soluble in water, $C^{12} H^{14} N^{2} + Cl^{2} H^{2}$.

Other salts have been formed but not examined; the action of iodine and nitric acid seems to be very interesting, but has been as yet so superficially examined, that it is of no use making any extract. In a note to the above treatise, Prof. Erdmann states it to be very probable that anilin is nothing more than Unverdorben's krystallin.*

Palmitic Acid from Palm Oil.

By saponifying palm-oil with potassa, Fremy obtained two acids, one of which he calls palmitic acid; it is volatile: by the distillation a very small quantity of oil is produced, from which it may be freed by means of alcohol.

The composition of the free hydrated acid is $C^{64} H^{124} O^6$, $H^4 O^2$; in the salts, however, it is $C^{64} H^{124} O^6$. The distilled acid has the same composition. The ammonia salt is insoluble in water, and has the formula $C^{64} H^{124} O^6$, $N^2 H^8 O + H^2 O$.

The æther is obtained by acting on alcohol with sulphuric and palmitic acids; it is solid, melts at 21° C., crystallizes in prisms. Formula $C^{64} H^{124} O^6 + 2 (C^4 H^{10} O)$; the acid is therefore bibasic. By the action of chlorine on palmitic acid a mixture of several products is obtained, which it is almost impossible to separate: one of them has the formula $C^{24} H^{108} Cl^{16} O^6$, $H^4 O^2$. (*Annalen der Chemie und Pharmacie*, xxxvi. p. 44.)†

Oils of Cascarilla and Caraway.

M. Vœlckel has examined cascarilla and cumin or caraway oils. The impure cascarilla oil is deep yellow; specific gravity 0·909; boils at 180° C. By distillation with water seemed to be separated into two oils, which, however, could not be perfectly isolated. The oil which first passed over was colourless, highly refractive, specific gravity 0·862; boiled at 173° C.: composed of carbon 86·926, hydrogen 10·487, oxygen 2·587. The portions which passed over afterwards were a little thicker, somewhat yellowish, and of a less agreeable odour. Composition, carbon 82·021, hydrogen 10·267, oxygen 7·712. (*Annalen der Pharmacie*, xxxv.)

Oil of caraway may be separated by distillation into several different oils; the first two portions boil at 193° to 195° C., the third at 213°, the fourth at 228°. The smell was almost the same in all.

C.	86·099	85·204	82·126	78·603
H.	11·090	10·517	9·827	9·217
O.	2·811	4·279	8·047	12·180

Caraway oil absorbs hydrochloric acid gas, but is separated again by distillation with water.

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* Some further particulars relating to *Anilin* will be found in our *Intelligence and Miscellaneous Articles*.—Edit.

† See Mr. Stenhouse's paper on the constituents of Palm-oil, in our last Number, p. 186.—Edit.

Caraway Oil—Æthereal Oils.

MM. Gerhardt and Cahours have made some experiments on æthereal oils. Roman caraway oil (essence de cumin) consists of two oils; one is a hydro-carbon, and boils at 165° C.; the other consists of carbon, hydrogen and oxygen, and is similar to hydro-benzoyl; they call the hypothetical radical cumyl.

Hydro-cumyl .... = $C^{20} H^{22} O^{2} + H^{2}$
Cuminic acid .... = $C^{20} H^{22} O^{2} + O$
Chloride of cumyl .... = $C^{20} H^{22} O^{2} + Cl^{2}$
Bromide of cumyl .... = $C^{20} H^{22} O^{2} + Br^{2}$
Hydrated cuminic acid ... = $C^{20} H^{22} O^{2} + O + H^{2} O$.

Hydro-cumyl is a colourless or slightly yellow-coloured liquid of strong odour, is changed by slow distillation, except when the operation is performed in an atmosphere of carbonic acid. By the absorption of oxygen is converted into cuminic acid; the metamorphosis takes place more rapidly when it is treated with caustic alkali, in which case hydrogen is evolved. Cuminic acid is colourless, crystallizes in prismatic needles, scarcely soluble in water, easily in alcohol, may be sublimed, and has an acid burning taste. Hydrocumyl treated with sulphuric acid and chromate of potash also forms cuminic acid. Hydrated cuminic acid distilled with four parts of caustic baryta gives an aromatic colourless liquid, which boils at 144° C.; its formula is $C^{18} H^{24}$; may be called cumen*; with fuming sulphuric acid forms cumensulphuric acid, which gives a crystallizable salt with baryta. With nitric acid forms nitrocumid, like nitrobenzid. (*Annalen der Pharmacie*, xxxv.) †

Potatoe-Spirit Oil—Oil of Anise—Oil of Fennel.

M. Cahours has examined some other oils. *Potatoe-spirit oil* is converted by perchloride of phosphorus into a fluid, which boils at 102°, and has the formula $C^{10} H^{22} Cl^{2}$; it is the chloride of amyl. Acetate of amilen is decomposed by chlorine (in the shade), it is changed into $C^{10} H^{18} Cl^{4} O + C^{4} H^{6} O^{3}$; in sunshine a crystalline substance is formed. Potatoe oil is converted by the action of air and spongy platinum into valerianic acid. (*Annalen der Pharmacie*, xxxv.)

Oil of anise.—The solid oil has the formula $C^{22} H^{26} O^{2}$. The specific gravity of the vapour is 5·68; with chlorine forms a fluid compound, $C^{22} H^{20} Cl^{6} O^{2}$, with bromine a crystalline one, $C^{27} H^{20} Br^{6} O^{2}$. With dilute nitric acid of 20° B

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* May not Cumen be identical with Mesitylene? Their boiling points differ very little,—276° and 287°,—and their composition is the same.—R. K.

† See also our *Intelligence and Miscellaneous Articles.*—Edit.

forms an acid whose formula is $C^{16} H^{14} O^{6}$; the silver salt is $C^{16} H^{12} O^{5} + Ag O$. With concentrated nitric acid the anisonitric acid is formed, $C^{16} H^{12} O^{6} (N^{2} O^{4})$; its silver salt, $C^{16} H^{10} O^{5} (N^{2} O^{4}) + Ag O$. [I may here remark that these two acids were prepared independently, in the beginning of last summer, by Dr. Weltzien, in Prof. Mitscherlich's laboratory: we may probably in a short time expect a better account of them than the above.—H. C.]

Anise oil gives with hydrated sulphuric acid a crystalline compound, with fuming sulphuric acid a peculiar acid, whose baryta salt is soluble.

Oil of fennel consists of two oils; the solid oil has the same formula as the solid oil of anise; the fluid oil has the formula of oil of turpentine: the latter combines with binoxide of nitrogen and forms a crystalline compound, $C^{15} H^{24} N^{2} O^{4}$.

Oil of Bergamot.

Oil of bergamot (from the fruit of *Citrus bergamia*) has been examined by MM. Soubeiran and Capitaine. The oil was distilled with water and dried with chloride of calcium; its specific gravity was 0·869; it refracted the polarized ray 25°. It was again distilled with water and the product collected in four portions: the first refracted 45°, the second 38°, the third 21°, and the fourth not at all. Oil of bergamot must therefore consist of at least two oils. Ohm found that oil of bergamot contained 7·098 per cent. oxygen; and he considers it as a hydrate of lemon oil, $3 (C^{10} H^{16}) + 2 H^{2} O$; according to MM. Soubeiran and Capitaine, the fractionated portions have different compositions; the oxygen varies from 3·37 to 16·14. By the action of anhydrous phosphoric acid an oil is obtained which has the same composition as citron and lemon oil: this body combines with hydrochloric acid gas. The camphor appears to have the usual formula. By the action of the phosphoric acid on the impure oil a peculiar acid is produced (acide phospho-bergamique), which forms soluble salts with lime and oxide of lead. (*Annalen der Pharmacie*, xxxvi.)

Crystallized Substance in Guarana.

MM. Martius, Berthemot and Deschastelus have examined the crystallized substance contained in "guarana;" the nature of guarana was for a long time doubtful, until Martius showed that it was a paste made of the fruit of a tree—*Paullinia sorbilis*, Mart. The guaranin is contained in it in combination with tannic acid, and it appears that the so-called guaranin is identical with caffein. As it has already been shown that thein is also identical with caffein, we have now three families in which it is found, viz. Rubiaceæ, Theaceæ and Sapindaceæ. (*Annalen der Chemie und Pharmacie,* xxxvi. p. 90.)

Action of Chlorine on Marsh Gas.

By the action of chlorine on natural marsh gas, M. Melsens has obtained the chloride of carbon, $C^2\,Cl^8$; hence marsh gas and the gas obtained from the acetates are perfectly identical.

Magnus's *and* Gros's *Salts of Platinum.*

If protochloride of platinum or Magnus's salt, $Pt\,Cl^2,\ N^2\,H^6$, be boiled with ammonia, the green compound is dissolved, and the evaporated fluid deposits acicular crystals; these are soluble in water and ammonia, precipitated by addition of alcohol; when heated evolves sal-ammoniac, and at last free hydrochloric acid; the residue is metallic platinum; by long boiling with potassa ammonia is also evolved. M. Reiset finds the formula $Pt\,Cl^2,\ N^4\,H^{12}$, consequently it is the radical in the salts of M. Gros. These salts have the general formula $Pt\,Cl^2,\ N^4\,H^{12} + O + \bar{A}$. When Reiset's salt is treated with nitric acid red vapours are evolved, and Gros's nitric salt is formed. When the aqueous solution of $Pt\,Cl^2,\ N^4\,H^{12}$ is treated with chlorine, Gros's chlorine salt is produced, viz. $Pt\,Cl^2\ N^4\,H^{12}\,Cl^2$. (*Annalen der Chemie und Pharmacie,* xxxvi. p. 3.) *

Gros's *Platina Salts.*

In the 19th *Arsberattelse*, p. 261, when speaking of Gros's platina salts, Berzelius protests against the introduction of so many improved theories, and says that a much simpler formula for these salts would be $Pt\,\underline{Cl} + (2\,N\,H^3 + \underline{Cl})$, if indeed this latter compound had been prepared in a separate state. He moreover says,—If the formula $\underline{N}\,\dot{\underline{H}}^4\,\dddot{S} + Pt\,\underline{Cl}\,\underline{N}\,\underline{H}^2$ does not explain why the salt is not precipitated by chloride of barium or nitrate of baryta, is the phænomenon any better explained, when we throw ammonia, water, chloride of platinum and amidogen together into one base? Hydrochloric and nitric acids also have an affinity for this base, and therefore the sulphate compound ought to be decomposed by the baryta salts, &c. *

Prussian Blue.

Leopold Gmelin has remarked, that the Prussian blue obtained from a protosalt of iron by means of the ferrosesqui-

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* See Dr. Kane's paper "On a New Class of Platina Salts," in the present Number.—Edit.

cyanuret of potassium cannot have the same composition as that obtained from a sesquioxide or persalt, and the ferrocyanuret; the former must contain three atoms of protocyanuret of iron and one atom of sesquicyanuret, while the common blue contains two atoms of sesquicyanuret of iron. M. Vœlckel has analysed the former substance in Wöhler's laboratory; it was prepared by adding ferrosesquicyanuret of potassium to excess of protochloride of iron. It contained, iron 25·589, potassium 5·284, cyanogen 33·684. Probable formula, $(Fe^2\,Cy^6 + 3\,K\,Cy^2) + 4\,(Fe^2\,Cy^6 + 3\,Fe\,Cy^2.)$ (*Annalen der Pharmacie*, xxxv.)

Double Carbonate of Lead and Soda.

During his experiments on the atomic weight of carbon, Berzelius discovered a double salt of carbonate of lead with carbonate of soda; it is obtained by dropping a solution of nitrate of lead into an excess of solution of carbonate of soda, and boiling the mixture. Its formula is $\dot{Na}\,\ddot{C} + 4\,\dot{Pb}\,\ddot{C}$. (*Arsberattelse*, xix.)

Acid Products of Citric Acid at high temperatures.

The researches of Lassaigne, Baup, Dahlstroem, Berzelius and Robiquet, had led us to suppose that three acids were formed from the decomposition of citric acid at high temperatures, which is fully confirmed by a series of experiments by G. L. Crasso, recently published in the *Annalen der Pharmacie* (vol. xxxiv. p. 53). The first formed is the aconitic acid of Dahlstroem and Berzelius (Baup's *acide citridique*) $C^4\,H^2\,O^3 + Ag$; then the itaconic acid (pyraconitic acid, Lassaigne's pyrocitric acid, and Baup's *acide citricique*) $C^5\,H^4\,O^3 + Ag.$; and lastly, citraconic acid (pyrocitric acid of Robiquet and Lassaigne, Baup's *acide citribique*), isomeric with the preceding. The two last appear to be bibasic, the first tribasic. No aconitic acid is formed previous to the disengagement of aceton and carbonic oxide. The two other acids are products of the aconitic acid. No distillation was continued longer than ten minutes. The acid first melts and gives off pure water, upon which white vinous acid and combustible vapours are disengaged, which evidently contain aceton and carbonic oxide. If the operation be now interrupted, the residue dissolved in water and brought to crystallization, pure unaltered citric acid is obtained, of which the analysis gave the formula for the citric acid dried at 100°, viz. $C^{12}\,H^{10}\,O^{11} + 3\,H^2\,O$; and besides which, a small portion of uncrystallizable mother-ley proportional to the quantity of aceton and carbonic oxide evolved. When further heated the white vapours disappear, the products are no longer combustible, and the recipient contains an acid colourless fluid abounding in aceton. When white vapours are no longer visible, oily stripes are formed in the neck of the retort, indicating the formation of a new product. If the yellow residue be dissolved in water it forms no crystals even at 0° cent., when evaporated to the formation of a pellicle, and the cold and solidified mass then treated with æther, but little citric acid remains undissolved. The æther on evaporation leaves behind a warty granular mass—the aconitic acid. Since citric acid does not form an æther on treating its alcoholic solution with hydrochloric gas, the aconitic acid may be thoroughly purified by dissolving it in five parts of absolute alcohol, and passing hydrochloric gas through the solution. The aconitic æther is then decomposed with a vinous solution of potash, the potash salt by acetate of lead, and the washed precipitate decomposed with sulphuretted hydrogen; it is then obtained as a laminar crystalline acid, which on resolution in æther and slow evaporation appears colourless and verrucous-crystalline: it consists of C 41·84, H 3·41, O 54·75. The silver salt was employed to determine the atomic weight, which was found to be 618·11; the formula is, therefore, $C^4 H^2 O^3 + H^2 O$. The silver salt is white, not quite insoluble in water; formula $= C^4 H^2 O^3 Ag O$. Aconate of barytes is gelatinous, when dried, amorphous. The salts of potash, soda and ammonia, do not crystallize distinctly, are easily soluble in water and in alcohol. Aconitic æther, the preparation of which has already been described, is colourless, of aromatic smell and bitter taste; specific gravity = 1·074 at +14° C.; boils at 236°, above which it decomposes almost entirely into white vapours and a black fatty residue.

Formula $= C^8 H^{12} O^4 = Ae\,O\,\overline{At}$.

The same æther has likewise been prepared by Dr. Marchand in the following manner. A mixture of half a pound of citric acid, three quarters of a pound of alcohol, and a quarter of a pound of concentrated sulphuric acid was distilled in a sand bath for about four hours, the liquid that passed over being constantly poured back. At last only sulphuric æther mixed with a little alcohol passed over, and the mass had a very dark brown colour. On mixing with water a quantity of coloured æther separated, which was washed with water till acid reaction ceased. When decolorated by means of animal charcoal, it formed a yellow fluid, very similar to the usual citric æther. Its boiling point above 230°. It could not be distilled without decomposition, so that it was impossible to take the specific gravity of its vapour. Dr. Marchand arrives at the same formula, $C^8 \ddot{H}^{12} O^4$, and states that this therefore is a combination of the oxide of ethyl, with an acid of the same constitution as aconitic acid.

The free aconitic acid is easily soluble in water, alcohol and æther; decomposes on being heated above its melting point, disengaging pyraconitic acid, empyreumatic oil, and leaving behind a black pitchy mass, which subsequently carbonizes entirely.

Itaconic acid (pyraconitic acid) is best obtained by heating the aconitic acid in a retort till the evolution of yellow vapours occurs. It is, further, this acid which forms the oily bands when the second period in the distillation has commenced: carbonic acid and water appear simultaneously with it. It condenses to an oily mass which sometimes passes into a crystalline solid. As soon as yellow vapours and empyreumatic oil begin to appear the distillation is to be stopped. On slowly cooling acicular crystals are formed in it, and the remaining mass no longer solidifies. On being rapidly cooled the oily acid solidifies entirely, and may be obtained pure by solution in six times its volume of water, and slow evaporation; colourless by recrystallization from alcohol or æther. From the aqueous solution it crystallizes in the form given by Baup for the citricic acid. The crystals give off no water at 100° cent., and consist of 46·62 C, 4·56 H, 48·82 O, agreeing perfectly with Baup's analysis of the *acide citricique*. On being heated to its boiling point, it passes into the isomeric volatile citraconic acid. The acid potash salt is easily soluble, crystallizes in small shining laminæ, which lose one atom of water at 100°; the dried salt contains 28·06 per cent. potash. The neutral baryta salt crystallizes in long slender stellate groups, which lose no water at 100°, it contains 54·92 per cent. barytes. The acid salt forms crystalline crusts. Neither salt of lime is distinctly crystalline. The neutral strontia salt resembles that of barytes, and contains 45·69 per cent. strontia. The silver salt forms a white powder nearly insoluble in water; formula $= C^5 H^4 O^3 \dot{A}g$. The atomic weight determined from the silver salt = 707·12, and its symbol $\overline{It}$. Itaconic æther had already been described by Malagutti as pyrocitric æther; it is colourless, of aromatic odour and bitter taste; specific gravity = 1·050 at 15°, boiling point = 227°; it is partially decomposed at a higher temperature: it consists of $C^9 H^{14} O^4 = Ae\,O, \overline{It}$.

The same æther has likewise been prepared by Marchand; by continuing the distillation of his mixture for a longer time, he found for it the same constitution.

The *citraconic acid* is identical with Baup's acide citribique and Robiquet's anhydrous pyrocitric acid. It is obtained by distilling the itaconic acid, which gives water and citraconic acid, which forms the inferior layer. On distilling afresh it is obtained anhydrous: or it may be obtained by treating the non-crystalline residue remaining from the itaconic acid with acetate of lead, and decomposing the precipitate with sulphuretted hydrogen. The acid is void of colour and smell, has a strong acid acrid taste, sinks in water, does not mix with it, but on longer contact soluble in it in every proportion; specific gravity = 1·247 at 14°; begins to volatilize at 90°, boils at 212° cent.; heated further it leaves a carbonaceous residue. Attracts moisture from the atmosphere, first becoming crystalline, then deliquescing. The hydrate crystallizes in four-sided prisms, which melt at 80°, and volatilize entirely a few degrees higher; soluble in water, alcohol and æther. On being heated in a retort they first give off water, and then anhydrous acid. The anhydrous acid = 54·12 C, 3·53 H, 42·35 O; the crystallized hydrate = 46·62 C, 4·56 H, 48·82 O. The atomic weight was determined from the anhydrous salts of silver, lead and barytes, and was found to be 715·40; its formula, therefore, = $C^5 H^4 O^3$, the symbol $\overline{Ct}$. Citraconic æther is prepared in the same way as itaconic æther, with which it agrees in every respect; its formula $C^9 H^{14} O^4 = Ae\,O, \overline{Ct}$. Anhydrous citraconic acid absorbs ammonia with violent disengagement of heat, forming a yellow, ductile, transparent, hygroscopic body, easily soluble in water and alcohol. This anhydrous citraconate of ammonia = $2\,C^5 H^4 O^3 + N^2 H^6$. Dissolved in water and evaporated, small crystalline laminæ of the same salt are obtained as crystallizes from the acid solution by saturating direct with ammonia; formula = $2\,C^5 H^4 O^3 + N^2 H^6 + H^2 O$. The neutral potash salt is easily soluble, forms a pulverulent mass; the acid salt crystallizes in easily soluble laminæ. Both soda salts easily soluble, non-crystalline. The acid salt of magnesia transparent, radiately crystalline. The neutral lime salt is amorphous, easy of solution; the acid salt crystallizes in prisms and laminæ; formula $2\,C^5 H^4 O^3 + Ca\,O, H^2 O + 3\,Aq$; lose one atom of water at 100°, three atoms at 120°, and at 140° blacken and evolve acid. The neutral salt of barytes is white, crystalline, of difficult solution in cold water; the acid salt forms white silky verrucous groups, loses no water at 100°, $= 2\,C^5 H^4 O^3 + Ba\,O\,H^2 O + Aq$. The neutral salt of strontia is not distinctly crystalline, efflorescent; the acid salt forms large colourless prisms, which become opake at 100°, and lose 26·19 per cent. in weight; at 120° gives off acid. The silver salt is only thrown down after an addition of ammonia, as a gelatine of difficult solution in water. From its boiling solution it crystallizes in long, thin lustrous needles, loses nothing at 100°, and burns with slight explosions; formula $C^5 H^4 O^3$ Aq.O. From the liquid filtered from this salt are deposited on evaporation crystals in six-sided columns, which at 100° lose one atom of water. The citraconate of lead exists in four different states. From an aqueous solution of the acid, to which some ammonia has been added, acetate of lead throws down a voluminous precipitate, which on being boiled is partially dissolved, but the greater portion is converted into an insoluble crystalline powder $= C^5 H^4 O^3 + Pb O$. From the filtered liquid crystallizes on cooling a non-crystalline powder $= C^5 H^4 O^3 + Pb O +$ Aq. If the neutral salt of ammonia be precipitated by the neutral acetate of lead a gelatinous precipitate is formed, which dried immediately becomes opake, slightly yellow and gummy, $= C^5 H^4 O^3 + Pb O + 2$ Aq. Basic acetate of lead throws down from citraconatic salts a white nearly insoluble crystalline powder $= C^5 H^4 O^3 + 2 Pb O$. The citraconate of zinc, and of the protoxide of mercury, are white and nearly insoluble. The neutral salt of nickel green, the acid salt green, crystalline. The acid salt of cobalt red, granular. The acid citraconate of the protoxide of manganese forms an opake ductile mass. The hydrate of the peroxide of iron is but slowly dissolved by citraconic acid.

Citric acid therefore presents four distinct periods of decomposition: the first from the melting point to the evolution of gas, during which water of crystallization (?) is given off. The second period begins with the evolution of aceton and carbonic oxide, and pyrocitric or aconitic acid is formed. This is decomposed in the following period, under formation of carbonic acid and pyraconitic or itaconic acid. In the fourth period empyreumatic oil is given off, and the residue is carbonified. Three different acids therefore originate in fixed order. The aconitic acid is probably tribasic, the itaconic and isomeric citraconic acids bibasic.

Wackenroder has published in the *Archiv der Pharmacie* (xxiii. p. 266–279) a series of experiments on the amount of water of crystallized citric acid, from which he concludes that the two kinds of citric acid hitherto admitted with four and five atoms of water do not exist, or are never formed from the usual citric acid under known conditions; that the crystallized citric acid cannot exist with less than three atoms of water; that the aqueous fluid which Crasso obtained by dry distillation, and regarded as water of crystallization, is nothing but hygroscopic water; further, that the citric acid which Crasso separated from the residuous acid heated until the disappearance of white vapours, and which he found by analysis to be $\overline{\text{Ci}} + 3\,\overline{\text{Aq}}$, must no longer be considered as citric acid dried at 100° C., but is the usual crystallized citric acid which has remained unchanged.

Wackenroder likewise contradicts the assertions of Robiquet and Crasso respecting the insolubility of citric acid in æther: he found that rectified æther, dry as well as moist, dissolved abundantly the melted acid, that the solutions did not become milky in closed vessels, and on evaporation gave a syrup which but slightly reddened dried litmus, and which on standing exposed to the atmosphere gradually passed into the crystallized state.

Before quitting this subject we may notice the observations of Berzelius (*Arsberettelse*, No. xviii.) on the views of Liebig and Dumas on the constitution of organic acids: he rejects them entirely, and states that the citrate of soda when heated to 190° may be a compound of two atoms of citrate of soda, and one atom of aconate of soda. In a remarkable paper "On certain Questions of the Day in Organic Chemistry," published in Poggendorff's *Annalen*, xlvii. p. 289, which is principally directed against Liebig's article "On the Composition of Organic Acids," *Annalen der Pharmacie*, xxvi. p. 113, Berzelius observes with regard to citric acid, that he has found that when the citrate of soda dried at 190° C. was dissolved in water and placed aside to crystallize, regular crystals of citrate but no aconate of soda was produced. As citrate of soda is insoluble in alcohol, but aconate somewhat soluble, he tried to extract the latter from the metamorphosed citrate by this means, but he obtained much less than the theory required. Citrate of silver when just precipitated is white and voluminous; it has then the formula $\dot{\text{A}}\text{q}\ C^4 H^4 O^4$, but by heating, or even by standing in the fluid, it becomes heavier, granular and crystalline, and has a composition similar to that of the metamorphosed soda salt. The metamorphosed silver salt was treated with alcohol and a quantity of hydrochloric acid not sufficient to decompose the whole of it; the mixture was well shaken until there was no more hydrochloric acid in the alcohol. The alcoholic solution when evaporated left a perfectly uncrystalline syrup, which, dissolved in water, let fall a trace of citrate of silver; out of the water was obtained a colourless syrup, similar to the aconitic acid, and which when saturated with carbonate of soda gives citrate of soda and aconate, which may be extracted by alcohol of 0·833, and the quantity of which fully agrees with that required by theory. Three atoms are decomposed, giving

$$2 \text{ atoms } \dot{A}q\,\overline{C} = 2\,(4C\;4H\;4O) + 2\,\dot{A}q, \text{ and}$$

$$1 \text{ atom } \dot{A}q\,\overline{Atc} = (4C\;2H\;3O) + \dot{A}q$$

The salt $2\,\dot{Na}\,\overline{Ci} + \dot{Na}\,\overline{Atc}$ is a chemical compound, which when dissolved in water attracts one atom of water, and forms three atoms of citrate of soda, &c.

Milk of the Cow-Tree.

We are indebted to Dr. Marchand for an examination of the milk of the cow-tree (*Palo de Vacca*). This vegetable milk had already been made the subject of inquiry by Boussingault and Boldero (*Ann. de Chim. et de Phys.* xxiii. p. 210), and by E. Solly, Jun. (L. & E. Phil. Mag., xi. p. 542), with whose paper Marchand appears to have been unacquainted. Solly's galactin seems to have been a mixture; he had, however, likewise recognized its difference from wax. According to the present examination, this milk would consist of water, fermentative sugar, lime, magnesia, combined with phosphoric acid, acetic acid (traces), butyric acid (?), resin $C^{10}\,H^{16}\,O$, resin $C^{20}\,H^{32}\,O$, resin $C^{50}\,H^{82}\,O$, so-called wax (Solly's galactin?), a substance resembling caoutchouc, $C^{41}\,H^{66}\,O^{3}$. We find, therefore, in the milk of the cow-tree only those substances which the milky saps of plants generally contain, but especially little albumen, and much of a caoutchouc-like substance. The resins are all compounds belonging to the radical $C^{5}\,H^{8}$.

1. Resin $C^{10}\,H^{16}\,O = 2\,(C^{5}\,H^{8})\,O$. The same composition as copaiva resin (according to Hess, however, this is composed of $C^{40}\,H^{62}\,O^{4}$).

2. Resin $C^{20}\,H^{32}\,O = 4\,(C^{5}\,H^{8})\,O$. Camphor oil, according to Martius, possesses the same composition. In the wax of *Ceroxylon andicola* Boussingault found a resin to which he likewise assigns this formula. Perhaps Mulder's anthiar resin is the same.

3. Resin $C^{50}\,H^{82}\,O = 10\,(C^{5}\,H^{8})\,H^{2}\,O$. This composition differs so much from those of the waxes that it can scarcely be reckoned in this class.

4. Caoutchouc-like substance $C^{40}\,H^{66}\,O^{3} = 8\,(C^{5}\,H^{8})\,H^{2}O + O^{2}$. The remarkable resemblance which this substance has to caoutchouc would lead one to believe that it is really identical, but according to Faraday's experiments the latter contains no oxygen. It is, however, possible, that caoutchouc in the state in which it is contained in the milky saps is capable of combining both with water and oxygen, and thus to produce a combination which from its properties calls to mind its origin. This view would agree perfectly

with Fourcroy's experiments, according to which, caoutchouc is precipitated from the milk-saps by the oxidizing action of the atmosphere. (*Journal für Praktische Chemie*, xxi. p. 43–54.)

Croconate of Copper.

According to L. Gmelin, when warm aqueous solutions of the croconate of potash and hydrochlorate or sulphate of copper are mixed, and the liquid let to cool, small rhombic prismatic crystals of the croconate of copper are deposited. They have a lively semi-metallic lustre, reflect light dark blue, transmit it of a brownish yellow. The powder is of an intense lemon-yellow colour. On being heated in a tube they give off water, then decompose violently under evolution of gas; the parts driven out of the tube burn with showers of sparks. The products of this decomposition are, first, a mixture of carbonic oxide and carbonic acid in variable proportions; second, an empyreumatic faint yellow acid fluid which smells of pyroligneous acid, colours the salts of iron brown, those of silver red (aldehydic acid?); third, a dull brownish-black coally residue, which on being heated, exposed to the atmosphere, burns lively to metallic copper and then to oxide. When the croconate of copper is heated in the atmosphere it gradually decomposes with slight explosions and showers of sparks. The analysis of the crystals, washed with cold water and dried in bibulous paper, gave C 23·36, H 2·23, O 43·41, Cu O 31·00. The formula for the crystallized salt is, therefore, $C^5 O^4 Cu O, + 3 H^2 O$. Of the three atoms of water two are expelled at 162°, the last does not go off without decomposition. (*Annalen der Pharmacie*, xxxvii. p. 58–65.)

The Fatty Acids.

An examination into the action and constitution of a series of fat bodies has been made under the guidance of Professor Liebig, the results of which have appeared in various numbers of the *Annalen der Pharmacie* for 1840. The questions to be solved were, do the solid fats, which contain not a trace of margaric acid, give margaric acid on dry distillation? How, and in what manner, can margaric acid originate from stearic or oleic acid? This question presupposed the most accurate knowledge of the composition of stearic, margaric and oleic acids. Professor Redtenbacher undertook the examination of stearic acid, and of the acid in the drying oils; Dr. Varrentrapp that of margaric acid, the oleic acid in the greasy oils, human fat, olive oil, and in almond oil. Connected with this question was the examination into the origin of sebacic acid, and of the substance characterized by its penetrating smell, to which Berzelius has assigned the name of Akroleïn, The composition and mode of formation of elaidic acid was undertaken, as his task, by Dr. Meyer. The accurate examination of coccostearic acid (coccinic acid), as well as of the products which originate from the action of nitric acid on fat bodies, was undertaken by M. Bromeis: the composition of the margaric acid in palm oil, and of the acid in cacao butter, by Mr. Stenhouse of Glasgow; on the composition of the fat substances of the nutmeg butter, and of the wax, by Mr. Playfair of Calcutta*.

[To be continued.]

XLIX. *Abstract of the History of a new class of Platina-salts, discovered by* M. Gros, *with some Remarks on their true Constitution.* *By* Robert Kane, *M.D., M.R.I.A.*†

THE very singular substance discovered by Magnus, and formed by boiling protochloride of platina in water of ammonia, is well known to consist of $Pt\,Cl + NH_3$. By this direct process it is formed but slowly and in small quantity, and a better mode of obtaining it was invented by Liebig. A solution of perchloride of platina is treated by a current of sulphurous acid gas, until the liquor becomes deep reddish brown, and ceases to precipitate a solution of sal-ammoniac. Being now brought to boil, and water of ammonia being added, Magnus's salt precipitates in fine deep green silky needles. In this way Gros obtained it in quantity sufficient for his researches.

When this substance is treated with warm nitric acid it (the acid) becomes brown, and finally converts the salt into a white powder. Red fumes are given off, and generally some double perchloride of platina and ammonium produced, and some metallic platina separated. The white salt is obtained pure by solution in warm water, from which it crystallizes on cooling. This salt is colourless, and in brilliant flat prisms. Heated with caustic potash it evolves ammonia, but not otherwise. It is not precipitated by sulphuretted hydrogen. On analysis it yields $Pt\,Cl\,N_3\,H_6\,O_6$.

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* [The authors had subjoined to this notice of the recent investigations respecting the fatty bodies, particulars of some of their results; but as several of the memoirs containing them have now appeared entire in our pages, together with an abstract of others, we deem it best to add merely the following references:—An abstract of the researches on the fatty substances by Redtenbacher, Varrentrapp, Meyer and Bromeis, was given in the Number for February, pres. vol., p. 113, preceded, at p. 102, by Dr. L. Playfair's paper on a new fat acid in the butter of nutmegs; and in our last number, p. 186, appears Mr. Stenhouse's memoir on palm oil and cacao butter.—Edit.]

† See p. 284. of the present Number.

When a solution of this salt is treated with sulphate of soda, a new compound separates in needles. This is also obtained by decomposing the first salt by dilute sulphuric acid, nitric acid being expelled. A solution of this salt yields no precipitate with nitrate of barytes, but gives sulphate of barytes when an excess of nitric or muriatic acid is added. Its analysis gives the formula $Pt\,Cl\,N_2\,H_6\,S\,O_4$.

On mixing a warm solution of the nitric acid salt with oxalic acid, a white powder, insoluble in water, is thrown down, which has the composition $Pt\,Cl\,N_2\,H_6\,C_2\,O_4$.

With a solution of common salt another compound is generated, the composition of which is $Pt\,Cl_2\,N_2\,H_6$.

When we compare the above formulæ with one another, we observe that there is common to all, the elements of an atom of protochloride of platina, one of amidogene and one of ammonium, $Pt\,Cl + N\,H_2 + N\,H_4$, and that the changeable element is such, as to form, with the ammonium, an ordinary ammoniacal salt. The simplest rational formula which could be devised for them, should therefore be to look upon the platina as having been raised to the state of percombination by the nitric acid, a chloramide being formed, $Pt + Cl\,N\,H_2$, resembling the bichloride, an atom of chlorine being replaced by amidogene, as an atom of oxygen may be replaced in chromic acid by chlorine. Then the formulæ of Gros's salts become

1. $(Pt + Cl\,.\,Ad) + (Am\,.\,O + N\,O_5)$.
2. $(Pt + Cl\,.\,Ad) + (Am\,.\,O + S\,O_3)$.
3. $(Pt + Cl\,.\,Ad) + (Am\,.\,O + C_2\,O_3)$.
4. $(Pt + Cl\,.\,Ad) + (Am\,.\,Cl)$.

MM. Gros and Liebig reject this view on the following grounds: 1st, that the difficulty which is found in expelling the ammonia from these compounds excludes the idea of the presence of an ordinary ammoniacal salt; and 2nd, that the sulphuric acid compound not immediately precipitating a salt of barytes, shows that common sulphate of ammonia does not exist therein. Gros therefore considers that the platina, chlorine, nitrogen, and hydrogen are all united together in a compound radical, which in No. 4. is united to chlorine, and in the others with oxygen and an acid. Thus, $Pt\,Cl\,N_2\,H_6 = X$ we have,

1. $Pt\,Cl\,N_3\,H_6\,O_6 = X\,.\,O + N\,O_5$.
2. $Pt\,Cl\,N_2\,H_6\,S\,O_4 = X\,.\,O + S\,O_3$.
3. $Pt\,Cl\,N_2\,H_6\,C_2\,O_4 = X\,.\,O + C_2\,O_3$.
4. $Pt\,Cl_2\,N_2\,H_6 = X\,.\,Cl$.

Liebig endeavours to establish a relation between this compound radical and ammonium, by proposing to consider the radical to consist only of $Pt\,Cl\,N_2\,H_2$; that this fulfils a function like that of azote, and combining with H_3 forms the true basis, which is associated in Gros's salts with hydrated acids or with chloride of hydrogen.

In this confused state of our knowledge of so interesting a class of bodies, I think it right to publish some facts with regard to their origin and constitution which I had discovered a long time ago; for after having completed my researches on the mercurial compounds formed by ammonia, I commenced the analogous examination of the platinum bodies, and had formed many new substances, among others the muriatic and oxalic salts of Gros, when his memoir appeared. I then thought that he would pursue the subject further, but as it would be useless for two persons to be losing time doing the same thing, and I thought that he was ahead of me in time, I laid the subject aside, and passed to other matters. I have lately resumed that investigation, and combining my old results with those which I since obtained, I consider that the nature of those bodies may be now explained.

I formed these salts originally, not as M. Gros did, from Magnus's salt, but by acting with ammonia on the perchloride of platina. There are produced a series of bodies, which it is not my object to describe here, but ultimately a colourless solution, which gives with alcohol a white precipitate, which is the basis of Gros's salts, that he had endeavoured in vain to isolate. Its composition is $Pt\,Cl\,N_2\,H_6\,O + 2\,Aq$. Its solution in water gives with acids all Gros's salts. Now in its formation the series of decompositions from the perchloride $Pt\,Cl_2$ is fully developed. And it is important also that by passing an excess of dry gaseous ammonia over perchloride of platina, Gros's muriate is produced, for its constitution may be represented as $Pt\,Cl_2 + 2\,N\,H_3$. It is evident, therefore, that these complex bodies are deduced from the perchloride of platina just as the complex mercurial amides are formed from corrosive sublimate, and as the ammoniacal sulphates, &c. of silver, zinc, copper, &c. are produced by an excess of ammonia on the simple salts. But the platinum is remarkable for involving together the types of both the mercurial and copper ammoniacal compounds, and hence rising to a still greater complexity of constitution. Thus in place of $Hg\,Cl + Hg\,Ad$ and $Hg\,I + 2\,Hg\,O + Hg\,Ad$, there are in the platinum series $Pt\,Cl_2 + 3\,Pt\,Ad_2 + 4\,Aq$, and $Pt\,.\,I_2 + Pt\,Ad_2 + 4\,Aq$. In combination there is, however, $Pt\,Cl_2 + Pt\,Ad_2$, and it is this which exists in Gros's salts combined with the common salt of ammonia. From these circumstances, I consider that we have good grounds for looking upon Gros's salts, the sulphate for example, as containing the bichloramide of platina united with two equivalents of sulphate of ammonia $(Pt\,Cl_2 + Pt\,Ad_2) + 2(NH_4 . O + S\,O_3)$. As to the objection made by Gros to the existence of common sulphate of ammonia, that neither the acid nor the base can be directly detected, it is well remarked by Berzelius that the view taken by Gros does not in the slightest degree render it easier of explanation. We find, however, many analogous cases. Thus in the double oxalate of chrome and potash, a very trifling quantity of the oxalic acid is shown on the addition of a salt of lime.

I consider that it is not calculated to advance science, when, in order to avoid difficulties in the rational explanation of the constitution and properties of any substance, the theory of organic radicals is employed as unreservedly as it appears now to be by many chemists. It gives an air of simplicity to reactions, very attractive at first sight, and very convenient for explanation, but which, by making us satisfied by surface resemblance or plausibility, may retard important investigations. If, in accordance with Gros's ideas, we make $Pt\,Cl\,N_2\,H_6 = X$, the question still remains, what is the structure of X? And that question should never be lost sight of. But the substance described by Reiset as the radical of Gros's salts*, and which has indeed the formula of it $(Pt\,Cl\,N_2\,H_6)$, is not so constituted; it is not at all of the series of the bichloride of platina, to which Gros's salts belong, but to the protochloride, being $Pt\,Cl + 2\,NH_3$, whilst Magnus's salt is $Pt\,Cl + NH_3$. This is evident from the way it is made.

L. *Some Remarks on* Messrs. Francis and Croft's *Abstracts from the Foreign Journals. By* A CORRESPONDENT.

SIR,

IT has lately been the fashion, especially among those who have spent some time under the great teachers on the continent, to declaim much against the backward state of chemistry in England, and to censure its cultivators in this country for their tardy reception of the new doctrines and new facts continually emanating from the fertile inventions and laborious investigations of their continental brethren.

Perhaps, to a certain extent, the charge may be well founded, and thanks are due to Messrs. Francis and Croft for their laudable endeavours to remove this opprobrium.

On reading their abstracts from the foreign journals inserted

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* Francis and Croft's Abstracts, p. 284.

in your last Number, I could not, however, avoid being struck with some inconsistencies in the reasonings adduced; and should these be a sample of what we are generally to meet with, we may account in good measure for the reluctance manifested by our countrymen to adopt every new theory that may be put forth, even under the sanction of men whose reputation stands deservedly so high. Theories now succeed each other with such surprising rapidity, that scarcely has one been blazoned to the world ere another is proposed to supersede it, and this experiences, it may be, a dominion of still shorter duration.

Take, for example, the theory of the constitution of alcohol, which MM. Dumas and Stas, in their last paper, propose to modify: let us consider the views that have been taken, or may be taken of its molecular arrangement, and then let us decide, if we can, which view is the true one, or even which may be said to merit the preference.

Alcohol. Empirical formula $C_4 H_6 O_2$
(1.) Bihydrate of Olefiant Gas $(C_4 H_3 + H) + 2 H O$.
(2.) Bihydrate of Etherine, $(C_4 H_4) + 2 H O$.
(3.) Hydrate of oxide of Ethyl, $(C_4 H_5 + O) + H O$.
(4.) Methylic Aldehyd + Marsh Gas, $(C_2 H_2 O_2) + C_2 H_4$.
(5.) Bihydruret of Aldehyd, &c. &c., $(C_4 H_4 O_2) + H_2$.

Each of these views may be said to be supported by experiment. (1.) Thus, when alcohol and sulphuric acid are mixed in certain proportions, and heated, olefiant gas and water are the results. (2.) In other proportions, the same ingredients yield etherine, or etherole, and water; (3.) whilst, in again differing proportions, oxide of ethyl and water are the results.

Thus much the effects of *acid* re-agency on alcohol: MM. Dumas and Stas have tried the effect of *alkalies* in producing its decomposition. The results, as might be expected, are equally various.

Heated with *hydrated* alkalies, acetate of the base is formed and hydrogen evolved. If the temperature be somewhat raised, carbonic acid and marsh gas result; here it is the *hydrated acid* that undergoes this decomposition, $C_4 H_4 O_4 = C_2 O_4 + C_2 H_4$.

In this instance the formula of acetic acid is represented to us as $C_4 H_4 O_4$; a little further on, when mentioning the results of the transmission of aldehyd, $C_4 H_4 O_2$ over heated lime and potassa, acetic acid is said to be formed by the action, and the formula given is $(C_4 H_3 O_3)$, the true formula for the anhydrous acid. Surely we must not admit and discard the presence of water in these cases just when it suits our convenience to do so, for by this mode of proceeding, alcohol and ether would become synonymous and convertible terms,

$C_4 H_6 O_2 = C_4 H_5 O$. In the experiment just described H is given off, and it is concluded that aldehyd is analogous to hydruret of benzule, while a few months ago the opinion of its being a hydrate of oxide of acetyl was promulgated; and both opinions are founded on experiment! but this by the way.

(4.) To return to the theory of alcohol. After stating, as above, the decompositions produced by hydrated alkalies, the abstract (for with that alone I am at present dealing) proceeds —"In the usual preparation of acetic acid, two atoms of hydrogen are replaced by two atoms of oxygen; by the action of alkalies chloracetic acid $C_4 H Cl_3 O_4$ [the hydrated acid] is decomposed into carbonic acid and chloroform $C_2 O_4 + C_2 H Cl_3$; acetic acid is decomposed into $C_2 O_4 + C_2 H_4$, or $C_2 O_4 + C_2 H_3 H$. Alcohol may therefore [!] be considered as composed of two bodies representing carbonic acid, one the methylic aldehyd, the other marsh gas, $C_4 H_6 O_2 = C_2 H_3$ [intended, I suppose, to be H_4] $+ C_2 H_2 O_2 =$ [or analogous to] $C_2 O_4 + C_2 O_4$." Why we are to consider alcohol as composed of marsh gas and methylic aldehyd, to me is by no means obvious from these premises. If, however, we are to imagine it as containing a compound of the methyl series, we might, as it seems to me, with as much reason suppose alcohol an oxide of methyl ($C_2 H_3 + O$) with which it is evidently isomeric. But I forget myself; this supposition is contrary to the theory of types.

The abstract continues: "In the usual method of forming acetic acid $C_2 H_2 O_2$ loses H_2 and takes up O_2, there then remains $C_2 O_2 O_2 + C_2 H_4 = C_4 H_4 O_4$, or acetic acid. By the action of alkalies H_4 is given off; the water of the hydrated alkali is decomposed, H_2 is developed, and O_2 assimilated." If these changes really occurred, it is evident the result would be an oxalate, not an acetate.

$$\left.\begin{array}{lr} \text{1 eq. hydrated acetic acid} = & C_4 + H_4 + O_4 \\ \text{2 eq. water from the alkali} = & H_2 + O_2 \\ \hline & C_4 + H_6 + O_6 \end{array}\right\} =$$

$$\begin{array}{llll} C_4 + & & O_6 & \text{2 eq. oxalic acid.} \\ & H_4 & & \text{Evolved.} \\ & H_2 & & \text{Evolved.} \\ \hline C_4 + & H_6 + & O_6 & \end{array}$$

Hence it appears that the alkalies act, by their affinity for carbonic acid, in a manner analogous to that in which sulphuric acid operates by its affinity for water.

(5.) Lastly, we may consider alcohol a bihydruret of aldehyd, and still appeal to experiment for confirmation of our view;

for if alcohol be passed through heated porcelain tubes, the products are water, aldehyd and carburetted hydrogen*.

$$4 \text{ eqs. alcohol} = C_{16}\,H_{24}\,O_8 = \left\{\begin{array}{llll} \text{2 eqs. aldehyd...} & C_8 & H_8 & O_4 \\ \text{4 eqs. water......} & & H_4 & O_4 \\ \text{1 eq. olefiant gas} & C_4 & H_4 & \\ \text{2 eqs. marsh gas} & C_4 & H_8 & \\ \hline & C_{16} & H_{24} & O_8 \end{array}\right.$$

The hydrogen set free from 2 eqs. of alcohol decomposing the other two, as here represented. These are but a few of the numerous views that might be taken of the constitution of alcohol. Can we then wonder if considerable deliberation should be exercised before closing with any particular theory? I do not pretend that some of these views are not more strongly supported by analogies than others; but into these details it would be out of place for me on the present occasion to enter.

A new acid, the *Ethalic*, is described in this paper, the formula of its hydrate is $C_{32}\,H_{32}\,O_4$; this, it will be remarked, is identical with the formula given by Fremy and Stenhouse for hydrated palmitic acid.

Since MM. Francis and Croft have undertaken these abstracts for the benefit of British chemists, I would beg to suggest rather more attention to the terminology and orthography of new compounds, a point by no means unimportant, especially when, as so very frequently happens in the present day, fanciful and arbitrary names, some nearly identical with those applied to totally distinct substances†, are imposed. It is desirable that in the same paper at least the same things should possess the same names; that, for example, a body should not sometimes be called valeric, sometimes valerianic acid; nor methyl, mæthyl; ethyl, æthyl, or the like.

It would certainly, moreover, be a convenience, if in these abstracts, which we sincerely hope may be continued, the temperature, instead of being given on the centigrade scale only, were expressed likewise in corresponding degrees of Fahrenheit, as being the scale generally used here.

With regard to the symbols mentioned in the foot note at p. 200, in which it is queried whether the plan of representing the constitution of organic bodies merely by the *position* of the figures, as proposed by Berzelius, be known in England, surely on the present occasion we may rejoice in our ignorance. Our symbols are already sufficiently abridged to di-

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* Turner's Elements of Chemistry, Gregory's edition, p. 859.

† For example, ammelin, ammelid; phlorizine, phlorizeine; benzile, benzole, benzule; and the chlorovalerisic and chlorovalerosic acids of the authors.

minish their usefulness. We do not require abbreviations of abbreviations. Mistakes arising from this cause are already too numerous. How liable they would be to multiply were we still further to complicate our systems of notation, may be imagined by reading the following sentence from the abstract already so often quoted; where, speaking still of the action of hydrated alkalies on various substances, it is said, "Acetic æther gives hydrogen and acetic acid; benzoic æther gives hydrogen, benzoic and acetic acids; iodide of ethyl gives iodide of potassium and olefiant gas, $C_4\,H_5\,I_2$ [should be I] $+\dot{K}=C_4\,H_4+\dot{K}I^2$ [should be I, and the dot over the K omitted] $+$ HO. This æther forms with chlorine; chloride of ethyl and iodine is separated; $\frac{4}{10}$ E F is exactly similar to RF." Here neither R nor F are defined.

I am, Sir, your obedient servant,
To R. Phillips, Esq., F.R.S. &c. &c. W. A. M.
March 5, 1841.

NOTE.

We do not find in the communication of our Berlin Correspondents any thing implying unwillingness on the part of English chemists to receive "new doctrines and new facts" from abroad. Messrs. Francis and Croft merely offer to assist us in supplying information which the chemists of our own country are, we believe, anxious to obtain. Whatever caution may be necessary in the reception of new *doctrines*, correct information respecting them must be desirable; and still more so with regard to new *facts*. Neither, if there are "inconsistencies in the reasonings" of Dumas and Stas, is it logical to infer "that these are a sample of what we are generally to meet with" among continental chemists; some of whom, indeed, are able opponents of those reasonings. It is our province to assist in bringing them under examination.

With regard to the remarks on the Symbolic Notation, we have to state that our Correspondents are not accountable for any ambiguity which may appear in the formulæ given in that portion of their communication which appeared last month. Some of those formulæ were altered by us, with the intention to suit the views generally taken by English chemists. In the continuation given in the present number, however, we have preferred leaving the symbols exactly as written by Messrs. Francis and Croft; and to this practice we intend in future to adhere. As to the terminology, we presume that in preparing the abstracts, they have retained the terms employed in the original papers, except in cases where the English equivalents were obvious. For want of uniformity in orthography we are alone to blame. Much diversity exists among our chemical writers; but our practice has been to prefer, as usual in English, the Latin orthography of Greek terms, and not to present them in a French garb. The advantage of forming terms from the Greek is lost by so disfiguring them as that they are scarcely to be recognised. We therefore prefer æther, æthyl, &c., more especially as we have not, like the French, the accented é to substitute for the diphthong; nor the ä as in German. We are fully aware that with the present immense influx of new names, the subject of chemical nomenclature requires strict critical supervision, and we trust it may engage the further attention of Professor Whewell, who has so ably treated certain parts of the subject in his Philosophy of the Inductive Sciences.—EDIT.

LI. *On the Cause of the Production of Daguerreotype Pictures.* *By* MARTYN J. ROBERTS, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ALTHOUGH the Daguerreotype process has long excited intense interest in the scientific world, we have not as yet had an explanation of the wonderful effects produced. I believe that even M. Arago has failed to elucidate the theory; and not only has this great philosopher been foiled, but the inventor of the process is unable to solve the problem; with these facts before me, I feel it almost presumptuous in so humble a votary of science as myself to attempt an explanation of the cause of the production of Daguerreotype pictures, but if my attempt has the effect of directing attention to the right path for arriving at a solution of the question, I shall be satisfied. We all know that light has a powerful influence on crystallization; solutions that will not crystallize in the dark instantly form on the admission of light; the crop of crystals is always more copious on the enlightened side of a glass containing a crystallizing liquid than on the dark side. Ice forms more rapidly during moonlight, and on the break of day, than on a dark night; but I need not adduce examples of the influence of light on crystallization, for the fact is allowed by all scientific men.

Let us then suppose, that in the Daguerreotype process the cleansed silver plate is exposed in the dark to the vapour of iodine; this deposits itself in a flocculent or powdery state on the plate, unable to form the peculiarly shaped iodic crystals, from the absence of light; but yet all other requisites being present, it may be considered in an incipient state of crystallization, or balanced so finely, that the admission of the excitant light instantly throws it into plate-formed iodic crystals, but only in those parts where the light has impinged, and here its perfection of, or continuity of crystallization, is merely in proportion to the intensity of light.

Having now the iodated plate removed from the camera obscura, where it has undergone a surface crystallization, more or less perfect in those parts where the lights and shades have fallen, we submit it to the mercurial vapour; the atoms, vesicles, or globules of this vapour being very minute, attach themselves to all the minute inequalities of face in the iodine; on those parts which are fully crystallized, the vapour is precipitated on the flat tabular surface of the crystals, and here offering a continuous and equal angle of reflection to the eye it appears white and resplendent. On the non-crystallized or imperfectly crystallized surface of the iodine, which being in a measure powdery and offering no determinate angle of reflection to the eye, the mercurial vapour adheres, but in no flat surface or continuous determinate angle capable of reflecting a mass of light; it may be said it is here unpolished.

Again, may not the angle under which it is necessary to view a Daguerreotype picture be that of the facet of the iodic crystal, and this be a further confirmation of my theory? The mercurial vapour covers the whole of the iodine, and thus protects it from the further action of light.

Such are the crude views I have formed on this subject, and I trust they may lead to a further elucidation.

I am, Gentlemen, yours, &c.

London, March 19, 1841. MARTYN J. ROBERTS.

LII. *Notices respecting New Books.*

A System of Crystallography. By JOHN JOSEPH GRIFFIN. Glasg. 1841.

WE have been favoured with a copy of a work bearing the above title, but which might better have been called "A description of 120 porcelain models of crystals, according to a method newly invented by the author"; for, with regard to Crystallography, we might, on first turning over the uncut leaves, and meeting with zenith and nadir of crystals, and north-east, north-west, and other meridians, have supposed that the object of the book was to create a laugh at the expense of the cultivators of that science; but recollecting the cost of publishing a rather bulky octavo volume, we felt compelled to suppose the author serious, and we accordingly began at the preface to separate some of the leaves.

The first line of the treatise defines crystallography to be "the art of describing crystals." But this, according to our notions on the subject, is only one, and not the most important of its objects. We have been accustomed to regard crystallography as a science of a much higher order, by which the mineralogist is enabled, from a fragment of a crystal, to discover its relation to some simple type by which he may connect it with the species of mineral to which it belongs.

This view of the subject, however, does not appear from the above definition to have entered into Mr. Griffin's contemplation, and his system therefore, even if it possessed the merit he ascribes to it, would render no additional service to mineralogy.

We cannot afford either time or space for even a brief analysis of the author's method, nor do we conceive that we should much benefit our readers by giving it; we shall therefore limit ourselves to a few extracts and remarks.

The preface begins thus: "There are many systems of crystallography in print, but none in general use," a fate from which we may confidently predict the work before us will not be exempt. "The different systems," it proceeds, "hitherto published have failed to satisfy the wants of the *public.*" We do not know to what *public* Mr. Griffin refers. Surely not to those by whom the tens of thousands of copies of 'Nicholas Nickelby' and 'Jack Sheppard' are eagerly purchased, or the novels of fashionable life as eagerly perused; and if not to these, what reading public is to be found, and more especially where the cultivators of science, whose *wants* the existing works on Crystallography have failed to satisfy?

The different systems to which our author alludes are, he continues, "either too difficult to learn, or, when learnt, too troublesome for service. Hence," he says, "crystallography is little studied by chemists or mineralogists; and the consequence is, that for want of *a language in which observations can be recorded,* the study of crystallization is also shunned." Our mineralogical readers must know that this statement is unfounded; and we can only account for its having dropped from the pen of Mr. Griffin, by supposing the preface to have been written after the work was finished, and the author to have entirely forgotten not only a work to which he has so frequently referred, but even the name of William Phillips.

The preface proceeds to state that "the present publication is an attempt to show the way, not merely how to describe a crystal, but how to do it *easily.*"

The following are examples of the *simple* and *easy* method of description employed by the author:

54. TURNERITE. Pictite. Cleavage=m, T.

5. 5. $t,\ m\frac{9}{20}t,\ M\frac{9}{10}T,\ m\frac{9}{5}t.\ \frac{1}{2}P\frac{1}{4}M\ Z^2n,\ \frac{1}{2}P-M\ Zn^2,\ \frac{1}{2}P-M\ Zs,$
$\frac{1}{2}P-M\frac{9}{10}T\ Zn^2e\ Zn^2w,\ 5\,(\frac{1}{2}p-m_xt)\ Zne^2\ Znw^2,$
$2P_xM\frac{9}{10}T\ Zs^2e\ Zs^2w,\ \frac{1}{2}p+m\frac{9}{5}t\ Zse^2\ Zsw^2,$ .... $P84^2$.

5. 5. $T,\ M\frac{9}{20}T,\ M\frac{9}{10}T,\ m\frac{9}{5}t.\ \frac{1}{2}P\frac{1}{4}M\ Z^2n,\ \frac{1}{2}p-m\ Zs,$
$3p-m_xt\ Zne\ Znw,\ p-m_xt\ Zse\ Zsw,$ .... $Ly82^2$.

Part II. p. 91.

ANORTHITE.

5. 3. $T,\ \frac{1}{2}M\frac{4}{8}T\ nw,\ \frac{1}{2}M\frac{5}{8}Tne.\ \frac{1}{4}p_xm_yt_zZnw^2,\ \frac{1}{4}P_xM_yT_z\ Z^2ne,$
$\frac{1}{4}p_xm_yt_z\ Zn^2e,\ \frac{1}{4}p_xm_yt_zZne^2,\frac{1}{4}p_xm_yt_z\ Zs^2e^2,\frac{1}{4}p_xm_yt_z\ Zs^2e,$
$\frac{1}{4}p_xm_yt_z\ Zs^2w^2,$ .... $R3^{30,31}$.

5. 3. $T,\ \frac{1}{2}M\frac{4}{8}T\ nw,\ \frac{1}{2}M\frac{5}{8}T\ ne.\ \frac{1}{4}p_xm_yt_z\ Znw^2,\ \frac{1}{4}P_xM_yT_z\ Z^2ne,$
$3\,(\frac{1}{4}p_xm_yt_z\ Zne),\ 4\,(\frac{1}{4}p_xm_yt_z\ Zse),\ 2\,(\frac{1}{4}p_xm_yt_z\ Zsw),$.... $R3^{3233}$.

5. 5. $T,\ \frac{1}{2}M\frac{4}{8}T\ nw,\ \frac{1}{2}M\frac{5}{8}T\ ne.\ \frac{1}{4}p_xm_yt_z\ Znw^2,\ \frac{1}{4}P_xM_yT^z\ Z^2ne,$
$\frac{1}{4}p_xm_yt_z\ Zn^2e,\frac{1}{4}p_xm_yt_zZne^2,$..................... $R3^{28,29}$.

5. 5. $T,\ \frac{1}{2}M\frac{4}{8}T\ n^2w,\ \frac{1}{2}m+t\ nw^2,\ \frac{1}{2}\quad\frac{5}{8}T\ n^2e,\ \frac{1}{2}m+t\ ne^2,$
$\frac{1}{4}p_xm_yt_z\ Znw^2,\ \frac{1}{4}P_xM_yT_z\ Z^2ne,\ \frac{1}{4}p_xm_yt_z\ Zne^2,$
$4(\frac{1}{4}p_xm_yt_z\ Zse),\ 3\,(\frac{1}{4}p_xm_yt_z\ Zsw),$ .... $R3^{34}$.

Part II. p. 93.

Here Z stands for zenith.
N ,, ,, nadir.
e, w, n, s, east, west, north, south.
P M T denote faces, p, m, t, axes.

And in p. 2 of the first part we find that the subscript indices, x, y, z, imply *unknown* or *variable* axes.

The indices representing *powers* of the quarters of the compass are thus *explained* in p. 24 of Part I.:

"In *Phillips's Mineralogy*, p. 174, there is a figure of a crystal of Fluorspar, which has 32 vertical planes, the arrangement of which is as follows, taking the nw quadrant as an example:

$$\begin{array}{ccccccccc} \mathrm{n} & \mathrm{n^4w} & \mathrm{n^3w} & \mathrm{n^2w} & \mathrm{nw} & \mathrm{nw^2} & \mathrm{nw^3} & \mathrm{nw^4} & \mathrm{w} \\ \mathrm{M}, & \mathrm{MT}\overset{+}{++}, & \mathrm{MT}\overset{+}{+}, & \mathrm{MT}+, & \mathrm{MT}, & \mathrm{M}+\mathrm{T}, & \mathrm{M}\overset{+}{+}\mathrm{T}, & \mathrm{M}\overset{+}{++}\mathrm{T}, & \mathrm{T}. \end{array}$$

"The signs $+$, $\overset{+}{+}$, $\overset{+}{++}$, in all these symbols, indicate the great, greater, and greatest *distance* of the poles of the axes referred to, while the figures 2, 3, 4 indicate the great, greater, and greatest *proximity* of the poles to whose symbol they are added."

It would have been more satisfactory to us to have found occasion to commend this performance, instead of regretting as we do that so much time and labour should have been employed upon a system so little inviting in its notation, and affording so small a chance of finding readers. ***

Elements of Chemistry, including the most recent discoveries and applications of the Science to Medicine and Pharmacy, and to the Arts. By ROBERT KANE, M.D., M.R.I.A., Professor of Natural Philosophy to the Royal Dublin Society, and of Chemistry to the Apothecaries' Hall of Ireland, &c. Part I. Dublin, 1840, 8vo, pp. 356, with 120 wood-cuts.

The following is an analysis of the first part of this work.

Introduction.—Objects, Utility, and Origin of Chemistry.

Chapter I. OF GRAVITY and COHESIVE FORCES AS CHARACTERIZING CHEMICAL SUBSTANCES.—Specific gravity of Liquids, Gases and Solids. Constitution of Matter. Infinite Divisibility. Ultimate Particles of Matter. Molecular Constitution. States of Aggregation. Limits of Cohesion. Capillarity. Elasticity of Gases. Correction for Pressure. Liquefaction of Gases. Solubility. Phænomena of Solution. Formation of Crystals by Fusion and Solution. Dimorphism. Crystallization. Primary and Secondary Forms of Crystals. Systems of Crystallization. Regular System. Hemihedral Forms. Rhombohedral System. Square, Right, and Oblique Systems. Doubly Oblique System. Pseudomorphism. Forms modified by Foreign Bodies. Isomorphism. Goniometers.

Chapter II. OF THE PROPERTIES OF LIGHT AS CHARACTERIZING CHEMICAL SUBSTANCES.—Simple Refraction. Double Refraction. Crystalline Systems. Decomposition of Light. Prismatic Colours. Analysis by Absorption. Natural Colours of Bodies. Polarization of Light. Action of Crystallized Bodies on Polarized Light. Crystalline Systems known by Polarized Light. Macled Crystals. Circular Polarization. Rotative Power of Liquids. Plagihedral Crystals. Wave Theory of Light. Interference of Light. Phosphorescence. Chemical Agencies of Light. Chemical Rays in the Spectrum.

Chapter III.—OF HEAT CONSIDERED AS CHARACTERIZING CHEMICAL SUBSTANCES.

Sect. I. *Of Expansion.*—Repulsive power of heat. Influence of cohesion on expansion. Of the measure of heat. Nature of temperature. Construction and use of air-thermometers. Mercurial thermometer, its principle of correction. Determination of the standard interval. Construction of the various thermometric scales. Estimation of temperatures above the boiling point of mercury. Daniell's pyrometer. Nobili's thermo-multiplier. Table of temperatures. Expansion of air. Results obtained by Dalton, Gay-Lussac, and Dulong and Petit. Rudberg's correction of them. Correction of volumes for change of temperature. Determination of the expansion of liquids. Expansibility of liquids changes with the temperature. Determination of the expansion of solids. Expansibility of solids increases with the temperature: exceptions to this rule. Effects of the expansion of compound metallic bars. Metallic thermometer of Breguet. Compound pendulum.

Sect. II. *Of Specific Heat.*—Method of mixtures. Process of Dulong and Petit. Researches of Lavoisier and Laplace. Calorimeter. Relation of specific heat and chemical constitution. Specific heats of atoms. Heat by chemical combination. Determination of the specific heats of gases.

Sect. III. *Of Liquefaction.*—Latent heat. Absorption of heat during liquefaction. Heat evolved in solidifying. Cold by liquefaction. Artificial cold produced by frigorific mixtures. Nature of special heat.

Sect. IV. *Of Vaporization.*—Latent heat of vapours. Vaporization accompanied by great increase of volume. Determination of the elasticities of vapours. Relations of vapours to temperature and pressure. Nature of the boiling point. Alterations of the boiling point with the superincumbent pressure, and the nature of the vessel. Apparent anomalous property of liquids. Sum of latent and sensible heat constant. Artificial cold produced by the evaporation of liquids. Spontaneous evaporation. Gases and vapours offer no resistance to each other's elasticity. Correction for moisture. Hygrometers. Moist bulb-hygrometer. Daniell's hygrometer. Analogy of solution to vaporization. Steam as a moving power. Similarity of constitution of gases and vapours. Speculation on the boiling points of the condensed gases.

Sect. V. *Of the Transmission of Heat through Bodies.*—Conduction of heat. Relative conducting power of solids and liquids. Communication of heat by gases. Communication of heat by diffusion and radiation. Properties of radiant heat. Of the radiating, absorbing, and reflecting powers of bodies. Researches of Melloni and Forbes on radiant heat. Reflecting power of bodies as determined by Buff. Permeability of bodies to heat from sources of different temperatures. Analogy of heat to coloured light. Polarization of heat. Relations between the physical constitutions of heat and light. Change of refrangibility of heat.

Sect. VI. *Of the Cooling of Bodies.*—Equilibrium of temperature. Theory of dew and frost. Sources of terrestrial heat. Central heat of the Earth.

Chapter IV.—OF ELECTRICITY CONSIDERED AS CHARACTERIZING CHEMICAL SUBSTANCES. Nature of electricity. Statical and dynamical electricity.

Sect. I. *Of Statical Electricity.*—Electricity produced by friction. Conductors and insulators. Relative conducting powers. Velocity of motion of electricity. Distribution of electricity. Opposite conditions of excitation. Gold-leaf electroscope. Electrical attractions and repulsions. Neutralization of opposite electricities. Law of electrical attractions. Theories of electricity. Theory of two fluids. Theory of one fluid.

Electrical machines. Theory of the electrical machine. Quadrant electrometer. Various modes of excitation. Excitation by induction. Theory of the prime conductor. Theory of the gold-leaf electroscope. Doublers and condensers. Construction and theory of the Leyden jar. Electrophorus of Volta. Induction an action of contiguous particles. Specific inductive capacity. Lateral inductive action. Difference between conducting and non-conducting bodies. Relation of induction to conduction. Other sources of statical excitation. Atmospheric electricity.

Sect. II. *Of Dynamical Electricity.*—Magnetic electricity, thermo-electricity, chemical electricity, animal electricity. Galvanic electricity. Conditions of the generation of a galvanic current. Simple galvanic circles. Connexion of galvanic and chemical action. The direction of the current determined by the chemical action. Principle of electrotype copying. Compound voltaic circles. Theory of the voltaic battery. Relation of intensity and quantity. Statical action of the battery. Volta's theory of contact. Chemical theory of the battery. Various forms of galvanic batteries. Interfering action of common zinc. Amalgamation of zinc plates. Forms of constant batteries by Mullins, Daniell and Grove. Relative conducting powers of bodies for voltaic electricity. First discovery of galvanism. Various kinds of galvanoscopes. *Thermo-electricity.* Thermo-electric currents. Production of cold by electricity. Construction of the thermo-multiplier of Nobili. *Magnetic electricity.* Magnetism a form of electricity. Magnetic properties of iron and steel. Magnetic attraction and repulsion. Intimate structure of magnets. Magnetic properties of a galvanic current. Attraction and repulsion of currents. *Electro-magnetism.* Action of a galvanic current on a magnet. Astatic combinations of magnets. Construction of the galvanometer. Magneto-electric induction.

Chapter V.—OF CHEMICAL NOMENCLATURE.—Table of simple bodies and their symbols. General principles of the Lavoisierian nomenclature. Construction of the names of simple bodies and primary compounds. Construction of the names of various classes of primary compounds. Names of secondary compounds. Ternary and quaternary compounds. Symbolical nomenclature.

Chapter VI.—OF CHEMICAL AFFINITY, AND ITS RELATIONS TO HEAT, TO LIGHT, AND TO COHESION.—Nature of chemical affinity. Principles of elective decomposition as characteristic of chemical affinity. Order of elective decomposition. Of double decomposition. Of quiescent and divellent forces. Order of affinity not constant. The power of affinity influenced by external modifying causes. Characteristic distinctions between affinity and cohesion. Diversity of chemical properties equally a cause and an effect of chemical action. Influence of external physical agents upon chemical affinity. 1. *Influence of cohesion.* Influence of cohesion on the order of chemical decomposition. Mode of arrangement of acids and bases which co-exist in solution. Distribution not invariable. 2. *Influence of elasticity.* Change in the order of decomposition produced by cohesion and elasticity. Change in the order of decomposition produced by various modifying causes. Relation of affinity to neutralizing power. *Influence of light on chemical affinity.* Photographic drawing. Colouring effects of the chemical rays. Interference of the chemical rays. Theory of the formation of Daguerreotype images.

Chapter VII.—OF THE LIGHT AND HEAT DISENGAGED DURING CHEMICAL COMBINATION.—Nature of combustion. Products of slow combustion. Aphlogistic lamp. Action of spongy platina on gaseous mixtures. Construction of the platina gas-lamp. Constitution of flame. Blowpipe. Heating effects of flame. Cooling effects of apertures. Construction of the safety-lamp. Determination of the quantity of heat evolved in perfect combustion. Experiments of Despretz and Hess. Theories of combustion proposed by Lavoisier and others. Heat of combination derived from the change of specific heat in combustion.

Chapter VIII.—OF THE INFLUENCE OF ELECTRICITY ON CHEMICAL AFFINITY.—Electro-chemical decomposition. Chemical affinity is electrical attraction. Electro-chemical classification. Electro-chemical theories proposed by Davy, Ampère, and Berzelius. Elements evolved not on attracting poles, but on the limiting surfaces of the liquid. Internal molecular mechanism of electro-chemical decomposition. Phænomena of transfer described. Of electrolysis and electrolytes. Electro-chemical equivalents. The chemical voltameter. Molecular arrangement of the elements of the generating cell. Theory of galvanism. Origin of the galvanic current. Compound bodies formed by the current. Agency of weak currents in the production of compound bodies. Synthetic action of electricity. An electro-chemical theory proposed by the author. Theory of the relations of electricity and affinity. Electro-chemical theory proposed by Becquerel. Of the measure of affinity.

Chapter IX.—ON THE LAWS OF COMBINATION.—Numerical determination of chemical equivalents. Scales of chemical equivalents. Table of the equivalents of simple bodies. Double decomposition occurs in equivalent proportions. Equivalents of compound bodies. Law of multiple proportions. Researches of Pronot on the definiteness of composition. Methods of determining the equivalent constitution of bodies. Law of multiple proportions. Theory of volumes. Equivalent volumes of simple and of compound bodies. Calculations of the theoretical specific gravities of compound vapours.

Chapter X.—OF THE RELATIONS OF CHEMICAL CONSTITUTION TO THE MOLECULAR STRUCTURE OF BODIES.

Sect. I. *Of the Atomic Theory.*—Dalton's theory of combination. Physical and chemical atoms.

LIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 141.]

Dec. 10, 1840. A MEMORANDUM, addressed to the Royal Society, November 28th, 1840, by Martin Barry, M.D., F.R.S., L. & Ed., was read.

Dr. Barry, in reference to the memorandum of Mr. Wharton Jones, claiming for himself the contemporaneous discovery of the germinal spot in the mammiferous ovum, states that, after having bestowed considerable pains to ascertain who was the original observer of a structure which has proved to be of great importance, he had mentioned incidentally in his paper the result of his inquiry, namely, that the merit of the discovery was due to Professor Rudolph Wagner; but observes that the inquiry may be resumed by all who will take the trouble to examine the works, both in German and English, on this subject; and that he will ever be open to conviction, and ready to declare his change of opinion, on the production of sufficient evidence.

A communication was also read, entitled "Supplementary Note to a Paper, entitled 'Researches in Embryology. Third Series;

a Contribution to the Physiology of Cells.'" By Martin Barry, M.D., F.R.S. L. & Ed.*.

In the paper referred to, the author had shown, that after the ovum of the Rabbit has entered the Fallopian tube, cells are found collected around its thick transparent membrane or "zona pellucida"; which cells, by coalescing, form a thinner membrane—the incipient chorion. He now adds, that the formation of this thinner membrane does not exhaust the whole layer of these cells; but that a stratum of them is found remaining on, and entirely surrounding the "zona," after the thinner membrane has risen from it. The fluid space also, between the "zona" and the thinner membrane, presents a large number of cells or discoid objects, each of which contains a brilliantly pellucid and highly refracting globule. In some parts, several of these discs, closely joined together, have the appearance of shreds of membrane; in others, there are found pellucid globules, some of which are exceedingly minute. The discs now mentioned collect at the periphery, for the thickening of the chorion. They seem to proceed from the region of the "zona;" and probably have their origin in the cells by which the latter is surrounded. If so, the author thinks we cannot suppose them to arise in any other way than that which, according to his observations, appears to be the universal mode of reproduction; namely, by division of the nuclei of the parent cells. Nor can we suppose that minuteness is any hinderance to their subsequent increase by the same means.

December 17, 1840.—1. "Present state of the Diamond Mines of Golconda." By T. J. Newbold, Esq., of the Madras Army, A.D.C. to Major-General Wilson, K.B. Communicated by S. H. Christie, Esq., M.A., Sec. R.S.

The author gives an account of the tract of country in which the diamond mines of Golconda are situated, and of the processes by which the diamonds are obtained. The latter consist merely in digging out the rolled pebbles and gravel, and carrying them to small square reservoirs, raised on mounds, having their bottom paved with stones, and then carefully washing them. Dry weather is selected to carry on these operations, in order to avoid the inconvenience and expense of draining. A description is then given of the mines of Banaganpully, Munimudgoo, Condapilly, Sumbhulpoor, and Poonah in Bundlekund.

2. "Magnetic-term Observations made at Milan." By Professor Carlini, Director of the Observatory at that place: also "Magnetic-term Observations made at Prague." By Professor Kreil, Director of the Observatory at that place.

3. "On the Production of Heat by Voltaic Electricity." By J. P. Joule, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

The inquiries of the author are directed to the investigation of the cause of the different degrees of facility with which various kinds of metal, of different sizes, are heated by the passage of voltaic electricity. The apparatus he employed for this purpose consisted of a

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* [An abstract of Dr. Barry's Third Series of Researches in Embryology appeared in L. and E. Phil. Mag., vol. xvi. p. 526; vol. xvii. p. 385.—Edit.]

coil of the wire, which was to be subjected to trial, placed in a jar of water, of which the change of temperature was measured by a very sensible thermometer immersed in it; and a galvanometer, to indicate the quantity of electricity sent through the wire, which was estimated by the quantity of water decomposed by that electricity. The conclusion he draws from the results of his experiments is, that the calorific effects of equal quantities of transmitted electricity are proportional to the resistances opposed to its passage, whatever may be the length, thickness, shape, or kind of metal which closes the circuit: and also that, *cæteris paribus*, these effects are in the duplicate ratio of the quantities of transmitted electricity; and consequently also in the duplicate ratio of the velocity of transmission. He also infers from his researches, that the heat produced by the combustion of zinc in oxygen is likewise the consequence of resistance to electric conduction.

The Society then adjourned over the Christmas recess, to meet again on the 7th of January next.

January 7, 1841.—The following communication was read, viz.—

"Variation of the Magnetic Declination, Horizontal Intensity, and Inclination observed at Milan on the 23rd and 24th December 1840." Communicated by Professor Carlini, Director of the Milan Observatory.

A paper was also read, entitled, "On the Chorda dorsalis." By Martin Barry, M.D., F.R.SS. L. & E.

The author of this communication, after pointing out the similarity in appearance between an object noticed by him in the mammiferous ovum, and the incipient chorda dorsalis described by preceding observers in the ova of other Vertebrata, mentions some essential differences between his own observations and those of others as to the nature and mode of origin of these objects, and their relation to surrounding parts. Von Baer, the discoverer of the chorda dorsalis, describes this structure as "the axis around which the first parts of the fœtus form." Reichert supposes it to be that embryonic structure which serves as "a support and stay" for parts developed in two halves. The author's observations induce him to believe that, instead of being "the axis *around which* the first parts of the fœtus form," the incipient chorda is the last-formed row of cells, which have pushed previously-formed cells farther out, and that, instead of being merely "a support and stay" for parts developed in two halves, the incipient chorda occupies the centre out of which the "two halves" originally proceeded as a single structure, and is itself in the course of being enlarged by the continued origin of fresh substance in its most internal part.

The author enters into a minute comparison of the objects in question; from which it appears that the incipient chorda is not, as Baer supposed, developed into a globular form at the fore end, but that the linear part is a process from the globular; and that the pellucid cavity contained within the latter—a part of prime importance, being the main centre for the origin of new substance—is not mentioned by Von Baer. Farther, that the origin of the "laminæ dorsales" of this naturalist (the "central nervous system" of Reichert) is not simultaneous with, but anterior to, that of the chorda.

The author then reviews the observations of Rathke and Reichert on the chorda dorsalis, which contain internal evidence, he thinks, of a process in the development of Fishes, Reptiles, and Birds, the same as that which he has observed in Mammalia; namely, the origin of the embryo out of the nucleus of a cell.

And it is his opinion that this observation may assist to solve a question on which physiologists are not agreed; for it shows, that if the nucleus of a cell is a single object, the first rudiments of the embryo are not two halves. The author thinks that unless the very earliest periods are investigated, it is in vain that we attempt to learn what that is, of which the rudiments of the embryo are composed. From not attending to this, physiologists have supposed their "primitive trace" to arise in the substance of a membrane, which the author, in his second series on the embryo*, showed could not be the case. To the same cause he thinks is referable an opinion recently advanced by Reichert, that the first traces of the new being are derived from cells of the yelk.

January 14.—A paper was read, entitled, "On the Corpuscles of the Blood." Part II. By Martin Barry, M.D., F.R.SS. L. and E.†

The observations recorded in this memoir are founded on an examination of the blood in every class of vertebrated animals, in some of the Invertebrata, and in the embryo of Mammalia and Birds. The nucleus of the blood-corpuscle, usually considered as a single object, is here represented as composed, in some instances, of two, three, or even many parts; these parts having a constant and determinate form. In the substance surrounding the nucleus, the author has frequently been able to discern, not merely "red colouring matter," but cell-like objects; and he points out an orifice as existing at certain periods in the delicate membrane by which this substance is surrounded. In a former memoir he had differed no less from previous observers regarding "cells." He had shown, for instance, that the nucleus of the cell, instead of being "cast off as useless, and absorbed," is a centre for the origin, not only of the transitory contents of its own cell, but also of the two or three principal and last-formed cells, destined to succeed that cell; and that a separation of the nucleus into two or three parts, is not, as Dr. Henle had supposed in the case of the Pus and Mucus-globule (the only instances in which the separation in question had been observed), the effect of acetic acid, used in the examination,—but that such separation is natural, apparently common to nuclei in general, and forming part of the process by which cells are reproduced. The author had farther shown the so-called nucleolus to be not a distinct object existing before the nucleus, but merely one of a series of appearances arising in succession, the one within the other, at a certain part of the nucleus, and continuing to arise even after the formation of the

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* [See L. and E. Phil. Mag., vol. xiv. p. 493.—Edit.]

† [For a notice of Part I. of this memoir, see vol. xvii. p. 300.—Edit.]

cell. These views he now confirms; and in the present paper shows that they admit of being extended to the corpuscles of the blood.

He then compares appearances observed in the latter with those he had traced in the ovum. These relate to the number of parts of which the nucleus is at different periods composed,—the nature of the nucleolus,—the communication between the nucleolus and the exterior of the cell,—the formation of the contents of the cell out of the nucleus,—the final division of the nucleus into the foundations of a limited number of young cells, destined to succeed the parent cell,—and the escape of the young cells for this purpose. It follows from these investigations, that the corpuscles of the blood are generated by a process essentially the same as that giving origin to those cells which are the immediate successors of the germinal vesicle, or original parent cell; it being also by a continuation of the same process that the corpuscle of the blood divides itself into the minuter objects figured by the author in his former paper on the blood.

He adds, that in its form and internal state, the blood-corpuscle found in the adult of certain animals, very much resembles that existing only in the fœtal life of others. It is incidentally remarked, that the fœtal brain, at certain periods, appears to consist almost entirely of objects very much resembling those which, in some stages, form the nuclei in the fœtal corpuscles of the blood.

The author concludes, by expressing his opinion, that the mode of evolution of the minute mammiferous ovum is deserving of close attention, in connexion with some of the processes by which nourishment is communicated, and the growth of the body effected, at all future periods of life.

January 21.—A paper was in part read, entitled, "On the action of certain Inorganic Compounds, when introduced directly into the Blood." By J. Blake, Esq., M.R.C.S. Communicated by P. M. Roget, M.D., Sec. R.S.

GEOLOGICAL SOCIETY.

[Continued from vol. xvii. p. 542.]

April 29, 1840.—A paper was first read, "On a few detached places along the coast of Ionia and Caria; and on the island of Rhodes;" by William John Hamilton, Esq., Sec. G.S.

The localities described in this paper are, 1. Fouges (anc. Phocæa); 2. Ritri (anc. Erythræ); 3. Sighajik (anc. Teos); 4. Scalanuova, near Ephesus; 5. Boodroom (anc. Halicarnassus); 6. Cnidus; 7. Island and shores of the Gulf of Syme; and 8. Rhodes.

1. *Fouges* is situated in a small bay at the northern extremity of the Gulf of Smyrna, and all the formations in its neighbourhood examined by Mr. Hamilton are volcanic. On the north side of the bay, a range of hills, from 300 to 400 feet high, extends several miles to the eastward, and consists in the uppermost part, of beds of smooth semivitrified red and gray trachyte, containing numerous cavities lined with mammillated chalcedony. The trachyte passes downwards into a soft, white, pumiceous sandy rock. The greater part of the hills to the north of the bay are composed of the same formation, traversed, in several places by north and east, narrow trap-

dykes, which have altered the adjacent rocks into an imperfectly banded jasper. About one mile to the north-east of Fouges, Mr. Hamilton noticed a mass of black hornstone, and to the west and north-west, near the water's edge, trappean and amygdaloidal rocks, overlaid by the pumiceous sandstone.

2. *Ritri* is situated on the shores of the bay of Erythræ, opposite the island of Scio; and the geological structure of the neighbouring district consists of red crystalline, apparently stratified trachyte, and of blue or gray, more or less, crystalline limestone, with associated sandstone. The two latter rocks are of anterior date to the trachyte, but Mr. Hamilton could not determine their relative geological age, as they appear to be destitute of organic remains. The beds of limestone are sometimes vertical. On the shore near the Acropolis the author noticed also vertical strata of indurated shale and jasper, and near the juncture of the trachyte and limestone, to the north of the Acropolis, that the calcareous beds were much shattered. The two long islands which form the anchorage, are also composed of blue semi-crystalline limestone, without traces of bedding.

3. *Sighajik.*—A rich alluvial plain, connecting the harbours of Sighajik and Teos, gradually rises to the eastward, towards the mountainous district, which extends to Smyrna. To the west the plain is separated from the sea by a range of hills composed of thickly-bedded, white, cretaceous limestone, resembling closely the limestone near Smyrna, described by Mr. Strickland*. In some places it is underlaid by beds of sandstone, and sand containing calcareous concretions. Just above the ruins of Teos, the limestone is very thinly bedded, with slightly micaceous marly way-boards, the inclination of the strata being 15° to the west; and near the ancient harbour the white limestone is underlaid by a hard, brown, micaceous sandstone, associated with beds of hard nodular limestone, evidently belonging to a much older formation. Low undulating hills of this sandstone bound the plain to the north-west. One of the two insulated remarkable hills, seen from the anchorage, also consists of it, the other being composed of vertical beds of blue marble, probably belonging to the same formation. To the north-west of the plain, this marble passes into a beautiful breccia, associated with strata of brown sandstone. Mr. Hamilton saw no igneous rocks *in situ*, but numerous blocks of greenstone are scattered about the country.

4. *Scalanuova.*—This town stands upon an insulated hill of blue semi-crystalline limestone, part of the western chain of Mount Messogis. The limestone is similar to that which occurs near Ephesus and Mount Prion, where it is associated with beds of yellow micaceous sandstone.

5. *Boodroom.*—The castle is built upon an insulated rock of similar limestone, connected with beds of argillaceous shale, of various colours. The hills to the north of the town, and on which are traceable the walls of the Acropolis of Halicarnassus, consist of the same formation, interstratified at one point with thin projecting bands of

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* Geol. Proceedings, vol. ii. p. 538; (or L. and E. Phil. Mag., vol. xi. p. 202) Geol. Trans., 2nd Series, vol. v. p. 393.

siliceous limestone. The low hills near the shore, and on which the ruins of Halicarnassus stand, are composed of horizontal beds of volcanic sand and trachytic conglomerate, formed chiefly of angular fragments of brown porphyritic trachyte. Five or six miles to the south-west of Boodroom is the conical hill of Chifoot-Kaleh, 1000 feet high. It consists entirely of reddish trachyte; and all the country between it and Boodroom is composed of trachyte or trachytic conglomerates. The hills to the west of Chifoot-Kaleh are also trachytic, with indications of columnar structure. Trachyte likewise forms part, if not all, the promontory of Karabaghla, and the islets to the westward of it. The north-east dip of the limestone of Boodroom, Mr. Hamilton thinks, may be owing to the protrusion of the igneous rocks of Karabaghla and Chifoot-Kaleh. The shore abounded in one place with pebbles of pumice.

6. *Cnidus* is situated near the extremity of Cape Krio, the western end of the south shore of the Gulf of Cos. The whole peninsula is formed of blue semi-crystalline limestone, shale and sandstone, the strata dipping near the extremity of the promontory 45° to the south-west, but increasing to a higher angle towards the east-north-east.

The following is given by Mr. Hamilton as the general structure of the country:—

Summit of the peninsula towards the west, thin-bedded calcareous shale and blue limestone, thickly bedded and cavernous. Eastward of the ruins, it is in some places interstratified with a hard greenish sandstone, resembling graywacke. The sides of the hills are occasionally obscured, by a loose limestone breccia of more modern origin.

The hills rise rapidly towards the east and north-east, and at the distance of two miles exceed 2000 feet in height. Their summit is a narrow ridge, a quarter of a mile in length from north-west to south-east, and consists of laminated calcareous shales, dipping 45° to the south-west. These shales present a very steep escarpment towards the north-east, but are overlaid towards the south-west by the blue limestone.

7. *Island and shores of the Gulf of Syme.*—The Gulf of Syme is separated from that of Cos by a narrow isthmus. The island is an uniform mass of grayish-white compact scaglia, with occasional bands and nodules of siliceous limestone. In some places the limestone is thickly bedded, but in others thinly, with way-boards of marl; and in one locality it was observed to rest on greenish sandstone. The thinner-bedded variety is sometimes reddish, and resembles the limestone of Mount Atairo, in the island of Rhodes. The strata are occasionally horizontal; but on the brow of the high table-land above the town of Syme and in other districts they are inclined from 30° to 35° to the north and north-north-west; and beyond the harbour of Panermiotis 20° to the south and south-south-east. Mr. Hamilton found no organic remains in the island.

The southern shore of the Gulf consists of the same whitish compact scaglia, with nodules of flint and jasper. Some portions of it are a breccia, composed of fragments of white limestone in a pale red paste, or of red limestone in a white paste. At the eastern extremity of the Gulf, a thinly bedded limestone alternates with bands of pale red jasper, the strata dipping 50° north-west; but in some places they are curiously contorted. The jasper increases in quantity towards the north-west, the limestone becoming less prominent. Mr. Hamilton did not land on the north side of the Gulf, but several points appeared to him, viewed from the sea, to consist of a brown arenaceous conglomerate.

8. *Rhodes.*—The northern half of the island, the portion visited by the author, consists chiefly of tertiary marine deposits, of secondary limestone and of scaglia, with sandstones and conglomerates. No igneous rocks were observed *in situ*, but numerous pebbles of greenstone and other traps were noticed in the conglomerates near the centre of the island.

TERTIARY STRATA.—These consist of a shelly testaceous limestone, sandstone, and conglomerates, and extend in a zone of variable breadth, having a quâquâversal dip, along those parts of the island visited by the author. At the north-east end, the tertiary strata rise into high and considerable hills, which stretch across the island from east to west.

The following is the order of succession :—

1. Summit of the hills three miles, south-south-west of the town of Rhodes.

	Feet.
Sandy gravel and conglomerate consisting of pebbles of scaglia	10 to 13
Fine sand, with indications of false stratifications, true dip 5° north-east	10 to 15
Gravel	8
Sand, with perpendicular veins of marl	10 to 12
Sand, with concretions of marl	
Sand, with bands of marl	

2. These beds repose on an extensive formation (considered to be from 200 to 300 feet thick) of yellow, calcareous, shelly conglomerate, the beds of which dip 10° to north-east. It contains numerous shells of the genera Pecten, Cardium and Venus, and it is the stone principally used in masonry. It extends to the town of Rhodes, and re-appears to the south of the table-land in nearly horizontal beds, some of which are very arenaceous. It is extensively developed in several places along the coast, as far as Lindo, where it rests unconformably against the secondary limestone.

3. A bed of sandy marl, containing thin bands of calcareous marl. Thickness not great.

4. A thick bed of conglomerate and gravel, extending a considerable distance to the south and south-west, and rising into lofty hills, which form steep and broken cliffs on the western coast of the island, several miles from Rhodes. It thins out gradually further south, resting at the entrance of a deep valley, upon upraised beds of blue and white scaglia and sand.

Near Archangelo, half-way between the town of Rhodes and Lindo, a similar system of tertiary rocks is extensively developed.

About one mile north of Lindo Mr. Hamilton noticed between the tertiary and secondary series a thick bed of large limestone pebbles, with sometimes quartz pebbles and boulders, cemented by a hard calcareous paste. This conglomerate rests immediately on the blue limestone, filling up its interstices; and it is considered by Mr. Hamilton to be the lowest tertiary deposit.

Secondary Rocks.—The greater part of Rhodes consists of scaglia, generally considered to be the equivalent of the cretaceous system of Europe. It is composed, (1.) of red and brown sandstones with conglomerates; (2.) of whitish gray and red scaglia limestone; and (3.) of blue limestone; but the last deposit Mr. Hamilton considers to belong to a different epoch.

1. The sandstones and conglomerates occur near the centre of the island, and apparently form the upper division of the deposit. A red conglomerate, which is found between Apollona and Embona, dips 50° to the south-south-west, and rests conformably upon whitish-gray scaglia. At the same locality exist indurated red marls and sandstone grits; and at the north-north-west foot of Mount Atairo is another bed of conglomerate, containing chiefly boulders of greenstone, and a greenish granular rock, but inclosing also rounded masses and pebbles of the gray scaglia of the neighbouring hills. The greenstone was not seen by Mr. Hamilton *in situ*.

2. The scaglia limestone is chiefly developed in the lofty ridge of Mount Atairo (anc. Mons Atabyrius), which is from 3500 to 4000 feet in height. The summit is a narrow ridge about two miles long, extending from north-east to south-west, or nearly in the direction of the axis of the island. The bed dips from 15° to 20° to the south-east. The upper portion consists of thick-bedded gray scaglia, without flints; lower down occurs a thinly-laminated limestone, with tabular masses or beds of flint; and still lower, the beds are again thicker and the flints are nodular. The total vertical dimensions of these deposits is from 800 to 900 feet. Beneath them, the scaglia is interstratified with a red marly limestone, and further down the hill are thick beds of scaglia without flints. Below the village of Embona, situated to the north-west of the mountain, a greenish compact sandstone crops out from beneath the limestone of Mount Atairo, and dips to the south-east. The range of hills to the north-north-east consists also chiefly of the gray limestone, which rests on the red and brown sandstones. Mr. Hamilton did not ascertain how far the formation ranges to the north-west.

The Acropolis of Camiro, on the east coast of the island, and six miles north of Lindo, stands upon an insulated table-rock of whitish compact scaglia, encircled at its base with tertiary strata.

3. The blue limestone is classed provisionally by Mr. Hamilton with the secondary rocks; but he is of opinion it may be of the same age as the limestone of Halicarnassus, and belong to a much older system. It occurs extensively along the east coast, particularly near Lindo, where it forms high and steep hills, against which remnants

of horizontal strata of tertiary limestone rest at a considerable height. The Acropolis of Lindo is situated upon beds of it, having an inclination of 20° to 25° to the north-west. It occurs likewise further north, between Rhodes and Archangelo, where it forms the high ridge of hills about two miles from the shore, and the low ridge of rocky islets in the middle of the plain, and parallel to the coast.

OLDER ROCKS.—The only locality at which these are satisfactorily shown, is half-way between Archangelo and Lindo, and close to the shore at the bottom of a deep bay. At this point the blue limestone, which in its lowest beds is hard and siliceous, and dips between 60° and 70° to the north-west, is underlaid constantly by a hard, black, schistose, crystalline rock, like the limestone of the Bosphorus.

In conclusion, Mr. Hamilton gives the following general statements: 1. The scaglia is more abundant in Rhodes and the south of Asia Minor than further north, and is apparently a prolongation of the scaglia which constitutes the mass of Mount Taurus. Nummulites have been found in it near Adalia, and Mr. Hamilton obtained near Deenair a species resembling one found in the scaglia of the Ionian Islands. 2. Igneous rocks are much more rare towards the south, and do not appear so often associated with the scaglia as with the older limestones. 3. Trachyte and other igneous products almost constantly accompany the blue semi-crystalline limestone, as at Erythræa and Boodroom. 4. In the absence of organic remains, Mr. Hamilton hesitates to state positively whether the blue limestone is an altered rock, or is an older formation which has been raised to the surface; but he is inclined to adopt the latter opinion, in consequence of the resemblance of the limestone to that near Constantinople, which is associated with schists, containing transition fossils.

A letter from Mr. Ottley, of Exeter, was then read, "On some specimens from the new red sandstone," considered by the writer to be casts of Alcyonia.

The specimens alluded to in this letter were found by Mr. Parker in a quarry about two miles from Exeter, in the road towards Bath. In the lower part of the quarry coarse sandstones and fine conglomerates occur, and in the upper a flat, flaggy sandstone. The beds dip 10° or 12° to the south-east. Interstratified with the conglomerate is a looser red sandstone, in which the branched concretions, considered to be of alcyonic origin by Mr. Ottley, principally occur; but they have been found also in the conglomerates, and the sandstone of the upper part of the quarry.

A paper was afterwards read, entitled, "Description of the remains of a Bird, Tortoise, and Lacertian Saurian, from the Chalk;" by Richard Owen, Esq., F.G.S.

Bird.—The three portions of Ornitholite were obtained by Lord Enniskillen from the chalk near Maidstone, and were recognised by him and Dr. Buckland as belonging to some large bird. One of the bones is nine inches in length, and has one extremity nearly entire, though mutilated, but the other is completely broken off. The extremity, partially preserved, is expanded. The rest of the shaft of the bone has a pretty uniform size, but is irregularly three-sided, with the sides flat and the angles rounded: its circumference is two inches and a quarter. The whole bone is slightly bent. The specimen differs from the femur of any known bird, in the proportion of its length to its breadth; and from the tibia or metatarsal bone, in its triedral figure, and the flatness of the sides, none of which are longitudinally grooved. It resembles most the humerus of the Albatross in its form, proportions and size, but it differs in the more marked angles bounding the three sides. The expanded extremity likewise resembles the distal end of the humerus of the Albatross, but it is too mutilated to allow the exact amount of similarity to be determined.

On the supposition that this fragment is really a part of the humerus, Mr. Owen says, its length and comparative straightness would prove it to have belonged to a longipennate natatorial bird, equalling in size the Albatross.

The two other portions of bone have been crushed; but Mr. Owen states that they belong to the distal end of the tibia, the peculiar strongly-marked trochlear extremity of which is well preserved. Their relative size to the preceding bone, supposing that specimen to be part of a humerus, is nearly the same as in the skeleton of the Albatross. There is no bird now known north of the Equator with which the fossils can be compared.

Tortoise.—The remains of the Chelonian Reptile consist of four marginal plates of the carapace, and some small fragments of the expanded ribs. The marginal plates are united by the usual finely-indented sutures, and each is impressed along the middle of its upper surface with a line corresponding to the margin of the horny plate which originally defended it. The external edge of each plate is slightly emarginated in the middle. These plates are narrower in proportion to their length than in any of the existing marine Chelonia; and they deviate still more in the character of their internal articular margin, from the corresponding plates of terrestrial Chelonia; but they sufficiently agree with the marginal plates of the carapace of the Emydes, to render it most probable that these cretaceous remains are referable to that family of Chelonia which live in fresh water or estuaries.

Lacertian Saurian.—This fossil belongs to the collection of Sir Philip Egerton; and it consists of a chain of small vertebræ in their natural relative position, with fragments of ribs and portions of an ischium and a pubis.

The bodies of the vertebræ are united by ball and socket-joints, the socket being on the anterior and the ball on the posterior part of the vertebra; and they are further proved to belong to the Saurian class of reptiles by the presence of many long and slender ribs, as well as by the conversion of two vertebræ into a sacrum, in consequence of the length and strength of their transverse processes. The remains of the ischium and the pubis are connected with the left side of the sacrum, proving incontestably that this reptile had hinder extremities as well developed as in the generality of Saurians. Of these extremities, as well as of the anterior and of the head, there are no traces.

Mr. Owen then proceeds to determine to which division of Saurians, having ball and socket vertebral joints, the fossil should be referred. In the Crocodilian or Loricate group, the transverse costigerous processes are elongated, and three, four, or five of the vertebræ which precede the sacrum are ribless, and consequently reckoned as lumbar vertebræ: in the Lacertian Sauriæ there are never more than two lumbar vertebræ, and those which have ribs support them on short convex processes or tubercles.

In the fossil from the chalk, the ribs are articulated with short processes of the kind just mentioned, resembling tubercles, and they are attached to the sides of the anterior part of all the vertebræ, except the one immediately preceding the sacrum. These characters, Mr. Owen says, in conjunction with the slenderness and uniform length of the ribs, and the degree of convexity in the articular ball of the vertebræ, prove incontestably, that the fossil is part of a Saurian, appertaining to the inferior or Lacertian group.

The under surface of the vertebræ is smooth, concave in the axis of the spine, and convex transversely. As there are twenty-one costal vertebræ anterior to the sacrum, including the single lumbar, the fossil, Mr. Owen observes, cannot be referred to the genera *Stellio*, *Leiolepis*, *Basiliscus*, *Agama*, *Lyriocephalus*, *Anolis*, or *Chamæleon*, but that a comparison may be instituted between it and the *Monitors*, *Iguanas*, and *Scinks*. In conclusion, he states, that in the absence of the cranium, teeth, and extremities, any further approximation of the fossil would be hazardous, and too conjectural to yield any good scientific result.

May 13, 1840.—A memoir was commenced "On the Classification and Distribution of the Older or Palæozoic Rocks of the North of Germany and of Belgium, as compared with formations of the same age in the British Isles;" by the Rev. Prof. Sedgwick, F.G.S., and Roderick Impey Murchison, Esq., F.G.S.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

At a meeting of this Society, held on Monday evening, March 8th, Dr. Hodgson, the President, in the Chair.

A communication was made by Mr. Tozer, of Caius College, on some mathematical formulæ for determining the permanent effects of Emigration and Immigration on numbers. The solution involves a consideration of all those causes by which the duration of life, or the rate of its production, may be affected, which may be called into operation by the transfer.

Where all the elements are constant the numbers of a people, after a given time, may be determined by the solution of an equation of finite differences in which the coefficients are constant; the probable change in the value of those coefficients may, when the data are sufficient, be calculated by a method suggested by Laplace.

Where the data are insufficient for the complete solution of the problem, Mr. Tozer considers the attempt at accurate investigation to afford the best means of analysing the processes which the mind employs in arriving at its conclusions, when the correctness or incorrectness of those conclusions is incapable of demonstration; and by showing where the data are insufficient, of suggesting the nature of the observations by which they may be supplied, and thus of gradually approximating the science to which the problem belongs to the demonstrative sciences.

Scientific Works lately published.

Taylor's Scientific Memoirs—Part VIII. completing the Second Volume.

Catalogues of the Miscellaneous Manuscripts and of the Manuscript Letters in the possession of the Royal Society.

The Philosophical Transactions. Part II. 1840.

A Description of British Guiana, Geographical and Statistical: exhibiting its Resources and Capabilities, together with the present and future Condition and Prospects of the Colony. By Robert H. Schomburgk, Esq.

On the Heat of Vapours and on Astronomical Refractions. By Sir J. W. Lubbock, Bart.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1841.

Chiswick.—Feb. 1. Snowing. 2. Snow-showers. 3. Frosty: dry and cold: very severe frost at night. 4. Frosty: overcast. 5. Dry cold haze: windy at night. 6. Boisterous. 7. Boisterous: hazy and cold. 8—11. Hazy and cold. 12. Dense fog: very fine: rain. 13. Overcast: rain. 14. Rain: cloudy. 15. Cloudy: slight rain. 16, 17, Hazy. 18. Fine. 19. Rain: cloudy and fine. 20. Cloudy and fine: rain. 21. Overcast and fine. 22. Dense fog. 23. Hazy: rain. 24. Hazy and cold. 25. Cloudy and cold: rain. 26. Rain. 27. Cloudy: rain. 28. Very clear: cloudy and fine.

Boston.—Feb. 1. Cloudy: snow A.M. and P.M. 2. Fine: snow early A.M.: snow P.M. 3. Cloudy: snow early A.M. and P.M. 4, 5. Cloudy. 6, 7. Stormy. 8. Cloudy: snow P.M. 9, 10. Cloudy. 11—13. Cloudy: rain P.M. 14. Cloudy. 15. Cloudy: rain P.M. 16. Cloudy. 17. Rain. 18, 19. Cloudy. 20, 21. Fine. 22, 23. Foggy. 24. Rain. 25. Cloudy: rain P.M. 26. Rain: rain P.M. 27. Rain. 28. Fine.

Applegarth Manse, Dumfries-shire.—Feb. 1, 2. Sprinkling of snow: frost P.M. 3. Snow-showers: frost. 4. Frost: fair but cloudy. 5. Frost: sprinkling of snow. 6. Frost: occasional snow-showers. 7. Frost: severe and cold. 8, 9. Frost: cold and withering. 10. Frost, but giving way. 11. Thaw and heavy rain: sleet. 12. Fog: rain: fine thaw. 13. Rain all day. 14. Rain in the evening: mild. 15. Rain all day. 16, 17. Fair but cloudy. 18. Wet all day. 19. Clear and cold. 20. Fine. 21, 22. Fine, but cloudy. 23. Rain A.M: moist P.M. 24. Clear and cold. 25. Cloudy and threatening rain. 26. Cloudy with high wind. 27. Frost in the morning. 28. Frost in the morning with snow on the hills.

Sun shone out 19 days. Rain fell 8 days. Frost 11 days. Snow 6 days.

Wind north 1 day. North-east 8 days. East-north-east 2 days. East 2 days. East-south-east 1 day. South-east 4 days. South 4 days. South-west 2 days. West 1 day. North-west 1 day. North-north-west 2 days.

Calm 6 days. Moderate 11 days. Brisk 4 days. Strong breeze 4 days. Boisterous 3 days.

Mean temperature of the month	36°·50
Mean temperature of February 1840	36 ·78
Mean temperature of spring-water	42 ·60
Mean temperature of spring-water, Feb. 1840	**44 ·16**

Meteorological Observations made at the Apartments of the Royal Society 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, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. Feb.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	30·358	30·346	30·268	30·12	30·42	30·47	30·7	42·0	29·7	31	23	32	35	32	N.	NE.	calm	NE.	·116	...	...	...	30
2.	30·072	30·093	29·931	29·90	30·25	30·10	27·7	32·4	25·8	31	18	28·5	33	26½	N.	NE.	calm	NE.	...	...	...	...	24
3.	30·000	30·020	29·977	29·80	30·07	30·01	22·2	29·7	21·6	28	14	25·5	33	27½	N.	E.	calm	ENE.	...	...	...	...	20
4.	29·760	29·803	29·670	29·62	29·87	29·77	25·7	25·7	22·2	30	22	27	34	27	E.	NE.	E.	NE.	...	...	...	...	21
5.	29·682	29·722	29·688	29·55	29·78	29·85	25·7	30·0	24·6	30	26	29·5	34	30½	NE.	NE.	E.	NE.	...	...	...	...	22
○ 6.	29·648	29·661	29·609	29·56	29·80	29·78	27·3	29·8	25·2	28	24	28	30½	26½	NE.	E.	E.	E.	...	...	...	...	24
7.	29·480	29·500	29·443	29·46	29·65	29·58	23·8	28·8	23·8	27	24	23·5	28½	27½	NE.	E.	E.	ENE.	...	...	...	0·05	20
8.	29·372	29·442	29·395	29·24	29·50	29·57	26·7	26·7	24·0	29	26	26	31½	27	N.	NE.	E.	NE.	...	...	...	...	23
9.	29·636	29·952	29·645	29·47	29·70	29·87	27·4	29·3	27·2	30	27	28·5	32½	28½	N.	NE.	E.	NE.	...	...	...	...	24
10.	30·014	30·017	29·959	29·76	29·80	29·68	28·3	30·2	27·0	32	27	27·5	33	27	SE.	SE.	calm	NE.	...	·04	...	...	25
11.	29·850	29·866	29·657	29·58	29·53	29·38	36·7	37·2	27·0	41	37	33	39	29	SE.	S.	calm	E.	...	·04	...	...	28
12.	29·672	29·745	29·640	29·33	29·40	29·48	41·7	42·5	35·6	51	38	38	40	36	S.	SW.	calm	S.	...	·04	·58	...	36
☾ 13.	29·672	29·659	29·313	29·30	29·28	29·02	44·8	49·0	41·4	51	44	41	45	37½	SSE.	S.	calm	SE.	...	·01	·10	...	39
14.	29·180	29·288	29·157	28·82	28·94	28·87	47·3	48·3	45·0	52	41	43	45½	41	S.	SW.	calm	SE.	·022	...	·09	2·06	42
15.	29·274	29·263	29·071	28·89	28·95	28·85	43·4	51·4	42·4	49	42	43	45	38	SSE.	S.	calm	ESE.	...	·15	...	...	42
16.	29·076	29·141	29·086	28·73	29·00	29·19	45·7	49·2	42·5	51	39	42	46	39½	E.	SE.	calm	SE.	·091	·11	·15	...	43
17.	29·268	29·584	29·584	28·97	29·30	29·34	40·3	50·0	40·8	46	36	36·5	46½	41	NW.	W.	calm	SE.	·125	·01	·06	...	41
18.	29·642	29·614	29·490	29·26	29·24	29·26	43·3	45·4	40·7	53	38	40	45	38	SE.	S.	calm	S.	...	·04	·02	...	41
19.	29·672	29·744	29·601	29·22	29·44	29·43	46·8	51·4	43·0	50	35	44	45	41	S.	SW.	calm	SW.	·088	·02	...	...	46
20.	29·846	29·977	29·808	29·33	29·51	29·85	46·7	50·0	40·4	56	40	42	48	40	S.	S.	SE.	S.	·033	·11	...	...	45
● 21.	30·198	30·296	30·162	29·77	30·05	30·15	45·3	52·2	44·7	56	29	36	46	38½	NW.	NE.	calm	S.	·063	...	·15	1·56	44
22.	30·390	30·345	30·323	29·93	30·18	30·19	40·3	50·2	40·2	42	38	32	46	38	NW.	NE.	calm	SW.	...	...	...	...	42
23.	30·356	30·323	30·298	29·98	30·24	30·28	38·6	43·7	38·6	44	32	38	48½	41	NE.	NE.	calm	NE.	...	...	...	...	41
24.	30·328	30·304	30·298	29·95	30·39	30·45	34·7	41·3	33·8	40	36	36	46	37	N.	NE.	E.	N.	...	·01	·07	...	36
25.	30·336	30·308	30·076	29·98	30·33	30·04	38·8	41·5	33·8	42	35	38	45	31½	N.	NE.	calm	W.	...	·07	...	...	37
26.	29·378	29·872	29·638	29·47	29·80	29·67	43·7	44·5	38·7	48	35	41·5	48	41	WNW.	NW.	calm	NW.	·108	·04	·07	...	42
27.	29·756	29·730	29·368	29·36	29·80	29·87	39·7	47·7	37·0	45	32	38	43½	34	NW.	N.	N.	NNW.	·100	·07	·24	...	37
☽ 28.	29·880	29·874	29·681	29·52	29·76	29·47	36·9	43·3	35·0	47	32	35	43½	28½	WNW.	NW.	calm	NNW.	·044	...	·05	0·11	34
Mean.	29·778	29·833	29·708	29·49	29·713	29·695	36·4	40·8	34·0	41·42	31·78	34·7	40·5	33·2					Sum. ·790	·76	1·58	3·78	Mean. 34

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

[THIRD SERIES.]

MAY 1841.

LIV. *Remarks on* Professor Challis's *Reply to* Mr. Airy's *Objections to the Investigation of the Resistance of the Atmosphere to an Oscillating Sphere.* *By* GEORGE BIDDELL AIRY, *Esq., M.A., F.R.S., Astronomer Royal.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE not had leisure earlier to notice the remarks of Professor Challis in your February Number, upon my objection to his investigation of the resistance to a spherical body oscillating in an elastic medium. I beg to express my sense of the courtesy with which Professor Challis has replied to my objection, and to avow my opinion that a discussion conducted in this manner cannot but be advantageous to the interests of science. With this feeling, I think it right again to state that the considerations urged by Professor Challis in his last communication, do not at all remove my objection to the fundamental parts of his investigation. And I trust that, by putting my own reasoning in a different form, I shall be able to show, to the satisfaction of Professor Challis, that the solution which he has adopted for expressing the movement of the particles of air surrounding the ball, is untenable.

The reasoning of a mixed nature upon which Professor Challis has founded and supported his investigation, and upon which also I have objected to it, is (if I may use such a term) extremely hazardous. I mean by this expression to assert, that, unless managed with the greatest caution, it is apt to introduce or to conceal important errors of principle. I shall, therefore, abandon this kind of reasoning, and shall confine myself to another mode of investigation, of which the results will be beyond doubt, namely, that by rectangular coordinates.

The point in dispute may be well stated in Professor Challis's words: "I have certainly considered it possible that the velocity of the fluid at a given distance from the centre to or from which it is directed, may, at a given instant, be different in different directions from the centre, provided there be no abrupt variation."

Now I undertake to show that,

1st. If the motion of the particles be directed to or from a centre (or approximately so, the whole investigation being approximate), the motions must be the same in all directions, in phase as well as in coefficient.

2nd. If, in order to support Professor Challis's expression for the movement of the particles to or from a centre, we suppose other movements perpendicular to these radii to coexist, then it will be shown that the possibility of such a combination of movements is not proved, and that (as it depends upon finding two quantities which shall satisfy three equations) the presumption is, that it is impossible.

I proceed now with the investigation.

If u, v, w, be the velocities in the directions x, y, z, of a particle which at the time t has the coordinates x, y, z (those velocities being expressed in terms of x, y, z, and t), and if ϱ be the density at that point and p the corresponding pressure, and if $\varrho = kp$; the equations to the motion of that particle (no accelerating force being supposed to act) are the following:

$$\frac{1}{\rho}\cdot\frac{dp}{dx} = -\frac{du}{dt} - \frac{du}{dx}u - \frac{du}{dy}v - \frac{du}{dz}w$$

$$\frac{1}{\rho}\cdot\frac{dp}{dy} = -\frac{dv}{dt} - \frac{dv}{dx}u - \frac{dv}{dy}v - \frac{dv}{dz}w$$

$$\frac{1}{\rho}\cdot\frac{dp}{dz} = -\frac{dw}{dt} - \frac{dw}{dx}u - \frac{dw}{dy}v - \frac{dw}{dz}w$$

$$\frac{d\rho}{dt} = -\frac{d.\rho u}{dx} - \frac{d.\rho v}{dy} - \frac{d.\rho w}{dz}.$$

These equations are rigorous. If, however, we confine ourselves to quantities of the first order of the displacements and velocities: since $u\frac{du}{dx}, v\frac{du}{dy}$, &c. are evidently of the second order: and since $u\,\frac{d\rho}{dx}$, or $ku\frac{dp}{dx}$, or $k\rho u\Big(\frac{du}{dt}$

$+ u\dfrac{du}{dx} + \&c.\Big)$ is a quantity of the second order: the equations are reduced to these:

$$\frac{d.\log\rho}{dx} = -k\frac{du}{dt}$$

$$\frac{d.\log\rho}{dy} = -k\frac{dv}{dt}$$

$$\frac{d.\log\rho}{dz} = -k\frac{dw}{dt}$$

$$\frac{d.\log\rho}{dt} = -\frac{du}{dx} - \frac{dv}{dy} - \frac{dw}{dz}.$$

And the method of testing the possibility of any assumed system of movement, or of discovering the conditions which must be satisfied by the system of movements, will be, to form the expressions on the right-hand side of these equations, and to try whether they satisfy the equations which must hold among the four differential coefficients of the same quantity.

I. Now suppose the whole velocity of the particle x, y, z, to be directed from the centre, and at the time t to have the value υ (motion towards the centre being implied by a negative value of υ); and suppose that centre to be the origin of coordinates. Then

$$u = \upsilon\frac{x}{r},\ v = \upsilon\frac{y}{r},\ w = \upsilon\frac{z}{r}: \text{ where } r = \sqrt{x^2+y^2+z^2}.$$

$$\frac{du}{dt} = \frac{x}{r}.\frac{d\upsilon}{dt},\ \frac{dv}{dt} = \frac{y}{r}.\frac{d\upsilon}{dt},\ \frac{dw}{dt} = \frac{z}{r}.\frac{d\upsilon}{dt}. \text{ Also}$$

$$\frac{du}{dx} = \frac{d\upsilon}{dx}.\frac{x}{r} + \upsilon\left(\frac{1}{r} - \frac{x}{r^2}.\frac{dr}{dx}\right) = \frac{d\upsilon}{dx}.\frac{x}{r} + \upsilon\left(\frac{1}{r} - \frac{x^2}{r^3}\right):$$

$$\frac{dv}{dy} = \frac{d\upsilon}{dy}.\frac{y}{r} + \upsilon\left(\frac{1}{r} - \frac{y^2}{r^3}\right): \quad \frac{dw}{dz} = \frac{d\upsilon}{dz}.\frac{z}{r} + \upsilon\left(\frac{1}{r} - \frac{z^2}{r^3}\right):$$

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = \frac{1}{r}\left(x\frac{d\upsilon}{dx} + y\frac{d\upsilon}{dy} + z\frac{d\upsilon}{dz}\right) + \frac{2\upsilon}{r}.$$ Substituting these values in the equations above, they become

$$\frac{d.\log\rho}{dx} = -\frac{kx}{r}.\frac{d\upsilon}{dt} \qquad = X$$

$$\frac{d.\log\rho}{dy} = -\frac{ky}{r}.\frac{d\upsilon}{dt} \qquad = Y$$

$$\frac{d\,.\log\rho}{dz} = -\frac{kz}{r}\cdot\frac{dv}{dt} \qquad = \mathrm{Z}$$

$$\frac{d\,.\log\rho}{dt} = -\frac{1}{r}\left(x\frac{dv}{dx}+y\frac{dv}{dy}+z\frac{dv}{dz}\right)-\frac{2v}{r} = \mathrm{T}.$$

In order that the supposed motion may be possible, these expressions must satisfy the following equations:

$$(1.)\ \frac{d\mathrm{X}}{dy} = \frac{d\mathrm{Y}}{dx}:\quad (2.)\ \frac{d\mathrm{Y}}{dz} = \frac{d\mathrm{Z}}{dy}:\quad (3.)\ \frac{d\mathrm{Z}}{dt} = \frac{d\mathrm{T}}{dz}.$$

Equation (1.), on performing the differentiation, becomes

$$\frac{kxy}{r^3}\cdot\frac{dv}{dt} - \frac{kx}{r}\cdot\frac{d^2v}{dy\,dt} = \frac{kxy}{r^3}\cdot\frac{dv}{dt} - \frac{ky}{r}\cdot\frac{d^2v}{dx\,dt},$$

or $x\dfrac{d^2v}{dy\,dt} = y\dfrac{d^2v}{dx\,dt}$.

Let $\dfrac{dv}{dt} = \omega$; then this equation becomes $\dfrac{1}{y}\cdot\dfrac{d\omega}{dy} = \dfrac{1}{x}\cdot\dfrac{d\omega}{dx}$. If we solve this equation, in the usual way of solving partial differential equations of the first order, we find that ω must be a function of x^2+y^2.

Treating equation (2.) in the same manner, we find that ω must be a function of y^2+z^2.

It is evident that these results can be united only by the supposition that ω is a function of $x^2+y^2+z^2$, or of r^2, or of r.

That is, $\dfrac{dv}{dt}$ is a function of r and t only; and the quantities x, y, z, do not enter into it except as combined in the formula $x^2+y^2+z^2$.

Integrating this function, $v = \mathrm{R}+\mathrm{S}$, where R is a function of r and t, and S is a function of x, y, z.

To form equation (3.), we remark that $\mathrm{Z} = -\dfrac{kz}{r}\cdot\dfrac{d\mathrm{R}}{dt}$, and therefore $\dfrac{d\mathrm{Z}}{dt} = -\dfrac{kz}{r}\;\dfrac{d^2\mathrm{R}}{dt^2}$. Also that part of T which depends on R is $-\dfrac{1}{r}\left(x\dfrac{d\mathrm{R}}{dr}\cdot\dfrac{x}{r}+y\dfrac{d\mathrm{R}}{dr}\cdot\dfrac{y}{r}+z\dfrac{d\mathrm{R}}{dr}\cdot\dfrac{z}{r}\right)$ $-\dfrac{2\mathrm{R}}{r} = -\dfrac{d\mathrm{R}}{dr}-\dfrac{2\mathrm{R}}{r}$. Put S′ for $-\dfrac{1}{r}\left(x\dfrac{d\mathrm{S}}{dx}+y\dfrac{d\mathrm{S}}{dy}\right.$ $\left.+z\dfrac{d\mathrm{S}}{dz}\right)-\dfrac{2\mathrm{S}}{r}$. Then $\mathrm{T} = -\dfrac{d\mathrm{R}}{dr}-\dfrac{2\mathrm{R}}{r}+\mathrm{S}'$: and $\dfrac{d\mathrm{T}}{dz}$ $= -\dfrac{z}{r}\cdot\dfrac{d^2\mathrm{R}}{dr^2}-\dfrac{2z}{r^2}\cdot\dfrac{d\mathrm{R}}{dr}+\dfrac{2z}{r^3}\mathrm{R}+\dfrac{d\mathrm{S}'}{dz}$.

Equation (3.) therefore becomes

$$0 = \frac{k z}{r} \cdot \frac{d^2 R}{d t^2} - \frac{z}{r} \cdot \frac{d^2 R}{d r^2} - \frac{2 z}{r^2} \cdot \frac{d R}{d r} + \frac{2 z}{r^3} R + \frac{d S'}{d z}.$$

The two parts of this expression which depend upon R and upon S' are essentially distinct, and the equation therefore expresses these two things:

$$0 = \frac{d S'}{d z} \quad \left(\text{and similarly } 0 = \frac{d S'}{d x}, \quad 0 = \frac{d S'}{d y}\right)$$

$$0 = k \frac{d^2 R}{d t^2} - \frac{d^2 R}{d r^2} - \frac{2}{r} \cdot \frac{d R}{d r} + \frac{2 R}{r^2}.$$

From the first of these equations $S' = C$,

$$\text{or } x \frac{d S}{d x} + y \frac{d S}{d y} + z \frac{d S}{d z} + 2 S = - C r,$$

the integral of which is $S = \frac{1}{r^2} \cdot \phi \left(\frac{x}{r}, \frac{y}{r}, \frac{z}{r}\right) - \frac{C r}{3}$.

The solution of the second equation is

$$R = \frac{f'(r - a t)}{r} - \frac{f(r - a t)}{r^2} + \frac{F'(r + a t)}{r} - \frac{F(r + a t)}{r^2},$$

where $k a^2 = 1$,

And hence the complete value of v in its most general form is

$$v = \frac{f'(r - a t)}{r} - \frac{f(r - a t)}{r^2} + \frac{F'(r + a t)}{r} - \frac{F(r + a t)}{r^2}$$

$$+ \frac{1}{r^2} \phi \left(\frac{x}{r}, \frac{y}{r}, \frac{z}{r}\right) - \frac{C r}{3}.$$

I will now explain these several terms.

1st. Motion from the centre is possible without any other motion if the velocity is expressed by $- \frac{C r}{3}$, that is, if every spherical layer to unlimited distance increases or diminishes constantly with the time in the same geometrical proportion. The meaning of this term will be fully understood if we examine the corresponding expression for $\frac{d . \log \rho}{d t}$, which is then S' or C. As we cannot conceive a case in which the surrounding atmosphere can have the density proper for this case, we may at once dismiss it as inapplicable to any practical problem.

2nd. Motion from the centre is possible without any other motion if the velocity is expressed by $\frac{1}{r^2} \phi \left(\frac{x}{r}, \frac{y}{r}, \frac{z}{r}\right)$, that is, if in every different direction it is the same, or different according to any arbitrary law; but if in different points of that direction, it is inversely as the square of the distance from the centre. This shows that the same quantity of air passes in every second of time in the same direction through every section of each small solid angle. As we cannot in any practical case provide for the incessant supply of air which this will require, we may lay aside this form of solution. I may remark, that this solution is made possible only by the neglect of terms of the second order of velocities; for it would easily be found, upon examining the forces corresponding to this motion, that there are forces depending on the squares of velocity which are inconsistent with this motion.

3rd. Motion from the centre is possible without any other motion if the velocity is expressed by $\frac{f'(r-at)}{r} - \frac{f(r-at)}{r^2} + \frac{F'(r+at)}{r} - \frac{F(r+at)}{r^2}$, where $k a^2 = 1$. This expression represents two series of waves, one rolling from the centre and the other towards the centre: but, for each of these waves, the phase and the intensity are the same in every direction from the centre.

The form of solution adopted by Professor Challis is $\cos\theta . \left(\frac{f'(r-at)}{r} - \frac{f(r-at)}{r^2}\right)$. If z be the axis from which θ is measured, $\cos\theta = \frac{z}{r}$, and this expression becomes $\frac{z . f'(r-at)}{r^2} - \frac{z . f(r-at)}{r^3}$. This expression is not included in any of the formulæ already discussed; and it evidently will not satisfy the conditions found above; and it cannot therefore apply to the problem before us.

II. But suppose (as an assumption of which the possibility is to be tested), that the motion from the centre is represented by the formula $\frac{zR}{r}$, as assumed by Professor Challis, and that this motion is accompanied by other motions normal to the radius for every point. These motions may be represented in the most general way by one velocity P in the plane passing through z and r, and another velocity Q perpendicular to that plane. Resolving all the assumed velocities in the directions of x, y, and z, we have

$$u = \mathrm{R}\,\frac{x\,z}{r^2} - \mathrm{P}.\frac{z}{r}\,.\,\frac{x}{\sqrt{x^2+y^2}} - \mathrm{Q}\,\frac{y}{\sqrt{x^2+y^2}}$$

$$v = \mathrm{R}\,\frac{y\,z}{r^2} - \mathrm{P}\,.\frac{z}{r}\,.\,\frac{y}{\sqrt{x^2+y^2}} + \mathrm{Q}\,\frac{x}{\sqrt{x^2+y^2}}$$

$$w = \mathrm{R}\,\frac{z^2}{r^2} + \mathrm{P}\,\frac{\sqrt{x^2+y^2}}{r}.$$

For simplicity, put $\dfrac{\mathrm{P}\,z}{r\sqrt{x^2+y^2}} = p$, $\dfrac{\mathrm{Q}}{\sqrt{x^2+y^2}} = q$: then

$$u = \mathrm{R}\,\frac{x\,z}{r^2} - p\,x - q\,y$$

$$v = \mathrm{R}\,\frac{y\,z}{r^2} - p\,y + q\,x$$

$$w = \mathrm{R}\,\frac{z^2}{r^2} + p.\frac{x^2+y^2}{z}.$$

From these,

$$\frac{d.\log\rho}{d\,x} = -k\,\frac{d\,u}{d\,t} = k\left(-\frac{x\,z}{r^2}\,.\,\frac{d\,\mathrm{R}}{d\,t} + x\,\frac{d\,p}{d\,t} + y\,\frac{d\,q}{d\,t}\right) = \mathrm{X}$$

$$\frac{d.\log\rho}{d\,y} = -k\,\frac{d\,v}{d\,t} = k\left(-\frac{y\,z}{r^2}\,.\,\frac{d\,\mathrm{R}}{d\,t} + y\,\frac{d\,p}{d\,t} - x\,\frac{d\,q}{d\,t}\right) = \mathrm{Y}$$

$$\frac{d.\log\rho}{d\,z} = -k\,\frac{d\,w}{d\,t} = k\left(-\frac{z^2}{r^2}\,.\,\frac{d\,\mathrm{R}}{d\,t} - \frac{x^2+y^2}{z}\,.\,\frac{d\,p}{d\,t}\right) = \mathrm{Z}$$

$$\frac{d.\log\rho}{d\,t} = -\frac{d\,u}{d\,x} - \frac{d\,v}{d\,y} - \frac{d\,w}{d\,z} = -\frac{d\,\mathrm{R}}{d\,r}\,.\,\frac{z}{r} - 2\,\mathrm{R}\,\frac{z}{r^2}$$

$$+\,p\,.\,\frac{x^2+y^2+2\,z^2}{z^2} + x\,\frac{d\,p}{d\,x} + y\,\frac{d\,p}{d\,y} + y\,\frac{d\,q}{d\,x} - x\,\frac{d\,q}{d\,y} - \frac{x^2+y^2}{z}.\frac{d\,p}{d\,z} = \mathrm{T}.$$

Now $\dfrac{d\,\mathrm{X}}{d\,y}$ must $= \dfrac{d\,\mathrm{Y}}{d\,x}$, or

$$0 = x\,\frac{d^2\,p}{d\,y\,d\,t} - y\,\frac{d^2\,p}{d\,x\,d\,t} + 2\,\frac{d\,q}{d\,t} + y\,\frac{d^2\,q}{d\,y\,d\,t} + x\,\frac{d^2\,q}{d\,x\,d\,t}.$$

And $\dfrac{d\,\mathrm{Y}}{d\,z}$ must $= \dfrac{d\,\mathrm{Z}}{d\,y}$, or

$$0 = -\frac{y}{r^2}\,.\,\frac{d\,\mathrm{R}}{d\,t} + \frac{2\,y}{z}\,.\frac{d\,p}{d\,t} + y\,\frac{d^2\,p}{d\,z\,d\,t} - x\,\frac{d^2 q}{d z\,d t}$$

$$+\,\frac{x^2+y^2}{z}\,.\,\frac{d^2\,p}{d y\,d\,t}.$$

And $\dfrac{d\,\mathrm{Z}}{d\,t}$ must $= \dfrac{d\,\mathrm{T}}{d\,z}$, or

$$
\begin{aligned}
0 = {} & \frac{k z^2}{r^2} \cdot \frac{d^2 \mathrm{R}}{d t^2} - \frac{z^2}{r^2} \cdot \frac{d^2 \mathrm{R}}{d r^2} - \frac{2 z}{r^3} \cdot \frac{d \mathrm{R}}{d r} + \frac{2 z^2}{r^4} \mathrm{R} \\
& - \frac{x^2 + y^2}{r^3} \cdot \frac{d \mathrm{R}}{d r} - 2 \mathrm{R} \frac{x^2 + y^2}{r^4} + k \cdot \frac{x^2 + y^2}{z} \cdot \frac{d^2 p}{d t^2} \\
& - 2 p \frac{x^2 + y^2}{z^3} + 2 \frac{d p}{d z} \cdot \frac{r^2}{z^2} + x \frac{d^2 p}{d x d z} + y \frac{d^2 p}{d y d z} \\
& + y \frac{d^2 q}{d x d z} - x \frac{d^2 q}{d y d z} - \frac{x^2 + y^2}{z} \cdot \frac{d^2 p}{d z^2}.
\end{aligned}
$$

The form assumed for R makes the four first terms of the last equation destroy each other. Removing these, and substituting the value of R in the other terms in which that letter occurs, we have *three* differential equations to determine *two* unknown functions. Unless it can be shown that one of these equations can be deduced from the other two, the discovery of these two unknown functions must be, in the nature of things, impossible. Without professing to have examined the equations minutely, I can only say at present that the three equations appear to me to be inconsistent, and therefore I consider that the motion assumed by Professor Challis is impossible, even when accompanied with any other transversal motions whatever. The *onus* of proving that the three equations are consistent, rests with the supporter of such a solution as that to which I have alluded.

I am, Gentlemen,
your very obedient servant,
G. B. AIRY.

Royal Observatory, Greenwich,
March 22, 1841.

LV. *On the Electrical Phænomena attending the Efflux of Condensed Air, and of Steam generated under Pressure.* By WM. GEO. ARMSTRONG, *Esq.*[*]

THE investigation of the causes which induce and modify the development of electricity during the emission of steam and compressed air, derives unusual importance from the prospect it affords of elucidating certain obscure principles in electrical science, and of leading to an explanation of several meteorological and other natural phænomena, of which no satisfactory theory has been hitherto adduced. The mere novelty of the subject, and its connexion with that mysterious and all-pervading agent, electricity, the further knowledge of which is so essential to our progress in natural philosophy,

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[*] Communicated by the Author: see p. 265 of our last Number.

impart peculiar interest to the inquiry, and render it worthy of zealous pursuit.

In the hope, therefore, of promoting an investigation which appears to me to possess so much interest and importance, I have recently undertaken a series of additional experiments, both on the electricity of expanding air, and on that of effluent steam, and I trust that the results which I have now to communicate will be found deserving of attention, and conducive to further discovery.

These experiments will be treated in the following pages under two heads; the first embracing the experiments on expanding air, and the second those on effluent steam.

In my last communication to the Philosophical Magazine* I stated—

1st, That I had succeeded in producing a powerful electrical development by discharging highly compressed air from an insulated receiver of considerable capacity.

2ndly, That on repeating the experiment a great number of times, I found the electricity manifested in the receiver to be generally *negative*, but occasionally *positive*.

3rdly, That the intensity of the development had proved exceedingly unequal, the receiver having sometimes been so highly electrified as to yield a spark a quarter of an inch long, while at other times its electricity had been feeble, and that frequently I could detect no electricity at all.

4thly, That the electricity of the emitted air had been positive in every instance that I had tried it.

5thly, That I had more frequently succeeded in producing an electrical development when the receiver was cold, and contained a little moisture, than when it was warm and dry.

Having thus briefly recapitulated what I have already written on the subject, the necessity of referring to the communication I have mentioned will be avoided.

I have now clearly ascertained that the temperature of the receiver, and the presence of water within it, powerfully influence the phænomena. The colder the receiver the stronger is the development of electricity; and if the receiver, when perfectly dry, be heated until it becomes unpleasantly hot to be touched with the naked hand, the discharge of the air ceases to produce any indication of electricity. If the inside of the receiver be damp, a higher temperature is required to obliterate the signs of electricity; and if a few ounces of water be poured into the receiver, it may be heated to any degree without preventing an electrical development, or even rendering it very inconsiderable. At low temperatures, however, the presence of water in the receiver, or a humid condition of

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* Number for February, p. 133.

the compressed air, appears to have little tendency to augment the effects. This I infer from the following experiment.

After thoroughly drying the interior of the receiver I introduced a quantity of caustic potash for the purpose of absorbing the moisture of the condensed air. I then charged the receiver, and set it in a cold place, where I allowed it to remain for about twelve hours, to afford sufficient time for the potash to produce the desired effect. After this I insulated the receiver and discharged the air, and found the effects to be nearly the same as when no precautions were taken to exclude moisture.

Whether the receiver be wet or dry, I find that a rapid discharge of the air is indispensable to the production of the phænomena. The effects are always strongest when the cock for discharging the air is fully opened; and when by only partially opening the cock the emission of the air is prolonged beyond the period of about a minute, no electricity appears to be developed.

My former experiments, of which the results were so exceedingly capricious, were made in very frosty weather, but those of which I am now speaking were made when the weather was mild and damp, and were much more uniform in their results; but whether the singular fluctuations observed in the first set of experiments were owing to the state of the atmosphere, or to some other cause which has escaped my detection, I feel quite unable to say.

In my latter experiments the electricity of the receiver was uniformly negative, and the intensity of the electrical development did not vary considerably. When the receiver was not insulated, the electricity of the effluent air was always positive; but when the receiver was supported upon an insulated stand, it frequently happened, especially if the receiver were not internally dry, that the gold leaves of the electroscope connected with the pointed conductor, by which the electricity was drawn from the jet of air, separated, first with *positive* electricity, then closed, and opened with *negative* electricity. This effect might, with much probability, be ascribed to the formation of a conducting communication between the insulated receiver and the pointed conductor, by means of watery particles ejected with the air; only, if this were the case, the pointed conductor would not retain its electricity, as it invariably does when the receiver is *not* insulated.

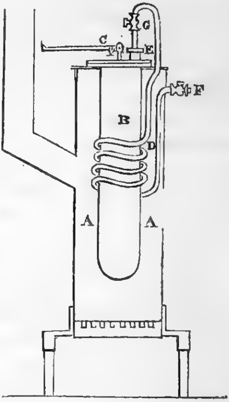
Sometimes the development of electricity does not take place until the air is almost wholly discharged, and then the pith balls suspended from the receiver, and the gold leaves of the electroscope attached to the pointed conductor, abruptly start open, the action being accompanied by a peculiar sound indicating the ejection of drops of water from the tube through which the air is escaping. It would be a curious fact if the sound thus occasioned were proved identical with that which Dr. Schafhaeutl mentions as uniformly accompanying the development of electricity in his experiments on the steam evolved from a Marcet boiler. This sound, however, often occurs *after* the electricity has appeared, but it is uniformly the signal for the electroscope to open when the development has not previously taken place. I cannot say that I have ever noticed a similar noise in any of my experiments on steam.

I am wholly at a loss for an explanation of these singular phænomena. It is well known that a copious precipitation of vapour, and a greatly accelerated evaporation, are the simultaneous effects of the sudden dilatation of condensed air in a receiver—the precipitation being caused by the extreme cold produced by expansion, and the increased evaporation by the diminution, which the precipitation occasions, of the quantity of transparent vapour contained in the air—and to the agency of these two principles I was at first inclined to attribute the effects in question, conjecturing that the variation in the kind of electricity developed in the receiver, might be owing to the predominance, sometimes of evaporation, and sometimes of precipitation. The remarkable influence of heat and moisture, and the effect of a slow evacuation of the receiver, by which precipitation is known to be prevented, seem to point out the connexion of the phænomena with one at least of the principles I have named; but I find so much difficulty in accounting for many of the effects I have recently observed, by reference to both or either of these causes, or to any other cause I can think of, that I am at present unable to form any opinion on the subject.

I now come to the second division of this paper, and shall commence with a description of the evaporating apparatus which I used in the experiments on effluent steam.

This apparatus was constructed under my own directions for the especial purpose of experiment, for which it is much better adapted than a steam-engine boiler; although from the smallness of its size it is not calculated to exhibit effects equally brilliant. It consists, chiefly, of a strong cylindrical boiler, and a stove, in which the boiler is placed vertically, in such a manner as to be exposed on all sides to the heat of the fire. The boiler is thirty inches deep and four inches wide in the inside, and is made of the alloy of copper and tin, usually called gun-metal. The stove is supported upon glass legs to insulate it.

The accompanying figure represents the entire apparatus. A A is a vertical section of the stove; B is the boiler; C a graduated safety valve; D a copper pipe which proceeds out of the boiler through the stuffing-box E, then enters the stove, in which it is coiled three or four times round the boiler a little above the fuel, and finally terminates outside of the stove in the cock F, at which the steam is discharged, after being thoroughly dried in passing through the pipe thus subjected to the fire. This pipe is easily removed, and instead of it I frequently inserted in the boiler a simple glass tube with a cock upon it. The fuel which I used was coke.



This apparatus, considering its diminutive size, proved exceedingly effective. When the rate of evaporation was about a gallon in an hour, and the pressure in the boiler 100 lbs. on the square inch, I could charge a pint coated phial of very thin glass in a couple of minutes, sufficiently to give a rather smart shock. I found the best method of charging the phial was simply to connect its knob with the boiler or the insulated stove which contained it, for I could always collect electricity in much greater abundance from the evaporating vessel than from the issuing steam.

I confidently anticipated, that, in accordance with all former experiments on the subject, the electricity of the evaporating vessel would invariably be negative, and that of the effluent steam uniformly positive; but I soon discovered, to my great surprise, that under certain conditions, these electrical states of the boiler and steam-cloud were *reversed*.

Lest it should be suspected that my assertion of this extraordinary fact is founded upon erroneous observation, I feel it necessary to state, that I was most unwilling to believe that ejected steam could, under any circumstances, evolve *negative* electricity, inasmuch as its doing so was diametrically opposed to the theory which in a former paper I ventured to advance in explanation of the electrical phænomena of effluent steam, and that I was only convinced of the reality of the change, after making repeated trials and applying a variety of tests, which gave the same result in every instance.

I made numerous experiments to ascertain what the conditions were under which the transmutation took place, and am led to believe, from the following circumstances, that an overheated state of that part of the boiler which contains the steam, is one, at least, of the causes which induce the effect. It will be readily perceived, that by closing the door of the stove a considerable increase of temperature would be occasioned in the upper part of the boiler where the steam was lodged, and I found that in almost every instance in which this was done, the *negative* electricity of the boiler, and the *positive* electricity of the steam, began to decline, and continued so to do until they vanished entirely, and then *positive* electricity gradually appeared in the boiler, and *negative* electricity in the emitted steam. A diminished state of the water in the boiler would also, of course, favour the accumulation of heat in the higher region of the boiler, and such a state of the water proved to be likewise conducive to the conversion of the electricities. I have reason, however, to think that there are other causes by which a change is effected in the electrical states of the boiler and steam-cloud; but before any definite opinion can be formed on this point, I consider further experiments to be necessary.

I shall now speak of the effects of pressure, first, when the steam-cloud evolves *positive* electricity, and secondly, when it liberates *negative* electricity.

In experiments to ascertain the comparative intensity of the electricity evolved by steam at different pressures, it is of course essential that equal *weights* of steam, and not equal *volumes*, should be emitted in equal times; and to effect this condition it is necessary that the discharging aperture should be diminished in the same proportion that the pressure is increased, and *vice versâ*. The safety-valve is in this respect a self-regulating apparatus, because it permits an escape of steam precisely equal to the production, which, with a uniform fire, is the same at all pressures. Another requisite in such experiments is, that the steam should issue in states of equal *dryness*. If a strong fire be used, the discharge at low pressures is so *voluminous*, that it is exceedingly apt to sweep unvaporized water out of the boiler, and thereby to dissipate the electricity, or conduct it to the valve.

By availing myself, then, of the safety-valve as a means of equalizing the escape, and by maintaining the fire at once uniform and moderate, I was enabled to effectuate equal emissions of dry steam, under successive augmentations of pressure, and the following are the results I obtained, so far as they apply to the usual condition of the steam-cloud, or that in which it evolves positive electricity.

In the first place I removed the valve from its seat, and suffered the steam to escape without any restraint whatever;

but under these circumstances I could detect no electrical development beyond what was attributable to the combustion of the fuel, which always produced sufficient electricity in the evaporating apparatus to affect the gold leaves of the electroscope, when a condensing plate was used. I then restored the valve, and when the pressure scarcely exceeded one pound on the square inch, the gold-leaf electroscope, connected with the boiler, gave the first indication of electricity distinguishable from that produced by combustion. The slightest addition to this pressure caused an excessive increase of electricity, and at three pounds on the square inch, small sparks could be drawn from the boiler at the rate of five or six in a minute. After this each successive equal increase of pressure produced a less augmentation of electricity than the preceding one. I should say that the electricity manifested at three pounds on the square inch, was not doubled until the pressure became fifteen pounds per square inch ; not trebled until it reached fifty pounds; not quadrupled until it was raised to 120 pounds; and that when the pressure was augmented to 250 pounds on the inch, which was as high as I ventured to carry it, the electricity of the boiler did not appear to be more than five times greater than at a pressure of only three pounds on the inch. Such were the results I obtained; but as the causes which tend to produce the opposite electrical states of the boiler and steam-cloud were probably not altogether quiescent in these experiments, the effects might possibly have been different, if the apparatus had been so constructed as to be more or less favourable to the operation of such causes. The feebleness of the electricity evolved by the steam discharged from Perkins's gun*, is probably owing to a counteracting agency of the nature I have mentioned; and the variation which has been observed in the intensity of the electrical effects, produced by the steam discharged at similar pressures from different steam-engine boilers, may be explained upon the same principle.

When circumstances were such as to render the electrical states of the boiler and steam-cloud the reverse of what they usually are, the effects of pressure were exceedingly incongruous and perplexing.

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* I presume it has been tried whether the steam from Perkins's gun is positive or negative ; but if not, I recommend the trial to be made, and I think it possible that the electricity may prove to be *negative*; especially if the pressure be run down a little and the steam not permitted to escape very rapidly. The condensation which must take place in the gun-barrel will be unfavourable to a development of electricity, and the steam ought, therefore, to be discharged, if practicable, by some other means.

The lowest pressure at which I could detect positive electricity in the boiler, was much the same as that at which negative electricity first appeared; and an increase of pressure, as far as about thirty pounds on the inch, augmented the intensity of the development in the same ratio, apparently, as when the boiler was negative; but further additions to the load on the valve were attended with very variable results, sometimes increasing, and sometimes diminishing the electricity. The most remarkable circumstance, however, was that a considerable increase of pressure always produced in the first instance a temporary recurrence to *negative* electricity in the boiler, which lasted for a minute or two, and then again gave place to *positive* electricity. I suspect that this curious effect is occasioned by the steam and water acquiring an increased temperature more rapidly than the metal of the upper part of the boiler, so that when the boiling point corresponding to the load on the valve is again attained, the metal in contact with the steam is not at first hot enough to reverse the natural electrical state of the boiler. If this view of the subject be correct, it follows that the degree of heat which is necessary in the metal to render the boiler *positively* electrified, has reference to the boiling point of the water, and the temperature of the steam.

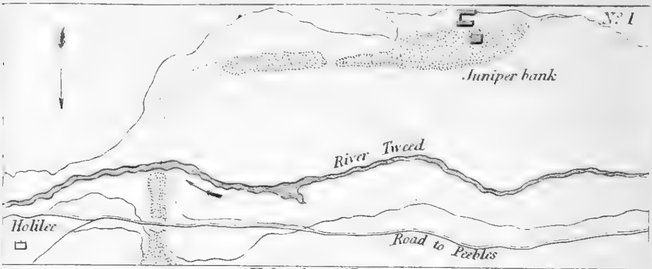
When the pressure was considerable, the tendency to negative electricity in the steam-cloud was greatly increased by passing the steam through the copper pipe acted upon by the fire; so much so indeed, that I could frequently obtain a jet of steam *negatively* electrified from the pipe, at the same time that a jet of steam *positively* electrified was escaping from the valve. At low pressures, however, the effect of passing the steam through the pipe was rather the reverse of what I have mentioned, and I therefore scarcely know whether to regard the result as favourable or adverse to the agency which I have ascribed to heated metal.

By much the most powerful effects were obtained when the steam was discharged through a short glass tube inserted in the boiler. When the tube was long a considerable condensation took place within it, which greatly diminished the electricity of the jet. The electricity was seldom so strong when the boiler was positive, and the steam-cloud negative, as when they were in the opposite and usual states.

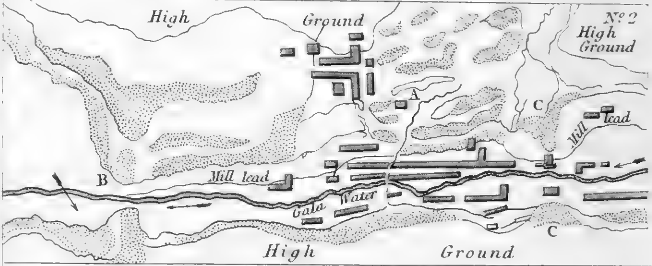
The neutrality of the steam in the boiler, whether the steam-cloud evolved positive or negative electricity, was clearly evinced by the electricity of the jet not being abated by passing the steam through the copper pipe, which was ten feet long, and only three-eighths of an inch in diameter.

The superior intensity of the electrical development which accompanies the emission of high-pressure steam, and the fact of the upper or distended portion of the jet being more highly electrified than the lower or unexpanded part, seem to favour the supposition that the evolution of electricity in the steam-cloud depends upon the *dilatation* of the steam. I have already adduced, in former communications, reasons for rejecting this hypothesis; but feeling anxious to remove all doubt upon the subject, I resorted to the following method of determining the question. To the cock F, which terminates the copper pipe, I attached a metallic cylinder, which was kept sufficiently hot to prevent any condensation of steam taking place within it, and was perforated at one end with a multitude of small holes, through which the steam, after expanding in the cylinder, was suffered to escape at a density scarcely exceeding that of the atmosphere. Now if *expansion* had produced the development of electricity, the steam would have become electrical in the cylinder, and would have parted with its electricity in passing through the holes, and thus the steam-cloud would have been rendered neutral; but so far from that being the case, I could perceive no diminution of electricity when the steam was subjected to this treatment. We may therefore confidently infer that the liberation of electricity in the jet does not proceed from expansion, and that although the intensity of the electrical development is so greatly affected by pressure in the boiler, it is quite independent of the density at which the steam is ejected.

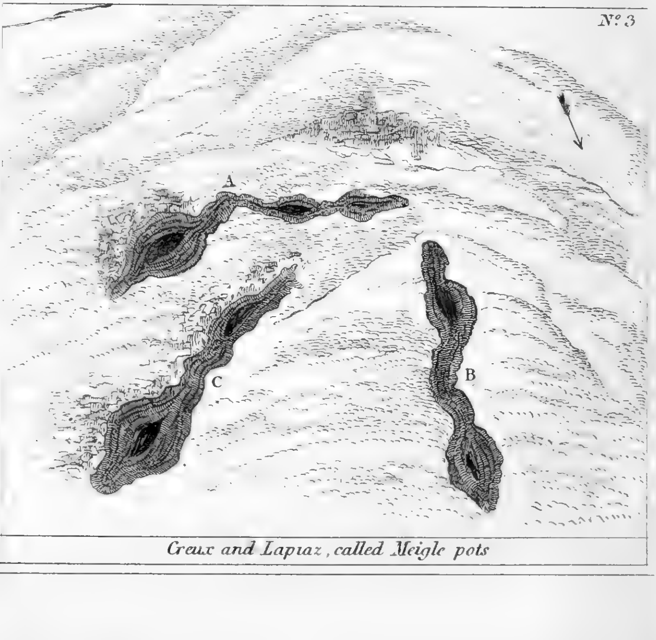
The precipitation of the steam appears then to be the only cause to which the liberation of electricity in the jet can be assigned. Assuming this conclusion to be correct, the next question is, does the precipitation of the steam give rise to the electricity of the evaporating vessel, as well as to that of the steam-cloud; or is the electricity manifested in the boiler, the effect of evaporation, or of some other process distinct from that which excites electricity in the jet? The neutrality of the boiler when the steam is confined, seems to imply, that the same cause which produces electricity in the cloud occasions also the opposite electricity in the evaporating vessel. I endeavoured to resolve the question so far as regards the agency of evaporation, by discharging the steam which from time to time remained in the boiler, after all the water had been evaporated, and the boiler always became electrified, provided the pressure of the steam were considerable. The cessation of escape from the valve was the only indication I could have of the perfect dryness of the boiler; and if reliance may be placed upon this criterion, the conclusion I



Vale above Holilee



Town and Vale of Galashiels



Creux and Lapiaz, called Meigle pots

think is inevitable, that the electricity which is excited in the evaporating vessel on the emission of steam, is independent of concomitant evaporation. I should observe, that the electricity which appeared in the boiler after, as well as shortly before, the exhaustion of the water, was uniformly *positive*.

Under these circumstances, I think there are certainly grounds for supposing that the same cause which excites electricity in the steam-cloud, produces also the contrary electricity of the boiler; but if this be so, I am not aware that there is any known principle upon which the effects can be explained, especially as the electricity of the boiler appears quite independent of the proximity of the jet.

The transmutation which, under certain conditions, takes place in the electrical states of the boiler and steam-cloud, is a part of the subject embarrassed with difficulty. What possible change can the steam undergo in the boiler, either by contact with heated metal, or otherwise, which can cause it to evolve, on its subsequent condensation, the opposite electricity to that which it usually liberates?

The effects of pressure appear equally inexplicable. It seems inconceivable to me that steam should acquire by pressure any property which would not be taken away by expansion, and yet we find that the influence of pressure in the boiler, upon the electricity of the jet, is not destroyed by suffering the steam to dilate before it is ejected.

I fear this paper has extended to a somewhat tedious length, and I shall therefore conclude it here by expressing a hope that what has been stated may have the effect of stimulating inquiry into the curious subjects of which I have treated.

WM. GEO. ARMSTRONG.

LVI. *On the supposed Moraines of Ancient Glaciers in Scotland. By* WILLIAM KEMP, *Esq., introduced by a Letter from* J. E. BOWMAN, *Esq., F.L.S.*

[Illustrated by Plate III.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

MR. KEMP, of Galashiels, having favoured me with a copy of a paper lately drawn up by himself, containing new proofs of the former existence of glaciers in Scotland, I thought it desirable that they should be generally known, because I am aware that some of my ablest geological friends are waiting for further evidence (and wisely so) before they adopt the theory of Prof. Agassiz. I therefore wrote to request Mr. Kemp to publish this paper, and illustrate it by diagrams. In his reply he modestly states that he had no design of doing this, "being, as you will at once perceive, no fit person for preparing papers for the press;" but he grants me full permission to do so, if I think it worthy, and has inclosed the accompanying diagrams, respecting which he says, "These very rude and hasty sketches of those gullied tracts called the Meigle Pots, together with the vale above Holilee, are pretty correct, being copied from sketches taken on the spot. But that of the vale of Galashiels is not so faithful, I having only examined it in detail; that is, the mounds round Gala House are not at all drawn to the truth, I having as yet got no leisure to take a plan, nor can I for many weeks to come."

Mr. Kemp's account of the "Meigle Pots" is highly interesting and important; and being, as far as I know, the only instance as yet observed in Britain of this modification of glacier action, it appears to me that I should have been remiss had I not suggested its publication. To make it more intelligible, I have given in a note an extract from a short but able account of the *Creux* and *Lapiaz* of Prof. Agassiz, by the talented editor of the 'Scotsman,' Mr. C. Maclaren. I will only add, that some of the extensive mounds of gravel described in the paper may possibly have been modified, if not originally produced, by the subsequent action of torrents.

J. E. Bowman.

Manchester, 10th Feb., 1841.

On the Moraines of Ancient Glaciers, &c. By WM. KEMP. *Read to the Galashiels Geological Society, Jan. 29th,* 1841.

GENTLEMEN,

HAVING had my attention turned to this interesting subject, first by Mr. Bowman's opinion that the terraces in this neighbourhood, which I attributed to the action of water, are in fact the moraines of ancient glaciers, besides having read in the Athenæum and other periodicals of Prof. Agassiz's discovery of similar evidences in different parts of Scotland, &c., I, among many others, hailed the announcement with great pleasure, as conviction at once flashed upon my mind, that his discovery, by the clear description given of those ancient moraines, would at once apply to those beautiful combs or mounds of gravel that have been so frequently noticed in many parts of the country, of which no satisfactory account had hitherto ever been given. How gratifying to the ardent followers of that science, is the discovery of that great man, in pointing to, and giving such a clear exposition of, these hitherto mysterious formations!

Such moraines as were probably formed by ancient glaciers are very conspicuous in the vale of the Gala, immediately around the town of Galashiels. The vale there will be at one part about three quarters of a mile broad, by upwards of three miles long, surrounded by hills of considerable elevation, except where the Gala enters the vale by a narrow passage at the north-west, and again where it opens to the south-east, and joins the more spacious vale of the Tweed. The glaciers there appear at one time to have extended over the vale to a great height, as lateral mounds of debris can be traced about 300 feet above the level of the water. But I wish to confine my observations to what was possibly a later æra, when the glaciers had much diminished, and were perhaps not above 150 feet high; for it is then that they seem to have left their most legible traces.

Nearly half a mile below Galashiels, where the Gala flows through a narrow channel, with high banks on each side, there is a very conspicuous moraine about 140 feet high upon the north-east side, and about 600 feet long, extending across the lower end of the vale. (See No. 2. B.* Pl. III.) Upon the south-west side, in a direct line, it can likewise be traced for a considerable distance, where it takes a curvilinear bend to the west, until it becomes imperceptible in the higher ground. The greater part of that moraine is composed of clay and boulders, many of which are quite sharp and angular, but the greater portion are rather well-rounded; and what, perhaps, is worthy of notice, the top, for about 25 feet down, is composed of unstratified gravel and coarse sand, which has seemingly been caused by the water filtrating through the mass while it was being thrown up, and sweeping away the finer comminuted particles. The breach through which the water flows is about 500 feet wide. This is, perhaps, what may be called a terminal moraine, left on its final retreat by the glacier of the Gala valley.

At the west end of Galashiels, rather more than a mile upward from that moraine, the remains of another may be seen, standing out from the banks on each side, the south end in particular being very conspicuous. (See No. 2. C C.) Likewise, along each side of the vale, there are lateral moraines from 50 to 70 feet high, in some places very distinctly marked.

A number of these interesting formations is to be seen near

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* The dotted portions indicate the moraines.

Gala house (see No. 2. A.), in a fine sheltered situation, where the vale extends to the south for a considerable distance, at a lateral opening between the hills.

Nearly parallel with the Gala there is a lateral moraine, which divides the town from Gala house and parks, and behind that there is a series of beautifully formed mounds, ranging nearly parallel one with another. These are not continuous, although some parts are of considerable length: one, called the long knoll, being about 700 feet long, of a serpentine form, averaging 35 feet high; others are like small hillocks, from 20 to 40 feet high, with narrow trough-like valleys between. To use a simile, I thought, while examining these interesting relics, that to convey the clearest conception of that place, would be to imagine portions of the rolling waves of a troubled sea to be instantly arrested in their course, while the intervening parts passed onward for a little space, and were then likewise immovably fixed.

Such seems to have been the action of glaciers in the vale of Galashiels, as to have scooped out the basin in some places to an unknown depth, which had afterwards been filled up by impalpable sedimentary sand, over which there is a covering of gravel varying from four to six feet deep, so that a great part of the town may literally be said to be built upon a quicksand*.

There are many lateral moraines along the valley of the Tweed; but the only remarkable terminal ones I have yet observed, are two, one at Dryburgh, the other at Holilee. The first is upon the Dryburgh side of the river, and memorable enough by a fine Doric temple being built upon its summit by the late Earl of Buchan. The one at Holilee (see No. 1.) has attracted the notice of many a passing traveller, by its singular uniformity and beauty; so much so, that the most skilful engineer could not construct a finer mound. It crosses the lower end of an extensive vale, immediately below which the hills on each side press close upon the river. The northern part of this mound is about forty feet high, and is composed of rolled gravel. The road to Peebles runs through it. It has swept across and along the opposite side of the vale for about a mile long, but the river has long since carried away a great part of it. A few years ago a splendid portion of it rose out of the vale to the height of sixty feet; but the farmer, unmindful of this fine geological monument of ancient time, after long cursing it for a barren and unpro-

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* This sand below the gravel is probably of anterior origin.—J. E. B.

ductive track, at considerable expense caused it to be much reduced in height, and now the plough (still with difficulty) passes annually over it*.

I have likewise observed several remains of moraines along the vale of the Ettrick. About three quarters of a mile above its junction with the Tweed, at the south end of the fine new bridge upon the road to Selkirk, the approach to the bridge is cut through a very bold one. It sweeps round the lower end of the rich haugh of Lindean, and forward to the side of the water, where it turns obliquely to the stream. About half a mile further up the river at Bridge-heugh, there is another upon the same side of a still bolder character, forming an immense bank about sixty feet high, crossing the vale about two-thirds of its breadth, which is there about half a mile broad, it having evidently once crossed it to the opposite side, as a part in a direct line is still remaining. Following up the water, we observe vast lateral moraines on each side. Upon the north there are three curvilinear ranges of hillocks, some 20, 40, and 50 feet high; these join towards the west, in a cross mound, terminating in an abrupt precipitous angular ridge at the side of the water, which at present undermines its broken end. This is fully sixty feet high, and, like the one at Bridge-heugh, seems to have crossed the vale, by the evidence of a remaining portion boldly projecting out of the south bank, a little below the extensive factory of the Messrs. Browns. Gentlemen, these are a few of the leading proofs by which I have attempted to prove the former existence of glaciers along the valleys of this district. You are all more or less familiar with those remarkable mounds I have pointed out; and if you turn to M. Agassiz's description of moraines, you will find that these are similar in every respect.

As a further proof, I have observed the striated and grooved appearance of the rocks in many places in this neighbourhood, particularly at a hill nigh Redhead, by the vale of the Caddon, and even in our own town, at the back of the Buckholmside factory.

At a land-strait or water-shed, between the Gala and the Tweed, about two miles and a half west from Galashiels, on the north side, upon a steep spur of the Meigle Hill, which here rises at an angle of near thirty degrees, there are three lines of round hollows with gutter-like tracks between. (See No. 3.) They are known by the name of the "Meigle Pots,"

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* This seems to be a beautiful example of a terminal moraine united to a lateral one, very similar to that of the Viesch, described by Agassiz, and represented in the 9th Plate of his *Etudes sur les Glaciers de la Suisse.* —J. E. B.

and are each nearly 200 feet long, not in straight lines, but of a serpentine form. The highest (No. 3. A.) is about 200 feet above the vale, running *nearly horizontally across* the steep spur of the hill, and has three circular hollows in its course, two about fifteen feet deep, and the third about thirty-five, the tracks between being from 10 to 12 feet deep. The next (No. 3. B.) commences close below the west end of that horizontal one, bending to the right and left down hill, and is more uniform in depth than either of the other two. It contains two deep and finely rounded oval holes, one about thirty, the other forty feet deep. The third (No. 3. C.) begins near the middle of the upper one, and continues down hill in a slanting direction towards the east: it has the most rugged appearance of the three, the rock being bare and precipitous upon the higher side, and terminating at the lower end with a circular cavity about fifty feet deep. The rough markings on the more precipitous or eastern sides of the hollows *a* and *c* represent the bare rock partially covered with soil and vegetation; but none such occur on the side of B, which runs down hill with the ground nearly level on each side, covered with a fine grassy surface.

The sides of these gullies are very steep, although mostly covered with a fine grassy surface. It is evident they had been originally much deeper, having been greatly filled up by debris, which from time to time has fallen from the heights above*. By what means can we conceive these hollows to have been scooped out of the rock, except by the agency of water, and by water falling from a considerable height? The rock seems favourable for being worn down in that manner, the graywacke strata rising near to the perpendicular, and the beds of the rock being thin, intersected with numerous cutters (joints). Yet where is the Water? None runs there now, nor can ever have run there in the present state of that locality. It seems to me likewise that these hitherto unaccountable ravines have been clearly explained by M. Agassiz in his description of the *Creux* and *Lapiaz*, names given to similar holes and guttered tracks which he had observed upon the flanks of the Swiss Alps, and which, he says, have been worn out by water pouring through fissures in the glaciers while they were slowly pressing onward†.

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* The Meigle Hill rises about 450 feet above the rude pillar represented in the sketch No. 3. This pillar stands about 60 feet above the upper line of hollows marked *a*, and is built on the lowest *terrace* I can trace in that quarter, which is No. 10 of the series described in Chambers's Journal; but others are more distinctly marked upwards to the south-west.

† On the sides of the Swiss valleys, round holes, such as cascades make,

Gentlemen, however hastily surveyed and imperfectly described, this is still another proof in confirmation of the glacier theory in Scotland, so lately promulgated by the great philosopher of Neufchatel, to whom the geological world is so deeply indebted, by revealing so bright a page in the dark history of the past. W. KEMP.

LVII. *An Examination of the Phænomena of Conical Refraction in Biaxal Crystals.* *By* R. POTTER, *Esq., B.A.**

THE verification of the two species of conical refraction, which were discovered by Sir William Hamilton to be results of Fresnel's analytical expression for the wave surface in biaxal crystals, forms an epoch in the progress of the conversion of the scientific world to a belief of the undulatory theory of light. Many waverers were confirmed in their belief by so singular a coincidence of theory and experiment; and Professor Lloyd, who made the experiments, had a harvest of reputation from them, such as is seldom reaped in the field of science.

I joined with the scientific world, in the confidence which it gave to Professor Lloyd's investigations; and although I had seen the undulatory theory fail in so many important cases, yet I believed that the true law of refraction in biaxal crystals had been discovered by Fresnel, as certainly as that of uniaxal crystals has been by Huyghens; but in both cases from wrong premises. It was from a desire to view these interesting phænomena, that I availed myself of an opportunity

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are sometimes found in the rock, but in places remote from running waters, and where the form of the surface will not permit us to suppose that any cascade could ever have existed. In other cases, a long, sinuous, dry, water-worn gutter or channel is observed, the course of which runs *across* instead of along the natural declivity of the ground. The study of the glaciers has enabled Agassiz to find a key to these enigmatical phænomena, which had perplexed previous inquirers. Streams of water flow along the surface of a glacier, and when one of these falls into a fissure which is open to the bottom, if it forms a cascade, it cuts a round cavity in the rock with the gravel and sand which it either finds there or carries down with it, as some of our rivulets work out the hollows termed caldrons. If the glacier is travelling downwards the cascade will travel with it, and convert the round cavity into a long gutter; or supposing the water to reach the bottom without falling in a cascade, still, in finding an issue below the glacier, it will be compelled to follow the sinuous openings left by inequalities in the bottom of the ice, and thus take a course at variance with the natural inclination of the surface. We have here an explanation of the *Creux* or holes, and the long water-worn gutters found in such unlikely situations, which bear the local names of *Lapiaz* or *Karren.* These are chiefly observed where the rock is soft, and are seldom visible on the granite.—*Scotsman.*

* Communicated by the Author.

of purchasing some crystals of arragonite, in order to have pieces of larger dimensions than I possessed before, and which from perusing Professor Lloyd's paper, in the seventeenth volume of the Transactions of the Royal Irish Academy*, I conceived to be necessary. The prepossession I had conceived in favour of Fresnel's law, as expressed in the equation to the wave surface in such crystals, was, however, changed into distrust, when I was enabled to observe the phænomena. I employed a different mode of experimenting from Professor Lloyd, on commencing; and possessed more confidence in the properties of an eye-lens. Such a lens exhibits to the sight what takes place at the focus of the eye and the lens, as an optical combination, when there is a real image; and when there is a virtual image, it is exhibited such as would have really arisen if the rays of light in falling upon the eye-lens had proceeded in a similar manner from a real image. The transition from a virtual to a real image, and *vice versâ*, occasions no difficulty; for both arise from the form of the pencil of rays, as it falls on the eye-lens; which is, in fact, the thing concerned in the representation of the image to the sense of vision.

Professor Lloyd passed a pencil of sun-light through the crystal, and received the transmitted pencil on a screen of ground glass, in most of his experiments, and only mentions using a lens whilst seeking for the conical refraction within the crystal which gives a cylindrical refraction in air; and then soon gave it up again for sun-light. I have chiefly worked with an eye-lens, or else for micrometer measures with a compound microscope; and having tried sun-light also, I concluded that it bears in accuracy some such relation to the former method as a camera-obscura bears to a telescope, and yields a less perfect result; but one which, as far as it goes, is exactly the same as that from the other method. With the screen you can only examine real images; with the lens you can examine virtual ones also, and that in a more critical manner.

Arragonite having been judiciously chosen by Professor Lloyd, on account of the considerable angular separation of its optic axes and its double refractive energy, as well as on account of the refractive indices, in the principal directions of the crystal, having been determined by Professor Rudberg, I have continued to use it solely. I have employed slices of the mineral cut perpendicularly to the axis of the crystal, which is parallel to the line bisecting any pair of optic axes, and of the thickness one-seventh, one half, and five-sixths of an inch.

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* [Prof. Lloyd also described these phænomena in L. and E. Phil. Mag., vol. ii. pp. 112, 207.—Edit.]

The one of one-seventh of an inch thickness does not give sufficient separation of the images for convenient use, and the one of five-sixths of an inch had its inconveniences, arising from the fact of crystals of arragonite being formed of twin crystals; and it is seldom that any component crystal is large enough to allow a ray of light to traverse the optic axis for any great distance without entering the twin one. This was only in some places possible with the specimen referred to, so that the experiments about to be related were generally made with the crystal of half an inch thickness.

The experiment with sun-light was made as follows: a piece of tinfoil with a small hole in it, made with the point of a pin, being attached to one surface of the crystal, and the whole then attached by means of wax to a moveable adjusting apparatus fixed to a stand, I placed the crystal in the sun-light reflected horizontally through a window by a mirror, with the tinfoil towards the incident light, so that the pencil entering the small hole would be in angular diameter 32′ nearly, or the sun's apparent diameter; then placing a piece of tissue-paper on the other surface, I saw generally upon it two round bright spots; but when the crystal was moved until the light entering it traversed the optic axis, or rather was symmetrically refracted with regard to that axis, then there appeared one round bright spot, with a dark ring, and then a broader bright ring round it: now drawing the tissue-paper away from the surface gradually, but keeping it parallel to it, the darker ring gradually disappeared, and the section of the pencil became larger, the central spot becoming more and more faint, and expanding into a conical pencil which blended with the other conical pencil arising from the bright ring. These phænomena show that within the crystal the light had been refracted in the form of a small solid cone, approximating to a cylinder, whose section was the bright spot seen at the second surface, and a larger hollow cone whose section was the bright ring. After emergence into air we see that the bright ring expanded into a hollow cone; but the bright spot, gradually disappearing, gave rise to an indefinite number of conical surfaces of various angles.

These and other properties of the emergent pencil are much better examined by means of the eye-lens; and first, placing the crystal in ordinary day-light coming through a small round aperture at some distance, with the second surface in the focus of the eye-lens, we perceive the same appearances as were seen on the tissue-paper at the second surface of the crystal. Using now a triangular aperture, the two spots appeared triangular images of the aperture, and at the optic axis the ring and spot possessed some trace of this triangular relation. The triangular appearance arose from the well-known optical property of a small hole, which gives on a screen beyond it an imperfect image of a bright object in front of it. In the experiment with the sun-light, although the central spot was round, yet it cannot be considered an image of the sun, in the same way as the two round spots generally seen.

The change from two spots to a ring and a spot was sudden, upon the direction of the light becoming that requisite for refraction symmetrical to the optic axis; and in experiments which will be related shortly, when a minute pencil of incident light was transmitted through a very minute aperture in the tinfoil, it required very nice adjustment to obtain the ring and spot. This shows that if a converging pencil were incident at the hole on the first surface, with its axis in the direction requisite for refraction symmetrical to the optic axis of the crystal, then no other ray but that in the axis of the pencil could be subject to the singular refraction in the crystal: but the other rays of the incident pencil giving each two rays within the crystal, these latter would be arranged around the optic axis, as the incident rays were around the axis of the pencil, and the appearances would be symmetrical; although introducing, in proportion to the magnitude of the pencil, confusion and indistinctness into the real phænomena of conical refraction. Professor Lloyd says, "The first-mentioned species of conical refraction, it has been observed, takes place *in air*, when a ray of common light is transmitted within the crystal in the direction of the line joining two opposite cusps of the wave. If we suppose such a ray to pass in both directions out of the crystal, it is evident that it must emerge similarly at both surfaces; consequently, the rays which are transmitted along this line within the crystal, and form a diverging cone at emergence at the second surface, must be incident in a converging cone at the first. Having, therefore, nearly ascertained the required direction by means of the system of rings in polarized light, I placed a lens of short focus at its focal distance from the first surface, and in such a position that the central part of the pencil might have an incidence nearly corresponding to the cusp-ray within," &c. And again, he says, "But to examine the emergent cone, it was necessary to exclude the light which passed through the crystal in all but one direction. For this purpose a plate of thin metal, having a minute aperture, was placed on the surface of the crystal next the eye, and the position of the aperture so adjusted, that the line connecting it with the luminous point on the first surface might be, as nearly as possible, in the direction of the cusp-ray. The exact adjustment to this direction was made by subsequent trial." After relating some preliminary experiments, he says again, " In all these experiments the emergent rays were received directly by the eye placed close to the aperture on the second surface. It was obviously desirable, however, to receive them on a screen, and thus to observe the section of the cone at different distances from its summit. After some trials, I effected this with the sun's light, the light of a lamp being too weak for the purpose. The emergent cone being made to fall on a screen of roughened glass, I was enabled to observe its sections at various distances, and therefore with all the advantages of enlargement. The light was sufficiently bright, and the appearance distinct, when the diameter of the section was between one and two inches."

This arrangement appears to have been continued throughout this series of experiments; for after discussing his results, he says, "Inasmuch as the cusp-ray, within the crystal, corresponds to a cone of rays without, it is evident that there must be a *converging* cone incident on the first surface, equal to that which diverges from the second."

We see in the preceding discussions, that Professor Lloyd, by experimenting with the view of verifying theoretical consequences, has fallen into two errors; the one an error of assumption, that a conical incident pencil of common light could be refracted as one ray within the crystal; and the other an error of omission, in not discovering that, however small the incident pencil may be, the refracted light consists of a larger cone and a smaller one, nearly a cylinder. The first error arose from applying the reasoning of geometrical optics, for uncrystallized media and common light, to the case of a double refracting medium and polarized light; for Professor Lloyd himself had discovered a peculiar state of polarization in the emergent pencil (he says, " Thus it appeared that all the rays of the cone are polarized in different planes"), and to have given the least chance of success the incident pencil ought to have been in the same state.

To investigate the form of the refracted light in a more severe manner, I placed an aperture made in a plate of metal, with a small pin, about one-fiftieth of an inch diameter, at seven inches from the flame of a lamp; and at sixteen inches on the other side of the aperture I placed the crystal with tinfoil attached to its first surface, and examining the light which passed through a minute hole, about one-thousandth of an inch in diameter, in the direction for refraction symmetrical to the optic axis, I found it on emergence giving the ring and central spot. In this experiment the angle of the incident cone was rather more than 4′.

In this experiment the light was reduced to a small pencil by apertures in thin metal plates, but I have seen the same phænomena when the incident pencil arose from a luminous point formed by a lens of short focus, at some distance from the crystal. The results of Professor Lloyd's method of experimenting were, that in the first instance he found the angle of the emergent cone about double what the calculation from theory gave, but by reducing the diameter of the aperture on the second surface this observed angle was reduced also; until with a very minute aperture an approximation to the calculated angle was obtained; and a hollow cone was seen, as required, in place of a solid cone, which appeared in the first instance.

It appears, that in order to obtain any tolerable approximation to the calculated quantities, an aperture was required to be applied to the second surface, so as to cut off all the ring and a large portion of the central spot. Such experiments cannot be considered as investigations of the refraction of biaxal crystals in the neighbourhood of the optic axes. The real nature of this refraction will be seen when we have examined Professor Lloyd's investigation in search of the conical refraction within the crystal giving a cylinder of rays in air.

He says, "The second kind of *conical refraction*, whose existence has been anticipated by Professor Hamilton, depends (it will be remembered) on the mathematical fact, that the wave-surface is touched in an infinite number of points, constituting a small *circle of contact*, by a single plane parallel to one of the circular sections of the surface of elasticity. It takes place when a *single external ray* falls upon a biaxal crystal in such a manner that one refracted ray may coincide with an optic axis. When this is the case, there will be a *cone of rays* within the crystal, determined by lines connecting the centre of the wave with the points of the periphery of the circle of contact. The angle of this cone is equal to

$$\tan^{-1}\frac{\sqrt{a^2-b^2}\,.\,\sqrt{b^2-c^2}}{b^2}$$

and its numerical value in the case of arragonite is 1° 55′, assuming the values of the three indices as determined for the ray E by Professor Rudberg (see Trans. R.I.A., vol. xvii. page 151.).

"As the rays constituting this cone will be refracted at emergence in a direction parallel to the incident ray, they will form a small cylinder of rays in air. This cylinder, it will be seen, is in all cases extremely small; for the diameter of its section made by the surface of emergence subtends an angle of 1° 55′ only, at a distance equal to the thickness of the crystal. Hence the experiments required to detect its existence and measure its magnitude, demand more care and precision than those already described.

" The incident light was that of a lamp placed at some distance; and in order to reduce as much as possible the breadth of the incident beam, it was constrained to pass through two small apertures, the first of which was in a screen placed near the flame, and the second perforated in a thin metallic plate adjoining to the first surface of the crystal. Under ordinary circumstances, it is obvious the incident ray will be divided into two within the crystal, and these will emerge parallel from the second surface. I was able to distinguish these two rays by the aid of a lens; and turning the crystal slowly, so as to vary the incidence gradually, I at length observed that there was a position in which the two rays changed their relative places rapidly on any slight change of incidence, and appeared at times to revolve round one another, as the incidence was altered. Being convinced that the ray was now near the critical incidence, I changed the position of the crystal, with respect to the incident ray, very slowly; and after much care in the adjustment, I at last saw the two rays spread into a continuous circle, whose diameter was apparently equal to their former interval.

" This phænomenon was exceedingly striking. It looked like a small ring of gold viewed upon a dark ground; and the sudden and almost magical change of the appearance from two luminous points to a perfect luminous ring, contributed not a little to enhance the interest.

" The emergent light, in this experiment, being too faint to be reflected from a screen, I repeated the experiment with the sun's light, and received the emergent cylinder upon a small piece of silver paper. I could detect no sensible difference in the magnitude of the circular sections at different distances from the crystal.

" When the adjustment was perfect, the light of the entire annulus was white, and of equal intensity throughout. But when there was a very slight deviation from the exact position, two opposite quadrants of the circle appeared more faint than the other two, and the two pairs were of complementary colours. The light of the circle was polarized, according to the law which I had before observed in the other case of conical refraction. In this instance, however, the law was anticipated from theory by Professor Hamilton."

With the exception of the last sentence but one, this description gives a correct and clear account of the appearances, but under a condition which Professor Lloyd has omitted to notice. The luminous ring is seen, and seen perfectly only, when the lens is so placed in its distance from the crystal, that what he calls the two rays, are, in fact, the two virtual images of the luminous point on the first surface. The position of these virtual images within the crystal is found by the formulæ of geometrical optics, their distance from the second surface, when the incidence is nearly perpendicular, being equal to $\frac{\text{thickness of the plate}}{\text{refractive index}}$.

When the eye-lens has any other distance than that which gives the distinct images of the aperture, we find the appearances changed, and that the bright ring is not the section of a cylinder of equal diameter at all distances from the crystal, as Professor Lloyd supposes he has made out, by means of sunlight. That he should have overlooked phænomena so interesting, can be accounted for only by his devotion to his theory, which has thus robbed him of a fine discovery.

Whether we draw the eye-lens away from the crystal, or push it nearer, we find similar appearances, and we see that the ring was formed by the intersection of two cones (or more correctly of two series of cones), the one converging so as to have the bright spot, we formerly found on the second surface, for its vertex, and the other diverging, and having the ring, formerly found on the second surface, for its base. I say more correctly two series of cones, for we formerly found that the bright spot only gradually became fainter. The same appearances are seen in pushing the lens nearer the crystal, for gradually one portion of the light of the ring increases in diameter, whilst the other diminishes, and a bright centre is formed, which comes to its smallest magnitude, and then diverges again into a solid cone of rays, having a bright centre which gradually fades away. The phænomena show that the cones, the one virtual as being beyond the second surface of the crystal, measured from the eye, and the other real as being formed in the emergent light, are in reality a series of cones having their vertices on opposite sides of, and at different distances from, the ring. When the rays of either series of cones are collected into their smallest space forming the bright spot, this is evidently only an approximate effect; and when the incident pencil is very small, this spot is seen evidently outside the crystal, rather than on the second surface for one series.

These results are certainly not in accordance with the theoretical investigations of Sir William Hamilton, whose discussions of the singular refractions near the optic axes, as indicated by Fresnel's expression for the wave-surface, have been referred to in the quotations from Professor Lloyd's paper.

The law of the polarization in the virtual ring and one of the cones, as far as he investigated it, were observed by Professor Lloyd; but if my view of the mode of formation of the ring by the intersection of two series of conical surfaces be correct, then there must be a peculiarity in the polarization hitherto unsuspected. The ring, as stated by Professor Lloyd, is polarized in every part in a different plane; so that if we attach a prism of calcareous spar, giving light polarized in one plane, to our eye-lens, we see that one small portion of the ring vanishes in every position of the prism, and that as we turn the prism and eye-lens round the line of vision, the dark part in the ring moves through the double of the angle which the prism does, making a whole revolution whilst the prism has made the half of one. Now if the ring be the intersection of two series of cones, whilst one of these series is diverging and the other converging, the part which disappears, in any one position of the prism, should be in the same sector, measured from the optic axis as centre, in both series; but after one has ceased to converge, the bright spot then formed should exhibit no polarization whatever, being composed of light polarized in all planes; and when it diverges again, the part which disappears should be in the directly opposite sector to that which disappears in the series of cones which has continued to diverge.

This is exactly what we see; but where the two series of cones are diverging together, they soon overlap, and as they are polarized in planes at right angles to each other at every point where they overlap, the light there exhibits no signs of polarization; so that, for the internal series, it is only before it has begun to overlap the other, that the polarization is evident. In this small space it is very easy to overlook it, and I believe I should not have discovered the law unless guided by my theoretical view, as to the formation of the ring.

It was before stated that the ring appeared at the position of the virtual images within the crystal: now as the images have their distances from the surface slightly different, depending, as we have seen, on their respective refractive indices at that point, therefore the two portions of the ring will have different positions of greatest distinctness; and this is easily seen, when experimenting in a dark room, with a bright pencil of light from a closed lamp, and when the incident pencil is very small; for then, when we desire to obtain the ring with maximum distinctness, we can see, that as one portion of it becomes more distinct the other becomes less so, on varying slowly the distance of the eye-lens.

A popular explanation of the formation of the ring, and consequently of the two series of cones, may be thus given: any physical point being taken in the small hole on the first surface of the crystal, it will have two images polarized in planes at right angles to each other; these are two points at the extremities of some diameter of the ring; and every other point having similarly two images, the locus of all these images forms the ring. Speaking more correctly, if we call E_1 and E_2 the two images of any physical point, then the locus of E_1 forms one ring and the locus of E_2 the other, as before described.

The conclusion we come to, from the preceding discussions, is that the phænomena of conical refraction were not adequately examined by Professor Lloyd. It may be that Sir William Hamilton and Professor Lloyd have expected results from their experiments for which Fresnel's expression for the wave-surface in biaxal crystals gives no warrant. The cylindrical refraction in air, being merely the transition from a series of converging cones to a series of diverging ones, may not be separable from them by any mode of experimenting. My present impression is that no such cylindrical refraction exists, but it is a point for future careful experimental re-examination, accompanied by an attentive comparison with the properties of Fresnel's expression. The angle of the cone which has its vertex at the second surface of the crystal, I find to be about 3° 30′, or something less than this, considering a point outside the crystal as the vertex; for a series of careful micrometrical measures with a compound microscope of a magnifying power of twenty-six times, and a minute incident pencil of lamp light, gave the diameter of the ring 1-53·4th of an inch, and by finding the apparent position of the ring, within the crystal, we have the angle of the cone as above. Now the angle from Sir William Hamilton's calculations should be about 3°, a sufficiently near coincidence to produce an impression in the mind in favour of Fresnel's expression.

We saw, however, that one image of the sun, on the second surface, in the first-mentioned experiment with sun-light, on approaching the position of the optic axis, expanded into a ring, whilst the other image became an ill-defined point in its centre: it is seen also that the two images leave the optic axis in the same relation to it as that in which they approached it. From this we conclude, that the space round the optic axis within the ring is traversed by only one image, and therefore that there is only one sheet of the luminiferous surface in this place: the form of the two series of cones also favours this view. This indicates what may not inaptly be termed an eyelet-hole in one sheet, and a conical point in the other.

To determine the real nature of the refraction about the optic axes of biaxal crystals, will evidently require a good deal more of minute examination.

If any fellow-labourers should enter on the subject, I should be very glad to see their results in print, and am glad to be able to inform them that Messrs. Watkins and Hill have undertaken to fit up for sale crystals of arragonite, mounted in a mode so as to facilitate the examination of the refraction near the optic axes.

Queen's College, Cambridge, Feb. 13, 1841.

LVIII. *On the Voltaic Decomposition of Aqueous and Alcoholic Solutions.* By Arthur Connel, *Esq., F.R.S.Ed., Professor of Chemistry in the United College of St. Salvator's and St. Leonard's, St. Andrew's.*

[Continued from p. 249, and concluded.]

II. *Alcoholic Solutions.*

I SHALL content myself with simply referring to the experiments, by which I have shown that under voltaic agency water entering into the constitution of absolute alcohol is resolved into its elements, hydrogen being given off at the negative pole, and oxygen being engaged in producing secondary effects on the hydrocarbon of the alcohol, and to those demonstrating the extraordinary effect which the solution of the minutest quantities of alkalies, acids and saline bodies, have in promoting this action*.

This fact, that the water of absolute alcohol suffers decomposition under galvanic action, furnishes the key for solving all cases of the voltaic decomposition of alcoholic solutions, by assimilating them to those which occur in aqueous solutions. The analogy between the two cases is complete, with the difference merely, that in the former the appearances are less marked, from the smaller quantity of water present, the inferior conducting power of the solution, and the slight modi-

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* Edinburgh Transactions, vol. xiii.; Jameson's Journal, 1835; and London, Edinburgh and Dubl. Phil. Mag., Dec. 1841.

fications of secondary action due to the presence of hydrocarbon. In short, as respects voltaic agency, an alcoholic solution may be regarded as an aqueous one, in which a foreign substance is present retarding the principal action, but affording an additional source of secondary agency.

Accordingly, when the alcoholic solution of an ordinary salt is acted on, the acid and alkali go to their proper poles, as in an aqueous solution, but much more slowly, and the hydrogen of the water of the alcohol is evolved at the negative pole, whilst the oxygen enters into secondary combination. When the base is of easy reduction, metal reduced by hydrogen appears at the negative; whatever substance is dissolved, if it is not of a nature to afford room for a secondary action with hydrogen, that element is given off in the same proportion as from water, due regard being had to some circumstances connected with the conducting power of the solution which have been formerly pointed out*. This fact was verified for alcoholic solutions of alkalies, haloid salts, and oxyacid salts.

It was also found, by experiments similar to those with aqueous solutions of hydracids and haloid salts, that when iodine appears at the positive pole in alcoholic solutions of hydriodic acid or of iodides, it is due to secondary action. Absolute alcohol was charged with dry hydriodic acid gas, and placed in A, fig. 2, (p. 243) and water in B and C, A being made negative, and C positive by seventy pairs of four-inch plates. Effervescence soon arose from both poles, but no discoloration was anywhere observed, nor acid in B or C until after the lapse of twenty minutes, when a slight brown discoloration from liberated iodine commenced in C with acid reaction. In half an hour the battery was reversed, when the hydriodic solution was instantly discoloured without elastic fluid from the pole in that solution, and with effervescence from the other. This experiment would of course have had more analogy to those with water, if B and C had contained alcohol; but the feeble conducting power of that liquid, and the risk of interfering with the reactions if any substance was dissolved in it, prevented its employment. The appearances are, however, best explained on the view that the water of the alcohol only suffers direct decomposition.

Analogous experiments led to the same view in regard to iodides.

The appearances with a positive pole of zinc led to the same conclusion. When a saturated solution of dry iodide of potassium in absolute alcohol was thus acted on by fifty

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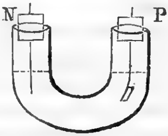
* Lond. Edinb. and Dubl. Phil. Mag., Dec. 1841.

pairs of two-inch plates in the bent tube, fig. 3, oxide of zinc soon separated at *both poles* without any appearance of iodine, or of effervescence at the positive. The deposition of oxide at that pole is in conformity with the view of the direct decomposition of water.

Fig. 3.



With a similar solution of dry chloride of lithium, oxide of zinc soon separated at the negative pole, with effervescence from that pole, but none from the positive; and it was somewhat uncertain whether any separation of oxide took place at the positive: but little doubt could exist that the oxide originated, as in the case of iodide of potassium, by the action of oxygen of the water of the alcohol on the zinc, and was subsequently dissolved and transferred to the negative pole.

The principal condition of the deposition of oxide of zinc at the positive pole, whether in aqueous or alcoholic solutions, appears to be a pretty rapid formation, from brisk action; and the less powerful the acid, and the less its quantity drawn to the positive side, the more of the oxide separates previous to solution and transference.

With respect to pyroxylic solutions, I have made few experiments; because if the general rule holds good in regard to alcohol, there can be little doubt that it will embrace pyroxylic spirit, since, as I formerly showed, the decomposition of its water is much more readily effected than that of alcohol. I found, experimentally, that when a solution of dry iodide of potassium in rectified pyroxylic spirit was placed in a tube A *, and water in a tube B, the two being connected by asbestus, and A made negative, and B positive by fifty pairs of two-inch plates, although iodine soon appeared in the neighbourhood of the positive pole in B, yet it was accompanied by acid passing into the water of B; and after forty minutes' action these appearances continued the same, only more decided, and without any appearance of iodine elsewhere. There is little doubt that the nature of the action was just the same as in aqueous and alcoholic solutions.

In the whole circumstances, although the evidence may not be of quite so decided a character in some of the cases of alcoholic solutions as in regard to those in water, still I think there need not be much hesitation in laying down as a still more general proposition than that above stated, that " When solutions of primary combinations of elementary substances, in water and in those liquids, such as alcohol and pyroxylic

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* Fig. 1. (p. 243.)

spirit, which contain water as such as an essential constituent, are submitted to voltaic agency, the dissolved substance is not directly decomposed by the current, but only the water of the solvent."

III. *Æthereal Solutions.*—Rectified æther gives no symptom of decomposition, nor of conducting power, under the influence of powerful currents, such as that from 200 pairs of four-inch plates; and although $\frac{1}{10000}$th of potash has a marked influence in promoting the voltaic decompostion of alcohol, the largest quantity of that alkali which æther is capable of dissolving has no effect in affording room for any galvanic agency. Neither do any of the ordinary substances soluble in æther, such as corrosive sublimate, chloride of platinum, or chromic acid, produce any such effect. Nor is an oxyacid salt, such as nitrate of uranium, when held in solution by it, resolved into its constituents under galvanic action. The conclusion drawn from all these experiments is, that æther does not like alcohol contain water as a constituent*.

Up to this point, then, æther and æthereal solutions resist all voltaic action. I have since, however, found that when rectified æther is saturated with dry muriatic acid gas, and then submitted to moderate galvanic action, hydrogen, retaining some æthereal vapour mixed with it, is liberated at the negative pole, and no gas from the positive, but the liquid acquires a yellow colour from dissolved chlorine.

When dry hydriodic acid gas was conducted into æther in a little Wolfe's apparatus, the liquid immediately separated into two layers, a lower dense and deep red, and an upper slightly coloured, which under voltaic agency yielded gas from the negative pole.

In regard to the latter of these experiments, there can be no doubt that in saturating the æther with hydriodic acid gas decomposition took place; and although the nature of this decomposition was not fully investigated, it seems probable that the lower liquid consisted principally of iodized hydriodic acid, resulting from the combination of oxygen derived from the æther with hydrogen of a portion of the hydriodic acid, on which view water of course would be formed, and become the subject of the subsequent voltaic action. During the saturation of æther with muriatic acid, no signs of decomposition were visible; but still it is not impossible that some internal changes may have taken place, and water resulted from the action united to muriatic acid. Unless such a view be adopted, I should be inclined to hold that there really was

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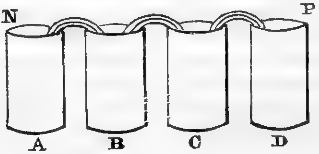
* Edinburgh Transactions, vol. xiii. p. 331.

direct decomposition of the muriatic acid by the voltaic current; for when I recollect that pure æther resisted very powerful voltaic currents, and that even potash, which has so wonderful an effect in promoting the galvanic decomposition of the water in absolute alcohol, did not make æther more susceptible of electric agency, I cannot allow myself to suppose that the decomposition in the case of an æthereal solution of a hydracid is that of water entering into the constitution of æther, but adhere to the original view, that æther contains no water, and that alcohol consists of æther and of water.

IV. *On the state in which the Haloid Salts are dissolved by Water and Alcohol.*—The question whether haloid salts are dissolved by water as such, or decompose it and assume the state of hydracid salts, is one on which chemists are still divided. The action of voltaic electricity on such solutions appears to me to decide the matter: during such action it is frequently difficult, particularly in the case of iodides, to observe any acid reaction at the positive pole, when both poles are plunged directly into the solution, on account of the reducing action of oxygen on the acid formed; and even in those cases in which acid is observed, that circumstance will not of itself prove the haloid to be dissolved as a hydracid salt, because it might be held that acid is formed by secondary action at the negative pole, from whence it is drawn to the positive. In this way only, on the hypothesis of solution as a haloid, and direct voltaic decomposition of water alone, can the separation of reduced metal at the negative pole be accounted for. A doubt might also exist whether the acid reaction at the pole might not arise from an oxyacid formed by secondary action at the positive. All such objections are, however, obviated by placing the poles beyond the solution, so as to get quit of secondary actions; and if in such circumstances we can show that the acid and the base go to their proper poles, and that this acid is a hydracid, we have, I conceive, sufficient evidence that the salt has been dissolved as a hydracid salt; for even laying aside for a moment the experiments by which I have endeavoured to show that the haloids, if existing as such in water, are not directly decomposed, let us take the different views of the nature of the galvanic action which suggest themselves when both poles are plunged into the solution in the ordinary manner, and consider them on the supposition that haloids are dissolved as such. First, let us suppose that one or other of the two substances, water or haloid, it matters not which, is decomposed, it is evident that we cannot account for the production of acid where secondary action is excluded. Next let us suppose that both substances are decomposed, and that either the elements going to the same pole unite on their journey, or by an interchange of elements the oxygen of water unites with the metal of the haloid, and the hydrogen of water with the electro-negative constituent of the haloid. The former of these alternations is contradicted by the fact, that the acid formed is a hydracid; and the latter, although it might account for the formation of acid and alkali, would not account for the liberation of the electro-negative constituent of the haloid at the positive pole, and of hydrogen in fixed and definite proportion at the negative, whatever be the strength of the solution. It appears to me then sufficient, in order to prove the aqueous solution of a haloid as a hydracid salt, to show the separation of the hydracid by voltaic action, under circumstances which exclude secondary action.

To exhibit this result the solution was placed in the tube B, fig. 2, (p. 243) and distilled water in A and C, A being made negative and B positive. When solutions of the chlorides of potassium and of calcium, and of the iodide of potassium were treated in this way by fifty pairs of two-inch plates, acid and alkali were speedily detected at their proper poles in C and A, and on the corresponding sides of the vessel B, and continued to increase whilst the action lasted. The acid collected in C was found in the case of the chlorides to be the muriatic. In the case of the iodide the nature of the acid was somewhat ambiguous with the above power, but when seventy pairs of four-inch plates were employed it was decidedly the iodic. There appeared, however, little doubt that this latter acid had originated in an oxidating action at the positive pole on reduced hydriodic acid; and this view was confirmed by connecting the tube B, containing a solution of iodide of potassium, with two other vessels, C and D, containing distilled water on the positive sides, as in fig. 4, and acting with seventy pairs of four-inch plates:—

Fig. 4.



when the acid produced in C was found to be the hydriodic, and that in D the iodic; in other words, the acid separated from B was the hydriodic, but on passing to the poles it was reduced, and the iodine oxidated; thus both in the case of chlorides and of iodides, the acid separated proved to be the hydracid.

It must, however, always be remembered, that although such production can be readily shown in many cases of haloids, it does not necessarily follow that this would hold in all cases. We are of course best prepared to expect it in the case of haloids, of which the constituents have the strongest affinity for oxygen and hydrogen, such as the ordinary haloids of the bases of the alkalies and alkaline earths; and accordingly, I showed that it applied to chlorides and iodides of potassium and calcium. But further, it was found to hold good in regard to the ordinary haloids of the common metals, such as zinc. I was prepared, however, to consider it as doubtful what might be the result in regard to the noble metals. Accordingly, when a moderately strong solution of chloride of gold was placed in the tube B *, and connected by asbestus with the tubes A and C, which were filled with distilled water, A being made negative, and C positive by a power of fifty pairs of two-inch plates, no decided indications of the formation of acid were obtained during an hour's action; for although towards the end there was a slight acid reaction at the positive pole, it was not greater than distilled water itself might have yielded, and there was a trace of alkali at the negative. Before, however, deciding that chloride of gold in solution does not yield to voltaic action, it would be necessary to repeat the experiment with a more powerful current, because it may possibly only be a case of more difficult electric resolution. In such cases also atomic constitution may have a considerable, if not the principal influence, on the result.

Whenever we have obtained a decided instance of the formation of acid in the above circumstances, we may conclude, with every probability, that all haloids, of the same nature and atomic constitution, of metals, of equal or more powerful affinities, are in the same situation. Thus having verified the rule for chloride of zinc, we may conclude that all protochlorides of more electro-positive metals, such as manganese, cerium, magnesium, barium, potassium, &c., are dissolved as muriates. On the other hand, for the whole series of metals of less powerful affinities, as well as for all haloids of more complex atomic constitution, the matter will still require to be investigated, and I purpose making some further researches on the subject.

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* Fig. 2. (p. 243.)

In regard to sal-ammoniac, I found that it was resolved into acid and alkali in the above circumstances, a result showing that in solution at least it is simple muriate of ammonia, and cannot justly be regarded as chloride of ammonium. The same reasoning above applied to the results with common haloids, can be readily extended to the hypothetical chloride of ammonium; and to complete the evidence on this point, I found that a solution of muriate of ammonia yielded the definite quantity of hydrogen from the negative pole.

The experiments with a positive zinc pole lead to the same result, at least when taken in conjunction with those showing that the haloids, if viewed as existing as such in solution, are not directly decomposed. The oxide of zinc which is dissolved and transferred, must have been taken up by acid which had been previously drawn to the positive side.

The analogy of the action with a positive zinc pole in alcoholic solutions of haloid salts, as formerly described, leads by similar reasoning to the view, that in moderately strong solutions of that description also, such as that of chloride of lithium, iodide of potassium, and moderately saturated alcoholic solutions of chloride of calcium, the haloid decomposes the water of the alcohol, and exists in solution as an oxysalt. Many phænomena of the voltaic action on such solutions will thus receive a more ready explanation than on the idea of these salts being dissolved as haloids; such as the appearance of alkalies and earths at the negative pole, which will thus result directly from the decomposition of a hydracid, instead of supposing the secondary action of the hydrogen and reaction of the metal on water.

We cannot easily get the same evidence on this subject by the method applied to aqueous solutions, of placing the poles in water beyond the solution, because, from the inferior conducting power of the alcoholic solution, less acid will be separated if it truly exist in the liquid, and we cannot distinguish whether it may not come from the point of junction of the alcoholic solution with the water in which the poles are placed.

If alcohol dissolves haloid salts as hydracid salts, there can be little doubt that pyroxylic spirit does the same: I incline to think that the greater solvent powers of the latter fluid than the former in regard to some substances, such as barytes, are due to its greater absolute quantity of water, although not greater atomic proportion.

V. *On the Conducting Powers of Solutions.*—Without going the length of holding that the additional conducting powers bestowed on water by dissolved substances is exactly proportional to the degree of chemical change under voltaic action resulting from the dissolved body, there seems in every instance in which increased conducting power is bestowed some chemical change, or at least voltaic transference, attending the increase of conduction. This chemical change may result either from the direct action of the current or from secondary agencies; and both circumstances lend their aid, where they occur, in augmenting conducting power.

In the case of salts the voltaic separation of acid and alkali at once explains the result, and in many of such cases we have an additional effect from secondary action at one or both poles.

Acids alone in solution, as is now generally known, and as I have myself verified experimentally for sulphuric acid and the hydracids, undergo transference to their proper pole, which circumstance appears to be the principal cause of their promoting conduction. In some instances secondary action at the poles also contributes to the result.

To ascertain whether alkalies have a similar action by suffering transference, a moderately strong solution of caustic potash was placed in a tube B*, connected by asbestus moistened with distilled water, with two tubes, A and C, containing the latter fluid, A being made negative, and C positive by seventy-two pairs of four-inch plates. The whole tubes were covered with a close glass covering, a piece of turmeric paper having been introduced into the liquids A and C between the asbestus and the poles. In a few minutes alkali was indicated at the negative pole, and went on increasing during half an hour, whilst the test paper in C was not discoloured, showing that the effect in A was not owing to capillary action. The experiment was then stopped, when the water in A, although not alkaline to test paper throughout, became decidedly so by concentration, whilst that in C showed no alkali even after concentration.

In the experiments also already detailed, in which acid and alkali were separately drawn to the poles in distilled water from saline solutions, the alkali usually reached the pole as soon as the acid.

There can thus be no doubt, that by voltaic action, the alkali in an aqueous solution is transferred to the negative pole.

Water coloured by bromine gives sensibly more effervescence under galvanic action, showing a superior conducting power of the solution.

The manner in which such simple substances increase the

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* Fig. 2. (p. 243.)

conducting power of water requires a little investigation. Chlorine, bromine, and iodine are generally admitted to be non-conductors themselves; and even if a little doubt may exist as to iodine in a state of fusion, it is scarcely possible that the minute quantity in an aqueous solution can operate in that way.

To ascertain whether such substances were capable of transference in solution, an aqueous solution of bromine, with a little undissolved bromine at the bottom, to maintain a state of saturation, was placed in the tube B, the arrangement being in all other respects the same as in the last-described experiment with a solution of potash ; and after an hour's action of seventy-two pairs of four-inch plates no discoloration from transference of bromine could be observed in the water either of A or of C; and the latter had only a scarcely perceptible smell of bromine, which I believe was due to the secondary decomposition of a trace of hydrobromic acid drawn into C, as both the liquids B and C showed some degree of acid reaction.

An aqueous solution of iodine was then substituted in B for that of bromine, a little iodine being also left at the bottom, and all other circumstances the same, and the battery recharged. After an hour's action there was no appearance of iodine either in A or C.

From these experiments, it is obvious that neither of these substances are transferred in solution by voltaic agency. We must therefore look for some other explanation of the increased conducting power, and that which readily occurs is a secondary action at the negative pole, by the union of hydrogen with the dissolved substance. To determine the accuracy of this view, the current from fifty pairs of two-inch plates was passed at the same time through a solution of bromine and diluted sulphuric acid, and the hydrogen evolved from the two negative poles collected, when after half an hour thirteen cubic inches were collected from the sulphuric solution, and only a bubble, the size of a pea, from the bromine solution : the difference had evidently combined with bromine.

When an aqueous solution of iodine, which had previously been purified by sublimation, solution in alcohol and precipitation by water, was substituted for that of bromine, the action was much more feeble. In a quarter of an hour only a small bubble of gas was collected from each negative pole, and in two hours and a quarter ·1 cubic inch from the sulphuric solution, and ·077 from the iodic.

It is thus evident, that both in the case of bromine and iodine the action is increased by the combination of the dissolved substance with hydrogen of the decomposed water, but that, as is to be expected, this circumstance occurs to a much larger extent in the case of bromine than of iodine*.

On connecting the rules regulating the voltaic decomposition of solutions and the transference of substances held dissolved, we observe that no substance, when in a state of transference, suffers direct voltaic decomposition. Acids and alkalies suffer transference, but not direct decomposition. On the other hand, salts, whether oxyacid or hydracid, are not transferred, but are resolved into their constituent acid and alkali.

We cannot, however, say that every substance which is not transferred is directly decomposed. Thus we can hardly doubt that such compounds as bromide of iodine do not suffer voltaic transference, seeing that their constituent elements are not transferred; and we have further seen that this combination is not directly decomposed in solution. Probably also some cases of chlorides exist, in which, from peculiarity of atomic constitution, or other circumstances, there is neither transference nor direct decomposition.

Erratum in the former part of this paper in No. 117: page 247, line 44, *for* non-negative *read* now negative.

LIX. *On the Natural Arrangement of the Consonantal Sounds.* *By* H. Wedgwood, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

SEEING from your last (February) Number, p. 124, that you do not consider speculation on such a subject foreign to the plan of your publication, I am induced to send you a scheme of all the simple consonantal sounds, exhibiting, in a tabular form, a complete synopsis of their relations with each other.

The first point to be settled is the list of the sounds that are to be the subject of arrangement, which are far from coinciding with those represented by the consonants of the English alphabet. In our mode of writing many simple consonantal sounds are represented by a combination of letters, and conversely, some of our simple consonants are used to represent compound sounds, while others are used on different occasions as the representatives of more than one simple or compound sound.

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* Long after these experiments were made and conclusions drawn, I observed that M. Becquerel had also found that bromine and iodine in solution unite with hydrogen under voltaic action.—*Institut, Juin,* 1840.

The letter C has in some situations the pronunciation of K, in others of S, and may therefore be considered superfluous in a system of which the object is the arrangement of *sounds* alone. The same may be said of Q, which is only used in combination with U, to represent the sound KW; and of X, which is pronounced GS, or KS.

The sound of S in *leisure*, *pleasure*, or of Z in *azure*, is a simple articulation, identical with the French pronunciation of the letter J, but having no exact representative in the English alphabet, in which the sound of J or G soft is a combination of the last-mentioned sound with a D immediately preceding.

The simple sounds expressed by double letters are those of SH; TH in *thin*, and TH in *then*, and NG in *bring*, *hang*.

In addition to the foregoing, the guttural articulations GH, CH, which we do not possess in English, are also simple sounds. Of these, the former is very common in Dutch (in which it is the pronunciation both of G and CH) and Gaelic; the latter in German and Lowland Scotch.

The following, then, will be found to be a complete list of the simple consonantal sounds arranged according to the organ employed in their pronunciation.

Gutturals: K, G, NG, CH, GH, H, pronounced by a constriction of the upper part of the throat and base of the tongue.

Labials: P, B, M, F, V, W, pronounced by the motion of the lips.

Dentals: T, D, N, TH (in *thick*), TH (in *this*), L, pronounced by the pressure of the front of the tongue against the teeth or front of the palate.

Palatals: S, Z, R, SH, J (French), Y, pronounced (as it appears to me) by the pressure of the sides of the tongue against the palate, leaving a passage for the air more or less free through the middle.

The first relation of which we shall take notice between sounds of the same organic class in the foregoing arrangement, is that which distinguishes the classes of Tenues and Medials, as they are commonly called; for which, however, a more characteristic appellation would be spirants and sonants respectively; inasmuch as the former class are pronounced by a mere expiration not falling within the musical scale; the latter, by a fundamental note of the vocal instrument, the organic articulation being in other respects the same in corresponding consonants of either class.

The consonants commonly arranged under the heads of *Tenues* and *Medials*, are the following only :—

Tenues.	Medials.
K	G
P	B
T	D.

It could not, however, altogether escape notice, that the following sounds stood in precisely the same relation to each other:—

F	V
TH (in *thick*)	TH (in *this*)
CH (German)	GH (Gaelic)
S	Z
SH	J (French).

The complete list of spirants and sonants will then be as follows:—

Spirants.		Sonants.
Gutturals	K CH (Germ.)	G GH (Gaelic), G (Dutch)
Labials...	P F, PH	B V
Dentals...	T TH (in *thick*)	D TH (in *this*)
Palatals...	S SH	Z J (French)

In casting our eye over the foregoing table, we can hardly fail to remark that the spirants of each organic class consist of a pair of consonantal sounds, of which the latter is, in every case except the first (in which the K is replaced by an equivalent C), represented by the same letter with the former, with the addition of an H subjoined; and there is an obvious tendency to the same mode of expression in the sonant column.

It is evident, then, that in the *spirant* column at least, a constant relation has been felt between the sounds of the former and latter rank of every organic class, a relation which may pretty accurately be expressed by calling the sounds of the latter rank a thick pronunciation of those of the former rank, although it is not easy to describe the mechanical arrangement of the organs of speech upon which such a modification depends. There can be no question that the same relation holds good between the consonants of each organic class in the sonant column. It is certain that V bears precisely the same relation to B, or J (French) to Z, that F bears to P, or SH to S.

In the ordinary arrangement of the mutes under the heads of Tenues, Medials, and Aspirates, the character of sound designated under the name of *aspirate* is treated as a modification analogous to that which gives rise to the classes of spirants and sonants, and thus the fact is overlooked that both spirants and sonants of every organic class have their correlatives among the aspirates or ranks of thick pronunciation.

Tenues.	Medials.	Aspirates.
K	G	CH
P	B	PH
T	D	TH

The remaining consonantal sounds are N, G, H; M, W; N, L; R, Y, which may be called liquids, from the facility with which they coalesce with other consonants; or neutrals, from not admitting of modification by spirant or sonant pronunciation.

The tabular arrangement of the entire series may then be completed as follows:—

		Spirant or Tenuis.	Sonant or Medial.	Neutral or Liquid.
Guttural	Pure or Clear	K	G.	NG
	Aspirate or Thick	CH (Germ.)	GH, G (Dutch)	H
Labial ...	Pure or Clear	P	B.	M
	Aspirate or Thick	F, PH	V, W (Germ.)	W
Dental ..	Pure or Clear	T	D.	N
	Aspirate or Thick	TH (in *thick*)	TH (in *this*)	L
Palatal..	Pure or Clear	S	Z, S (Germ.)	R
	Aspirate or Thick	SH	J (French.)	Y, J (Germ. Italian).

The characteristic of the sounds of the latter column appears to me to be, that they are each produced by a stationary condition of the vocal organs most nearly approaching to that to which the mouth must be reduced immediately before the pronunciation of the corresponding sonant or spirant.

Hence the peculiar facility with which the sound of NG is combined with that of G, M with B, N with D, L with TH, and R with S; and although we cannot illustrate, in the same manner, the relation of H with GH, CH; of W with V and F, and of Y with J (French) and SH, yet it seems to me to be of the same character, and certainly no one can doubt the intimate connexion of each of the last-mentioned groups of sounds.

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H. Wedgwood.

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

[Continued from p. 293.]

Benzoënitric and Cinnamonitric Acids.

IN the *Annalen der Pharmacie*, vol. xxx. p. 341, is a paper on Balsam of Peru, by Plantamour, in which a peculiar acid is mentioned, viz. the carbobenzoic; in vol. xxxiv. p. 297, is a treatise by Mulder on the benzoënitric acid; and in the *Journal für Praktischen Chemie*, vol. xxii. p. 193, one by Mitscherlich on cinnamonitric acid. These three papers are so intimately related that it is better not to separate them at all, but to consider them in one report.

When benzoic acid is boiled with excess of nitric acid, it dissolves and colours the fluid red; binoxide of nitrogen is given off. The development of gas ceases after boiling for several hours, and the solution becomes colourless. From the cold solution crystals similar to benzoic acid are deposited, the fluid eventually becomes one solid crystalline mass, from which the benzoënitric acid may be extracted by boiling water (Mulder). According to Mitscherlich, the evolution of binoxide of nitrogen is accidental; it arises from the action of nitric acid on the already formed benzoënitric acid; it is therefore not necessary to boil the fluid for any length of time.

The same acid is produced by the action of nitric acid on several substances which have been said to be converted into benzoic acid by nitric acid. Some substances first form benzoic acid, as is the case with cinnamic acid and oil of cinnamon when treated with dilute acid. Plantamour did not discover the nitrogen in this acid: he gave the formula $C^{15} H^{10} O^{3}$; Mulder and Mitscherlich's formula is $C^{14} H^{8} O^{4} + N^{2} O^{3}$, or $C^{14} H^{10} O^{8} N^{2} - H^{2} O$.

This acid separates from water as a crystalline mass, it is easily soluble in boiling water, and if there is not sufficient to dissolve it all, an oleaginous substance is formed which is heavier than water; benzoic acid has the same property. At 10° C. 400 parts of water dissolve one part of the acid, at 100° ten parts. It is easily soluble in alcohol and æther; melts at 127°, and begins to sublime at 110°; the pure acid sublimes unchanged; chlorine has no action on it. Benzoënitric acid is not easily decomposed by heated sulphuric acid; it becomes red, and then contains an acid which gives a soluble salt with baryta. The benzoënitrates are for the most part soluble in water and alcohol, crystallizable, explode by heating, and when gently warmed give off nitrobenzid. The potash salt crystallizes in acicular crystals, and, by heating gives much nitrobenzid; the soda salt is deliquescent (Mulder), easily crystallizable (Mitscherlich). The ammonia salt easily loses ammonia and forms a bisalt. The lime salt is acicular, and contains two atoms of water, which are driven off at 190°. The baryta salt loses four atoms of water at 100°. The strontia salt loses two atoms and a half at 130°. When acid nitrobenzoate of ammonia is dropped into a solution of sulphate of zinc, a gelatinous basic salt is formed, the filtered solution gives lamellular crystals, $C^{14} H^{8} N^{2} O^{7} + Zn\,O + 5\,Aq.$ The basic salt contains four atoms of base to one of acid. The manganese salt crystallizes with one atom of water. The copper salt is a blue powder soluble in hot water; contains one atom of water. There are two salts of lead, a simple benzoënitrate and a benzoënitrate, $5\,(C^{14} H^{8} O^{7} N^{2} + Pb\,O) + Pb\,O$. The silver salt is partly soluble in water, gives much nitrobenzid when carefully heated, &c. &c. (Mulder).

The cinnamonitric acid is formed by adding pulverized cinnamic acid to strong nitric acid; the temperature must not rise above 60°. The cinnamic acid is at first dissolved, the mixture soon becomes hot, and a crystalline substance separates; it is washed with water, dissolved in and crystallized from alcohol. It is white with a trace of yellow, melts at 270°; heated above 270° it boils and is decomposed. Almost insoluble in boiling water. At 20° is soluble in 327 parts of alcohol, and by this property may be easily separated from other acids. Cinnamic acid is soluble in 4·2 parts, benzoic in 1·96, and benzoënitric in somewhat less than its own weight.

Cinnamonitric acid, when boiled with a small quantity of water, does *not* form the oily body mentioned above; is somewhat soluble in boiling hydrochloric acid without decomposition. Its salts with the alkalies are easily soluble; the other salts are either difficultly soluble or insoluble. The potash and soda salts form verrucous crystals; when an excess of potash is added to the potash salt, prismatic crystals are obtained. The ammonia salt decomposes like the benzoate. Of all the others the magnesia salt is the most soluble. These salts explode.

The æther is formed by heating cinnamonitric acid with twenty parts of alcohol and a little sulphuric acid for several hours, at a temperature not above 80°. The acid is dissolved, and the fluid when cooled deposits the æther in prismatic crystals; the æther is not decomposed by ammonia. It melts at 136°, and boils at 300°, at which temperature it is partly decomposed.

Several acids of similar composition give crystallizable æthers; the most beautiful is that of benzoënitric acid ($C^4 H^{10} O, C^{14} H^8 N^2 O^7$). The following crystallize well: picrinnitrate and anisinnitrate of æthyloxide, and the benzoënitrate of methyloxide.

Cinnamic acid may be very easily distinguished from benzoic acid by the formation of the cinnamonitric acid. The formula is $C^{18} H^{12} N^2 O^7$, or $C^{18} H^{14} O^3 + N^2 O^5 - H^2 O$. The uncombined acid is $C^{18} H^{14} N^2 O^8$.

If more than one part of cinnamic acid is added to eight parts of nitric acid, the temperature rises above 60°, and benzoënitric and another, as yet unexamined, acid are formed. The crystallized acid (benzoënitric) contains one atom of water, according to Mulder; he calls it "acide nitrobenzique:" benzoënitric is more applicable.

Cinnamic acid does not combine with sulphuric acid as benzoic acid does. By distilling cinnamic acid with lime, carbonate of lime and carbon remain behind, and the product is a mixture of several substances; it smells like benzin, but is fluid under 0°; it perhaps contains some benzin, but is certainly a mixture. The same is the case with the products obtained by passing camphor and oil of cinnamon through a red-hot tube, according to the experiments of H. C. (D'Arcet's fluid, which is said to be like benzin, but to boil at 140°, I could not obtain; the oil procured began to distil at 140°, but the boiling point rose to 250°.—H. C.)

Ferric Acid.

M. Fremy has found that if a mixture of the peroxide of iron and potash, or better one of peroxide of iron and nitre and potash, or also peroxide of potassium be kept for a time at a lively red heat, a brown mass is obtained, which, when treated with water, affords a very beautiful violet solution. The combination contained in this solution can likewise be formed in the moist way, by passing chlorine into a very concentrated solution of potash in which the hydrate of the peroxide of iron is suspended. The combination prepared in either way is of a beautiful violet, very soluble in pure water, insoluble in alkaline water, in which it forms a brown precipitate, which redissolves in pure water with a purple colour. It appears to be less stable than the manganate of potash. Under certain circumstances (for instance, with time in much water), it decomposes at the common temperature into peroxide of iron, which falls to the ground, and into oxygen, which escapes, while all colour disappears. A temperature of 100° produces the same decomposition instantaneously.

It is decomposed by all organic substances, from which cause it is impossible to filter the solution. Hitherto all attempts to isolate the combination have proved unsuccessful. When the red solution is treated with an acid and the potash nearly saturated, peroxide of iron is thrown down and oxygen disengaged.—*Comptes Rendus*, vol. xii. p. 23.

On the series of Bodies derived from the fuming liquor of Cadet (Alkarsin).

In a paper entitled "Researches on the Cacodyl Series," *Annalen der Chemie und Pharmacie*, xxxvii. p. 1, Dr. Bunsen has described the products of the decomposition of alkarsin.

The primary member of this series has the formula $C^4 H^{12} As^2$, and therefore represents common alcohol, in which two atoms of oxygen are replaced by two of arsenic. This body may be taken as an excellent representative of the organic radicals. The arsenic in these compounds cannot be completely oxidized by nitric acid; moreover, it cannot be determined by means of the usual oxidizing substances, or of oxide of copper, because explosions frequently occur, and the separation of arsenic from copper is difficult. The best substance is oxide of nickel, which may be prepared by heating the sulphate to a white heat; by burning the compound with this oxide the arsenic is fully oxidized, but not so the carbon and hydrogen. The tube for burning is connected with a Liebig's condenser, containing water; if the analysis has not been well conducted this water acquires a smell of alkarsin. The contents of the tube are quite soluble in aqua regia; the solution is treated with sulphuric acid to drive off the nitric acid, evaporated to dryness, moistened and filtered: in this solution the arsenic may be determined in the usual method. The carbon and hydrogen may be determined by burning the compound with oxide of copper or chromate of lead; if oxide of copper be used, a rather long tube must be employed, the anterior part of which is filled with chromate of lead or copper turnings. In preparing these compounds all the vessels must be filled with carbonic acid.

Oxide of cacodyl, alkarsin (vide *Annalen* xxiv.; also Dumas, *Annalen*, xxvii.). Formula $C^4 H^{12} As^2, O$. Specific gravity of the vapour, found by experiment, 7·555, calculated, 7·832.

This body reacts perfectly neutral, but combines with acids, and in this respect is similar to some inorganic bodies, which are half base half acid. Phosphoric acid dissolves a large quantity of it, and forms a stinking thick fluid which does not crystallize. The alkarsin may be distilled over from this compound unchanged. Dilute cold nitric acid forms with alkarsin a thick fluid. Alkarsin is dissolved by digestion with concentrated hydrous sulphuric acid; the liquid on cooling forms a mass of small white acicular crystals: they may be freed from the matrix by pressure between paper, deliquesce in the air and react acid. Nitrate of cacodyloxide gives peculiar precipitates in metallic solutions, but their nature is not as yet determined. With the hydracids alkarsin forms water and a haloid salt. It extracts the oxygen from the oxides of mercury, silver, gold, &c.: even arsenic acid and indigo are reduced by it. Alkarsin may be used as a test for the presence of arsenic. If the substance obtained by Marsh's apparatus be boiled with water until dissolved, and a little potash and acetic acid added, and this solution evaporated to dryness, the mass when heated in a glass tube gives the horrible smell of alkarsin, if any arsenic be present: antimony has no such effect. If a drop of protochloride of tin be added to the heated substance, the characteristic smell of chloride of cacodyl is developed.

Sulphuret of cacodyl, $\mathrm{Cd\,S}$, is obtained by distilling chloride of cacodyl with hydrosulphuret of barium, $\mathrm{Ba\,S,\ \underline{H}\,S}$; hydrosulphuric acid (sulphuretted hydrogen) is given off. The distillation with the hydrosulphuret must be repeated. The water and excess of $\mathrm{\underline{H}S}$ may be removed by chloride of calcium and carbonate of lead; the fluid must then be carefully protected from the air. The acid fluid which accompanies the Cadet's fluid gives a considerable quantity of this protosulphuret when treated with the above-mentioned barium salt. The aqueous solution of cacodylic acid (alkargen) when treated with hydrosulphuric acid also gives it. In an alcoholic solution the higher sulphuret is produced.

$\mathrm{Cd\,S}$ is a clear æthereal liquid which does not fume in the air, of a disagreeable penetrating odour, which reminds one of alkarsin and mercaptan. Does not become solid at − 40° C.: distils over with water, although its boiling point is much higher than 100°. Easily inflammable in the air; the sulphur is perfectly oxidized by strong nitric acid. It is nearly insoluble in water, but communicates to it a strong smell; may be mixed with æther and alcohol in all proportions; combines with sulphur into a crystalline sulphuret. Selenium also forms a similar compound, which crystallizes out of æther in colourless laminæ. Iodine forms a crystalline substance. Oxygen converts it into alkargen, and an, as yet, unexamined crystallized matter. Hydrochloric acid forms $\mathrm{Cd\,\underline{Cl} + \underline{H}S}$. Sulphuric acid gives $\mathrm{Cd\,O,\ \dddot{S} + \underline{H}S}$,

&c. &c. Acetic acid does not decompose it. Formula Cd S, or $C^4 H^{12} As^2$, S. Specific gravity of vapour 7·72, calculated 8·39.

Protoseleniuret of cacodyl, $C^4 H^{12} As^2$, Se, or Cd Se: pure chloride of cacodyl is distilled several times with a solution of seleniuret of sodium: it is a transparent yellowish fluid of very unpleasant smell; insoluble in water, soluble in æther and alcohol; absorbs oxygen and deposits colourless crystals: burns in the air with a blue flame. Nitrate of silver gives with Cd Se, Ag Se + Cd O, $\overset{\cdot\cdot\cdot\cdot\cdot}{\underline{\mathrm{N}}}$. Corrosive sublimate gives first black seleniuret of mercury, and then a white precipitate, viz. hydrargochloride of cacodyloxide. This compound is soluble in hot water, and crystallizes from it on cooling.

Protocyanide of cacodyl, Cd $\underline{\mathrm{Cy}}$, is formed by distilling concentrated hydrocyanic acid with alkarsin, or better by treating a strong solution of bicyanide of mercury with alkarsin. By distillation an oily fluid collects under the water, and on cooling forms large beautiful prismatic crystals; these crystals are then distilled over baryta, &c. &c. Melts at 32° C.; boils at about 140° C.; burns in the air with a reddish-blue flame. Soluble in æther and alcohol, but little in water. It is the most poisonous of all the cacodyl compounds, and one must be excessively careful in experimenting with it. A solution of silver gives cyanide of silver with Cd $\underline{\mathrm{Cy}}$. The nitrate of the dioxide of mercury is reduced by it, not so the nitrate of the oxide. Sublimate gives the hydrargochloride of cacodyloxide, &c. &c. Formula $C^4 H^{12} As^2, N^2 C^2$; specific gravity of the vapour 4·63, calculated, 4·547. The specific gravity of the vapour of cacodyl, $C^4 H^{12} As^2$, must therefore be 7·281. We must reserve the conclusion of this paper for our next.

LXI. *On the Isomorphism of Oxamethane and Chloroxamethane. By* M. F. DE LA PROVOSTAYE, *Professor to the Faculty of Rennes**.

AMONGST the numerous and important researches to which the theory of substitutions has already given birth, those of M. Malaguti upon chloroxalic æther† are without doubt some of the most remarkable. We here see two complete series of products, all derived from each other without destruction, and by simple combinations or transformations;

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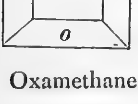
* From the *Annales de Chimie et de Physique*, vol. lxv. p. 322.

† An Abstract of this paper will appear in our next Number.—EDIT.

series which constantly present, on one side hydrogen, and on the other chlorine which takes its place atom by atom. It is certainly impossible to imagine a chemical isomorphism better established and more perfectly characterized; nevertheless I know not whether up to the present time it has been possible to prove crystallographical isomorphism either for these products, or for the products obtained by similar substitutions[*]. Most frequently indeed the two substances, or at least one of them, does not crystallize, and is altogether incapable of exact measurement. To any one remembering the beautiful researches of M. Mitscherlich and the very important consequences which flow from them, it was, however, of the highest interest to arrive at an accurate solution upon this point. M. Malaguti's kindness has enabled me to give it. This chemist sent me some crystals of oxamethane and of chloroxamethane. The former had been obtained by the cooling of an æthereal alcoholic solution, the latter had been formed in a solution simply æthereal. They were perfectly beautiful, and capable of being measured. Now the result of their examination is, that these two substances are isomorphous. I here give the determinations on which this assertion is founded.

The chloroxamethane belongs to the rhombic (right rectangular prismatic) system; it crystallizes in the form of a prism with a rectangular base *l*, the angles of which are of 60° and 120°.

At each summit are two faces *o* forming the bevil, which intersect each other at an angle of 108° 54′ very nearly. We have then



	Measured angles.
P : *l*	= 120°
l : *l* on the other side	= 120°
P : *o*	= 125° 30′—35′.

Oxamethane crystallizes in excessively thin flexible laminæ,

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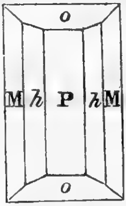
[*] M. Laurent has preferred a claim in the *Comptes Rendus de l'Institut*, on the subject of this note. In a memoir presented to the Academy some months ago, but which has not yet been published, he announced *that certain naphthalic combinations, in which chlorine takes the place of hydrogen, are isomorphous.* I am far from wishing to dispute the merit of these conjectures; but as a fact of this nature can only be established by *measures*, which he had not taken, I still think I may say that oxamethane and chloroxamethane present the first, and until now, the only example of isomorphism, really proved amongst these two classes of substances.

the measurement of which is rather difficult although the faces are brilliant. The summits are almost always altered; they are, however, found capable of measurement on very small crystals.



Measured angles.

P : *o* = 125° 25′—35′
P : *h* = 151° 30′—50′
P : M = 132° 30′—45′

The result of these measurements is that oxamethane also belongs to the rhombic system. The angle of the two faces which form the bevil at the summit is the same as the corresponding angle of the crystals of chloroxamethane. As to the vertical planes, the angles are different; but it is easy to see that the two forms may be derived from the same fundamental form. In fact, the calculated axes, by taking M for the primitive prism, are

Vertical axis . . . . $a = 0{\cdot}715$
Horizontal axis turned towards the observer $b = 1$
Horizontal axis directed from left to right $c = 0{\cdot}924$.

By setting out from these axes, we have, for the notation of the faces and for the calculated value of the angles,

Notation.

Chloroxamethane.	Oxamethane.
$o = \overset{\vee}{P}\infty$,	$o = \overset{\vee}{P}\infty$,
$P = \infty\overset{\vee}{P}\infty$,	$P = \infty\overset{\vee}{P}\infty$,
$l = \infty\overset{\vee}{P}\frac{5}{8}$.	
	$M = \infty P$,
	$h = \infty\overset{\vee}{P}2$.

Calculated angles.

P : *l* = 120°	P : M = 132° 43′
l : *l* on the other side = 120°	P : *h* = 151° 34′
P : *o* = 125° 33′	P : *o* = 125° 33′

The agreement between the observed and the calculated values is such, that it is impossible that the isomorphism can be doubtful.

LXII. *On the Composition of Chalk Rocks and Chalk Marl by invisible Organic Bodies: from the Observations of Dr. Ehrenberg*[*]. *By* THOMAS WEAVER, *Esq., F.R.S., F.G.S., M.R.I.A., &c. &c.*[†]

THE remarkable discoveries effected, and the new light thrown on geology by the indefatigable researches of Dr. Ehrenberg, during several years past, through the medium of the microscope, particularly in respect of the Infusoria and Polythalamia tribes, highly instructive and interesting as they must be to all naturalists, are especially so to the geologist, since they open to him a large field of inquiry, eminently deserving of cultivation. To draw attention to this subject, which involves no less than an investigation as to what extent minute organic bodies, invisible to the naked eye, may have contributed to the production of all limestone formations, whether of an origin posterior or anterior to the epoch of the chalk, descending thus in the series to the primary limestones, it appeared to me that a sketch taken from a portion of the labours of Dr. Ehrenberg might be not only useful, but especially acceptable to such geologists as may not be conversant with the language of the original. I propose then, in the first instance, to advert briefly to the earlier researches of Dr. Ehrenberg concerning the Coral tribes in general, and those of the Red Sea in particular[‡]; and in the second, to present such extracts from the Memoir, the title of which stands at the head of this paper[§], as may answer the purpose of a general view.

At the instigation of the Royal Academy of Sciences of Berlin[||], Dr. Ehrenberg and his friend, the late Dr. Hemprich,

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[*] Communicated by the Author.

[†] With an Appendix touching the researches of M. Alcide d'Orbigny.

[‡] See in the Abhand. der König. Acad. d. Wissenschaften zu Berlin for the year 1832:—

1. Contributions to the physiological knowledge of the Coral animals in general, and in particular of those of the Red Sea, with an attempt to classify them according to their physiological distinctions; read 3rd March, 1831, with additions printed 1st Dec. 1833, pp. 225–380.

2. On the Nature and Structure of the Coral Banks of the Red Sea, read 22nd March 1832; revised and printed in Feb. 1834, pp. 381–432.

[§] *Ueber die Bildung der Kreidefelsen und des Kreidemergels durch unsichtbare Organismen*, in the Transactions of the Royal Academy of Sciences of Berlin, for the year 1838, read 20th Dec. 1838, and 18th Feb. 1839, pp. 59–149.

[||] See the Report read to the Academy by M. Alr. von Humboldt on the Travels of Doctors Ehrenberg and Hemprich through Egypt, Dongola, Syria, Arabia, and the Eastern declivity of the highlands of Abyssinia, in the years 1820–1825, conveying a clear idea of the arduous and extraordinary labours of those gentlemen in all branches of Natural History: Berlin, 1826. Dr. Hemprich fell a sacrifice to his exertions in Abyssinia, on the 30th of June, 1825.

visited the Red Sea during a period of eighteen months, namely, nine months from the year 1823 to 1824, and an equal number in 1825, having been nearly twelve months of the time on board ship, in which interval they passed over nearly the whole extent of that sea, saw many of its islands and coral banks, and landed with a view to special examination on forty-eight different points of the two coasts; but the whole number of islands and special points of the coast seen by them amounts to about 150, independently of the long coast of Sinai in Arabia, which they examined in continuity. In these laborious efforts, attended with extreme danger, they collected 110 species of Coral animals, being nearly three times as many as had been found or described by all former observers, namely, by Shaw, Forskål and Savigny, and later by Rüppel.

To determine the subjects of that collection with the greater precision, it became necessary to undertake a review of the whole class of the Coral animals, and the more so as Dr. Ehrenberg found that his own observations were frequently in collision with the systematic distinctions that have prevailed up to the present time. In this review the author has especially compared the four most recent extensive systems, namely, of Schweigger in 1820, Rapp in 1829, Cuvier in 1830, and Blainville likewise in 1830, which may be said to embody the judgment of the present generation upon the labours of earlier periods, and to comprise the sum of existing knowledge in this department of natural history. He has in particular turned his attention to the work of Blainville *, since it contains the greatest number of new details, having been enriched by the latest manuscript observations and drawings of Quoy and Gaimard, the result of their second voyage round the world with Capt. D'Urville. In these newer works, the labours of Lamarck having been critically employed, the author was relieved from the necessity of noticing them in a special manner.

The attempt to reconcile the observed discrepancies led the author to separate the Coral animals into two organic natural groups, which are well marked and distinct from each other, and which he named *Anthozoa* (Flower-animals) and *Bryozoa* (Moss-animals). In the course of these researches the author found that the whole group of the Anthozoa, which consist of the proper (single-mouthed) coral animals, and which had been gradually distributed under more than 158 generic names, including even heterogeneous animals and plants, might, according to his own observations of their correspondence in

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* *Dictionnaire des Sciences Naturelles*, 1830.

affinity and relations of structure and development, be reduced to eighty-six genera, but which number might perhaps be still further diminished, as a few genera might be classed as subgenera. The Anthozoa he has divided into two orders, *Zoocorallia* (Animal-corals) and *Phytocorallia* (Plant-corals). In the Memoir is given a systematic description of the Orders, Tribes, Families, Genera and Species of the Anthozoa, while further details are reserved for the author's work, entitled, *Symbolæ Physicæ.* The subjoined Table will show the general arrangement, extending to the genera.

ANTHOZOA.

Ore ventriculoque distinctis, tubo cibario anoque discreto nullis, corpore intus radiatim lamelloso. (Vibratio nulla, gemmæ et spontanea divisio frequentissimæ.)

ORDO I.—ZOOCORALLIA.

Corpore aut omnino molli, aut Cephalopodum more intus lapidem generante (secernente nec excernente) hinc sæpe omnino libera et, præter formam, animalium characteres omnes perfectius servantia.

			Species.			Fossil Genera.	Genera.
			Of the Red Sea.				
			Unproved.	Observed by himself.	Total living.		
TRIBUS I.	Zoocorallia Polyactinia.	Familia I. ACTININA. Genera 9. Species living . 50. In the Red Sea 23.	...	16	29	...	1. Actinia.
			...	1	1	...	2. Metridium.
			...	1	1	...	3. Megalactis.
			1 ?	...	1 ?	...	4. Thalassianthus.
			...	1	10	...	5. Cribrina.
			...	1	3	...	6. Actinodendron.
			...	1	1	...	7. Epicladia.
			...	1	1	...	8. Heterodactyla.
			...	...	3	...	9. Lucernaria.
		Fam. II. ZOANTHINA. Genera 4. Fossil 2. Species living . 12. In the Red Sea 7.	...	2	3	...	10. Hughea.
			1	...	2	...	11. Zoanthus.
			1	1	4	...	12. Mammillifera.
			...	2	3	...	13. Palythoa.
			...	...	...	F.	Siphonia.
			...	...	...	F.	Lymnorea.
		Fam. III. FUNGINA. Genera 6. Fossil 5. Species living . 12. In the Red Sea 3.	...	1	3	F.	14. Fungia.
			...	1	5	...	15. Haliglossa.
			...	...	3	...	16. Polyphyllia.
			...	...	...	F.	17. Cyclolithas.
			1 ?	...	1 ?	F.	18. Turbinalia.
			...	...	...	F.	19. Trochopsis.
			...	...	...	F.	Diploctenium.

			Species.				
			Of the Red Sea.				
			Unproved.	Observed by himself.	Total living.	Fossil Genera.	Genera.
TRIBUS II.	Zoocorallia Octactinia.	Fam. IV. XENINA. Genera 3. Species living . 7. In the Red Sea 6.	...	3	3	...	20. Xenia.
			...	3	3	...	21. Anthelia.
			...	...	1	...	22. Rhizoxenia.
		Fam. V. TUBIPORINA. Genus 1. Species living . 3. In the Red Sea 1.	...	1	3	...	23. Tubipora.
		Fam. VI. HALCYONINA. Genera 6. Species living 28. In the Red Sea 13.	...	1	1	...	24. Halcyonium.
			...	5	12	...	25. Lobularia.
			...	2	2	...	26. Ammothea.
			...	2	4	...	27. Nephthya.
			...	3	8	...	28. Sympodium.
			...	...	1	...	29. Cliona.
		Fam. VII. PENNATULINA. (α) HALISCEPTRA. Genera 4. Species living . 7. In the Red Sea 0.	...	...	4	...	30. Veretillum.
			...	...	1	...	31. Pavonaria.
			...	...	1	...	32. Umbellularia.
			...	...	1	...	33. Scirpearia.
		(β) HALIPTERIA. Genera 3. Fossil 1. Species living . 10. In the Red Sea 0.	...	...	2	...	34. Renilla.
			...	...	3	F.?	35. Virgularia.
			...	...	5	...	36. Pennatula.
TRIBUS III.	Zoocorallia Oligactinia.	Fam. VIII. HYDRINA. Genera 2. Species living . 6. In the Red Sea 0.	...	...	4	...	37. Hydra.
			...	...	2	...	38. Coryna.
		Fam. IX. TUBULARINA. Genera 4. Species living . 12. In the Red Sea 0.	...	...	4	...	39. Syncoryna.
			...	...	3	...	40. Tubularia.
			...	...	4	...	41. Eudendrium.
			...	...	1	...	42. Pennaria.
		Fam. X. SERTULARINA. Genus 1. Species living . 4. In the Red Sea 1.	1 ?	...	4	...	43. Sertularia.
		ZOOCORALLIA...	5	49	151	8	

Ordo II.—Phytocorallia.

Corpore aut lapideam aut corneam materiam adglutinantem secernente, ac dorso (solea) excernente ejusque ope semper adnato (Ostrearum more).

			Species.			Fossil Genera.	Genera.
			Of the Red Sea.				
			Unproved.	Observed by himself.	Total living.		
Tribus IV.	Phytocorallia Polyactinia.	Familia XI. Ocellina. Genera 14. Fossil 12. Species living 41. In the Red Sea 7.	...	...	2	...	44. Desmophyllum.
			...	...	3	...	45. Cyathina.
			...	1	1	F.	46. Stephanocora.
			...	...	3	F.	47. Monomyces.
			...	1	9	F.	48. Oculina.
			...	...	4	F.	49. Turbinaria.
			...	3	8	F.	50. Explanaria.
			...	...	6	F.	51. Cladocora.
			...	...	...	F.	52. Columnaria.
			...	1	1	F.	53. Strombodes.
			...	...	...	F.	54. Cyathophyllum.
			...	...	...	F.	55. Pterorrhiza.
			...	1	4	F.	56. Anthophyllum.
			...	...	...	F.	57. Stylina.
		Fam. XII. Dædalina. (*α*) Astræina. Genera 4. Fossil 3. Species living 28. In the Red Sea 15.	1	2	7	...	58. Caryophyllia.
			...	4	7	F.	59. Favia.
			...	8	14	F.	60. Astræa.
			...	...	...	F.	61. Favosites.
		(*β*) Mæandrina. Genera 7. Fossil 6. Species living 28. In the Red Sea 5.	...	2	7	F.	62. Mæandrina.
			...	2	12	F.	63. Manicina.
			...	...	1	...	64. Merulina.
			...	1	4	F.	65. Pavonia.
			...	...	2	F.	66. Agaricia.
			...	...	2	F.	67. Polyastra.
			...	...	...	F.	68. Monticularia.
Tribus V.	Phytocorallia Dodecactinia.	Fam. XIII. Madreporina. Genera 3. Fossil 3. Species living 41. In the Red Sea 24.	...	7	21	...	69. Heteropora.
			...	17	20	F.	70. Madrepora.
			...	...	...	F.	71. Catenipora.
			...	...	...	...	Pleurodictyum.
		Fam. XIV. Milleporina. Genera 4. Fossil 2. Species living 23. In the Red Sea 11.	...	...	...	F.	72. Calamopora.
			...	4	6	...	73. Seriatopora.
			1	2	7	F.	74. Millepora.
			...	3	10	...	75. Pocillopora.
Tribus VI.	Phytocorallia Octactinia.	Fam. XV. Isidea. Genera 4. Species living 10. In the Red Sea 1.	...	...	1	...	76. Corallium.
			...	...	4	...	77. Melitæa.
			...	1	3	...	78. Mopsea.
			...	...	2	...	79. Isis.
		Fam. XVI. Gorgonina. Genera 6. Fossil 1. Species living 63. In the Red Sea 3.	1?	...	3	...	80. Prymnoa.
			1?	...	10	...	81. Muricea.
			...	...	23	...	82. Eunicea.
			1?	...	4	...	83. Plexaura.
			...	...	12	F.?	84. Gorgonia.
			...	...	11	...	85. Pterogorgia.
Tribus VII.	Phytocorallia Oligactinia.	Fam. XVII. Alloporina. Genus 1. Species living 1. In the Red Sea 0.	...	...	1	...	86. Allopora.
		Phytocorallia	5	61	235	27	
		Zoocorallia	5	49	151	8	
		Anthozoa	10	110	386	35	
		Of the Red Sea	120				

In the preceding Table we see that of the forty-three genera of Zoocorallia, there are eight which are found fossil; the living species amount to 151, of which fifty-four exist in the Red Sea, and forty-nine of these have been observed by the author, five remaining unproved. Of the forty-three genera of Phytocorallia there are twenty-seven which are found fossil; the living species amount to 235, of which sixty-six exist in the Red Sea, and sixty-one of these have been observed by the author, five remaining unproved. The general result is, that out of eighty-six genera of Anthozoa, thirty-five occur in the fossil state; and that of 386 known living species of Anthozoa, 120 exist in the Red Sea, of which 110 species were observed by the author. The same Table also shows that of the seventeen families of known Coral animals, thirteen exist in the Red Sea, while four are wholly wanting, namely, those of Pennatulina, Hydrina, Tubularina and Alloporina. The total number of known living species comprised in each family is also given, as well as the relative number actually existing in the Red Sea.

The 120 species of Anthozoa existing in the Red Sea thus constitute nearly one third of the whole number of living species, and being comprised in forty-four genera, the latter rather exceed one half of the number of known living genera.

Of the known living Corals there are eight genera peculiar to the Red Sea, namely, *Megalactis*, *Thalassianthus?*, *Epicladia*, *Heterodactyla*, *Anthelia*, *Ammothea*, *Stephanocora* and *Strombodes*. It appears also that eighty-eight species are peculiar to it, not having been hitherto observed anywhere else.

Among the genera of the Red Sea that of *Strombodes* excites peculiar interest, having previously been found only in the fossil state. It affords a key to the structure of the remarkable Cyathophylla, differing from the view hitherto entertained, and rendering it quite clear that the internal central star of the encased forms is not a young one, but the oldest or mother star, which is often surrounded by broad radiated mantle-folds productive of buds.

It appears probable that the Red Sea and the part of the Mediterranean so nearly adjoining on the Libyan coast, possess only two forms out of the 120 species in common, namely, *Actinia Tapetum* and *A. Mesembryanthemum*.

Of the Bryozoa group, Dr. Ehrenberg gave in the same memoir, contained in the volume of the Transactions for 1832, only the more general results of his investigations, without entering into detail; but the subject is resumed in his later memoir, inserted in the volume for 1838, in which he has presented a tabular view of the Bryozoa, distributed into Orders, Families and Genera, with their characteristics. According to this view the Bryozoa comprise four Orders, *Polythalamia*, *Gymnocoræ*, *Thallopodia* and *Scleropodia*; the Polythalamia being divided into *Monosomatia* (single-bodied), consisting of fifty-six genera, and *Polysomatia* (many-bodied or polyparian), composed of twenty-two genera, forming altogether seventy-eight genera of Polythalamia. The following Table exhibits the general arrangement.

BRYOZOA.

Animalia asphycta, tubo cibario simplici, sacciformi aut tubuliformi, vera corporis articulatione nulla aut sensim numerosiore, corporis forma gemmis aut novis articulis accedentibus sensim aucta, hinc indefinita, nunquam sponte dividua, omnia et singula verisimiliter periodice ovipara, ideoque hermaphrodita.

ORDO I.—POLYTHALAMIA.

Libere vagantia et loricata.

Monosomatia.

Familia I. MILIOLINA.
Genera 2. ? Miliola, ? Gromia.

Familia II. NODOSARINA.
Gen. 11. Glandulina, Mucronina, Nodosaria, Orthocerina, Dentalina, Lingulina, Frondicularia, Rimulina, Vaginulina, Planularia, Marginulina.

Familia III. TEXTULARINA.
Gen. 6. Bigenerina, ? Dimorphina, Textularia, Grammostomum (*Vulvularia*), Polymorphina, Virgulina.

Familia IV. UVELLINA.
Gen. 11. Guttulina (et *Globulina*), Uvigerina, Bulimina, Valvulina, Rosalina, Clavulina, Globigerina, Pyrulina, Sphæroidina.

Familia V. ROTALINA.
Gen. 22. Operculina, Soldania, Planorbulina, Rotalia, Trochulina, ? Spirulina, Calcarina, Pleurotrema, Planulina, Discorbis, Omphalophacus, ? Gyroidina, Truncatulina, Lenticulina, Nonionina, Cristellaria, Siderolina, Dendritina, Robulina, Anomalina, Saracenaria, Cassidulina.

Familia VI. PLICATILIA.
Gen. 6. Biloculina, Spiroloculina, Triloculina, Articulina, Quinqueloculina, Adelosina.

Polysomatia.

Familia VII. ASTERODISCINA.
Gen. 5. Asterodiscus, Lunulites, Orbitulites, Cupularia, Flustrella.

Familia VIII. SORITINA.
Gen. 2. Sorites, Amphisorus.

Familia IX. FRUMENTARINA.
Gen. 3. ? Dactylopora, ? Ovulites, ? Polytripe.

Familia X. HELICOSORINA.
Gen. 5. Peneroplis, Pavonina, Vertebralina, Orbiculina, ? Heterostegina.

Familia XI. HELICOTROCHINA.
Gen. 3. Polystomella, ? Amphistegina, ? Geoponus.
Familia XII. ALVEOLINEA.
Gen. 2. Melonia, Alveolina.
Familia XIII. FABULARINA.
Gen. 2. Fabularia, Coscinospira.

ORDO II.—GYMNOCORÆ.

Libere vagantes, nudæ.

Familia I. CRISTATELLINA.
Gen. 2. Cristatella, Zoobotryon.

ORDO III.—THALLOPODIA.

Stolonibus thallove membranaceo affixa, incrustantia
nec adnata, sed loricata.

Familia I. HALCYONELLEA.
Gen. 8. Halcyonella, Vesicularia, Bowerbankia, Farrella (= *Lagenella*)[*], Valkeria, Stephanidium, n. G., Dynamene, Halodactylus (= *Alcyonidium*).
Familia II. CORNULARINA.
Gen. 1. ? Cornularia.
Familia III. ESCHARINA.
Gen. 5. Eschara, Melicertina (= *Melicerita*)[†], Crisia, Acamarchis, Notamia.
Familia IV. CELLEPORINA.
Gen. 5. Cellepora, Flustra, Membranipora, Briolophus, n. G., Apsendesia.
Familia V. AULOPORINA.
Gen. 1. Tubulipora.

ORDO IV.—SCLEROPODIA.

Stolonibus destituta, excreto fulcro axique anorganicis
firmiter affixa eisque fruticulosa.

Familia I. MYRIOPORINA.
Gen. 9. Hornera, Idmonea, Retipora, Distichopora, Myriopora, Tilesia, Cricopora, Ceriopora, Spiropora.
Familia II. ? ANTIPATHINA.
Gen. 1. Antipathes.

"The two last orders, the Thallopodia and Scleropodia," the author observes, "are considerably richer in forms, and it would be very easy by an uncritical compilation to enlarge greatly the number of names; but such confusion has been produced in names by Lamouroux and later writers, the same body being often designated by many new names, that I shall not venture to extend my judgment further at present. What

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[*] The name *Lagenella* was appropriated to an infusorial form in 1832.

[†] *Melicerta* is already employed among the Radiaria, *Melicertum* with the Acalepha, *Melicerita* is not correct in language.

Perhaps hereafter it may be advisable to substitute *Textilaria* for Textularia, *Polystomatium* for Polystomella, *Cyclodiscus* for Discorbis, &c.

has been advanced will suffice to show clearly the position of the Polythalamia, such as it appears to me, in the animal kingdom."

On Chalk and Chalk Marl.

The memoir on the chalk and chalk marl is distributed under the following heads:—

1. Historical Introduction, pp. 59—68.
2. New method of observing, pp. 68—70.
3. On calcareous-shelled organisms, invisible to the naked eye, as the principal constituents of writing chalk, pp. 70—74.
4. On Chalk Marl and its relations to Chalk, and to the Flints of the Chalk, pp. 74—87.
5. On the compact limestone of Upper Egypt and Arabia, as formed by the Polythalamian calcareous animalcules of the White Chalk of Europe, pp. 87—90.
6. On the principal organic calcareous forms which constitute all chalk, and the local differences, pp. 90—95.
7. Preliminary view of new researches respecting living Polythalamia, and their relation to the formation of the sand of Sea Downs, pp. 96—106.
8. Application of the preceding observations to the systematic distinctions of Polythalamia, with a tabular view of the Bryozoa, according to their orders, families and genera, with their characteristics, pp. 107—121.

(N.B. Of this tabular view I have given a transcript above.)

9. On the geographical distribution of living Polythalamia on the African and Asiatic coasts of the Mediterranean, and in the Red Sea, with a tabular view of the genera and species, pp. 121—127.
10. A concise Diagnosis of the new families, genera and species,
    1. Of the siliceous Infusoria of the chalk marl, containing thirty-one new species, of which seventeen species belong to five new genera, and fourteen species to five former-known genera, pp. 128—130.
    2. Of the calcareous-shelled Polythalamian animalcules of the chalk and sea sand, sixty-seven new species, beside two new species from the Jura (Oolite) limestone, pp. 130—135.
11. A summary view of the conclusions drawn from the preceding expositions, pp. 135—139.
12. Explanation of the Plates, pp. 140—147.
13. A tabular view of the organic bodies invisible to the naked eye, which form the chief constituents of chalk, chalk marl, the compact limestone of Egypt and Arabia, and the nummulitic limestone of the Pyramids of Geza or Gyzeh.

The reader being thus put in possession of the general scope of the work, I now proceed to exhibit in full the conclusions to which the author has been led (as indicated under the head of No. 11), to which I shall subjoin further extracts taken from different portions of the Memoir, for the purpose of general illustration.

Conclusions.

1. Many, and probably all, *White Chalk Rocks* are the produce of microscopic coral-animalcules, which are mostly quite invisible to the naked eye, possessing calcareous shells of $\frac{1}{24}$ to $\frac{1}{288}$ line in magnitude, and of which much more than one million are well preserved in each cubic inch, that is, much more than ten millions in one pound of chalk *.

2. The *Chalk Marls* of the Mediterranean Basin are the produce of microscopic Infusoria possessing siliceous shells or cases, mostly quite invisible to the naked eye, intermingled with a small proportion of the calcareous animalcules of the chalk.

3. The peculiar state of aggregation in *White Chalk* does not arise from a precipitate of lime previously held in solution in the water of the sea, nor is it the result of the accumulation of the small animalcules, but it proceeds from a disintegration of the assembled microscopic organisms into much minuter inorganic calcareous particles; the reunion of which into regular, elliptical, granular laminæ, is caused by a peculiar crystalloid process, which may be compared to crystallization, but is of a coarser nature, and essentially different from it. The best writing chalk is that in which this process has been developed to the greatest extent.

4. The compact limestone rocks also which bound the Nile in the whole of Upper Egypt and extend far into the Sahara or Desert, being neither white nor of a staining quality, as well as the West Asiatic compact limestone rocks in the north of Arabia, are, in the mass, composed of the coral animalcules of the European chalk. This affords a new insight into the ancient history of the formation of Libya from Syene to the

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* It is to be understood that I speak only of such Polythalamia as are well preserved, wholly disregarding their fragments. Of the well-preserved there are contained in one fourth part of a cubic line, or in one twelfth of a grain of chalk, frequently 150 to 200 in number, equal to 600–800 in each cubic line, or 1800–2400 in each grain, and from 1,036,000 to 1,382,400 in each cubic inch; and hence in one pound of chalk the number far exceeds ten millions.

The larger Polythalamia and Bryozoa of the chalk are best obtained from the sediment produced by brushing the chalk under water; the entirely microscopic forms remain long suspended in water.

Atlas, and of Arabia from Sinai to Lebanon, thus opening a large field to organic distribution.

5. Many of the chalk-like formations bordering on the Mediterranean in Sicily, Barbary and Greece, really belong to the period of the European chalk formation, as proved by their organic contents, although commonly held to be different from the chalk, and considered as tertiary *.

6. The chalk beds of the South of Europe, around the basin of the Mediterranean, are distinguished from those of the north and east of Europe by numerous well-preserved chalk animalcules, and less numerous inorganic laminæ; while in the north and east of Europe these relations are reversed †.

7. In the South of Europe the beds of marl which alternate with the chalk consist of siliceous shells of Infusoria, and flints are wanting; while in the North of Europe beds of flint alternate with the chalk, and marls with Infusoria are wanting. This exchange of character tends to explain the peculiar relation of flint to chalk, indicating that the pulverulent siliceous particles of Infusoria have been converted into compact nodules of flint.

8. It has been lately remarked that the chalk which contains flints is deficient in numerous siliceous Infusoria, when compared with the Bilin slaty Tripel or polishing slate (*Polirschiefer*) containing semi-opal; but this deficiency now disappears, and a rich substitute takes its place, the Infusoria in the North of Europe having been employed in the formation of flints; while in the south, remaining unchanged, they are preserved in the Infusoria marls.

9. The chalk animalcules resemble most those of the sea-sand and the Miliolites, which, up to the present day, have been ranged among the Mollusks with the Cephalopods; but neither of these are either Cephalopods or Mollusks, nor even Infusoria (as asserted by a late observer); but they are Bryozoa, animals of Moss-corals, which are most nearly related to Flustra and Eschara.

10. The sea downs of some, and probably of most coasts, are still in course of formation by living Bryozoa, which, though very small, resembling grains of sand, are yet, for the most part, larger than the chalk animalcules, and a large pro-

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* In Sicily, however, there occur many breccias of chalk, which have suffered a subsequent change, and may be referred to the tertiary epoch.

† Thus in the white and yellow soft writing chalk of the North of Europe the inorganic crystalloid portions sometimes equal or rather exceed in mass the organic remains; but in the South of Europe, in Sicily, these organisms with their fragments are greatly predominant, consisting, as it appears, exclusively of well-preserved Polythalamia.

portion of the sand of the Libyan Desert has been proved to consist of such grains. It is only in Nubia above Syene that the desert sand becomes a pure detritus of granite*.

11. In the various countries of the earth in which occur white and earthy, as well as coloured and compact rocks, composed of microscopic calcareous animalcules, the genera and species of these animalcules present so striking an agreement with those of the white chalk of Rügen, that they may well be deemed characteristic of one and the same period of geological formation. It cannot be asserted for a certainty that the same forms have been observed any where else†.

12. In the beds subjacent to and more ancient than the chalk, namely, in those of the Oolite or Jura limestone formation, we have also clear evidence of the existence of other microscopic Polythalamia. These, however, are such as have not hitherto been found anywhere in the chalk.

13. The early assertion that *all* limestone was the produce of animals‡, though resting on no sufficient foundation, and therefore justly held in slight regard by modern geologists, yet now deserves every attention, since it clearly appears that a limestone formation widely extended on the surface of the earth is composed of microscopic animals, visibly converted in a gradual manner into inorganic chalk and compact limestone. If similar phænomena appear also in the Jura limestone formation, and should become still further confirmed, these considerations (combined with the long-known existence of coarser corals and shells in both formations) tend to show how necessary it is, when examining the composition of any considerable portion of the solid mass of the earth, to strengthen our natural senses by artificial means, in order to obtain a distinct knowledge of the extent to which organic life may have contributed to its production.

14. The extreme minuteness of the chalk animalcules is strikingly proved by this, that even in the finest levigated whiting multitudes of them are still present, and may be applied without suffering change to the most varied technical purposes. Thus in the chalk coating given to painted chambers, paper, or even glazed visiting-cards (when not coated with white lead

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* On these very interesting and not easily developed relations, I hope, at a future day, to be able to make a more special communication.

† If I have applied the same name in some cases both to animalcules of the chalk and to forms existing in the present sea-sand, or in recent fossil beds, it has arisen partly from my being unacquainted with the original forms of the latter, and partly from my desire not to create unnecessary perplexity by the adoption of new names. It should be observed that they are distinguished by marks of interrogation. All those which I could really compare were different.

‡ By Linnæus in 1745 and 1748, and Buffon in 1749.

alone), may be seen a pretty mosaic of well-preserved, moss-coral animalcules, but which are invisible to the naked eye. And thus our natural vision receives from such a surface the impression of the purest white, little deeming that it contains the bodies of millions of self-existing beings, of varied and beautiful forms, more or less closely crowded together (as in Plate IV., where the subjects are magnified 300 times).

Explanation of the Plates and Tabular View.

The Memoir is accompanied by four Plates*, presented with the view of facilitating a comparison between the organic relations of minute fossil bodies invisible to the naked eye, and those of still living bodies visible to the naked eye.

Thus the first three Plates exhibit recent small bodies naturally visible, with which the naturally invisible forms of the fourth Plate may be readily associated.

The first three Plates serve also to elucidate the true nature of the Polythalamia (hitherto mistaken), showing their greater affinity to the Bryozoa (Flustra) than to all other animal forms, and in particular the great difference there is between them and Cephalopods and Infusoria. They represent partly the unfolded, soft, external parts of living subjects, and partly dead, naked bodies, artificially divested of their calcareous shell, and not hitherto figured.

Lastly, these first three Plates serve to convey a view, according to some of their principal divisions, of the structure of the whole group of forms occurring in Polythalamia, and in particular to illustrate their frequent assemblage in families, or Polyparies, as they are termed. Plate I. contains simple forms; Plates II. and III. composite or family forms, Polyparies; of which Plate II. contains family forms assembled in single rows, and Plate III. family forms arranged in many rows.

If, as already observed, we examine a wall or paper whitened with finely levigated chalk, or a glazed visiting-card not coated with white lead alone, but also with chalk, they would appear, when magnified 300 times, more or less rich in subjects, as represented in Plate IV.

Plate I. contains *simple recent Polythalamia* from the sea-sand of Rimini. Fig. 1. *Rotalia Beccarii*; the shell only was known, but the figures show also the form of the animal when deprived of its shell by an acid, the form of both being the same. Fig. 2. *Marginulina Raphanus* (*Nodosaria Raphanus, Nautilus Raphanus priorum*), also very common at Rimini and other Italian coasts, and which had hitherto been erroneously ranked with Orthocera.

Plate II. contains *Polyparies of recent Polythalamia assem-*

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* These plates do not accompany Mr. Weaver's paper.

bled in single rows, from the Red Sea and the Mediterranean. The two subjects represented in this Plate were collected by me in the year 1823, and it is peculiarly interesting, through my newly-discovered method of observing*, to have been able to see in several divisions of the internal body the remains of the siliceous Infusoria, of which they had made a repast fifteen years before. Fig. 1. *Peneroplis planatus,* d'Orbigny, *Nautilus planatus* of Fichtel and Moll, from the Red Sea. The shells of this animalcule were hitherto only known, but the soft organic animal form which they inclose is here also represented. Fig. 2. *Coscinospira Hemprichii,* a form from the Red Sea, also found in the Libyan part of the Mediterranean, and which was formerly erroneously placed adjoining the *Spirula* of the Cephalopods, and more recently as connected, through *Lituolites nautiloides,* with *Spirolina.*

Plate III. contains *Polyparies of recent Polythalamia assembled in many rows.* This Plate contains the only living animalcule of the Polythalamia group, hitherto so far observed as to admit of its classification. The three forms given in this Plate, constructed of many rows of animalcules, may be distinctly associated with the Flustra and Eschara of the Bryozoa, to which, through the well-known *Lunulites* and *Orbitulites* (hitherto ranked with coral animals), they approximate in a convincing manner. Fig. 1. *Orbiculus numismalis,* from the sea-sand of the Antilles Isles. Fig. 2. *Sorites orbiculus* = *Nautilus orbiculus,* Forskål, *Nummulina* (*Assilina*) *nitida,* d'Orbigny, ? from the Red Sea. The same species lives also in the Mediterranean. In a part magnified 300 times we see the animalcule with eight feelers protruding from its cell. In some of the cells may be seen distinct shells of siliceous Infusoria; in others appear oviform globules. Fig. 3. *Amphisorus Hemprichii* closely resembles the *Sorites*; but it has cells on both sides bearing single animalcules, and hence

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* The new method of observing is the following:—Place a drop of water upon a lamina of mica, and put into it of scraped chalk as much as will cover the fine point of a knife, spreading it out and leaving it to rest a few seconds; then withdraw the finest particles which are suspended in the water, together with most of the water, and let the remainder become perfectly dry. Cover this remainder so spread out with *Canadian balsam,* the turpentine of the *Pinus* (*Abies*) *balsamea,* and hold it over a lamp until it becomes slightly fluid without froth. A preparation thus made seldom fails, and when magnified 300 times in diameter we see that the mass of the chalk is chiefly composed of minute well-preserved organisms. In this preparation all the cells of the Polythalamia appear at first black with a white central spot, which is caused by the air contained in the cells, which, as is well known, appear under water as annular black bodies; but by degrees the balsam penetrates into all the single cells, the black rings of the air vesicles disappear, and we recognize all the small cells of the Polythalamian animals, often presenting a very pretty appearance.

the discs are twice as thick as in *Sorites.* If we compare *Sorites* with *Flustra,* we may place *Amphisorus* by the side of *Eschara,* but, being both free moving bodies, they are different from them.

Plate IV. contains the *invisible animalcules of the chalk and chalk marl,* displayed in twelve specimens of rock; 1 to 9 being portions from the chalk, and 10 to 12 from the chalk marl, magnified 300 times. In these specimens the calcareous Polythalamia amount to sixteen species, and the siliceous Infusoria to twelve species, with siliceous spicula of sponges. The twelve localities from which these specimens of the rock masses were derived are the following:—No. 1 to 5, *writing chalk*; namely, 1. from Puszkary, in Poland, opposite Grodno, from the shore of the Memel; 2. from Jütland, in Denmark; 3. from the island of Rügen in Pomerania; 4. from Gravesend, on the Thames; 5. from Meudon, near Paris; *firmer* writing chalk, No. 6, from Cattolica in Sicily; *compact, not writing* chalk, No. 7, from the Mokattum hills near Cairo; and No. 8, from the Catacombs of Thebes in Upper Egypt; *compact gray limestone,* No. 9, from the mountain mass of Hamam Faraūn in Sinai, Arabia; *chalk marl,* No. 10, from Oran in Africa; No. 11, from Caltasinetta in Sicily; No. 12, from Greece.

In the *general table* indicated above, under the head of No. 13 of the contents of the memoir, a list is given of the principal forms of the invisible organic bodies which constitute the rocks from which the twelve above-mentioned specimens were taken, as well as the chalk of Brighton, the chalk marl of Zante in the Ionian Islands, and the nummulite limestone of the Pyramids of Geza in Egypt. From this it results that the principal forms in these rocks consist of twenty-five species of calcareous-shelled Polythalamia, thirty-nine species of siliceous-shelled Infusoria, seven species of soft-shelled Infusoria of the flints, and five species of siliceous plants.

The twenty-five species of calcareous-shelled Polythalamia, belonging to eight genera, are the following:—

Flustrella *concentrica*; Globigerina *bulloides*?, G. *helicina*?; Planulina *sicula*, P. **turgida*; Robulina *cretacea*; Rosalina **foveolata*, R. *globularis*?, R. **lævigata*, R. *pertusa*; Rotalia **globulosa*, R. *ocellata*, R. *ornata*, R. *perforata*, R. *scabra*, R. *stigma*; Textularia *aciculata*?, T. **aspera*, T. *brevis*, T. **dilatata*, T. **globulosa*, T. *perforata*, T. *spinosa*, T. **striata*; Turbinulina *italica*? Quinqueloculina? from Benisuef, is doubtful. N.B. *Textularia globulosa*, when in fragments, is not easily distinguished from *Rotalia globulosa*; and in like manner the fragments of *Textularia perforata* may be confounded with *Rotalia perforata.*

The thirty-nine species of siliceous-shelled Infusoria belong to fourteen genera, and are as follow:—

Actinocyclus *ternarius*, A. **quaternarius*, A. **quinarius*, A. *senarius*, A. *septenarius*, A. *octonarius*, A. *denarius*; Cocconema *Cretæ*; Cornutella *clathrata*; Coscinodiscus *Argus*, C. *centralis*, C. *lineatus*, C. **minor*, C. **Patina*; Denticella *Fragilaria*, D. *tridens*; Dictyocha *Fibula*, D. *Navicula*, D. *polyactis*, D. *speculum*, D. *stella*, D. *triangula*; Eunotia *zebra*; Fragilaria *rhabdosoma*, F. *striolata*?; Gallionella *aurichalca*?, G. *sulcata*; Haliomma *Medusa*, H. *crenatum*; Lithocampe *lineata*, L. *Radicula*, L. *solitaria*; Navicula *africana*, N. *Bacillum*, N. *eurysoma*, N. *ventricosa*, N. *sicula*; Pyxidicula *prisca*; Synedra *ulna*.

The seven species of soft-shelled Infusoria of the flints belong to three genera, and are the following:—Chætophyta *Pyritæ*; Peridinium *pyrophorum*†; Xanthidium *bulbosum*, X. *furcatum*, X. *hirsutum*, X. *ramosum*, X. *tubiferum*.

The five species of siliceous plants belong to two genera, namely, Spongia (*Tethya*?) *aciculosa*, S. *cancellata*, S. **Cribrum*, S. *binodis*; Spongilla (*Tethya*?) *lacustris*‡.

Of these principal forms the before-mentioned rocks partake in the proportions as stated below: namely,

	Species of Calcareous Polythalamia.	Species of Infusoria.		Species of Siliceous Plants.
		Siliceous, in Chalk.	Soft-shelled in Flints.	
The *Chalk* of				
Puszkary contains	6			
Rügen	7	1		
Jütland	6			
Gravesend	6	3	5	
Brighton	7	1	4	
Meudon	9	.....	2	
Cattolica	9			
The *Chalk Marl* of		Siliceous Infusoria.		
Caltasinetta	7	27	......	4
Oran	2	18	......	1
Zante	5	8	......	2
Greece	3	9	......	1
The *Compact Chalk* of				
Egypt	8			
Arabia	6			
The *Nummulite Limestone* of The Pyramids of Geza	6	Containing 4 species of Nummulite, the largest of which is one inch in diameter.		

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† *Peridinium delitiense* has hitherto been found only in flint pebbles near Delitzsch, yet accompanied with forms that are common in the flints of the chalk.

‡ In the preceding lists, the species which are marked with an asterisk * are those which most frequently occur, forming the masses of the rocks. The *Rotalia globulosa* occurs in all the localities.

On the Chalk Marl, and its relations to the Chalk and its Flints.

The whole coast of Oran in Africa appears to belong to the chalk formation, composing the plain east of the town, and extending thence to the Atlas. The marl brought from thence as tertiary by M. Rozet in great quantities I had an opportunity of examining in Paris, and I found not only Polirschiefer and an Infusoria conglomerate, but calcareous animalcules of the same species as occur in the chalk of Poland, Rügen, Denmark, and Paris, and which there mainly contribute to its mass. It thus appeared that the so-called tertiary formation of the coast of Barbary might, without much hazard, be brought into a nearer connexion with the chalk. In his description of this tract, M. Rozet states*, "The tertiary formation is extensively developed in Oran, forming the soil of the large plain on the east of the town, and on the south to the Atlas. It forms also the sea-coast to an extent of 3000 metres between Mers el Kebir and Cape Falcon, and the whole soil of the adjacent plain. The lower bed is a blue marl, like that which we found at Algiers and within the Atlas. It appears destitute of organic remains. The second or upper deposit consists of marly and calcareous beds in alternation, forming a thickness of 30 to 40 metres. In the plain these beds are apparently horizontal, as well as in the elevated plain of the Rammra hill; but in the hills south-west of the town of Kasba they are, on an extent of two hours march, inclined to the north, at an angle sometimes exceeding 30°. The beds of limestone are white and chalk-like, yellowish and coarse granular, usually forming the lower part, succeeded by others alternating with yellow marls, which are often slaty and charged with sand, and between them are found layers of ostreæ and other shells. Among them two beds are distinguished, each one metre in thickness, composed of very white finely-laminated marl, containing numerous well-preserved impressions of fishes, so that in a cubic mass of one foot we seldom fail to find three or four fishes. In these beds of marl thus enclosing the fishes, other organic remains do not appear; but in the calcareous and sandy beds which intervene, occur layers of large oysters mingled with grypheæ. The upper part of this deposit is composed of a calcareous breccia, which is exhibited at the surface in the soil of the whole plain on the south-west of Oran."

This exact description of the position and thickness of the white marl with impressions of fishes, has a reference to the

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* Rozet, *Voyage dans la Régence d'Alger*, Paris, 1833. tome 1. chap. v. pp. 56, 63.

Infusoria conglomerate of Oran, to which I have already adverted. It is probably what formed the Tripel of the earlier periods of Italy. When M. Rozet speaks (at p. 28–30) of the great extent of the tertiary tract near Algiers as similar in its relations to those of Oran, I cannot agree with him. On the contrary, forming my judgment by the organic remains, I consider the desert tract near Algiers as really composed of a tertiary formation, which reposes on chalk. This opinion is founded on my observation, that the tract in Libya, extending from Alexandria to Siwa, is composed of tertiary beds, while from Cairo to Geza the chalk formation occurs, which terminates at the granite of Syene, but is far spread into the Desert. The valley of Siwa appears to form the northern boundary of the chalk in Eastern Libya.

In the South of Italy, at Caltasinetta and its neighbourhood, the relations had been correctly seized by our late friend Frederick Hoffmann, from whose diary I have been favoured with an extract by M. von Dechen. He represents the series of strata which occupy the greater part of Sicily as composed of limestones, sandstones, clays, and marls; the lower members being probably referable to the Jura formation, succeeded by such as clearly belong to the chalk, and many beds of which perfectly resemble the hard chalk of the north-west of Germany (Teutoburger Wald). Among the marls are white chalk-like thinly laminated masses, analogous to Tripel, designated by Hoffmann as *white chalk marl*, and which especially occur in the southern part of the island. The beds of the chalk formation usually dip 20° to 30°, while the strike is nearly constant, from 15° to 45° S. of E. and N. of W., parallel to the south coast. The tertiary beds which succeed the chalk are composed of loose sand, friable sandstone, testaceous breccias, clays and limestones. They cover the chalk unconformably, resting on the truncated edges of the latter. The chalk beds are upon the whole poor in organic remains, and these are seldom distinct; there occur Hippurites, Nummulites, Lenticulites, and in a few places indistinct Ammonites and Belemnites, while the tertiary beds are quite filled with innumerable Mollusks, of which nine-tenths are still living in the Mediterranean. This distinction is so striking that it scarcely required the difference of relative position in order to draw a correct line between the two formations. Even had so circumspect a geologist as Frederick Hoffmann not correctly seized and pronounced with decision on these local relations, the numerous microscopic siliceous Infusoria with calcareous Polythalamia which I have found in the chalk marl would have led to the same conclusion.

If we compare Hoffmann's description of this portion of

Sicily with that given by Rozet of the coast near Oran, we cannot avoid recognizing a similarity of relations; and the thinly laminated marly beds with impressions of fishes, between Caltasinetta and Castrogiovanni, which Hoffmann refers with certainty to the chalk formation, correspond to the similar beds which occur near Oran, but which were said to be tertiary. And the parallel is confirmed by the microscopic siliceous Infusoria and calcareous animalcules which I have discovered in both.

The genera and species of the siliceous Infusoria in Sicily are so similar to those of Oran and Zante, that of thirty-six species, four occur in all the three countries, three in Caltasinetta and Zante, seven in Caltasinetta and Oran, while in all of them the *Coscinodiscus Patina* is greatly predominant. Of all these siliceous animals, not a single species has been found in the chalk of the North of Europe, nor even in the flints. On the other hand, the calcareous-shelled animalcules, which in the South of Europe accompany the siliceous animals, comprise about one half of the same species that are found in the North, yet exceeding them in quantity.

From the examination of the organic constituents of the chalk marl we learn the hitherto unknown fact, that numerous swarms of microscopic Infusoria were in existence within the period of the secondary formation of the earth's surface, chiefly belonging to such as possess siliceous cases or shells, and which for the greater part are members of such sections of the Bacillaria family as had previously appeared to be confined to the tertiary or newest formations.

Of the thirty-nine or forty species of siliceous Infusoria occurring in the chalk formation, thirty-four or thirty-five have not hitherto been found in the recent state; but it is remarkable that the remaining five or six species so closely resemble existing species of the present day, that they present no peculiar character by which they could be distinguished from them, and hence the application of new names appeared inadmissible. They are, *Eunotia zebra*, *Fragilaria rhabdosoma*, *F. striolata ?*, *Gallionella aurichalca*, *Navicula ventricosa*, *Synedra ulna**.

In the chalk itself only four out of the thirty-nine or forty species of siliceous Infusoria have hitherto been met with,

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* The indifference shown to climate by Infusoria, and the peculiarity of their organic development, seem to render it possible that they might be more readily preserved through many catastrophes of the earth than other forms. By the faculty which they possess of spontaneous division, a single individual can, under very favourable circumstances, be multiplied in the course of a few hours to the extent of millions.

namely, *Fragilaria rhabdosoma*, *Fragilaria striolata ?*, *Gallionella aurichalca*, and *Pyxidicula prisca*. They are very rare, and found only in the vicinity of the beds of flint.

On the Composition of the Compact Limestone of Upper Egypt and Arabia by the invisible Animalcules of the White Chalk of Europe.

Both the nummulite limestone of the pyramids of Geza on the left bank of the Nile, and the same kind of rock on the right bank near Cairo, contain numerous microscopic animalcules of the chalk, which serve as a cement to the Nummulites. I had often examined microscopically specimens which I had brought from thence, but I did not succeed in separating and rendering visible the different elements with equal clearness, until I applied my newly-acquired practice, which was much facilitated by immersing these stones a longer time in water. The same result attended the examination of the other calcareous rock masses of Upper Egypt and Arabia, showing that the animalcules of the chalk occupy in a surprising manner a wide extent of country in Libya.

Nummulite limestone, wherever occurring, has been most usually referred to the tertiary period, although perhaps often belonging to the chalk. In Egypt it possesses no great extent. On the right bank of the Nile it is deposited only in the small hills near Cairo, and on the left bank, as it appears, in a tract extending from Siout to the declivity of the compact limestone, which latter constitutes the mass of the rocks that line the course of the Nile in Upper Egypt. It forms the foundation and principal material of the Pyramids. Northward it is directly bordered by the slimy delta of the Nile, the productive soil of Egypt. Between the Oasis of Jupiter Ammon and the Mediterranean, is a wide elevated plateau or tableland of rock, among whose numerous organic remains are known tertiary forms. The whole of Upper Egypt, as far as Syene, has a similar character. In 1828, though assured that its limestone rocks were more ancient than the tertiary period, yet, from want of distinct fossils, I was doubtful whether they might not be referred to the Jura formation. On the south, and not far from Syene, this limestone is incumbent on sandstone (Quadersandstein ?), and the two repose on granite and the primary rocks connected therewith. I gave these views in 1828 in the geologically coloured map which accompanied the first section of the first volume of my Travels in Egypt, Libya, Nubia, and Dongola.

It now results, from the microscopic examination which has taken place, that the whole of the limestones of Benisuef, Siout and Thebes, on the western bank of the Nile, and of Cairo and Kineh (including the gray marl near Kineh), on the eastern bank, and which inclose the Nile at an elevation of frequently 100 to 300 feet above its level, extending along the river full sixty German miles in length, are, like the Nummulite limestone, composed of an inconceivable accumulation of microscopic calcareous-shelled animalcules, which are of precisely the same genera and species as those which constitute the chalk of Europe. The table-land formed by these calcareous rocks extends far westward into the Desert, and it is perhaps principally composed of them.

A new and unexpected light is thus thrown on these extensive regions. The phænomena apparent in Egypt may be connected with those of Western Africa. It has been already shown that the same animalcules constitute the territory of Oran, stretching far along the foot of the Atlas; and when we consider the equality of surface which prevails in the plain of the Great Desert, or Sahara, of the North of Africa, and compare it with what I have myself seen along the whole extent of its eastern border, as well as on a large portion of its northern, we may be well permitted to think of a similarity of composition.

But these distinct indications of a similar organic influence extend not merely to the west but also to the east of Cairo, expanded into Asia. The specimens collected by Dr. Hemprich and myself from Hamam Faraūn, and Tor in the Sinaian portion of Arabia, which I had formerly considered as ash-gray marl and yellowish-gray limestone of the tertiary epoch, were now proved, by the new method of examination, to consist of quite the same microscopic chalk animalcules as constitute the hilly masses of Upper Egypt. And from hence this formation appears to be continued eastward far into the interior of the Great Desert plain, trending toward Palestine; but on the Arabian coast of the Red Sea we did not find it further south than Tor, which locality alone, among all the points of the east, yielded flints similar to those which occur in the European chalk.

We have here to remark on the absence of siliceous animals in this limestone and marl formation, while the so-called Egyptian pebbles and jaspers occupy the same position in horizontal layers as the flints in the North of Europe, appearing as their substitute. But in these jaspers the organic siliceous elements are no longer to be distinctly found by reason of their intermixture with other substances, and their consequent opacity, giving rise to dendritic and other delineations. It seems as if the solution and conversion of the organic into the inorganic in the Egyptian pebbles (*Cailloux d'Egypte*) is throughout more perfect than it is in many flints, although the constituent elements of both kinds of stone are very probably quite the same.

On the principal Organic Calcareous Forms which compose the mass of all Chalk.

From what has been already stated, it is evident that the production of the calcareous mass of the chalk is not to be attributed, as formerly conceived, to the larger organic bodies, but to the minuter, and in the greatest measure to such as are invisible, consisting of eight genera of Polythalamia with twenty-five species, and excluding all such as may be distinguished by the naked eye, that is, exceeding $\frac{1}{24}$th of a line in magnitude; the latter, however, are comparatively rare. It is possible that several other, and perhaps many species of the same genera, may yet be discovered in the chalk, as well as other genera, since the investigations hitherto made could only be applied to a minimum of its substance; yet, as these were conducted by me on chalk from many regions, it does not appear probable that other sections of the animal kingdom will be found to have taken so great a share in the formation of chalk as the Polythalamia, the principal prevailing forms of which I have indicated.

From the preceding it is also apparent that the chalk rocks of all countries agree in their constituent organic forms not only according to the zoological class, but also in genera, and for the most part in species likewise; this character being not confined to the white tender writing chalk of Europe, but extending also to the compact limestone rocks of the North of Africa and the West of Asia. Particularly striking is the characteristic persistence of single forms through all these different and widely-separated countries. Thus in all of them are to be found *Rotalia globulosa*, with *Textularia globulosa*, *T. aciculata ?*, and *T. striata*, as well as *Planulina turgida*, thus giving a common character to all these rock formations; and this character becomes the more important, when we consider that these forms are the most numerous, and in fact are the chief constituents of the chalk*.

If now the question be asked whether the forms which occur in such masses in chalk belong to it exclusively, and are hence to be considered characteristic of that formation, I am almost disposed to reply in the affirmative. The analogous forms

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* The Polythalamian forms which Mr. Lonsdale noticed in the English chalk in 1837 as visible to the naked eye, and amounting to 1000 in one pound of the chalk, and which, with Mr. Lyell, he has named *Lenticulina*

which occur in sea-sand, tertiary sand, and indeed in all modern formations, are viewed for the most part as different and larger species, although of the same genera; and it does not appear that any of these forms can be referred with perfect certainty to such as are now living in the sea.

To the theory of the formation of limestone, the observation is important, that these organic deeply-seated relations are not peculiar to the chalk formation. The tertiary calcareous beds consist, in like manner with the chalk, of multitudes of such Polythalamian animals, which compose in many quarters sandy sea-downs of great extent; and even in the sandy desert of Libya we can recognize distinct Polythalamia. On the other hand, having succeeded in discovering microscopic Polythalamia in the compact flints of the Jura limestone from Cracow, which are of decidedly different forms from those of the chalk, the calcareous animals being *Nodosaria urceolata*, n. sp., and *Soldania elegans*, n. sp., and the siliceous *Pyxidicula prisca?*, with *fragments of soft sponges*, it becomes apparent that such invisible organic bodies were also present in the formation of the Jura limestone.

[To be continued.]

LXIII. *Notice of an undescribed Native Subsulphate of Iron from Chili. By* John Prideaux, *Esq.*[*]

THIS specimen, of which I have not found any description, was brought to Sir Charles Lemon's Mining School by Edward Hookham, one of the students, having been sent from Chili to Captain N. Vivian, by his son, without any geological reference.

Form, mammillary, or curved lamellar about one-sixth of an inch thick. *Structure*, fibrous parallel, transverse to the laminæ; *fracture* corresponding; fibres crystalline, but too minute to be easily definable. *Brittle* in mass, fibres rather flexible. H 2·5; specific gravity below 2·5.

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and *Discorbis*[†], appear, judging by the figures, to be referable to *Rotalia ornata* and *R. globulosa*, including perhaps fragments of *Textularia globulosa*.

I may here remark, that my continued researches on the Polythalamia of the chalk have convinced me, that very frequently in the earthy coating of flints, which is partly calcareous and partly siliceous, the original calcareous-shelled animal forms have exchanged their lime for silex, without undergoing any alteration in figure, so that while some are readily dissolved by an acid, others remain insoluble; but in the chalk itself all similar forms are immediately dissolved.

[*] Communicated by the Author.

[†] Dr. Buckland's Bridgewater Treatise, 2nd Edition, vol. i. p. 448. 1837. Lyell's Elements of Geology, 1838.

Lustre shining, silky on the fibrous face; dull on that of the laminæ. *Transparency* of the laminæ 0, fibres translucent. *Colour* pale greenish gray; externally yellowish, from adhering sulphur. *Taste* slightly acid and astringent; *inodorous.*

Solubility. In cold water little or none; but the fibres fall asunder: on applying heat, the colour changes to orange, and the hot water dissolves a portion, acquiring its taste. In muriatic acid swells like sponge, assuming a rich orange colour, and soon dissolves, except a little residue of sulphur and earth.

By dry heat, in tube closed at one end, the fibres separate and become orange-coloured, giving off much water and a little sulphur: by increased heat, sulphuric acid is driven off, and the residue left red. In open tube, much the same: on charcoal, before the blowpipe, exhales the odour of sulphur; and shrinks exceedingly, leaving a residue of oxide of iron.

By analysis,

		Oxygen.
Peroxide of iron	31	= 9·5
Sulphuric acid	26	= 15·5
Water	33	= 29·
Sulphur, earth and loss	10	
	—— 100	

the acid containing 1·5, and the water three times the oxygen of the base, formula $2\ Fe_2\ O_3 + 3\ S\ O_3 + 18\ Aq$, containing half as much acid as the persulphate of iron, and a little excess, to which the sapidity is probably due.

Its aspect and composition suggest the name Fibroferrite.

J. PRIDEAUX.

LXIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

May 27, 1840.—THE memoir "On the Classification and Distribution of the Older Rocks of the North of Germany," &c., by Prof. Sedgwick and Mr. Murchison, commenced at the previous meeting, was concluded*.

In an introduction of considerable length, the authors enter on a historical review of the different steps by which they had been led, during the former year, to place nearly all the older slates of Devonshire, and a considerable part of the slate rocks of Cornwall, in a group intermediate between the carboniferous and Silurian systems, and therefore coeval with the old red sandstone of Herefordshire. To the vast group of slate rocks, so defined, they proposed the name of Devonian System; and their leading object in visiting Belgium, the Rhenish provinces, the Hartz, &c., was to ascertain whether in any of those countries there was a group of strata (no matter of what mineralogical character) with Devonian fossils, and in a position

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* See L. E. and D. Phil. Mag., vol. xvii. p. 508.—EDIT.

intermediate between the carboniferous and Silurian systems. Should such a group exist on the continent, then would the Devonian system be established, not merely on plausible arguments derived from its suite of fossils, but also on the more direct evidence of natural sections.

With these views the authors endeavoured (1.) to ascertain the natural descending order of the formations on the right bank of the Rhine, between the Westphalian coal-field and the chain of the Taunus. (2.). To ascertain the same order in Belgium, and among the ancient rocks on the left bank of the Rhine, north of the Hundsruck.

In the course of the summer they also made (with similar objects) several traverses through the Hartz, and one long traverse from the Thuringerwald to the north flank of the Fichtelgebirge, in the hope of bringing into relation with their previous observations, the country which has become so celebrated from the labours of Count Munster.

The authors follow this order in the descriptive parts of their paper. But before commencing their detailed sections, they explain at some length the method of determining the order of superposition among rocks, like those of Belgium and the Rhenish provinces, which are not only much contorted, but often in a reversed position. This order of superposition can be made out only by sections, which are of two kinds, *vertical* and *horizontal*. *Vertical sections,* where the strata are not inverted, not only indicate the natural group of strata, but their true order of superposition, both of which may often be ascertained on a single line of traverse. But *horizontal sections,* showing the intersection of successive groups of strata with the actual surface of the country (and represented by the colours of a geological map), can only be examined by following the lines of strike. The colours of such a section (if derived from strata originally conformable) must show the masses, however contorted, in their true juxtaposition. Hence we may define from the horizontal sections of a country a true consecutive geological series; and if the relative age of any two contiguous terms be known, the relative age of all the other members of the section may be inferred with certainty, though the formations be in an inverted position, as seen on the line of any one vertical section. It was by this laborious method of "horizontal sections," or, to use his language, by determining the symmetry of position of the several formations, that Professor Dumont first disentangled the perplexing phænomena of the Belgian provinces*. The authors, after acknowledging the great value of this principle, state that they endeavoured never to lose sight of it in estimating the value and interpreting the meaning of the many vertical sections they examined in their long traverses through the provinces they describe.

§ 1. *Coal-fields of Westphalia, &c.*—The authors commence their descending sections, on the right bank of the Rhine, with the productive coal-field, which occupies an irregular triangular area, bounded towards the north by greensand and cretaceous deposits,

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* See L. E. and D. Phil. Mag., vol. xvii. p. 303.—Edit.

towards the south-east by older formations, afterwards to be described, and towards the south-west by an irregular line, skirting the low country near the Rhine, and passing near Mulheim, Ketwick, Werder, and thence to a point a few miles north-east of Elberfeldt. In its lithological character and fossil contents it is not to be distinguished from the coal-fields of England. It is affected by many anticlinal and synclinal lines, which have brought a lower and unproductive portion to the surface, and thrown the productive portions into a number of irregular troughs, ranging in the direction of the strike, east-north-east.

The lower and unproductive coal-field is composed in part of coarse grits, well exposed on the banks of the Ruhr, between Herdecke and Schwerte, and of yellowish and light-coloured sandstones and grits, with thin seams of coal and impressions of plants; and the strata are underlaid by dark gray micaceous slates and thin-bedded hard sandstones, of great thickness, marked by many obscure impressions of small plants. The lowest member of this series contains much dark pyritous shale (*Alaun Schiefer* of the Germans), and reposes on the upper calcareous zone of Westphalia (mountain limestone). Several sections are described which confirm this order of superposition. The authors then state that this lower division of the coal-field is greatly expanded towards the north-east; that it is lithologically almost identical with the great *culm-field* of Devon, and resemble it also in its numerous impressions of small plants. It is the *Flötzlehrer Sandstein* of the German geologists, and had been regarded by them as the highest member of the graywacke series; but in the recently published map of Von Dechen, it is placed on the parallel of the millstone grit of England.

§ 2. *Carboniferous limestone of Westphalia* (*Berg-Kalk*), *Kiesel Schiefer, and bituminous limestone, &c.*—The authors next describe the limestone which commences at Cromford, near Ratingen, and ranges east-north-east to Velbert. Thence deflecting to the valley of Regrath, north of Tonnesheide, it is cut off, and does not form a continuous band (as represented in all the German maps), with a lower limestone, which commences a few miles further south, and ranges through Metman to Elberfeldt. Near Cromford the limestone is thick-bedded, and in its structure and fossils resembles the great scar-limestone of England. For proofs the authors give several detailed sections, and quote published lists of fossils. In its range to the east it becomes more cherty, and abounds in casts of the stems of Encrinites, so as to resemble the screw-stones of Derbyshire. At several places (*e. g.* Isenbugel, Velbert, &c.) the connexion of the limestone with the upper series is well exposed. The upper beds of limestone pass into dark flat-bedded flinty slate, which is overlaid by psammite and shale, with thin courses of flinty slate, and these dip under the lower members of the coal-field. Again, there is at Velbert a clear proof that the limestones, screw-stones, and flinty slate, dip under the alum-slates of the neighbourhood.

Following the strike of the county still further to the east, the limestone range loses its mineral character; but a large group of strata (dipping under the alum-slates above-mentioned, and resting on dark shales, like those which form the base of the limestone) occupy the exact place of the carboniferous limestone in the transverse sections. The group is characterized by dark flinty slate (*Kiesel Schiefer*) and dark and often fetid thin-bedded limestone, and so closely resembles the culm-limestone series of Devonshire, that the description of one formation might almost serve for the other. Like the culm-limestone, it also contains many Goniatites and Possidoniæ; and among the latter, the *Possidonia Becheri* of Devon. It wants, however, the numerous species of mountain limestone fossils of the beds above-noticed, a fact which the authors explain by a reference to a change in all the physical conditions of the deposit. This group, following all the sinuosities of a most contorted country, and sometimes doubled back upon itself for many miles together, may be traced by its *Kiesel Schiefer* and Possidonia schists, and sometimes by its black fetid limestones, to the eastern limit of the chain of older rocks near Bleiwasche and Stadtberge.

§ 3. *Devonian System.*—The authors next describe the rocks immediately inferior to the carboniferous groups. The mountain limestone of Cromfort, above-described, rests on dark-coloured shale, but the descending section is much obscured by overlying deposit. In the long range of the same series, from Elberfeldt to Menden, there are many clear transverse sections, exhibiting in greater or less perfection the following *descending order.*—(1.) Immediately under the lower limestone shales are many reddish bands, with calcareous concretions, in which the Possidonia and some of the species of the superior groups are still found. (2ndly.) These are succeeded by a well-marked range of psammites and coarse flagstone. (3rdly.) From beneath the psammites rise a series of shales, and bands of psammite of dark colour, with here and there thin courses of inferior limestone, in which we find flattened Goniatites, and shells of a species different from those of the overlying formations, among which especially is noticed the *Terebratula aspera* of Schlotheim. These are, therefore, considered as forming a part of an inferior system, and the first and second groups of the section may be regarded as made up of beds of passage between the carboniferous system and that which is below it. The sequence here given is compared with the highest beds of the Devonian series, immediately under the culm measures, and with the yellow sandstones of Ireland described by Mr. Griffith.

3 *a*. *Lower Limestone of Westphalia, &c.*—This limestone rises immediately from below the third group of the preceding section. Its range (from the neighbourhood of Ratingen, in the valley of the Rhine, to the confines of Hessia) is described in detail. Its changes of mineral structure—its separation here and there into two zones—its contraction in one place and its great expansion in another—its enormous flexures and occasional inversions of position—its re-appearance at Warstein and Attendorn, in consequence of such flexures,—all these phænomena are noticed in their turn. As a whole, it has so great a resemblance to the limestone of South Devon, that

through large tracts of Westphalia the two could not, by a series of land specimens, be distinguished from one another. The fossils of this limestone are very abundant, and several sections are given in detail, to show their local distribution. Among the most characteristic and abundant in these sections the following are enumerated: *Stromatopora polymorpha*, *S. concentrica*, *Favosites ramosa*, *Favosites polymorpha*, *F. spongites*, *F. Gothlandica*, *Strygocephalus Burtim*, *Gypidium*, *Terebratula aspera*, *Turritella coronata*, *T. bilineata* (Schlotheim), *Buccinum spinosum* (Sowerby), &c. &c. From all these facts, it is inferred, that this lower limestone of Westphalia is a true Devonian limestone, exactly or very nearly on a parallel with the great limestone of South Devon.

Local and detailed lists are added, and detailed sections are given, connecting the whole series both with the upper and lower formations, especially one from the Possidonia schists and black limestones, near Schelke, through the Devonian limestone, and to the lower formations exposed on the banks of the Lenne, towards Altena. In this section there is no ambiguity, and the defective evidence in the sections of Devonshire, when we endeavour to connect the culm-measures with the South Devon limestone, is here amply supplied.

The authors then describe in detail the sections at Paffrath, near Bensberg, on the right bank of the Rhine, near Cologne, where the same Devonian limestone occurs, with a magnificent series of fossils; its position is, however, reversed, as it seems to dip under the limestone near Bensberg, which is referred to the upper part of the Silurian system.

To the same geological epoch the authors also refer the complicated metalliferous deposit of Dillenburgh, and the limestones of the Lahn in the country of Nassau. At the former place the great contortions and the extraordinary intrusions of trappean rocks make the relations difficult; but, considered on a great scale, the vast fossiliferous and calcareous group reposes on rocks considered of the Silurian age: it contains a true Devonian group of fossils, and its upper portion at Herbon is surmounted by a Possidonia schist, perfectly identical with that of Westphalia. The limestones of the Lahn at Dietz, Weolburg, Wetzlar, &c., are still more unequivocally Devonian; and though the alternating masses of limestone and schist are of enormous thickness (rivalling in that respect the whole series of limestones and slates in South Devon), and the sections often obscure, yet in descending the Lahn from Dietz to Nassau and Bad Ems, they had a proof that the calcareous system is underlaid by Silurian rocks. The appearance of these Devonian deposits near the eastern limit of the old rocks, on the right bank of the Rhine, is accounted for by enormous undulations, which have repeated over again, in three or four great parallel troughs, the formations which appear in their true place in Westphalia, on the northern limit of the same ancient formations.

§ 4. *Silurian System.*—The authors next describe the great series of rocks which rise from beneath the lower Westphalia limestone, and state that in the long range from Elberfeldt to Iserlohn the descending order is unequivocal. The passage downwards is sometimes effected by flagstones, with bands of shale, containing thin calcareous courses. In other places, the shales are more abundant, occasionally becoming much indurated; and in the range towards the north-east (for example, near Meschede) this group becomes greatly expanded, and contains many quarries of roofing-slate, with a true oblique cleavage. This part of the series is compared with the shales under the Eifel limestone, and with the Wissenbach slates, which underlie the Devonian limestone series of Dillenburg. The great difference in the development of this group produces a great difference in the fossils, but on the whole, they are regarded as forming a passage between the Devonian and Silurian types. A list of fossils is subjoined, and the authors regard the numerous Goniatites as rather connecting the group with the overlying Devonian rocks; while the trilobites and orthoceratites, &c., some of which cannot perhaps be distinguished from known Silurian fossils, seem to link it to the Silurian system.

Below the preceding comes a group of vast thickness, composed of earthy schistose beds, passing on one hand into shale, on the other into coarse slate, and alternating indefinitely with bands of psammite, sometimes passing into coarse arenaceous flagstone, sometimes into thick beds of sandstone. Nearly throughout are occasional obscure vegetable impressions, and in the upper part are courses of limestone and calcareous bands, with innumerable impressions of fossils. In the lower part, the limestone bands seem gradually to disappear, and the whole passes into a formation of graywacke and graywacke slate, in some rare instances producing a good roofing-slate. For many miles south of the undisturbed range of the lower Westphalia limestone, the prevailing dip is about north-north-west. The country round Siegen is regarded as a kind of dome of elevation, composed of the lower part of this series; for still further south the dip is reversed to the south-south-east; and in a traverse from Siegen to the Taunus, across the strike (a distance of about fifty miles), the same dip is continued, with very few interruptions. Considering their high inclination, this fact seems to give an almost incredible thickness to the deposits in question. But the vertical sections do not give the order of superposition; for at Dillenburg, and on the Lahn, two great Devonian troughs are brought in among the older strata, without any general change of dip; and if we accepted the vertical sections as the sole proofs of superposition, we must place the Devonian and a part of the carboniferous series under the chain of the Taunus. The authors therefore endeavoured to apply the method of Professor Dumont, and found their results confirmed by the sections of the lower Lahn.

Many other local details are given, and the authors having determined the geometrical position of the great mineral masses, next attempt to define their age from their fossils. In the arenaceous and calcareous group under the lower Westphalia limestone, many species of the genus Pterinea (Goldfuss), Homalonotus, Orthis, &c. &c., begin to prevail. Along with these are forms at present unknown in England, *e.g.* Hysterolites of Schlotheim, and two species of Delthyris,—*D. macroptera* and *D. microptera* (Goldfuss). The same groups of fossils are found on the banks of the Rhine; and in a quarry near Unkel are many fossils of the genus Orthis, among which were *O. pecten*, *O. flabellula*, *O. rugosa*. Along with them was *Terebratula Stricklandii*, and the group was considered characteristic of the lower Silurian rocks of England.

On a review of the whole evidence, the authors place this vast succession of strata in the Silurian system, without professing to separate the several parts into distinct groups, on a parallel with the several groups of the Silurian system of England. This is forbidden by the absence of distinct calcareous bands, and also by the great vertical range of some of the fossil species, which are found almost from the highest beds to the lowest of the whole series. Several lists of fossils are then given, in confirmation of these general views; and it is thence concluded, that the great sequence of coarse earthy schists, calcareous bands, arenaceous flagstones, psammites, &c., are the representatives of the upper Silurian system, and that the lowest quartzose, graywacke, flagstone, roofing-slate, &c., which in some places have no fossils, and in others have numerous repetitions of a few species of the genus Orthis, belong to the lower Silurian, or upper part of the Cambrian Systems.

Part II. *Older formations on the left bank of the Rhine.—The Hartz.—Upper Franconia, &c.*

§ 1. The authors commence with a short description of the physical region extending from the coal-field of Belgium to the south-eastern flank of the Ardennes, and then in like manner describe the country between the same coal-field and the limestone of the Eifel. They afterwards discuss, at some length, the methods used by Professor Dumont to determine the superposition of the natural groups; and partly from considerations derived from the symmetrical arrangement of the mineral masses, and partly from the direct evidence of sections, especially in the Eifel country, show that the geological sequence has been correctly determined. So far adopting the views of Professor Dumont, the descending order in the provinces above-mentioned is as follows:—

(1.) Coal country..........	*Terrain Houillier.*
(2.) Anthraxiferous country.	*Terrain Anthraxifère.*
(3.) Slate country.........	*Terrain Ardoisier.*

The second of these divisions is subdivided into four natural groups or systems, viz. Upper calcareous system; Upper quartzo-schistose system; Lower calcareous system; Lower quartzose-schistose system.

The slate country is also divided into three groups,—Upper, Middle, and Lower.

The order being assumed as fixed, the next question is as to the British equivalents of the successive divisions or subordinate systems.

Respecting the Belgian coal-field, there is no doubt: it is on the same horizon with the great coal-fields of England. Through a considerable part of its south-eastern boundary it is inverted, so as to dip under the older formations; but on a part of its northern boundary the older formations emerge in their regular order.

The upper limestone of the second division is undoubted mountain limestone. The only question, then, is respecting the equivalents of the three lower divisions of the *Terrain Anthraxifère*, which are, by Professor Dumont, respectively classed with the Ludlow rock, Wenlock limestone, and Caradoc sandstone formations. This classification is not accepted by the authors, for reasons stated in detail.

The *upper quartzo-schistose system* is separable at two parts very different from one another: the higher, often characterized by an open-grained yellowish psammite; the lower (with many variations of structure, and with occasional subordinate calcareous bands) abounding in a dull greenish-gray earthy schist, not unlike the "mudstone" of the Ludlow rocks. But the higher grits and psammites pass insensibly into the bottom beds of the upper limestone (mountain limestone), and contain a series of fossils so near the carboniferous type, that it is difficult to draw a line between the two deposits; and the lower earthy schists do not contain (among the specimens brought away by the authors) one single species found in the Ludlow rock.

The *lower limestone* of the second division is then described in detail, both as seen in Belgium and the Eifel. The authors dwell some time on the remarkable association of the Eifel dolomites with volcanic rocks of different ages: but they contend that the dislocation and contortions of the older strata, and their changes of mineral structure, are not generally due to the more recent igneous eruptions. A comparison of the lists of fossils from the Eifel and lower Belgian limestone, show that they belong to a group identical with that of the lower limestone of Westphalia and the limestone of Paffrath, and that they present the closest analogies with the fossils derived from the limestones of South Devon; some of the most abundant species, both of shells and corals, being identical in all the localities. Hence the authors conclude, that the second and third members of the *Terrain anthraxifère* of Professor Dumont form a part of the Devonian system, and not a part of the Silurian system.

Lower quartzo-schistose system.—In Belgium it is harder and more quartzose than the upper division, and also of more varied mineral structure; and in its upper portion contains some thick beds of conglomerate, which, from their mineral structure and the supposed analogies of the lower limestone with the mountain limestone of England, have been classed with the old red sandstone. Without attributing any value whatever to these conglomerates, as terms of comparison with English formations, and regarding them only as mineral accidents, the authors place them near the base of the Devonian system, and consequently near the lower limit of the old red sandstone.

In the Eifel, the system is better developed and more fossiliferous, and exhibits the following descending order: (1.) Calcareous shales, forming the base of and passing into, the limestone; (2.) Indurated shales, alternating with sandstone and flagstone, occasionally of a reddish colour; (3.) Sandstone, flagstone, arenaceous slate, quartzite, &c., gradually passing into a slate formation. The authors refer to various lists of fossils, and conclude that, though several species are in common with those of the overlying Devonian system, yet that as a group they are distinct: 1st. Because the carboniferous species disappear; 2ndly. Because some of the most characteristic species of the lower limestone (such as the *Strygocephalus Burtini*, &c.) are wanting; 3rdly. Because new (and Silurian) types begin to abound; more especially shales of the genus *Pterinea*, several species of *Orthis*, the *Homalonotus Knightii*, *Calymene Blumenbachii*, &c. They further remark, that the species of Silurian fossils which appear in the Eifel lists are mostly derived from the lower shales, which are regarded as beds of passage. Along with known Silurian fossils there occur also (as remarked in deposits under the Westphalian limestone, Part I.) many other fossils, *Delthyris microptera* and *D. macroptera*, &c., in great abundance. The lower quartzo-schistose rocks of Professor Dumont are therefore placed in the Silurian system, but without any attempt to subdivide it into distinct portions analogous to those of England. And as there is no well-defined separation between this system and the overlying Devonian, still less is there any well-defined separation between its lower limits and the central slate rocks of the Ardennes.

The slate country of the Ardennes is subdivided into three groups of slate rocks,—Upper, Middle, and Lower. All the fossils obtained by the authors from the upper group are of Silurian types. From the middle and lower groups they obtained no fossils: but as all the groups are linked together, and the upper is placed by its fossils in the lower part of the Silurian system, they assign the two lower groups to the upper Cambrian system. They then enter on some mineralogical details connected with the structure of the slates of the Ardennes; and among the crystalline beds of the lowest group (which they regard as only an altered portion of that which is next superior), point out some examples of slates derived from a cleavage transverse to the beds, and intersected by a true second cleavage plane, a rare phænomenon among the slates of England; but noticed by the authors among some rocks on the south coast of Devon and the north coast of Cornwall.

§ 2. *Formations between the Eifel and the Hundsruck.—Left bank of the Rhine, &c.*—Crossing the strike of the beds from the Eifel to the Moselle, by several distinct traverses, the authors met with the same series of deposits in descending order: viz. 1st. Calcareous shales; 2nd. Arenaceous flagstones and shale; the upper part frequently exhibiting a reddish tinge, and with portions more or less calcareous, and the lower part passing into a great formation of arenaceous flagstone, indurated slate and coarse slate, and occasionally of fine quartzite. The series is here and there highly fossiliferous, containing several species of Pterinæa, Delthyris, Orthis, &c.,

occasionally presenting obscure impressions of plants, and casts of a large Homalonotus, of a Silurian species. The sequence, determined more by the symmetrical position of the great mineral masses than by direct superposition, as seen in vertical sections, gradually passes into rocks of a more decidedly slaty structure, and almost without fossils. Passing to the right bank of the Moselle, and in the same way making traverses through the chain of the Hundsruck (which is elevated on the line of strike, *i. e.* east-north-east), they again had an ascending series, and thence concluded that the whole chain was only a portion of the great system under the Eifel limestone in an altered form. The Silurian fossils discovered among the crystalline quartzites and schists of the chain (*e. g.* one or two species of Orthis, a large winged Delthyris, &c.), confirmed this view. Hence also the chain of the Taunus, which is the physical prolongation of the Hundsruck, must be referred to a similar place in the general series; a conclusion at which the authors also arrived from an examination of the sections on the right bank of the Rhine, though obscured by the enormous development of masses of contemporaneous trap (*Schaalstein*).

The authors then give some details respecting the trappean and volcanic rocks on both banks of the Rhine, and conclude, that the quartzite and chlorite slates, &c. of the Hundsruck and Taunus are but altered forms of a great Silurian group under the Eifel limestone; and that the causes which at an ancient epoch dislocated, contorted, and mineralized the strata, have perhaps not yet entirely ceased, and that the hot springs of Wisbaden and bubbling fountains of Nassau, may be referred to their last expiring efforts.

On a review of all the facts stated in this and the preceding Part of their communication, the authors conclude, (1.) That from the carboniferous deposits of Westphalia and Belgium, to the lowest fossiliferous deposits of the Rhenish provinces, there is a great and uninterrupted series of formations, which are in general accordance with the British series, though the subordinate groups do not admit of direct comparison; (2.) That, considered in a broad point of view, the natural successive groups of strata and the natural successive groups of fossils, are in general accordance; but as the boundaries of the physical groups are ill defined, so also the boundaries of the fossil groups are ill defined, and pass into or overlap one another; (3.) That as there are no great mineralogical interruptions, producing a discontinuity and a want of conformity among the deposits, so also there seems to be no want of continuity among the groups of the great palæozoic series of animal forms. If the extreme terms be compared together, all the objects are dissimilar; but if the proximate fossil groups be put side by side, there are many points of resemblance, and many of specific agreement; (4.) That the Devonian system is, therefore, a natural system, not merely made out, as in England, by a plausible interpretation of fossil evidence, but defined, in the Rhenish provinces, both by its group of fossils and its place in a true descending section. And as the old red sandstone of Herefordshire passes on the one hand into the carboniferous limestone, and on the other into the upper Silurian rocks, without interruption, it follows, that the Devonian system, as above defined, is contemporaneous with, and the representative of, this old red sandstone.

§ 3. *Chain of the Hartz.—Fichtelgebirge, &c.*—The general strike of this chain is nearly the same as in the provinces before described (*i. e.* east-north-east), and therefore almost at right angles to the prevailing direction of the chain, as laid down on a geological map. The mineral structure and the fossils are also nearly the same, and the numerous contortions throw the same difficulties in the way of determining the true order of superposition; and these difficulties are greatly increased by protruding masses of granite, which have not only altered the structure of all the neighbouring rocks, but literally broken the chain into fragments, several of which are thrown into a reversed position.

The igneous rocks of the region are stated to be of four kinds: (1.) Trappean rocks in beds and protruding masses, nearly on the line of strike; (2.) Granite, sending veins into the older slates and trappean rocks; (3.) Quartziferous porphyry in masses and dykes, identical in structure, and apparently in relation with the Elvans of Cornwall; (4.) Trappean rocks (*melaphyre*, &c.), associated with the *rothe todte liegende* and coal-measures on the south-eastern skirts of the chain.

Silurian fossils are found in several parts of the Hartz, but the authors saw no rocks which they could compare with the central slates of the Ardennes, or the oldest slates of the Rhine; but they give two sections which ascend into a higher system. The first is from Huligenstein to the neighbourhood of Clausthal, and appears to give the following ascending order:

(1.) Devonian limestone, well characterized by its fossils.

(2.) A series of psammites and shales, with one or two species of Possidonia.

(3.) A series of coarse sandstones and grits, surmounted by shales and psammites highly charged with plants, and mineralogically resembling the Devon culm-beds.

Plants are, however, found below the Devonian limestone, and even thin bands of culm; and the section is obscure: but if the interpretation given to it be correct, a part of the country near Clausthal rises into the carboniferous series.

Their next section commences with the limestone of Ebingerode (on the south side of the Brocken mountain). The limestone abounds with Devonian corals and other fossils, and some parts of it cannot be distinguished from the lower limestone of Westphalia. Other parts of it are pierced with trappean rocks and are overlaid by ferriferous deposits; in which respects, as well as in its fossils, it is strictly analogous to the Devonian limestones of Dillenburg and the Lahn. Again, the ferriferous bands are overlaid by black shale, containing *Kiesel schiefer*, and (if we are not misinformed) containing Possidonia schist. The analogy presented with the uppermost part of the Devonian series in Westphalia seems, therefore, perfect.

From these facts the authors conclude, that the older rocks of the Hartz are chiefly Silurian and Devonian, with a few traces of the lower carboniferous.

If the great contortions and strike of the Rhenish provinces were produced contemporaneously with those of the Hartz, then must the great derangements of the Hartz have taken place *after* the deposit of the Belgian and Westphalian coal-fields. But the principal dislocations of the Hartz must have taken place *before* the deposit of the red conglomerates, sandstones, coal-beds and trappean masses, which rest on its eastern flank. Hence the authors conclude, that none of these red conglomerates are of the date of the old red sandstone; and that the coal-beds belong to the highest part of the carboniferous series, where it passes into the base of the new red sandstone.

Lastly, The authors describe a hasty traverse, made from the Thuringerwald through the forest of Upper Franconia, and thence to the north flank of the Fichtelgebirge. On the northern limits of the section (the strike and many accidents of position remaining as before) were rocks with a true slaty cleavage, which might (at least mineralogically) be compared with the upper slates of the Ardennes; and further south, the analogy was confirmed by bands of limestone, with stems of Encrinites, but with very few other fossils. Still further south occur a few impressions of plants, and the whole system appears to be at length overlaid by a series of limestones and schists, some of which are very rich in fossils. One of these zones of limestone (the lowest according to Count Munster) rests on calcareous slates, containing a *cardiola* of the upper Ludlow rock. It is in this zone that the Clymenia is most abundant. Goniatites, Orthoceratites, &c., are abundant in a higher zone; and the series is overlaid by a limestone with many species of true carboniferous productæ, &c., and identified with the mountain limestone. From these facts the authors conclude, that the fossiliferous region near Hoff (south of the Fichtelgebirge) belongs to the Devonian system, with the exception of the highest beds, which are carboniferous.

Such are the results arrived at by the authors, which seem to be in general accordance with one another, and to bear out the classification they proposed for the older British formations.

LONDON ELECTRICAL SOCIETY.

April 20, 1841.—The Society held their first meeting of this Session. After the election of members had been announced, the Chairman stated that Walter Hawkins, Esq. had presented the Society with a dried specimen of the *Gymnotus Electricus.* The Secretary read a letter he had received from J. P. Gassiot, Esq., F.R.S., in which was a description of a voltameter containing five pairs of electrodes, so arranged as to enable the experimenter to use one or more pairs at pleasure. A communication was received from George Mackrell, Esq., detailing experiments wherein the five terminal wires of a voltaic battery were ignited at the surface of acid water, forming a part of the circuit. The negative wire was heated to a greater degree than the positive. A paper from Thomas Pollock, Esq. was read, describing the deflections of the galvanometer, dependent on the change of colour, &c. produced in certain liquids by the application of heat to one arm of a V tube containing them. The author concluded that Dr. Black's law of capacity for heat will enable us to explain these electrical phænomena.

These papers will be printed in the "Proceedings" of the Society, the first part of which will appear on the first of July. The Society then adjourned to Tuesday, May 18th.

CHEMICAL SOCIETY OF LONDON.

On the 23rd of February, 1841, a meeting was held (in the rooms of the Society of Arts, the use of which had been kindly granted for the purpose,) of gentlemen desirous of uniting themselves for the purpose of forming a Chemical Society. At that meeting the following gentlemen were appointed as a Provisional Committee for carrying this object into effect.

Messrs. A. Aikin, W. T. Brande, H. J. Brooke, J. T. Cooper, J. Cumming, J. F. Daniell, T. Everitt, T. Graham, W. R. Grove, H. Hennell, G. Lowe, R. Porrett, R. Warington.

Mr. Warington, by whom the requisite preliminary arrangements had been made, was appointed Secretary to the Provisional Committee.

A number of gentlemen engaged in the practice or pursuit of chemistry having been invited by the Provisional Committee to become original members, the first meeting of the Society took place, also in the rooms of the Society of Arts, on the 30th of March, Professor Graham in the Chair.—The minutes of the previous meeting, held on the 23rd of February, having been read and confirmed, the following report of the Provisional Committee was brought up and adopted, with amendments, for the present government of the Society.

AMENDED REPORT OF THE PROVISIONAL COMMITTEE.

Name and Objects of the Society.—That the Society be designated the Chemical Society of London.

That this Society is instituted for the advancement of Chemistry and those branches of science immediately connected with it.

For this purpose periodical meetings of its members shall be held for the communication and discussion of discoveries and observations relating to such subjects; an account of which shall be published from time to time, by the Society, in the form of Proceedings or Transactions.

That the formation of a library of books relating to its proper subjects, of a museum of chemical preparations and standard instruments, and the establishment of a laboratory of research, are also ulterior objects of the Society.

Constitution of the Society.—That the Society consist of Ordinary Members, Foreign Members, and Associates. The government of the Society to be vested in the ordinary members only.

That all persons who have received the circular of the provisional committee inviting them "to join the Society as original members and to support it with their active co-operation," and who have given their assent so to do, on or before the 30th March, 1841, are the first ordinary members of the Society.

That the ordinary members do elect out of their own body, by ballot, a President, four Vice-Presidents, a Treasurer, two Secretaries, and a Council of twelve persons, four of whom may be non-resident, by whom the business of the Society shall be conducted.

That after the appointment of officers and a council, every candidate for admission be proposed according to a form of recommendation agreed upon, subscribed by three members of the Society, to one, at least, of whom, he shall be personally known; and such certificate shall be read and suspended in the Society's rooms or place of meeting for three ordinary meetings.—The method of voting for the election of members to be by ballot.—The ballot to take place at the meeting at which the certificate is read for the fourth time.—The election shall not be valid unless twelve or more members ballot. When three-fourths, or more, of the members balloting, shall be in favour of the candidate, he shall be elected a member; but when fewer than three-fourths of the members balloting, shall be in favour of the candidate, he shall not be elected a member.

That the Annual Contribution to be paid by ordinary members, resident within twenty miles of London, be two pounds, due on each successive 30th of March, and payable in advance for the current year. That for ordinary members resident beyond twenty miles of London, the Annual Contribution shall be one pound.

That no admission fee shall be required of those members who join the Society during the first yearly session.

That Foreign Members and Associates shall be recommended to the Society by the Council, and shall not be required to contribute to the funds of the Society.

That Foreign Members shall be balloted at the next meeting after that at which they are recommended for election.

That Associate Members be elected for a period of three years; that they be thereafter eligible as ordinary members, or may be re-elected associate members.

That the Ordinary Meetings of the Society be held on the Tuesday of every second week, at 8 o'clock in the evening,—from the beginning of November until the end of May.

That the rooms of the Society of Arts, John-street, Adelphi, be the place of meeting for the Society, until more convenient and fitting accommodation can be found.

The following gentlemen were then elected as Officers and Council for the ensuing year:—

President: Professor T. Graham.—*Vice-Presidents*: W. T. Brande, Esq.; J. T. Cooper, Esq.; Professor J. F. Daniell.—*Treasurer*: Arthur Aikin, Esq.—*Secretaries*: E. F. Teschmacher, Esq., Robert Warington, Esq.—*Council*: Dr. T. Clarke; Professor J. Cumming; Dr. C. Daubeny; T. Everitt, Esq.; T. Griffiths, Esq.; W. R. Grove, Esq.; H. Hennell, Esq.; G. Lowe, Esq.; Professor W. H. Miller; W. H. Pepys, Esq.; R. Porrett, Esq.; Dr. G. O. Rees.

The names of the gentlemen who had become members, in number seventy-five, were then read.

The Society then adjourned until Tuesday, the 13th of April.

LXV. *Notices respecting New Books.*

A Collection of Letters illustrative of the Progress of Science in England from the Reign of Queen Elizabeth to that of Charles the Second. Edited by JAMES ORCHARD HALLIWELL, Esq., F.R.S., F.S.A., &c. 8vo. [Printed for the Historical Society of Science.]

MANY of our readers are probably aware that a society has recently been formed, on the joint-stock principle of the Camden and a few other societies, for the purpose of printing documents connected with the history of the sciences, and forming by these means a medium for the circulation of such information on that subject as could scarcely be given to the world in any other way. The work whose title we have given above is the first offspring of this newly-established body; and although it is not distinguished by any very striking novelty or the produce of any great change in our previous knowledge of the history of science at the period to which it refers, yet it not only offers very comprehensive views of the real state of English science in the sixteenth and seventeenth centuries, but also affords us some very curious biographical information respecting our men of science during that time. It must above all be remembered, that our only chance for obtaining works of this class is dependent on the present or some similar plan, for there is no public encouragement to print them; no publisher of course would undertake their publication, and the chance of a private person being reimbursed of any sum he may have been hardy enough to have risked in the same way is very small. Every lover of such researches will, therefore, we think agree with us in commending any method by which we can possess ourselves of a series of publications of this nature in an useful form, and at a comparatively trifling expense.

The "Historical Society of Science" has commenced its labours by the publication of a miscellaneous collection of letters on scientific subjects, most of them hitherto inedited, from original manuscripts preserved in various English libraries. They are arranged in chronological order, without any regard to the different subjects on which they treat. The earliest letter in the volume is one from Eden, a chemist, to Lord Burghley, written on August 1st, 1562, in which, among other twaddle, he describes an operation by which he made a whole quantity of silver! The next is a curious relic of early English science,—a letter from Thomas Digges to Lord Burghley, dated 1574, on astronomical subjects. Then we have mechanical inventions by Ralph Rabbards, a long letter from Dr. Dee to Lord Burghley on the method of discovering hidden treasures, and one from

H. Cole to the same nobleman on some "grene ore" found at Richmond. We must not omit to mention, as belonging to this early period, a very curious letter from Tycho Brahe to Thomas Savelle, dated 1590, in which he sends his remembrances to Dr. Dee, "quem in patriam feliciter reversum audivi," and likewise to Thomas Digges. This last-mentioned document is very curious, because it shows that a greater intercourse was in all probability then maintained between the scientific men of this and other countries than is usually admitted to have been the case.

It is unnecessary, however, to continue an enumeration of all the letters here printed, few of which are without their value, and almost all are curious illustrations of the science of the period. We may call the attention of our readers more particularly to a letter from Briggs to Pell at p. 55, which affords a good illustration of the early history of logarithms in this country, and to the correspondence of Sir Charles Cavendish, who was one of the most munificent patrons of science this country has ever possessed. As a specimen of the others, we will here insert the following letter from Mr. Sawtell, a name previously unknown to us in science, addressed to Lilly, the celebrated astrologer, which gives an account of a strange tide at Weymouth, in July, 1666.

"Waymouth, August 6th, 1666.

"Mr. Lilly,—I wrote to you the 18th of the former month concerning the wonderfull motion of the tide as it was carefully here observed for 4 hours time, viz. July 17th, from about 10 in the morning untill 2 in the afternoone. I also wrote the same to the Post-office in London; notwithstanding which, it was put into the printed intelligence in one manner, and in the Gazette in another manner, and neyther of them aneere the truth, and for what eyther reason or policie I cannot imagine, but made me to be derided here, until I shewed a coppy to many of what I wrote them to London; since which they reply if they print one false that is so wonderfull, how many us believe the rest, &c. Sir, I intreated you by my letter to have given me a few lines in answer that you had received mine, but to this morning I have not received any, which makes me doubt that you have not received mine, or else that you doubted the truth, or that it was not of any worth. If it came not to your hand, pray be pleased by a few lines to give me notice and I shall give you (if you desire it) a very true, sure, and punctuall account of it, or in any particular thing or accident that you shall require of it. If you doubted the truth I'll assure you that to what I wrote you ther is not a title false, I am very confident, viz. that the sea did ebb and flow seven times in four hours time; with the rest of particulars writen you, to which a clowde, as it were, of witnesses will appeare; that there were many more ebbings and flowings it is probable to be true by the report of many; which say that the sea did soe all the morning, and that it did soe likewise againe in the evening, but they say that this was not so violent; but this 4 houres time that I gave you account off was as I have said, carefully observed by so many and that of the chiefest mariners, merchants, and other gentlemen, as well as other sorts of people, both men and weomen, one friend calling and sending for another, that to me it seemes an absurdity in the least to doubt the truth; the chiefe occation of observation being the extreame violence of it, one wherof I heard being in a house, being talking with one, we had much pitty for a ketch there riding in the road, we thinking of a very great and suddent storme of wind, but looking out of doores found noe wind att alle, but saw the sea at a distance full of ravelling waves with much noyes. If not of worth to you, pray take the will for the deed. I aymed at the best. The thing to all persons of this town was and still is wonderfull, and I thought it worthy of publique note, to which end I sent it to London preferring you before Mr. Gadbury, who otherwise had had the account. However, Sir, if you will not afford me no other account of it, you let me begg you to give me a few lines that you have received mine, that I may know that it came to your hand; and if by writing so presumptuously to you, strangers to each other, I have offended you, pray pardon me; it is but the second offence; I hope I then shall not committ the third: yet in the meane while shall remaine, Sir,

"Your friend in what I may,
"CHRISTOPHER SAWTELL."

"*For Mr. William Lilly, Astrologer, at the*
corner-house, over against Strand-bridge,
these in London. Post paid, 3d."

Independent of its more weighty and important attractions, there is much in the volume that will be found to be interesting to the general reader; and, what is very unusual with works of a scientific nature, some parts of it are really entertaining. It only remains for us to say, that the editor appears to have performed his task carefully and judiciously.

LXVI. *Intelligence and Miscellaneous Articles.*

ORTHODOX GEOGRAPHY.

"*Azimghur School.*—The Local Committee brought to our notice a prejudice which existed amongst some of the Hindus at this place. An unwillingness existed amongst some of the pupils to receive geographical instruction according to the English system, from an idea that the Christian, as well as the Hindu religion, had certain *peculiar geographical tenets*.

"We recommended to the Local Committee to act with caution, until the jealousy should subside, which we doubted not it soon would, and we suggested that geographical instruction should be given orally with the aid of maps. We instanced the readiness with which geography had been studied in the Sanscrit College from the English books, and pointed out that learned Brahmins did not hesitate to study the Hindu astronomy, according to the Sidhantas, though they are as opposed to the conceits of the Puranas as is European geography."—*Report of the Committee of Public Instruction of the Presidency of Fort William, Bengal.*

CUMYLE—OIL OF CUMMIN.

The following are some additional particulars relative to the substances obtained from oil of cummin, by MM. Gerhardt and Cahours, referred to in our last, p. 282.

Cumyle (unknown radicle)........ $C^{40} H^{22} O^2$
Hydruret of cumyle (oil of cummin) $C^{40} H^{22} O^2 + H^2$

If the hydruret of cumyle be gradually dropped on fused hydra of potash, hydrogen is evolved, and every drop is converted into te minate of potash. The effect is so rapidly produced, that by this method two pounds of cuminate of potash may be obtained in an hour.

The analysis of the product obtained by distilling cuminic acid with excess of barytes, and the density of its vapour, lead to the formula $C^{36} H^{24}$, which represents four volumes of it. It is therefore formed in the same manner as benzen. In fact, we have $C^{40} H^{24} O^4 - C^4 O^4 = C^{36} B^{24}$, the alkaline base employed effecting the separation of carbonic acid.

This product, to which the authors have given the name of *cumen*, possesses striking analogies with benzen: thus it dissolves in Nordhausen sulphuric acid, and produces an acid corresponding to the sulphobenzidic acid of Mitscherlich.—*L'Institut*, No. 389.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1841.

Chiswick.—March 1. Cloudy. 2. Frosty: rain. 3. Fine: cloudy. 4. Clear and fine: rain. 5. Overcast: slight rain. 6. Clear and very fine. 7, 8. Very fine. 9—12. Foggy in the morning: very fine throughout the day: evening clear. 13. Slight haze: foggy. 14. Foggy: very fine: dense fog at night. 15, 16. Foggy: very fine. 17. Cloudy and showery. 18. Cloudy. 19. Overcast: showery. 20. Fine: stormy with rain. 21. Very fine: slight rain at night. 22. Rain: fine. 23. Fine. 24—26. Very fine. 27. Showery: clear. 28—30. Cloudy and fine. 31. Clear: fine but windy: rain at night.

Boston.—March 1. Cloudy: rain P.M. 2. Fine: rain P.M. 3, 4. Fine. 5. Fine: rain A.M. and P.M. 6. Fine. 7. Fine: beautiful morning. 8. Foggy. 9—13. Fine. 14. Cloudy. 15. Fine: three o'clock P.M. thermometer 65°. 16. Fine: two o'clock P.M. thermometer 65°. 17. Cloudy. 18—21. Windy. 22. Rain: 23. Windy. 24, 25. Fine. 26. Fine: rain P.M. 27. Cloudy. 28. Fine. 29. Cloudy: rain early A.M.: rain P.M. 30. Fine: rain P.M. 31. Fine.

N.B. This is the warmest month of March since March 1830.

Applegarth Manse, Dumfries-shire.—March 1. Cold and clear: snow on hills melting. 2. Rain all day. 3. Slight frost. 4. Frost harder: snow. 5. Heavy rain: snow gone. 6. Fine A.M.: rain P.M. 7. Fine throughout. 8. Fine spring day: rain A.M. 9. Fine spring day: fair. 10. Growing day: fog P.M. 11. Beautiful day. 12. Fine A.M.: raw fog P.M. 13, 14. Fine throughout. 15. Fog A.M.: cleared up. 16. Fine throughout. 17, 18. Fine A.M.: rain P.M. 19. Rain and hail. 20. Heavy showers. 21, 22. Rain P.M. 23. Cloudy and threatening. 24, 25. Showery and foggy. 26. Showers A.M.: cleared and fine. 27. Showers A.M. 28, 29. Showers P.M. 30. Fair A.M.: rain and wind P.M. 31. Boisterous and wet.

Sun shone out 23 days. Rain fell 18 days. Frost 2 days. Snow 1 day. Fog 3 days.

Wind East 1 day. South-east 4 days. South-south-east 1 day. South 4 days. South-south-west 1 day. South-west 13 days. West-south-west 2 days. West 3 days. North-north-west 1 day.

Variable 1 day. Calm 11 days. Moderate 8 days. Brisk 5 days. Strong breeze 5 days. Boisterous 2 days.

Mean temperature of the month	44°·07
Mean temperature of March 1840	39 ·35
Mean temperature of spring-water	45 ·60
Mean temperature of spring-water, March 1840	42 ·60

Meteorological Observations made at the Apartments of the Royal Society 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, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. March.	Barometer.						Thermometer.									Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·614	29·693	29·585	29·24	29·46	29·57	39·3	42·6	36·9	41	27	37	45	33	SE.	S.	calm	NNW.	...	·10	...	...	36
2.	29·688	29·685	29·393	29·30	29·35	29·10	37·4	43·2	35·0	49	37	34	45	32	SW.	SW.	W.	SW.	·025	·30	·08	...	37
3.	29·324	29·665	29·315	28·89	29·24	29·63	42·4	46·7	37·3	50	29	42	45	37½	NW.	SW.	W.	SW.	·283	·02	·23	...	39
4.	29·912	29·904	29·818	29·48	29·75	29·60	36·8	48·3	35·2	46	33	36	41	26½	NW.	W.	NW.	SW.	...	·08	...	...	36
5.	29·876	29·857	29·299	29·39	29·42	29·10	42·0	45·4	36·2	50	36	39	41½	32	S.	S.	calm	SW.	·083	·22	...	...	40
6.	29·764	29·960	29·723	29·26	29·58	29·68	43·2	50·3	40·5	52	41	41	48½	33½	NW.	NW.	calm	SW.	·158	...	·32	...	41
○ 7.	30·080	30·255	30·047	29·53	29·77	30·12	51·7	52·5	42·6	61	42	49	51	41	W.	SW.	calm	W.	...	...	...	0·77	43
8.	30·414	30·369	30·355	29·85	30·18	30·24	50·5	59·2	49·6	61	35	46	52½	41	SW.	SW.	calm	SW.	...	...	...	...	45
9.	30·444	30·392	30·385	29·90	30·21	30·24	44·5	60·3	43·4	60	30	44	51	38½	SSW.	SW.	calm	SW.	...	...	...	...	46
10.	30·490	30·462	30·426	29·93	30·30	30·35	46·7	56·5	43·3	63	27	44	49	41½	S.	S.	calm	WSW.	...	...	...	...	45
11.	30·512	30·473	30·401	29·97	30·33	30·27	45·2	58·7	40·0	64	30	44	56	39	S.	SE.	calm	var.	...	...	...	...	44
12.	30·400	30·366	30·273	29·85	30·25	30·28	46·6	59·3	42·2	67	31	41·5	53½	34½	E.	SW.	S.	SE.	...	...	...	...	46
13.	30·398	30·376	30·331	29·87	30·30	30·30	46·6	61·0	44·6	59	37	46	56	35	N.	E.	E.	SE.	...	...	...	...	47
14.	30·388	30·351	30·192	29·94	30·18	30·11	40·2	56·7	37·7	57	33	36·5	53½	37	E.	W.	calm	SW.	...	...	...	0·04	45
☾ 15.	30·176	30·152	30·005	29·70	30·04	29·94	42·8	53·7	39·6	66	30	43	56½	42½	NE.	W.	W.	W.	...	...	...	...	40
16.	29·942	29·921	29·729	29·40	29·75	29·48	49·7	60·6	41·6	65	35	53	62	41½	S.	S.	W.	SE.	...	...	...	...	47
17.	29·630	29·603	29·577	29·08	29·35	29·30	51·3	61·7	49·2	56	41	51·5	49	47	S.	S.	S.	SW.	...	·01	...	...	49
18.	29·444	29·525	29·404	28·98	29·11	29·15	51·7	56·7	46·8	56	40	49	52	43	E var.	SW.	S.	SW.	...	...	...	...	48
19.	29·628	29·713	29·598	29·14	29·20	29·25	49·7	56·0	44·2	54	34	46·5	51½	43	SSWvar.	SW.	S.	WSW.	...	·01	...	...	48
20.	30·650	29·647	29·565	29·24	29·39	29·32	50·3	54·0	44·3	57	39	47	50	39½	E.	SW.	SE.	E.	·022	·13	...	...	46
21.	29·672	29·625	29·482	29·22	29·32	29·14	49·5	56·4	43·6	54	48	48	51	39	S.	S.	SSW.	S.	·133	·12	...	1·20	46
22.	29·378	29·596	29·362	28·84	29·01	29·27	51·9	54·3	48·7	58	36	52	50	45	S var.	SW.	W.	S.	·036	·02	·12	...	50
● 23.	29·878	30·128	29·803	29·29	29·44	29·78	49·3	56·6	43·2	59	43	47	50½	39	SSW.	W.	W.	SW.	·025	...	...	...	48
24.	30·270	30·222	29·192	29·70	29·94	29·94	51·4	58·3	46·6	58	30	50	48½	44½	SE.	SW.	W.	SW.	...	...	...	...	46
25.	30·160	30·123	29·952	29·60	29·85	29·70	51·8	55·7	44·3	61	32	47	48½	44½	SSE.	S.	calm	S.	...	...	...	...	45
26.	29·728	29·698	29·593	29·21	29·55	29·40	55·5	59·0	47·0	65	39	52	48½	42	S.	S.	calm	SE.	...	·02	...	...	49
27.	29·778	29·863	29·721	29·21	29·48	29·63	51·5	64·2	45·0	58	27	48	52	39½	S.	SW.	calm	W.	·036	·08	·26	...	48
28.	29·970	29·914	29·897	29·43	29·60	29·65	45·0	59·7	41·2	54	40	46	47	43	SSW.	S.	SW.	SSW.	·105	...	...	1·36	44
29.	29·938	29·922	29·787	29·37	29·66	29·47	51·2	56·6	44·8	58	40	51·5	53	42½	S.	SW.	calm	SSE.	...	·10	·02	...	48
☽ 30.	29·912	29·915	29·836	29·34	29·60	29·35	48·2	57·0	43·0	55	41	47	49	39½	W.	W.	calm	S.	·072	·08	·10	...	48
31.	29·678	29·638	29·497	29·06	29·02	29·20	49·0	55·0	44·7	56	41	47	49	40	S var.	W.	W.	SW.	·069	·03	·07	...	46
Mean.	29·940	29·968	29·856	29·42	29·665	29·753	47·1	55·4	42·7	57·09	35·61	45·3	50·1	39·2					Sum. 1·047	1·32	1·20	3·37	Mean. 45

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

[THIRD SERIES.]

JUNE 1841.

LXVII. *On some of the Products of the Action of Nitric Acid on Castor Oil.* *By* THOMAS GEORGE TILLEY[*].

THE action of nitric acid on the fat acids is a subject that has been lately investigated by Laurent[†] and Bromeis[‡], and the interesting results obtained by them having called the attention of chemists to the subject, I was induced, at the suggestion of Professor Liebig, to examine in his laboratory the products of the oxidation of the oil of *Ricinus Communis*, differing as it does in such a remarkable degree from the other fixed oils.

One part of this oil was accordingly mixed with twice its weight of nitric acid diluted with an equal bulk of water, and subjected to a gentle heat in a retort. The parts which distilled over were preserved. After some time the action became very violent, and gas was formed in such quantities as to expel the contents of the retort forcibly from its mouth. The retort must then be removed from the fire, when the action gradually subsides. When again placed on the fire, protected by a sand-bath, the action is not so violent. The process of oxidation must be continued for some days, more or less, according to the strength of the nitric acid employed. When the quantity of nitrous acid fumes decreases, the retort is removed from the fire. In the receiver are found nitric acid, water, and a peculiar volatile oily acid, which new compound will form the subject of the following pages. If the

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[*] Read before the Chemical Society, April 27, 1841. Communicated by the Society.

[†] *Ann. de Chim. et de Phys.*, lxvi.

[‡] *Annalen der Pharmacie*, xxxv. p. 86. [Noticed in L., E. and D. Phil. Mag. for February, pres. vol. p. 115.]

fatty mass which remains in the retort be mixed with water and distilled, an additional quantity of the acid will be obtained.

It must be separated from the acid fluid on which it floats, mixed with water and redistilled, which process must be repeated several times. After this it must be dried by standing over melted phosphoric acid, chloride of calcium not being adapted to the purpose, it being a little soluble in the acid.

The acid procured in this way is perfectly colourless and transparent, having an agreeable aromatic smell and a sweet stimulating taste. It is sparingly soluble in water, imparting to that fluid its peculiar smell. It is soluble in nitric acid, alcohol and æther. When raised to the temperature of 148° C. it begins to boil, and a small part distils over; but if kept for some time at that temperature, it becomes black and is decomposed, giving off empyreumatic products, so that it cannot be distilled alone. It burns with a clear flame, giving little smoke. It does not become solid though cooled to − 17° C. To this body I have given the name of *Œnanthylic Acid*, for reasons which will be stated in another part.

To determine its composition the acid was burned with oxide of copper.

1st. 0·3305 substance gave 0·7810 carbonic acid, and 0·3330 water.

2nd. 0·4295 substance gave 0·9905 carbonic acid, and 0·4009 water.

This gives in 100 parts,

	1st.	2nd.
Carbon	65·34	65·33
Hydrogen......	10·83	10·60
Oxygen.........	23·83	24·07
	100·00	100·00

Œnanthylic Æther.—This æther is formed by dissolving the acid in strong alcohol, and passing a stream of hydrochloric acid gas through the solution. To the mixture is then added carbonate of potash, in order to neutralize all free acid; it is then distilled. The æther passes over into the receiver, and may be freed from any alcohol that it may contain by agitation with water. It must, lastly, be distilled over chloride of calcium in a stream of carbonic acid gas, as it is decomposed by the oxygen of atmospheric air, at the temperature of its boiling point.

The œnanthylic æther so obtained is a colourless fluid, lighter than water, having a peculiar and agreeable smell not unlike that of nitrobenzide. It has a sweet and somewhat pungent taste, leaving a disagreeable sensation on the palate. It is soluble in æther and alcohol, and burns with a clear blue flame, giving out no smoke. It is solid at the temperature produced by a mixture of snow and salt, and is then crystalline.

Burnt with oxide of copper, 0·2195 substance gave 0·5485 carbonic acid, and 0·3195 water; making in 100 parts,

Carbon. .	68·57
Hydrogen.	11·57
Oxygen.	19·86
	100·00

and leading to the following composition:

			Per cent.
18 atoms	Carbon	= 1375·80	68·71
18 ,,	Hydrogen	= 224·63	11·22
4 ,,	Oxygen	= 400·00	20·07
		2000·43	100·00

Now supposing this æther to be composed of one atom oxide of æthyl and one atom of anhydrous acid, we have for the composition of the latter $C^{14} H^{13} O^{3}$, and adding to this one atom of water to form the hydrate, we have, in 100 parts,

			Per cent.
14 atoms	Carbon	= 1070·09	65·05
14 ,,	Hydrogen	= 174·71	10·62
4 ,,	Oxygen	= 400·00	24·38
		1644·80	100·00

which calculated result agrees with the numbers found by actual experiment given at page 418.

Œnanthylate of Silver.—This salt may be easily prepared by precipitating a neutral ammoniacal solution of the acid by nitrate of silver; it falls in the form of a white flocculent powder.

1st. 0·7165 of this salt gave ·3265 silver, which is equivalent to 0·3509 oxide of silver. This gives per cent.,

Oxide of silver	48·89
Œnanthylic acid	51·11
	100·00

making the atomic weight of the anhydrous acid 1517·0, the calculated result being 1532·33.

By burning the salt dried *in vacuo* over sulphuric acid, with oxide of copper, 0·7350 gave 0·9360 carbonic acid, and 0·3675 water.

2nd. 0·8785 gave 1·1300 carbonic acid, and 0·4505 water. This gives in 100 parts,

	1st.	2nd.
Carbon	35·20	35·56
Hydrogen	5·55	5·68
Oxygen	10·27	9·78
Oxide of silver	48·89	48·98
	100	100

and leads to the following theoretical composition:

				Per cent.
14	atoms	Carbon	1070·090	35·86
13	,,	Hydrogen	162·233	5·43
3	,,	Oxygen	300·000	10·07
1	,,	Oxide of silver.	1451·61	48·64
		Atomic weight	2983·384	100·00

The formula for this salt is then $C^{14} H^{13} O^{3} + Ag O$.

Œnanthylate of Baryta.—This salt is formed by boiling carbonate of baryta with an alcoholic solution of œnanthylic acid till the fluid no longer possesses acid reaction. The solution must be filtered while hot, and on cooling deposits the salt in beautiful pearly scales, which are insoluble in æther, but soluble in water and alcohol.

0·3520 of this salt gave 0·1735 carbonate of baryta, or 0·1346 baryta, the atomic weight of the acid, calculated from this, being 1545·54. It gives in 100 parts,

38·23	oxide of barium
61·77	œnanthylic acid
100·00	

1st. 0·4395 of the same salt, burned with oxide of copper, gave 0·6645 carbonic acid, and 0·2740 water.

2nd. 0·3460 salt gave 0·5200 carbonic acid, and 2190 water.

After adding to the carbon that which remained, combined as carbonic acid, with the baryta in the combustion tube, we obtain in 100 parts,

Carbon	44·84	44·58
Hydrogen	6·91	6·87
Oxygen	10·02	10·32
Baryta	38·23	38·23
	100·00	100·00

which gives the following theoretical numbers:

				Per cent.
14	atoms	Carbon..........	=1070·09	42·98
13	„	Hydrogen.......	= 162·23	6·51
3	„	Oxygen..........	= 300·00	12·07
1	„	Oxide of barium	= 956·88	38·44
			2479·20	100·00

It will be seen by the preceding numbers that an excess of carbon is found; but this may be accounted for by the high temperature required to burn the last particles of carbon, by which some of the carbonic acid might have been driven off from the carbonate of baryta in the combustion tube.

The formula for this salt is one atom œnanthylic acid and one atom oxide of barium, or $C^{14} H^{13} O^{3} + Ba O$.

Œnanthylate of Potash is obtained by neutralizing the carbonate of that alkali by œnanthylic acid; it does not crystallize, but, on evaporation, assumes the form of a thick transparent jelly.

The *Copper Salt* crystallizes in beautiful needles of a rich green colour, soluble in alcohol, and sparingly so in water.

The *Œnanthylate of Strontian* appears in the form of bright pearly scales, very much resembling the analogous salt of baryta.

It will at once be perceived by the analyses which have been given, that the composition of anhydrous œnanthylic acid is $C^{14} H^{13} O^{3}$, and it enters as such into the following combinations:

Œnanthylate of water......... $C^{14} H^{13} O^{3} + H^{2} O$
„ oxide of æthyl. $C^{14} H^{13} O^{3} + C^{4} H^{5} O$
„ „ barium $C^{14} H^{13} O^{3} + Ba O$
„ „ silver . $C^{14} H^{13} O^{3} + Ag O$.

Now it was discovered some time ago that wine owes its peculiar smell to a certain acid in combination with oxide of æthyl. To this acid Liebig and Pelouze, its discoverers, gave the name œnanthic acid (flower of wine). Œnanthic acid has the following composition, $C^{14} H^{13} O^{2}$ + aq.

The analogy between this acid and the one described in this paper will at once be seen, and the composition of the two acids would lead us to suppose that they are oxides of the same radical, which may for the present be considered as composed of $C^{14} H^{13}$; while the two acids are respectively

$$R + 2 O$$
$$R + 3 O.$$

On these grounds I have given the higher oxide the name *Œnanthylic acid*, and propose for the lower oxide, that of

Liebig and Pelouze[*], the name *Œnanthylous acid.* It is not improbable that œnanthylic acid may be identical with the azoleinic acid of Laurent[†]; but he did not obtain it in a state of purity. When the œnanthylate of silver is distilled, there pass into the receiver an oil and a solid body, neither of which possesses acid properties. The solid body is soluble in hot alcohol, and on being allowed to cool, it crystallizes in beautiful needles.

Suberic acid is another product of the oxidation of castor oil; it remains in the retort mixed with oxalic acid: it may be purified by repeated crystallizations, and boiling with nitric acid.

Thus obtained and dried at 100° C., 0·2710 substance gave ·5490 carbonic acid, and 0·1990 water, or, in 100 parts,

	Found.	Atoms.	Calculated.
Carbon ...	55·97	8	55·64
Hydrogen .	8·15	7	8·03
Oxygen ...	35·78	4	36·33
	100·00		100·00

giving the formula $C^8 H^6 O^3 + aq$.

The other acids found by Laurent did not appear to have been formed; but this is not affirmed with positive certainty, as the examination was not proceeded with. A trace of lipinic acid, however, may be obtained during the evaporation of the fluid parts, pressed from the suberic acid.

LXVIII. *On Bleaching Salts.* *By* M. Detmer, *Esq.*[‡]

A SHORT time ago a notice was published by M. Millon on the Bleaching Salts of Chlorine, in which a new view was offered of the constitution of these compounds. They have for some time past generally been considered as compounds or mixtures of a metallic chloride with a hypochlorite of a metallic oxide; bleaching powder or the chloride of lime, for instance, as consisting of chloride of calcium and hypochlorite of lime, in single equivalents, the acid of the last salt containing one atom of oxygen to one atom of chlorine. The reaction of chlorine upon lime supposed, may be very simply stated: two atoms of lime take up two of chlorine; one atom only of the lime is decomposed, of which the calcium and oxygen respectively unite with an atom each of chlorine, form-

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[*] *Ann. de Chim. et de Phys.*, t. lxiii. 118.

[†] *Ann. de Chim. et de Phys.*, t. lxvi. 172.

[‡] Read before the Chemical Society, April 27, 1841. Communicated by the Society.

ing chloride of calcium and hypochlorous acid. The hypochlorous acid combines with the other atom of lime.

Starting from the composition of chlorochromic and chlorosulphuric acids, which are represented by Walter and Regnault as chromic and sulphuric acids in which the third proportion of oxygen is replaced by chlorine ($Cr\,O_2 + Cl$ and $S\,O_2 + Cl$), Millon supposes that the bleaching chlorides have a similar relation to the peroxides of their metals. The peroxide of calcium being $Ca\,O_2$, or $Ca\,O + O$, bleaching powder is $Ca\,O + Cl$, or the peroxide of calcium, with chlorine substituted for its second proportion of oxygen. In support of this view Millon adduces observations of his own on the composition of the bleaching compounds of chlorine with different metallic oxides, such as oxides of lead and protoxide of iron, as well as potash, soda and lime, in which the proportion of chlorine was found to vary, but to correspond with the excess of oxygen above one equivalent in the peroxides of the same metals. In particular, potash was found to absorb two equivalents of chlorine, and soda only one, the peroxide of potassium being $K\,O + 2\,O$, while the peroxide of sodium is $Na\,O + O$.

The attention of the author was particularly directed to ascertain the accuracy of the latter statement. A solution of carbonate of soda was charged with chlorine gas till it acquired a yellow colour and retained not a trace of carbonic acid. The solution was then briskly agitated with air, by which the excess of free chlorine escaped. In analyzing the solution afterwards, one portion of it was treated with a few drops of ammonia, and the chlorine afterwards precipitated by nitrate of silver; another portion was evaporated to dryness for the sodium, which was obtained in the state of chloride of sodium.

In four experiments the liquids charged with chlorine contained chlorine and sodium in the following proportions, in 100 parts:—

Sodium	47·88	45·26	46·81	44·76
Chlorine	52·12	54·74	53·19	55·24,

while, if the bleaching chloride of soda contained 1 eq. of chlorine to 1 eq. of soda, its composition would be

1 eq. sodium	46·91
1 eq. chlorine	53·09
	———100·00

The results correspond as closely as could be expected with this theoretical statement. There can be no doubt then that the chloride of soda contains one of chlorine to one of soda. This is the result required by Millon's theory, the peroxide of sodium containing, according to him, one of oxygen and one of soda; but it is equally consistent with Balard's theory that the salt is a mixture of single equivalents of chloride of sodium and hypochlorite of soda. To determine the quantity of chlorine which water dissolves, a stream of the gas was sent through water at 59° for five hours. One hundred grammes of water were found to take up 0·663 gramme of chlorine; or 200 cubic inches of water dissolved 207 cubic inches of gas. The chlorine was estimated by converting it into hydrochloric acid by the addition of a few drops of ammonia, slightly acidulating afterwards by nitric acid, and precipitating by nitrate of silver. A solution of 2·58 chloride of potassium in 38·96 water was found to dissolve less chlorine than pure water, in the proportion of 180 to 257. Chlorine gas being allowed to stream through a solution of 9·245 grammes carbonate of potash in 96·495 grammes of water, till saturation, the solution lost all its carbonic acid and took up 6·631 grammes of chlorine. Here 1 eq. of potash = 590 has taken up 656 chlorine, which is very nearly $1\frac{1}{2}$ eq. of chlorine = 663. But when the quantity of free chlorine in the liquid is deducted, the latter is found to contain only 1·34 equivalents of chlorine to 1 eq. of potash. In two other experiments, in which the liquid was agitated with air after being saturated with chlorine, to allow the excess of gas to escape, there were found to 1 eq. of potash 1·44 and 1·43 equivalents of chlorine. The carbonate of potash, therefore, without doubt, takes up more than a single equivalent of chlorine. But the quantity of chlorine combined with the potash is still greatly short of two equivalents, the proportion required by M. Millon's theory; the peroxide of potassium containing two oxygen to one potash, or KO_3. The conclusion therefore is inadmissible, that the chloride of potash is analogous in constitution to the peroxide of potassium.

It remains to account for the property which potash is found to possess of taking up more chlorine than is necessary to convert it into chloride of potassium and hypochlorite of potash. On transmitting chlorine through carbonate of potash, a stage in the absorption is very observable, at which the liquid becomes all at once of a yellow colour. This happens when what remains of the potash is entirely converted into bicarbonate of potash. The suddenness of the appearance of the yellow colour appears to be due to a reaction of the carbonic acid upon the hypochlorite of potash in solution, by which hypochlorous acid is set free and tinges the liquid. By the continued application of chlorine to the bicarbonate of potash, it is converted into a mixture of chloride of potassium, hypochlorite of potash, and free hypochlorous acid. By the ultimate action of the chlorine all the bicarbonate of potash is decomposed, the carbonic acid entirely expelled, and a portion of hypochlorous acid remains free in solution.

This formation of free hypochlorous acid does not occur with carbonate of soda, owing to the much weaker affinity which that base has for carbonic acid, and its forming a much less stable bicarbonate than potash does. The free carbonic acid cannot therefore react upon the hypochlorite of soda, and liberate hypochlorous acid as the free carbonic acid does upon the hypochlorite of potash. The same formation of free hypochlorous acid occurs in a more striking degree when chlorine is sent through a solution of acetate of potash; that solution, it is well known, absorbs a large quantity of gas, and acquires the strong yellow colour, the odour, and all the other properties of free hypochlorous acid. It is here evident, that by the action of chlorine upon acetate of potash, chloride of potassium is formed, with the binacetate of potash, free hypochlorous acid, and the hypochlorite of potash. If the large absorption of chlorine by carbonate of potash is due to carbonic acid, it follows that caustic potash should not absorb any excess of chlorine, but that the property should be confined to the carbonate. Accordingly, in two experiments, the proportion of chlorine absorbed by caustic potash was found to be as nearly as possible a single equivalent. In one experiment 449·1 chlorine, in the other 424·8 chlorine were taken up, instead of 442·6 chlorine, by a single equivalent or 589·9 of potash. Caustic potash, therefore, dissolves no more chlorine than caustic soda. There appears therefore to be no reason to abandon the old theory, that the bleaching solutions of chlorine in alkalies and alkaline earths contain a chloride and hypochlorite, for these bleaching compounds certainly do not correspond with metallic peroxides, as has been lately maintained.

LXIX. *On a linear Method of Eliminating between double,* treble, *and other Systems of Algebraic Equations. By* J. J. Sylvester, *F.R.S., Professor of Natural Philosophy in University College, London.*

Part I.—Binary Systems.

LET U and V be two integer complete homogeneous functions of x and y, one of the mth, the other of the nth degree; and let it be required to express the condition of the coexistence of the two equations $U = 0$ $V = 0$ by means of the equation $C = 0$, where C is free from all appearances of x or y.

This equation, according to the system of notation developed in a preceding paper, and which has been since adopted and sanctioned by the high authority of M. Cauchy, I call the final derivative: the quantity C is designated the final derivee: and it is our present object to show how this may be obtained in a *prime* form, that is to say, divested of irrelevant factors: in this state it must consist of terms, each containing $m + n$ letters, of which n belong to the coefficients of V, and m to those of U.

Of course in applying this rule it is to be understood that every combination of powers in U or V has a single letter prefixed for its coefficient, and that in the final derivee powers are represented by repetitions of the same character.

Every term in U or V being of the form $C\,x^p \,.\, y^q$, $x^p \,.\, y^q$ is called an argument, c its prefix.

Assume two integer positive numbers r and r', and also two others s and s', such that $r + r' = n - 1$ $s + s' = m - 1$, and form from $U = 0$ $V = 0$ two new equations,

$$x^r \,.\, y^{r'} \,.\, U = 0 \quad x^s \,.\, y^{s'} . V = 0.$$

Such equations are termed the Augmentatives of the two given ones respectively; also $x^r \,.\, y^{r'}$. U and its fellow are termed the Augmentees of U and V.

r and r' are termed the indices of augmentation belonging to U, s and s' the same belonging to V.

Finally, it will be useful hereafter to call the given polynomials U and V themselves the proposees, and the given equations which assert their nullity, the propositive equations, or, briefly, the propositives.

Now as many augmentees of either proposee can be formed as there are ways of stowing away between two lockers (vacancies admissible) a number of things equal to the index of the other*; hence we shall have n augmentees of U, and m of V: thus there will be $m + n$ augmentatives each of the degree $m + n - 1$, and the number of arguments is clearly $m + n$ also, so that they can be eliminated linearly, and the final derivee thus found, containing $m + n$ letters (properly aggregated) in each term, will be in its prime form, that is, incapable of further reduction, and void of irrelevant factors.

It is worthy of remark, that the final derivee obtained by arranging in square battalion the prefixes of the augmentees, permuting the rows or columns, and reading off diagonal products, affected each with the proper sign (according to the well known rule of Duality), will not only be free from fac-

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* "Tot Augmenta utriusvis ex æquationibus propositis formari possunt quot modi sint inter duo receptacula (utrivis vel ambobus omnino vacare licet) rerum, quarum numerus indicem alterius æquat, distributionem faciendi."

torial irrelevancy, but also of linear redundancy, which latter term I use to signify the reappearance of the same combination of prefixes, sometimes with positive and sometimes with negative signs: furthermore, it follows obviously from the nature of the process that no numerical quantity in the final derivee will be greater than the higher of the indices of the two given polynomials.

Part II.—TERNARY SYSTEMS.

CASE A.—Indices all equal.

Method 1.

Let there be now three proposees, U, V, W, integer complete homogeneous functions of x, y, z, each of the degree n: let $r + r' + r'' = n - 1$, $s + s' + s'' = n - 1$, $t + t' + t'' = n - 1$,

$$x^r . y^{r'} . z^{r''} . \mathrm{U}, \ x^s . y^{s'} . z^{s''} . \mathrm{V}, \ x^t . y^{t'} . z^{t''} . \mathrm{W}$$

will, as above, be called the augmentees of U, V, W, and every other part of the notation previously described is to be preserved.

$$\text{Suppose now } \mathrm{U} = 0, \ \mathrm{V} = 0, \ \mathrm{W} = 0,$$

we shall have as many augmentative equations formed from each proposee as there are ways of stowing away n things between *three* lockers (vacancies admissible)*, i. e. $n . \frac{n + 1}{2}$ of each kind; in all, therefore, $3 . \frac{n . n + 1}{2}$, and every one of these will be of the degree $2n - 1$, so that the number of arguments to be eliminated is equal to the number of ways of stowing away $2n - 1$ things between three lockers (empty ones counting), i. e. $\frac{2n . (2n + 1)}{2}$.

As yet, then, we have not *enough* equations for eliminating these linearly.

Make, however, $\alpha + \beta + \gamma = n + 1$,

and write $\mathrm{U} = x^\alpha . \mathrm{F} + y^\beta . \mathrm{F}' + z^\gamma . \mathrm{F}'' = 0$,

$$\mathrm{V} = x^\alpha . \mathrm{G} + y^\beta . \mathrm{G}' + z^\gamma . \mathrm{G}'' = 0,$$

$$\mathrm{W} = x^\alpha . \mathrm{H} + y^\beta . \mathrm{H}' + z^\gamma . \mathrm{H}'' = 0,$$

it will always be possible to make the multipliers of x^α, y^β, z^γ integer functions: for if we look to any argument in U, V, or W, it is of the form $x^a . y^b . z^c$, and one of the letters a, b, c must be *not less* than its correspondent α, β, γ, for otherwise $a + b + c$ would be not greater than $\alpha + \beta + \gamma - 3$, i. e. n would be not greater than $(n + 1) - 3$, or $n - 2$, which is

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* See for Latin translation the preceding note.

absurd: if now any one, as (a), be equal to or greater than α, it may be made to supply an integer part to the multiplier of x^{α}.

Here it may be asked what is to be done with such terms as $K . x^{a} . y^{b} . z^{c}$, when two letters a, b are each not less than their correspondents α, β: the answer is, such terms may be made to enter under the multiplier of x^{α}, or of x^{β}, or to supply a part to both in any proportion at pleasure*.

From the equations above we get, by linear elimination,

$$F . G' . H'' + G . H' . F'' + H F' . G''$$
$$- G . F' . H'' - H . G' . F'' - F . H' . G'' = 0.$$

This may be denoted thus: $\Pi (\alpha, \beta, \gamma) = 0$, which equation I call a *secondary* derivative, and the left side of it a *secondary* derivee; α, β, γ may likewise be termed the indices of derivation (as r, s, t, etc. are of augmentation).

Now since $\alpha + \beta + \gamma = n + 1$, it is clear then the index of $\Pi (\alpha, \beta, \gamma)$ is always $n + n + n - (n + 1)$; i. e. $2 n - 1$.

1st. Let any two of the indices of derivation be taken zero, then it is easily seen that all the terms in $\Pi (\alpha, \beta, \gamma)$ vanish, and consequently the secondary derivative equations obtained upon this hypothesis become mere identities, and are of no use.

2nd. Let any *one* of them become zero.

It is manifest, from the doctrine of simple equations, that

$$\Pi (\alpha, \beta, \gamma) \text{ may be made equal to} \left\{ \lambda . U + \mu . V + \nu . W \right\} . \frac{1}{x^{\alpha}},$$
$$\text{or} \left\{ \lambda' . U + \mu' . V + \nu' . W \right\} . \frac{1}{x^{\beta}},$$
$$\text{or} \left\{ \lambda'' . U + \mu'' . V + \nu'' . W \right\} . \frac{1}{x^{\gamma}},$$

upon the understanding that

$\lambda = G' . H'' - G'' . H'$, $\mu = H' . F'' - H'' . F'$, $\nu = F' . G'' - F'' . G'$,
$\lambda' = G'' . H - G . H''$, $\mu' = H'' . F - H . F''$, $\nu' = F'' . G - F . G''$,
$\lambda'' = G . H' - G' . H$, $\mu'' = H . F' - H' . F$, $\nu'' = F . G' - F' . G$.

The three rows of coefficients will be respectively of the degrees $(n - \beta) + (n - \gamma)$, $(n - \gamma) + (n - \alpha)$, $(n - \alpha) + (n - \beta)$.

Thus if any one of the indices α, β, γ be zero, $\Pi (\alpha, \beta, \gamma)$ becomes identical with $\lambda^{?} . U + \mu^{?} . V + \nu^{?} . W$, where the multipliers of U, V, W are of $2 n - (\alpha + \beta + \gamma)$ dimensions, i. e. of $(n - 1)$ dimensions, and may accordingly be put under the form

$$\Sigma A . x^{r} . y^{r'} . z^{r''} . U + \Sigma . B . x^{s} . y^{s'} . z^{s''} . V + \Sigma . C x^{t} . y^{t'} . z^{t''} . W,$$

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* The prefixes of any such terms (say K) may be conceived as made up of two parts, an arbitrary constant, as e and $(K - e)$; e will disappear spontaneously from the final derivee.

that is to say, becomes a *linear function* of the augmentatives, and therefore if combined with them in the process of linear elimination would *give rise* to the identity $0 = 0$.

Hence we must reject all such secondary derivatives as have zero for one of the indices of derivation. But all others, it may be shown, will be *linearly* independent of one another, and of the augmentees previously found. Hence, besides $3 \cdot \frac{n \cdot n+1}{2}$ equations of augment of the degree $2n-1$, we shall have of the same degree so many equations of derivation as there are ways of stowing away between three lockers $(n+1)$ things, under the condition that no locker shall ever be left *empty*, i. e. $\frac{n \cdot (n-1)}{2}$.*

Thus, then, in all we have $n \cdot \frac{n-1}{2} + 3 \cdot \frac{n \cdot n+1}{2} = \frac{2n(2n+1)}{2}$ equations, which is exactly equal to the number of arguments to be eliminated. Hence the final derivee can be obtained by the usual explicit rule of permutation, and moreover will be *its lowest form*, for it will contain in each term $\frac{n \cdot (n+1)}{2}$ prefixes belonging to the augmentatives of U, and a like number pertaining to those of V and of U, as well as $n \cdot \frac{n-1}{2}$ belonging to the secondary derivatives, each prefix in any one of which is triliteral, containing a prefix drawn out of those belonging to each of the proposees.

Thus every member containing $n \cdot \frac{n+1}{2} + n \cdot \frac{n-1}{2}$, i. e. n^2 of the original prefixes belonging to U, V, W, singly and respectively, the final derivee evolved by this process will be in its lowest terms; as was to be proved.

CASE A.—Indices all equal.

Method 2.

It is remarkable that we may vary the method just given by making $r+r'+r''=n-2$, $s+s'+s''=n-2$, $t+t'+t''=n-2$. The augmentatives will thus be of the degree $2n-2$.

Furthermore, we must make $\alpha+\beta+\gamma=n+2$. It will still be possible to satisfy by integer multipliers the equations

$$\begin{aligned} U &= x^\alpha . F + y^\beta . F' + z^\gamma . F'', \\ V &= x^\alpha . G + y^\beta . G' + z^\gamma . G'', \\ W &= x^\alpha . H + y^\beta . H' + z^\gamma . H''. \end{aligned}$$

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[*] Vide page 426 for the Latin version.

[these it will be useful in future to term the *equations*, x^α, y^β, z^γ being the *arguments*, and F, G, H, etc. the *factors* of decomposition] for otherwise calling the indices of x, y, z in any original argument a, b, c, their sum or n would be not greater than $(n+2)-3$, i. e. $(n-1,)$ which is absurd.

For the same reasons as in the last case no index of augmentation must be made zero: the degree of each will be $(n-\alpha) + (n-\beta) + (n-\gamma,)$ i.e. $(2n-2,)$ and their number $\frac{(n+1)n}{2}$; the number of augmentatives will be $3 \cdot \frac{(n-1)n}{2}$ linearly uninvolved, each of the degree $2n-2$, and therefore containing $\frac{(2n-1)2n}{2}$ arguments.

Now $\frac{(n+1)n}{2} + \frac{3 \cdot (n-1)n}{2} = \frac{(2n-1)2n}{2}$.

Hence the final derivee may be found, and it will be in its *lowest terms*, for every member will contain $\frac{3 \cdot (n-1)n}{2}$ letters due to the augmentative, and $\frac{3 \cdot (n+1)n}{2}$ due to the partial derivative equations; in all then there will be $3n^2$ letters in each term.

This second method being applied to three quadratic equations of the most general form, leads to the problem of eliminating between six simple equations which lies within the limits of practical feasibility, and it is my intention to register the final derivee upon the pages of some one of our scientific Transactions as a standing monument for the guidance of hereafter coming explorers*.

Scholium to Case (A).

If we attempt to carry forward these processes to quaternary systems, it becomes necessary to make

$$\left.\begin{array}{l} \alpha+\beta+\gamma+\delta=(r-2)n+1 \\ \text{or else } \alpha+\beta+\gamma+\delta=(r-2)n+2 \end{array}\right\}, \text{where } r \text{ is the number}$$

of proposees.

Now if the factors in the equations of decomposition are all integer, one of the indices of derivation must be not greater than the corresponding index in any of the original arguments, which may easily be shown to be always impossible for a system of equations, *complete* in *all* their terms, whenever their

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* Elimination between *two* quadratics leads to a final derivee made up of *seven* terms only; the final derivee of *three* quadratics is made up of at least several *thousand*; nay, I believe I may safely say, several *myriads* of terms!

number r is greater than three, if $\alpha+\beta+\gamma+\delta=(r-2)\,n+2$; but if $\alpha+\beta+\gamma+\delta=(r-2)\,n+1$ only possible for the case of $n=2$, and for that case alone.

Particular method applicable to *four Quadratics.*

Let $U=0$, $V=0$, $W=0$, $Z=0$, be four quadratic equations existing between x, y, z, t:

Make

$$\begin{array}{llll} x\,.\,U=0 & x\,.\,V=0 & x\,.\,W=0 & x\,.\,Z=0 \\ y\,.\,U=0 & y\,.\,V=0 & y\,.\,W=0 & y\,.\,Z=0 \\ z\,.\,U=0 & z\,.\,V=0 & z\,.\,W=0 & z\,.\,Z=0 \\ t\,.\,U=0 & t\,.\,V=0 & t\,.\,W=0 & t\,.\,Z=0. \end{array}$$

Also write

$$\begin{aligned} U &= x^2\,.\,F + y\,.\,F' + z\,.\,F'' + t\,.\,F''' = 0 \\ V &= x^2\,.\,G + y\,.\,G' + z\,.\,G'' + t\,.\,G''' = 0 \\ W &= x^2\,.\,H + y\,.\,H' + z\,.\,H'' + t\,.\,H''' = 0 \\ Z &= x^2\,.\,K + y\,.\,K' + z\,.\,K'' + t\,.\,K''' = 0. \end{aligned}$$

By eliminating linearly we get

$\Sigma\{F\Sigma G'.\,(H''K'''-H'''.\,K'')\}=0$, which will be of the third degree, since the factors represented by the *unmarked* letters F, G, H, K are of zero, and all the rest of *unit* dimensions.

Similarly we may obtain other equations, so that besides the *sixteen* augmentatives already written down, we have four secondary derivatives, namely,

$$\Pi\,(2\,111)=0 \quad \Pi\,(1\,2\,11)=0 \quad \Pi\,(11\,21)=0 \quad \Pi\,(111\,2)=0.$$

Thus we have *twenty* equations and as many arguments to eliminate, since a perfect cubic function of four letters contains twenty terms.

The final derivee will contain $16+4\cdot4$ letters, i. e. 32, 8 or 2^3 belonging to each system of original prefixes in each member, and will therefore be in its lowest terms: for one of the canons of form teaches us, *à priori*, that every member of the derivee deduced from any number of assumed equations must contain in each member as many prefixes belonging to one equation of the system as there are units in the product of the indices of all the rest taken together.

Corollary to Case (A).

Either of the two methods given as applicable to this case enables us to determine integer values of X, Y, Z, which shall satisfy the equation

$$X\,.\,U+Y\,.\,V+Z\,W=F\,.\,x^p.\,y^q.\,z^r,$$

where F is the final derivee and $p+q+r=3\,n-2$. For by the doctrine of simple equations we know how to express F in terms of the linear functions, out of which it is obtained by permutation, i. e. we are able to assign values of A, B, C, and their antitypes, as also of L and its antitype, which shall satisfy the equation

$$\Sigma(\mathrm{A}.x^{r}.y^{r'}.z^{r''}.\mathrm{U})+\Sigma.(\mathrm{B}.x^{s}.y^{s'}z^{s''}.\mathrm{V})+\Sigma(\mathrm{C}.x^{t}.y^{t'}.z^{t\,'}.\mathrm{W})$$
$$+\Sigma(\mathrm{L}.\Pi(\alpha,\beta,\gamma))=\mathrm{F}.x^{f}.y^{g}.z^{h} \quad . \quad . \quad . \quad (1.)$$

where A, B, C, as well as L and all the quantities formed after them, are made up of integer combinations of the original prefixes.

Now the functions $\Pi(\alpha,\beta,\gamma)$ may be expressed in three ways in terms of U, V, W, as has been already shown.

We may therefore suppose these functions to be divided into three groups, and make

$$\left.\begin{aligned}\Sigma\,\mathrm{L}\Pi(\alpha\,\beta\,\gamma) = \Sigma\,&\frac{\mathrm{Q}.\mathrm{U}+\mathrm{Q}'.\mathrm{V}+\mathrm{Q}''.\mathrm{W}}{x^{\alpha}}\\ +\Sigma\,&\frac{\mathrm{R}.\mathrm{U}+\mathrm{R}'.\mathrm{V}+\mathrm{R}''.\mathrm{W}}{x^{\beta}}\\ +\Sigma.&\frac{\mathrm{S}.\mathrm{U}+\mathrm{S}'.\mathrm{V}+\mathrm{S}''.\mathrm{W}}{x^{\gamma}}\end{aligned}\right\} \quad . \quad . \quad (2.)$$

And it is evident that the equations (1.) and (2.) lead immediately to the equation

$$\mathrm{X}.\mathrm{U}+\mathrm{Y}.\mathrm{V}+\mathrm{Z}.\mathrm{W}=\mathrm{F}x^{a+f}.y^{b+g}.z^{c+h},$$

if we call a, b, c the *greatest values* attributed respectively to α, β, γ.

Now if we suppose the first method to be followed,

$$f+g+h=2n-1.$$

And it will always be possible to make a, b, c of what values we please *subject* to the condition of $a+b+c=n-1$; for *one* at least of the indices of derivation in $\Pi(\alpha,\beta,\gamma)$ must be not greater than its correspondent among a, b, c; otherwise $\alpha+\beta+\gamma$ would be not less than $(a+b+c)+3$,

$$\text{but } \left.\begin{aligned}\alpha+\beta+\gamma&=n+1\\ a+b+c&=n-1\end{aligned}\right\}, \text{ which is absurd.}$$

Hence we can satisfy $\mathrm{X}.\mathrm{U}+\mathrm{B}.\mathrm{Y}+\mathrm{Z}.\mathrm{W}=\mathrm{F}.x^{p}.y^{q}.z^{r}$, p, q, r being subject to the condition of $p+q+r=3n-2$, but otherwise arbitrary.

Moreover, we can *not* do so if $p+q+r$ be *less* than $3n-2$, for that would require $a+b+c$ to be less than $n-1$. Now if two of the indices of derivation, as α and β, be made equal to $a+1$, $b+1$ respectively, the third $\gamma=(n+1)-(a+b+2)=(n-1)-(a+b)$, and is therefore greater than c:

so that $\alpha + \beta + \gamma$ for this case becomes greater than $a + b + c$, and the method falls to the ground.

In fact, I have discovered a theorem which lets me know this, *à priori*, a law which serves as a staff to guide my feet from falling into error in devising linear methods of solution, and the importance of which all candid judges who have studied the general theory of elimination cannot fail to recognize. To wit, if $X_1\ X_2\ X_3 \ldots\ldots X_n$ be (n) integer complete polynomial functions of n letters $x_1\ x_2 \ldots x_n$, and severally of the degree $b_1\ b_2\ b_3 \ldots\ldots b_n$; then it is always possible to satisfy the identity,

$$P_1 . X_1 + P_2 . X_2 + P_3 . X_3 + \ldots\ldots + P_n . X_n$$
$$= F . x_1^{\alpha_1} . x_2^{\alpha_2} . x_3^{\alpha_3} \ldots x_n^{\alpha_n}$$

if $\alpha_1 + \alpha_2 + \alpha_3 + \ldots\ldots + \alpha_n$ be equal to or greater than $b_1 + b_2 + b_3 + \ldots\ldots + b_n - n + 1$, but otherwise *not*[*]. This again is founded immediately upon a simple proposition, of which I have obtained a very interesting and instructive demonstration, shortly to appear, and which may be enumerated thus: "*The number of augmentees of the same degree that can be formed, linearly independent of one another, out of any number of polynomial functions of as many variables, may be either equal to or less than the number of distinct arguments contained in such augmentees, but* never greater. *The latter will be the case when the index of the augmentees diminished by unity is less than the sum of the indices of the original unaugmented polynomials each so diminished; the former, when the aforesaid index is equal, to or greater than the aforesaid sum.*"

To return to the particular case of finding X, Y, Z to satisfy $X . U + Y . V + Z . W = F . x^p . y^q . z^r$.

This has been already done according to the first method; if we employ the *second* method of elimination we shall have

$$f + g + h = 2n - 2;$$

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[*] Hence it is apparent, that in applying the method of multipliers, a curious and important distinction exists between the cases of there being two equations, and there being a greater number to eliminate from: for in the first case the element of arbitrariness needs never to appear; in the *latter* it cannot possibly be excluded from appearing in the multipliers.

This will explain how it comes to pass that the method of the text may be employed to give *various* solutions of the $X . U + Y . V + Z . W = F . x^p . y^q . z^r$; thus not only can (p), (q) and (r) be variously made up of $(f + a)$, $(g + b)$, $(h + c)$, but also $\Pi\,(\alpha, \beta, \gamma)$ when two of the indices (α, β suppose) are each not greater than the assigned greatest values a, b may be made to figure indifferently either under the form

$$\frac{\lambda . U + \mu . V + V . W}{x^\alpha} \text{ or that of } \frac{\lambda' . U + \mu . V + V' . W}{x^\beta}.$$

but, now since $\alpha + \beta + \gamma = n + 2$, we shall easily see by the same method as above, that the least value of $a + b + c$ (where a, b, c denote respectively the greatest values of α, β, γ, appearing in the denominator of the fractional *forms* used to express $\Pi(\alpha, \beta, \gamma)$, will be one greater than before, or n; so that $f + g + h + a + b + c$ will still be equal to $3n - 2$, as we might, *à priori*, by virtue of our rule, have been assured.

Ternary Systems.

Case (B).—*Two of the indices equal; the third* less *by a unit.*

Let $U = 0$, $V = 0$, $W = 0$, be the three given equations severally of the degree $n, n, (n-1)$.

Make $r + r' + r'' = n - 2$, $s + s' + s'' = n - 2$, $t + t' + t'' = n - 1$, by multiplying U into $x^{r} . y^{r} . z^{r''}$, V into $x^{s} . y^{s'} . z^{s''}$, W into $x^{t} . y^{t'} . z^{t''}$, we obtain augmentees each of the same, namely, the $(2n - 2)$th degree.

The number of these is, $\frac{(n-1)n}{2} + \frac{(n-1)n}{2} + \frac{n . n+1}{2}$.

Again, make $\alpha + \beta + \gamma = n + 1$.

It will still be possible, as before, to form equations of decomposition in which $x^{\alpha} y^{\beta} z^{\gamma}$ are the arguments, and affected with *integer* factors. For if we look to W *even*, all its arguments are of the form $x^{a} . y^{b} . z^{c}$, where $a + b + c = (n-1)$, and each of these cannot be less than its correspondent, for that would be to say that $(n-1)$ is not greater $(n+1) - 3$, *à fortiori*, U and V can be decomposed in the manner described. Thus, then, we shall obtain as many secondary derivees as in the last case (Method 1.), i. e. $\frac{n . (n-1)}{2}$ (since $\alpha + \beta + \gamma$ is still equal to $\overline{n+1}$), as before. Moreover, each of these will be of $(n-\alpha) + (n-\beta) + (n-1-\gamma)$, i. e. of $2n - 2$ dimensions.

Altogether, therefore, we have

$$\left\{\frac{(n-1)n}{2} + \frac{(n-1)n}{2} + \frac{n(n+1)}{2}\right\} + \frac{(n-1).n}{2}$$

linear independent equations of the degree $2n - 2$, and the number of arguments to eliminate is $\frac{(2n-1)2n}{2}$. Now these two numbers are equal. Thus we obtain a final derivee containing of U's coefficients $\frac{(n-1)n}{2} + \frac{(n-1)n}{2}$, an equal number of V's, but of W's $\frac{n \,.\, (n+1)}{2} + \frac{(n-1)\,n}{2}$; now $n\,(n-1)$, $n\,(n-1)$ and n^2 exactly express the number that ought to appear of each of these respectively: hence the final derivee is clear *of irrelevant factors.*

Ternary Systems.

Case (C). *Two of the indices equal; the third one,* greater *by a unit.*

Here, calling (n) the highest index, the augmentees must each be made of the degree $(2\,n-3,)$ their number will evidently be $\frac{(n-2)\,(n-1)}{2} + \frac{(n-1)\,n}{2} + \frac{(n-1)\,n}{2}$, making the sum of the indices of derivation now, as before, equal to $(n+1)$; it will be still possible to form integer equations of decomposition, which will give rise to augmentatives of the degree $(n-\alpha) + (n-1) - \beta + (n-1) - \gamma$, i. e. of $(2\,n-3)$ dimensions. The total number of equations, what with augmentatives and secondary derivatives, will be

$$\left\{\frac{(n-2)\,(n-1)}{2} + \frac{(n-1)\,n}{2} + \frac{(n-1)\,n}{2}\right\} + \frac{n\,(n-1)}{2} = \frac{4\,n^2-4\,n+2}{2} = \frac{(2\,n-2)\,(2\,n-1)}{2},$$

i. e. is equal to the exact number of distinct arguments contained between them.

Also the final derivative will contain in each member $\frac{(n-2)\,(n-1)}{2} + \frac{n\,.\,(n-1)}{2}$, i. e. $(n-1)\,(n-1)$, letters belonging to the first equation, and $\frac{(n-1)\,n}{2} + \frac{n\,.\,(n-1)}{2}$, i. e. $n\,.\,(n-1)$ belonging to those of the second and of the third, and will therefore be in its *lowest terms.*

Corollary to Case (B) and (C).

It is not necessary, after all that has been already said, to do more than just point out that the processes applicable to these cases enable us to determine X, Y, Z, which satisfy the equation $X\,.\,U + Y\,.\,V + Z\,.\,W = F\,.\,x^f\,.\,y^g\,.\,z^h$,

$$\text{where } f+g+h = 3\,n-3 \text{ for case (B).}$$
$$\text{and } f+g+h = 3\,n-4 \text{ for case (C).}$$

22 Doughty Street, Mecklenburgh Square, April 29, 1841.

[To be continued.]

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

[Continued from p. 372.]

The Cacodyl Series (continuation).

PROTOCHLORIDE of cacodyl, $Kd\underline{Cl}^2$, cannot be obtained pure by distilling the oxide of cacodyl with hydrochloric acid; a basic salt is then formed. The best method is to distil hydrargochloride of cacodyloxide with hydrochloric acid; the product must be dried over lime and chloride of calcium, and then distilled alone in a tube filled with carbonic acid:

$$\left.\begin{array}{c} Kd\,O + Hg^2\,Cl^4 \\ H^2\,Cl^2 \end{array}\right\} \begin{array}{l} Kd\,Cl^2 \\ H^2\,O \\ Hg^2\,Cl^4. \end{array}$$

The liquid thus obtained is æthereal, colourless, does not solidify at — 45° C., and is converted a little above 100° C. into a colourless gas, which inflames in the air. The fluid itself, when burnt, deposits metallic arsenic, or arsenious acid, accordingly as the air has more or less access. In oxygen it explodes violently when heated. When the air has but very slow access to it, it deposits beautiful colourless crystals. In chlorine it inflames and burns with deposition of carbon. Its smell is extremely penetrating and stupifying. In greater quantities it excites to such an extent the mucous membrane of the nose, that it swells, and blood drops from the eyes. It does not dissolve in water, but imparts its odour to it. In æther it is insoluble, but soluble in alcohol. Dilute nitric acid dissolves it without decomposition, the concentrated acid causes explosion. The chlorine is precipitated entirely by silver salts. Caustic lime and baryta do not decompose it, except at a high temperature. An alcoholic solution of potash forms chloride of potassium, and an æthereal volatile substance containing no chlorine ($C^4\,H^{10}\,As^2$?), easily soluble in water and alcohol. Sulphuric and phosphoric acids separate the hydrochloric acid. Formula $C^4\,H^{12}\,As^2\,Cl^2$; sp. gr. of vapour 4·56; calculated 4·86.

By passing dried hydrochloric acid gas into alkarsin, a hydrate of protochloride of cacodyl is formed; it is a thick oily fluid, which is decomposed by distillation.

Protoiodide of Cacodyl, $Kd\,\underline{I}^2$.—Formed by the distillation of cacodyloxide with concentrated hydriodic acid; in the receiver under the water there appears a yellowish oily liquid, which by slow cooling forms beautiful rhomboidal laminæ of a yellow substance; it is then cooled, and the iodide remaining fluid is separated, and again distilled with hydriodic acid. In order to free it from water, it is allowed to stand some days in a closed tube in contact with lime and chloride of calcium, and then distilled in a tube filled with carbonic acid until the half or, at the most, two thirds has passed over. The iodide thus produced is a thin yellowish liquid, smells somewhat similar to the chloride, has a high specific gravity; remains fluid at — 10° C.

Boiling point considerably above 100° C. The vapour of it is yellowish. If allowed to stand in the air, forms beautiful prismatic crystals. The iodide is soluble in æther and alcohol; insoluble in water; decomposed by sulphuric and nitric acid. When heated in the air it burns yielding vapour of iodine. Formula $C^4\ H^{12}\ As^2\ I^2$. The specific gravity of the vapour could not be determined, because it is easily decomposed by mercury. The calculated is 5·816.

Protobromide.—Formed by distilling concentrated hydrobromic acid with hydrargochloride of cacodyloxide; it forms a fuming yellow liquid, very similar in its properties to $Kd\ \underline{C}l$.

Protofluoride is obtained in a manner analogous to the preceding; it is a colourless fluid of insupportable smell, insoluble in water, but suffers decomposition by it. It attacks glass; $C^4\ H^{21}\ As^2\ Fl^2$.

*Hydrargo-chloride of Cacodyloxide**.—When a dilute alcoholic solution of oxide of cacodyl is added to a dilute solution of corrosive sublimate, a voluminous white body is precipitated, which consists of the above compound mixed with calomel; from this it may be freed by repeated crystallizations. It may be formed with any other compound of cacodyl which is similarly constituted to the oxide. An excess of sublimate decomposes the salt. Formula $C^4\ H^{12}\ As^2\ O + Hg^2\ Cl^4$. Bunsen leaves it for the present undetermined whether the oxygen is only 1, 1½ or 2 atoms. He assumes that sublimate is contained in it, because potassa precipitates oxide of mercury, and hydriodic acid produces the scarlet iodide and iodide of cacodyl, &c. &c. By distillation with phosphorous acid it is decomposed into chloride of cacodyl and calomel. Chloride of gold is reduced by it.

100 parts of boiling water dissolve 3·47 parts of the salt; at 18° C. the solution contains only 0·21 parts. It is soluble in alcohol, inodorous if not brought into the nose; taste disagreeably metallic; in larger quantities is poisonous, &c.

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* We may here remark, that this name is a literal translation from the German Quecksilberchlorid Kakodyloxyd. We do so because an anonymous writer has criticised our notices, on account of the nomenclature, in a short paper in the Phil. Mag.

The *hydrargobromide* is very similar to the hydrargochloride.

Basic chloride of cacodyl is formed by treating the chloride with water, or by distilling with hydrochloric acid. Formula $(C^4 H^{12} As^2 + O) + 3 (C^4 H^{12} As^2 + Cl^2)$, or $Kd O + 3 Kd Cl^2$. Density of the vapour 5·46; according to calculation it should be 5·35. This substance is very similar to the chloride; it boils at 109° C.

Basic bromide of cacodyl is prepared in the same manner as the above, to which it is very similar. It is yellow, becomes colourless on heating, and acquires its yellow colour again on cooling. Formula $(C^4 H^{12} As^2 + O) + 3 (C^4 H^{12} As^2 + Br^2)$, or $Kd O + 3 Kd Br^2$.

Basic iodide of cacodyl is produced, together with the iodide, on distilling the oxide with hydriodic acid. It is deposited from the neutral compound in yellow crystalline crusts. It may be purified by recrystallization in alcohol and pressure between bibulous paper. The water may be got rid of by allowing it to stand some days in a fluid state in contact with chloride of calcium, and then distilling. It absorbs oxygen with such rapidity, that Bunsen was not able to make an analysis of it.

The iodide of cacodyl and the oxide may be mixed together without combining, if they are both anhydrous; but if a drop of water be added, the solution becomes instantly one solid mass, which consists of the basic iodide. The substance is very soluble in alcohol, but little in water: melts below 100° C., and may be distilled over unchanged. In the air it evolves white vapours, and heats so much that it becomes fluid, and sometimes inflames. By distillation with hydriodic acid it cannot be reconverted into the neutral iodide, and in so far it differs from similarly constituted inorganic salts.

M. Bunsen intends continuing his researches on these interesting bodies.

Oil of Esdragon—Oil of Sabine.

M. Laurent has analysed the oil of Esdragon. Its formula is $C^{24} H^{32} O^2$; it boils at 206° C.; has a specific gravity of 0·945; that of its vapour was found by experiment to be 6·157, calculated 6·158. It combines with sulphuric acid to form sulphodraconic acid; the formula of the baryta salt is $C^{24} H^{32} O^2, \dddot{S} + \dot{B}a$; treated with nitric acid, it gives five new crystallizable acids.

Oil of Sabine has the same constitution and same properties as oil of turpentine. By the action of ammonia on oil of cinnamon M. Laurent obtained a substance which he calls cinnhydramid, $C^{18} H^{18} N_{\frac{4}{3}} O$; he remarks, that the law of substitution has not been followed in this case; the oil has lost 1 atom of oxygen and taken $H^3 N$.

Bromide of camphor, $C^{20} H^{32} O^2 + Br^4$, is crystallized, deliquesces in the air; bromine flies off and leaves camphor; this change is effected immediately by ammonia. When distilled it is also decomposed, but generally some hydrobromic acid, and an oily bromide are formed, $C^{20} H^{32} O^2 + Br^4 = C^{20} H^{30} Br^2 O^2 + H^2 Br^2$. (*Compt. Rendus*, x. p. 531.)

Oil of Potatoe-Spirit—Amilen.

M. Cahours has published some more of his experiments on the oil of potatoe-spirit. *Hydrochlorate of amilen* is obtained by distilling equal parts of the oil and chloride of phosphorus; the product is washed with water in which a little potassa has been dissolved, dried over chloride of calcium, and distilled. It is a colourless, aromatic, agreeably smelling fluid, insoluble in water; boils at 102°; does not change litmus paper, and is not decomposed by nitrate of silver. It burns with a flame with green margin. Formula $C^{10} H^{22} Cl^2$. M. Cahours examined the action of chlorine on this substance when in the sunshine; hydrochloric acid gas was developed, but soon ceased. The product was a clear colourless liquid, smelling like camphor. Formula $C^{10} H^6 Cl^{18} = C^{10} H^{22} Cl^2 - H^{16} + Cl^{16}$.

Acetate of amilen is easily obtained by distilling a mixture of two parts of acetate of potash, one part potatoe-oil, and one part sulphuric acid; the product is dried by means of chloride of calcium, and distilled over oxide of lead. It is a colourless volatile liquid, which boils at 125°. The smell is somewhat similar to that of acetic æther; lighter than water; insoluble in the same, but soluble in alcohol, æther and potatoe-oil; is decomposed by an alcoholic solution of potassa into acetate of potash and potatoe-spirit oil. Density of the vapour 4·458; calculated 4·475. If chlorine be passed into this æther heated to 100°, until no more hydrochloric acid gas is given off, a product is obtained, whose formula is $C^4 H^6 O^3, C^{10} H^{18} O Cl^4$, similar to Malaguti's "Ether acétique Chloruré." In the sunshine this body is acted on by chlorine; small crystals are formed, but the products have not been examined. By the action of spongy platinum the oil of potatoe-spirit is converted into valerianic acid. (*Ann. de Chim. et de Phys.* lxxv.)

On the Resins of Benzoin. By M. Van der Vliet.

Benzoin is pulverized and boiled with carbonate of soda; one of the resins is dissolved, the brown solution is filtered, treated with hydrochloric acid, and thrown boiling hot on a filter. The resin remains on the filter, and a solution of benzoic acid passes through. The benzoin is boiled two or three times, as stated above, and then treated with æther, which dissolves half of it; on evaporation the æther deposits a resin. The third resin which remains behind is soluble in alcohol. This latter is Berzelius's betaresin, that soluble in æther the alpharesin, and the third gammaresin; these resins thus prepared are not perfectly pure. The gammaresin must be boiled with water to free it from benzoic acid, it then melts at 180° C, and is pure. The alpharesin forms an insoluble compound with carbonate of soda; by long boiling of the benzoin with carbonate of potash, all alpharesin is extracted, for it is converted into gammaresin and becomes soluble. Carbonate of soda effects this change very slowly. After the extraction of the alpharesin by æther, there remain behind betaresin, the alpharesin compound and the impurities; these latter remain when the mass is boiled with alcohol; on cooling the alpharesin compound is precipitated; the betaresin remains dissolved. The properties of these resins have been described by Unverdorben, Poggendorff's *Annalen*, xvii., Berzelius's *Chemie*, vii.

The formula for the alpharesin is $C^{70} H^{84} O^{14}$; the atomic weight, as found from the analysis of the lead compound, is 7214·20; calculated 7274·58. Betaresin is $C^{40} H^{44} O^{9}$; the analysis of the lead salt gives, as atomic weight, 4099·9; calculated 4231·94.

Gammaresin is $C^{30} H^{40} O^{5}$; atomic weight 3042·64; according to analysis 3062·81. It is probable, from what has been said above concerning the metamorphosis of alpharesin into gammaresin, that benzoin contains only two resins for

$$\begin{array}{rl} \text{Alpharesin} \;\ldots\ldots\ldots & C^{70} + H^{84} + O^{14} \\ \textit{minus}\ \text{Gammaresin} & C^{30} + H^{40} + O^{5} \\ \hline \text{Betaresin}\ldots\ldots & C^{40} + H^{44} + O^{9} \end{array}$$

They may be considered as oxides of $C^4 H^4$ combined with a hydrocarbon; thus

$$\begin{array}{lrl} \text{Alpharesin} \ldots\ldots & 14\,(C^4 H^4)\, O^{14} & + \; 14\, C H^2 \\ \text{Betaresin} \ldots\ldots & 9\,(C^4 H^4)\, O^{9} & + \;\;\; 4\, C H^2 \\ \text{Gammaresin} \ldots & 5\,(C^4 H^4)\, O^{5} & + \; 10\, C H^2 \end{array}$$

(*Annalen der Pharmacie*, xxxiv.)

[The account of Mr. Johnston's experiments, *Phil. Mag.* vol. xvii. p. 384, is too short to allow of any conclusions being drawn from it. I am not acquainted with the methods Mr. J. used for purifying his resins. It appears, however, that he has not observed the alpharesin of Van der Vliet. In paragraph 4, he says that potassa decomposes benzoin into $C^{40} H^{22} O^{9}$ ($C^{40} H^{44} O^{9}$) and $C^{40} H^{30} O^{9}$. The former is evidently the betaresin; probably all alpharesin had been decomposed into the other two. The soluble resin, $C^{40} H^{30} O^{7}$, agrees tolerably well with the gammaresin. I regret extremely that I have no opportunity of obtaining further particulars of Mr. Johnston's experiments on the subject.—H. C.]

Veratric Æther.

Dr. Will has prepared veratric æther by dissolving the acid in strong alcohol, and saturating it with hydrochloric acid gas. On mixing with water the æther separates as a thick oily fluid, which, by washing with a dilute solution of carbonate of soda, gradually solidifies. It is then dried by means of sulphuric acid under the air-pump. It melts at 42° C.; is almost inodorous; has a bitter burning taste; scarcely soluble in water; soluble in alcohol, out of which it crystallizes in groups of acicular crystals; not volatile without partial decomposition. Specific gravity at 18° C. = 1·141. Formula $C^{4} H^{10} O, C^{18} H^{18} O^{7}$. (*Annalen der Pharmacie*, xxxvii.)

Action of Anhydrous Sulphuric Acid on Anhydrous Camphoric Acid.

M. Walter has examined the action of anhydrous sulphuric acid on anhydrous camphoric acid. The camphoric acid is dissolved by fuming sulphuric acid; on heating carbonic oxide is developed, but no other gas. By saturating the fluid with carbonate of baryta a soluble salt is obtained, which must be evaporated *in vacuo*; it does not crystallize, but forms an amorphous colourless mass, soluble in water and alcohol. Formula $C^{9} H^{14} O^{3}, SO^{2}, BaO$. Camphoric acid is $C^{10} H^{14} O^{3}$; the decomposition is thus $C^{10} H^{14} O^{3} + SO^{3} = C^{9} H^{14} O^{3} + SO^{2} + CO$. The lead salt is $C^{9} H^{14} O^{3}, \ddot{S} + \dot{P}b$. The potassa salt is crystalline; the lime salt is amorphous; they have both similar formulæ. The acid itself may be obtained crystallized, but not dry. (*Annales de Chemie.*)

M. Walter has published some experiments on the action of anhydrous phosphoric acid on camphoric acid. The action is quite different from that of sulphuric acid; the one atom of carbon has its place taken by the sulphurous acid in the former case, but in the latter the decompositions are more complicated. When the two acids are distilled together much gas is evolved, and an oily fluid passes over. The gas consists of one volume of carbonic acid, and four volumes of carbonic oxide. The fluid is a hydrocarbon, C 88·4; hydrogen 11·6. It might be oil of turpentine, but M. Walter considers it more probable that it is a kind of naphtha, containing 89 per cent. carbon, and explains his finding too little by the circumstance that the fluid contains phosphuretted hydrogen, from which it is with difficulty freed. (*Annales de Chim. et de Phys.* lxxv.)

LXXI. *On the Manufacture of Platinum.* *By* A CORRESPONDENT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WILL you allow me, through your pages, to suggest to chemists that the price of that necessary article, platinum, might be reduced by means of the electrotype process? I believe that the high price of this metal is in some measure due to the labour required to reduce it into a malleable state by the method of Wollaston. This portion of the expense might, I think, be reduced to a very small part of the whole, if the metal were at once reduced from its solution by the slow action of electricity, as this mode involves no labour, while the necessary apparatus is cheap. Might not the same method be applied to reduce nickel to a malleable state? This being a cheap metal, not liable to rust, it might perhaps be advantageously used for some purposes for which platinum is too expensive.

I am, Gentlemen, your obedient servant,

C.

LXXII. *On testing for Arsenic and Antimony by Hume's Process.* *By* J. MARSH, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IF you should think the following communication of sufficient importance, I beg you will give it a place in your valuable Journal.

In testing for arsenic I have found, by repeated experiments, that Hume's test (the ammonia-nitrate of silver) is extremely useful as a discriminative test for arsenic or antimony. The way that I use it is simply as follows:—After the matter to be tested has been acted on in my apparatus, a piece of common window-glass, (which I prefer, as it can be obtained with ease,) porcelain or mica, is to have one of its surfaces moistened with Hume's test; it is then to be held horizontally, with its moistened side downwards, directly over the ignited jet of gas, about half an inch from the tip of the flame. If arsenic be present, the well-known characteristic lemon-yellow colour is instantly produced; if antimony be in the mixture, a curdy-white precipitate is obtained: if, on the contrary, neither arsenic nor antimony is in the matter under examination, the hydrogen instantly reduces the silver of the test-liquor to the metallic state.

It is really beautiful to see the admirable manner in which this test performs its duty, and I submit the same with confidence to the attention of your numerous readers and correspondents.

I am, Gentlemen, yours truly,
J. Marsh.

Royal Arsenal, Woolwich, May 10th, 1841.

LXXIII. *On the Composition of Chalk Rocks and Chalk Marl by invisible Organic Bodies: from the Observations of Dr. Ehrenberg.* *By* Thomas Weaver, *Esq., F.R.S., F.G.S., M.R.I.A, &c. &c.*

[Continued from p. 397.]

On the Geographical Distribution of Living Polythalamia on the African and Asiatic Coasts of the Mediterranean, and in the Red Sea.

THE materials collected by Dr. Hemprich and myself in the Mediterranean refer to four points on the Libyan coast, and one point on the Syrian coast. In regard to a second point on the latter coast (St. Jean d'Acre), I have acquired a knowledge of some forms from the collection of Dr. Parthey.

From the Red Sea nine forms were made known to us by d'Orbigny, collected from sand presented to him by Deshayes. But from the collections made by Dr. Hemprich and myself from thirteen points along the whole length of the Red Sea, it appears that very numerous forms exist. Of seven of those points, one occurs on the western (African) coast at Suez, and six on the eastern (Arabian) coast, namely, at Tor, Erraie and el Ard, Moileh, el Wusch and Gumfüde; and of the remaining six, five are islands on the Arabian side, namely, Sanafer, Maksure, Barkan, Sanac and Ketumbul, and one an island on the African side of the Red Sea, namely, Massaua.

It is possible that by repeated and closer examination of the marine productions collected by us, many other Polythalamia may be found besides those already discovered. In the mean time, as a preliminary, I have drawn up a list of the species hitherto met with*. From this it results that the total num-

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* Of d'Orbigny's nine species from the Red Sea, there are three which I cannot identify, namely, *Triloculina bicarinata, Quinqueloculina limbata,*

ber of species of Polythalamia observed in the Red Sea are *fifty*, and in the Mediterranean, on the Libyan and Syrian coasts, *twenty-seven*. The new species derived from the two seas amount to *fifty-four*, of which *twenty-seven* species are peculiar to the Red Sea, and *seventeen* are common to both seas. Particularly worthy of notice is the wide distribution and massy development of the *Peneroplis planatus* and *Sorites Orbiculus*, which are rare on the European coast. These forms are not only present almost everywhere in the East, but constitute the predominant masses. On the other hand, the *Rotalia Beccarii*, which composes the Italian hills, occurs only singly and very rarely in the Red Sea; and I nowhere found it on the Libyan and Syrian coasts. The *Sorites Orbiculus* I have also from St. Domingo.

In reviewing these subjects, even a superficial comparison of them with the contents of the chalk and chalk marl, is attended with the striking result, that none of these living forms are found among the animalcules of the chalk, not even among those which compose the compact limestone of the Egyptian and Arabian rocks, and which are still partly washed by the sea near Hamam Faraûn.

Remarks on Polythalamia.

After a preliminary view of the researches of earlier labourers in this branch of zoological inquiry, Dr. Ehrenberg observes:—A lively interest respecting the minute Polythalamian bodies which enter into the composition of sea-sand was excited anew by the work of Alcide d'Orbigny in 1826, in which are contained a great number of new species, while many of those which were previously known are examined with greater care, and an improved and easier view is taken of the whole subject. By his active exertions he had collected between 600 and 700 species from the sea-sand of France, Italy, England, the Isle of France, Sandwich Islands, the Malouine and Marian Isles, &c., of which, however, only 425 received names. The whole mass of these microscopic animalcules, which he again decidedly associates with the Mollusks and Cephalopods,

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and *Q. punctata*; but the other six are probably those with which I have become acquainted, and to which I have therefore given the same names, namely, *Textularia communis, Calcarina Defrancii, C. Gaudichaudii, Quinqueloculina sulcata,* and *Vertebralina striata.* His *Assilina* (*Nummulina*) *nitida* I hold to be the *Sorites Orbiculus.*

Although I possess and have compared many of the Polythalamia which have been described by d'Orbigny derived from the same localities, yet I am in want of a great number of the originals named by him, and as this author has generally given new names unaccompanied by descriptions, I have not in most cases been able to determine to what form the name given by him belongs.

but in a distinct order under the name of *Foraminifères*, are distributed by him into five families, according to the spiral or other form in the grouping of the cells; these families comprising fifty-two genera. On this work Deshayes made various critical remarks in the *Dictionnaire Classique*. D'Orbigny expressly states that the animal of the Polythalamia (his Foraminifera) resembles the Sepia in the structure of its body, although much smaller, and then proceeds to give the essential characters of the living body of the Polythalamia, yet without naming specifically or generically any one animal from which they were taken[*].

Both Blainville and Dujardin have made the correct observation that the minute shells of the Polythalamia are external cases, and not, as incorrectly viewed by Denys de Montfort and Alcide d'Orbigny, internal bones. Yet in referring the microscopic so-called Cephalopods to the Infusoria, Dujardin commits a mistake[†]. It was this contradiction between observers that induced Férussac, in his great work, *Histoire Naturelle des Mollusques*, to exclude the Foraminifers from the class of the Mollusks; and others entertained similar objections, yet without assigning to them a correct position.

In the year 1831 I laid before the Academy contributions to the knowledge of Coral animals, with an attempt to class them physiologically; which attempt was entirely founded on my own observations of the living animalcules, when, accompanied by Dr. Hemprich, I travelled on the Red Sea in the years 1823 and 1825. In that work I designated the Coral animals as composed of two strongly marked organically distinct groups, under the names of *Anthozoa* and *Bryozoa*. In the year 1831 also, I communicated in the *Symbolæ Physicæ* the first development made of the complicated structure of the *Halcyonella stagnorum*, one of the Bryozoa, and showed that it was quite similar to that of Flustra.

The researches of Dujardin in 1835 gave an entirely new direction to the ideas which had been formed of the Polythalamia, showing that not a trace of resemblance was to be found between them and Sepia; on the contrary, the greatest simplicity of structure became apparent, bespeaking a simple animal body covered by a shell, with the power of extending or contracting itself at will. But when Dujardin expressly compares the Polythalamia to the *Proteus* (*Amoeba*) of the Infusoria, such an association cannot be admitted, unless it be first proved that a polygastric structure exists in those bodies. He has given to them the new name of *Rhizopodes*.

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[*] *Annales des Sciences Naturelles*, 1826, t. vii. p. 245.

[†] *Annales des Sciences Naturelles. Seconde Série*, t. iv. p. 343, 1835.

I showed, in 1837, that the Polythalamia could not well possess an organization similar to that of the Infusoria, as not a single known true species of Infusoria has a calcareous shell; and I had, in 1823, discovered, as I conceived, a true living Polythalamia of earlier authors, resembling in organization the very complex Flustra. The correctness of this view was fully established in 1839, after having examined anew, according to my improved method, the small *Nautilus Orbiculus* of Forskâl, which d'Orbigny designated in 1826 as *Nummulina* (*Assilina*) *nitida*, specimens of which I had collected from the sand of the Red Sea in 1823, and which I have named *Sorites Orbiculus*. The result proved that the disc-like shell was a Polypary, often composed of more than one hundred single animalcules, the cells of which quite resemble those of a Flustra, the animal putting forth and retracting from six to eight tentacula. And I even discovered in the interior of the single cells well-preserved siliceous Infusoria, the last food taken by the animal; and in some of them also small globular bodies, which, without much constraint, may be considered as eggs. Though I had at an early period observed that the disc was composed of many cells, yet I could not perceive an opening to them; but the discovery of Infusoria in their interior led me to consider by what means they could have been introduced. Reflection reminded me that I had often seen Coral animals which in the expanded state exhibited many large bodies with tentacula and a large mouth, yet when contracted left scarcely a trace of the openings through which they were protruded from the common Polypary. As such I remembered *Pennatula*, *Lobularia*, *Halcyonium* and similar forms, in which I had frequently observed, that in the skin of the animal existed calcareous particles, which on the contraction of the skin so completely closed the opening as to render it no longer perceptible. Renewed examination of the closed surface of the cells of the *Nautilus Orbiculus*, Forskâl, now showed to me that in them also dendritic calcareous particles exist, the close approximation of which closes the cell, so that the cover of the cell is in fact the dried skin of the animalcule. I now made an experiment in proof, by dissolving the small shell in dilute muriatic acid, in order to obtain the animal body in a free state; and it succeeded perfectly. I obtained as many animalcular bodies as there were cells, connected together by band-like processes, and in the interior of many of them there were well-preserved siliceous Infusoria. I then treated in the same manner the *Flustra pilosa* and *F. membranacea* of the Baltic, and found in their interior also siliceous Infusoria. The same results followed a similar examination of the shells of *Rotalia* from the sand of Rimini, of the shells of *Peneroplis planatus*, *Pavonina Antillarum*, and of *Orbiculina numismalis* from the sea-sand of St. Domingo, as well as of other shells with their animals from the sand of the Red Sea and the Mediterranean; so that now a view is obtained of the more general organization of the principal groups of the Polythalamia.

It results clearly from what has been said in respect of these species, which are so common and widely distributed, and which have hitherto been designated in systems as small Nautili, that the straight-jointed shells of Nodosaria (formerly viewed as Orthocera), as well as the spiral shells of Rotalia, Cristellaria, &c. (considered as Nautili or Ammonites), and the shells of Biloculina resembling vermiform tubes (*Serpula*), are none of them internal calcareous parts which were encased by an animal body, similar to the internal bone of Sepia, or the cylindrical spiral bone of Spirula; but, on the contrary, that they are external calcareous shells, bearing analogy to those of Mollusks, or more correctly to those of Flustra and Cellepora, which, after separation by an acid, disclose and render visibly free the internal simple body or the Polypary, exhibiting precisely the same form. If the shell of Polythalamia be frequently perforated with pores, this is no proof that no other openings exist, or that the animals receive nourishment through many tubes, for the same structure is not unfrequently found in Flustra accompanied with the peculiar opening from which the fore-part of the animal body may be protruded; and in these exist also fringe-like filaments, which are extensile and retractile, and by no means to be compared to the pseudopodia or variable feet of Amoeba, but probably bear analogy to the mantle fringes of many Mollusks, applicable to the purposes of creeping and attachment, and for which perhaps they were specially designed. Moreover, Flustra possess a distinct large animal organization; and the siliceous Infusoria, and probable eggs found in Polythalamia, clearly bespeak in them also similar relations, the discovery of which, however, had hitherto been prevented by the calcareous encasement and the minuteness of the objects.

It has resulted from the examination of the soft small animal bodies of living Polythalamia, that while many resemble Flustra or Eschara assembled in families or polyparies, each such family being often composed of hundreds of much minuter single animalcules, many others are single animals after the manner of Mollusks. Hence arise external characters and forms which have often a reference to very different relations, which it is first necessary to distinguish before we can succeed in obtaining a clear view of the whole. The assiduous and careful labours of d'Orbigny retain their full value, serving as a basis to all future researches; and if in the present communications I shall have succeeded in turning the inquiry into a more physiological channel, my object will be attained.

To the term *Polythalamia*, (originally introduced by Dr. Breyn, of Danzig, in 1732,) a different extension or signification under other names has been given by different authors. To remove this unsteadiness and wanton change of names, which only lead to obscurity, it appears advisable to apply the term Polythalamia, in preference, as Soldani had done, to that group in which the animalcules actually live in many cells, and do not, like the Nautili, possess many empty cells. This distinction, that the animal of the Polythalamia has no empty cells, but that all its cells are simultaneously occupied, is of particular importance in their systematic arrangement among other animal bodies. Where there are many cells, they consist either of so many single animals, the whole constituting a polypary, or of organically filled integrant portions of one and the same individual forming groups. Both structures are foreign to the true Cephalopods. The shell-bearing Cephalopods may with Linnæus be divided into the *unilocular* and *multilocular*.

On the other hand, the want of a sipho which has been assigned as a character of Polythalamia, and from which they were named *Asiphonoidea* by De Haan, is incorrect, inasmuch as many really possess a part which may be fully compared to a sipho, if not in function, yet in form, namely, the tube which connects the separate cells of Nodosarina and of all individual many-celled forms. It is only in the *Miliolina* family among the simple Polythalamia, and it is only in the families of *Asterodiscina* and *Soritina* among those forming polyparies, that the want of a sipho is really necessary, because they live individually in single cells. But all the Nodosarina, Textularina, Uvellina, Rotalina, and Plicatilia among the simple Polythalamia, and the Frumentarina, Helicosorina, and Alveolinea among those which form polyparies, possess tubes of connexion between the cells, which very frequently resemble also in form the sipho of the Nautilus. D'Orbigny, it is true, states also that the cells of Foraminifers are connected by several openings; that, however, proceeds from an erroneous view, for such Polythalamia alone present several openings at the border of the cells, whose calcareous surface is interrupted in the form of a net-work, exhibiting often a relation analogous to that which is frequent in Madrepora and Astræa, in which the soft body is not divided or sharply cut off by compact calcareous plates, but the soft parts appear interwoven with minute calcareous rods, in a lattice-like manner. These numerous small connecting openings, which are sometimes visible in some of the Rotalia and Rosalina, and also in the Textularia, I do not consider essential, but hold that the true channel of connexion has always a large diameter, and is simple for each single animal. The erroneous view of d'Orbigny and of all his followers becomes so complicated, that polyparies are held to be single animals, and consequently the various connecting openings to be those of a simple individual.

With respect to d'Orbigny's genus *Nummulina*, although it has derived advantage from his diligent investigations, I consider it as composed of very heterogeneous elements, which belong to quite different divisions of animals. Some species of the sub-genus *Assilina*, and perhaps all of them, may belong to the families *Soritina* and *Asterodiscina*, while the *Assilina nitida* of the Red Sea is assuredly Forskål's *Nautilus Orbiculus*, that is, *Sorites Orbiculus*.

I am of opinion that all those species which are provided with visible traces of mouths or openings, as in Lamarck's genus *Lenticulina* with d'Orbigny's character of Nummulina, are to be received among the Polythalamia; but that all such species as have the form of a lens or disc, and are provided with internal spiral cells, but without a trace of such mouths, the cells being moreover separated from the external surface by thick calcareous layers, are to be considered as internal bones. These mouthless Nummulina are rather to be ranged with the Velellida of the Acalepha along with *Porpita*, where similar internally cellular coin-shaped bones exist. The considerable size of many Nummulina is also striking and foreign to Polythalamia, but agrees very well with the family of the Velellida, as also in the want of traces of the attachment of muscles, and in the want of a sipho or channel of connexion between the cells. Until better informed, therefore, I decidedly exclude the mouthless Nummulina from the Polythalamia, and retain only Lamarck's Lenticulina in the sense attached to d'Orbigny's Nummulina in a young state.

The distinctive character of the Polythalamia, when compared with their nearest relatives the *Flustra*, *Eschara*, *Cristatella*, &c., consists in the shell, and in their freedom of motion. But with this may be combined the power of attaching itself to other bodies, just as in the *Cristatella* (or *Hydra* also) which often remains long attached, and then creeps again. Those bodies which are apparently Polythalamian, but are really adherent and immoveable, belong to the *Cellepora*, *Flustra*, *Tubulipora*, and similar forms. The simplest Polythalamian form is the *Miliola* in Dujardin's sense, if there be really such self-existent animals, and they be not the young of others, or of many-celled forms most nearly related to Biloculina. And perhaps *Gromia oviformis* might be so viewed, should it not prove to be a *Difflugia* (an Infusoria). In this series I myself place provisionally, as doubtful, those numerous small globules of the sand of Rimini which have no distinct opening, or sometimes present a very minute one. The next simplest form is that of a simple straight row of cells, as in the *Nodosaria*, a jointed continued development of a simple body. *Textularina*, *Uvellina* and *Rotalina* (Lenticulina), may, as to external form, be viewed as Nodosarina developed in another manner, namely, in botryoidal or spiral forms.

I have here to make a remark that appears important. In the entire vast mass of known Polythalamia, a case or vestment prevails which is either cuticular or composed of a calcareous substance, while in Infusoria either a cuticular or siliceous substance prevails, so that hitherto no calcareous-shelled Infusoria nor siliceous-shelled Polythalamia had presented themselves. Yet among the fossil microscopic organisms of the chalk marl of Sicily, we find intermingled with the Infusoria shells bodies whose forms may be ranked with Polythalamia, namely, with Nodosarina, but the shells of which are siliceous, insoluble in acids, and which to the eye have a more transparent vitreous aspect than the calcareous shells when penetrated by balsam. I have hence been induced to place these siliceous-shelled forms, until a further knowledge may be acquired of their organization, among the polygastric Infusoria near the shelled Amoeba, in a separate family, under the name of *Arcellina composita*, or *Polycystina*[*]. Such siliceous-shelled *Polycystina*, resembling calcareous-shelled Polythalamia, are the genera *Lithocampe*, *Cornutella* and *Haliomma*, with several species.

I wish here to draw attention to a small character hitherto unregarded, which is distinctive of true Polythalamia, and often even of their fragments. It consists in this, that in the tube or channel of connexion between the cells, the mouth of the tube which belongs to the earlier smaller cell is overgrown and surrounded by the succeeding larger cell. If the mouth of the last cell be prolonged in a beak-like form, we find in all the earlier smaller cells a distinct tube, quite similar to the hard remains of the sipho in the Nautilus; but so placed that the tube always projects forward from the smaller into the larger cell, and never backward from the larger into the smaller

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[*] This view has been already indicated in the work "On the Infusoria as perfect organisms," 1838, p. 136.

cell. In the Nautilus, this projection of the tube of connexion is reversed, always proceeding from the larger to the smaller chamber, so that in the last, the greatest chamber, the body of the animal thus acquires a smooth foundation, upon which it can move more freely. In true Nautili also the base of the cells is concave or undulated in the forward direction, while in the Polythalamia it appears without exception to be either quite straight or convex in that direction. This character was also observed by Fichtel and Moll.

The tabular view which I have given of the Bryozoa, founded as it is on the new observations which I have made, is drawn up with special regard to a definite expression of fossil phænomena, the ancient names of d'Orbigny being mostly retained. This very diligent precursor in these studies first laid down a foundation rich in forms and systematically ordered, which may serve for all future investigations, and has given names to families which are well adapted to his purpose; but these I have been obliged to alter, yet not arbitrarily, inasmuch as from the difference of our views it became necessary to separate from each other the forms which constitute his families, according as they are either simple Polythalamia, or Polythalamia composing polyparies.

Since the foregoing pages were drawn out, a newer work by Dr. Ehrenberg has made its appearance, embracing communications made to the Berlin Academy, on the continued researches of the author between September 1839 and August 1840, and bearing the title, "On the numerous Living Species of Animals found in the Chalk Formation*." Of this very interesting publication I had designed presenting an abstract, but having learned that a complete English edition of the work is about to appear† accompanied by the engravings, I now confine myself to a few notices immediately connected with the preceding part of this paper.

In this memoir Dr. Ehrenberg repeats his objections to the views entertained by MM. Alcide d'Orbigny and Dujardin. It has been seen, that to the Polythalamia, whose minute and often microscopic calcareous shells compose in inconceivable numbers, and in now nearly 1000 known different forms, the principal mass of chalk rocks and of many sands of the sea, M. d'Orbigny had several years since ascribed an external animal bearing the form of a Sepia, the small shell itself, which

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* *Ueber noch zahlreich jetzt-lebende Thierarten der Kreidebildung*, pp. 94, with four plates, Berlin, 1840.

† In the Scientific Memoirs of Mr. R. Taylor. Its publication cannot fail to prove very acceptable to British Naturalists in general.

often resembles an Ammonite or Nautilus, being considered as the internal bone. On the other hand, at a later period, M. Dujardin denied that these animals possessed any organic structure, stating that they consisted simply of an animated slime capable of extension, encased by an indurated external shell, and associating them with the pseudopodian Amoeba of the Infusoria. Dr. Ehrenberg now further demonstrates, by figures and descriptions, their true organic structure, thus fully establishing his former positions, both as to simple Polythalamia and Polythalamia forming Polyparies. He proves that they are not internal bones, but external shells encasing a soft body, the shell being perforated, as it were, in all parts by numerous pores, from which the animal projects long filaments, capable at will of extension, retraction and bifid division, and productive of locomotion. The author further observes: M. Dujardin has, in August 1840, presented to the Paris Academy a *Mémoire sur une Classification des Infusoires en rapport avec leur organisation*, in which a new arrangement of the Infusoria is exhibited, and in this the Polythalamia are again introduced as *Rhizopodes* in association with *Amoeba* and *Actinophrys* of the Infusoria, forming a separate family. If, however, anatomical and physiological details are to be taken into account when we proceed to the systematic arrangement of different organic bodies, and we are not governed merely by the relations of external forms, M. Dujardin's arrangement cannot be deemed a happy one. He has in no case shown a polygastric structure in the Rhizopodes, and that it is not polygastric is proved anew by my investigations now communicated.

It has been shown in a former part of this paper that Dr. Ehrenberg had recognized six species of Infusoria in the chalk formation, so closely resembling living species as not to be distinguished from them, and hence he was led to give to them the same names; namely, *Eunotia Zebra*, *Fragilaria rhabdosoma*, *Fragilaria striolata ?*, *Gallionella aurichalca*, *Navicula ventricosa*, and *Synedra ulna*. He had also referred, with a mark of interrogation, the following four species of calcareous-shelled Polythalamia to the white chalk, in which they are very extensively distributed, namely, *Globigerina bulloides*, *Globigerina helicina*, *Rosalina globularis*, and *Textilaria aciculata*, all of which were stated by M. d'Orbigny to have occurred in the living state only in the Adriatic Sea and the Ocean. If any doubt had existed as to the identity of all these fossil and living species, it has been completely removed by the later researches of Dr. Ehrenberg, by which the actual number of known species found in the chalk formation and in the living state has been extended to fifty-seven, namely, of calcareous-shelled Polythalamia nine species, and of siliceous-shelled Infusoria forty-eight species. The following is a list of these species and of the localities in which they occur, both in the living and fossil state. In the fossil localities, W. C. signifies white chalk, C. M. chalk marl, and C. C. compact chalk.

Calcareous-shelled Polythalamia.

	Living.	Fossil.
1. Globigerina bulloides	In the Adriatic Sea and the Ocean	W. C. Denmark.
2. —— helicina	————	W. C. Cattolica.
3. Rosalina globularis .	————	W. C. Gravesend.
4. Planulina (Synon. Rotalia) ocellata. .	North Sea, near Cuxhaven	W. C. Cattolica.
5. Rotalia globulosa . .	————	W. C. in Russia, Poland, Prussia, Denmark, England, France and Sicily; and C. M. in Greece, Zante, Sicily and Oran.
6. —— stigma	————	W. C. Cattolica. C. M. Caltasinetta.
7. —— (Synon. Planulina?) turgida . . .	————.	W. C. England, France, Prussia, Denmark. C. M. Oran. C. C. Egypt and Arabia.
8. Textilaria aciculata .	————, and Adriatic and the Ocean	W. C. Prussia, Denmark, England and Sicily. C. M. Greece. C. C. Egypt and Arabia.
9. —— globulosa . . .	North Sea.	W. C. of all European countries, from Wolsk to Ireland. C. M. Sicily, Oran, and Greece. C. C. Egypt and Arabia.

Siliceous-shelled Infusoria.

10. Actinocyclus quinarius	North Sea, Tjörn Isle in the Cattegat	C. M. Caltasinetta.
11. —— biternarius . . .	North Sea, Tjörn	C. M. Oran and Caltasinetta.
12. —— senarius	North Sea, Cuxhaven, Christiania, Tjörn . . .	C. M. Oran, Caltasinetta, and Greece.
13. —— septenarius . .	North Sea in the Cattegat	C. M. Oran, Caltasinetta, and Zante.
14. —— octonarius . . .	————	C. M. Oran and Caltasinetta.
15. —— nonarius	N. Sea, Cattegat near Tjörn.	C. M. Oran.
16. —— denarius	————	C. M. Oran.
17. —— undenarius . . .	————, and Bay of Christiania.	C. M. Oran and Zante.
18. —— bisenarius	Cattegat near Tjörn . . .	C. M. Oran.
19. —— quindenarius . .	———— . . .	C. M. Oran.
20. Amphitetras antediluviana	———— . . .	C. M. Oran and Greece

	Living.	Fossil.
21. Biddulphia pulchella.	Baltic, N. Sea, Mediterranean, and Ocean near Cuba	C. M. Greece.
22. Cocconema lanceolatum	Brackish and fresh waters.	C. M. Greece.
23. Coscinodiscus Argus.	North Sea, Cuxhaven	C. M. Caltasinetta and Oran.
24. —— eccentricus	————————, Tjörn in Cattegat, and Mexican Gulf, Vera Cruz.	C. M. Oran.
25. —— lineatus	North Sea, Cuxhaven	C. M. Caltasinetta.
26. —— minor	————————	C.M. Caltasinetta, Oran, and Zante.
27. —— Oculus Iridis	————————	C. M. Greece.
28. —— Patina	————————	C. M. Zante.
29. —— radiatus	————————, and Baltic, Wismar.	C. M. Oran, Caltasinetta, and Zante.
30. Dictyocha sulcata	North Sea near Tjörn	C. M. Caltasinetta, Oran, Zante, and Greece.
31. —— Fibula	N. Sea, Christiania and Tjörn, & Baltic, Wismar	C. M. Oran and Caltasinetta.
32. —— Pentasterias	N. Sea, Christiania haven.	C. M. Zante.
33. —— Speculum	N. Sea, Cuxhaven, Christiania and Tjörn, Baltic, near Kiel	C. M. Caltasinetta, Oran, Zante, and Greece.
34. Eunotia granulata	Brackish and fresh waters.	C. M. Greece.
35. —— Zebra	Berlin fresh waters	C. M. Greece.
36. Fragilaria rhabdosoma	Berlin, Halle, Copenhagen, Sweden	W. C. Gravesend.
37. —— striolata	. . . .	W. C. Gravesend.
38. Gallionella aurichalca	Berlin fresh waters, Leipzig, Thuringia, Franconia, Würzburg, Stuttgart, and on rocks near the Faroe Isles	W. C. Rügen.
39. —— sulcata	North Sea, Cuxhaven	C. M. Caltasinetta, Oran, Zante, and Greece.
40. Grammatophora africana	N. Sea, Heligoland, Tjörn	C. M. Oran.
41. —— angulosa	North Sea, Tjörn	C. M. Oran.
42. —— oceanica	Callao in Peru, Vera Cruz in Mexico, Tjörn in Cattegat, Wismar in Baltic, and the Mediterranean.	C. M. Oran.
43. —— undulata	Among marine Confervæ near Vera Cruz.	C. M. Greece.
44. Haliomma radians	North Sea, Cuxhaven	C. M. Greece.
45. Navicula Didymus.	N. Sea, Cuxhaven, Baltic, Wismar	C. M. Caltasinetta.
46. —— Entomon	N. Sea, Christiania haven.	C. M. Greece.
47. —— norwegica	————————	C. M. Greece.
48. —— quadrifasciata	————————, and Tjörn Isle.	C. M. Greece.
49. —— ventricosa	Paris, Berlin, Saxony, Bohemia, Buchtarma in Altai, and Irtysch.	C. M. Oran.
50. —— viridula	Berlin fresh waters, Weissenfels in Saxony, and Wismar in Mecklenburg.	C. M. Greece.

	Living.	Fossil.
51. Peridinium pyrophorum	Baltic, near Kiel	Flints of the W. C. near Gravesend, and Flints of the plain of North Germany near Delitzsch.
52. Striatella arcuata . . .	Gulf of Flensburg, Breakers near Gothenburg.	C. M. Oran.
53. Synedra ulna	Baltic near Wismar, Berlin fresh waters, North of Germany, Denmark, Scotland, Holland, the Ural, and perhaps Isle of France, and Mascarene Isles	C. M. Oran.
54. Tessella Catena . . .	Breakers near Gothenburg and Berlin waters.	C. M. Caltasinetta.
55. Triceratium Favus . .	North Sea, Cuxhaven . .	C. M. Greece.
56. Xanthidium furcatum	Berlin	Flints of W. C. Gravesend, and Flints of Delitzsch.
57. —— hirsutum	Peat waters near Berlin.	Flints of W. C. Gravesend, and Flints of Delitzsch.

Of these fifty-seven species, thirty belong to the geologically acknowledged chalk and its Sicilian marls. The remainder from Oran, Greece (probably Egina), and Zante, though perhaps from beds not equally well defined by relative position as chalk marls, yet occurring, as they do, with numerous decided calcareous and siliceous animals of the chalk,—the geological relations of these species may also be considered as firmly established.

These new discoveries naturally lead to the conclusion that we have now no very definite boundary between secondary and tertiary tracts, and that the first dawn or eocene period of the present living organic creation, must be sought for deeper than the chalk formation; a view that appears to be confirmed by the occurrence of a living *Trochus* below the chalk, of the *Paludina vivipara* and *Cyclas cornea* in the Weald Clay, and of the *Terebratula caput serpentis* in the Upper Oolite. But as this and other interesting conclusions and views entertained by the author will be shortly laid open to the reader, with a full detail of the progressive researches made, I shall not now enter further upon the important matter contained in the volume.

APPENDIX.

Closely connected with the preceding subjects is the valuable Memoir of M. Alcide d'Orbigny, which has recently appeared, entitled, "On the Foraminifers of the White Chalk of the Paris Basin*." The subjoined extracts may serve to convey a view of the general scope of the work, which, placed in parallel with that of Dr. Ehrenberg, cannot but excite a double interest in the mind of the reader.

Previously to entering upon the direct object of the Memoir, M. d'Orbigny indulges in a few general reflections.

Let us, says the author, cast a rapid glance upon what has existed and upon what still exists in nature, in reference to the Foraminifers. We have found them distributed through the oolite series, extending from the lias to the uppermost beds; but in the cretaceous system they appear still more numerously and more varied in their forms. The Neocomian beds, those of the gault and the green sand, contain many; but in proportion as we ascend from the lower to the higher strata, they increase infinitely. In these latter the rock may be said to be often composed of them, and, as an example, we may mention the largest of the Pyramids of Egypt. In the white chalk the number is nearly as great as in those seas in which they now most abound. In a word, we have found Foraminifers in the cretaceous basins of the Seine, the Loire, the Gironde, and of the whole South of France, and in Belgium.

If we pass to the tertiary tracts, a whole world is opened to us. The multiplied Foraminifers which appear in the basins of Paris, Bourdeaux, Touraine, Italy, Austria, Germany, England, and Belgium, often form there the greater part of the mass. A bed of considerable thickness in the environs of Gentilly, near Paris, is entirely composed of them, the Foraminifers being in contact with each other, scarcely united by a slight cement. In a cubical inch of the rock we have found *fifty-eight thousand*, which is equal to *three thousand millions* in a metre, and shows what myriads may exist in the Paris basin. These small bodies, which we thus see forming entire beds in the lowest portions of the tertiary series, are not less common in the higher stages; for in Austria, and

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* *Mémoire sur les Foraminifères de la Craie Blanche du Basin de Paris*, in the 4th vol. part 1 of the Transactions of the Geological Society of France, 1840.

in the environs of Sienna in Italy, they often constitute one-sixth of the fossil mass; they are also extensively distributed in the Crag of England* and of Belgium. So much in reference to what has existed; let us now throw a glance upon that which exists.

We are in the present day acquainted with Foraminifers from every region of the sea, and we know that they exist in extent from the equator to the frozen portions of continents. If we judge of the important part they play by their numbers in certain quarters, it will be impossible to doubt that their remains form the greater part of the banks of sand which impede navigation, obstruct gulfs and straits, fill up ports, and form with corals those isles which are daily rising in warm regions from the bosom of the ocean.

Thus these minute shells, which, anterior to our epoch, have assisted in leveling basins of immense extent, and in forming mountains, are now still constantly changing the depth of coasts and modifying the bottom. This view of their agency in nature is doubtless sufficient to prove the importance which attaches to their study.

We will add, that the comparative study of the fossil Foraminifers of all beds has proved to us a fact important to geology, namely, that each bed has its characteristic species, which serve to distinguish it, let the circumstances be what they may; and as these minute shells are infinitely more common than those of Mollusks, the knowledge to be derived from them is so much the more certain, and becomes extremely interesting.

Another fact no less curious has been demonstrated to us by the study of living species from every region of the globe†. Many genera are peculiar to the hottest zones of continents, while others, on the contrary, are found only in temperate or cold regions. Hence the geographical distribution of living genera and species offers to us a means of comparison of the highest importance with a view to the determination of the temperature of the waters in which fossil species lived,

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* Mr. Lyell has communicated to us the species which he discovered in the Crag.

† We are acquainted at present with nearly *fifteen hundred* living and fossil species of Foraminifers; and how many important facts may be derived from the study of these small bodies may be seen in three works which we are now publishing: 1. the Fauna of the Antilles, printed in *l'Histoire politique, physique, et naturelle de l'Ile de Cuba*, by M. de la Sagra; 2. that of the Canaries, published in *l'Histoire Naturelle* of those islands, by MM. Webb and Berthelot; 3. the Fauna of the southern extremity of America, forming a part of our *Voyage dans l'Amérique Méridionale.*

and may lead to very satisfactory results in geology, if we may judge by the fruits of our observations in this respect.

We could have desired to establish some general facts of much greater extent, founded on new observations recently made by us on the class of the Foraminifers; but the present occasion not admitting such an extension, let us pass to the Foraminifers of the white chalk of the Paris basin.

The geological position of the white chalk of Paris is so well known that we have not thought it necessary to speak of it; yet, if we seek to determine its position relatively to the other cretaceous beds by means of the Foraminifers it contains, compared with living species, the *facies* of the genera and species proves to us, that the chalk of Maestricht, of Fauquemont (Belgium), of Tours, of Chavagne, and of Vendome, is above it; while, on the contrary, all the other beds are below it; thus in the chalk of Maestricht and the upper beds of the basins of the Loire, we recognize only genera still existing, or at least occurring in tertiary tracts, while the white chalk of the Paris basin already exhibits to us different genera, such as *Flabellina*, *Verneuilina*, and *Gaudryina*, and a great number of species quite distinct.

It would therefore be easy to establish, by means of the Foraminifers alone, the relative antiquity of the cretaceous beds; but we must previously make two geographical sections quite independent of each other, founded on the zoological forms; the first comprising the entire basin of the Seine, of the Loire, of Belgium, and of England, in which we find a striking analogy between the species found in all the beds, from the lowest to the highest, with a regular passage from one to the other; the second, comprising the West and South of France, in which the species of Foraminifers have not only no analogy with those of the other section, but in which, moreover, almost all the genera are different. If we seek an example of this fact, we shall find it on comparing the green sand of the environs of Mans with that of the mouth of the Charente. The first, which in fact contains species approximating to those of the white chalk of Paris, contains already several species analogous to those which have lived up to that bed; while the second, with perfectly distinct species, presents to us genera different from all that we know in the cretaceous beds of the North of France and of Belgium.

The Foraminifers are sufficient to establish the following descending order of superposition in the cretaceous beds :—

Group of the North of France and of Belgium.	*Group of the West and South of France.*
Upper chalk of Maestricht and Fauquemont (Belgium).	
Coral chalk of Valognes and Nehou.	
Coral chalk of the basin of the Loire, at Vendome (Loir and Cher), at Chavagne (Maine and Loire), at Tours (Indre and Loire).	
White chalk of Ciply (Belgium).	
White chalk of Paris, of the departments of Yonne and Aube, and of England.	
	Nummulite chalk of Royan (Charente Inférieure), of Saint Martory (Haute Garonne), of Saint Gaudens, &c.
	Coral chalk of Saintes (Charente Inférieure).
Chalk marl of the Loire, with *Gryphæa columba.*	Ammonite chalk of Martrons, near Rochefort (with *Gryphæa columba*).
	Caprine chalk of the Isle of Aix, of the Corbières (Aude).
Green sand of Mans (Sarthe).	Green sand of Fouras, of the Isle of Aix, and Corbières.
Gault of the environs of Troyes (Aube).	
Neocomian tract of Aube.	

To establish zoologically what we have advanced, let us pass in review the succession of the genera, and endeavour to convey an idea of the modifications which have taken place in the Foraminifers of the cretaceous system, in the ascending order of the beds.

At the epoch of the Neocomian formation we have hitherto found only the genus *Textularia.*

The green sand presents, as we have said, two series of genera nearly distinct. That of the mouth of the Charente contains the genera *Dentalina, Cristellaria, Lituola, Alveolina, Chrysalidina,* and *Cuneolina*; that of Mans, the genera *Dentalina, Citharina, Frondicularia, Flabellina, Cristellaria, Bulimina,* and *Guttulina.* Hence we see, that, with the exception of two genera common to both localities, all the rest are different in each of them.

If we follow our examination of the succession of genera in the cretaceous groups of the South and the North, we shall find—

1. That in the South the same genera of the green sand are reproduced in the Caprine chalk. By degrees they prevail at length in the upper beds, and are reduced to the *Cristellaria* alone in the environs of Saintes; but near the mouth of the Gironde (at Royan) they are accompanied by the genera *Nummulina* and *Guttulina,* as well as on the whole line of the foot of the Pyrenees, at Saint Martory, at Saint Gaudens, extending into the department of Aude; thus pre-

senting a zone well characterized by the abundance of Nummulina, of which we have not found the analogue in the cretaceous beds of the North of France.

2. That in the North the succession is far from taking place in the same manner; and that the Foraminifers, in much greater numbers, present a larger suite in superposition, with facts not less curious. The genus *Citharina*, which constitutes the greatest portion of the species in the oolite formation, ceases with the green sand of Mans, being found no further in the cretaceous beds. In the chalk marl of the banks of the Loire we meet for the first time with the genus *Lituola* with the *Dentalina*; but all at once, in the white chalk, we observe a great number of species, among which, with all the genera and even some analogous species of the green sand of Mans, there appear for the first time on the globe the genera *Nodosaria*, *Marginulina*, *Valvulina*, *Rotalina*, *Rosalina*, *Truncatulina*, *Uvigerina*, *Verneuilina*, *Gaudryina*, *Globigerina*, *Pyrulina*, *Sagrina*, *Flabellina*, and *Frondicularia*. These genera contain a considerable number of species; but with the white chalk the genus *Flabellina* ceases, which had continued hitherto from the green sand, and the genera *Verneuilina* and *Gaudryina*, which first appear in the white chalk, also terminate with it; while in its interior the Frondicularia abound, as well as species whose cells form a pile on a single line.

The white chalk of Ciply, although contemporaneous with that of the Paris basin, since it also contains Flabellina, does not present the same species, and may perhaps be a little higher in the series, but we have not as yet sufficient data to enable us to affirm this fact.

In the beds which we consider higher in the series than the white chalk of Paris, namely, in the coral chalk of Tours, of Chavagne, and of Vendome, we meet for the first time with the genera *Polystomella*, *Polymorphina* and *Globulina*, yet accompanied with the same genera as those of the white chalk, with the exception of those whose discontinuance we have noticed; again, in the upper chalk of Maestricht and Fauquemont we have, with the three genera just mentioned, also the genera *Nonionina*, *Faujasina*, and *Heterostegina*. All are found living at present, or at least occurring in tertiary tracts; but we arrive at the last beds of the cretaceous group without having seen a single species of the *Miliola* of Lamarck (our order of Agathistègues), which, as we ascertained in 1825, only commences with the tertiary beds, and may be considered as the most certain sign of a change of formation.

This rapid survey shows that in ascending from the lower to the higher beds of the cretaceous group, the genera and species of Foraminifers progressively increase, and that the forms, at first very simple, analogous to those of oolitic tracts, afterwards more complicated and specially appropriate to the lower beds of the cretaceous system, have at last been replaced in the upper parts by forms still more varied, the whole recurring in tertiary tracts, and even in the living state; facts which it has appeared to us important to establish in the history of Palæontology.

M. A. d'Orbigny then proceeds to describe the species of Foraminifers found by him in the white chalk of the Paris basin. The following is a list of them, together with their localities:—

	Localities.
1. Nodosaria limbata	Meudon: very rare.
2. Dentalina aculeata	Common at Sens: more rare at Meudon and in England.
3. ——— communis	Meudon: rare. Its analogue is found fossil in the Subapennine tracts of Italy and Austria, and living in the Adriatic.
4. ——— gracilis	At Sens and in England.
5. ——— nodosa	Common at Sens, more rare at Meudon and St. Germain.
6. ——— Lorneiana	Only in the environs of Sens.
7. ——— sulcata	Very common at Sens, Meudon, and St. Germain, and in the chalk of England. Found also in the green sand of the environs of Mans (Sarthe).
8. ——— multicostata	Sens, St. Germain: rare. Also at Maestricht rarely.
9. Marginulina trilobata	Common at Sens, very rare at Meudon, St. Germain, and in England: found only in the young state.
10. ——— compressa	Meudon: very rare. Occurs also in the green sand in the environs of Mans.
11. ——— elongata	Common near Sens, very rare at Meudon and St. Germain. Occurs also in the chalk of Ciply.
12. ——— gradata	Only near Sens.
13. ——— raricosta	Meudon: very rare.
14. Frondicularia radiata	Meudon and St. Germain: very rare.
15. ——— elegans	Meudon and Sens: very rare.
16. ——— Verneuiliana	Common at Sens, on the banks of the Yonne; rare at St. Germain and Meudon.
17. ——— Archiaciana	Meudon and Sens: rare.
18. ——— ornata	Found only once at Meudon.
19. ——— tricarinata	Environs of Sens: seems to be rare.
20. ——— angulosa	Meudon: very rare.
21. Flabellina rugosa	Sens and Meudon: common.
22. ——— Baudouiniana	Only at Sens.
23. ——— pulchra	Meudon: very rare.
24. Cristellaria rotulata	Very common in the white chalk of Meudon, St. Germain, Sens, and in England. Occurs also in the green sand near Mans.
25. ——— navicula	Sens and Meudon: rare.
26. ——— triangularis	Sens: very rare.
27. ——— recta	Meudon and St. Germain: rather rare.

	Localities.
28. Cristellaria Gaudryana	Only at St Germain: rare.
29. Lituola nautiloidea	Very common at Sens in the complete state, at St. Germain only young, and adult very rarely at Meudon. Occurs also in the chalk of England.
30. Rotalina Voltziana	Very common at Meudon, St. Germain, and in England.
31. ——— Micheliniana	Common at St. Germain, Meudon, and in England; rare at Sens.
32. ——— umbilicata	Common at Meudon and St. Germain; rare at Sens and in England; common also in the tertiary tracts of Austria. Its analogue is found living at Rimini in the Adriatic, there being no difference between the fossil and living species.
33. ——— crassa	St. Germain, Meudon, and England: rather rare.
34. ——— Cordieriana	St. Germain, England, and upper chalk of Maestricht.
35. Globigerina cretacea	St. Germain and England.
36. ——— elevata	Common near Sens; rare in England.
37. Truncatulina Beaumontiana	Meudon and England: rare.
38. Rosalina Lorneiana	Common at St. Germain and Meudon; rare at Sens and in England.
39. ——— Clementiana	Rare at St. Germain, more common in England.
40. Valvulina gibbosa	St. Germain: rare.
41. Verneuilina tricarinata	St. Germain and Sens: rather rare.
42. Bulimina obtusa	Very common at Meudon; rare at St. Germain and in England.
43. ——— obliqua	Very common at Meudon, St. Germain, Sens, and in England.
44. ——— variabilis	Very common at Sens; rare at Meudon, St. Germain, and in England.
45. ——— brevis	Very common at Meudon, St. Germain, and Sens.
46. ——— Murchisoniana	St. Germain and England: rare.
47. Uvigerina tricarinata	Sens: very rare.
48. Pyrulina acuminata	Very rare at Sens and St. Germain; very common at Meudon.
49. Gaudryina rugosa	Meudon, St. Germain, and Sens: rather common.
50. ——— pupoides	Rather common at Meudon, Sens, St. Germain, and in England.
51. Textularia trochus	Only at Meudon.
52. ——— turris	Sens, Meudon, St. Germain, and England, without being common.
53. ——— Baudouiniana	St. Germain and Meudon: rare.
54. Sagrina rugosa	St. Germain and Meudon.

From the preceding list it appears, that of the fifty-four species found in the white chalk of the Paris basin, *thirty-eight* occur at Meudon, *thirty-three* at Saint Germain, and *twenty-eight* at Sens: of these numbers, *nine* are peculiar to Meudon, *two* to Saint Germain, and *six* to Sens, while all the others are simultaneously common to two or three localities, thus proving the perfect identity of the beds. It will be seen also, that of these fifty-four species, *twenty-two* are common to the white chalk of England also.

Of the fifty-four species, *seven* occur also in lower or higher beds: thus in the green sand of Mans are found three species, *Dentalina sulcata*, *Marginulina compressa*, and *Cristellaria rotulata*; in the coral chalk of Tours, which is higher in position than the white chalk, two species, *Bulimina obtusa* and *Textularia turris*; and in the chalk of Maestricht, being the highest in position, two species, *Dentalina multicostata* and *Rotalina Cordieriana*. We also find two species, the analogues of which occur both fossil in the tertiary tracts of Austria and Italy, and in the living state in the Adriatic, namely, *Dentalina communis* and *Rotalina umbilicata*. With these exceptions there still remain *forty-seven* species peculiar to the white chalk, showing clearly that it forms a bed distinct from all the rest of the cretaceous system, belonging to a small local fauna well-defined.

On comparing the above genera given by M. d'Orbigny with those named by Dr. Ehrenberg in his tabular view of the Bryozoa, inserted in the early part of this paper, it will be seen that Nodosaria, Dentalina, Marginulina, Frondicularia are included in the family of the Nodosarina of the latter author; Cristellaria, Rotalina, Truncatulina, included in his family of the Rotalina; Globigerina, Rosalina, Valvulina, Bulimina, Uvigerina, Pyrulina, in his family of the Uvellina; and Textularia in his family of the Textularina. The *Lituola nautiloidea* of Lamarck and d'Orbigny is the *Coscinospira nautiloides* of Ehrenberg, included in the Fabularina family of the latter.

If we now, observes M. d'Orbigny, compare the fauna of the Foraminifers of the white chalk with those of different seas, with a view of determining the analogy of composition, and of obtaining data respecting the temperature of that basin at the time when these species lived, we shall find this analogy more striking in the Adriatic Sea than anywhere else. There only, the same as in the chalk, are found in abundance Nodosaria, Dentalina, Marginulina, Frondicularia; there only occur a considerable number of species of Bulimina. This sea alone in the present day contains living Frondicularia; of Frondicularia so varied in the white chalk; and, to complete the approximation, it exhibits to us the only two living species, the analogues of which are found in the fossil state in the white chalk, namely, *Dentalina communis* and *Rotalina umbilicata*. This analogy of zoological forms would lead us to believe, 1st, that the basin in which is deposited the white chalk of Paris was subject to a warm temperature; 2nd, that it was circumscribed, protected from waves and from every violent current proceeding from a distance, since the bodies are deposited there without having experienced the slightest wearing previous to their becoming fossil; 3rd and lastly, that it extended to the whole of the white chalk of England.

Concluding Remarks.

The preceding extracts from the labours of Dr. Ehrenberg and M. A. d'Orbigny show that microscopic Polythalamia are found in all calcareous formations from the lias upward; but in England they have been lately discovered in still deeper strata. Mr. Tennant was, I understand, the first to announce their discovery in 1839 in the mountain limestone of England. In 1840 they were also met with in the Kendal limestone, from which Mr. Lonsdale has prepared thin slices mounted on glass, which appear transparent under a strong light, exhibiting the crowded state of the microscopic Polythalamia in great perfection. Mr. Bowerbank also has been led to turn his attention to this subject by examining the siliceous bodies of the chalk, green sand, and oolites*.

I had written thus far, when an interesting article by the Rev. Dr. Buckland, in reference to the researches of Dr. Ehrenberg up to 1839, met my eye, entitled, "On the agency of Animalcules in the formation of Limestone†," which notices in particular the researches of MM. Tennant and Darker on this subject in the Derbyshire limestone and the Stonesfield slate, as well as the labours of Mr. Bowerbank, referred to above, and conveying judicious reflections. Dr. Buckland justly remarks, that in the application of the microscope from the living to the fossil Infusoria and Foraminifers we are commencing a new and important era in Palæontology. A very interesting branch of the inquiry will be to ascertain whether these microscopic bodies retain throughout a distinctive character in the several formations into whose composition they enter. In the unbounded field of nature presented to the consideration of the Microscopical Society of London lately established, no subject appears more worthy of their attention than an examination of the microscopic organic constituents of all the older limestone formations of the British Isles, as well as of other countries; and it is much to be desired that this attention may not be wanting, although the concurrence of many labourers may be required to reap a harvest of great promise, yet of indefinite extent.

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* Proceedings of the Geological Society, March 11, 1840.

† Edinburgh New Philosophical Journal, January to April, 1841.

LXXIV. *Second Letter to* Prof. Faraday, *from* Robert Hare, *M.D., Professor of Chemistry in the University of Pennsylvania*[*].

My dear Sir,

39[†]. IN the month of July last I had the pleasure to read, in the American Journal of Science, your letter in reply to one which I had addressed to you through the same channel. I should sooner have noticed this letter, but that meanwhile I have had to republish two of my text books, and, besides, could not command, until lately, a complete copy of all those numbers of your researches to which you have referred.

40. The tenor of the language with which your letter commences, realizes the hope which I cherished, that my strictures would call forth an amicable reply. Under these circumstances, it would grieve me that you should consider any part of my language as charging you with inconsistency or self-contradiction, as if it could be my object to put you in the wrong, further than might be necessary to establish my conception of the truth. Certainly it has been my wish never to go beyond the sentiment "amicus Plato, sed magis amica veritas." I attach high importance to the facts established by your "Researches," which can only be appreciated sufficiently by those who have experienced the labour, corporeal and mental, which experimental investigations require. I am, moreover, grateful for the disposition to do me justice, manifested in those researches; yet it may not always be possible for me to display the deference, which I nevertheless entertain. I am aware that when, in a discussion, which due attention to brevity must render unceremonious, diversities of opinion are exhibited, much magnanimity is requisite in the party whose opinions are assailed; but I trust that both of us have truth in view above all other objects, and that so much of your new doctrine as tends to promote that end, will not be invalidated by a criticism, which, though free, is intended to be perfectly fair.

41. In paragraph 11 your language is as follows:—"*My theory of induction makes no assertion as to the nature of elec-*

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[*] Communicated by the Author.

[†] As originally printed for the American Journal of Science, the paragraphs of my first letter to Prof. Faraday were not numbered; but as numbers were attached to the paragraphs in the republication of it in the London and Edinburgh Philosophical Magazine and Journal [vol. xvii. p. 44], I have directed them to be attached to this, my second letter, in due succession.

tricity, nor at all questions any of the theories respecting that subject."

42. Owing to this avowed omission to state your opinions as to the nature of electricity, as preliminary to the statement of your "*theory,*" and because I was unable to reconcile that theory with those previously accredited, I received the impression that you claimed no aid from any imponderable principle. It appeared to me that there was no room for the agency of any such principle, if induction were an *action* of *contiguous ponderable particles consisting of a species of polarity.* It seemed to follow that what we call electricity, could be nothing more than a polarity in the ponderable particles, directly caused by those mechanical, or chemical frictions, movements or reactions, by which ponderable bodies are electrified. You have correctly inferred that I had not seen the fourteenth series of your researches, containing certain paragraphs (38). From them it appears that the polarity, on which so much stress has been laid, is analogous to that which has long been known to arise in a ponderable body, about which the electric equilibrium has been subverted by the inductive influence of the electricity accumulated upon another such body. This is clearly explained in paragraph 4 of your letter, by the illustration, agreeably to which three bodies, A, B, C, are situated in a line, in the order in which they are named, in proximity, but not in contact. "A is electrified positively, and then C is uninsulated." It is evident that you are correct in representing that, under these circumstances, the extremities of B will be oppositely excited, so as to have a reaction with any similarly excited body, analogous to that which takes place between magnets; since the similarly excited extremities of two such bodies would repel each other, while those dissimilarly excited would be reciprocally attractive. Hence, no doubt, the word polarity is conceived by you to convey an idea of the state of the body B. If I may be allowed to propose an epithet to convey the idea which I have of the state of a body thus electrified, I would designate it as an electro-polar state, or as a state of electro-polarity.

43. It does not appear to me, that in the suggestion of the electro-polarity, which we both conceive to be induced upon the body B (4), so long as it concerns a mass of ponderable matter, there is any novelty. The only part of your doctrine which is new, is that which suggests an analogous state to be caused in the particles of the bodies through which the inductive power is propagated. Admitting each of the particles of a dielectric, through which the process of ordinary induction takes place, to be put into the state of the body B, it does not

appear to me to justify your definition of electrical induction. I think that, consistently with your own exemplification of that process, you should have alleged ordinary induction to be *productive* of an *affection* of particles, *causing* in them a species of polarity. In the case of the bodies A, B, C (4), B is evidently passive. How then can we consider as active, particles represented to be in an analogous state? If in B there is no action, how can there be any action in particles performing a perfectly similar part? Moreover, how can the inductive power of an electrical accumulation upon A *consist* of the polarity which it induces in B?

44. Having supposed (8) an electrified ball, A, an inch in diameter, to be situated within a thin metallic sphere, C, of a foot in diameter, you suggest, that where one thousand concentric metallic spheres interpose between A and the inner surface of C, the electro-polar state of each particle in those spheres would be analogous to that of B, already mentioned. Of course, if there be any action of those particles, there must be an action of B; but this appears to me not only irreconcilable with any previously existing theory, but also with your own exposition of the process by which B is polarized.

45. Supposing concentric metallic hemispheres to be interposed only upon one side of A, you aver that, agreeably to your experience, more of the inductive influence would be extended towards that side of the containing shell than before (14). Admitting this, I cannot concede that the greater influence of the induction, resulting from the presence of the metallic particles, is the consequence of any *action* of theirs, whether in *contiguity* or in *proximity*. Agreeably to my view, the action is confined to the electrical accumulation in the sphere A. Between the electricity accumulated in this sphere, and that existing in or about the intervening ponderable particles, there may be a *reaction*; but evidently these particles are as inactive as are the steps of a ladder in the scaling of a wall.

46. Suppose a powerful magnet to be so curved as to have the terminating polar surfaces parallel, and to leave between them an interval of some inches. Place between these surfaces a number of short pieces of soft iron wire. These would, of course, be magnetized, and would arrange themselves in rows, the north and south poles becoming contiguous. Would this be a sufficient reason for saying that the inductive influence of the magnetic poles was an *action* of the contiguous wires? Would not the phænomena be the consequence of an *affection* of the contiguous pieces of wire, not of their *action*?

47. As respects the word charge, I am not aware that I have been in the habit of attaching any erroneous meaning to

it, as your efforts to define it in paragraph 3 would imply. I have been accustomed to restrict the use of it to the case which you distinguish as an inductive charge, illustrated by that of the Leyden jar. To designate the states of the conductors of a machine, I have almost always employed the words *excited* or *excitement*. In my text book these words are used to designate the state of glass or resin electrified by friction, while that of coated surfaces, whether panes or jars, inductively electrified, has been designated by the words *charge* or *charged*.

48. I understood the word contiguous to imply contact, or contiguity, whereas it seems that it was intended by you to convey the idea of proximity. In the last-mentioned sense it is not inconsistent with the idea of an action, at the distance of half an inch: but by admitting the word contiguous to be ill chosen, you have with great candour furnished me with an apology for having mistaken your meaning.

49. Any inductive action which does not exist at sensible distances (20) you attribute to *ordinary* induction, considering the case of induction through a vacuum as an *extraordinary* case of induction. To me it appears that the induction must be the same in both cases, and that the *circumstances* under which it acts are those which may be considered in the one case as *ordinary*, in the other *extraordinary*. Thus take the case cited in your reply (8, 9, 10). Does the interposition of the spheres alter the character of the inductive power in the sphere A?

50. Either the force exercised by the charge in A is like that of gravitation, altogether independent of the influence of intervening bodies, or, like that of light, is dependent on the agency of an intervening matter. Agreeably to one doctrine, the matter, by means of which luminous bodies act, operates by its transmission from the luminous surface to that illumined; agreeably to another doctrine, the illuminating matter operates by its undulations. If the inductive *power* of electrified bodies be not analogous to gravitation, it must be analogous to the power by which light is produced, so far as to be dependent on intervening matter. But were it to resemble gravitation, like that force it would be uninfluenced by intervening matter. If your experiments prove that electrical induction is liable to be modified by intervening matter, it is demonstrated that in its mode of operating it is analogous to light, not to gravitation. It is then proved that, agreeably to your doctrine, electrical induction requires the intervention of matter; but you admit that it acts across a vacuum, and, of course, acts without the presence of *ponderable* matter. Yet it requires intervening matter of some kind, and since that matter is not

ponderable, it must of necessity be imponderable. When light is communicated from a luminous body in the centre of an exhausted sphere, agreeably to the undulatory hypothesis, its efficacy is dependent on the waves excited in an intervening imponderable medium. Agreeably to your electro-polar hypothesis, the inductive efficacy of an electrified body in an exhausted sphere would be due to a derangement of electric equilibrium, by which an opposite electric state would be produced at the surface of the containing sphere from that at the centre (26, 27). This case you consider as one of extraordinary induction; but when air is admitted into the hollow sphere, or when concentric spheres are interposed, you hold it to be a case of ordinary induction. Let us then, in the case of the luminous body, imagine that concentric spheres of glass are interposed, of which the surfaces are roughened by grinding. In consequence of the roughness thus produced, the rays, instead of proceeding in radii from the central ball, would be so refracted as to cross each other. Of the two instances of illumination, thus imagined, would the one be described as *ordinary*, the other as *extraordinary radiation?* But if these epithets are not to be applied to radiation, wherefore, under analogous circumstances, are they applicable to induction? Wherefore is induction, when acting through a plenum, to be called ordinary, and yet, when acting through a vacuum, to be called extraordinary? In the well-known case of the refracting power of Iceland spar, light undergoes an *ordinary* and *extraordinary refraction*; not an *ordinary* and *extraordinary radiation.* The candle, of which, when viewed through the spar, two images are seen, does not *radiate ordinarily* and *extraordinarily.*

51. If there be occasionally, as you allege (21), large intervals between the particles of radiant heat, how can the distances between them resemble those existing between particles acting at distances, which are not sensible? The repulsive reaction between the particles of radiant caloric, as described by you (21), resembles that which I have supposed to exist between those of electricity; but I cannot conceive of any description less suitable for either, than that of particles which do not act at sensible distances.

52. Aware that the materiality of heat, and the Newtonian theory, which ascribes radiation to the projection of heat or light-producing particles, have been questioned, I should not have appealed to a doctrine which assumes both the materiality of heat and the truth of the Newtonian theory, had not you led the way; but, agreeably to the doctrine and theory alluded to, I cannot accord with you in perceiving any

similitude between the processes of conduction and radiation (21).

53. Consistently with the hypothesis that electricity is material, you have shown that an enormous quantity of it must exist in metals. To me it seems equally evident that, agreeably to the idea that heat is material, there must exist in metals a proportionably great quantity of caloric. The intense heat produced when wires are deflagrated by an electrical discharge, cannot otherwise be consistently accounted for. Agreeably to the same idea, every metallic particle in any metallic mass must be surrounded by an atmosphere of caloric; since the *changes* of dimensions, consequent to variations of temperature, can only be explained by corresponding variations in the quantity of caloric imbibed, and in the consequent density of the calorific atmospheres existing in the mass which undergoes these changes*.

54. Such being the constitution of expansible bodies agreeably to the hypothesis in question, it seems to me that the process, by which caloric is propagated through them by *conduction*, must be extremely different from that by which it is transmitted from one part of space to another by *radiation*. In the one case, the calorific particle flies like a bullet projected from a gun, but with an inconceivably greater velocity, which is not sensibly retarded by the reflecting or refracting influence of intervening transparent media: in the other case,

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* I subjoin the language which I have held respecting the constitution of expansible solids, during the last twenty years.

"The expansion of matter, whether solid, liquid, or aëriform, by an increase of temperature, may be thus explained:—

"In proportion as the temperature within any space is raised, there will be more caloric in the vicinity of the particles of any mass contained in the space. The more caloric in the vicinity of the particles, the more of it will combine with them; and in proportion to the quantity of caloric thus combined, will they be actuated by that reciprocally repellent power, which, in proportion to its intensity, regulates their distance from each other.

"There may be some analogy between the mode in which each ponderable atom is surrounded by the caloric which it attracts, and that in which the earth is surrounded by the atmosphere; and as in the latter case, so probably in the former, the density is inversely as the square of the distance.

"At a height at which the atmospheric pressure does not exceed a grain to the square inch, suppose it to be doubled, and supported at that increased pressure by a supply of air from some remote region; is it not evident that a condensation would ensue in all the inferior strata of the atmosphere, until the pressure would be doubled throughout, so as to become at the terrestrial surface 30 pounds instead of the present pressure of 15 pounds? Yet the pressure at the point from which the change would be propagated would not exceed two grains per square inch.

"In like manner, it may be presumed that the atmospheres of caloric are increased in quantity and density about their respective atoms, by a slight increase in the calorific tension of the external medium."

it must be slowly imparted from one calorific atmosphere to another, until the repulsion, sustained on all sides, is in equilibrio. It is in this way that I have always explained the fact that metals are bad radiators, while good reflectors*.

55. In paragraph 25 you allege that conduction of heat differs from electrical induction, because it passes by a very slow process; while induction is, in its distant influence, simultaneous with its force at the place of action. How then can the passage of heat by conduction be "*a process precisely like that of radiation*" (21), which resembles induction in the velocity with which its influence reaches objects, however remote?

56. Although (21) you appeal to the "modern views respecting radiation and conduction of heat," in order to illustrate your conception of the contiguity of the particles of bodies subjected to induction, yet (in 25) you object to the reference which I had made to the same views, in order to show that the intensity of electro-polarization could not be inversely as the number of the polarized particles, interposed between the "inductric" surfaces. Let us then resort to the case above suggested, of the influence of the surfaces of the

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* I will here quote the rationale which has been given in my lectures for the last twenty years:—

"Metals appear to consist of particles so united with each other, or with caloric, as to leave no pores through which radiant caloric can be projected. Hence the only portion of any metallic mass which can yield up its rays by radiation, is the external stratum.

"On the other hand, from its porosity, and probably from its not retaining caloric within its pores tenaciously as an ingredient in its composition, charcoal opposes but little obstruction to the passage of that subtile principle, when in the radiant form; and hence its particles may all be simultaneously engaged in radiating any excess of this principle with which a feeble affinity may have caused them to be transiently united, or in receiving the rays emitted by any heated body, to the emanations from which they may have been exposed.

"We may account in like manner for the great radiating power of earthenware and wood.

"For the same reason that calorific rays cannot be projected from the interior of a metal, they cannot enter it when projected against it from without. On the contrary, they are repelled with such force as to be reflected without any perceptible diminution of velocity. Hence the pre-eminence of metallic reflectors.

"It would seem as if the calorific particles which are condensed between those of the metal, repel any other particles of their own nature which may radiate towards the metallic superficies, before actual contact ensues; otherwise, on account of mechanical imperfection, easily discernible with the aid of a microscope, mirrors would not be as efficacious as they are found to be in concentrating radiant heat. Their influence, in this respect, seems to result from the excellence of their general contour, and is not proportionably impaired by blemishes."

poles of a magnet upon intervening pieces of iron wire. In 1679, 14th series, you suggest this as an analogous case to that of the process of *ordinary* electrical induction, which we have under consideration. Should there be in the one case, a thousand pieces of wire interposed, in the second, a hundred, will it be pretended that the intensity of their reciprocal inductive reaction would be inversely as the number; so that the effect of the last-mentioned number of wires would be *equivalent* to that of the first? Were intervals to be created between the wires, by removing from among the number first mentioned alternate wires, it would seem to me that the energy of their reciprocal influence would be diminished, not only as the number of them might be lessened, but also as they should consequently be rendered more remote.

57. If, as you suggest, the interposition of ponderable particles have any tendency to promote inductive influence (14), there must be some number of such particles by which this effect will be best attained. That number being interposed, I cannot imagine how the intensity of any electro-polarity, thus created in the intervening particles, can, by a diminution of their number, acquire a proportionable increase; and evidently in no case can the excitement in the particles exceed that of the "inductric" surfaces, whence the derangement of electrical equilibrium arises.

58. The repulsive power of electricity being admitted to be inversely as the squares of the distances, you correctly infer, that the aggregate influence of an electrified ball B, situated at the centre of a hollow sphere C, will be a constant quantity, whatever may be the diameter of C. This is perfectly analogous to the illuminating influence of a luminous body situated at the centre of a hollow sphere, which would of course receive the whole of the light emitted, whatever might be its diameter; provided that nothing should be interposed to intercept any portion of the rays. But in order to answer the objection which I have advanced, that the diminution of the density of a "*dielectric*" cannot be compensated by any consequent increase of inductive intensity, it must be shown, in the case of several similar hollow spheres, in which various numbers of electrified equidistant balls should exist, that the influence of such balls upon each other, and upon the surfaces of the spheres, would not be directly as the number of the balls, and inversely as the size of the containing spaces. Were gas-lights substituted for the balls, it must be evident that the intensity of the light in any one of the spheres would be as the number of lights which it might contain: now one of your illustrations (8), above noticed, makes light and elec-

trical induction obey the same law as respects the influence of distance upon the respective intensities.

59. From these considerations, and others above stated, I infer, that if electrical induction were an action of particles in proximity, operating reciprocally with forces varying in intensity with the squares of the distances, their aggregate influence upon any surfaces, between which they might be situated, would be proportionable to their number; and since experience demonstrates that the inductive power is not diminished by the reduction of the number of the intervening particles, I conclude that it is independent of any energy of theirs, and proceeds altogether from that electrical accumulation with which the inductive change is admitted to originate.

60. In paragraph 31, you say, "that at one time there was a distinction between heat and cold. At present that theory is done away with, and the phænomena of heat and cold are referred to the same class, and to different degrees of the same power."

61. In reply to this I beg leave to point out, that although, in ordinary acceptation, cold refers to relatively low temperature, yet we all understand that there might be that perfect negation of heat, or abstraction of caloric, which may be defined absolute cold. I presume that, having thus defined absolute cold, you would not represent it as identical with caloric. For my own part, this would seem as unreasonable as to confound matter with nihility.

62. Assuming that there is only one electric fluid, there appears to me to be so far an analogy between caloric and electricity, that negative electricity conveys, in the one case, an idea analogous to that which cold conveys in the other. But if the doctrine of Du Fay be admitted, there are two kinds of electric matter, which are no more to be confounded than an acid and an alkali. Let us, upon these premises, subject to further examination your argument (1330), that insulation and conduction should be identified, "*since the moment we leave in the smallest degree perfection at either extremity, we involve the element of perfection at the opposite end.*" Let us suppose two remote portions of space, one replete with pure vitreous electricity, the other with pure resinous. Let there be a series of like spaces, containing the resinous and vitreous electricities in as many different varieties of admixture, so that in passing from one of the first-mentioned spaces, through the series to the other, as soon as we should cease to be exposed to the vitreous fluid, in perfect purity, we should begin to be exposed minutely to the resinous; or that, in passing from the purely resinous atmosphere, we should

begin to be exposed to a minute portion of the vitreous fluid; would this be a sufficient reason for confounding the two fluids, and treating the phænomena to which they give rise as the effect of one only?

63. But the discussion into which your illustrations have led me refers to things, whereas conduction and insulation, as I understand them, are opposite and incompatible properties; so that, inasmuch as either prevails, the other must be counteracted. Conduction conveys to my mind the idea of *permeability* to the electric fluid, insulation that of *impermeability*; and I am unable to understand how these irreconcilable properties can be produced by a difference of degree in any one property of electrics and conductors.

64. If, as you infer, glass have, comparatively with metals, an almost infinitely minute degree of the conducting power, is it this power which enables it to prevent conduction, or, in other words, to insulate? Let it be granted that you have correctly supposed conduction to comprise both induction and discharge, the one following the other in perfect conductors within an inexpressibly brief interval: insulation does not prevent induction, but, so far as it goes, prevents discharge. In practice, this part of the process of conduction does not take place through glass during any time ordinarily allotted to our experiments, however correct you may have been in supposing it to have ensued before the expiration of a year or more, in the case of the tubes which you had sealed after charging them. But conceding it to have been thus proved, that glass has, comparatively with metals, an infinitely small degree of the conducting power, is it this minute degree of conducting power which enables it to prevent conduction, or, in other words, to insulate?

65. Induction arises from one or more properties of electricity, insulation from a property of ponderable matter; and although there be no matter capable of preventing induction, as well as discharge, were there such a matter, would that annihilate insulation? On the contrary, would it not exhibit the property in the highest perfection?

66. As respects the residual charge of a battery, is it not evident that any electrical change which affects the surface of the glass, must produce a corresponding effect upon the stratum of air in contact with the coating of the glass? If we place one coating between two panes, will it not enable us, to a certain extent, to charge or discharge both? Substituting the air for one of them, will it not in some measure be liable to an affection similar to that of the vitreous surface, for which it is substituted? In the well-known process of the conden-

sing electrometer, the plate of air interposed between the discs is, I believe, universally admitted to perform the part of an electric, and to be equivalent in its properties to the glass in a coated pane.

67. When I adverted to a gradual relinquishment of electricity by the air to the glass, I did not mean to suggest that it was attended by any more delay than the case actually demonstrates. It might be slow or gradual, compared with the velocity of an electric discharge, and yet be extremely quick comparatively with any velocity ever produced in ponderable matter. That the return should be slow when no coating was employed, and yet quick when it was employed, as stated by you (38), is precisely what I should have expected, because the coating only operates to remove all obstruction to the electric equilibrium. The quantity or intensity of the excitement is dependent altogether upon the electrified surfaces of the air and the glass. You have cited (1632) the property of a charged Leyden jar, as usually accoutred, of electrifying a carrier ball. This I think sanctions the existence of a power to electrify, by convection, the surrounding air to a greater or less depth; since it must be evident that every aërial particle must be competent to perform the part of the carrier ball.

68. Agreeably to the Franklinian doctrine, the electricity directly accumulated upon one side of a pane repels that upon the other side. You admit that this would take place were a vacuum to intervene; but when ponderable matter is interposed you conceive each particle to act as does the body B, when situated as described between A and C (4). But agreeably to the view which I have taken, and what I understand to be your own exposition of the case, B is altogether passive, so that it cannot help, if it does not impede, the repulsive influence. Moreover, it must be quite evident, that were B removed, and A approximated to C, without attaining the striking distance, the effect upon C, and the consequent energy of any discharge upon it from A, would be greater instead of less. If, in the charge of a coated pane, the intermediate ponderable vitreous particles have any tendency to enhance the charge, how happens it that, the power of the machine employed being the same, the intensity of the charge which can be given to an electric is greater in proportion to its tenuity?

69. In reference to the direction of any discharge, it appears to me that as, in *charging*, the fluid must always pass from the cathode to the anode, so in reversing the process it must pursue the opposite course of going from the anode back to the cathode. Evidently the circumvolutions of the circuit are as

unimportant as respects a correct idea of the direction, as their length has been shown by Wheatstone to be incompetent to produce any perceptible delay.

70. The dissipation of conductors being one of the most prominent among electrical phænomena, it appears to me to be an objection to your theory, if, while it fails to suggest any process by which this phænomenon is produced, it assumes premises which seem to be incompatible with the generation of any explosive power. If discharge only involves the restoration of polarized ponderable particles to their natural state, the potency of the discharge must be proportionable to the intensity of the antecedent polarity; yet it is through conductors liable, as you allege, to polarization of comparatively low intensity (31) that discharge takes place with the highest degree of explosive violence.

71. Having inquired how your allegation could be true, that discharge brings bodies to their natural state, and yet causes conductors to be dissipated, you reply (34), that different effects may result from the same cause, acting with different degrees of intensity, as when by one degree of heat ice is converted into water, by another into steam. But it may be urged, that although, in the case thus cited, different effects are produced, yet that the one is not inconsistent with the other, as were those ascribed to electrical discharges. It is quite consistent that the protoxide of hydrogen, which, *per se*, constitutes the solid called ice, should, by one degree of calorific repulsion, have the cohesion of its particles so far counteracted as to be productive of fusion; and yet that a higher degree of the same power should cause them to recede from each other, so as to exist in the aëriform state.

72. In order to found, upon the influence of various temperatures, a good objection to my argument, it should be shown that while a certain reduction of temperature enables aqueous particles to indulge their innate propensity to consolidation, a still further reduction will cause them, in direct opposition to that propensity, to repel each other so as to form steam.

73. In your concluding paragraph you allege, "*that when ponderable particles intervene, during the process of dynamic induction, the currents resulting from this source do require these particles.*" I presume this allegation is to be explained by the conjecture made by you (1729), that since certain bodies, when interposed, did not interfere with dynamic induction, therefore they might be inferred to cooperate in the transmission of that species of inductive influence. But if the induction takes place without the ponderable matter, is it right to assume that this matter *aids*, because it does not prevent

the effect? Might it not be as reasonably inferred, in the case of light, that, although its transmission does not require the interposition of a pane of glass, yet, that when such a pane is interposed, since the light is not intercepted, there is reason to suppose an active cooperation of the vitreous particles in aid of the radiation? It may be expedient here to advert to the fact, that Prof. Henry has found a metallic plate to interfere with the dynamic induction of one flat helix upon another. I have myself been witness of this result.

74. Does not magnetic or electro-dynamic induction take place as well *in vacuo* as *in pleno*? Has the presence of any gas been found to promote or retard that species of reaction? It appears that, agreeably to your experiments, ponderable bodies, when made to intervene, did not enhance the influence in question, while in some of those performed by Henry it was intercepted by them. Does it not follow that ponderable particles may impede, but cannot assist in this process?

75. I am happy to find that, among the opinions which I expressed in my letter to you, although there are several in which you do not concur, there are some which you esteem of importance, though you do not consider yourself justified in extending to them your sanction, being constrained, in the present state of human knowledge, to hold your judgment in suspense. For the present I shall here take leave of this subject, having already so extended my letter as to occupy too much of your valuable time. I am aware that as yet I have not sufficiently studied many of the intricate results of your sagacity, ingenuity and consummate skill in experimental investigations. When I shall have time to make them the subject of the careful consideration which they merit, I may venture to subject your patience to some further trials.

Philadelphia, Jan. 1, 1841.

LXXV. *On the Principles of the Application of Analysis to the Motion of Fluids. By the Rev.* J. Challis, *M.A., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge*[*].

FOR the sake of simplicity, the following remarks will be restricted to the motion of an incompressible fluid: they may without difficulty be extended to fluid motion in general.

It is well known that one of the fundamental equations of fluid motion is obtained on the principle that the mass of each small element remains the same while its position and form

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[*] Communicated by the Author.

are varying. If u, v, w be the parts of the velocity at a point whose coordinates are x, y, z, resolved in the directions of the axes of rectangular coordinates, the equation thus obtained for an incompressible fluid is,

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0 \quad \ldots\ldots \quad \text{(A.)}$$

Under this form it is not proper for application to proposed instances of motion, for which purpose it must admit of exact or approximate integration, and therefore consist of partial differential coefficients of a principal variable. The transformation into the required form is made by assuming $u\,dx + v\,dy + w\,dz$ to be an exact differential of a function of x, y and z. For if ϕ be the function, evidently

$$u = \frac{d\phi}{dx}, \quad v = \frac{d\phi}{dy}, \quad w = \frac{d\phi}{dz},$$

and

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} = 0 \quad \ldots\ldots \quad \text{(B.)}$$

No general rule has hitherto been given for determining when it is allowable to assume $u\,dx + v\,dy + w\,dz$ to be an exact differential, nor has it been ascertained to what particular circumstance of the motion this analytical condition refers. This must be considered a defect in the mathematical theory of hydrodynamics.

The equation (A.) may also be arrived at by the following considerations. In whatever manner a mass of fluid is in motion, we may conceive an unlimited number of surfaces described in it, so that each is perpendicular to the direction of the motion at a given time of the particles through which it passes. These surfaces are not necessarily continuous, but must be supposed to consist of continuous parts possessing the general properties of curve surfaces. Hence if, in two of the surfaces separated by an indefinitely small interval, two indefinitely small rectangular areas be taken opposite to each other, so that the lines joining the corresponding angular points are normals to the surfaces, then the directions of motion in the fluid element lying between them will, by a known property of curve surfaces, all pass through two *focal* lines perpendicular to the directions of the normals, and situated in planes at right angles to each other. The normal velocity will in general vary in passing from one point to another of the intersecting surface, in a manner depending on the arbitrary disturbance of the fluid; but taking it to be uniform through the extent of each of the indefinitely small areas, the continuity of the fluid will be maintained if the same quantity of fluid passes the two areas at the same time. On this principle I have obtained the equation (A.) in the Cambridge Philosophical Transactions (vol. v. part ii. p. 182). The mathematical reasoning is too long to be inserted. I have recently obtained the corresponding equation for compressible fluids by a similar process.

The above method of obtaining the equation (A.) has the advantage of making known the condition that is introduced when $u\,dx + v\,dy + w\,dz$ is assumed to be an exact differential of a function of x, y, and z. For if V be the velocity at the point $x\,y\,z$ distant by r from a focal line, and θ, η be two angles fixing the direction of the normal through that point, we may put $V = f(\theta, \eta) \times \phi(r)$, the latter factor expressing the law of the variation of V at a given instant through a small space resulting from the convergence or divergence of the normals. Hence if α, β, γ be the coordinates of the intersection of the normal with the focal line, we shall have

$$u = f(\theta, \eta) \cdot \phi(r) \cdot \frac{x-\alpha}{r}; \quad v = f(\theta, \eta) \cdot \phi(r) \cdot \frac{y-\beta}{r};$$

$$w = f(\theta, \eta) \cdot \phi(r) \cdot \frac{z-\gamma}{r}; \text{ and } u\,dx + v\,dy + w\,dz$$

$$= f(\theta, \eta) \cdot \phi(r) \left\{ \frac{x-\alpha}{r}\,dx + \frac{y-\beta}{r}\,dy + \frac{z-\gamma}{y}\,dz \right\}.$$

Now since $r^2 = (x-\alpha)^2 + (y-\beta)^2 + (z-\gamma)^2$, the above quantity is an exact differential of a function of x, y, and z, if α, β, γ, θ, and η be constant; that is, if *the variation with respect to space be in the direction of the motion.* This condition, which has hitherto been unnoticed, is independent of the particular mode in which the fluid is disturbed, and being introduced when $d\phi$ is substituted for $u\,dx + v\,dy + w\,dz$, must be attended to in all the subsequent calculations in which ϕ is involved.

The preceding reasoning fails if the lines of motion be not convergent, or the motion be such as the fluid might take if it were either wholly solid, or consisted of solid disconnected parts: for instance, when the fluid mass revolves about a fixed axis and the velocity is a function of the distance from the axis. In fact, for this instance, $u\,dx + v\,dy + w\,dz$ is not an exact differential.

As it appears from the above reasoning that, in general, at the same time that the equation (A.) is transformed into one consisting of partial differential coefficients of a principal variable, the condition is introduced, that the variation with respect to space is in the direction of the motion, it follows that the transformed equation and its integral containing arbitrary functions must be immediately applied to the parts of the fluid disturbed in a given and arbitrary manner, and where, consequently, the direction of the motion is known. The velocity, and direction of the velocity, at other parts are to be inferred from the laws of propagation of the motion and variation of the velocity which the integration reveals.

Admitting the extension of the foregoing considerations to a compressible fluid, the following is the manner in which the problem of the resistance to a vibrating sphere requires, on these principles, to be treated. The general equation in rectangular coordinates, viz.

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

is first to be transformed into polar coordinates, having the centre of the vibrating sphere for the pole. The polar coordinates being r the distance of the point $x\,y\,z$ from the centre of the sphere, θ the angle which r makes with the straight line in which the centre is moving, and η the angle which the plane of these two lines makes with a vertical plane, regard also being had to the motion of the sphere, the transformation, effected in the manner adopted by Poisson in Art. 2 of his Memoir in the *Connaissance des Tems* for 1834, leads to the equation

$$\frac{d^2 . r\,\phi}{d\,t^2} = a^2 . \left\{ \frac{d^2 . r\,\phi}{d\,r^3} + \frac{1}{r^2 \sin\theta} \cdot \frac{d . \left(\sin\theta\,\frac{d . r\,\phi}{d\,\theta}\right)}{d\,\theta} \right.$$
$$\left. + \frac{1}{r^2 \sin^2\theta} \cdot \frac{d^2 . r\,\phi}{d\,\eta^2} \right\},$$

which is independent of the motion of the sphere. If now the reasoning be just by which I have argued, that into this equation must be introduced the condition, that the variation with respect to space be in the direction in which the velocity is impressed, it will plainly reduce itself to

$$\frac{d^2 . r\,\phi}{d\,t^2} = a^2 . \frac{d^2 . r\,\varphi}{d\,r^2};$$

in which, from what has been already said, ϕ may contain as a factor an arbitrary function of θ and η. This is the equation I have employed in the solution I have given of the problem in the Number of this Journal for December 1840, and by it I have been conducted to the value 2 for the coefficient of resistance. I find also that the part of the velocity directed to or from the centre of the sphere varies inversely as the *square* of the distance, and that in a plane drawn at any instant through the centre of the sphere perpendicular to the direction of its motion, the fluid is stationary. Poisson, by integrating the equation with the term containing the differential coefficient with respect to θ included, finds the coefficient of resistance to be $1\frac{1}{2}$, the law of variation of the velocity to be inversely as the *cube* of the distance, and the velocity of the fluid in the above-mentioned plane and in contact with the sphere, to be *half* the velocity of the sphere. With these statements I leave the problem to the consideration of mathematicians, not venturing, in a question of so much difficulty, to assert unhesitatingly the correctness of my own views.

As an objection may be raised against the reasoning in my communication in the December Number, because it takes account only of terms involving the first power of the velocity, which, by reason of the small factor $\frac{r}{\lambda}$, may not be of larger magnitude than rejected terms involving the square of the velocity, I take this opportunity of mentioning that, in a paper submitted to the Cambridge Philosophical Society, I have carried the approximation to the next degree, and, as might have been anticipated, I obtain the former result, the pressure depending on the square of the velocity being the same on the preceding as on the following half of the surface of the vibrating sphere. In the same paper I have attempted to give an answer to the query proposed at the end of the above-mentioned communication, respecting the motion of a small sphere submitted to the mechanical action of the vibrations of an elastic medium. I find that there will be a motion of vibration of the sphere depending on the first power of the velocity of the vibrating medium, and a permanent motion of translation depending on the second power. This result is important if it serves to explain how the vibrations of the æther (assuming them to be like those of air) may at the same time produce two different effects, such as light and heat, or light and chemical action.

Cambridge Observatory, April 13, 1841.

LXXVI. *Contributions to Electricity and Magnetism. No. IV. On Electro-dynamic Induction.* (*Continued.*) *By* Joseph Henry, *LL.D., Professor of Natural Philosophy in the College of New Jersey, Princeton**.

Introduction.

1. In the course of my last paper, it was stated that the investigations which it detailed were not as complete in some parts as I could wish, and that I hoped to develope them more fully in another communication. After considerable delay, occasioned by alterations in the rooms of the physical department of the college, I was enabled to resume my researches, and since then I have been so fortunate as to discover a series of new facts belonging to different parts of the general subject of my contributions. These I have announced to the Society at different times, as they were discovered; and I now purpose to select from the whole such portions as relate particularly to the principal subject of my last paper, namely, the induction at the beginning and ending of a galvanic current, and to present them as a continuation, and, in a measure, as the completion, of this part of my researches. The other results of my labours in this line will be arranged for publication as soon as my duties will permit me to give them a more careful examination.

2. In the course of the experiments I am about to describe, I have had occasion to repeat and vary those given in my last paper; and I am happy to be able to state, in reference to the results, that, except in some minor particulars, which will be mentioned in the course of this paper, I have found no cause to desire a change in the accounts before published. My views, however, of the connexion of the phænomena have been considerably modified, and I think rendered much more definite by the additional light which the new facts have afforded.

3. The principal articles of apparatus used in these experiments are nearly the same as those described in my last paper, namely, several flat coils and a number of long wire helices (III. 6, 7, 8.†). I have, however, added to these a constant battery, on Professor Daniell's plan, the performance of which has fully answered my expectations, and confirmed the accounts given of this form of the instrument by its author. It consists of thirty elements formed of as many copper cylinders, open at the bottom, each five inches and a half in height, three inches and a half in diameter, and placed in earthen

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* From the Transactions of the American Philosophical Society, vol. viii., having been read June 19, 1840.

† When the numerals II. or III. are included in the parenthesis, reference is made to the corresponding Nos. of my contributions.

cups. A zinc rod is suspended in each of these, of the same length as the cylinders, and about one inch in diameter. The several elements are connected by a thick copper wire, soldered to the copper cylinder of one element, and dipping into a cup of mercury on the zinc of the next. The copper and zinc, as usual, are separated by a membrane, on both sides of which is placed a solution of one part of sulphuric acid in ten parts of water; and to this is added, on the side next the copper, as much sulphate of copper as will saturate the solution. The battery was sometimes used as a single series, with all its elements placed consecutively, and at others in two or three series, arranged collaterally, so as to vary the quantity and intensity of the electricity as the occasion might require.

4. The galvanometers mentioned in this paper, and referred to in the last, are of two kinds; one, which is used with a helix, to indicate the action of an induced current of intensity, consists of about five hundred turns of fine copper wire, covered with cotton thread, and more effectually insulated by steeping the instrument in melted cement, which was drawn into the spaces between the spires by capillary attraction. The other galvanometer is formed of about forty turns of a shorter and thicker wire, and is always used to indicate an induced current, of considerable quantity, but of feeble intensity. The needle of both these instruments is suspended by a single fibre of raw silk.

5. I should also state, that in all cases where a magnetizing spiral is mentioned in connexion with a helix, the article is formed of a long, fine wire, making about one hundred turns around the axis of a hollow piece of straw, of about two inches and a half long: also the spiral mentioned in connexion with a coil is formed of a short wire, which makes about twenty turns around a similar piece of straw. The reason of the use of the two instruments in these two cases is the same as that for the galvanometers, under similar circumstances; namely, the helix gives a current of intensity, but of small quantity, while the coil produces one of considerable quantity, but of feeble intensity.

SECTION I.—*On the Induction produced at the moment of the beginning of a Galvanic Current, &c.*

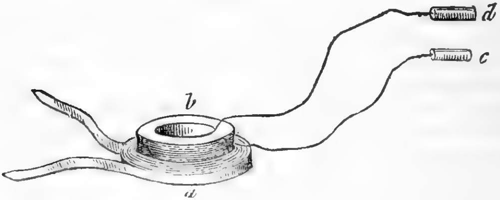
6. It will be recollected that the arrangement of apparatus employed in my last series of experiments gave a powerful induction at the moment of breaking the galvanic circuit, but the effect at making the same was so feeble as scarcely to be perceptible. I was unable in any case to get indications of currents of the third or fourth orders from the beginning in-

duction, and its action was therefore supposed to be so feeble as not materially to affect the results obtained.

7. Subsequent reflection, however, led me to conclude, that in order to complete this part of my investigations, a more careful study of the induction at the beginning of the current would be desirable; and accordingly, on resuming the experiments, my attention was first directed to the discovery of some means by which the intensity of this induction might be increased. After some preliminary experiments, it appeared probable that the desired result could be obtained by using a compound galvanic battery, instead of the single one before employed. In reference to this conjecture the constant battery before mentioned (3.) was constructed, and a series of experiments instituted with it, the results of which agreed with my anticipation.

8. In the first experiment, coil No. 2, which it will be remembered (III. 7.) consists of a copper riband of about sixty feet long, and coiled on itself like the main spring of a watch, was connected with the compound battery, and helix No. 1 (III. 8.), formed of one thousand six hundred and sixty yards of fine copper wire, was placed on the coil to receive the induction, as is shown in Figure 3, which is again inserted here for the convenience of the reader.

Fig. 3.



a represents coil No. 1, *b* helix No. 1, and *c*, *d*, handles for receiving the shock.

This arrangement being made, currents of increasing intensity were passed through the coil by constantly retaining one of its ends in the cup of mercury forming one extremity of the battery, and successively plunging the other end into the cups which served to form the connexions of the several elements of the battery. With the current from one element, the shock at breaking the circuit was quite severe; but at making the same it was very feeble, and could be perceived in the fingers only or through the tongue. With two elements in the circuit, the shock at beginning was slightly increased;

with three elements the increase was more decided, while the shock at breaking the circuit remained nearly of the same intensity as at first, or was comparatively but little increased. When the number of elements was increased to *ten*, the shock at making contact was found fully equal to that at breaking, and by employing a still greater number, the former was decidedly greater than the latter; the difference continually increasing until all the thirty elements were introduced into the circuit.

9. In my last paper, a few experiments are mentioned as being made with a compound battery of Cruickshank's construction; but from the smallness of the plates of this, and the rapidity with which its power declined, I was led into the error of supposing that the induction at the ending of the current, in the case of a short coil, was diminished by increasing the intensity of the battery (see paragraph 19. of No. III.); but by employing the more perfect instrument of Professor Daniell in the arrangement of the last experiment, I am enabled to correct this error, and to state that the induction at the ending remains nearly the same when the intensity of the battery is increased. If the induction depends in any degree on the quantity of current electricity in the conductor, then a slight increase in the induction should take place; since, according to theory, the current is somewhat increased in quantity, in the case of a long coil, by the increase of the intensity of the battery. Although very little, if any, difference could be observed in the intensity of the shock from the secondary current, yet the snap and deflagration of the mercury appeared to be greater from the primary current when *ten* elements of the battery were included in the circuit, than with a single one. The other results which are mentioned in my last paper, in reference to the compound battery, are, I believe, correctly given.

10. The intensities of the different shocks in the foregoing experiments were compared by gradually raising the helix from the coil (see Fig. 3.), until, on account of the distance of the conductors, the shock in one case would be so much reduced as to be scarcely perceptible through the fingers or the tongue, while the shock from another arrangement, but with the same distance of the conductors, would be evident, perhaps, in the hands. The same method was generally employed in the experiments in which shocks are mentioned as being compared, in the other parts of this paper.

11. Experiments were next made to determine the influence of a variation in the length of the coil, the intensity of the

battery remaining the same. For this purpose, the battery consisting of a single element, and the arrangement of the apparatus as represented in Fig. 3, the coil was diminished in length from sixty feet to forty-five, then to thirty, and so on. With the first-mentioned length the shock, at making contact with the battery, was, of course, very feeble, and could be felt only in the tongue; with the next shorter length it was more perceptible, and increased in intensity with each diminution of the coil, until a length of about fifteen feet appeared to give a maximum result.

12. The diminution of the intensity of the shock in the last experiment, after the length of the coil was diminished below fifteen feet, was due to the diminution of the number of spires of the coil, each of which, by acting on the helix, tends to increase the intensity of the secondary current, unless the combined length of the whole is too great for the intensity of the battery. That this is the fact is shown by the following experiment: the helix was placed on a single spire or turn of the coil, and the length of the other part of the copper riband, which did not act on the helix, was continually shortened, until the whole of it was excluded from the circuit; in this case the intensity of the shock at the beginning was constantly increased. We may therefore state generally, that, at the beginning of the battery current, the induction of a unit of its length is increased by every diminution of the length of the conductor.

13. In the experiment given in paragraph 11, the intensity of the shock at the *ending* of the battery current diminishes with each diminution of the length of the coil; and this is also due to the decrease of the number of the spires of the coil, as is evident from an experiment similar to the last, in which the helix was placed on a coil consisting of only two turns or spires of copper riband; the shock at the ending, with this arrangement, was comparatively feeble, but could be felt in the hands. Different lengths of coil No. 2. were now introduced into the same circuit, but not so as to act on the helix; but although these were varied from four or five feet to the whole length of the coil (sixty feet), not the least difference in the intensity of the shock could be perceived. We have, therefore, the remarkable result, that the intensity of the ending induction of each unit of length of the battery current is not materially altered, at least within certain limits, by changing the length of the whole conductor. From this we would infer that the shock depends more on the intensity of the action than on the quantity of the current, since we

know that the latter is diminished in a given unit of the conductor by increasing the length of the whole.

14. We have seen (8.) that, with a circuit composed of ten elements of the compound battery and the coil No. 2, the shock, at the beginning of the current, was fully equal to that at the ending. It was, however, found, that if, in this case, the length of the coil was increased, this shock was diminished; and we may state, as an inference from several experiments, that however great may be the intensity of the electricity from the battery, the shock at the beginning may be so reduced, by a sufficient increase of the length of the primary circuit, as to be scarcely perceptible.

15. It was also found, that when the thickness of the coil was increased, the length and intensity of the circuit remaining the same, the shock at the beginning of the battery current was somewhat increased. This result was produced by using a double coil; the electricity was made to pass through one strand, and immediately afterwards through both: the shock from the helix in the latter case was apparently the greater.

16. By the foregoing results we are evidently furnished with two methods of increasing, at pleasure, the intensity of the induction at the beginning of a battery current; the one consisting in increasing the intensity of the source of the electricity, and the other in diminishing the resistance to conduction of the circuit while the intensity remains the same.

17. The explanation of the effects which we have given, relative to the induction at the beginning, is apparently not difficult. The resistance to conduction in the case of a long conductor and a battery of a single element is so great, that the full development of the primary current may be supposed not to take place with sufficient rapidity to produce the instantaneous action on which the shock from the secondary current would seem to depend. But when a battery of a number of elements is employed, the poles of this, previous to the moment of completing the circuit, are in a state of electrical tension; and therefore the discharge through the conductor may be supposed to be more sudden, and hence an induction of more intensity is produced.

18. That the shock at both making and breaking the circuit in some way depends on the rapidity of formation and diminution of the current, is shown by the following experiment, in which the tension just mentioned does not take place, and in which, also, the current appears to diminish more slowly. The two ends of the coil were placed in the two cups which

formed the poles of the battery, and permanently retained there during the experiment; also, at the distance of about six inches from, say the right hand end of the coil, a loop was made in the riband, which could be plunged into the cup containing the left hand end. With this arrangement, and while only the two extreme ends of the coil were in connexion with the cups of mercury, of course the current passed through the entire length of the riband of the coil; but by plunging the loop into the left hand cup, the whole length of the coil, except the six inches before mentioned, was excluded from the battery circuit. And again, when the loop was lifted out of the cup, the whole length was included. In this way the current in the coil could be suddenly formed and interrupted, while the poles of the battery were continually joined by a conductor; but no shock with either a single or a compound battery could be obtained by this method of operation.

19. The feebleness of the shock at the beginning of the current, with a single battery and a long coil, is not entirely owing to the cause we have stated (17.), namely, the resistance to conduction offered by the long conductor, but also depends, in a considerable degree, if not principally, on the adverse influence of the secondary current, induced in the primary conductor itself, as is shown by the result of the following experiment. Helix No. 1. was placed on a coil consisting of only three spires or turns of copper riband; with this, the shock, both at making and breaking the circuit with a single battery, could be felt in the hands. A compound coil was then formed of the copper ribands of coils No. 3. and 4. rolled together so that the several spires of the two alternated with each other, and when this was introduced into the circuit so as not to act on the helix by its induction, and the battery current passed through, for example, coil No. 3, the shock at making contact with the pole of the battery was so much reduced as to be imperceptible in the hands, while the shock at breaking the contact was about the same as before this addition was made to the length of the circuit. The ends of coil No. 4. were now joined so as to produce a closed circuit, the induced current in which would neutralize the secondary current in the battery conductor itself; and now the shock at making the contact was nearly as powerful as in the case where the short conductor alone formed the circuit with the battery. Hence, the principal cause of the feebleness of the effect at the beginning of the battery current, is the adverse action on the helix of the secondary current produced in the conductor of the battery circuit itself. The shock at the

breaking of the circuit, in this experiment, did not appear affected by joining or separating the ends of coil No. 4.

20. Having investigated the conditions on which the inductive action at the beginning of a battery current depends, experiments were next instituted to determine the nature of the effects produced by this induction: and first the coils were arranged in the manner described in my last paper (III. 79.), for producing currents of the different orders. The result with this arrangement was similar to that which I have described in reference to the ending induction, namely, currents of the third, fourth, and fifth orders were readily obtained.

21. Also, when an arrangement of apparatus was made similar to that described in paragraph 87 of my last paper, it was found that a current of intensity could be induced from one of quantity and the converse.

22. Likewise, the same screening or rather neutralizing effect was produced, when a plate of metal was interposed between two consecutive conductors of the series of currents, as was described (III. Section IV.) in reference to the ending induction. In short, the series of induced currents produced at the beginning of the primary current appeared to possess all the properties belonging to those of the induction at the ending of the same current.

23. I may mention, in this place, that I have found, in the course of these experiments, that the neutralizing power of a plate of metal depends, in some measure, on its superficial extent. Thus a broad plate which extends, in every direction, beyond the helix and coil, produces a more perfect screening than one of the same metal and of the same thickness, but of a diameter only a little greater than that of the coil.

24. The next step in the investigation was to determine the direction of the currents of the different orders produced by the beginning induction, and for this purpose the magnetizing spirals (5.) were used, and the results obtained by these verified by the indications of the galvanometer. It should be stated here, as a fact which was afterwards found of some importance, that although the needle of the galvanometer was powerfully deflected when the instrument was placed in the circuit of the secondary current, yet a very feeble effect was produced on it by the action of a current of the third, fourth, or fifth order. The directions, however, of these current, as indicated by the feeble motion of the needle, were the same as those given by the magnetizing spiral.

25. The direction of the different currents produced at the making of the battery current, as determined by these instruments, is as follows: namely, the direction of the secondary

current is, as stated by Dr. Faraday, adverse to that of the primary current, and, also, the direction of each succeeding current is opposite to that of the one which produced it. We have, therefore, from these results, and those formerly obtained (III. 92.), the following series of directions of currents, one produced at the moment of beginning, and the other at that of ending of the battery current.

	At the beginning,		At the ending.
Primary current	+	……	+
Secondary current . . .	—	……	+
Current of the third order .	+	……	—
Current of the fourth order	—	……	+
Current of the fifth order .	+	……	—

26. These two series, at first sight, may appear very different, but, with a little attention, they will be seen to be of the same nature. If we allow that the induction at the ending of a galvanic battery should be opposite to that at the beginning of the same, then the sign at the top of the second column may be called minus instead of plus, and we shall have the second series — + — + alternating precisely like the first.

27. In connexion with the results given in the last two paragraphs, it is due to Mr. Sturgeon that I should state that, in a letter addressed to me, and published in the Annals of Electricity, he has predicted, from his theory, that I would find, on examination, the series of alternation of currents for the beginning induction which I have here given. I may, however, add, that it appears to me that this result might have been predicted without reference to any theory. There was no reason to suppose the induction at the beginning would be different in its nature from that at the ending, and therefore the series which would be produced from the former might be immediately inferred from that belonging to the latter, by recollecting that the direction of the induction at the beginning should be opposite to that at the ending. I do not wish it to be supposed, however, from this remark, that I had, myself, drawn any inference from my experiments as to the alternations of currents which might be produced by the beginning induction; the truth is, that this action was so feeble with the arrangement of apparatus I employed, that I supposed it could not produce a series of currents of the different orders.

28. In the course of the experiments given in this section, I have found that a shock can be produced without using a coil, by arranging about ten elements of the battery in the

form of a circle, and placing the helix within this. The shock was felt in the hands at the moment of closing the circuit, but the effect at opening the same was scarcely perceptible through the tongue. An attempt was also made to get indications of induction by placing the helix within a circle of dilute acid, connected with a battery instead of a coil, but the effect, if any, was very feeble.

29. I have shown, in the second number of my contributions, that if the body be introduced into a circuit with a battery of one hundred and twenty elements, without a coil, a thrilling sensation will be felt during the continuance of the current, and a shock will be experienced at the moment of interrupting the current by breaking the circuit at any point. This result is evidently due to the induction of a secondary current in the battery itself, and on this principle the remarkable physiological effects produced by Dr. Ure, on the body of a malefactor, may be explained. The body, in these experiments, was made to form a part of the circuit, with a compound galvanic apparatus in which a series of interruptions was rapidly made by drawing the end of a conductor over the edges of the plates of the battery. By this operation a series of induced currents must have been produced in the battery itself, the intensity of which was greater than that of the primary current.

30. In this connexion I may mention that the idea has occurred to me, that the intense shocks given by the electrical fish may possibly be from a secondary current, and that the great amount of nervous organization found in these animals may serve the purpose of a long conductor*. It appears to me, that in the present state of knowledge, this is the only way in which we can conceive of such intense electricity being produced in organs imperfectly insulated and immersed in a conducting medium. But we have seen that an original current of feeble intensity can induce, in a long wire, a secondary current capable of giving intense shocks, although the several strands of the wire are separated from each other only by a covering of cotton thread. Whatever may be the worth of this suggestion, the secondary current affords the means of imitating the phænomena of the shock from the electrical eel, as described by Dr. Faraday. By immersing the apparatus (Fig. 3.) in a shallow vessel of water, the handles being placed at the two extremities of the diameter of the helix, and the hands plunged into the water parallel to a line joining the two poles, a shock is felt through the arms; but

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* Since writing the above, I have found that M. Masson has suggested the same idea in an interesting thesis lately published.

when the contact with the water is made in a line at right angles to the last, only a slight sensation is felt in each hand, but no shock.

31. Since the publication of my last paper, I have exhibited to my class the experiment (No. III. Sect. III.) relative to the induction at a distance on a much larger scale. All my coils were united so as to form a single length of conductor of about four hundred feet, and this was rolled into a ring of five and a half feet in diameter, and suspended vertically against the inside of the large folding doors which separate the laboratory from the lecture-room. On the other side of the doors, in the lecture-room, and directly opposite the coil, was placed a helix, formed of upwards of a mile of copper wire, one-sixteenth of an inch in thickness, and wound into a hoop of four feet in diameter. With this arrangement, and a battery of one hundred and forty-seven square feet of zinc surface divided into eight elements, shocks were perceptible in the tongue, when the two conductors were separated, to the distance of nearly seven feet; at the distance of between three and four feet, the shocks were quite severe. The exhibition was rendered more interesting by causing the induction to take place through a number of persons standing in a row between the two conductors.

SECT. II.—*On apparently two kinds of Electro-dynamic Induction.*

32. The investigations arranged under this head had their origin in the following circumstances. After the publication of my last paper, I received, through the kindness of Dr. Faraday, a copy of the fourteenth series of his researches, and in this I was surprised to find a statement which appeared in direct opposition to one of the principal facts of my communication. In paragraph 59, I state, in substance, that when a plate of metal is interposed between the coil transmitting a galvanic current, and the helix placed above it to receive the induction, the shock from the secondary current is almost perfectly neutralized. Dr. Faraday, in the extension of his new and ingenious views of the agency of the intermediate particles in transmitting induction, was led to make an experiment on the same point; and apparently, under the same circumstances, he found that it "makes not the least difference, whether the intervening space between the two conductors is occupied by such insulating bodies as air, sulphur, and shell-lac, or such conducting bodies as copper and other non-magnetic metals."

33. As the investigation of the fact mentioned above forms

an important part of my paper, and is intimately connected with almost all the phænomena subsequently described in the communication, I was, of course, anxious to discover the cause of so remarkable a discrepancy. There could be no doubt of the truth of my results, since a shock from a secondary current which would paralyze the arms was so much reduced by the interposition of plates of metal, as scarcely to be felt through the tongue.

34. After some reflection, however, the thought occurred to me that induction might be produced in such a way as not to be affected by the interposition of a plate of metal. To understand this, suppose the end of a magnetic bar placed perpendicularly under the middle of a plate of copper, and a helix suddenly brought down on this; an induced current would be produced in the helix by its motion towards the plate, since the copper, in this case, could not screen the magnetic influence. Now, if we substitute for the magnet a coil through which a galvanic current is passing, the effect should be the same. The experiment was tried by attaching the ends of the helix to a galvanometer*, and the result was as I expected: when the coil was suddenly brought down on the plate the needle swung in one direction, and when lifted up, in the other; the amount of deflection being the same, whether the plate was interposed or not.

35. It must be observed in this experiment, that the plate was at rest, and consequently did not partake of the induction produced by the motion of the helix. From my previous investigations, I was led to conclude that a different result would follow, were a current also generated in the plate by simultaneously moving it up and down with the helix. This conclusion, however, was not correct, for on making the experiment, I found that the needle was just as much affected when the plate was put in motion with the helix as when the latter alone was moved.

36. This result was so unexpected and remarkable, that it was considered necessary to repeat and vary the experiment in several ways. First, a coil was interposed instead of the plate, but whether the coil was at rest or in motion with the helix, with its ends separated or joined, the effect on the galvanometer was still the same; not the least screening influence could be observed. In reference to the use of the coil in this experiment, it will be recollected that I have

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* The arrangement will be readily understood by supposing in Fig. 3. the handles removed, and the ends of the helix joined to the ends of the wire of a galvanometer; also, by a plate of metal interposed between the helix and the coil.

found this article to produce a more perfect neutralization than a plate.

37. Next, the apparatus remaining the same, and the helix at rest during the experiment, currents were induced in it by moving the battery attached to the coil up and down in the acid. But in this case, as in the others, the effect on the galvanometer was the same, whether the plate or the coil was interposed or not.

38. The experiment was also tried with magneto-electricity. For this purpose, about forty feet of copper wire, covered with silk, were wound around a short cylinder of stiff paper, and into this was inserted a hollow cylinder of sheet copper, and into this again, a short rod of soft iron; when the latter was rendered magnetic, by suddenly bringing in contact with its two ends the different poles of two magnets, a current, of course, was generated in the wire, and this, as before, was found to affect the galvanometer to the same degree when the copper cylinder was interposed, as when nothing but the paper intervened.

39. The last experiment was also varied by wrapping two copper wires of equal length around the middle of the keeper of a horse-shoe magnet, leaving the ends of the inner one projecting, and those of the outer attached to a galvanometer. A current was generated in each by moving the keeper on the ends of the magnet, but the effect on the galvanometer was not in the least diminished by joining the ends of the inner wire.

40. At first sight, it might appear that all these results are at variance with those detailed in my last paper, relative to the effect of interposed coils and plates of metal. But it will be observed, that in all the experiments just given, the induced currents are not the same as those described in my last communication. They are all produced by motion, and have an appreciable duration, which continues as long as the motion exists. They are also of low intensity, and thus far I have not been able to get shocks by any arrangement of apparatus from currents of this kind. On the other hand, the currents produced at the moment of *suddenly* making or breaking a galvanic current are of considerable intensity, and exist but for an instant. From these, and other facts presently to be mentioned, I was led to suppose that there are two kinds of electro-dynamic induction; one of which can be neutralized by the interposition of a metallic plate between the conductors and the other not.

41. In reference to this surmise, it became important to examine again all the phænomena of induction at suddenly

making and breaking a galvanic current*. And in connexion with this part of the subject, I will first mention a fact which was observed in the course of the experiments given in the last section, on the direction of the induced currents of different orders. It was found, that though the indications of the galvanometer were the same as those of the spiral, in reference to the direction of the induced currents, yet they were very different in regard to the intensity of the action. Thus, when the arrangement of the apparatus was such that the induction at making the battery circuit was so feeble as not to give the least magnetism to the needle, and so powerful at the ending as to magnetize it to saturation, the indication of the galvanometer was the same in both cases.

42. Also, similar results were obtained in comparing the shock and the deflection of the galvanometer. In one experiment, for example, the shock was so feeble at making contact that it could scarcely be perceived in the fingers, but so powerful at the breaking of the circuit as to be felt in the breast; yet the galvanometer was deflected about thirty-five degrees to the right, at the beginning of the current, and only an equal number of degrees to the left, at the ending of the same.

43. In another experiment, the apparatus being the same as before, the magnetizing spiral and the galvanometer were both at once introduced into the circuit of the helix. A sewing needle being placed in the spiral, and the contact with the battery made, the needle showed no signs of magnetism, although the galvanometer was deflected thirty degrees. The needle being replaced, and the battery circuit broken, it was now found strongly magnetized, while the galvanometer was only moved about as much as before in the opposite direction.

44. Also, effects similar to those described in the last two paragraphs were produced when the apparatus was so arranged as to cause the induction at the beginning of the battery current to predominate. In this case the galvanometer was still nearly equally affected at making and breaking battery contact, or any difference which was observed could be referred to a variation in the power of the battery during the experiment.

45. Another fact of importance belonging to the same class has been mentioned before (24.), namely, that the actions of the currents of the third, fourth, and fifth orders produce a very small effect on the galvanometer, compared with that of the secondary current; and this is not alone on account of the diminishing power of the successive inductions, as will be evident from the following experiment. By raising the helix from

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* See my last paper in Lond. and Edinb. Phil. Mag., March 1840, p. 200.

the coil, in the arrangement of apparatus for the secondary current, the shock was so diminished as to be inferior to one produced by the arrangement for a tertiary current; yet, while with the secondary current the needle was deflected twenty-five degrees, with the tertiary it scarcely moved more than one degree; and with the currents of the fourth and fifth orders the deflections were still less, resembling the effect of a slight impulse given to the end of the needle.

46. With the light obtained from the foregoing experiments, I was the more fully persuaded that some new and interesting results might be obtained by a re-examination of my former experiments, on the phænomena of the interposed plate of metal, in the case where the induction was produced by making and breaking the circuit with a cup of mercury; and in this I was not disappointed. The coil (Fig. 3.) being connected with a battery of ten elements, the shocks, both at making and breaking the circuit, were very severe; and these, as usual, were almost entirely neutralized by the interposition of a zinc plate. But when the galvanometer was introduced into the circuit instead of the body, its indications were the same whether the plate was interposed or not; or, in other words, the galvanometer indicated no screening, while, under the same circumstances, the shocks were neutralized.

47. A similar effect was observed when the galvanometer and the magnetizing spiral were together introduced into the circuit. The interposition of the plate entirely neutralized the magnetizing power of the spiral, in reference to tempered steel, while the deflections of the galvanometer were unaffected.

48. In order to increase the number of facts belonging to this class, the last experiments were varied in several ways; and first, instead of the hard steel needle, one of soft iron wire was placed in the spiral, with a small quantity of iron filings almost in contact with one of its ends. The plate being interposed, the small particles of iron were attracted by the end of the needle, indicating a feeble, temporary development of magnetism. Hence the current which moves the needle, and is not neutralized by the interposed plate, also feebly magnetizes soft iron, but not hard steel.

49. Again, the arrangement of apparatus being as in paragraph 46, instead of a plate of zinc, one of cast iron, of about the same superficial dimensions, but nearly half an inch thick, was interposed; with this the magnetizing power of the spiral, in reference to tempered steel, was neutralized; and, also, the action of the galvanometer was much diminished.

50. Another result was obtained by placing in the circuit

of the helix (fig. 3.), at the same time, the galvanometer, the spiral, and a drop of distilled water; with these the magnetizing power of the spiral was the same as without the water, but the deflection of the galvanometer was reduced from ten to about four degrees. In addition to these, the body was also introduced into the same circuit; the shocks were found very severe, the spiral magnetized needles strongly, but the galvanometer was still less moved than before. The current of low intensity, which deflects the needle of the galvanometer in these instances, was partially intercepted by the imperfect conduction of the water and the body.

51. To exhibit the results of these experiments with still more precision, an arrangement of apparatus was adopted similar to that used by Dr. Faraday, and described in the fourteenth series of his researches, namely, a double galvanometer was formed of two separate wires of equal length and thickness, wound together on the same frame; and, also, a double magnetizing spiral was prepared by winding two equal wires around the same piece of hollow straw. Coil No. 1, connected with the battery, was supported perpendicularly on a table, and coils Nos. 3 and 4 were placed parallel to this, one on each side, to receive the induction, the ends of these being so joined with those of the galvanometer and the spiral that the induced current from the one coil would pass through the two instruments, in an opposite direction to that of the current from the other coil. The two outside coils were then so adjusted, by moving them to and from the middle coil, that the induced currents perfectly neutralized each other in the two instruments, and the needle of the galvanometer and that in the spiral were both unaffected when the circuit of the battery was made and broken. With this delicate arrangement the slightest difference in the action of the two currents would be rendered perceptible; but when a zinc plate was introduced so as to screen one of the coils, the needle of the galvanometer still remained perfectly stationary, indicating not the least action of the plate, while the needle in the spiral became powerfully magnetic. When, however, a plate of iron was interposed instead of the one of zinc, the needle of the galvanometer was also affected.

52. From the foregoing results it would seem that the secondary current, produced at the moment of suddenly beginning or ending of a galvanic current, by making and breaking contact with a cup of mercury, consists of two parts, which possess different properties. One of these is of low intensity, can be interrupted by a drop of water, does not magnetize hardened steel needles, and is not screened by the interposition

of a plate of any metal, except iron, between the conductors. The other part is of considerable intensity, is not intercepted by a drop of water, developes the magnetism of hardened steel, gives shocks, and is screened or neutralized by a closed coil, or a plate of any kind of metal. Also, the induced current produced by moving a conductor towards or from a battery current, and that produced by the movement up and down of a battery in the acid, are of the nature of the first-mentioned part, while the currents of the third, fourth, and fifth orders partake almost exclusively of the properties of the second part.

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53. The principal facts and conclusions of this section were announced to the Society in October 1839, and again presented in the form in which they are here detailed in June last. Since then, however, I have had leisure to examine the subject more attentively, and after a careful comparison of these results with those before given, I have obtained the more definite views of the phænomena which are given in the next section.

SECT. III.—*Theoretical Considerations relating to the Phænomena described in this and the preceding Communications. Read November* 20, 1840.

54. The experiments given in the last No. of my contributions were merely arranged under different heads, and only such inferences drawn from them as could be immediately deduced without reference to a general explanation. The addition, however, which I have since made to the number of facts, affords the means of a wider generalization; and after an attentive consideration of all the results given in this and the preceding papers, I have come to the conclusion that they can all be referred to the simple laws of the induction at the beginning and the ending of a galvanic current.

55. In the course of these investigations the limited hypotheses which I have adopted have been continually modified by the development of new facts, and therefore my present views, with the further extension of the subject, may also require important corrections. But I am induced to believe, from its exact accordance with all the facts, so far as they have been compared, that if the explanation I now venture to give be not absolutely true, it is so, at least, in approximation, and will therefore be of some importance in the way of suggesting new forms of experiment, or as a first step towards a more perfect generalization.

56. To render the laws of induction at the beginning and the ending of a galvanic current more readily applicable to

the explanation of the phænomena, they may be stated as follows:—1. During the time a galvanic current is increasing in quantity in a conductor, it induces, or tends to induce, a current in an adjoining parallel conductor in an opposite direction to itself. 2. During the continuance of the primary current in full quantity, no inductive action is exerted. 3. But when the same current begins to decline in quantity, and during the whole time of its diminishing, an induced current is produced in an opposite direction to the induced current at the beginning of the primary current.

57. In addition to these laws, I must frequently refer to the fact, that *when the same quantity of electricity in a current of short duration is passed through a galvanometer, the deflecting force on the needle is the same, whatever be the intensity of the electricity.* By intensity is here understood the ratio of a given quantity of force to the time in which it is expended*; and according to this view, the proposition stated is an evident inference from dynamic principles. But it does not rest alone on considerations of this kind, since it has been proved experimentally by Dr. Faraday, in the third series of his researches.

58. In order to form a definite conception of the several conditions of the complex phænomena which we are about to investigate, I have adopted the method often employed in physical inquiries, of representing the varying elements of action by the different parts of a curve. This artifice has been of much assistance to me in studying the subject, and without the use of it at present, I could scarcely hope to present my views in an intelligible manner to the Society.

59. After making these preliminary statements, we will now proceed to consider the several phænomena; and, first, let us take the case in which the induction is most obviously produced in accordance with the laws as above stated (56.), namely, by immersing a battery into the acid, and also by withdrawing it from the same. During the time of the descent of the battery into the liquid, the conductor connected with it is constantly receiving additional quantities of current electricity, and each of these additions produces an inductive action on the adjoining secondary conductor. The amount, therefore, of induced current produced during any moment of time will be just in proportion to the corresponding increase in the current of the battery during the same moment. Also, the amount of induction during any moment while the cur-

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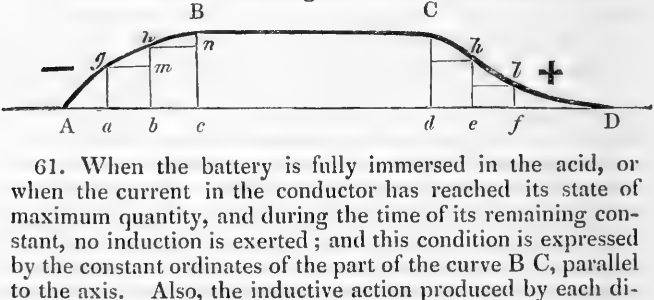
* Or more correctly speaking, the ratio of two quantities of the same species representing the force and time.

rent of the battery is diminishing in quantity will be in proportion to the decrease during the same moment.

60. The several conditions of this experiment may be represented by the different parts of the curve, A, B, C, D, fig. 17, in which the distances, A *a*, A *b*, A *c*, represent the times during which the battery is descending to different depths into the acid; and the corresponding ordinates, *a g*, *b h*, *c* B, represent the amount of current electricity in the battery conductor corresponding to these times. The differences of the ordinates, namely, *a g*, *m h*, *n* B, express the increase in the quantity of the battery current during the corresponding moments of time represented by A *a*, *a b*, *b c*; and since the inductive actions (59.) are just in proportion to these increases, the same differences will also represent the amount of induced action exerted on the secondary conductor during the same moment of time.

Fig. 17.



61. When the battery is fully immersed in the acid, or when the current in the conductor has reached its state of maximum quantity, and during the time of its remaining constant, no induction is exerted; and this condition is expressed by the constant ordinates of the part of the curve B C, parallel to the axis. Also, the inductive action produced by each diminution of the battery current, while the apparatus is in the progress of being drawn from the acid, will be represented by the differences of the ordinates at the other end, C D, of the curve.

62. The sum of the several increments of the battery current, up to its full development, will be expressed by the ordinate *c* B, and this will, therefore, also represent the whole amount of inductive action exerted in one direction at the beginning of the primary current; and, for the same reason, the equal ordinate, C *d*, will represent the whole induction in the other direction at the ending of the same current. Also, the whole time of continuance of the inductive action at the beginning and ending will be represented by A *c* and *d* D.

63. If we suppose the battery to be plunged into the acid to the same depth, but more rapidly than before, then the time represented by A *c* will be diminished, while the whole

amount of inductive force expended remains the same; hence, since the same quantity of force is exerted in a less time, a greater intensity of action will be produced (57.), and consequently a current of more intensity, but of less duration, will be generated in the secondary conductor. The intensity of the induced currents will, therefore, evidently be expressed by the ratio of the ordinate *c* B to the abscissa A *c*. Or, in more general and definite terms, the intensity of the inductive action at any moment of time will be represented by the ratio of the rate of increase of the ordinate to that of the abscissa for that moment[*].

64. It is evident from the last paragraph, that the greater or less intensity of the inductive action will be immediately presented to the eye, by the greater or less obliquity of the several parts of the curve to the axis. Thus, if the battery be suddenly plunged into the acid for a short distance, and then gradually immersed through the remainder of the depth, the varying action will be exhibited at once by the form of A B, the first part of the curve, fig. 17. The steepness of the part A *g* will indicate an intense action for a short time A *a*, while the part *g* B denotes a more feeble induction during the time represented by *a c*. In the same way, by drawing up the battery suddenly at first, and afterwards slowly, we may produce an inductive action such as would be represented by the parts between C and D of the ending of the curve.

65. Having thus obtained representations of the different elements of action, we are now prepared to apply these to the phænomena. And, first, however varied may be the intensity of the induction expressed by the different parts of the two ends of the curve, we may immediately infer that a galvanometer, placed in the circuit of the secondary conductor, will be equally affected at the beginning and ending of the primary current; for, since the deflection of this instrument is due to the whole amount of a current, whatever may be its intensity (57.), and since the ordinates *c* B and C *d* are equal, which represent the quantity of induction in the two directions, and, consequently, the amount of the secondary current, therefore the deflection at the beginning and ending of the battery current will, in all cases, be equal. This inference is in strict accordance with the results of experiment;

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[*] According to the differential notation, the intensity will be expressed by $\frac{dy}{dx}$. In some cases the effect may be proportional to the intensity multiplied by the quantity, and this will be expressed by $\frac{dy^2}{dx}$, x and y representing, as usual, the variable abscissa and ordinate.

for, however rapidly or slowly we may plunge the battery into the acid, and however irregular may be the rate at which it is drawn out, still if the whole effect be produced within the time of one swing of the needle, the galvanometer is deflected to an equal degree.

66. Again, the intensity of one part of the inductive action, for example that represented by A *g*, may be supposed to be so great as to produce a secondary current capable of penetrating the body, and of thus producing a shock* while the other parts of the action, represented by *g* B and C D, are so feeble as to affect the galvanometer only. We would then have a result the same as one of those given in the last section (42.), and which was supposed to be produced by two kinds of induction; for if the shock were referred to as the test of the existence of an induced current, one would be found at the beginning only of the battery current, while, if the galvanometer were consulted, we would perceive the effects of a current as powerful at the ending as at the beginning.

67. The results mentioned in the last paragraph cannot be obtained by plunging a battery into the acid; the formation of the current in this way is not sufficiently rapid to produce a shock. The example was given to illustrate the manner in which the same effect is supposed to be produced, in the case of the more sudden formation of a current, by plunging one end of the conductor into a cup of mercury permanently attached to a battery already in the acid, and in full operation. The current, in this case, rapid as may be its development, cannot be supposed to assume *per saltum* its maximum state of quantity; on the contrary, from the general law of continuity, we would infer that it passes through all the intermediate states of quantity, from that of no current, if the expression may be allowed, to one of full development; there are, however, considerations of an experimental nature which would lead us to the same conclusion (18, 90.), and also to the further inference that the *decline* of the current is not instantaneous. According to this view, therefore, the inductive actions at the beginning and the ending of a primary current, of which the formation and interruption is effected by means of the contact with a cup of mercury, may also be represented by the several parts of the curve, fig. 17.

68. We have now to consider how the rate of increase or diminution of the current, in the case in question, can be altered by a change in the different parts of the apparatus;

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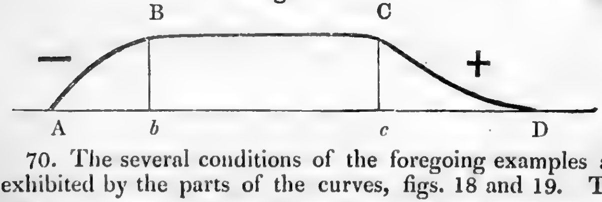
* The shock depends more on the intensity than on the quantity. See paragraph 13.

and, first, let us take the example of a single battery and a short conductor, making only one or two turns around the helix; with this arrangement a feeble shock, as we have seen (11.), will be felt at the making, and also at the breaking of the circuit. In this case it would seem that almost the only impediment to the most rapid development of the current would be the resistance to conduction of the metal; and this we might suppose would be more rapidly overcome by increasing the tension of the electricity; and, accordingly, we find, that if the number of the elements of the battery be increased, the shock at making the circuit will also be increased, while that at breaking the circuit will remain nearly the same. To explain, however, this effect more minutely, we must call to mind the fact before referred to (17.), that when the poles of a compound battery are not connected, the apparatus acquires an accumulation of electricity, which is discharged at the first moment of contact, and which in this case would more rapidly develope the full current, and hence produce the more intense action on the helix at making the circuit.

69. The shock, and also the deflection of the needle, at breaking the circuit with a compound battery and a short coil (9.), appears nearly the same as with a battery of a single element, because the accumulation just mentioned, in the compound battery, is discharged almost instantly, and, according to the theory (71.) of the galvanic current, leaves the constant current in the conductor nearly in the same state of quantity as that which would be produced by a battery of a single element; and hence the conditions of the ending of the current are the same in both cases. Indeed, in reference to the ending induction, it may be assumed as a fact which is in accordance with all the experiments (9, 13, 73, 74, 75, 76, &c.), as well as with theoretical considerations*, *that when the circuit is broken by a cup of mercury, the rate of the diminution of the current, within certain limits, remains the same, however the intensity of the electricity or the length of the conductor may be varied.*

Fig. 18.



70. The several conditions of the foregoing examples are exhibited by the parts of the curves, figs. 18 and 19. The

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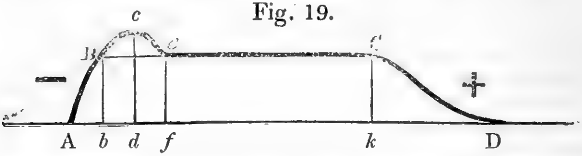
* See the theory of Ohm.—Taylor's Scientific Memoirs, vol. i. p. 312, 511, and vol. ii. p. 401.

gradual development of the current in the short conductor, with a single battery, and the gradual decline of the same, are represented by the gentle rise of A B and fall of C D, fig. 18; while, in the next fig. (19.), the sudden rise of A B indicates the intensity which produces the increased shock, after the number of elements of the battery has been increased. The accumulation of the electricity, which almost instantly subsides, is represented by the part B *c e*, fig. 19; and from this we see, at once, that although the shock is increased by using the compound battery, yet the needle of the galvanometer will be deflected only to the same number of degrees, since the parts B *c* and *c e* give inductive actions in contrary directions, and both within the time of a single swing of the needle, and, consequently, will neutralize each other. The resulting deflecting force will, therefore, be represented by *e f*, which is equal to C *k*, or to *b* B, in fig. 18.

Fig. 19.



The intensity of the shock at the breaking is represented as being the same in the two figures, by the similarity of the rate of descent of the part C D of the curve in each.

71. We have said (69.) that the quantity of current electricity in a short conductor and a compound battery, after the first discharge, is nearly the same as with a single battery. The exact quantity, according to the theory of Ohm, in a unit of length of the conductor, is given by the formula

$$\frac{n\,A}{r\,n + R}.$$

In this *n* represents the number of elements; A, the electromotive force of one element; *r*, the resistance to conduction of one element; and R, the length of the conductor, or rather its resistance to conduction in terms of *r*. Now, when R is very small, in reference to *r n*, as is the case with a very short metallic conductor, it may be neglected, and then the expression becomes

$$\frac{n\,A}{r\,n} \text{ or } \frac{A}{r};$$

and since this expresses the quantity of current electricity in a unit of the length of the circuit, with either a single or a compound battery, therefore, with a short conductor, the quantity of current electricity in the two cases is nearly the same.

72. Let us next return to the experiment with a battery of a single element (68.), and instead of increasing the intensity of the apparatus, as in the last example, let the length of the conductor be increased; then the intensity of the shock at the beginning of the current, as we have seen (14.), will be diminished, while that of the one at the ending will be increased. That the shock should be lessened at the beginning, by increasing the length of the conductor, is not surprising, since, as we might suppose, the increased resistance to conduction would diminish therapidity of the development of the current. But the secondary current, which is produced in the conductor of the primary current itself, as we have seen (19.), is the principal cause which lessens the intensity of the shock, and the effect of this, as will be shown hereafter, may also be inferred from the principles we have adopted.

73. The explanation of the increased shock at the moment of breaking the circuit with the long conductor, rests on the assumption before mentioned (69.), that the velocity of the diminution of a current is nearly the same in the case of a long conductor as in that of a short one. But, to understand the application of this principle more minutely, we must refer to the change which takes place in the quantity of the current in the conductor by varying its length; and this will be given by another application of the formula before stated (71.). This, in the case of a single battery, in which n equals unity, becomes

$$\frac{A}{r + R};$$

and since this, as will be recollected, represents the quantity of current electricity in a unit of length; of the conductor, we readily infer from it that, by increasing the length of the conductor, or the value of R, the quantity of current in a unit of the length is lessened. And if the resistance of a unit of the length of the conductor were very great in comparison with that of r (the resistance of one element of the battery), then the formula would become

$$\frac{A}{R},$$

or the quantity in a single unit of the conductor would be inversely as its entire length, and hence the amount of current electricity in the whole conductor would be a constant quantity, whatever might be its length. This, however, can never be the case in any of our experiments, since in no instance is the resistance of R very great in reference to r, and therefore, according to the formula (73.), the whole quantity of current electricity in a long conductor is always somewhat greater than in a short one.

74. Let us, however, in order to simplify the conditions of the induction at the ending of a current, suppose that the quantity in a unit of the conductor is inversely as its whole length, or, in other words, that the quantity of current electricity is the same in a long conductor as in a short one; and let us also suppose, for an example, that the length of the spiral conductor, fig. 3, was increased from one spire to twenty spires; then, if the velocity of the diminution of the section of the current is the same (69.) in the long conductor as in the short one, the shock which would be received by submitting the helix to the action of one spire of the long coil would be nearly of the same intensity as that from one spire of the short conductor; the quantity of induction, however, as shown by the galvanometer, should be nearly twenty times less; and these inferences I have found in accordance with the results of experiments (75.). If, however, instead of placing the helix on one spire of the long conductor, it be submitted at once to the influence of all the twenty spires, then the intensity of the shock should be twenty times greater, since twenty times the quantity of current electricity collapses, if we may be allowed the expression, in the same time, and exerts at once all its influence on the helix. If, in addition to this, we add the consideration, that the whole quantity of current electricity in a long conductor is greater than that in a short one (73.), we shall have a further reason for the increase of the terminal shock, when we increase the length of the battery conductor.

75. The inference given in the last paragraph, relative to the change in the quantity of the induction, but not in the intensity of the shock from a single spire, by increasing the whole length of the conductor, is shown to be true by repeating the experiment described in paragraph 13. In this, as we have seen, the intensity of the shock remained the same, although the length of the circuit was increased by the addition of coil No. 2. When, however, the galvanometer was employed in the same arrangement, the whole quantity of induction, as indicated by the deflection of the needle, was diminished almost in proportion to the increased length of the circuit. I was led to make this addition to the experiment (13.) by my present views.

76. The explanation given in paragraph 74 also includes that of the peculiar action of a long conductor, either coiled or extended, in giving shocks and sparks from a battery of a single element, discovered by myself in 1831 (see Contrib. No. II.). The induction, in this case, takes place in the con-

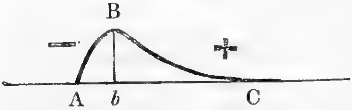
ductor of the primary current itself, and the secondary current which is produced is generated by the joint action of each unit of the length of the primary current. Let us suppose, for illustration, that the conductor was at first one foot long, and afterwards increased to twenty feet. In the first case, because the short conductor would transmit a greater quantity of electricity, the secondary current produced by it would be one of considerable quantity or power to deflect a galvanometer; but it would be of feeble intensity, for although the primary current would collapse with its usual velocity (69.), yet, acting on only a foot of conducting matter, the effect (74.) would be feeble. In the second case, each foot of the twenty feet of the primary current would severally produce an inductive action of the same intensity as that of the short conductor, the velocity of collapsion being the same; and as they are all at once exerted on the same conductor, a secondary current would result, of twenty times the intensity of the current in the former case.

77. To render this explanation more explicit, it may be proper to mention, that a current produced by an induction on one part of a long conductor of uniform diameter, must exist, of the same intensity, in every other part of the conductor; hence the action of the several units of length of the primary current must enforce each other, and produce the same effect on its own conductor that the same current would if it were in a coil, and acting on a helix. I need scarcely add, that in this case, as in that given in paragraph 74, the whole amount of induction is greater with the long conductor than with the short one, because the quantity of current electricity is greater in the former than in the latter.

78. We may next consider the character of the secondary current, in reference to its action in producing a tertiary current in a third conductor. The secondary current consists, as we may suppose, in the disturbance, for an instant, of the natural electricity of the metal, which, subsiding, leaves the conductor again in its natural state; and whether it is produced by the beginning or ending of a primary current, its nature, as we have seen (22.), is the same. Although the time of continuance of the secondary current is very short, still we must suppose it to have some duration, and that it increases, by degrees, to a state of maximum development, and then diminishes to the normal condition of the metal of the conductor; the velocity of its development, like that of the primary current, will depend on the intensity of the action by which it is generated, and also, perhaps, in some degree, on the resistance of the conductor; while, agreeably to the hypothesis we have assumed (69.), the velocity of its diminution is nearly a

constant quantity, and is not affected by changes in these conditions; hence, if we suppose the induction which produces the secondary current to be sufficiently intense, the velocity of its development will exceed that of its diminution, as in the example of the primary current from the intense source of the compound battery of many elements. Now this is the case with the inductions which produce currents of the different orders, capable of giving shocks or of magnetizing steel needles: the secondary currents from these are always of considerable intensity, and hence their rate of development must be greater than that of their diminution; and, consequently, they may be represented by a curve of the form exhibited in fig. 20, in which there is no constant part, and in which the steepness of A B is greater than that of B C. There are, however, other considerations, which will be noticed hereafter (89.), which may affect the form of the part B C of the curve, fig. 20, rendering it still more gradual in its descent, or, in other words, which tend to diminish the intensity of the ending induction of the secondary current.

Fig. 20.



79. It will be seen at once, by an inspection of the curve, that the effect produced, in a third conductor, and which we have called a tertiary current, is not of the same nature as that of a secondary current. Instead of being a single development in one direction, it consists of two instantaneous currents, one produced by the induction of A B, and the other, by that of B C, in opposite directions, of equal quantities, but of different intensities. The whole quantity of induction in the two directions will each be represented by the ordinate B *b*, and hence they will nearly neutralize each other, in reference to their action on the galvanometer, in the circuit of the third conductor. I say, they will *nearly* neutralize each other, because, although they are equal in quantity, they do not both act in absolutely the same moment of time. The needle will, therefore, be slightly affected: it will be impelled in one direction, say to the right, by the induction of A B; but before it can get fairly under way, it will be arrested, and turned in the other direction, by the action of B C. This inference is in strict accordance with observation: the needle, as we have seen (24.), starts from a state of rest, with a velocity which, apparently, would send it through a large arc; but before it has reached, perhaps, more than half a degree, it suddenly stops, and turns in the other direction. As the

needle is first affected by the action of A B, it indicates a current in the adverse direction to the secondary current.

80. Although the two inductions in the tertiary conductor nearly neutralize each other, in reference to the indications of the galvanometer, yet this is far from being the case with regard to the shocks, and the magnetization of steel needles. These effects may be considered as the results alone of the action of A B; the induction of B C being too feeble in intensity to produce a tertiary current of sufficient power to penetrate the body, or overcome the coercive power of the hardened steel. Hence, in reference to the shock and magnetization of the steel needle, we may entirely neglect the action of B C, and consider the tertiary excitement as a single current, produced by the action of A B; and, because this is the beginning induction (56.), the tertiary current must be in an opposite direction to the secondary. For a similar reason, a current of the third order should produce in effect a single current of the fourth order, in a direction opposite to that of the current which produced it, and so on: we have here, therefore, a simple explanation of the extraordinary phænomenon of the alternation of the directions of the currents, of the different orders, as given in this and the preceding paper. See paragraph 25.

81. The operation of the interposed plate (32, 47, 48, &c.), in neutralizing the shock, and not affecting the galvanometer, can also be readily referred to the same principles. It is certain that an induced current is produced in the plate (III. 64.), and that this must react on the secondary in the helix; but it should not alter the total amount of this current, since, for example, at the ending induction, the same quantity of current is added to the helix, while the current in the plate is decreasing, as is subtracted while the same current is increasing. To make this more clear, let the inductive actions of the interposed current be represented by the parts of the curve, fig. 20. The induction represented by A B will react on the current in the helix, and diminish its quantity, by an amount represented by the ordinate *b* B; but the induction represented by B C will act, in the next moment, on the same current, and increase its quantity by an equal amount, as represented by the same ordinate B *b*; and since both actions take place within a small part of the time of a single swing of the needle, the whole deflection will not be altered, and consequently, as far as the galvanometer is concerned, the interposition of the plate will have no perceptible effect.

82. But the action of the plate on the shock, and on the magnetization of tempered steel, should be very different;

for although the quantity of induction in the helix may not be changed, yet its intensity may be so reduced, by the adverse action of the interposed current, as to fall below that degree which enables it to penetrate the body, or overcome the coercive force of the steel. To understand how this may be, let us again refer, for example, to the induction which takes place at the ending of a battery current: this will produce, in both the helix and the plate, a momentary current, in the direction of the primary current, which we have called *plus*; the current in the plate will react on the helix, and tend to produce in it two inductions, which, as before, may be represented by A B and B C of the curve, fig. 20; the first of these, A B, will be an intense action (78.), in the *minus* direction, and will therefore tend to neutralize the intense action of the primary current on the helix; the second (B C) will add to the helix an equal quantity of induced current, but of a much more feeble intensity, and hence the resulting current in the helix will not be able to penetrate the body; no shock will be perceived, or at least a very slight one, and the phænomena of screening will be exhibited.

83. When the plate of metal is placed between the conductors of the second and third orders, or between those of the third and fourth, the action is somewhat different, although the general principle is the same. Let us suppose the plate interposed between the second and third conductors; then the helix, or third conductor, will be acted on by four inductions, two from the secondary current and two from the current in the plate. The direction and character of these will be as follows, on the supposition that the direction of the secondary current is itself *plus*:

The beginning secondary . . .	intense and . . .	*minus.*
The ending secondary	feeble and	*plus.*
The beginning interposed . . .	intense and . . .	*plus.*
The ending interposed	feeble and	*minus.*

Now if the action, on the third conductor, of the first and third of the above inductions be equal in intensity and quantity, they will neutralize each other; and the same will also take place with the action of the second and fourth if they be equal, and hence, in this case, neither shock nor motion of the needle of the galvanometer would be produced. If these inductions are not precisely equal, then only a partial neutralization will take place, and the shock will only be diminished in power; and, also, perhaps, the needle will be very slightly affected.

84. If, in the foregoing exposition, we throw out of con-

sideration the actions of the feeble currents which cannot pass the body, and, consequently, are not concerned in producing the shock, then the same explanation will still apply which was given in the last paper (III. 94.); namely, in the above example, the helix is acted on by the minus influence of the secondary, and the plus influence of the interposed current.

85. We are now prepared to consider the effect on the helix (fig. 3.) of the induced currents produced in the conductor of the primary current itself. These are true secondary currents, and are almost precisely the same in their action as those in the interposed plate. Let us first examine the induced current at the beginning of the primary, in the case of a long coil and a battery of a single element; its action on the helix may be represented by the parts of the curve, fig. 20. The first part, A B, will produce an intense induction opposite to that of the primary current; and hence the action of the two will tend to neutralize each other, and no shock, or a very feeble one, will be produced. The ending action of the same induced current, which is represented by B D, restores to the helix the same quantity of current electricity (but in a feeble state) which was neutralized by A B, and hence the needle of the galvanometer will be as much affected as if this current did not exist. These inferences perfectly agree with the experiment given in paragraph 19. In this, when the ends of the interposed coil were joined so as to neutralize the induced current in the long conductor, the shock at the beginning of the primary current was nearly as powerful as with a short conductor, while the amount of deflection of the galvanometer was unaffected by joining the ends of the same coil.

86. At first sight it might appear that any change in the apparatus which might tend to increase the induction of the primary current (16.) would also tend to increase, in the same degree, the adverse secondary in the same conductor; and that hence the neutralization mentioned in the last paragraph would take place in all cases; but we must recollect, that if a more full current be suddenly formed in a conductor of a given thickness, the adverse current will not have, as it were, as much space for its development, and, therefore, will have less power in neutralizing the induction of the primary than before. But there is another, and, perhaps, a better reason, in the consideration, that in the case of the increase of the number of elements of the battery, although the rapidity of the development of the primary current is greater, yet the increased resistance which the secondary meets with, in its motion against the action of the several elements, will tend to

diminish its effect. Also, by diminishing the length of the primary current, we must diminish (76.) the intensity of the secondary, so that it will meet with more resistance in passing the acid of the single battery, and thus its effects be diminished.

87. The action of the secondary current, in the long coil at the *ending* of the primary current, should also, at first sight, produce the same screening influence as the current in the interposed plate; but, on reflection, it will be perceived that its action in this respect must be much more feeble than that of the similar current at the beginning; the latter is produced at the moment of making contact, and hence it is propagated in a continuous circuit of conducting matter, while the other takes place at the *rupture* of the circuit, and must therefore be rendered comparatively feeble by being obliged to pass through a small portion of heated air; very little effect is therefore produced on the helix by this induction (19.). The fact that this current is capable of giving intense shocks, when the ends of a long wire, which is transmitting a primary current, are grasped at the time of breaking the circuit, is readily explained, since, in this case, the body forms, with the conductor, a closed circuit, which permits the comparatively free circulation of the induced current.

88. It will be seen that I have given a peculiar form to the beginning and ending of the curves, figs. 17, 18, &c. These are intended to represent the variations which may be supposed to take place in the rate of increase and decrease of the quantity of the current, even in the case where the contact is made and broken with mercury. We may suppose, from the existence of analogous phænomena in magnetism, heat, &c., that the development of the current would be more rapid at first than when it approximates what may be called the state of current saturation, or when the current has reached more nearly the limit of capacity of conduction of the metal. Also, the decline of the current may be supposed to be more rapid at the first moment than after it has lost somewhat of its intensity, or sunk more nearly to its normal state. These variations are indicated by the rapid rise of the curve, fig. 17, from A to *g*, and the more gradual increase of the ordinates from *h* to B; and by the rapid diminution of the ordinates between C and *l*, and the gradual decrease of those towards the end of the curve.

89. These more minute considerations, relative to the form of the curve, will enable us to conceive how the time of the ending of the secondary current, as we have suggested (78.), may be prolonged beyond that of the natural subsidence of

the disturbance of the electricity of the conductor on which this current depends. If the development of the primary current is produced by equal increments in equal times, as would be the case in plunging the battery (59.) into the acid with a uniform velocity, then the part A B of the curve fig. 17 would be a straight line, and the resulting secondary current, after the first instant, would be one of constant quantity during nearly the whole time represented by A *c*; but if the rate of the development of the primary current be supposed to vary in accordance with the views we have given in the last paragraph, then the quantity of the secondary current will begin to decline before the termination of the induction, or as soon as the increments of the primary begin to diminish; and hence the whole time of the subsidence of the secondary will be prolonged, or the length of *b* C, fig. 20, will be increased, the descent of B C be more gradual, and the intensity of the ending induction of the secondary current be diminished (see last part of paragraph 78.).

90. Besides the considerations we have mentioned (88.), there are others of a more obvious character, which would also appear to affect the form of particular parts of the curve. And first we might perhaps make a slight correction in the drawing of figs. 17, 18, &c., at the point A, in consideration of the fact that the very first contact of the end of the conductor with the surface of the mercury is formed by a point of the metal, and hence the increment of development should be a little less rapid at the first moment than after the contact has become larger; or in other words, the curve should perhaps start a little less abruptly from the axis at the point A. Also Dr. Page has stated* that he finds the shock increased by spreading a stratum of oil over the surface of the mercury; in this case it is probable that the termination of the current is more sudden, on account of the prevention of the combustion of the metal by means of the oil, and the fact that the end of the conductor is drawn up into a non-conducting medium.

91. The time of the subsidence of the current, when the circuit is broken by means of a surface of mercury, is very small, and probably does not exceed the ten thousandth part of a second; but even this is an appreciable duration, since I find that the spark at the ending presents the appearance of a band of light of considerable length, when viewed in a mirror revolving at the rate of six hundred times in a second; and I think the variations in the time of ending of the current under different conditions may be detected by means of this instrument.

92. Before concluding this communication, I should state

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[*] Silliman's Journal.

that I have made a number of attempts to verify the suggestion given in my last paper (III. 127.), that an inverse induction is produced by a galvanic current by a change in the distance of the conductors, but without success. These attempts were made before I had adopted the views given in this section, and since then I have found (80.) a more simple explanation of the alternation of the currents.

93. In this Number of my Contributions, the phænomena exhibited by the galvanic apparatus have alone been discussed. I have, however, made a series of experiments on the induction from ordinary electricity, and the reaction of soft iron on currents, and I think that the results of these can also be referred to the simple principles adopted in this paper; but they require further examination before submitting them to the public.

[Prof. Henry's 3rd Series appeared in L. & E. Phil. Mag. vol. xvi. p. 200].

LXXVII. *Correction of an Error in Prof.* Dove's *Letter on the Law of Storms. By Sir* David Brewster, *K.H.*

To Richard Taylor, Esq.

Dear Sir,

I OBSERVE in the Philosophical Magazine for November 1840, a letter addressed to you on the Law of Storms, which contains the following passage:—

"In an article upon Lieut.-Colonel Reid's law of storms in the Edinburgh Review, I find my meteorological researches again alluded to, but upon a distinct ground. The anonymous Reviewer, in his patriotic anxiety to satisfy his readers of the purely British growth of this theory, allows that some remarkable passages upon the subject had previously appeared in the memoirs of the Berlin Professor, but that these are mere ingenious speculations, *for they are no more.* The term *passage*, for a memoir (on barometric minima) of seventeen closely printed pages, strikes me as a little extraordinary, but perhaps this arises from my imperfect acquaintance with the nice distinctions of your language. I leave it to my English readers to determine, with what degree of justice results deduced from a greater number of contemporary observations, than (as I believe) had ever previously or have even since been brought together, can be represented as *no more than ingenious speculations.*"

As the article on General Reid's law of storms in the Edinburgh Review was written by me, I feel it necessary to state that *Professor Dove's name is not even once mentioned in that Review, nor his labours in any way referred to.*

Had I been disposed to enter into any discussion respecting the earliest discovery of the rotatory character of storms, I should certainly not have awarded the honour to Prof. Dove, but to the late Colonel James Capper, of the East India Company's service. We agree with General Reid in giving the merit of first suggesting the theory to Colonel Capper; but we must at the same time claim for Mr. Redfield the greater honour of having fully investigated the subject, and, apparently, established the theory upon an impregnable basis.

I am, dear Sir, ever yours,

D. Brewster.

St. Leonard's, St. Andrew's, May 15th, 1841.

LXXVIII. *Proceedings of Learned Societies.*

CHEMICAL SOCIETY OF LONDON.

April 13, 1841. — THE following papers were read:—

1. "On the Preparation and Formation of Yellow Prussiate of Potash," by Professor Liebig.

In order to explain the reaction between animal matters and carbonate of potash, when fused together at a red heat, which gives rise to this salt, it is necessary to keep in mind the following properties of the salt: When heated to redness in a close vessel, ferrocyanide of potassium is decomposed into cyanide of potassium, carburet of iron and nitrogen gas; that is, looking upon the ferrocyanide of potassium as a double cyanide, the cyanide of iron is converted into carburet of iron and nitrogen gas, while the cyanide of potassium escapes decomposition. The cyanides of metals in general which can combine with carbon, are decomposed in the same way as the cyanide of iron; thus the cyanide of silver when heated gives at first a little cyanogen, but afterwards it fuses, and glowing suddenly, gives nitrogen gas, the carbon remaining in combination with the silver. The addition of carbonate of potash to the heated ferrocyanide of potassium prevents the decomposition of any cyanogen, cyanide of potassium being then formed, together with oxide of iron; and when charcoal forms a third ingredient of the fused mixture, the oxide of iron is reduced to the metallic state. Hence ferrocyanide of potassium cannot be supposed ready formed in the red-hot mixture of the iron pot in which it is manufactured, that mixture containing both charcoal and carbonate of potash.

A general view is then taken of the process of manufacture of this salt. Animal substances, such as dried blood, horn, hoofs, and bristles, with common pearlashes, are the materials employed. The animal matter is used either in its natural state, or it is previously submitted to distillation, as in the preparation of ammonia, and the residual charcoal merely employed for the manufacture of the prussiate. The projection of animal matter into the melted potash occasions a lively effervescence, from the evolution of carbonic acid and some combustible gases. The liquid is stirred after each addition of the materials. The usual proportions employed are equal parts of pearlashes and animal matter, or ten parts of the former and eight parts of carbonized animal matter. Three or four per cent. of iron filings are usually added to the mixture. After each addition of animal matter the heat is urged until the whole is fused, and the melted material, which is of a thick consistence, is not removed from the pot until the charcoal is seen to be equally diffused through the whole mass. The mass, after cooling, is placed in an iron pan filled with water, the clear liquid after a time drawn off, and water boiled several times on the insoluble residue. The liquids are evaporated for crystallizing the salt at a temperature not exceeding 203° Fahr. The formation of prussiate takes place after the solution of the melted mass, by the action of the matters dissolved upon the insoluble residue; for this melted mass yields nothing but cyanide of potassium to alcohol, and contains no prussiate. In explanation of the formation of cyanide of potassium in the melted mass, it is stated that metallic potassium readily produces that salt when fused with calcined blood, disengaging at the same time a considerable quantity of charcoal; the proportion of nitrogen to carbon, in cyanogen, being one equivalent of the first to two of the last, while in blood, hair, and horn, the proportion is 1 to 6. Now when these animal matters are fused at a high temperature with potash, the free charcoal reduces the potash to the state of potassium; the latter then acts upon the azotized carbonaceous matter, forming cyanogen, with which it unites. A second mode in which cyanide of potassium is produced, is when ammoniacal gas is conducted over a mixture of carbonate of potash and charcoal at a red heat. This is accounted for by the action of ammonia upon charcoal alone at a red heat; the gas is entirely converted into hydrocyanic acid and hydrogen ($N\,H_3$ and $2\,C = H,\ N\,C_2$ and $2\,H$). Now hydrocyanic acid decomposes carbonate of potash at a red heat, forming cyanide of potassium. Hence the product of cyanide of potassium is most considerable when the animal matter is used in its natural state, and not previously carbonized, a fact of which the manufacturers of prussiate of potash have long been aware from experience. To account for the subsequent conversion of the cyanide of potassium in the process into prussiate, it is absolutely necessary that iron exist in the fused mass; but it may indifferently be in the condition of metallic iron, the protosulphuret or the protoxide of iron. The first is readily dissolved by a solution of cyanide of potassium with evolution of hydrogen gas ($3\,K\,Cy$ with $H\,O$ and $Fe = 2\,K\,Cy,\ Fe\,Cy$ and $K\,O$ and H); the second with the formation of sulphuret of potassium, and the third with that of caustic potash. When the iron is added in the state of protosulphate to a solution of cyanide of potassium, one-third of the latter salt becomes cyanide of iron (a brown insoluble matter), which is dissolved by the other two-thirds of the alkaline cyanide, and the ferrocyanide formed. These processes are not altered in the slightest degree by mixing caustic potash or its carbonate, or the sulphuret of potassium, with the solution of cyanide of potassium. Much of the iron necessary, it is well known, is derived from the corrosion of the iron pot in which the fusion is conducted. Professor Liebig assigns an important place to the sulphur of the sulphate of potash, usually present to the extent of 12 or 16 per cent. in pearlashes, in effecting this corrosion. In the decomposition of the sulphate of potash by charcoal, bisulphuret of potassium is formed, and carbonate of potash. Thus,

$$2SO_3 + 2KO \text{ and } 4C = KS_2 \text{ and } KO, CO_2 \text{ with } 2CO_2 \text{ and } CO.$$

The bisulphuret of iron assumes an atom of iron, either from the sides of the iron vessel or from iron filings which are added; the double sulphuret thus formed is very fusible, and will consequently be equally diffused through the melted mass.

The deficiency of product which frequently occurs in the manufacture of prussiate of potash is ascribed principally to two causes: 1st, to the want of iron in the fused mass. The cyanide of potassium is then, instead of being converted into ferrocyanide when thrown into water, decomposed by the free caustic potash when heat is applied to its solution. Uniting with the elements of water, its cyanogen is converted into formic acid and ammonia:

$$NC_2, K \text{ and } 4HO = C_2HO_3 + KO \text{ and } NH_3.$$

This destruction of the cyanide may be avoided by adding iron or its sulphuret to the ley, or better, the protosulphate of iron. Another cause of loss of cyanide in the pot itself is pointed out. The bisulphuret of potassium yields sulphur to the cyanide of potassium, and converts the latter into sulphocyanide of potassium. But if the mixture contain a quantity of iron sufficient to unite with all the sulphur, the formation of sulphocyanide will be prevented. Indeed, sulphocyanide of potassium itself is decomposed by iron at a high temperature, and resolved into sulphuret of iron and cyanide of potassium. It is thus seen that, by increasing the proportion of iron, the formation of sulphocyanide is at once prevented, and sulphuret of iron offered in quantity more than sufficient for its solution afterwards by the cyanide of potassium. The quantity of iron necessary to add in the fusion varies from 12 to 20 per cent., with the proportion of sulphate of potash in the potashes used. If a sulphocyanide appears in the mother liquors, the proportion of iron must be increased. The only remaining condition for the formation of ferrocyanide of potassium, is the complete exclusion of air during the fusion. Cyanide of potassium cannot be kept in fusion exposed to air without absorbing oxygen, and being converted into cyanate of potash; hence the advantage which English manufacturers derive from effecting this fusion in close vessels. Cyanate of potash may also be produced by the action of cyanide of potassium upon the sulphate of potash existing in the potashes, sulphuret of potassium being at the same time formed. Now cyanate of potash is decomposed, by the application of heat to its solution, into carbonate of potash and ammonia. The ammonia which escapes during the evaporation of the ley may therefore come from this source as well as from the decomposition of cyanide of potassium by potash, already adverted to.

Mr. G. Lowe observed that the preparation of prussiate of potash from the waste gas liquors was difficult, owing to the presence of sulphur; but he believed that the suggestions of Professor Liebig would enable chemists to remove this obstacle to the manufacture.

2. "On the Formation of Mellon," by Mr. E. A. Parnell, of University College.

This paper referred to the decomposition which occurs in the process for mellon, from the substance considered by Liebig to be the isolated radical of the sulphocyanides (as obtained by the action of chlorine or nitric acid on sulphocyanide of potassium); for which substance, having previously shown it to contain hydrogen and oxygen in addition to the elements belonging to the true sulphocyanogen, the author proposes the term *metasulphocyanogen*. It became necessary, therefore, to seek for other products of the decomposition of this substance than those hitherto recognized, namely, mellon, sulphur, and bisulphuret of carbon; and in decomposing pure and dry metasulphocyanogen by heat, water, sulphuretted hydrogen, and hydrosulphocyanic acid, in addition to the above, were detected. Admitting the formula for metasulphocyanogen, $S_{12}\ Cy_6\ H_3\ O$, to which he has been led by analyses, the decomposition is explained as follows:—Three equivalents of metasulphocyanogen, containing $S_{36}\ C_{36}\ N_{18}\ H_9\ O_3$, are resolved into four of mellon, $C_{24}\ N_{16}$; two of hydrosulphocyanic acid, $S_4\ C_4\ N_2\ H_2$; four of sulphuretted hydrogen, $S_4\ H_4$; eight of bisulphuret of carbon, $S_{16}\ C_8$; twelve of sulphur, and three of water, $H_3\ O_3$. The sum of the elements of these compounds will be found to comprise $S_{36}\ C_{36}\ N_{18}\ H_9\ O_3$; or three equivalents of metasulphocyanogen.

April 27.—The following communications were read:—

1. A letter from Mr. M. Scanlan, of Wolverhampton, describing the appearance of flashes of light observed during the crystallization of nitrate of strontian in the dark.

2. "Action of Nitric Acid on Castor Oil," by Mr. T. G. Tilley. (See present Number, page 417.)

3. "On Bleaching Salts," by M. Detmer, Esq. (See present Number, page 422.)

4. The following Note by Professor Graham, "On the Preparation of Chlorate of Potash." It is well known that the ordinary processes for this important salt are attended with some practical difficulties. When a stream of chlorine gas is passed through a strong solution of carbonate of potash, the absorption of the gas is rapid and complete, till one-half of the alkaline carbonate is decomposed; but the remaining portion, which is in the state of bicarbonate, is not so easily acted upon. To decompose the latter salt completely, chlorine must be applied in excess, and the decomposition is attended by the formation of free hypochlorous acid, as has been proved by Mr. Detmer. The liquid is also at the end highly bleaching, and contains much hypochlorite of potash. The boiling necessary to convert the latter into chlorate of potash and chloride of potassium occasions, according to M. Morin, a considerable loss of oxygen, and thus lessens the product of chlorate. When a strong solution

of caustic potash is substituted in this process for the carbonate, the absorption of chlorine proceeds without interruption; but the liquid when saturated bleaches strongly from hypochlorite formed. A long-continued boiling is required to destroy this property completely, and as oxygen escapes, the chlorate obtained must be deficient in quantity in a corresponding proportion. The process which the author recommends, and which is attended with none of these inconveniences, consists in mixing carbonate of potash intimately with an equivalent quantity of dry hydrate of lime, and exposing the mixture to chlorine gas. This mixture, although quite dry, absorbs the gas with prodigious energy, the temperature rises much above 212°, and water is freely evolved. When saturated it may be moderately heated, which destroys a mere trace of hypochlorite it contains. The whole lime is found in the state of carbonate, and the potash as chlorate and chloride of potassium. The solution of the two latter salts is neutral, without any bleaching property, and free from lime. The chlorate of potash may be crystallized from it in the usual way. Carbonate of potash, when moistened and exposed to chlorine, without the hydrate of lime, absorbs the gas with great avidity, and certainly answers better than a strong solution of the same salt; but the absorption becomes slow after the salt is in the state of bicarbonate, and subsequently a large quantity of the bleaching hypochlorite of potash is produced. In the new process described above, there is no reason to believe that the carbonate of potash is decomposed by the dry hydrate of lime till the chlorine is presented to the mixture; then, while the lime attracts the carbonic acid, the chlorine acts simultaneously upon the potash, and the carbonate of potash is thus readily decomposed. The same principle of calling in a secondary agency to promote combination may be taken advantage of in many other cases. One of these, of some interest, is the promotion of the absorption of sulphuretted hydrogen by hydrate of lime, through the influence of other salts. Thus hydrate of lime, dry or slightly damped, ceases to absorb sulphuretted hydrogen long before it is saturated with that gas; but if mixed with an equivalent of hydrated sulphate of soda, the absorption takes place with greatly increased avidity, and goes on till two equivalents of sulphuretted hydrogen are taken up for one equivalent of lime. But here, with the assistance of sulphuretted hydrogen, the hydrate of lime decomposes the sulphate of soda, sulphate of lime being formed, while caustic soda combines with the sulphuretted hydrogen.

The author has found that the last mixture may be applied with advantage, from its great absorbing power, in purifying coal-gas, where the highest degree of purification is desirable, and where the products, sulphate of lime and hydrosulphuret of sulphuret of sodium, can be œconomically applied. He recommends it to be introduced into the last of the dry lime-purifiers.

5. An extract from a letter from Ollive Sims, Esq., Shelton, Staffordshire Potteries, was read, announcing a considerable and very accessible source of the hitherto very rare mineral, phosphate of yttria.

The crushed cobalt ore, from Johannisberg in Sweden, when converted into zaffre, or dissolved by acids, leaves a yellowish mineral in crystalline grains, in the proportion of about one pound avoirdupois from one thousand pounds of ore. This mineral is the phosphate of yttria. It may be decomposed by fusion with alkaline carbonates, or by boiling with pretty strong sulphuric acid.

LONDON ELECTRICAL SOCIETY.

May 18th, 1841.—The Secretary presented the Society with a copy of the second edition of his "Electrotype Manipulation."

A letter from Thomas Pine, Esq. was read, containing many interesting facts on the power exercised by the points of living vegetables in "drawing off" electricity from the atmosphere. That many important functions in vegetation result from this, the author concluded from the singular circumstance of dew being deposited only on the apex of points or leaves. Apparently smooth leaves are, when examined by a microscope, found studded with these natural attractors; and, which is still more illustrative of the case in question, plants vegetate with more vigour in an electrified atmosphere than when the soil is electrified. This communication was merely a general sketch of conclusions which the author promised to illustrate more at large on a future occasion.

Read, "Some general observations on Electrotype Manipulation, and on the construction of a constant Acid Battery." By Charles V. Walker, Esq., Hon. Sec.

The conclusions adduced by the author are the results of a long series of experiments, and tend to generalize the subject. He describes the most advantageous diaphragms, battery cells, and materials for moulds, and speaks very much in favour of a "constant acid battery," the construction of which is described. It is slow in its action, but the deposits obtained are in every respect good. No battery can be better fitted to deposit coatings on objects.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

March 22, 1841.—Professor Miller gave an account of his observations on supernumerary rainbows, made for the purpose of comparing the observed places of the principal bows, and their supernumeraries, with their places as computed by the Astronomer Royal on the undulatory hypothesis. (Transactions of the Cambridge Philosophical Society, vol. vi. p. 379.) The bows were formed in the manner first employed by M. Babinet, by a vertical cylindrical stream of water. The incident light was homogeneous, or nearly so. In some cases thirty supernumerary bows were seen within the primary, and twenty-five exterior to the secondary.

The following table exhibits the observed radii of the brightest part of each principal bow and its first and second dark rings, and the theoretical radii of the brightest part of each principal bow and its second dark ring, computed from the interval between the geometrical bow and the observed place of the first dark ring.

1. Diameter of cylinder of water = 0·0206 inch; index of refraction = 1·3318.

(Radius of geometric primary bow = 42° 15′.)

	Observation.	Theory.
Radius of brightest part of primary	41° 51′·4	41° 45′·4
Radius of first dark ring........	41 7	
Radius of second dark ring	40 16	40 14·4

(Radius of geometric secondary bow = 50° 34′.)

Radius of brightest part of secondary	51° 25′	51° 27′·5
Radius of first dark ring	52 37	
Radius of second dark ring	54 7	54 12.

2. Diameter of cylinder of water = 0·02105 inch; index of refraction = 1·33464.

(Radius of geometric primary bow = 41° 50′·4.)

	Observation.	Theory.
Radius of brightest part of primary..	41° 27′·7	41° 24′·7
Radius of first dark ring	40 51·4	
Radius of second dark ring	40 4·4	40 5′·7

(Radius of geometric secondary bow = 51° 19′).

Radius of brightest part of secondary	51° 57′	52° 5′·3
Radius of first dark ring..........	53 5	
Radius of second dark ring........	54 27·6	54 27

3. Diameter of cylinder of water = 0·0135 inch; index of refraction = 1·33453. In this series of observations the values of the diameter of the cylinder and of the index of refraction are rather doubtful.

(Radius of geometric primary bow = 41° 52′.)

	Observation.	Theory.
Radius of brightest part of primary	41° 20′	41° 18′
Radius of first dark ring..........	40 33	
Radius of second dark ring........	39 29	39 32

(Radius of geometric secondary bow = 51° 17′·5.)

Radius of brightest part of secondary	52° 16′	52° 18′·5
Radius of first dark ring	53 37	
Radius of second dark ring........	55 31	55 26.

April 26, 1841.—Prof. Challis read a communication on the motion of a small sphere submitted to the dynamical action of the vibrations of an elastic medium. The mathematical reasoning embraced terms involving the *square* of the velocity of the vibrating medium, and the principal conclusion arrived at was, that the motion of the sphere consists partly of a vibratory motion, and partly of a permanent motion of translation, the latter depending on the terms which contain the square of the velocity. It was thought that this result may have important applications in the physical theories of light and heat.

The solution of the above problem involves that of another of more immediate interest, viz. the determination of the resistance to the motion of a ball-pendulum vibrating in the air. Professor Challis obtains the same coefficient of resistance as in several previous solutions which he has given of this question, the terms involving the square of the velocity being found to have no effect on its value. The difference between his result and that obtained by other writers on the same question, is shown to be entirely owing to his adopting a new principle in the application of analysis to fluid motion, which may be thus stated. If u, v, w be the velocities resolved in the directions of three rectangular axes of a fluid particle situated at the point whose coordinates are x, y, z, then, in order that the motion may admit of determination by the integral of a partial differential equation, it is necessary to suppose that $u\,dx + v\,dy + w\,dz$ is an exact differential of a function of x, y, and z. This supposition must therefore be true, independently of the particular mode of disturbance and of the form of the arbitrary function by which it is expressed. The quantity in question is an exact differential, for reasons drawn from the nature of curve surfaces, and independently of all that is arbitrary, if the variation of the coordinates from one point to another at a given instant be in the direction in which the motion is impressed by the arbitrary disturbance, and not otherwise. Hence the variation of the coordinates in the partial differential equation to which that supposition conducts, must be subject to the same limitation. It is plain that this principle, if true, must materially affect the manner of treating many hydrodynamical problems, and would seem therefore to merit the attention of mathematicians *.

GEOLOGICAL SOCIETY.

June 10, 1840.—A notice of a mass of trap in the mountain limestone on the western extremity of Bleadon Hill, Somersetshire, and on the line of the Bristol and Exeter Railway, by the Rev. D. Williams, F.G.S.

This is the first discovery of trap *in situ* in the Mendip Hills or in Somersetshire, with the exception of the Hestercombe granite, described by Mr. Horner †, and a slate porphyry, observed by Mr. Williams, a little north of Simmon's birth, in Exmoor. The rock varies in character from a granular to a porphyritic and amygdaloidal greenstone. It occurs near a line of fault, which has brought the lias on a level with the carboniferous limestone; and when first exposed on the eastern side of the railway cutting, it appeared to be conformably interstratified with the limestone; but the cutting of the western side (the line of railway ranging north and south) has subsequently proved that the trap is clearly intrusive, intersecting at a considerable angle the limestone beds. On the east side the trap is in contact with the lias, but no change appears to have been produced in that formation, though the mountain limestone is stated to be considerably altered. The trap at the lower part presents a broad bed-like mass, but it rapidly diminishes in its upward course through the limestone thinning away entirely. Mr. Williams states, that the limestone appears to have yielded along the line of one of

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* [On the subject of this communication see L. E. and D. Phil. Mag., vol. xvii. p. 465: also present vol. pp. 132, 321, and Mr. Challis's communication in the present Number.—Edit.]

† Geol. Trans., 1st Series, vol. iii. p. 348.

the north-west joints. He acknowledges his obligation to Mr. Peniston, the resident engineer, for a correct section of the cutting.

A memoir descriptive of a "Series of Coloured Sections of the Cuttings on the Birmingham and Gloucester Railway," by Hugh Edwin Strickland, Esq., F.G.S.

The author commences by expressing his regret at the irrecoverable loss, which science has experienced, in full advantage not having been taken of the valuable geological information, which has been exposed by the railway cuttings in different parts of England during the last ten years; and he suggests the propriety of each line of railway being systematically surveyed by a competent observer, while the cuttings are in progress.

Anxious to contribute towards so desirable an end, Mr. Strickland gladly yielded to a request made to him by Captain Moorsom, the chief engineer of the Birmingham and Gloucester Railway, to undertake a geological survey of the line; and he expresses his obligations to that gentleman and to Captain J. Vetch for the valuable assistance they afforded him. The line was originally surveyed by Mr. Burr, when only the trial shafts had been sunk, and before the cuttings were commenced; but Mr. Strickland bears testimony to the accuracy of the account which Mr. Burr laid before this Society.—(Geol. Proceedings, vol. ii. p. 593; or L. & E. Phil. Mag. vol. xii. p. 573.)

The direction of the railway ranges nearly parallel to the strike of the strata, and therefore intersects only the new red sandstone and red marl, the lias, and superficial detritus.

New red sandstone and red marl.—The lowest rock exposed belongs to the new red or bunter sandstone, resting on the anticlinal axis of the Lickey, ten miles south-south-west of Birmingham, and one mile south of the termination of the altered rock, or Lickey Quartz*. The sandstone is there thick-bedded, soft, and red, and dips on the western flank about 5° west-south-west, and on the eastern 5° east-south-east. In Grovely Hill, on the north-east of the Lickey, it passes occasionally into a hard quartzose conglomerate with a calcareous paste†; and at Finstal, on the south-west of the Lickey ridge, the upper portion of the sandstone is light-coloured, and contains obscure vegetable impressions, being a prolongation of the stratum, with similar impressions, at Breakback Hill, on the west of Bromsgrove‡.

On each side of the Lickey, the sandstone is conformably overlaid by red marl, which extends on the north-east to Birmingham§, and on the south-west to Stoke Prior and the neighbourhood of Hadnor,

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* See Mr. Murchison's Silurian System, p. 492.

† Similar conglomerates occur in Worcestershire, Staffordshire, and Warwickshire.—Silur. Syst., p. 42. Geol. Trans., 2nd Series, vol. v. p. 347.

‡ Geol. Trans., 2nd Series, vol. v. p. 341; Proceedings, vol. ii. p. 564; [or L. and E. Phil. Mag., vol. xi. p. 319.—Edit.]

§ The red marl extends from Birmingham along the London railway as far as Berkswell, forming the basin, in which occurs the lias outlier of Knowle south-west of Berkswell. The true boundary of the sandstone and marl in this district has been only recently ascertained; it ranges from where the railway intersects a ridge of lias. On the north side the marl is there cut off by a fault, but on the south, at Dunhamstead, the following juncture section is exposed:—

(*a.*)	Lias clay with contorted beds of lias limestone.	
(*b.*)	White micaceous sandstone, with numerous specimens of a smooth oval bivalve	2 Feet.
(*c.*)	Lias clay	6
(*d.*)	Grey marl	35
(*e.*)	Red marl	

Dip of the beds 5° north-north-east.

In the hill south of Dunhamstead, the grey marl (*d*) abuts against the red marl (*e*) in consequence of a fault. For the next five miles the railway traverses a valley of red marl, between the escarpment of the lias and a ridge of Keuper sandstone. On the south-east of Spetchley the strike of that sandstone is altered by a fault from south by east to south-west, and a projecting angle has been produced which is intersected by the railway. This stratum is a feeble representative of the Keuper sandstone of Burg Hill, &c.*, consisting chiefly of greenish marl with thin laminæ of white sandstone, about twenty feet thick, with red marl above and below. At Norton the railway ascends the lias escarpment, and cuts through a section exactly analogous to the one given above. A mile further south the lias clay contains many calcareous concretions abounding with fossils, including *Plagiostoma giganteum*, *Modiola minima*, and a coral. At Abbot's Wood the fissile sandstone at the base of the lias is again exposed, having been brought up by a fault. At Defford and Eckington the lias clay encloses numerous specimens of *Pachyodon Listeri* (Stuchbury), or *Unio Listeri* of Sowerby, and *Ammonites Turneri*. At Bredon a higher portion of the lias series was reached, and a different suite of fossils found, the most marked being *Pleurotomaria Anglica*, *Hippopodium ponderosum*, *Gryphæa incurva*, *Nautilus striatus*, and several species of Ammonites. Between Cheltenham and Gloucester the lias has yielded great abundance of organic remains, a considerable number of which are considered to be new, and with the exception of *Hippopodium ponderosum*, *Gryphæa incurva*, and one or two others, they are distinct from the fossils of Bredon Hill; and at Hewlitt's, east of Cheltenham, the lias near the base of the marlstone presents another series of distinct fossils. The lower lias, therefore, Mr. Strickland observes, affords evidences of at least four well-marked successions of molluscous faunæ, in a vertical height of 400 or 500 feet, and unaccompanied by any change in the mineral character of the deposits.

SUPERFICIAL DETRITUS.—The author then proceeds to describe the deposits of superficial detritus, and he states, that they entirely confirm the views which he had previously entertained, respecting the distinction between the ancient terrestrial alluvia in which bones of

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Hewell Grange, nearly north, by Cofton Hacket to Northfield, and thence north-east to the south suburbs of Birmingham.

* Proceedings, vol. ii. p. 503; [or L. and E. Phil. Mag., vol. xi. p. 318. —EDIT.] Geol. Trans., 2nd Series, vol. v. p. 332.

mammalia occur, and the submarine drift which covers most parts of the island*.

He divides the detritus into fluviatile and marine; and the latter, according to its origin, into local and erratic; and this, according to its composition, into gravel with flints and without flints.

Marine erratic gravel without flints†.—Commencing his details with the Birmingham end of the line, Mr. Strickland shows, that these accumulations occur extensively on all sides of that town, and at intervals along the line of the railway till it approaches the valley of the Avon. Mammalian remains appear to be totally wanting. Chalk flints are so extremely rare in it around Birmingham as to prove that the materials were transported from the north. At Mosely it is upwards of 80 feet thick, and consists of rolled pebbles, rarely exceeding 4 inches in diameter, of various granitic and quartzose rocks and altered sandstones, imbedded in a clean ferruginous sand; and a bed of sand 30 feet thick, without pebbles, occurs in the middle of the gravel. Between Cotteridge and Wytchall is an erratic boulder, or shapeless mass of porphyritic trap, about 5 feet by 4, with the angles slightly rounded. At the Lickey, gravel analogous to that near Birmingham, but with a large proportion of slate rocks, attains, on the line of the railway, a height of 387 feet, and at the Lickey Beacon of more than 900 feet. Sugar's Brook is the next locality noticed by Mr. Strickland, but from that point no gravel occurs for sixteen miles. Near Abbot's Wood is another extensive deposit of quartzose gravel and ferruginous sand, devoid of flints and resting upon lias.

Marine erratic gravel with flints.—These accumulations commence immediately south of the Avon. The village of Bredon stands on a platform, seventy feet above the ordinary level of the Avon, composed of lias with an uneven surface, and capped with 10 to 15 feet of this gravel. It contains no mammalian remains.

Fluviatile gravel.—The only example of this drift, on the line of the railway, occupies the two opposite flanks of the Avon at Defford and Eckington, north of Bredon. At these localities the surface is a tabular platform which does not exceed forty-five feet above the Avon, including a capping of ten feet of gravel precisely similar to the flinty gravel of Bredon, but containing abundance of mammalian remains. They were chiefly found in the cutting north of Eckington, at the lower part of the deposit, and often on the surface of the lias clay; and are referrible to *Elephas primigenius, Hippopotamus major, Bos Urus,* and *Cervus giganteus*? On the north, or Defford side of the Avon, the remains of *Elephas primigenius* and *Rhinoceros trichorhinus* have been obtained. Associated with these bones are numerous freshwater shells, agreeing with those found at Cropthorne‡; the most abundant species being *Cyclas amnica* and *C. cornea.* In endeavouring to account for the presence of these re-

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* See Reports of the British Association, vol. vi., Sessional Meetings, p. 61.

† Northern drift of Mr. Murchison, Silur. Syst., p. 523.

‡ Silur. Syst. p. 555; and Proceedings, vol. ii. pp. 6 and 95; [or L. and E. Phil. Mag., vol. iv. p. 148; vol. v. p. 297.—Edit.]

mains at only one point in the line of the railway, Mr. Strickland states that he can offer no other explanation than that previously proposed by him *, namely, that after the beds of marine gravel had been deposited and laid dry by the elevation of the land, a large river or chain of lakes extended down the valley of the Avon, at a height varying from twenty to fifty feet above its present course; and that the gravel previously accumulated by marine currents was remodified by the river, and mixed up with remains of mammalia which tenanted its banks, or of mollusca which inhabited its waters.

Local gravel.—This species of detritus occurs abundantly at Cheltenham, and consists exclusively of detritus from the oolites and lias of the vicinity. No bones or terrestrial remains have been found in it; and, therefore, the author assigns to it, in the absence of other evidence, a marine origin.

Modern alluvia.—The only deposits of this nature mentioned in the paper, are the peaty accumulations on the banks of the Avon and its tributaries.

The memoir was accompanied by a copy of the Railway Section, and of the Tewkesbury branch, and the junction branch from the main line to the London and Birmingham Railway, presented by Capt. Moorsom, but coloured geologically by Mr. Strickland.

A letter addressed to Mr. Murchison by Capt. Lloyd, dated London, May 11th, 1840.

Having read in the instructions prepared by the Royal Society for the Antarctic Expedition under Capt. James Ross, that the island of Bourbon presents indications of the sea having formerly occupied a higher level than at present, and having observed similar appearances in the Mauritius, Capt. Lloyd was induced to lay the following facts before the Society.

The island of Mauritius is belted by an enormous coral reef throughout its whole circumference, except for about ten miles of the broadest and extreme southern side, or from Point Souffleur to Souillac, commonly called Port Savanne. Along that part of the island the coast is bold, and consists of a basaltic rock.

Near the Rivière des Galets, between Savanne and the Baie du Cap, the sea foams against a barrier of coral from five to fifteen feet in height, and wears it into the most fantastic shapes. At a considerable distance inland, and almost concealed by trees and shrubs, are two remarkable points or headlands of coral, from twenty to twenty-five feet above the present level of the sea. They present the same marks of abrasion as the barrier reef now undergoing the action of the waves. The Observatory, Port Louis, is built also on a stratum, ten feet above high-water mark, of very hard coral, which requires blasting. There are besides in several parts of the island, and at considerable distances inland, enormous blocks of coral surrounded with the debris of oyster and other shells and broken corals. Appended to Capt. Lloyd's communication are two letters from agents employed by him to collect information respecting inland blocks of coral. One of the letters is from Mr. Hill, surveyor

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* Reports of British Association, vol. vi. Sections, p. 64.

of roads, and contains the following data respecting two blocks near Souillac :—

	1st Block.	2nd Block.
Distance from the sea	610 feet	1356 feet.
Probable height above high water	50 —	
Length	12 —	30 —
Breadth	10 —	12 —
Height	7½ —	14 —
Girth round the largest projections	40 —	77 —

If the first of these blocks had been transported by the sea, Capt. Lloyd says, it could have attained its present position only by passing over the almost perpendicular coast.

The other letter is from Mr. Sherlock, and gives the following measurements of two blocks on the Black river :—

	Height.	Width.	Circumference.	Distance from sea.
1st block	13 feet	30 to 40 feet	121 feet	350 feet.
2nd block	10 —	25 feet	Length, 41 feet	840 —

Mr. Sherlock adds, there is no coral in the interior, except a small bed on the habitation, Le Gentele.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1841.

Chiswick.—April 1. Cloudy. 2. Fine : clear. 3. Slight haze : cloudy and fine. 4. Cloudy : slight rain. 5. Cloudy and fine. 6, 7. Very fine. 8. Fine in the morning: hail-shower at 1 P.M.: cloudy and fine at night. 9. Overcast and cold. 10—12. Cloudy and cold. 13. Cloudy : rain. 14. Cloudy : slight rain. 15. Showery. 16, 17. Fine. 18. Overcast and cold. 19. Fine. 20. Overcast. 21. Cold and dry. 22. Cloudy and cold. 23. Heavy rain. 24. Fine. 25. Very fine : slight rain : cloudy and windy at night. 26. Hazy : fine. 27. Very fine. 28. Rain. 29. Rain : very fine : clear at night. 30. Dry haze throughout the day : clear at night.

Boston.—April 1. Fine : rain early A.M. 2. Stormy : rain early A.M. 3, 4. Fine. 5. Cloudy : rain P.M. 6. Fine. 7. Cloudy : rain P.M. 8. Cloudy. 9. Fine : rain early A.M. 10. Cloudy. 11. Cloudy : rain early A.M. : hail-storm P.M. 12. Cloudy : rain early A.M. 13. Fine : rain early A.M. 14, 15. Fine : rain P.M. 16. Fine : ice this morning. 17. Fine. 18. Cloudy. 19. Fine. 20. Fine : hail and rain P.M. 21. Cloudy : rain P.M. 22. Cloudy : rain early A.M. 23. Rain. 24. Fine. 25. Fine : rain early A.M. 26. Cloudy. 27, 28. Fine. 29. Cloudy. 30. Fine.

Applegarth Manse, Dumfries-shire.—April 1. Fine A.M : rain P.M. 2. Fine : showers P.M. 3. Fine : one shower. 4. Fine A.M. : rain P.M. 5. Fine : slight shower. 6. Fine and fair all day. 7. Rain P.M. 8. Fair A.M. : shower P.M. 9. Fine and fair all day. 10, 11. Occasional showers. 12. Fine and fair. 13. Wet P.M. 14. Showery P.M. 15. Rain and hail. 16. Rain, sleet and hail. 17—20. Showers. 21. Fair and cold. 22, 23. Frosty morning : fine. 24. Very wet P.M. 25. Rainy forenoon. 26. Wet all day. 27. Rain A.M. : cleared up. 28. Beautiful day : thunder and rain. 29, 30. Fine and fair.

Sun shone out 27 days. Rain fell 20 days. Thunder 2 days. Frosty mornings 2. Hail 2 days.

Wind north ½ day. North-east 3 days. East 2 days. South-east 4 days. South-south-east 2 days. South 2 days. South-south-west ½ day. South-west 7 days. West-south-west 1 day. West 6 days. North-west 1 day. North-north-west 1 day.

Calm 6 days. Moderate 14 days. Brisk 4 days. Strong breeze 4 days. Boisterous 2 days.

Mean temperature of the month	44°·40
Mean temperature of April 1840	48 ·05
Mean temperature of spring-water	51 ·00

Meteorological Observations made at the Apartments of the Royal Society 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, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. April.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·642	29·632	29·596	29·15	29·51	29·40	46·3	54·7	43·7	52	41	46	51	38	W.	W.	N.	SW.	·061	·05	·08	...	45
2.	29·662	29·727	29·600	29·12	29·45	29·60	47·7	51·7	44·6	56	26	47	48½	36½	NW.	NW.	NW.	W.	·058	...	·04	...	46
3.	29·728	29·707	29·640	29·25	29·49	29·47	45·0	53·6	38·2	57	26	43·5	48½	35½	SW.	NW.	calm	SW.	...	...	...	...	44
4.	29·676	29·642	29·467	29·14	29·35	29·23	43·7	53·6	38·3	55	39	46	51	36½	S.	SW.	calm	WSW.	...	·11	...	...	41
5.	29·404	29·436	29·371	28·94	29·28	29·55	47·0	56·0	43·0	56	35	47·5	51	40½	SE.	S.	calm	E.	·102	...	...	...	43
○ 6.	29·710	29·745	29·661	29·28	29·64	29·63	46·4	54·2	43·2	55	31	46·5	53½	35	N.	NE.	calm	E.	...	·02	·08	...	43
7.	29·806	29·781	29·777	29·40	29·60	29·56	43·3	53·8	40·3	52	37	38	49½	39½	E.	SW.	N.	S.	·036	·03	...	0·11	40
8.	29·806	29·779	29·775	29·29	29·45	29·68	47·0	54·0	42·4	53	34	45	52	38½	NW.	W.	W.	NW.	·033	...	·07	...	45
9.	29·910	29·988	29·820	29·43	29·83	29·93	48·0	52·5	41·7	55	37	47	51½	32	NW.	N.	calm	NNW.	·125	·01	·03	...	44
10.	30·082	30·020	29·958	29·62	29·98	29·97	44·7	53·7	40·3	49	31	46	49	37	NW.	E.	calm	NE.	...	...	...	...	41
11.	29·954	29·939	29·923	29·65	30·00	30·00	40·8	46·7	38·6	49	32	42	45½	33	N.	NE.	calm	NE.	...	·01	·07	...	38
12.	29·912	29·962	29·907	29·54	29·90	29·80	41·2	47·2	36·7	45	27	43	48½	32½	NNW.	NE.	calm	N.SSW.	·025	...	·05	...	35
☾ 13.	30·100	30·130	30·077	29·63	29·78	29·83	45·3	46·6	37·7	56	42	46	52	38½	S.	SW.	calm	W.	...	·08	·02	...	38
14.	30·110	30·088	29·983	29·58	29·76	29·78	49·8	53·0	45·0	57	32	49	51	42	S.	SW.	calm	W.	·080	·01	...	0·70	41
15.	29·916	29·899	29·761	29·31	29·65	29·55	48·7	56·7	42·4	58	26	47	47	35	S.	NW.	calm	SW.	...	·05	·10	...	43
16.	29·742	29·762	29·714	29·28	29·48	29·56	45·7	55·3	37·7	57	31	46·5	49	37½	SSW.	NW.	calm	W.	·063	...	·10	...	41
17.	29·914	29·946	29·874	29·45	29·75	29·63	46·4	54·6	42·4	60	39	48	53	33	W.	NE.	calm	SW.	...	...	...	...	41
18.	29·972	29·975	29·924	29·37	29·67	29·67	49·4	54·6	42·8	61	41	51	50½	40½	SW.	W.	calm	SW.	...	·14	...	...	43
19.	29·908	29·895	29·881	29·30	29·60	29·70	48·7	59·2	44·8	58	32	48·5	51	39½	NW.	W.	NW.	W.	·166	...	...	...	42
20.	29·800	29·803	29·682	29·27	29·54	29·70	46·8	56·8	42·2	56	36	48	49	39	W.	W.	calm	W.	...	...	...	...	39
● 21.	29·852	29·904	29·810	29·46	29·92	30·03	47·6	52·8	41·7	53	40	46·5	47½	37	NNE.	NE.	calm	NE.	...	...	·36	0·12	45
22.	29·882	29·866	29·734	29·48	29·94	29·72	45·3	53·2	41·0	53	41	45	49	28½	NE.	NE.	E.	SW.	...	·32	·07	...	39
23.	29·424	29·645	29·450	29·10	29·53	29·41	43·7	51·6	42·3	48	28	44	51	31	NW.	NW.	E.	SW.	·500	·52	·09	...	44
24.	29·712	29·677	29·528	29·20	29·37	29·18	48·5	55·0	40·2	56	39	49	51½	33½	S var.	SW.	calm	SE.	·333	·15	·32	...	43
25.	29·688	29·846	29·650	29·13	29·28	29·20	50·5	65·7	43·9	59	52	50	54	38	S.	S.	W.	SSE.	·072	...	·21	...	46
26.	30·040	30·100	29·980	29·39	29·53	29·74	57·7	59·3	50·2	66	55	59	51	45½	S.	SW.	SW.	S.	...	...	...	...	51
27.	30·140	30·103	30·078	29·47	29·80	30·00	63·3	72·2	56·5	75	46	65·5	55	47½	S.	SW.	calm	SSE.	...	...	...	...	56
☽ 28.	30·200	30·132	30·099	29·50	30·10	30·10	63·3	72·4	57·0	76	45	66	61	41½	N.	NE.	calm	SE.	·022	·07	...	1·00	58
29.	30·152	30·141	30·098	29·56	30·06	30·18	52·3	71·6	50·6	71	42	53	59	45½	N.	NE.	calm	SE.	·072	·01	...	...	55
30.	30·226	30·171	30·091	29·68	30·17	30·03	58·4	69·2	46·6	68	41	53·5	64	41	NE.	NE.	calm	SE.	...	...	...	...	47
Mean.	29·869	29·881	29·796	29·36	29·681	29·694	48·4	56·4	43·2	57·40	36·79	48·4	51·5	37·6					Sum. 1·748	1·58	1·69	1·93	Mean. 44

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE,
SUPPLEMENT TO VOL. XVIII. THIRD SERIES.

LXXIX. *Examination of a Fourth Experiment adduced by* Prof. Faraday *in support of* M. de la Rive's *Theory, and regarded by Dr.* Fusinieri *to be demonstrative. By Dr.* STEPHEN MARIANINI, *Acting Professor of Particular and Experimental Physics in the University of Modena, &c.*

[Continued from p. 202.]

Second Article. Examination of the Observations recently made by Prof. de la Rive *in support of his Theory, and which Dr.* Fusinieri *has considered sufficient to answer the preceding Objections*[*].

X. DR. AMBROGIO FUSINIERI seems to blame me for not having alluded, in my fourth Memoir[†], to the interesting work of M. de la Rive, entitled, "Inquiry into the Cause of Voltaic Electricity[‡]." I did not instantly allege in my excuse the having written and sent the said memoir to the Secretary of the Italian Society of Sciences, some time previous to my becoming acquainted with the said work, which had been kindly presented to me only in the November of 1836, by my illustrious colleague, Prof. de la Rive, this circumstance having little interest for the scientific, for whom I intend to write, and still less for science; but I applied myself instead to study the work itself, as soon as I was able to do so.

Whoever does not know this production of M. de la Rive, no doubt, on hearing how Signor Fusinieri speaks of it, would believe that in the same manner in which, in my second memoir, I had examined all the arguments and principal experiments brought forward by the Professor of Geneva in support of his theory, and had shown the insufficiency of them with other experiments; so also M. de la Rive might undertake to examine my arguments, and might show the erroneousness of them; but what would be his surprise, when setting himself to read this work (without doubt having other claims to interest),

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[*] *Annali de Scienze del Regno Lombardo-Veneto, Bimestre* iv., 1837, p. 192.

[†] Ibid.

[‡] *Recherches, &c.*

he should see that the same facts are reproduced which were already published by him in 1828, when he wished to found his theory; and of the many arguments and experiments brought forward by me, he only speaks of two or three secondary in importance, and not with the profundity which one might expect from so distinguished a physicist!

In the first part of my said memoir I treat of the influence which voltaic currents exercise in changing the relative electromotive property of the metals, and of that which is exercised upon them by the liquid conductors, in which are immersed the metals themselves; and I established some facts which ought to serve as the *rationale* of some experiments produced by M. de la Rive as inexplicable except upon his theory. But of these we will not speak here. Nor let any one interpret this silence into denial or disapprobation, since the same M. de la Rive, in his historical sketch of the principal facts discovered in electricity, makes mention of all these, and not only without starting a doubt of them, but also with expressions very flattering to me*.

In the second part of that memoir I undertake to show the insufficiency of the theory of M. de la Rive to explain the phænomena of the electromotors, as well simple as complex.

Treating of the simple electromotors, and of the case in which the two plates are immersed in the same liquid, M. de la Rive adduced in support of his theory that an electric current is produced when there are immersed in one liquid two portions of the same metal susceptible of being attacked by it. And I observed that if these two portions have some heterogeneity, the fact accords with the theory of Volta; if not, there is no theory, either chemical or physical, which can explain it. (Memoir above mentioned, § XXIII.)

M. de la Rive observed that if a voltaic pair of copper and tin is immersed in an acid or saline solution, the tin is positive; and if it is immersed in ammonia, the copper is positive, because more acted upon by the liquid than the tin. And I showed that this second part of the experiment is not true, except after the ammonia may have altered, in a contrary sense, the electrotism of the two metals, and that, in consequence, it contradicts the theory of M. de la Rive, instead of supporting it. The same observations I made upon the similar experiment of M. de la Rive, with copper and iron. (§ XXIV.)

Another fact which M. de la Rive adduced was, that on immersing iron and lead in concentrated nitric acid, the iron in the first moment is negative, "*because* (it was said) *there*

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* *Esquisse historique des principales découvertes faites dans l'électricité depuis quelques années.* Par M. Auguste de la Rive. Genève, 1833.

was not yet any chemical action;" but if one waits until the chemical action shall commence, or if the part of the plate of iron immersed in the liquid is exposed for a moment to the air, which quickly determines the action, that iron itself becomes positive. And I in the first place inquired whence might arise the negative electrization in the iron, and consequently positive in the lead, in the first instant that the copper is immersed, if it had yet no chemical action. And how stands now the theory of M. de la Rive? In the second place I demonstrated that the nitric acid increases the electrotism both in the iron and the lead, but more in the latter than in the former. I showed, in the third place, that it was an error to believe that causing the iron to be exposed to the air might be the cause by which it was electrified positively, on again putting it into the acid, because the cause of this positive electrization of the iron was the alteration which the lead suffered in the acid in which it was left immersed whilst the iron was kept in the air. And I proved, lastly, that to explain those experiments by the chemical theory, it is necessary to suppose now that the action of the acid renders the iron positive with respect to the lead, now that it renders it negative; now we must suppose that the action of the acid may be less forcible in the first instants upon the iron than upon the lead; now we must suppose the contrary. (§ XXVII.)

Another experiment quoted by M. de la Rive was, that two plates, the one of gold, the other of the purest platina, being fixed to the extremity of the galvanometric wire, and immersed in the pure nitric acid, a very sensible current took place when a drop of hydrochloric acid was added to the said liquid. And I demonstrated by ten experiments, first, that the nitric acid, mixed with very little hydrochloric acid, increases the relative electromotive faculty both in the gold and in the platina, but more in the latter than in the former; secondly, that such phænomena are not derived from electricity immediately developed by the chemical action; thirdly, that it is impossible to explain all the phænomena which are observed in these experiments by the new theory. (§ XXV.)

And to all these facts stated in the first section of the second part of my before-mentioned memoir, and which contradict the chemical theory of electromotors, what arguments does M. de la Rive oppose? Perhaps Signor Fusinieri believes that the silence of the learned Genevan is sufficient confutation, or that he may have replied sufficiently in saying that the electricity developed by the chemical action *is not always in proportion to the action itself.* I should sooner have expected that Signor Fusinieri might have seen in this proposition a proof of the persuasion which M. de la Rive begins to entertain that his theory is not sufficient to explain the phænomena of the electromotors, if my experiments are true; though, if Dr. Fusinieri would do them the honour to repeat them, I doubt not that he would quickly be persuaded that that modification is not sufficient for the theory; but it would be necessary to add, that sometimes the electrization is in an inverse ratio to the force of the chemical action, that is, to the energy of the cause from which it is pretended to be derived. But we are here at a point of my memoir which M. de la Rive discusses in his *Recherches*, p. 125.

XI. In describing the experiment of which I have now spoken, M. de la Rive remarked that these plates of gold and pure platina, immersed in the pure nitric acid, produce no current whatever: and I said I had obtained the same phænomena with the nitric acid as with the acid itself mixed with a little hydrochloric acid, except that the galvanometric deviations were somewhat less. Now, however, M. de la Rive repeats the same thing; that is, the not having obtained a similar current in making use of the pure nitric acid; and Signor Fusinieri rather inclines to believe in the result of De la Rive, having himself made the experiment with another scientific man; and he thinks the observation just, which the same Genevan physicist then adds, namely, that as I experimented in Venice, it was a matter of difficulty that the nitric acid should be entirely free from hydrochloric. And here it vexes me that I must point out to Signor Fusinieri that this difficulty also presented itself to me, as is seen in the same page in which I speak of this experiment; and not only did I see the objection, but also removed it, by proving that by leaving the nitric acid exposed for a long time to the Venetian air, instead of acquiring to a much greater degree the aptitude to cause that current, it went on losing it; that is, on the contrary, it went on losing the power of altering to positive the relative electromotive faculty of the platina and the gold so far as to acquire the opposite property. (§ XXVI.)

But there is yet more. Is it then true that there is that *difference in the facts*, which Dr. Fusinieri remarks in this proposition, namely, that De la Rive may not have seen in immersing the gold and the platina in the nitric acid, the small electric currents seen by me? And do we not read at the eleventh page of his *Recherches*, that in the act of immersing that pair in the nitric acid there are seen slight galvanometric deviations? "Du moins (these are his own words) le léger courant que l' on aperçoit au premier moment de l'immersion, n'est pas plus fort que celui qui a lieu quand on se sert de deux lames homogènes de platine, et il est dû aux impuretés dont il est impossible de préserver complètement les surfaces métalliques; aussi disparait-il très-promptement, et quoique les deux métaux restent dans le liquide, on n'en voit bientôt plus aucune trace."

Then also the learned Genevan sees the voltaic current on immersing in the pure nitric acid the gold and the platina fixed to the ends of the galvanometric wire: he observes that the current itself is not stronger than that produced by the two homogeneous plates of platina; and he attributes it to the impurities, from which he says it is impossible completely to preserve metallic surfaces. And in proof that this and no other is the cause of that current, M. de la Rive adduces the fact that it disappears immediately, and that however long the two metals may remain in the liquid, in a few moments there is no more trace of it.

The difference, then, is not in the fact, but in the manner of studying and interpreting it. M. de la Rive not being able to persuade himself that there can be an electric current without chemical action, and insisting that neither the gold nor the platina can be injured by the nitric acid, supposes it impossible entirely to purify the surfaces of the two metals. Instead of this, I imagine that the current is produced because the platina touches the gold metallically, by means of the galvanometric wire, and is immersed with it in a liquid. If the current is weak, it is because the metals are sufficiently near in the voltaic scale; if it vanishes, or rather, if it becomes sensibly weaker in a short time, it is because the current itself increases the electromotive faculty of the gold, and diminishes that of the platina, so that in a short time the two metals become almost homogeneous in the voltaic sense of that term.

M. de la Rive attributes the vanishing of the current to the disappearance of the impurities from the surfaces of the two metals. But I shall here ask if it is desired that the nitric acid may destroy those impurities by means of the little current which is developed, or that it may be destroyed independently of it. In the first case, I shall inquire how it happens that if the gold is immersed in the acid a moment before the platina, the galvanometric deviation is extremely weak, and sometimes even nothing. In the second case, the current which is observed when the two plates are allowed to remain immersed in the acid for some time before putting them in communication with each other by means of the wire of the galvanometer, will have to be explained.

There is still a remark to make respecting the disappearance of the current, which M. de la Rive declares to happen a few moments after the circuit is complete; so that to see that current disappear, two conditions are necessary. The first is, that the experiments may be made with a galvanometer not very sensitive, that is, such as may not indicate the current itself, except by one or two degrees of deviation. The second is, that the two metals may be immersed in the acid whilst they are still in communication with each other, exactly as M. de la Rive conducts the experiment. But if use is made of a galvanometer rather sensitive, such, for example, as may show the current by a deviation of ten or more degrees, it is very true that in a few moments the force of the current is much weakened; but it does not fail to be visible with some degree of deviation, even after more than a minute; so that the deviation is strong at first, but proceeds always more slowly in the times succeeding to the first immersion of the plates.

But experimenting with the galvanometer of moderate delicacy, if the plates are kept some little time in the liquid before completing the circuit with the galvanometric wire, it is seen that the current does not then cease so quickly; and it is stronger if more time is allowed to pass before closing the circuit itself.

The following are some experiments instituted already many times here in Modena, and with the galvanometer sufficiently sensitive, above mentioned.

A small plate of pure gold, and one of pure platina, being immersed for an instant in nitric acid, deprived entirely of hydrochloric acid whilst they remained in contact by means of the wire of the galvanometer, produced a deviation of 4°.

Two other similar plates being left immersed for half a minute, and then the circuit being closed for a moment, the deviation was of 10°.

The experiment being repeated with two other plates, and the circuit being completed for a minute after they were immersed, 25°.

Being left immersed for five seconds before completing the circuit, 75°.

The two small plates were kept at the distance of a centimeter from each other, and were immersed together in the liquid for a square centimeter of their surfaces.

The nitric acid increases the relative electromotive faculty of both those metals, but much more that of the platina than that of the gold, and thence may be drawn easily the explanation of these and of various other phænomena which are observed with the voltaic pair of platina and gold, when nitric acid is made use of for a moist conductor. (See my above-mentioned memoir at §§ XXV. XXVI.)

XII. I must not abandon this argument without making some observations respecting a singular proposition which is read towards the end of the tenth page of M. de la Rive's work, expressed in the following terms: "Lorsque le liquide dans lequel plongent les deux élémens du couple, est le même, il n'existe pas un seul cas dans lequel on ait vu le métal le moins attaqué être positif par rapport à l'autre."

Yet in the actual state of the science it is very easy to find many cases in which it is not yet known which element of the pair may be the most acted upon, although it may be undoubted that the pair itself produces a voltaic current. Which, for example, of the following substances—silver, gold, platina, carburet of iron, and peroxide of manganese—is most acted upon by distilled water? Yet, whatever pair may be made with the said metals, there is a current when it is immersed in the said liquid. If it be shown that charcoal, well freed from hydrogen and extinguished for some time, may be more acted upon by distilled water than the silver, gold and platina, by which it may have to electrify itself, how does it happen when positively voltaically united with those metals and immersed in the same liquid? If it be shown that charcoal itself, when it is oxidated, may be less acted upon by distilled water than the noble metals, yet made into a pair with them it acquires negative electricity. If it be shown that tin may be more acted upon than copper, nickel, bismuth, cobalt, and iron, by the said liquid, yet tin becomes positive when immersed in such liquid voltaically united with any one of the said metals. Zinc united with cadmium is positive even in distilled water. Is it known which of the two is most acted upon by that fluid?

Here, then, are at least twenty cases, all observed by me separately, in which it is doubtful upon which of the two elements of the pair the liquid exercises the strongest chemical action; and it is very certain that one is electrified positively and the other negatively. And this number of doubtful cases is doubled when it is observed, that when making use of alcohol instead of distilled water, the effects are different, although the currents may in this case be weaker. The description of some experiments made with the said pairs may not be useless.

A plate of platina, and one of silver, fixed to the ends of the wire of the galvanometer, were immersed at the same time in rectified alcohol, at thirty-three degrees of the scale of Bauiné, and there was a deviation of two degrees on the part of the platina. Both the plates touched the liquid with a surface of four square centimeters.

A plate of gold with a surface of eight square centimeters, coupled with one of platina having a surface of one square decimeter, immersed in the said liquid, produced the deviation of a degree and a half, which indicated that the gold was positive in contact with the platina.

And thus, with any other of the above-mentioned pairs whatever, a current was obtained, which indicated that one of the metals was positive and the other negative.

XIII. But let us suppose that all these doubtful cases may not argue against the chemical theory; let us admit that the metal which shows itself positive may be also the most acted upon; and that if this is not yet demonstrated, it will be proved one day. What must we say of so many other cases, in which it is certain which of the two metals is the most acted upon, and equally certain that it is not electrified positively, as the new theory would assert?

Copper united with iron and immersed in ammonia is electrified negatively in the first moment of immersion. The same copper is charged also with negative electricity when united with tin and with lead, the pair being immersed in the same liquid; yet the copper is more acted upon by the ammonia than are the other two metals.

In the nitric and sulphuric acids, diluted or concentrated, are not copper and iron more acted upon than tin and lead? Yet copper as well as iron is electrified negatively when it is immersed in the said liquid, united with tin or lead.

Sulphuric acid, diluted by two hundred parts of water, acts less upon cobalt than the copper, polished antimony, and antimony slightly oxidated, which being immersed in the said acid, promotes effervescence. But the pairs, cobalt and copper, cobalt and antimony, immersed in the said acid, show the cobalt always positive.

Polished antimony, cobalt, bismuth, nickel, tin, and lead, are all less acted upon by acetic acid than copper is; yet they are all positive when, in voltaic association with copper, they are immersed in the said acid.

And here are twenty cases (and it is far from difficult to find others) in which the two elements of the pair are immersed in the same liquid, and the metal the least acted upon is positive with respect to the other. And it may be said that the experiments of the Genevan physicist must have been restricted within very narrow limits, if not one of these cases came under his notice; since, if the above facts are true, of which whoever will repeat them may assure himself, it appears to me impossible longer to admit, with M. de la Rive, that "le sens du courant est toujours d'accord avec la théorie chimique, et que le métal sur lequel l'action chimique du liquide est la plus vive est toujours positif par rapport à l'autre." (From the work above cited, p. 14.)

I should believe, moreover, that one only of the above-mentioned facts would suffice to justify the proposition which may be read at the end of § XXVII. of my memoir already quoted, "*that the new theory is not sufficient to explain all the phænomena presented by simple electromotors, when the two plates of which they are formed are immersed in the same liquid.*"

XIV. Then when the two elements of the voltaic pair are immersed in different liquids, the cases are very many which contradict the new theory, and as elsewhere I have observed, they may be multiplied almost at the will of the experimenter. These were considered by M. de la Rive as apparent anomalies, and to explain them, I supposed that the two electric principles tend to reunite themselves immediately in both liquids, that such reunion is more easy in the liquid which gives a positive action, and therefore the metal there immersed is negative.

Concerning this hypothetical explanation, after having pointed out in § XXX. of my memoir three things to me incomprehensible in it, I demonstrated, with the assistance of experiment, that admitting those immediate reunions of the electric principles, very many facts are found in contradiction to the theory. And I concluded it "*a thing not easy to be admitted that nature availed herself of those immediate recompositions of the two electric principles, except in those experiments which do not in any manner square with the new theory.*" And I terminated that section with some experiments derived from two then recent observations of Signor Becquerel, from which was inferred clearly "*that in the voltaic pair is a cause which gives origin to electricity which cannot be confounded with the chemical action, and which is more powerful than it.*"

Now I would ask of Dr. Fusinieri what reply can be found in the work of M. de la Rive to these objections? Does he think it may suffice to find the same things (confuted by me) repeated in it, and without any notice whatever of the confutation? The silence of M. de la Rive would suffice certainly to reply where objections evidently erroneous were treated of; but where I bring forward experiments easy to be repeated, why had not Dr. Fusinieri, acute observer as he is, the curiosity to see them before pronouncing a judgment so disadvantageous with respect to them?

XV. Let us see, finally, what may be replied to the arguments with which I proved that the Delarivian theory was not sufficient to explain the phænomena of the complex electromotors.

The chemical theory being shown insufficient to explain the phænomena of the simple electromotors, the insufficiency as to the explanation of the phænomena of the complex electromotors was naturally deduced as a corollary from it; and I opposed besides the argument of the invariability of the tension, whatever may be the liquid in which the voltaic pairs are immersed, when neither their number nor their quality vary. And to anticipate the reply which would perhaps be made to that argument, that is, that although the different fluids may not exert equal chemical actions upon the voltaic pairs, yet the difference of the actions exerted upon the two elements of the pair being equal, the tension also must be invariable; I described an experiment made with two electromotors of eight pairs, which had equal tension, although the differences of the chemical actions exercised upon the two elements of each pair in each of the electromotors were any thing but equal; for in one all the plates were immersed in distilled water, and in the other the plates of zinc were immersed in distilled water, and those of copper in dilute sulphuric acid. (§ XXXIV.)

I undertook to examine also the truly ingenious method with which M. de la Rive attempted to explain the effects of the piles dependent upon the number of the pairs. To explain such effects, M. de la Rive supposes that the tendency which the two electric principles have to neutralize each other, acts so, that when the poles are not united by any conductor, it is the pile itself which serves them as a conductor, in producing the effect of one meeting the other. Thus the force of the tension will depend upon the greater or less facility which the voltaic apparatus shall present to the transmission of the two fluids. And since it is known that the greater the number of the plates to pass through, the more difficult is the transmission, so the electricity accumulated at one pole will not affect the condenser, except in so far as the pile itself shall be composed of such a number of pairs that the resistance opposed by the apparatus to the reunion of the two fluids shall be sufficiently great for the tension to become sensible.

And examining in the abstract this tendency, supposed by M. de la Rive, in the two electric fluids, to run to neutralize each other, even by the way which the electromotor itself offers to them, I permitted myself to suggest the following dilemma. Either this tendency exists also in the simple electromotors, that is formed by one pair alone, or it exists only in the complex electromotors. If it be admitted that such tendency exists even in one pair alone, it is not intelligible how it happens that the two electricities do not avoid passing the damp conductor, the metallic way, which is so much more *conductive*, being open to them. If, on the other hand, it be admitted that that tendency exists solely in the complex electromotor, then it is not intelligible how such a property arises in it, if the elements of which it is formed (which are also so many electromotors) are all without it. And to him who might have objected to such an argument, that the greater part of the two electric principles takes the metallic way to go to neutralize itself, and that adding pairs to pairs the tension increases, because the quantity which can pass through the electromotor is less;—to him who, I say, might have thus objected, I recalled the fact that the alternations of moist and metallic conductors diminish certainly the quantity of electricity which in a given time passes through the voltaic apparatus, but do not alter the tension.

I asked, in the second place, why the least indication may not be had of such currents in a direction contrary to the usual one. And finally, I found it difficult to be admitted that the two electric principles tend to retrocede in the pile in order to neutralize each other, whilst the virtue of the pile consists, on the contrary, in the tendency to *accumulate* one of such principles at the positive pole, and the other at the negative. (§ XXXV.)

The examination instituted by me respecting this manner of explaining the effects of the pile was not limited to the preceding abstract considerations, but was pursued also with experiments. If the tension of a pile is, as M. de la Rive says, relative to the greater or less difficulty which the pile itself opposes to the passage of the electricity, it will be sufficient, I said, to render the passage of the electric through the electromotor more difficult, to see the tension at the poles augmented; and therefore, after having watched the tension of a *couronne de tasses*, I disposed the pairs in other cups, so much more ample than the first, that the liquid stratum (which was, as in the first apparatus, rain water) interposed between each pair was about six times greater; and although here the resistance might be much greater which the electricity must encounter in moving itself in the apparatus, yet the tension was found to be not at all increased.

In the work of M. de la Rive no reply is given either to the experiments or to the reasonings here above-mentioned, with which I intended to prove the insufficiency of his theory in explaining the phænomena of the complex electromotors.

But behold us finally at some of my experiments, which the learned Genevan undertakes to examine.

Such experiments are only variations of the last one above recorded. In order to render the transmission of the electric fluid through the electromotor more difficult, I put between each two pairs several inactive arcs, that is, formed of one sole copper wire, and the tension was equal to that which was observed when the electromotor was arranged as usual. Sometimes in such experiments the transmissiveness of the apparatus becomes so much enfeebled, that there is no longer an indication of a current even *to* the galvanometer, as, for example, when I introduced between the three active pairs three hundred and ten inactive ones. Yet, notwithstanding, I found no difference in the tension of those three pairs from when they were arranged as usual, and the current excited by them did not pass through the water of three hundred other glasses, and the metallic arcs which put them in communication with each other. And since M. de la Rive said that the decomposing power of the electromotor apparatus must vary according to the relation which exists between the conductivity of the liquid which connects the two poles and the apparatus itself, I therefore brought forward experiments which showed that not to be true, because every time that I rendered the passage of the current sensibly more difficult, by adding now twenty, now forty, now a hundred inactive pairs, the decomposition taking place at the *poles* became always slackened. (§ XXXVI. and XXXVII.) And see how M. de la Rive discourses about these experiments at pages 151 and 152 of the work quoted.

"La principale objection du savant italien a été dirigée contre le principe que j'avais admis, savoir que les deux fluides électriques accumulés aux deux pôles de la pile peuvent se neutraliser directement par l'intermédiaire de la pile elle-même qui lui sert de conducteur. D'après ce principe, dit il, si l'on diminue la conductibilité de la pile on doit augmenter la tension de ses deux pôles; or, on ne produit pas ce dernier effet en interposant dans le liquide qui sépare les couples, un plus ou moins grand nombre de diaphragmes de cuivre, interposition, qui cependant doit diminuer la conductibilité de la pile. Il y a plus; cette interposition non seulement n'augmente pas la tension, mais elle diminue même le pouvoir chimique de la pile dans la décomposition de l'eau; cependant lorsque les pôles sont réunis par un conducteur imparfait, s'il est vrai que la proportion d'électricité qui passe par ce conducteur et par la pile dépende de leur conductibilité rélative, il doit en passer d'autant plus à travers le conducteur que la pile conduit plus mal. A l'appuis de ces observations M. Marianini cite plusieurs experiences."

"Je suis tout-à-fait d'accord avec M. Marianini sur les consequences qu'il tire du principe que j'ai admis, mais je diffère sur l'application qu'il en fait; je reconnais avec lui que tout ce qui diminue la conductibilité de la pile, doit augmenter la tension de ses pôles pourvu qu'on n'altère en rien la quantité d'électricitè développée par chacun de ses couples; j'ai montré par des faits que c'était une condition indispensable. Mais la manière dont M. Marianini diminue la conductibilité de la pile, rentre-t-elle bien dans ce cas? Non, car le zinc et le cuivre entre lesquels il place les diaphragmes de cuivre ou de tout autre métal, n'étant plus dans les mêmes conditions que le zinc et le cuivre des autres couples, il est facile de comprendre que l'électricité positive du premier et la négative amenée par le second, se réunissent en beaucoup moins grande proportion à cause de la diminution de conductibilité qui résulte pour le liquide qui les sépare, de l'interposition des diaphragmes. Dès lors d'après la théorie que nous avons donnée, l'électricité libre de tous les autres couples diminue dans le même rapport que celle du couple que nous venons de considérer, de sorte que si d'une part les deux principes électriques accumulés aux deux pôles ont moins de facilité à se réunir d'autre part ils sont développées en moindre quantité. On conçoit que lorsqu'il s'agit de la tension, cas dans lequel l'élément du temps n'entre pour rien, puisque le condensateur reste en contact avec le pôle aussi long temps qu'on le veut, les deux effets que nous venons de signaler puissent se compenser: mais il ne peut en être de même pour les décompositions opérées par le courant et en général pour tous les effets dynamiques, car il n'y a pas de temps suffisant pour l'accumulation des deux principes électriques, et tout ce qui diminue la quantité de l'electricité libre dégagée en un temps donné par chaque couple, et par conséquent aux deux pôles, doit diminuer l'intensité des effets produits par la circulation de cette électricité."

M. de la Rive agrees, therefore, with me, that if the principle admitted by him be true, all that which diminishes the conductivity of the pile must augment the tension of the poles; but he suggests further, that what diminishes the conductivity of the pile may in no degree alter the quantity of electricity developed by each one of its pairs.

We will observe here directly, that before these *instructions* of M. de la Rive, the quantity of electricity developed by each couple was never said, that I know, to influence the tension.

Every one knows that an electromotor of ten pairs has always the same tension, whether the plates are large or small, or even some small and others large. And it is known likewise, that the quantity of electricity developed varies much with the variation in the dimensions of *all* the pairs, or even only of *some*; the galvanometers giving the most certain proofs of it.

But granted even that this might be, is it then true that the method by which I diminished the conductivity of the pile may not concern this case, that is, alter the quantity of electricity developed by each pair? M. de la Rive seems to say, that those pairs between the copper and the zinc, of which I placed metallic diaphragms, are not in the same condition with the other pairs. But the truth is, that it is not between the copper and the zinc of one pair that I placed the diaphragms; but between the zinc of one pair and the copper of another I put some inactive pairs, that is, some arcs, formed of copper wire, which were immersed in cups containing the liquid conductor, as those which served for the active pairs. And this interpolation I did not make merely between some pairs; but in some experiments those arcs were interpolated between every one of them; so that all came to be in the same condition. If ever M. de la Rive should say that those pairs so interpolated are not in the same condition as those of another electromotor, where there may not be that interpolation of inactive pairs, and that, therefore, each pair of the interpolated electromotor developes less electricity than each one of the other, I would ask, *whence* the electricity is developed according to the new theory? Is it not perhaps by the chemical action of the liquid upon the metal? and ought it not to be proportional to the difference of the actions exerted upon the zinc and upon the copper? Now, how do those inactive arcs interrupt the action of the liquid upon the elements of the pairs; and then how do they alter the quantity of electricity developed?

And if yet it should be pretended that the presence of that inactive copper wire in the glass, where the zinc or the copper of a couple was immersed, might influence the action exerted by the liquid upon the zinc, or upon the copper of the same pairs; what influence will the copper wires placed in the other successive cups exert? The experiment shows that the tension is always the same, when placing one only of those inactive arcs between the pairs of one pile, as when placing six, eight, ten, or any other number whatever between them.

And these observations, made relative to tension, apply also, with the proper difference, to the decomposing power, which always diminishes, when, the other things being placed similarly, the conductivity of the pile (whatever may be the method) is diminished.

But wishing also to concede that those diaphragms or interpolated metallic arcs impart to the pairs the condition that M. de la Rive desires, how shall we explain the non-alteration of the tension, when the electromotor is rendered less conductive by disposing the pairs in much larger cups, by which the liquid strata, through which the electric fluid must pass, are more extended? This is the first and principal experiment which I oppose to M. de la Rive, regarding his manner of explaining the effects of tension and of decomposition, and of which he does not make mention.

I said that this was the *principal* of such experiments, because the insertion of liquid strata of great extent between the pairs occurring to me as difficult, I have applied to it again with series of metallic homogeneous arcs, knowing the difficulty which the electric current encounters in passing through alternate liquid and metallic conductors. Now, I have repeated that principal experiment also on a large scale, that is, with two electromotors; the one with small troughs, in which the plates were so disposed, that between the copper of one pair and the zinc of the next there was only a stratum of water of the size of one millimeter; and the other with great receivers, so that the stratum of water, which divided one pair from the other, was half a meter. The number of the pairs being equal in both apparatus, the tension was also equal in both.

I have tried also to oblige the current to pass through many cups of water connected by means of little siphons filled with the same liquid; but I did not succeed by this in obtaining increased tension.

Finally, in order not to vary in any way the conditions of the apparatus with respect to the development of the electricity according to the electro-chemical theory, I prepared a pile of three pairs, in one of which the plate of zinc communicated with that of copper by a small metallic wire a thousand meters in length, and in each of the other two the wire which connected the copper and the zinc was five hundred meters in length. The apparatus was arranged so that that communication could be easily taken away, and one substituted for it, not longer than four or five millimeters. Now, when the metallic communications between the zinc and the copper were formed of short wires, the deviation which the pile produced in the galvanometer was twenty degrees; and when the long were substituted for the short wires, it was only fourteen; yet the tension was always the same in both cases.

For if M. de la Rive, in all these experiments, found the conditions always varied in order that the chemical action upon each couple might excite the same electricity, I should desire very much that he would point out at least one experiment in which the couples remaining equal both in number and quality, but the conductivity of the pile varying, the tension might be seen likewise to vary. This experiment I expected to find described in the above-mentioned work, after his having said that he agreed with me, that all which diminishes the conductivity of the pile, must, according to his theory, augment the tension of the poles; I expected also, that he would only disagree with me in regard to the mode of diminishing the said conductivity. I find instead, that M. de la Rive says, that in my method of weakening the conductivity of the pile, if on one part the two electric principles met together at the poles have less facility of reuniting themselves, they are developed in less quantity. But truly he should agree that they might be developed not in less quantity, although with less tension, that the electric principles might show themselves *unvaried* in the tension at the poles of the pile, notwithstanding that its conductivity might vary.

[To be continued.]

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

[Continued from p. 442.]

Action of Chlorine on Oxalic Æther.

MALAGUTI has published a series of interesting experiments on the action of chlorine on oxalic æther. He has succeeded in preparing a chloroxalic æther, in which the entire quantity of the hydrogen of oxalic æther is replaced by an equivalent quantity of chlorine, $C^{12} Cl^{10} O^{4}$ *. The resulting chloroxalic æther affords with liquid ammonia oxamid, with gaseous ammonia chloroxamethane, a body comparable in every respect to oxamethane. The chloroxamethane is converted by the action of liquid ammonia into chloroxalovinate of ammonia, from which may be obtained the chloroxalovinic acid, which differs from oxalovinic acid solely from its containing chlorine in the place of hydrogen. This acid may be obtained in an anhydrous state by the action of alcohol on chloroxalic æther, which may again be made to give birth to a chlorated acid, especially by the oxidizing action of alkalies. One of the most remarkable

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* [The system of formulæ in this abstract is different from that in the former abstracts; the small French value for carbon having been assumed.]

results of these experiments is the preservation of the chemical properties of the oxalic æther after it has exchanged ten atoms of hydrogen for ten of chlorine. In examining the following table, in which are stated comparatively all the transformations of oxalic æther and of chloroxalic æther, we shall arrive at the conviction that the one series is merely a repetition of the other.

Oxalic Æther, and Compounds derived from it.	Chloroxalic Æther, and Compounds derived from it.
$C^{12} H^{10} O^{4}$ oxalic æther	$C^{12} Cl^{10} O^{4}$ chloroxalic æther
$C^{12} H^{10} O^{4}, C^{4} O^{3}$ oxalovinic acid	$C^{12} \overline{Cl}^{10} O^{4}, C^{4} O^{3}$ chloroxalovinic acid
$C^{12} H^{10} O^{4}, C^{4} O^{3}, H^{2} O$ hydrate of oxalovinic acid	$C^{12} Cl^{10} O^{4}, C^{4} O^{3}, H^{2} O$ hydrate of chloroxalov. acid
$C^{12} H^{10} O^{4}, C^{4} O^{3}, B O$ oxalovinates	$C^{12} Cl^{10} O^{4}, C^{4} O^{3}, B O$, chloroxalovinates
$C^{12} H^{10} O^{4}, C^{4} O^{2} N^{2} H^{4}$ oxamethane	$C^{12} Cl^{10} O^{4}, C^{4} O^{2} N^{2} H^{4}$ chloroxamethane
$C^{4} O^{2} N^{2} H^{4}$ oxamid	$C^{4} O^{2} N^{2} H^{4}$ oxamid.

Not long ago, if, after the inspection of this table, chloroxalic æther had been said to be, chemically speaking, nothing further than oxalic æther containing chlorine instead of hydrogen, no objection would have been made. But this is perhaps no longer the case at present; the mode of expression will nevertheless be the most simple and most pure as given by experiment.

Before concluding this notice, we may direct attention to a paper in the *Annales de Chimie et de Physique*, by De la Provostaye, on the Isomorphism between Oxamethane and Chloroxamethane, of which a translation has already been presented to the readers of this Journal (p. 372); his conclusions as to their isomorphism must, however, be confirmed before they can be adopted. Prof. H. Rose, in a paper recently published on the Combinations of the Volatile Chlorides with Ammonia, (Taylor's Scientific Memoirs, vol. ii. p. 32,) which contains some valuable remarks on Dumas's theory of substitutions, hints at the measurements of the crystals not being satisfactory.

State of Urea in Urine.

Very different views have been entertained respecting the state of urea in urine. Persoz considers it to be merely a product of sundry reactions, especially of heat, on urine, which he conceives to be proved from the syrupy mass obtained by freezing urine at −13° to −18° centigrade, when treated with nitric acid, only producing crystals of the nitrate of urea, after it has been for some time heated. Morin admits in urine solely a chlorine compound of his uril. Cap and Henry, on the contrary, maintain that urea is present in the urine of man as the lactate of urea. The experiments which M. Lecanu has recently published speak in favour of none of these opinions. We will here briefly give the conclusions which he has drawn from his experiments, viz. that the extraction of urea, by the various methods in which heat is employed, is not at all, as Persoz supposes, due to the production of this substance under the influence of heat; that Morin's protochloride of uril is nothing more than an intimate mixture, or rather a combination of urea with the chloride of ammonium; that the methods by which Cap and Henry supposed they had extracted lactate of urea give merely mixtures in which urea and lactic acid exist in a free state; that, lastly, urea can be extracted from urine, by means of alcohol nearly pure, without the employment of acids or alkalies, of which it might be believed that they destroy the natural state of combination of the urea.

Errata in Messrs. Francis and Croft's *Notices of the Results of the Labours of Continental Chemists.*

To Richard Taylor, Esq.

Dear Sir,

At your request we furnish you with a list of the errata contained in our abstracts. Most of them are such that every chemist will have been able to detect and correct them immediately, for instance, Aq O instead of Ag O, where a silver salt is spoken of; and Ag instead of Aq, where nothing about silver occurs. We are inclined to believe that most of your readers will have had the kindness to regard them as typographical errors, and will not have considered their occurrence a fair occasion to make a display of their superior knowledge, as a certain Mr. W. A. M. has done, who informs us that $\dot{K} I$ should be written without a dot over the K. We shall endeavour to obviate the occurrence of such errata in future: at the same time it must be recollected that the great distance prevents us from having proofs transmitted.

Yours truly,

Berlin, May 1841.

W. F. and H. C.

Page	202,	10 lines from bottom,	*for*	Melates,	*read*	Oxalates
„	—	7 „ „	„	H. Rose,	„	J. Liebig
„	204,	5 „ „	„	$\dddot{Fe}^2$,	„	$\dddot{Fe}$
„	—	7 lines from top,	„	$C^2 H^3$,	„	$\bar{C}^2 H^4$
„	205,	5 „	„	valeric,	„	valerianic
„	206,	16 „	„	$\dot{K} I^2$,	„	K I
„	—	18 „	„	E F,	„	Æ I
„	—	— „	„	R F,	„	K I
„	—	15 lines from bottom,	„	mæthyl,	„	methyl
„	207,	12 lines from top,	„	rose,	„	rises
„	209,	— „	„	oxide,	„	sesquioxide
„	—	6 lines from bottom,	„	mere,	„	pure
„	210,	15 lines from top,	„	$C^4 H$,	„	$C^4 H^5$
„	279,	8 lines from bottom,	„	$H N^6 O^6$,	„	$H^{44} N^6 O^6$
„	281,	21 lines from top,	„	$C^{24} H^{108}$,	„	$C^{64} H^{108}$
„	282,	last line,	„	$C^{27} H^{20}$,	„	$C^{22} H^{20}$
„	284,	13 lines from bottom,	„	improved	„	unproved
„	285,	lines 16 and 18 from bottom,	„	Ag,	„	Aq
„	289,	line 4 from top,	„	Aq O,	„	Ag O
„	291,	lines 3 and 4 from top	„	$\dot{A}q$,	„	$\dot{A}g$

LXXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 311*.]

Jan. 28, 1841. THE reading of a paper entitled, "On the action of certain Inorganic Compounds when introduced directly into the Blood." By James Blake, Esq., M.R.C.S. Communicated by P. M. Roget, M.D., Sec. R.S., was resumed and concluded.

The present paper is a continuation of a memoir read at the Académie des Sciences of Paris, in 1839, and entitled, "Effets de diverses substances salines, injectées dans le systême circulatoire †.

After some preliminary remarks on the mode in which the experiments were conducted, and on the assistance derived from the *hæmadynamometer* of Poiseuille (or instrument for measuring the pressure of the blood circulating in the vessels), the author gives a list of the various saline substances of which he noted the effects when they were severally injected either into the venous or the arterial systems, arranged according to the nature of those effects. He finds, in general, that all the salts having the same base exert similar actions when introduced directly into the blood. He carefully inquires into the phænomena apparently arising from the direct contact of each of the substances above enumerated with the animal tissues; and more particularly into the effects produced on the heart, on the muscular and the nervous tissues, and on the pulmonary and systemic capillaries.

The first series of experiments related are those on the action of the salts of magnesia: these are found, when introduced in any quantities into the blood, to arrest altogether the action of the heart; but a still more remarkable effect which results, is the complete prostration of muscular power. The salts of zinc have a similar operation under the same circumstances, but produce the same effects in smaller quantities. The action of the salts of copper, of lime, of strontia, of baryta, and of lead, are considered successively in the order in which they are more closely related by their physiological actions. The author particularly notices the peculiar action which the salts of the three last-named substances exercise on the muscular tissues, occasioning contractions in them during many minutes after death produced by their introduction into the blood. These muscular movements were, in some cases, observed forty-five minutes after the cessation of the heart's action. Experiments with the salts of silver and of soda are then detailed; substances, which exhibit a remarkable similarity in their actions on the pulmonary tissue, on the heart, and on the systemic capillaries: for while, in the case of all the other salts already mentioned, death seems to be produced by the destruction of the irritability of the heart, the fatal result with the salts of silver and of soda is the consequence of their action on the tissue of the lungs. The physiological actions of the salts of

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[* Two notices of communications made to the Royal Society, accidentally omitted from their proper place, will be found with the Miscellaneous Articles in the present Number.]

† Published in the "Archives Générales de Médecine; Nov. 1839."

ammonia and of potass were found by the author not to correspond with any of the preceding. Although agreeing perfectly with one another in their action upon the heart and systemic capillaries, they differ extremely in their effects on the nervous tissue; ammonia being particularly distinguished from all inorganic compounds in this respect, and being very analogous to poisons derived from organic products, which it also resembles in its chemical properties.

The general conclusion which the author is led to draw from these researches is, that there exists a close relation between the chemical properties of the substances experimented upon, and their physiological effects; his experiments tending to prove, that, when introduced into the blood, substances which are isomorphous exert similar actions on the living tissues. He notices, however, two exceptions to this law; namely, the similarity of the actions exerted on the pulmonary tissue by the salts of lead with those of silver, although these salts are not isomorphous; and also the action on the nervous tissue of the salts of ammonia being different from that of the salts of potass. But he remarks that the oxide of lead bears a close analogy to the oxide of silver in its relation to organic compounds. The general fact previously announced by the author in his memoir read to the Academy of Sciences at Paris, namely, that salts with the same base have analogous actions, may be considered as a corollary of the above law.

February 4.—A paper was read, entitled, "On some Electro-Nitrogurets." By Wm. Robert Grove, Esq., M.A., F.R.S.

The author states that he has made many attempts to render permanent the ammoniacal amalgam, and that he has succeeded in freezing it by means of solid carbonic acid, during which solidification, and also while in its solid state, it underwent no chemical change. He subsequently attempted to procure a permanent compound by electrolyzing a solution of hydrochlorate of ammonia with an extremely fusible alloy at the cathode; but this attempt was unsuccessful. It then occurred to him, that by using an oxidable metal at the anode, which could be revived in conjunction with nascent hydrogen and nitrogen at the cathode, one or both of these elements might be combined with the solid metal, and so form permanent compounds.

The experiment made in this manner with the metals zinc, cadmium, and copper, was perfectly successful. A spongy mass collected at the cathode, which floated upon the liquid, and which, when washed and dried, was analysed by heating in a tube retort; five grains of the zinc compound gave 0·73 of a cubic inch of permanent gas, which on examination proved to be nitrogen with one-fourth hydrogen. The same quantity of the cadmium compound gave 0·207 cubic inch of nitrogen with no admixture of hydrogen. A like weight of the copper compound gave 0·107 of nitrogen. No ammonia was evolved from either; and the author is inclined to think that the hydrogen yielded by the zinc compound resulted from the reaction of the metal upon combined water. The specific gravity of specimens of these substances which the author tried were respectively 4·6, 4·8, and 5·9. A mixed solution of chloride of gold and hydrochlorate of ammonia, electrolyzed with platinum electrodes, gave a black powder of the specific gravity 10·3; five grains of which, being heated, gave only 0·05 cubic inch of gas. The author proceeds to observe, that the similarity in appearance and mode of formation of these compounds and of the mercurio-ammoniacal amalgam, is strong evidence of identity of constitution, and that the non-permanence of the latter substance is due to the mobility of the mercury; for if we place the compounds in similar circumstances, that is, solidify the mercurial one, or liquefy those of the other metals, the phænomena are perfectly analogous. The experiments also bear immediately upon those of Thénard, Savart, and others, where ammonia, passed over heated metals, was found to be decomposed more completely by the oxidable than by the inoxidable metals, and to alter their physical characters without materially increasing their weight*. On examining papers connected with this subject, the author found that Mr. Daniell had cursorily noticed a deposit somewhat analogous to those here treated of, which was formed upon the negative plate of his constant battery when this was charged on the zinc side with hydrochlorate of ammonia, and the nature of which that gentleman observed was worthy of further examination, but had not had time to investigate.

February 11.—A paper was read, entitled, "Contributions to Terrestrial Magnetism, No. 2." By Major Edward Sabine, R.A., V.P.R.S.†

This paper is the second of a series, in which the author purposes to communicate to the Royal Society the results of magnetic observations in different parts of the globe, having for their object to supply the requisite data for deducing the numerical elements corresponding to the present epoch of the general theory of terrestrial magnetism. It consists of two sections; the first comprises the observations of Captain Belcher, R.N., and the officers of H. M. S. Sulphur, at twenty-nine stations on the west coast of America, and the adjacent islands, between the latitudes of 60° 21′ N. and 18° 05′ S. The second contains a new determination, by the same officers, of the magnetic elements at Otaheite, made in consequence of the discrepancies in the results obtained by previous observers, and of a note in M. Gauss's Allgemeine Theorie, in which Otaheite is spoken of as a highly important station for the future improvement of the calculations of the theory. Abstracts are given of the original observations which are deposited in the Hydrographic Office of the Admiralty, as well as a full detail of the processes of reduction by which their results have been computed. The values of the horizontal and total intensities are expressed in terms by which the results of observation are immediately comparable with the maps of MM. Gauss and Weber in the "Atlas des Erdmagnetismus."

By an investigation into the "probable error" of a single inde-

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[* Notices of the experiments of Savart and Despretz on this subject were given in Phil. Mag., Second Series (Phil. Mag. and Annals, N. S.), vol. iv. p. 155; vol. vi. p. 147.]

[† A notice of No. 1. appeared in Phil. Mag., Third or present Series (L. and E. Phil. Mag.), vol. xvii. p. 144.]

pendent determination of the magnetic intensity with Hansteen's apparatus, derived from the data furnished by Captain Belcher's observations, the author shows the extreme improbability that the differences in the results obtained at Otaheite by Messrs. Erman, FitzRoy and Belcher, should be occasioned by instrumental or observational error. They are also far greater than can, with any degree of probability, be ascribed to periodical or accidental variations in the magnetic force from its mean value. The only known cause adequate to their explanation is what may with propriety be termed *Station error*; that is, local disturbing influences, in an island composed chiefly of volcanic rocks, and where the spot of observation selected by the different observers may not have been precisely the same.

By a reference to the magnetic survey of the British Islands, the occurrence of station error is shown to be frequent in countries of far less decided igneous character than Otaheite; and that its existence may always be apprehended where rocks of that nature approach to, or rise through, the superficial soil. The absolute determinations of fixed observatories are as liable to station error as those of the magnetic traveller, since no continuance or repetition of the observations can lead to an elimination of the error; it consequently presents a practical difficulty to the proposed determination of the elements of the theory from exact observation at only a few selected positions on the globe. The remedy is to be found in the combination of fixed observatories and magnetic surveys: the observations of the survey, being made in concert with, and based on those of the fixed observatory, will be furnished thereby with corrections for the secular, periodical, and accidental variations of the elements, and will consequently determine *mean* values: and a proper combination of the mean values thus determined, over a space sufficiently extensive to neutralize *district* anomalies, as well as those of a more strictly local character, will furnish, in their turn, a correction for the station error, if any, of the fixed observatory.

A paper was also read, entitled, "On the Calculation of Attractions, and the Figure of the Earth." By C. J. Hargreave, B.A., of University College. Communicated by John T. Graves, M.A., F.R.S., Professor of Jurisprudence, University College, London.

The principal object of the calculations contained in this paper is to investigate the figure which a fluid, consisting of portions, varying in density according to any given law, would assume, when every particle is acted upon by the attraction of every other, and by a centrifugal force arising from rotatory motion. That such has been the original condition of the earth has been assumed as the foundation of most of the mathematical calculations connected with this inquiry; although the hypothesis itself may admit of doubt. The principal difficulty of this problem consists in the computation of the attraction of a body of any given figure, and composed of strata varying in their densities according to any given law. In solving it, the author follows the steps of Laplace as far as the point where the equation, known by his name, first appears. It has, however, since been discovered by Mr. Ivory, that the theorem of

Laplace is true only of spheroids of a particular kind, and consequently it is to this kind that Laplace's solution of the problem is restricted. The method given in the present paper is not confined in its operation to any particular class of spheroids; the coefficients of the series into which the required function is developed being determined absolutely, and without reference to the form of the spheroid to which they are to be applied. The principal change consists in the different manner of treating the partial differential equation; and its integration, effected by the author, renders the analysis more direct, the operations more simple, and the theory complete.

February 18.—A paper was in part read, entitled, "Memoir on a portion of the Lower Jaw of an Iguanodon, and other Saurian Remains discovered in the strata of Tilgate Forest, in Sussex." By Gideon Algernon Mantell, Esq., LL.D., F.R.S.

February 25.—The reading of a paper, entitled, "Memoir on a portion of the Lower Jaw of an Iguanodon, and other Saurian Remains discovered in the strata of Tilgate Forest, in Sussex." By Gideon Algernon Mantell, Esq., LL.D., F.R.S., was resumed and concluded.

When the author communicated to the Royal Society, in the year 1825*, a notice on the teeth of an unknown herbivorous reptile, found in the limestone of Tilgate Forest, in Sussex, he was in hopes of discovering the jaws, with the teeth attached to it, of the same fossil animal, which might either confirm or modify the inferences he had been led to deduce from an examination of the detached teeth. He was, however, disappointed in the object of his search until lately, when he has been fortunate enough to discover a portion of the lower jaw of a young individual, in which the fangs of many teeth, and the germs of several of the supplementary teeth, are preserved. The present paper is occupied with a minute and circumstantial description of these specimens, and an elaborate inquiry into the osteological characters and relations presented by the extinct animals to which they belonged, as compared with existing species of Saurian reptiles; the whole being illustrated by numerous drawings. The comparison here instituted furnishes apparently conclusive proof that the fossil thus discovered is a portion of the lower jaw of a reptile of the Lacertine family, belonging to a genus nearly allied to the Iguana. From the peculiar structure and condition of the teeth it appears evident that the *Iguanodon* was herbivorous; and from the form of the bones of the extremities it may be inferred that it was enabled, by its long, slender, prehensile fore-feet, armed with hooked claws, and supported by its enormous hinder limbs, to pull down and feed on the foliage and trunks of the arborescent ferns, constituting the flora of that country, of which this colossal reptile appears to have been the principal inhabitant.

Some particulars are added respecting various other fossil bones found in Tilgate Forest, and in particular those of the *Hylæosaurus*, or Wealden Lizard (of which genus the author discovered the remains of three individuals), and of several other reptiles, as the

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[* See Phil. Mag., Second Series, vol. ii. p. 444; v. p. 153; vii. p. 54: also Third Series, vol. v. p. 77; and present Number, p. 568.]

Megalosaurus, *Plesiosaurus*, and several species of *Steneosaurus*, *Pterodactylus*, and Chelonia, as also one or more species of a bird allied to the Heron. All these specimens are now deposited in the British Museum.

A paper was also read, entitled, "On a Theorem of Fermat." By Sir John William Lubbock, Bart., V.P., and Treas. R.S.

March 4.—A paper was read, entitled, "Miscellaneous Observations on the Torpedo." By John Davy, M.D., F.R.S.

The experiments described in this paper were made on a single fish, of middle size, recently taken out of the water. Portions of the electrical organs, cut transversely in thin slices, exhibited under the microscope many elliptical particles, apparently blood-corpuscles, the long diameter of which was about 1-800th, and the short about 1-1000th of an inch, and a few filaments, apparently nervous, irregularly scattered; some of them tortuous, and all about the 2000th of an inch in diameter. The latter bore no resemblance to muscular fibres. The blood contained some globular particles, having a diameter of the 4000th of an inch, mixed with the elliptical. The mucus for lubricating the surface was found to contain globules apparently homogeneous in substance, but of irregular outline, and in size varying from the 2000th to the 270th of an inch.

A paper was also read, entitled, "On a remarkable property of the Diamond." By Sir David Brewster, K.H., D.C.L., F.R.S.L., V.P.R.S. Ed.

On re-examining the phænomena of parallel bands of light and shade exhibited by reflexion at the plane surface of a diamond, which the author had noticed some years ago, he concludes that they result from the reflexions of the edges of veins or laminæ, of which the visible terminations are inclined at different angles, not exceeding two or three seconds, to the general surface. He gives an account of several analogous facts observable in other crystals, more especially those of carbonate of lime, artificially polished in surfaces inclined to the natural planes of cleavage.

March 11.—The following papers were read:

1. "On a Cycle of Eighteen Years in the Mean Annual Height of the Barometer in the Climate of London; and on a Constant Variation of the Barometrical Mean, according to the Moon's Declination." By Luke Howard, Esq., F.R.S.

For obtaining the general results communicated in the present paper, the author has followed the same method as that he had adopted in the two former papers laid before the Society on the connexion of the barometrical variation with the lunar phases and apsides. Tables are given of the barometrical averages on successive solar years, from 1815 to 1832, so constructed as to exhibit the variation of the moon's influence according to her declination; and also of these averages on successive cycles of nine solar years, classed according to the moon's place in declination, on either side of the equator. The results deduced from these comparisons are, first, that the barometrical mean in this climate is depressed by the moon's declination being to the south of the equator; and, secondly, that this depression takes place gradually, commencing with the moon's being in full north declination, and proceeding through her remaining positions to the time when she crosses the equator to resume the northern declination; at which season, the whole pressure that had been withdrawn from the atmosphere is suddenly restored. The author thinks there is evidence of a great tidal wave, or swell in the atmosphere, caused by the moon's attraction, preceding her in her approach to, and following her slowly as she recedes from these latitudes; so that were the atmosphere a calm fluid ocean of air, of uniform temperature, this tide would be manifested with as great regularity as those of the ocean of waters. But the currents uniformly kept up by the sun's varying influence effectually prevent this from taking place, and involve the problem in complexity. He finds that there is also manifested in the lunar influence a gradation of effect, which operates through a cycle of eighteen years. The mean pressure of the atmosphere during the first part of this period increases; and then, after preserving for a year its maximum amount, again decreases through the remaining years of the cycle, but exhibits, towards its minimum, some fluctuations before it again regularly increases*.

2. "On a remarkable depression of the Barometer in November 1840, agreeing very closely in its movements and results with that of December 1821." By Luke Howard, Esq., F.R.S.

The object of the author in the present paper is to show the close correspondence of the extraordinary depression of the barometer in the months of October and November of last year (1840), and of the remarkably stormy weather which prevailed at the same period, with similar phænomena occurring in December 1821, when the moon's place in declination underwent the same changes during those two periods, at an interval of nineteen years.

3. "General results of Meteorological Observations at Constantinople." By J. W. Redhouse, Esq. Communicated by John Davy, M.D., F.R.S.

4. "Term-Observations made at Prague in November and December 1840, and January 1841." By C. Kreil. Communicated by S. Hunter Christie, Esq., M.A., Sec. R.S.

March 18.—The following Magnetical and 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 by the Master-General of the Ordnance, viz.—

1. "Magnetic-term Observations, taken at Kerguelen's Land, for May and June 1840." By Capt. James Clark Ross, R.N., F.R.S., Commander of the Expedition.

2. "Hourly Magnetic Observations taken at Kerguelen's Land, commencing May 25, and ending June 27, 1840." By Capt. James Clark Ross, R.N., F.R.S., &c.

3. "Meteorological Observations taken on board Her Majesty's Ship *Erebus*, for October, November, and December 1839, and from

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[* On the subject of this paper see p. 555 and 559.]

January to August 1840." By Capt. James Clark Ross, R.N., F.R.S., &c.

4. "Meteorological Observations taken on board Her Majesty's Ship *Terror*, for November and December 1839, and from January to July 1840." By Capt. T. B. M. Crozier, R.N.

5. A paper was also in part read, entitled, "On the Localities affected by Hoar-frost, the peculiar currents of Air excited by it, and the Temperature during its occurrence at High and Low Stations." By James Farquharson, LL.D., F.R.S., Minister of the Parish of Alford.

March 25.—The following communications were read, viz.—

1. The reading of a paper entitled, "On the Localities affected by Hoar-frost, the peculiar currents of Air excited by it, and the Temperature during its occurrence at High and Low Stations." By James Farquharson, LL.D., F.R.S., Minister of the Parish of Alford, was resumed and concluded.

The author states that he has been accustomed, for the last forty years, to make observations on the occurrence of hoar-frost, and the circumstances under which it takes place, with a view of obtaining a correct explanation of the causes of that phænomenon. It is well-known, he observes, that the localities chiefly affected with hoar-frost are the bottoms of valleys, and land-locked places of all kinds, whether natural or artificial. The altitude to which its effects reach on the sides of the valleys is dependent on the mean temperature of the day and night at the time of its occurrence: when that temperature is high, the lower places only are affected by the frost; but when low, the frost extends to much higher grounds. Hoar-frost occurs only during a calm state of the air, and when the sky is clear; but the stillness of the air in the bottoms of the valley is invariably accompanied by downward currents of air along all the sloping sides of the valley; and it is to this fact, first noticed by the author, that he wishes more particularly to direct the attention of the Society, as affording a decisive proof of the correctness of the views he entertains, being in accordance with the theory of Dr. Wells. He finds that after sunset, in all seasons of the year, and at all mean temperatures of the air, and whether or not the ground be covered with snow, whenever the sky is clear, although there may be a dead calm at the bottoms of the valleys, currents of air, more or less strong and steady, run downwards on the inclined lands, whatever may be their aspect with reference to the points of the compass. These currents are the result of the sudden depression of temperature sustained by the surface of the earth in consequence of rapid radiation, by which the stratum of air in immediate contact with that surface, becoming specifically heavier by condensation, descends into the valley, and is replaced by air which has not been thus cooled, and which therefore prevents the formation of hoar-frost on the surface of these declivities.

2. "Term-Observations of Magnetic Observations, the Variation of the Magnetic Declination, Horizontal Intensity and Inclination at Prague; for June, July, September, and October 1840." By Prof. Kreil. Communicated by S. Hunter Christie, Esq., Sec. R.S.

3. "Term-Observations of the Variation of the Magnetic Declination, Horizontal Intensity and Inclination at Milan; for June 1840." By Francesco Carlini, For. Memb. R.S., Director of the Observatory.

4. "On Ground-gru, or ice formed, under peculiar circumstances, at the bottom of running water." By James Farquharson, LL.D., F.R.S., Minister of the Parish of Alford.

The author brings forward in this paper several recent observations on the formation of ice at the bottom of rivers, the conditions of which corroborate the views regarding the cause of that phænomenon, which he presented in a paper on this subject, published in the Philosophical Transactions for 1835 (p. 329) *, namely, that it occurs in consequence of the loss of heat by radiation from the bottom of the water, in a manner precisely analogous to the formation of hoar-frost on the surface of dry land, as first explained by Dr. Wells. He then answers some of the objections to that theory propounded in an article, under the title of GROUND-GRU, in the Penny Cyclopædia, and shows that those objections are founded in error, and possess no validity.

5. "Meteorological Observations made at the Magnetic Observatory at St. Helena, from February to October 1840." By Lieut. J. H. Lefroy, R.A.

6. "Meteorological Observations made at the Magnetic Observatory at Toronto, Upper Canada, from January to October 1840." By Lieut. E. J. B. Riddell, R.A.

7. "Observations on Magnetic Direction and Intensity made at the Observatory at Milan during the 24th, 26th and 27th of January 1841." By Prof. Carlini.

8. "Note on an irregularity in the Height of the Barometer, of which the argument is the Declination of the Moon." By Sir John William Lubbock, Bart., V.P. and Treas. R.S.

In the Companion to the British Almanac for 1839, the author inserted some results which were obtained with a view of ascertaining the influence of the moon on the barometer and on the dew-point. Mr. Luke Howard's researches on this subject (*ante*, p. 552) having recalled his attention to that paper, he found that some of the results he had given appeared to indicate that the moon's position in declination influences the barometer. In order to render this more manifest, he combines in the present paper all the observations he gave in the Companion to the British Almanac in three categories. These observations correspond to different angular distances of the moon from the sun, or times of transit; but as the inequality of the ocean, of which the argument is the moon's declination, is independent, or very nearly so, of the time of the moon's transit, it is probable that so also is that in the height of the barometer. In this case we may with propriety combine in the same category observations which correspond to similar declinations, although to different times of transit. The results stated by the author seem to indicate an ele-

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[* An abstract of this paper will be found in Phil. Mag., Third Series, vol. vii. p. 137.]

vation of nearly one-tenth of an inch for 17 degrees of declination. The inequality has a contrary sign to the inequality of the same argument in the tides of the ocean.

April 1.—The following letter, addressed to the President, was read:—

"4, Trafalgar Square, London, March 25th, 1841.

"My Lord,—I have the honour of transmitting to Your Lordship for presentation to the Royal Society, an original portrait of *Sir Isaac Newton* by *Vanderbank*, a Dutch painter of some note in that age.

"This picture has now been many years in my possession, and the tenure by which I have kept it (as a collateral descendant of so illustrious a man) was too flattering not to have been a source of great personal gratification.

"But I consider such a portrait to belong of right to the scientific world in general, and more especially to that eminently distinguished Society of which Newton was once the head, and which is now so ably presided over by Your Lordship.

"I have, therefore, to request Your Lordship will do me the honour to present this original portrait of Sir Isaac Newton to the Royal Society in my humble name.

"Accident having destroyed some of the papers of my family, I am unable of myself to trace the entire history of this portrait, but I believe more than one member of the Royal Society is competent to do so, and it is well known to collectors; and a small mezzotinto engraving of it was published about forty years ago. It was painted the year before Newton died, and came into the family of the celebrated Lord Stanhope, who left it by his will to my grandfather, the late Dr. Charles Hutton, a distinguished member of the Royal Society, expressly on the well-authenticated account of that eminent mathematician having been remotely descended from Sir Isaac Newton, in the following way, as I find on a family manuscript; viz. 'that the mother of the well-known James Hutton and the mother of Dr. Charles Hutton were sisters; and the grandmother of James Hutton and the mother of Sir Isaac Newton were also sisters.'

"I have ever considered this very distant connexion with so great a man should not be an inducement to lead me into any but casual mention of the circumstance, that I might avoid the imputation of a vain boast; nor would it have been brought forward now, except to explain the cause by which this portrait came into the possession of an individual who is happy in relinquishing it to grace the Hall of Meeting of the Royal Society.

"I have the honour to subscribe myself,
"Your Lordship's very obedient humble Servant,
"Charles Vignolles."

"*The Right Honourable the Marquess of Northampton,*
&c. &c. &c.
President of the Royal Society."

The following papers were read, viz.—

1. "A Meteorological Journal for 1840, kept at Allenheads,

Northumberland, with a few remarks on the Rain-gauge." By the Rev. W. Walton, F.R.S.

The author shows that if the mouth of a rain-gauge be placed in any plane which is not perfectly horizontal, the results will be liable to inaccuracy, whatever may be the direction in which the rain falls. He thinks that, on many occasions, the drops of rain diminish in their size during their descent on entering warmer regions of the atmosphere, so as finally to disappear.

2. "The Scholar's Lute among the Chinese." By — Lay, Esq. Communicated by S. H. Christie, Esq., Sec. R.S.

The Kin, which is the stringed instrument here described, was the one played upon by Confucius and the sages of antiquity, and is therefore held sacred by men of letters. It is made of the Woo-tung, or *Dryandria cordifolia.* It is convex above and plane below, and is wider at one end than at the other; it has two quadrangular apertures in the plane surface, which open into as many hollows within the body of the instrument; and it is furnished with seven silken strings of different diameters, which pass over the smaller end, and are distributed between two immovable pegs below. A bridge within a short distance of the wider extremity gives these strings the necessary elevation and a passage to the under surface, where, by means of a row of pegs, they are tightened or relaxed at pleasure. The length of the sounding-board is divided by thirteen studs of nacre, or mother-of-pearl, as a guide for the performer; and they are placed so that the length of each string is bisected, trisected, &c., that is, divided into aliquot parts as far as the eighth subdivision, with the omission of the seventh, the number of sections being represented by the arithmetical series

2, 3, 4, 5, 6, 0, 8.

Thus the intervals, or magnitudes of the different tones sounded by this instrument, do not accord with those produced on our violin, but agree more with the old Scotch music. The study of this instrument, and the art of playing upon it, are rendered extremely difficult by the complexity of the Chinese notation of written music, which leads to frequent omissions and blunders. Thus every air which a Chinese plays has cost him the labour of many months to learn; and so tiresome is this acquisition, that the author has heard some extemporize very prettily without being able to play a single air. Their performance, however, is very graceful; and though the melody be simple, every scope is given to variety by the mode of touching the strings. The author enters into an examination of the musical theory of the sounds produced by this instrument.

April 22.—The following papers were read, viz.—

1. Magnetic-term Observations taken on board H.M.S. Erebus and Terror, at Hobart Town, on the 29th and 30th August, and the 23rd and 24th September, 1840, by, and under the direction of James Clark Ross, Captain R.N., F.R.S., and Commander of the Antarctic Expedition.

2. Magnetic-term Observations made at the fixed Magnetic Observatory, Van Diemen's Land, on the 28th, 29th and 30th August,

and the 23rd and 24th September, 1840; by James Clark Ross, Captain R.N., F.R.S., Commander of the Antarctic Expedition.

3. Hourly Magnetic Observations for August and September, 1840, taken at the Ship's Magnetic Observatory, Van Diemen's Land, under the direction of James Clark Ross, Captain R.N., F.R.S., Commander of the Artarctic Expedition.

The above papers were communicated by the Lords Commissioners of the Admiralty.

4. Variation de la déclinaison, intensité horizontal et inclinaison magnétique, observés à Milan, pendant vingt-quatre heures de suite, le 24 et 25 Fevrier et Mars, 1841, par Signior Carlini, Forn. Memb. R.S.

5. "Remarks on the Birds of Kerguelen's Land." By R. McCormick, Esq., Surgeon R.N. of H.M.S. Erebus. Communicated by the Lords Commissioners of the Admiralty.

The birds usually met with by the author in this island were petrels and penguins; and besides these, he found two species of gull, a duck, a shag, a tern, a small albatros, and a species of Chionis; and also a remarkable nocturnal bird allied to the Procellaria. Brief notices are given of the forms and habits of these birds.

6. "Geological Remarks on Kerguelen's Land." By R. McCormick, Esq., Surgeon R.N. of H.M.S. Erebus. Communicated by the Lords Commissioners of the Admiralty.

The northern extremity of the island is described as being entirely of volcanic origin. The trap rocks, of which the headlands are composed, form a succession of terraces nearly horizontal. Basalt is the prevailing rock: it assumes the prismatic form, and passes into greenstone, and the various modifications of amygdaloid and porphyry. The general direction of the mountain-ranges inclines to the south-west and north-east, and they vary in height from 500 to 2500 feet. Many of the hills are intersected by trap dykes, usually of basalt. Several conical hills, with crater-shaped summits, are found, evidently the remains of volcanic vents. Three or four very singular isolated hills, composed of an igneous slaty sandstone, occur in Cumberland Bay, presenting very smooth outlines, and consisting of piles of broken fragments, through which the mass protrudes, in places, in prismatic columns. Vast quantities of *débris* are accumulated at the base of the hills, in many places to the height of 200 or 300 feet or more, affording strong evidence of the rapid disintegration this land is undergoing, from the sudden atmospheric vicissitudes to which it is exposed.

The whole island is deeply indented by bays and inlets, and its surface intersected by numerous small lakes and water courses. These, becoming swollen by the heavy rains, which alternate with frost and snow, rush down the sides of the mountains and along the ravines in countless impetuous torrents, forming, in many places, beautiful foaming cascades, wearing away the rocks, and strewing the platforms and valleys below with vast fragments of rocks and slopes of rich alluvium, the result of their decomposition.

The most remarkable geological feature in the island is the occurrence of fossil wood and coal, and what is still more extraordinary; these are imbedded in the igneous rocks. The wood, which is for the most part highly silicified, is found enclosed in the basalt; whilst the coal crops out in ravines, in close contact with the overlying porphyritic and amygdaloidal greenstone.

A few outline sketches of the rocks and scenery, in various parts of the island, accompany this paper.

A paper was also in part read, entitled, "On the proportion of the prevailing Winds, the mean Temperature, and depth of Rain in the climate of London, computed through a cycle of eighteen years, or periods of the Moon's Declination." By Luke Howard, Esq., F.R.S.

April 29.—The reading of Mr. Howard's paper was resumed and concluded.

In this paper the author investigates the periodical variations of the winds, rain and temperature, corresponding to the conditions of the moon's declination, in a manner similar to that he has already followed in the case of the barometrical variations, on a period of years extending from 1815 to 1832 inclusive. In each case he gives tables of the average quantities for each week, at the middle of which the moon is in the equator, or else has either attained its maximum north or south declination. He thus finds that a north-east wind is most promoted by the constant solar influence which causes it, when the moon is about the equator, going from north to south; that a south-east wind, in like manner, prevails most when the moon is proceeding to acquire a southern declination; that winds from the south and west blow more when the moon is in her mean degrees of declination, going either way, than with a full north or south declination; and that a north-west wind, the common summer and fair weather wind of the climate, affects, in like manner, the mean declination, in either direction, in preference to the north or south, and most when the moon is coming north.

He finds the average annual depth of rain, falling in the neighbourhood of London, is 25·17 inches.

From his observations on the temperature, he deduces the following conclusions:—1. That the pressure of an atmospheric tide, which attends the approach of the moon to these latitudes, raises the mean temperature 0·35 of a degree. 2. That the rarefaction under the moon in north declination lowers the temperature 0·13 of a degree. 3. That the northerly swell following the moon as she recedes to the south further cools the air 0·18 of a degree. 4. That this cold continues while the moon is away south, reducing the mean temperature yet lower by 0·04 of a degree*.

The following papers were then read:—

1. "A new Method of solving Numerical Equations." By Mr. Thomas Weddle, of Stamfordham. Communicated by S. H. Christie, Esq., M.A., Sec. R.S.

The object of this paper is to develope a new and remarkably

[* On the subject of this paper see *ante*, p. 552 and p. 555.]

simple method of approximating to the real roots of numerical equations, which possesses several important advantages. After describing the nature of the transformations which are subsequently employed, the author proceeds to develope the process he uses for obtaining one of the roots of a numerical equation. Passing over the difficult question of determining the limits of the roots, he supposes the first significant figure (R) of a root to have been ascertained, and transforms the proposed equation into one whose roots are the roots of the original, divided by this figure $\left(\text{or } \frac{x}{\text{R}}\right)$: one root of this equation lying between 1 and 2, the first significant figure (r) of the decimal part is obtained, and the equation transformed into another whose roots are those of the former, divided by $1 +$ this decimal (or $1+r$). This last equation is again similarly transformed; these transformations being readily effected by the methods first given. Proceeding thus, the root of the original equation is obtained in the form of a continued product. After applying this method to finding a root of an equation of the 4th, and likewise one of the 5th degree, the author applies it to a class of equations to which he considers it peculiarly adapted, namely, those in which several terms are wanting. One of these is of the 16th degree, having only six terms; and another is of the 622nd degree, having only four terms.

2. "Additional Note on the Contraction of Voluntary Muscles in the living body." By William Bowman, Esq., F.R.S., Demonstrator of Anatomy in King's College, London, and Assistant Surgeon to King's College Hospital *.

This communication contains a short account of some recent examinations made by the author on the human muscular fibre affected by tetanus. The effect of the violent contractions which characterize this disease, is to produce, in many parts of the muscles, considerable ecchymosis, which gives the contiguous portions a pale and gray aspect. In other places the muscles lose, in a great measure, their fine fibrous character, and exhibit a soft mottled surface, which is easily torn. The primitive fasciculi, when microscopically examined, present indications of strong contraction, appearing swollen into a fusiform shape, and having their transverse striæ in some parts much more closely approximated, and in others separated to much greater distances than in the natural state, or even altogether obliterated, in consequence of the whole texture being broken up into those primitive elements of which the discs are constructed; and frequently they are broken across without a corresponding rupture of the sarcolemma.

The author is led from his observations to the conclusions,—1st, that the contraction of a muscle is the essential cause of its rupture; 2ndly, that there is no repellent force between the contractile elements of muscular fibre; and, lastly, that the contraction of volun-

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[* An abstract of Mr. Bowman's former paper appeared in Phil. Mag. Third Series, vol. xvii. p. 386.]

tary muscle is not a sustained act of the whole congeries of contractile elements composing it, but a rapid series of partial acts, in which all duly share, becoming by turns contracted and relaxed.

The paper is accompanied by drawings of the microscopic appearances therein described.

May 6.—The following papers were read, viz.—

1. "Investigation of a New and Simple Series, by which the Ratio of the Diameter of a Circle to its Circumference may easily be computed to any required degree of accuracy." By William Rutherford, Esq., of the Royal Military Academy, Woolwich. Communicated by Samuel Hunter Christie, Esq., M.A., Sec. R.S.

Among various formulæ for the rectification of the circle discovered by the author, he has found the one given in this paper to be that best fitted for computation; and he has been enabled by means of it, with comparatively little labour, to extend the number, expressing the ratio of the diameter to the circumference, to 208 places of decimals, a degree of accuracy hitherto unattainable, without a great amount of labour, by means of any of the series which have yet been employed.

The celebrated series of Mr. John Machin, for the rectification of the circle, is derived from the formula

$$\frac{\pi}{4} = 4\tan^{-1}\frac{1}{5} - \tan^{-1}\frac{1}{239},$$

which converges with considerable rapidity, but gives rise to tedious computations, in consequence of the divisor 239 being a prime number. But by converting the above formula into the following,

$$\frac{\pi}{4} = 4\tan^{-1}\frac{1}{5} - \tan^{-1}\frac{1}{70} + \tan^{-1}\frac{1}{99},$$

a series is obtained by which the extended computation above mentioned was readily effected.

The methods of computation are then stated in detail, and the resulting value of π is given to 208 places of decimals, which is presumed to be accurate to the last figure, the computations having been actually carried as far as 210 figures.

2. "On the Phænomena of thin plates of Solid and Fluid Substances exposed to polarised light." By Sir David Brewster, K.H., D.C.L., F.R.S., & V.P.R.S. Ed.

From a theoretical investigation of the phænomena described in this paper, the author deduces the important law, that when two polarized pencils, reflected from the surface of a thin plate, lying on a reflecting surface of a different refractive power, interfere, half an undulation is not lost, and white-centred rings are produced. When the inclination is exactly 90°, the pencils do not interfere, and no rings are produced.

GEOLOGICAL SOCIETY.

[Continued from p. 527.]

June 10, 1840 (*Continued*).—On the mineral veins of the Sierra Almagrera, in the province of Almeria, in the South of Spain, by J. Lambert, Esq., F.G.S.

The Sierra Almagrera extends from the mouth of the Almanzora (lat. about 37° 17′, long. about 15° 40′) in a N.N.W. direction for twelve miles. Its width is about a mile and a half, and its greatest height 1400 feet. It is composed of clay-slate, resting upon mica-slate, accompanied by beds of milky quartz, and crossed by numerous ferruginous veins containing sulphate of barytes and gypsum. The strata of the clay-slate are generally horizontal, but are sometimes inclined from 15° to 20°, and even more, where disturbed by masses of greenstone.

The vein of the Barranco Jaroso was the first discovered, and it is now of considerable richness. Its excavations extend more than 200 yards in length, with every indication of the lode continuing. The direction of the vein is north to south, between one and one and a half hours, or 15° to 22½° east of north; and the inclination is from 65° to 70° east. The breadth of the vein, where it was commenced at the surface, was half a yard, but it had increased to three yards at the depth of forty yards, the point to which it had been carried in April 1840. The mineral contents of the vein consist of parallel divisions of several varieties of galena, as crystallized, radiated with an antimonial aspect, brilliant large-grained, fine steel-grained, and black, of oxide and carbonate of lead, and argillaceous iron ore; carbonate of iron and carbonate of copper also occur; and sulphates of barytes and gypsum are abundant.

Old workings, supposed to have been conducted by the Romans, occur in great numbers, principally at the mouths of the Barrancos or ravines of Pinalbo del Frances and de la Torre. Quantities of mine-timber, decayed iron tools and lamps of clay, have been found in them; but in no case does it appear that gunpowder was used in making the excavations. Large heaps of slags and scoria are of frequent occurrence; one of the most important being situated between the confluence of the Almanzora and the Rambla de Muleya at the foot of the little hill Cabéza de las Herrerias (Head of the Forges).

This hill, Mr. Lambert says, presents the aspect of a volcanic crater, and has disturbed the tertiary deposits of the neighbourhood. He states, that it is an enormous mass of oxide of iron, with a multitude of veins of sulphate of barytes; and that it is absolutely honey-combed by old excavations in the barytes veins, the contents of which, he believes, were used as a flux in smelting the argentiferous minerals. The tertiary beds at this locality consist of clays resting upon conglomerates, and are all charged with iron. They are stated to contain also "ferruginous and jaspery dendrites," veins of felspar and crystals of barytes.

A tertiary formation extends from the foot of the Sierra Almagrera to the Sierras de Filabres, de Alhamilla and Cabrera. The upper part consists of an arenaceous conglomerate alternating with marls, and beds of quartz and other pebbles of various sizes. The clays contain gypsum and sands very similar to those of the vicinity of Paris; and numerous organic remains, belonging principally to the genera Ostrea, Pecten, and Dentalium; likewise corals. The formation is disturbed in many parts by protruded masses of greenstone; also by porphyries, trachytes and basalts, which are stated to present very singular phænomena. Gneiss projects above the tertiary strata in many places.

The paper was accompanied by specimens of galena, of which the following analyses are given in the paper itself:—

Specimen, No. 1.	70·8 per cent. Lead	1·05 per cent. Silver	16 oz. per gal.
——— 2. Radiated	62·1 ———	0·65 ———	$10\frac{4}{10}$ —
——— 3. Black ...	22·25 ———	0·325 ———	$5\frac{2}{10}$ —

About 400 tons of numbers 1 and 2, have been extracted from the mine during the months it had been worked; and the produce about the time the paper was written was fifteen tons a day.

A notice on the Sierra de Gador, and its lead mines, by Josias Lambert, Esq., F.G.S.

The Sierra de Gador, celebrated for its lead mines, is situated between the Sierra Nevada and the Mediterranean. Its length from west to east is nearly forty miles; its breadth varies from five to ten miles, and its highest point,—the Collado de los Valientes, near its western extremity,—is upwards of 6000 feet above the sea. From that point eastward, the height gradually decreases till it is reduced near the Almeria to the level of that river. The southern face is precipitous, and from its base to the Mediterranean extends the tertiary plain of Dalias. The western flank is also precipitous, but the northern face rises more gently from the river Almeria, which separates the Sierra de Gador from the Sierra Nevada.

The principal mass of this range of mountains is composed almost exclusively of a limestone, considered by Mr. Lambert to belong to the lowest of the transition rocks, because its stratification is in general conformable to that of the nucleus of the Sierra Nevada, and because it is believed to contain no organic remains. It is of a grey or dark colour, and of a compact or finely grained texture, but it is sometimes, though rarely, friable. It passes downwards by alternations and transitions into clay-slate, talcose or mica-slate, and in the upper part, it is connected with breccias and limestone conglomerates.

The limestone is generally traversed by veins of calcareous spar and fluor spar, frequently so arranged as to resemble the stripes in the skin of a zebra. It is also occasionally magnetic, on account of disseminated particles of magnetic iron. The stratification is regular in some places, but it generally presents great inflexions and disruptions, and curves of a thousand different natures. The prevailing strike of the beds is from east to west, but from the western base of the mountain at Castala to near the highest point in the Loma del Sueño and Collado de los Valientes, it is from north to south. The inclination of the beds varies very much both in direction and inclination; the former being sometimes south, but often north, and occasionally east; and the latter differing from 15° to 45°. The beds are for the greater part compact, but the higher consist of a breccia, mixed with slate-clay and clayey ochres. The stratification in these beds is sometimes well pronounced, and at others less so, being of great thickness. The fragments of the calcareous breccia are angular and of various sizes, and are cemented by carbonate of lime.

In the ravine of Cartala, protruded masses of trap, containing veins of asbestos, amphibole, and porcellanite, are stated to have dislocated the strata; an inconsiderable vein of trap is also described as interposed between two beds of limestone. The slate clays near the masses of trap, are said to be frequently of a green colour.

At the eastern extremity of the chain, the limestone is overlaid by beds of gypsum, containing masses and strings or small veins of native sulphur.

There is no doubt that mines in this mountain chain were worked by the Romans. The ore is generally found in nests or masses of inconsiderable size; also in veins and branches of limited extent in any constant direction, crossing each other, and forming almost always communications between the nests. Mr. Lambert is therefore induced to consider these metallic accumulations as of contemporaneous origin with the limestone. At the mine of Arnafe, on the western edge of the Sierra, the ore occurs between two beds of limestone, having the same direction east, 20° north, and dipping with them 80° to the N.E. It is one foot thick, and is accompanied by clay. The same agreement has been found in Santa Rosa, Santa Catalina, Cruzados, Trinidad primero, and in all the mines situated upon the western declivity, looking towards Berja.

In the Loma del Sueno, and the interior parts of the Sierra, where the beds incline only 20° to 30°, but frequently exhibit great dislocations, which change their position entirely, and often form crests, fissures, and hollows filled with argillaceous substances, are found the greatest masses of ore, lying between the beds, and conforming to all their modifications. Fluor almost always accompanies the galena.

One level has been carried nearly 600 yards in length, from the bottom of a precipice of nearly equal altitude, in order to undermine the rich deposits on the edge of the Loma del Sueño, but hitherto nothing has been met with but compact and slaty limestone. Mr. Lambert therefore infers, that the ore is more superficial, and he adds, there is no instance of its having been found at a greater depth than 200 yards from the surface.

The lower parts of the fissures which traverse the limestone, are frequently filled with fragments of ore enveloped in a red earthy soil, and associated with angular as well as rounded fragments of limestone. In the alluvial detritus of the ravines, and the dry deltas at their mouth, fragments and masses of ore have been extracted, often in considerable quantities, and at the Pecho de las Lastras to the extent of 100,000 tons.

In the limestone mountains which stretch westward from the Sierra de Gador to Marbella, within forty miles of Gibraltar, lead ore is found in variable quantities, but not so abundantly as in the Sierra de Gador.

In conclusion, Mr. Lambert observes, that the improvident method of working the ore in that mountain is fast destroying the best mines; that new trials have not been attended with anything like success; and that the hardness of the rock renders sinkings very expensive, and compels adventurers with limited funds to abandon their undertakings, unless ore be speedily obtained.

On the polished and striated surfaces of the rocks which form the beds of Glaciers in the Alps, by Professor Agassiz.

This paper was accompanied by a series of plates intended to represent the effect of glaciers upon the rocks over which they move.

These effects, consisting of surfaces highly polished, and covered with fine scratches, either in straight lines or curvilinear, according to the direction of the movement of the glacier, are constantly found, not only at the lower extremity, where they are exposed by the melting of the glaciers, but also, wherever the subjacent rock is examined, by descending through deep crevices in the ice. Grains of quartz and other fragments of fallen rocks, which compose the moraines that accompany the glaciers, have afforded the material which, moved by the action of the ice, has produced the polish and scratches on the sides and bottom of the Alpine valleys through which the glaciers are continually, but slowly descending. It is impossible to attribute these effects to causes anterior to the formation of the glacier, as they are constantly present and parallel to the direction of the movement of the ice. They cannot be considered as the effects of an avalanche, for they are often at right angles to the direction in which an avalanche would descend; they are constantly sharp and fresh beneath existing glaciers, but less distinct on surfaces which have for some time been left exposed to atmospheric action by the melting of the ice. In the valley of the Viesch, the direction of the scratches is from north to south, or towards the Rhone; the direction of those which accompany the glacier of the Rhone is from east to west; that of those beneath the glacier of the Aar is first from west to east, as far as the Hospice of the Grimsel; and then from south to north, from the Grimsel to the Handeck. If we would account for these scratches by the action of water, we must imagine currents of enormous depth filling these highest Alpine valleys, and descending in opposite directions from the narrow crest that lies between them. In the upper part of the valley of the Viesch, is a glacier, beneath which runs a rapid torrent, co-extensive in length with the great current, to which the above hypothesis would attribute the polish and scratches on the rocks of the valley. This small torrent corrodes the bottom of the valley into sinuous furrows and irregular holes, and polishes the sides of its bed; but the polish is of a different aspect from that produced by the action of the ice, and of the stones and sand which it carries with it. The polished surfaces beneath the ice are often salient and in high relief. The sides also of the valleys adjacent to the actual glaciers are frequently polished and scratched at great heights above the ice, in a manner identical with the surface beneath it, but different from the polish of the bed of the torrent.

The amount of polish and scratches varies with the nature of the rocks. In the valley of Zermatt and Riffelhorn, rocks of serpentine are most exquisitely polished; so also are the granites on the sides of the glacier of the Aar, where they have not been long exposed to the action of the air. Gneiss and limestone do not preserve their polish under similar exposure, but retain it while they are protected by ice or a covering of earth.

These facts seem to show, that the striated and polished condition of rocks beneath and on the sides of glaciers, is due to the action of the ice, and of the sand and fragments of stone forming the moraines which accompany it.

On a bed of lignite near Messina, by Dr. R. Calvert.

About thirty years ago, Dr. Calvert discovered a bed of lignite, a quarter of a mile from Messina, up a Fiumera to the left of Fort Gonzago. It cropped out to the north at an angle of about 45°, and was at least a yard in thickness. The lignite was swept down a precipice by the country people to make room for sticks to burn lime with; a superb quarry of which was then worked on the opposite side of the field. Dr. Calvert laid in a winter stock of the lignite; the dragoons used it in their forge, and the commander of the forces in his kitchen. Owing, however, to the unskilfulness of the people who dug the lignite (soldiers and officers' servants), the roof fell in, and the property above being injured, the excavations were stopped. Some of the lignite emitted a bad effluvia when burned.

A letter from Richard Greaves, Esq., addressed to Dr. Buckland, and dated June the 6th, 1840, on the discovery of bones of birds, fishes, and mammalia, in the limestone cliff at Eel Point in Caldy Island, and about eighty feet above the sea.

A note from Mr. Hamilton, Sec. G.S., addressed to Dr. Buckland, on the irregular occurrence of rounded fragments of rock crystal, throughout the Hastings sands, in the neighbourhood of Tunbridge Wells. Mr. Hamilton's principal object is to call attention to the inquiry whence the fragments were obtained; and to the assistance which this knowledge would afford in determining the origin of the other materials forming the Hastings sands.

A letter, dated May 6th, 1840, from M. Roemer, of Hildesheim, to Dr. Fitton, on the chalk and the subjacent formations to the Purbeck stone inclusive in the north of Germany.

a. Chalk with flints.—This formation, presenting characters which exactly agree with those of the chalk of England, is found only in the island of Rugen. It there consists of a white limestone, with frequent layers of flints, and includes the same fossils.

M. Roemer is of opinion that the Rugen deposit is of the age of the Maestricht beds, though most geologists believe it to be younger.

In the north of Germany there are very thick deposits of sandstone and sandy marls, which correspond, M. Roemer says, to the upper subdivision of the chalk formation. The characteristic fossils are *Callianassa* (*Pagurus*) *Faujasii*, *Belemnites mucronatus*, small corals, &c. No Ammonites have been found in it. The localities where it occurs, are Gehrden near Hanover, Goslar, Quedlenburg, and Halberstadt.

b. Chalk without flints exists near Piena and Luneberg, with the same external characters as in England. Near Ilseburg, Lemforde, Dulmen, and other localities, it is represented by sandy marls and sandstones. It contains *Belemnites mucronatus,* many Scyphia, some Ammonites, &c.

c. Chalk marl (Pläner Kalk).—This formation is extensively distributed and well exposed in the north of Germany, and exhibits everywhere the same characters as in England. It contains no *Belemnites mucronatus,* but *Ammonites varians, A. Mantelli, A. Gutoni, Turrilites costatus, T. undulatus, Plicatula inflata,* &c.

d. Upper greensand.—This formation, as a greyish green marl, with grains of silicate of iron, occurs only near Dresden and near Wal in Westphalia; also in the neighbourhood of Hildesheim. It contains *Ammonites falcatus, Terebratula biplicata, Ostrea carinata, Spatangus subglobosus,* &c.

The chalk marl in general gradually becomes more sandy, and passes into a sandstone with veins of oxide of iron, but which contains no fossils. The sandstone constantly accompanies the chalk marl.

e. Gault.—This deposit has not been detected with certainty in the north of Germany, but M. Roemer thinks it may be represented by a marl which occurs between Hanover and Hildesheim, and contains *Hamites compressus;* and by a blue clay near Ottbergen. He has not been able to find it near Aix-la-Chapelle.

f. Lower greensand.—The mineralogical characters of this formation are the same as in England. It occurs in Saxony, at the foot of the Hartz, near Celfeld, near Bilefeld, and near Hattern in Westphalia, also near Aix-la-Chapelle. Its fossils are not very numerous.

g. Hils conglomerate.—To M. Roemer, geologists are indebted for first pointing out the existence, as distinct deposits, of the Hils conglomerate and the Hils clay.

The *Hils conglomerate* consists of a yellowish or brownish marl, containing grains of quartz, schist, and oxide of iron. It forms very thick beds, and includes in some localities very rich iron ores. It occurs near Brunswick, at Goslai, and near Essen on the Ruhr in Westphalia. Its fossils are very numerous, and partly identical with those of the lower greensand of England; for example, it contains *Terebratula latissima, T. depressa, T. oblonga, T. sella, Ostrea carinata, O. macroptera, Pecten quinquecostatus,* &c. M. Roemer asks if it be the equivalent of the Neocomien.

h. The *Hils clay* is a bluish pure clay, 100 feet thick. M. Roemer considers it to be the Speeton clay of England, as it contains *Mya depressa* (Phillips), *Glyphæa ornata* (*Astacus ornata,* Phillips), and *Isocardia angulata* (Phillips), with a great quantity of other fossils, which are in part Jurassic species, namely, *Ammonites sublævis, A.*

mutabilis, and *A. coronatus*. M. Roemer has noticed the Hils clay near Hildesheim, near Celfeld, where it contains a considerable layer of iron; at the foot of the Deister near Hanover, and near Trenndorf.

i. Weald clay.—A stiff bluish or brownish clay, seldom containing subordinate beds of limestone and sandstone. The fossils are almost exclusively the same as those enumerated in Dr. Fitton's memoir on the beds below the chalk in the south-east of England; and are entirely freshwater, with the exception of an Astarte.

k. Hastings sandstone.—In the north of Germany this formation is composed of a white, grey or fawn-coloured sandstone, sometimes alternating in the upper part with greyish clay, and contains from seven to ten beds of coal. Its general thickness amounts to 500 or 800 feet. The beds of coal vary from one to three feet in thickness, and are separated by sandstone, which is sometimes only a few feet thick. The fossils belong to the genera *Paludina, Unio, Endogenites, Abies, Sphenopteris,* and *Lonchopteris,* and M. Roemer has found every species mentioned in Dr. Fitton's memoir before alluded to. The sandstone is generally less ferruginous than in England.

l. Purbeck strata.—These beds are described by M. Roemer as consisting of shelly limestones alternating with thin layers of sandstone, and concretional masses of grit. He has observed two dirt-beds, but as yet no Cycadeoidea. The shells which he has found, are partly marine, partly freshwater, and belong to the genera *Paludina, Ostrea, Cyrena, Gervilia, Serpula,* &c.

m. Portland stone.

The Wealden formation, M. Roemer states, is exhibited near Helmsted. He hopes it will be exposed near Hildesheim. More westward it extends from Hanover by Minden, to Iburg and Rheine near Munster in Westphalia; furnishing almost everywhere a very good coal. The fossils found in the strata below the lower greensand, M. Roemer has accurately figured and described in his works; and they proved the identity of the Wealden deposits of England and the north of Germany.

A letter from H. B. Mackeson, Esq. to Dr. Fitton, dated Hythe, June 7th, 1840.

On the 19th of May, Mr. Mackeson discovered some portions of a large saurian, he believes an Iguanodon, near the bottom of the lower greensand in the vicinity of Hythe. On the 6th of June, he revisited the quarry, and ascertained that the work-people had followed the remains for upwards of fifteen feet. On that occasion, Mr. Mackeson superintended the disinterment of probably a tibia. This bone, and others previously obtained, with their bulky matrix, required a cart and two horses for their removal. Up to the date of the letter, no vertebræ, teeth, phalangeal or smaller bones of the extremities had been found by the workmen. In the same quarry, Mr. Mackeson has obtained a large Ammonite, *Gervillia aviculoides*, and other shells characteristic of the lower greensand.

Nov. 4.—A paper was read on Glaciers, and the evidence of their having once existed in Scotland, Ireland, and England, by Professor Agassiz, of Neuchâtel.

M. Agassiz commences by observing, that the study of glaciers is not new, as Scheuchzer visited, and even drew, most of the glaciers of Switzerland; and as, at a later period, Gruner and De Saussure examined them in great detail, and left few of their phænomena uninvestigated. Hugi also, in his account of the Alps, and Scoresby, in his descriptions of the arctic regions, have communicated much valuable information respecting glaciers, but without giving rise to any important geological results. Venetz and De Charpentier first ascribed to the agency of glaciers, the transport of the erratic boulders of Switzerland, supposing that the Alps formerly attained a greater altitude than at present, and that the glaciers extended to the plains of Switzerland, and even to the Jura. This assumed greater height of the Alps M. Agassiz dissents from, as no geological phænomena compel him to admit it; and the arrangement of the boulders proves that the blocks were not pushed forward by the glaciers, as conjectured by M. de Charpentier. Moreover, the phænomena of erratic boulders extend over all the temperate and northern regions of Europe, Asia and America, and, consequently, could not depend upon so local an event as a greater altitude of the Alps. The consideration of these difficulties induced M. Agassiz to resume the study of glaciers; and after devoting the suitable portion of five successive summers to the study of their details, and all that has been written respecting their structure, he has arrived at the conviction, that the formation of glaciers did not only depend upon the actual configuration of the globe, but was also connected with the last great geological changes in its surface, and with the extinction of the great mammifers which are now found in the polar ice. He is also convinced that the glaciers did not advance from the Alps into the plains, but that they gradually withdrew towards the mountains from the plains which they once covered. In this belief, he says, he is supported by many considerations which escaped previous observers, depending chiefly on the form and relative position of the erratic blocks, and the commonly called diluvial gravel, the former being in Switzerland always angular, and resting on the latter, which consists of rounded materials. Considered in this point of view, glaciers assume an entirely new importance, for they introduce a long period of intense cold between the present epoch and that during which the animals existed, whose remains are buried in the usually called diluvial detritus.

Having established his theory as completely as he could, by repeated investigations of Switzerland and the adjacent portions of France and Germany, M. Agassiz became desirous of investigating a country in which glaciers no longer exist, but in which traces of them might be found. This opportunity he has recently enjoyed, by examining a considerable part of Scotland, the north of England, and the north, centre, west and south-west of Ireland; and he has arrived at the conclusion, that great masses of ice, and subsequently glaciers, existed in these portions of the United Kingdom at a period immediately preceding the present condition of the globe, founding his belief upon the characters of the superficial gravels and erratic blocks, and on the polished and striated appearance of the rocks *in situ*.

M. Agassiz does not suppose that his views respecting glaciers will at once meet with the general concurrence of geologists; and he admits that the study of the phænomena of glaciers in different latitudes, as well as at different altitudes, together with the examination of their different effects where in contact with the sea, will introduce many modifications in the consideration of analogous phænomena in countries where glaciers have disappeared; but he is prepared to discuss his theory within the limits of observed facts, conscious of having searched for truth solely to advance the interests of science.

To avoid useless discussion, he states, that in attributing to the action of glaciers a considerable portion of the results hitherto ascribed exclusively to that of water, he does not wish to maintain that everything hitherto assigned to the agency of water has been produced by glaciers; he only wishes that a distinction may be made in each locality between the effects of the different agents; and he adds, that long-continued practice has taught him to distinguish easily, in most cases, the effects produced by ice from those produced by water.

Proceeding to the consideration of facts, he says the distribution of blocks and gravel, as well as the polished and striated surfaces of rocks *in situ*, do not indicate the action of a mighty current flowing from north-west to south-east, as the blocks and masses of gravel everywhere diverge from the central chains of the country, following the course of the valleys. Thus in the valleys of Loch Lomond and Loch Long, they range from north to south; in those of Loch Fine and Loch Awe from north-west to south-east; of Loch Etine and Loch Leven from east to west; and in the valley of the Forth from north-west to south-east, radiating from the great mountain masses between Ben Nevis and Ben Lomond. Ben Nevis in the north of Scotland, and the Grampians in the south, are considered by the author to constitute the great centres of dispersion in that kingdom; and the mountains of Northumberland, Westmoreland, Cumberland, and Wales; as well as those of Ayrshire, Antrim, the west of Ireland, and Wicklow, to be other points from which blocks and gravel have been dispersed, each district having its peculiar debris, traceable in many instances to the parent rock, at the head of the valleys. Hence, observes M. Agassiz, it is plain the cause of the transport must be sought for in the centre of the mountain ranges, and not from a point without the district. The Swedish blocks on the coast of England do not, he conceives, contradict this position, as he adopts the opinion that they may have been transported on floating ice.

In describing the phænomena presented by erratic blocks and gravel, M. Agassiz first insists upon the necessity of distinguishing between stratified gravel and mud containing fossils, which could not have been accumulated by true glaciers, although the materials may have often been derived from them; and unstratified masses, composed of blocks, pebbles, and clay. These stratified deposits he considers to be of posterior origin to the glacier epoch. The till of Scotland, or the great unstratified accumulation of mud and gravel, containing blocks of different size heaped together without order, and containing no organic remains but bones of Mammalia and insignificant fragments of shells, he is of opinion was also not produced by true glaciers, although intimately connected with the phænomena of ice. The polished and striated surfaces of the blocks leave no doubt on M. Agassiz's mind that these masses have been acted upon by ice in the same manner as the blocks which are observed under existing glaciers, and which are more or less rearranged by water derived from the melting of the glaciers.

Similar detritus fills the bottom of all the Alpine valleys, as that of the Rhone from its mouth to its junction with the Lake of Geneva, and the valley of Chamounix: it is found between the Hospice de Grimsel and the borders of the lower glacier of the Aar; thence to the neighbourhood of Goutharen in the valley of Oberhasli, at Im Grund, in the plains of Meiringen, and in Interlasken; also between Thun and Berne. At all these localities, M. Agassiz considers, the blocks were left, when the glaciers extended to them.

With respect to the valley of the Aar, M. Agassiz says it is easy to prove that the rounded pebbles of Alpine rocks spread along its whole course, were not transported to their present position by that river, because between the glacier from which it issues and Berne, the flowing of the stream is interrupted by the barrier of Kirchet, the Lake of Brienz, and the Lake of Thun; and because between these lakes its velocity is so small, that it transports only mud and very fine gravel, and that the pebbles over which the river flows below Thun do not issue from the lake. Supposing that the volume of the Aar was formerly greater, why, asks M. Agassiz, are not the lakes of Brienz and Thun filled in the same manner as the plain of Meiringen and the bottom of the valley which separates the two lakes? All difficulties, however, he is of opinion, vanish, if the pebbles be considered the detritus of retreating glaciers, and that the hollows occupied by the lakes of Brienz and Thun were filled with glaciers.

The existence of a glacier in this valley is not imagined by the author to explain the origin of the detritus, as its having existed is proved by the polish on the rocks *in situ*, from the glacier of the Aar to Meiringen, a distance of twenty English miles, at the height of 8000, 7000, and 6000 feet successively above the level of the sea; and even on the shores of the Lake of Thun. Similar phænomena have been noticed by M. Agassiz in Scotland, in the valleys of Loch Awe and Loch Leven, near Ballachalish, and in England in the neighbourhood of Kendal.

The author then proceeds to describe the moraines of Switzerland, or the accumulations of blocks and pebbles deposited longitudinally on the borders, and transversely in front, of glaciers, and successively abandoned by them in their retreat. The longitudinal moraines differ from glacier-detritus remodelled or spread out by water, in being disposed in ridges with a double talus, one flank of which is presented to the glacier, and the other to the side of the valley; and their continuity and parallelism at the same height easily distinguish them from the debris disposed along the bottoms of valleys by currents. They occur on the flanks of all glaciers, but they have been also observed by M. Agassiz where no glaciers exist, as in the valleys of the Rhone, the Arve, the Aar, &c.; likewise in Scotland, near Inverary, at Muc Airn, at the outlet of Loch Traig, at Strankaer, and on the borders of the bay of Beauley; in Ireland to the south-east of Dublin, and near Enniskillen; and in England in the valley of Kendal, as well as near Penrith and Shap.

The common origin of moraines, and of accumulations of rounded pebbles and of blocks, M. Agassiz says, cannot be doubted. The former are simple ridges formed on glaciers; the latter, materials rounded and polished under glaciers, or great masses of ice, and exposed by the melting of the ice, and re-disposed by the water thus produced.

The author next describes the differences in the internal arrangement of the various accumulations. In the stratified deposits the materials are comparatively much smaller than in glacier-detritus; the pebbles also are elongated, and fine gravel and mud ordinarily form the upper beds. On the contrary, in the detritus of glaciers large and small materials are associated without order, the largest blocks being often in the upper part; and where very large *angular* blocks occur, they rest on the surface. In moraines there is a further distinction, blocks of all dimensions and every form are intermingled; and this difference, he says, is easily understood, by recollecting that moraines are composed of the angular blocks which fall on the surface of the glacier, as well as of pebbles with rounded edges.

The striated and polished surfaces, so often observed on solid rocks *in situ*, are next described by M. Agassiz. Without denying absolutely the power of water to produce such effects, he says that he has sought for them in vain on the borders of rivers and lakes, and on sea-coasts; and that the effects produced by water are sinuous furrows proportioned to the hardness of the rocks; not even uniform polished surfaces, such as those presented by the rocks under discussion, and which are independent of the composition of the stone; moreover wherever the moveable materials which are pressed by the ice on rocks *in situ* are the hardest, there occur, independent of the polish, striæ more or less parallel, and in the general direction of the movement of the glaciers. Thus in the neighbourhood of glaciers are found those rounded bosses which Saussure distinguishes by the name of "roches moutonnés." These phænomena M. Agassiz has traced under the glacier of the Aar, and he has observed them in the valley of the Rhone, and of Chamounix; also in Scotland, on the banks of Loch Awe and Loch Leven; and he says they are very remarkable in the environs of Kendal.

The most striking points in the distribution of the striæ, are their diverging at the outlets of the valleys, and their being oblique, and never horizontal on the flanks, which they would be, were they due to the agency of water, or floating masses of ice. The cause of this obliquity the author assigns to the upward expansion of the ice, and the descending motion of the glacier.

The most remarkable striated rocks in the Alps are near Handeck, and near the cascade of Pissevache; and the best examples M. Agassiz has seen in Scotland, are those of Ballahulish, and in Ireland those of Virginia.

If the analogy of the facts which he has observed in Scotland, Ireland, and the north of England, with those in Switzerland, be correct, then it must be admitted, M. Agassiz says, that not only glaciers once existed in the British Islands, but that large sheets (*nappes*) of ice covered all the surface.

The author then details the proofs that glaciers did not descend from the mountain summits into the plains, but are the remaining portions of the sheets of ice which at one time covered the flat country. It is evident, he says, if the glaciers descended from high mountains, and extended forward into the plains, the largest moraines ought to be the most distant, and to be formed of the most rounded masses; whereas the actual condition of the detrital accumulations is the reverse, the distant materials being widely spread, and true moraines being found only in valleys connected with great chains of lofty mountains.

It must then be admitted, the author argues, that great sheets of ice, resembling those now existing in Greenland, once covered all the countries in which unstratified gravel is found; that this gravel was in general produced by the trituration of the sheets of ice upon the subjacent surface; that moraines, as before stated, are the effects of the retreat of glaciers; that the angular blocks found on the surface of the rounded materials were left in their present position at the melting of the ice; and that the disappearance of great bodies of ice produced enormous debacles and considerable currents, by which masses of ice were set afloat, and conveyed, in diverging directions, the blocks with which they were charged. He believes that the Norwegian blocks found on the coast of England have been correctly assigned by Mr. Lyell to a similar origin.

Another class of phænomena connected with glaciers, is the forming of lakes by the extension of glaciers from lateral valleys into a main valley; and M. Agassiz is of opinion, that the parallel roads of Glen Roy were formed by a lake which was produced in consequence of a lateral glacier projecting across the glen near Bridge Roy, and another across the valley of Glen Speane. Lakes thus formed naturally give rise to stratified deposits and parallel roads, or beds of detritus at different levels.

The connexion of very recent stratified deposits with glacier-detritus, M. Agassiz observes, is difficult to explain; but he conceives that the same causes which can bar up valleys and form lakes, like those of Brienz, Thun and Zurich, may have formed analogous barriers at the point of contact with the sea sufficiently extensive to have produced large salt-marshes to be inhabited by animals, the remains of which are found in the clays superimposed on the till of Scotland; and he adds, that the known arctic character of these fossils ought to have great weight with those who study the vast subject of glaciers.

In conclusion, the author remarks, that the question of glaciers forms part of many of the great problems of geology; that it accounts for the disappearance of the large mammifers inclosed in the polar ice, as well as for the disappearance of the organic beings of the so-called diluvian epoch; that in Switzerland it is associated with the elevation of the Alps, and the dispersion of the erratic blocks; and that it is so intimately mixed up with the subject of a general diminution of the terrestrial heat, that a more profound acquaintance with the facts noticed in this paper will probably modify the opinions entertained respecting it.

Nov. 18.—The reading of the first part of a Memoir on the Evidences of Glaciers in Scotland and the North of England, by the Rev. Prof. Buckland, D.D., Pres. G.S., commenced on the 4th of November, was resumed and concluded.

Dr. Buckland's attention was first directed by Prof. Agassiz in October 1838 to the phænomena of polished, striated, and furrowed surfaces on the south-east slope of the Jura, near Neuchâtel, as well as to the transport of the erratic boulders on the Jura, as the effects of ice; but it was not until he had devoted some days to the examination of actual glaciers in the Alps, that he acquiesced in the correctness of Prof. Agassiz's theory relative to Switzerland. On his return to Neuchâtel from the glaciers of Rosenlaui and Grindelwald, he informed M. Agassiz that he had noticed in Scotland and England phænomena similar to those he had just examined, but which he had attributed to diluvial action: thus in 1811 he had observed on the head rocks on the left side of the gorge of the Tay, near Dunkeld, rounded and polished surfaces; and in 1824, in company with Mr. Lyell, grooves and striæ on granite rocks near the east base of Ben Nevis. About the same time Sir George Mackenzie pointed out to the author in a valley near the base of Ben Wyvis, a high ridge of gravel, laid obliquely across, in a manner inexplicable by any action of water, but in which, after his examination of the effects of glaciers in Switzerland, he recognizes the form and condition of a moraine.

After these general remarks, Dr. Buckland proceeds to describe the evidence of glaciers observed by him in Scotland last autumn, partly before and partly after an excursion, in company with Prof. Agassiz; but he forbears to dwell on the phænomena of parallel terraces, though he is convinced that they are the effects of lakes produced by glaciers.

The district which Dr. Buckland examined previously to his excursion with Prof. Agassiz, lay in the neighbourhood of Dumfries; and the line of country which he investigated subsequently, extended in Scotland from Aberdeen to Forfar, Blair Gowrie, Dunkeld, and by Loch Tumel and Loch Rannoch to Schiehallion and Taymouth, and thence by Crief, Comrie, Loch Earn Head, Callender and Stirling, to Edinburgh; and in England by Berwick, Wollar, the Cheviots, Penrith, and Shap Fell, to Lancashire and Cheshire.

Moraine near Dumfries.—The picturesque ravine of Crickhope Linn, about two miles north of Closeburn, and one mile east of Thornhill, intersects nearly horizontal strata of new red sandstone, and is traversed by the Dolland rivulet. On emerging from the upper end of the ravine a long terminal moraine is visible, stretching nearly across the mountain valley, from which the Dolland Burn descends to fall into Crickhope Linn; and it resembles, when viewed from a distance, a vallum of an ancient camp, being covered with turf. It is formed principally of an unstratified mass of rolled pebbles, derived from the slates of the adjacent Lowder Hills, with a few rounded fragments of granite, the nearest rock of which *in situ* is that of Loch Doon, in Galloway, thirty miles to the north-west. Its height varies from twenty to thirty feet; its breadth at the base is about one hundred feet, and its length is four hundred yards. At the southern extremity it is traversed by the Dolland rivulet, and at the northern by the Crickhope Water; and in the centre it is intersected by a road.

Moraines in Aberdeenshire.—Dr. Buckland considers the gravel and sand which cover the greater part of the granite table-land from Aberdeen to Stonehaven to be the detritus of moraines; and the large insulated tumuli and tortuous ridges of gravel, occupying one hundred acres, near Forden, a mile east of Achinbald, to be terminal moraines; also the blocks, large pebbles, and small gravel spread over the first level portions of the valley of the North Esk, after emerging from the Sub-Grampians, to be the residue of moraines re-arranged by water.

Moraines in Forfarshire.—The cones and ridges of gravel at Cortachy and Piersie, near Kirriemuir, and at the confluence of the Carity valley with that of the Proson, are considered by Dr. Buckland to have been produced by glaciers, and modified in part subsequently by water. The polish and striæ on a porphyritic rock near the summit of the hill, on the left side of the main valley, and immediately above the moraines, he is of opinion must also be assigned to glacier action. The vast longitudinal and insulated ridges of gravel, extending for two or three miles up the valley east of Blair Gowrie, and the transverse barriers forming a succession of small lakes in the valley of the Lunanburn, to the west of that town, he considers to be moraines; likewise the lofty mounds comprising the ornamental grounds adjacent to Dunkeld Castle; the detritus covering the left flank of the valley of the Tay, along a great part of the road from Dunkeld to Logierait; that on the left flank of the Tumel valley from Logierait to Killicrankie; and on the left flank of the Garrie, from Killicrankie to Blair Athol.

The vast congeries of gravel and boulders on the shoulder of the mountain, exactly opposite the gorge of the Tumel, Dr. Buckland is of opinion was lodged there by glaciers which descended the lateral valley of the Tumel from the north side of Schichallion and the adjacent mountains, and were forced across the valley of the Garry, in the same manner as modern glaciers of the Alps (that of the Val de Bagne, for example,) descend from the transverse, and extend across the longitudinal valleys. Dr. Buckland mentions the mammillated, polished and striated slate rocks, about one mile above the falls of the Tumel, on the left portal of the gorge of the valley, as the effects of a glacier which descended the gorge: he notices also the rounded outline and polish on veins of quartz, which project eight or ten inches above the weathered surfaces of masses of mica slate near the same locality. Similar mammillated masses of mica slate retaining striæ and flutings are visible at Bohaly, one and a half miles east of Tumel Bridge.

Evidences of Glaciers on Schiehallion.—The north and north-east shoulders of the mountain present rounded, polished, and striated surfaces, many of which have been recently exposed by the construction of new roads. On the left flank of the valley called the Braes of Foss, and near the thirteenth milestone, a newly-exposed porphyry dyke, forty feet wide, exhibited a polished surface and striated, parallel to the line of descent which a glacier from Schiehallion would take; and on the right flank, one hundred yards north of the eleventh milestone, another and smaller dyke of porphyry presented similar phænomena. In the intermediate space the recently uncovered slate rocks and quartzite are rounded, polished, grooved, and striated, parallel to the direction which a glacier would assume where each surface is situated.

Moraines at Taymouth.—Two lofty ridges of gravel, which cross the park at right angles to the sides of the valley between the village of Kenmore and Taymouth Castle, the hill, on which stands an ornamental dairy-house, and the gravel, on which are situated the woods overhanging the left bank of the lower end of Loch Tay, Dr. Buckland considers to be moraines, or the detritus of moraines; also the deeply-scored and fluted boulders of hornblende rock, with other debris near Fortingal, at the junction of Glen Moulin with Glen Lyon.

Moraines in Glen Cofield.—A remarkable group of moraines occurs on the high lands which divide the valleys of the Tay and the Bran; and between the sixteenth and fourteenth milestones thirty or forty round-topped moraines, from thirty to sixty feet high, are crowded together like sepulchral tumuli. These mounds, composed of unstratified gravel and boulders, Dr. Buckland says cannot be referred to the action of water, as they are placed precisely where a current descending from the adjacent high lands would have acted with the greatest velocity; and they exactly resemble some of the moraines in the valley of the Rhone, between Martigny and Löek. The village of Amulrie is considered by the author to stand on a group of low moraines; and the road for two or three miles from it, towards Glen Almond, to traverse small moraines or surfaces of mica slate, rounded by glaciers. A few conical moraines appear also on the high lands between Glen Almond and Crieff.

Proofs of Glaciers in and near Strath Earn.—This part of the valley of the Earn is flanked irregularly with ridges and terraces of gravel, the detritus of moraines; and on its north side, in the woods adjacent to Lawers House, near Comrie, hard slaty rocks of the Devonian or old red sandstone system have been rounded and striated. At the west end of Comrie, near the bridge, blue slate rocks have been also rounded and guttered.

Evidence of Glaciers near Comrie.—In this district Dr. Buckland tested the value of the glacial theory by marking in anticipation on a map the localities where there ought to be evidences of glaciers having existed, if the theory were founded on correct principles. The results coincided with the anticipations. On a hill above the gorge, called the Devil's Caldron, near Fentallich, are rounded surfaces of greenstone, partially covered by moraines; and at Kenagart, also immediately above the Devil's Caldron, is a small cluster of moraines, easily separable into lateral and terminal. Two miles up the valley a medial moraine forms a ridge on the level ground, in front of the confluence of Glen Lednoch and Glen Garron. The farmhouse of Invergeldy is stated to stand on the detritus of a moraine, and the glen descending to it from Ben-na-cho-ny to be partially obstructed with moraines. The surface of the granite at Invergeldy, which supplied the stone for Lord Melville's monument at Crieff, is rounded and mammillated, but too much weathered to present a polish or striæ. On a hill of trap, however, half a mile south of the farm of Lurg, there is a distinct polish, striated in the direction which a glacier descending the subjacent valley would assume. In Glen Turret, on the shoulder of the mountain immediately above the south-west extremity of Loch Turret, a very deep ravine intersects a vast lateral moraine, which Dr. Buckland shows must have been lodged there whilst the Loch was a mass of ice, and the valley above it filled with a glacier more than five hundred feet above the present level of the lake. At the falls of the Turret, at the lower extremity of the gorge, is an extensive lodgement of moraines; and at the upper end, on the left bank of the Turret, near a gate which crosses the road, the slate-rocks are polished and furrowed; and at both these localities Dr. Buckland had anticipated that glacial action ought to be found.

Evidence of Glaciers near Loch Earn.—On the north bank of the Loch rounded and furrowed surfaces and portions of lateral moraines are exposed by the roadside; and at Loch Earn Head is a group of conical moraines at the junction of Glen Ogle with Loch Earn, and at the very point where, had they been brought by a rapid current, they would have been propelled into the Loch. It is nevertheless the exact position where the terminal moraine of a glacier would be deposited.

Moraines near Callender.—Moraines are stated to cover more or less the valley of the Teith from Loch Katherine to Callender, and the lofty terraces flanking the valley from Callender to Doune are considered to be the detritus of moraines, modified by the great floods which accompanied the melting of the ice. One of them, near Callender, has been mapped as the vallum of a Roman camp. The little lakes on the right bank of the Teith, four miles east of Callender, Dr. Buckland considers due to moraines obstructing the drainage of the country; and the greater part of the first table-land on the right bank of the Teith, between Callender and Doune, including the portion on which stands Mr. Smith's farm, to be composed of re-arranged glacial detritus.

Proofs of Glacial Action at Stirling and Edinburgh.—Having thus shown that glaciers once existed in the glens and mountainous districts of Scotland, Dr. Buckland proceeds to point out the evidence of glacial action at points but little raised above the level of the sea, and distant from any lofty group of mountains. In 1824 he had noticed that the trap-rock then recently exposed on the summit of the hill [at Stirling], between the castle and the church, was polished and striated, but at his last visit in 1840 these evidences had become obliterated by weathering. The grooves and scratches described by Sir James Hall on the Costorphine hills near Edinburgh, and on the surface of Calton Hill, Prof. Agassiz is of opinion cannot be explained by the action of water; but they resemble, he says, the effects produced by the under-surface of modern glaciers. In his recent examination, in company with Mr. McLaren, of the Castle Rock at Edinburgh, Dr. Buckland found further proofs of the correctness of the glacial theory, by discovering at points where he anticipated they would occur, namely, on the north-west angle of the rock, distinct striæ upon a vertical polished surface; and at its base a nearly horizontal portion of rock, covered with deep striæ; also on the south-west angle obscure traces of striæ and polished surfaces*. Some of these effects may be imagined to have been produced by stones projecting from the sides or bottom of floating masses of ice; but it is impossible, Dr. Buckland observes, to account by such agency for the polish and striæ on rocks at Blackford Hill, two miles south of Edinburgh, pointed out to him by Lord Greenock in 1834. On the south face of this hill, at the base of a nearly vertical cliff of trap, is a natural vault, partly filled with gravel and sand, cemented by a recent infiltration of carbonate of lime. The sides and roof of the vault are highly polished, and covered with striæ, irregularly arranged with respect to the whole surface, but in parallel groups over limited extents. These striæ, Dr. Buckland says, cannot be referred to the action of pebbles moved by water; 1st, because fragments of stone set in motion by a fluid cannot produce such continuous parallel lines; and 2ndly, because if they could produce them, the lines would be parallel to the direction of the current: it is impossible, he adds, to refer them to the effects of stones fixed in floating ice, as no such

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* In October 1840, Mr. McLaren found a polished surface on a portion of rock near the south-west base of Arthur's Seat.

Dr. Buckland has in his possession lithographs copied from drawings made by Mr. James Hall, of distinct west and east furrows which extend over a portion of the north side of the summit of Calton Hill, and on the surface of the carboniferous sandstone at Craig Leith Quarry. Dr. Buckland saw similar dressings in 1824 in a sandstone quarry near the house of Lord Jeffrey, two miles west of Edinburgh; and in 1840, in a railway section at Bangholm Bower, one mile north-east of Edinburgh, he found in stratified till and sand many striated and fluted boulders.

masses could have come in contact with the roof of a low vault. On the contrary, it is easy, he says, to explain the phænomena of the polish by the long-continued action of fragments of ice forced into the cave laterally from the bottom of a glacier descending the valley, on the margin of which the vault is placed; and the irregular grouping of the parallel striæ to the unequal motion of different fragments of ice, charged with particles of stone firmly fixed in them, like the teeth of a file. The cave is not three hundred feet above the level of the sea, and the proving of glacial action at this point justifies, the author states, the belief that glaciers may also at that period have covered Calton Hill and the Castle Hills of Edinburgh and Stirling.

A paper "On the Geological Evidence of the former existence of Glaciers in Forfarshire," by Charles Lyell, jun., Esq., F.R.S., F.G.S., was commenced.

Dec. 2.—Mr. Lyell's memoir "On the Geological Evidence of the former existence of Glaciers in Forfarshire," commenced on the 18th of November, was concluded.

Three classes of phænomena connected with the transported superficial detritus of Forfarshire, Mr. Lyell had referred, for several years, to the action of drifting ice; namely, 1st, the occurrence of erratics or vast boulders on the tops and sides of hills at various heights, as well as in the bottoms of the valleys, and far from the parent rocks; 2ndly, the want of stratification in the larger portion of the boulder formation or till; and 3rdly, the curvatures and contortions of many of the incoherent strata of gravel or of clay resting upon the unstratified till*. When, however, he attempted to apply the theory of drifting ice over a submerged country to facts with which he had been long acquainted in Forfarshire, he found great difficulty in accounting for the constant subterposition of the till with boulders to the stratified deposits of loam and gravel; for the till ascending to higher levels than the gravel, and often forming mounds which nearly block up the drainage of certain glens and straths; for its constituting, with a capping of stratified matter, narrow ridges, which frequently surround lake-swamps and peat-mosses; and for the total absence of organic remains in the till.

Since, however, Professor Agassiz's extension to Scotland of the glacial theory, and its attendant phænomena, Mr. Lyell has re-examined a considerable portion of Forfarshire, and having become convinced that glaciers existed for a long time in the Grampians, and extended into the low country, many of his previous difficulties have been removed. There are, nevertheless, facts connected with the ridges of stratified materials resting upon till, which he is unable to explain. He also states, that though he had for years inferred from the evidence of fossil shells sent to him from Canada by Capt. Bayfield, that the climate of North America, in the latitude of Que-

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* See Mr. Lyell's paper on the Norfolk Drift, Phil. Mag., May 1840, [Third Series, vol. xvi. p. 345,] and the Abstract of the paper in the Proceedings of the Society, vol. iii. p. 171.

bec, was far more intensely cold at one period than it is now*, yet, that his thoughts had been diverted from the consideration of a long-continued covering of snow on the Scottish mountains, by the knowledge that the climate of Great Britain during the several tertiary epochs was warmer than it is at present. He is of opinion that, during a period immediately antecedent to the existing, several oscillations of temperature may have occurred in the northern hemisphere.

Forfarshire, Mr. Lyell divides geologically into three principal districts: 1st, the Grampians, composed of granite, gneiss, mica-slate, and clay-slate, flanked by a lower range of vertical beds of old red sandstone, associated with trap; 2ndly, the great synclinal trough of Strathmore, occupied by the middle and newer members of the old red sandstone; and 3rdly, the anticlinal chain of the Sidlaw Hills, consisting of the inferior or grey beds of the old red sandstone, usually accompanied by trap. He further states, that it represents, on a small scale, both geologically and physically, the portion of Switzerland where erratic blocks are most abundant, the Grampians with their crystalline rocks being comparable to the Alps, the secondary chain of the Sidlaw Hills to the Jura, and Strathmore to the great valley of Switzerland; and that the resemblance is increased by the occurrence in Strathmore and on the Sidlaw Hills of angular and rounded blocks of Grampian rocks.

The superficial detritus of Forfarshire, Mr. Lyell divides into three deposits: 1st, the thin unstratified covering on the Grampians, derived from the disintegration of the subjacent strata, with a slight intermixture of pebbles traceable to rocks at a higher level, not far distant; 2ndly, the unstratified materials enclosing boulders which occur at the base of the hills on both sides of every glen, and not due to taluses formed by landslips, but constituting terraces of transported debris, with a nearly flat top, and sometimes with two steep sides, one towards the river, and the other of less height towards the mountain; and 3rdly, the stratified gravels, sands and clays which overlie the unstratified detritus. Mr. Lyell confines his observations principally to the second and third divisions.

The terraces or lateral mounds very generally increase in width and depth as they descend from the higher to the lower glens, attaining in the latter sometimes a thickness of 100 feet, and occasionally so great a breadth as to leave only sufficient room for the river to pass. The inferior part is always unstratified, consisting of mud and sand, in which large angular and rounded fragments of rocks are imbedded. These boulders are more and more rounded as their distance increases from the hills whence they could have been detached; but they are more frequently flat-sided than pebbles which have been rounded by water; and they become more diversified in character by the junction of every tributary glen. In the upper part the mounds often consist of 40 to 80 feet of the same materials as the lower, but regularly stratified. Mr. Lyell then

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* See Proceedings, vol. iii. p. 119 [or L. & E. Phil. Mag. vol. xv. p. 399].

proceeds to illustrate his subject by describing in detail the phænomena presented by the valley of the South Esk and those of its tributaries.

The South Esk springs from a shallow lake nearly 3000 feet above the level of the sea, and twenty miles from Strathmore. For six miles the river flows through a district composed partly of gneiss, traversed by veins of granite or eurite, and partly of granite. The fragments derived from this high region may be traced downwards continuously for twelve miles to Cortachie; and as a proof that the detritus forming the lateral mounds has followed the same downward course, Mr. Lyell states that it preserves throughout, as well in the main as in the lateral glens, an uniformly grey colour; while the detritus of the lower zone of mica-slate is invariably tinged red, this colour being also imparted to the debris of the still lower portions of the glens, notwithstanding the intermixture of pale brown materials obtained from the clay-slate of that district. Another proof of the detritus not having been drifted upwards, is the absence in the higher portions of the glens of the blocks of pure white quartz which abound in the region of mica-schist, and have been derived from the numerous veins and beds of quartz belonging to that formation. The chief exception to this arrangement is a boulder of conglomerate in the bed of the Proson, evidently derived from hills two miles to the south, but which are considerably above the level of the glen. A few other similar exceptions have been noticed, but the distances to which the stragglers have been traced are inconsiderable. The phænomena exhibited by the lateral mounds, Mr. Lyell states, agree well with the hypothesis of their being the lateral moraines of glaciers; and he adds, that he had never been able to reconcile these phænomena, particularly the want of stratification, with the theory of the accumulations of the detritus during submergence, and the removal by denudation of the central portions of a deposit which had by that means filled the glens. The distribution of an enormous mass of boulders on the southern side of Loch Brandy, and clearly derived from the precipices which overhang the Loch on the three other sides, is advanced as another proof in favour of the glacial theory. It is impossible to conjecture, Mr. Lyell says, how these blocks could have been transported half a mile over a deep lake; but let it be imagined that the Loch was once occupied by a glacier, and the difficulty is removed. Loch Whorral, about a mile to the east of Loch Brandy, is also surrounded on its north, east and western sides by precipices of gneiss, and presents on its southern an immense accumulation of boulders with other detritus, strewed over with angular blocks of gneiss, in some instances twenty feet in diameter. This moraine is several hundred yards wide, and exceeds twenty feet in depth, terminating at the borders of the plain of Clova in a multitude of hillocks and ridges much resembling in shape some terminal moraines examined by Mr. Lyell in Switzerland.

The great transverse barrier at Glenairn, where the valley of the South Esk contracts from a mile to half a mile in breadth, and is flanked by steep mountains, Mr. Lyell formerly regarded as very difficult of explanation. Seen from below, this barrier resembles an artificial dam 200 feet high, with numerous hillocks on its summit. On the eastern side it appears to have been denuded to the extent of about 300 yards by the Esk. Its breadth from north to south is about half a mile. The lower part, 30 feet in depth, laid open in the river cliff, consists of impervious, unstratified mud, full of boulders; but the total vertical thickness of this deposit is stated to be from 50 to 80 feet; and the upper part of the barrier is composed of from 50 to 100 feet of very fine stratified materials. It is not possible, Mr. Lyell observes, to account for the accumulation of this barrier by the agency of water, particularly as no tributary joins the Esk at this point; but if the barrier be supposed to be the large terminal moraine of a receding glacier, then its form and position, he says, are easily to be understood. M. Agassiz, in his work on glaciers, shows, that when these masses of ice enter a narrow defile from a broader valley, the lateral moraines are forced towards the centre, and the mass of transported matter is spread more uniformly over the whole. Such a terminal moraine left by a receding glacier in a defile, Mr. Lyell states, would dam back the waters of the glacier, and produce a lake; and the phænomena presented by the barrier of Glenairn, and the plain which extends in its rear, are fully explicable on the assumption of their having been produced by a glacier. The stratification of the upper portion of the barrier is also shown to be partly in accordance with the effects produced by the formation of ponds of water on the surface of moraines; but Mr. Lyell states, that the accumulation of so great a capping of stratified materials is still the most obscure character of the deposits under consideration.

At Cortachie, about four miles below the barrier of Glenairn, the South Esk enters the country of old red sandstone, and a mile and a half lower it is joined by the Proson, and a mile yet lower by the Carity. In the district in which these streams unite there is a considerable thickness of unstratified matter full of Grampian boulders, and covered for the greater part with stratified gravel and sand. In some cases the latter exhibit the diagonal laminæ common in subaqueous formations; and in others the strata are so contorted, that a perpendicular shaft might intersect the same beds three times. In the latter instances the surface of the subjacent red boulder clay has not partaken of the movement by which the stratified deposit was contorted; and in consequence Mr. Lyell ascribed the effect, when he first beheld it in 1839, to the lateral pressure of large masses of drifted ice repeatedly stranding upon a shoal of soft materials*. In the middle of the tract between the South Esk and the Proson is a dry valley, and to the south of this valley, near the Proson, an excavation was made ten years ago, which exposed extremely contorted beds overtopped by others perfectly horizontal, having been formed by tranquil deposition after the disturbance of strata previously deposited. The phænomena exhibited by the till in this

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* See Proceedings, vol. iii. p. 178.

district, Mr. Lyell conceives, might be well accounted for by supposing the union of three or four large glaciers; but he considers it difficult to explain the accumulation of the overlying stratified materials, the top of which must be 600 feet above the level of the sea, and facing the Strath. In following out the narrow ridge which intervenes between the Proson and the Carity, during last October, in company with Dr. Buckland, the latter drew the author's attention to a spot half a mile south-west of the House of Pearsie, where the surface of a porphyry rock was polished, furrowed, and scratched. The quarrymen of Forfarshire also state as a general fact, that rocks of sufficient hardness, when first laid bare, are smooth, polished and scored; and Mr. Blackadder has found on the Sidlaw Hills large boulders of sandstone grooved and polished. Another general fact mentioned by Mr. Lyell is, that the unstratified boulder-clay becomes more and more impervious in the lower part of the Grampian glens, not in consequence of the influx of distinct materials, but in the author's opinion of the grinding down by the ice of the mud and other detritus.

Mr. Lyell then describes the phænomena of the second district, or Strathmore. Though this district may be considered as one great strath, yet it is divided into many longitudinal ridges and valleys. The former, sometimes 300 feet in height, are for the greater part parallel to the strike of the old red sandstone, and are generally covered to the depth of sixty or more feet with till and erratics, derived from the Grampians and the subjacent strata. This covering is so general, that the structure of the district can be detected only in the ravines through which the principal rivers pass. The till constitutes invariably the oldest part of the detritus. The boulders which it contains sometimes exceed three feet in diameter: on the north muir of Kerriemuir is a block of trap-rock, six feet by five feet, and near it is a mass of mica-schist, nine feet long by four feet wide and three high. The till has been ascertained by Mr. Blackadder to fill, in many places, deep hollows in the sandstone, which would become lakes or peat-mosses if the till were extracted. This distribution of the detritus, Mr. Lyell observes, may be explained on the supposition that, if the cold period came on slowly, the advance of the glaciers would push forward the detritus accumulated at their termination, and fill up, wholly or in part, the lakes or other cavities which they would encounter in their progress. Along most of the river courses, and in the lowest depressions of Strathmore, the till is covered by stratified sand and gravel.

One of the most remarkable peculiarities 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, to the south of Blairgowrie, then through the lowest part of Strathmore, and afterwards 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 of stratified gravel, sand, loam and clay, in some instances curved or contorted. The form of the included spaces is sometimes oval, sometimes quadrangular. The finest examples are in the lower tract, which has the Dean for its southern boundary, and the road from the bridge of Ruthven to the south of the grounds of Lindertis for its northern. The Grampian boulders are throughout the same; but there are associated with them masses of actinolite schist, which Mr. Blackadder has ascertained could be derived only from the valley of the Tay. The fragments of secondary rocks belong to the formations of the districts in which they occur. Though the country occupied by these marl-loch lakes is not traversed longitudinally by any river, yet it is so low, that if the transported matter were removed, a very slight depression would cause the sea to flow from Lunan Bay by Forfar to Blairgowrie and Dunkeld. Mr. Lyell therefore formerly conceived that an estuary might have extended in that direction, and that the till might have been drifted by masses of ice floated from the Grampians and contiguous hills. The overlying ridges of sand and gravel he thought might have been bars formed one after the other, in the same manner as the bar of sand and shingle, which now crosses the mouth of the Tay. The inland ridges of sand with boulders, which Mr. Lyell noticed in Sweden, and certainly produced under the sea, confirmed him in this view. These Swedish ridges are from fifty to several hundred yards broad, but sometimes so narrow on the top as to leave little more than room for a road; they are from fifty to a hundred feet high, and they may be often traced in unbroken lines for many leagues, ranging north and south. In his account of these ridges, in a memoir published in the Philosophical Transactions*, Mr. Lyell states his belief that they were thrown down at the bottom of the Gulf of Bothnia, in lines parallel to the ancient coast, and during the successive rise of the land. They usually consist of stratified sand and gravel, the layers being often at high inclinations; but where they are composed of boulders, no stratification is observable. After a long search, Mr. Lyell succeeded in finding shells in a layer of marl belonging to a ridge in the suburbs of Upsala, about twelve feet below the summit of the ridge, and eighty above the sea. The shells consisted of *Mytilus edulis*, *Cardium edule*, *Tellina Baltica*, *Littorina littorea*, and *Turbo ulvæ*, the most common species in the Baltic, and they constituted the greater part of the layer. On the summit of the ridge, at a short distance, he noticed angular masses of gneiss and granite, from nine to sixteen feet long, which had evidently been lodged when the ridge was submarine.

In Forfarshire Mr. Lyell never succeeded, as in the above case in Sweden, in finding marine shells in the ridges of sand; nor does he remember to have seen in Sweden transverse ridges at right angles to the north and south. The glacier theory, the author states,

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* 1835, pp. 15, 16.

appears to offer a happy solution of the problem of the marl-loch gravels, the longitudinal banks being regarded as lateral and medial moraines, and the transverse ridges as terminal. The chief objections are the stratification of the upper part of the banks, and the necessity of assuming a glacier thirty-four miles in length, with a fall of only 300 or 400 feet of country.

It has always appeared to Mr. Lyell and Mr. Blackadder remarkable, that the marl-loch gravels at Forfar are nearly 100 feet above the tract of till which separates them from the valley of South Esk, in Strathmore. In the present configuration of the country, water could not deposit the Forfar gravels without extending to the South Esk, the detritus of which is distinct, and separated by a low district of till without gravel. The only explanations of these phænomena Mr. Lyell considers to be either that the till is the moraine of a glacier, or that there has been a local change of relative levels of lands, by which the gravel of Forfar was uplifted, or the till to the northward depressed.

Another line of stratified detritus ranges at a higher level from the Loch of Lundie, along the Dichty Water, to the sea at Moray Firth, a distance of thirteen miles; and it is stated that many others might be enumerated. It is only on the coast to the east and west of Dundee, at heights varying from twenty to forty feet, that stratified clay and gravel have been found by Mr. Lyell to contain marine shells, all belonging to known existing species, except a *Nucula.* Although these remains prove a certain amount of upheaval subsequent to the deposition of the till, or to the commencement of the glacial epoch, including an equal movement in the interior, still Mr. Lyell objects to a general submergence of that part of Scotland, since the till and erratic blocks were conveyed to their present positions; as the stratified gravel is too partial and at too low a level to support such a theory; and he would rather account for the existence of the stratified deposits, by assuming that barriers of ice produced extensive lakes, the waters of which threw down ridges of stratified materials on the tops of the moraines. With respect to the geological age of the beds containing the marine shells, Mr. Lyell is of opinion that it is synchronous with that of the older of the recent formations on the Clyde, examined by Mr. Smith of Jordan Hill, and Mr. E. Forbes; and with respect to the age of the till and stratified gravel last formed, he is of opinion that it is very modern, because these accumulations constitute exclusively the dams of certain marl-lochs to the very bottom of the sediment formed, in which all the Testacea and skeletons of quadrupeds, as well as the remains of plants which have been found, are of existing species.

The third district, or that of the Sidlaw Hills, claimed Mr. Lyell's attention more particularly on account of the Grampian boulders with which it abounds. This range, whose greatest height is 1500 feet above the sea, is composed of anticlinal strata of grey sandstone, belonging to the old red sandstone, with associated trap. It is covered, as well as the whole of the country between Strathmore and the Tay, with the impervious till, containing Grampian boulders and fragments of the subjacent grey sandstone. The finest instances of erratics observed by Mr. Lyell occur on Pitscanly Hill, 700 feet, and the adjacent hill of Turin, 800 feet above the level of the sea. About forty feet below the summit, on the southern side of the former, is a block of mica-slate thirteen feet long, seven broad, and seven in height above the ground. Four smaller and equally angular masses, from three to six feet in diameter, lie close to its north end, as if severed from it. One of the nearest points at which this gneiss occurs *in situ*, is the Craig of Balloch, fifteen miles distant, on the northern extremity of the Creigh Hill, and between these points intervenes the great valley of Strathmore and the hills of Finhaven. Other Grampian boulders, from three to six feet in diameter, occur on the hills between Lumley Den and Lundie, at the height of 1000 feet; and Mr. Blackadder has found fragments of mica-schist one foot in diameter on the summit of Craigowl, the highest point of the Sidlaw Hills, and exceeding 1500 feet above the level of the sea.

In conclusion, Mr. Lyell offers some remarks on the conditions under which glaciers may have existed in Scotland, and the differences between them and those of the glaciers of Switzerland. He states that the glaciers of the latter country being situated 11° further to the south, they can present but an imperfect analogy with permanent masses of ice in Forfarshire, and that it is to South Georgia, Kerguelen's Land and Sandwich Land that we must look for the nearest approach to that state of things which must have existed in Scotland during the glacial epoch. In those regions of the southern hemisphere the ice reaches to the borders of the sea, and the temperature of summer and winter being nearly equalized, the glaciers probably remain almost stationary, like those of the Alps in winter, and can be diminished by only the first two of the three causes which tend to check an indefinite accumulation of snow in Switzerland; viz. 1st, evaporation without melting; and 2ndly, the descent of glaciers by gravitation, considered by M. Agassiz to be not very influential:—the third cause, the descent of glaciers arising from alternate liquefaction and freezing, he conceives must be wholly suspended in these regions.

As the tertiary strata prove that a warm climate certainly preceded the assumed glacial epoch in the northern hemisphere, and as a mild climate has since prevailed, Mr. Lyell says, there are three distinct phases of action to be considered in studying the supposed glaciers of Scotland: 1st, the coming on of the epoch; 2nd, its continuance in full intensity; and 3rd, its gradual retreat. At the commencement of the first condition, only the higher mountains would send down glaciers to be melted in the plains below, as at present in Switzerland, and in Chili between the 40th and 50th degrees of latitude. The ice would therefore thus be constantly advancing and retreating, but progressively, century by century, gaining ground, in consequence of diminishing summer heat; and pushing its terminal moraines forward, it would fill up lakes and other inequalities, till it finally reached the sea. During the second condition, when the motion of the ice would be very small, there would be, Mr. Lyell states, vast accumulations of snow filling the plains and valleys to a great height, and leaving bare only the higher peaks and precipices of the mountains. From these points, he conceives the erratic blocks were detached and conveyed almost imperceptibly along the surface of the frozen snow to great distances. Lastly, at the breaking up and gradual retrèat of the glaciers during the third period, he is of opinion, the boulders were deposited in the various situations in which they are now found, and that moraines, or lateral and transverse mounds, were successively deposited, and lakes formed, by which stratified materials were accumulated in certain positions.

The second part of Dr. Buckland's Memoir on the Evidence of Glaciers in Scotland and the North of England, was then read.

The first part of the Memoir concluded with an account of glacial phænomena in the neighbourhood of Edinburgh (see *antè,* p. 569); and the line of country more particularly described in this portion extends southward from Edinburgh by Berwick, Newcastle, the Cheviots, the lake districts of Cumberland and Westmoreland, Kendal and Lancaster, to Shap Fell. A large portion of the low lands between Edinburgh and Haddington is composed of till or unstratified glacier-mud containing pebbles. In the valley of the North Tyne, about one mile east of Haddington, is a longitudinal moraine midway between, and parallel to, the river and the high road; and Dr. Buckland directs attention to the trap-rocks which commence a little further eastward, and are intersected by the Tyne for four or five miles above Linton, as likely to present scored and striated surfaces, where the valley is most contracted. Four miles west of Dunbar another long and lofty ridge of gravel stretches along the right bank of the river; and for three miles to the south-east of Dunbar extends a series of terraces or modified lateral moraines. In the high valleys at the east extremity of the Lammermuir hills, between Cockburn's Path and Ayton, moraines dispersed in terraces are also visible at various heights on both sides of the river; and on the left margin of the estuary of the Tweed, three miles north of Berwick, round tumuli and oblong mounds of gravel are lodged on the slope of a hill 300 or 400 feet above the level of the sea.

Moraines in Northumberland.—On many parts of the coast of Northumberland, especially near Newcastle, deposits of till rest upon the carboniferous rocks. At the village of North Charlton, between Belford and Alnwick, Mr. C. Trevelyan pointed out to Dr. Buckland in 1821, a tortuous ridge of gravel which was supposed to be an inexplicable work of art; but which he became convinced, after an examination in 1838 of the upper glacier of Grindelwald and that of Rosenlaui, is a lateral moraine. Dr. Buckland was prevented from examining the gorges through which the Burns descend from the eastern extremity of the Cheviots, but he directs attention to them as points where striæ and other proofs of glacial action may be found. Immediately below the vomitories of the eastern valleys of the Cheviots, enormous moraines are stated to cover a tract four miles from north to south, and two from west to east; and the high road to wind among cultivated mounds of them from near Woller, through North and South Middleton, and by West and East Lillburn to Rosedean and Wooperton. On the left bank of the College Burn*, immediately above the bridge at Kirknewton, Dr. Buckland discovered last autumn a moraine thirty feet high, stratified near the top to the depth of a few feet, but composed chiefly of unstratified gravel, inclosing fragmentary portions of a bed of laminated sand about three feet thick. Some of these fragments were in a vertical position, others were inclined, and the laminæ of which they were composed, were, for the greater part, variously contorted. He is of opinion that these detached portions were severed from their original position, moved forward, and contorted by the pressure of a glacier, which descended the deep trough of the College Burn from the northern summit of the Cheviots.

Evidence of Glaciers in the mountains of Cumberland and Westmoreland.—Proofs of glacial action, Dr. Buckland says, are as abundant throughout the lake districts and in the districts in front of the great vomitories through which the waters of the lakes are discharged, as in Scotland and Northumberland. Thus, in the vicinity of Penrith, near the junction of the Eden with the Eamont and the Lowther, are extensive moraines loaded with enormous blocks of porphyry and slate, brought down, Dr. Buckland observes, by glaciers, which descended from the high valleys on the east flanks of Helvellyn, and in the mountains around Patterdale, into the lake of Ulleswater (considered to be then occupied by ice), and from the valleys by which the tributaries of the Lowther descend from the east flank of Martindale, from Haweswater and Mardale. A remarkable group of these moraines is by the road side near Eden Hall four miles east of Penrith; and the detritus of moraines is stated to occupy the greater part of the valley of the Eamont, from Ulleswater to its junction with the Eden. On the southern frontier of these mountains in Westmoreland and Lancashire similar moraines occur on an extensive scale. Thus, immediately below the gorge through which the Kent descends from the mountains of Kentmere and Long Sleddale, many hundred acres of the valley of Kendal are covered with large and lofty insulated piles of gravel; and smaller moraines, or their detritus, nearly fill the valley from Kendal to Morecombe Bay. Five miles north-east of Kendal, on the high road from Shap, on the shoulder of the mountain in front of the valley of Long Sleddale, and at an elevation of 500 feet, a group of moraines occupies about 200 acres, and is distinguished from the adjacent slate rocks by a superior fertility. On the south of Kendal, the high roads from Burton and Milnthorpe to Lancaster, pass for the greater part over moraines or their detritus; and Lancaster Castle, placed in front of the vomitory of the Lune, is stated to stand on a mixed mass of glacial debris, probably derived from the valley of the Lune. The districts of Furness, Ulverston, and Dalton are extensively covered with deep deposits of glacier origin, derived from the mountains surrounding the

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* For a notice by the late Mr. Cully, of a sudden flood in this district in 1830, see Proceedings of the Geological Society, vol. i. p. 149.

upper ends of Windermere and Coniston lakes; and they contain a large admixture of clay, in consequence of the slaty nature of many of the mountains. A capping of till and gravel, thirty to forty feet thick, overlies the great vein of hæmatite near Ulverston. The numerous boulders upon the Isle of Walney also indicate the progress of the moraines from Windermere and Coniston to the north-west extremity of Morecombe Bay.

Dr. Buckland was prevented from personally examining, during his late tour, the south-west and west frontiers of the Cumberland mountains, but he conceives that many of the conical hillocks laid down on Fryer's large map of Cumberland, in the valley of the Duddon, at the south base of Harter Fell, are moraines; that some of the hillocks in the same map on the right of the Esk, at the east and west extremities of Muncaster Fell, are also moraines formed by a glacier which descended the west side of Sca Fell; and that many of the hillocks near the village of Wastdale were formed by moraines descending westward. Dr. Buckland is likewise convinced that moraines exist near Church in the Valley; also between Crummoch Water and Lorton, in the valley of the Cocker; and near Isle, in the valley by which the Derwent descends from Bassenthwaite lake towards Cockermouth, though there are no indications of them on Fryer's map.

Near the centre of the lake district are extensive medial moraines on the shoulder of the hill called the Braw Top, and formed by glaciers at the junction of the valley of the Greta with that of Derwent Water.

Dr. Buckland had no opportunity of seeking for polished and striated surfaces in the high mountain valleys of the lake district; but he found them on a recently exposed surface of greywacke in Dr. Arnold's garden at Fox Howe near Ambleside; likewise near the slate quarry at Rydal; and on newly bared rocks by the side of the road ascending from Grassmere to the Pass of Wythburn; he is also of opinion that many of the round and mammillated rocks at the bottom of the valley, leading from Helvellyn by the above localities to Windermere, owe their form to glacial action.

The remarkable assemblage of boulders of Criffle granite at Shalkbeck, between Carlisle and Cockermouth, Dr. Buckland conceives may have been transported across the Solway Frith on floating masses of ice, in the same manner as the Scandinavian blocks are supposed to have been conveyed across the Baltic to the plains of Northern Germany.

Dispersion of Shap Fell Granite by Ice.—The difficulties which had long attended every attempt to explain the phænomena of the distribution of the Shap Fell boulders, Dr. Buckland considers, are entirely removed by the application of the glacial theory. One of the principal of these difficulties has been to account for their dispersion by the action of water; northwards along the valley, descending from Shap Fell to Shap and Penrith; southwards in the direction of Kendal and Morecombe Bay; and eastward, over the high table-land of Stainmoor Forest, into the valley of the Tees, as far as Darlington. A glacier descending northwards from the Fell would, on the contrary, carry with it, Dr. Buckland says, blocks to the village of Shap, and strew them thickly over the space where they are now found; another, taking a southern course, would drop the boulders on the hills and valleys over which the road descends by High Borough Bridge to Kendal; and a third great glacier, proceeding eastwards betwixt Crosby, Ravensworth, and Orton, would cross transversely the upper part of the valley of the Eden, near Brough, and accumulate piles of ice against the opposite escarpment until they overtopped its lowest depression in Stainmoor Forest, and disgorged their moraines into the valleys of the Greta and the Tees. There are abundant proofs, Dr. Buckland states, of the existence of this glacier in large mud and boulder moraines, in the ascent of the gorge between Shap Fell and Birbeck Fell, and in the furrows and striæ, as well as the mammillated forms of the rocks at the portals of the gorge, particularly on the northern side. In the physical structure of this neighbourhood, Dr. Buckland points out other conditions which would have facilitated the accumulation of glaciers, as the lofty mountains of Yardale Head, which overtop Shap Fell on the north-west, and the still higher mountains to the west, whose snows must have nourished enormous glaciers; and he concludes by stating that Professor Agassiz, during an independent tour, arrived at similar conclusions respecting the mode by which the Shap boulders were distributed.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 156.]

March 13, 1840.—The following communications were read:

On the Regulator of the Clock-work for effecting uniform Movement of Equatoreals. By G. B. Airy, Esq. Astronomer Royal.

The subject of this communication is a mathematical investigation of a mechanical problem of great importance in practical astronomy. The author remarks, that the accuracy given to a most delicate and valuable species of observations, by the use of clock-work attached to equatoreals, is so great, and the importance of the application so evident, that any investigation which assists in elucidating the principles on which such apparatus should be constructed, and especially any which points out the nature of one important defect to which it may be liable, cannot but be regarded as interesting to the practical astronomer and the instrument-maker.

After adverting to the different methods of giving motion by a train of wheel-work to the polar axis of the equatoreal, which have been adopted in the principal instruments hitherto erected, as the Dorpat telescope, the Kœnigsberg heliometer, the Cambridge equatoreal, &c., the author proceeds to consider the various means which have been put in practice for effecting the regulation. In the mountings constructed by Fraunhofer, the axis of the regulator is vertical; it carries a horizontal cross-arm, to the extremities of which are attached springs nearly transverse in direction to the cross-arm, carrying at the ends small weights. When the regulator is made to revolve with a certain velocity, the centrifugal force of the balls bends the springs till the balls just touch the inner surface of a drum which surrounds the regulator: the smallest additional velocity causes the balls to press against the drum and create a friction which immediately reduces the velocity; and the drum is made slightly conical, so that by raising or depressing it the velocity may be altered at pleasure. This construction not only partakes of the defects common to all the others, but is liable besides to this peculiar objection, that the determinate rate will depend most essentially on the strength of the springs, and will therefore depend on temperature and other varying causes. The other constructions (which were practically introduced by Mr. Sheepshanks) depend upon the same principle as that of the governor of the steam-engine. Two balls suspended from the upper part of a vertical axis, by rods of a certain length, are made to expand by the rotatory velocity of the axis; and this expansion, when it reaches a certain extent, is made to press a lever against some revolving part, and thereby to create a friction which immediately checks the velocity. In some cases the balls are suspended by rods from the extremities of a horizontal arm carried by the vertical axis. This construction, adopted in the south equatoreal of the Royal Observatory, may be considered analogous to Fraunhofer's, substituting for the springs the gravity of the balls;—a change which can hardly fail to be advantageous.

Now, the uniformity of rotatory motion of the spindle, in these various constructions, depends entirely on this assumption; that if, upon the whole, the retarding forces are equal to the accelerating forces, the revolving balls will move in a circle and in no other curve. But this assumption is not correct. If, for instance, we consider the case of balls, suspended as in the governor of the steam-engine; the motion of each of the balls may be the same (omitting the moments of inertia of the various parts of the machine, which are trifling) as that of a ball, suspended by a string, and put in motion by an arbitrary impulse; and a ball so suspended may move in a curve differing insensibly from an ellipse. Now this elliptic motion actually takes place. In some instances observed by the author, the balls of the regulator, instead of revolving in a circle, revolved in an ellipse of considerable excentricity, and the rotatory motion of the spindle was therefore exceedingly variable. The effect of this irregularity on the motion of the equatoreal, whether the inequalities of motion are followed by the polar axis, or merely communicate a general tremor to the frame, must be injurious.

The inequality now mentioned is only one case of a very extensive theorem, which may be thus enunciated:—" Whenever the equilibrium of forces requires that a free body be brought to a determinate position, either absolute or relative to other parts of the mechanism with which it may be connected, the body will not remain steadily in that position of equilibrium, but will oscillate on both sides of that position, and (so far as the action of those forces affect it) will have no tendency to settle itself in the position of equilibrium." This theorem supposes that some cause of disturbance has once put the body into a state of oscillation; and renders it necessary to take account of such oscillation in planning any mechanism which depends upon assuming the position of equilibrium to be nearly preserved.

If we examine the theory of the regulator, we shall see that the friction which checks the motion takes place when the balls are most distant from the axis, and (as the equable description of areas is nearly observed) this occurs when the angular motion is least. The whole maintaining force acts without check when the balls are nearest to the axis, that is, when the angular motion is greatest. Therefore, when the angular motion is least, the acting forces tend still to diminish it; when greatest, they tend to increase it. Hence the inequalities of angular motion will increase till some new forces come into play, which act in some different manner: and thus is explained the obstinate adherence of the governor balls in some cases to their elliptic motion.

The author next proceeds to consider the ways in which an attempt may be made to counteract the injurious effects of such oscillations. These appear to be only two; one, to make the oscillations of velocity much slower (or to make their periodic time longer); the other, to make the oscillations quicker (or to make their periodic time shorter). The first of these methods has the effect of giving greater smoothness to the motion (an object of great importance); and it is the principle which was adopted with success in the clock-work of the Cambridge equatoreal. The second method endangers the smoothness of the motion; but, as the error has but a short time for accumulation, it ensures that the object shall remain steady under the wire of the telescope far more completely than the first. The construction attached to the clock-work of the south equatoreal of the Royal Observatory is on this principle; and it appears to answer extremely well.

The mathematical problem proposed by the author in the present communication is an investigation into the motion of governor balls, for the purpose of deducing the time of rotation corresponding to a given expansion of the balls, and the periodic time of their oscillations, and the consequent oscillations in the angular speed of the spindle; and the subject is discussed on four different suppositions, which, with their several principal results, are as follows: 1. When the balls are supposed to be acted upon by no force. The result is, that the periodic time of oscillation is somewhat greater than half the time of rotation. 2. When the axis which carries the balls has a fly-wheel attached to it. In this case the periodic time of the oscillations cannot be less than half the time of rotation, and may be in any proportion greater. 3. When the balls are suspended by rods from a horizontal arm carried by the regulator-spindle. The result is, that the periodic time of the oscillations may be made small in any proportion to the time of rotation. 4. On an assumed law of accelerating force and retarding friction. The result is, that the effect of these forces is to increase continually the inequality of motion.

Note on an Arabic Globe belonging to the Society. By R. W. Rothman, Esq. M.A., Foreign Secretary.

The instrument in question is a small bronze globe, about six inches in diameter, brought some time ago from the East, having the positions of the principal stars marked by silver studs, with their Arabic names engraved; and the object of the present note is to point out the differences between the names of the stars as found on the globe, and those given in the Catalogue of Ulugh Beg, with which, in general, the globe agrees, though in some instances the differences are worthy of notice. From the position of the colour, &c. it is inferred that the globe is not of ancient date; but it bears no mark indicative of the precise period of its construction.

Elements of Galle's Second Comet; computed by M. Petersen, and communicated by Professor Schumacher.

April 10.—The following communications were read:—

Observations made at the Cape of Good Hope, in the year 1838, with Bradley's Zenith Sector, for the Verification of the Abbé De Lacaille's Arc of the Meridian. By Thomas Maclear, Esq. F.R.S. Communicated by the Lords Commissioners of the Admiralty*.

The present paper is the second which has been received from Mr. Maclear on the subject of the important and interesting operations now going on at the Cape, relative to the measurement of an arc of the meridian.

In the former communication, an account of which is given in the Monthly Notice for April 1839 (Phil. Mag. Third Series, vol. xiv. p. 522), Mr. Maclear detailed his proceedings for the purpose of identifying the terminal station of Lacaille's arc; the present contains the sector observations, with their reduction, and the determination of the amplitude of the arc.

The sector was delivered to Mr. Maclear in Table Bay, by Captain Maitland, of H. M. ship Wellesley, on the 9th of December, 1837, and was conveyed the same day to the Royal Observatory under his personal superintendence; and on the 10th the instrument was put together and erected in the centre room. As this room was originally constructed for a zenith-tube of limited dimensions, it became necessary to enlarge the apertures by sawing through a portion of the iron bars of the grating forming the floor of the lantern, and of the rafters above. In this tedious operation nearly a month was consumed; but, in the meanwhile, a tent and a tripod for the support of the sector were prepared, a list of stars selected, and a variety of details settled, in which the author states Sir J. Herschel cordially assisted with his advice.

The site of Lacaille's Observatory in Cape Town being covered by a large building, erected since 1752, the sector could not be placed exactly over Lacaille's station; it was accordingly raised in the court-yard of the house, under a tent, and every disposition

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* [See Phil. Mag. Third Series, vol. xvi. p. 594.]

made, which the confined locality admitted of, to secure the canvass against the effects of the wind. The instrument was erected and adjusted on the 29th of January, and the observations commenced the same evening. They were carried on until the 19th of February, but under very disadvantageous circumstances, principally from the violence of the north-east wind acting on the unsettled canvass, and the showers of sand carried into the tent from the street. This series of observations proved to be unsatisfactory, and was not used for the determination of the amplitude.

As one of the objects for which the present observations were undertaken was to determine the influence of Table Mountain on the direction of the plumb-line, the sector was next transferred to a station close up towards the precipitous front of the mountain, on its north side, and about 1000 feet above the level of the sea. Previous, however, to the commencement of the observations at this station, Mr. Maclear removed the bisecting wires, which had been found too thick for several of the stars employed, and substituted cobweb. The observations began on the 24th of February, and were continued till the 13th of March, when the sector was dismounted and carried, as before, by coolies to the office of the Engineer Department in Cape Town.

The next step in the proceedings was to transport the sector to Klyp Fonteyn, the northern extremity of the arc. The party arrived at the station on the 24th of March, and immediately prepared to erect the sector on the corn-floor described in the former Notice as Jacobus Cotsee's foundation, which is situated at, and rather within, the south extremity of the ruin supposed by Captain Everest to be the granary of Lacaille. Before the instrument was set up, the several pieces forming the bearing and upper adjustments of the tube were separated, and carefully cleared from sand and dust. A tent having been raised, and fixed to iron pins driven into the floor, the tube was placed on its bearings, and the two barometers suspended from the sector tripod. At the distance of twenty-one feet exactly, and due east of the sector axis, a nail was driven into the floor, on which the axis of the repeating circle was placed.

The observations for zenith distances began on the 28th of March, and were continued to the 21st of April, a sufficient number having then been made for settling the question of the amplitude. Before leaving the station plans were made of the place, and of the foundations which had been discovered by Lieutenant Williams and the sappers; a base-line was measured, and the nature of the country to the north of the station examined. It was on the 6th of April that the foundation was discovered, whose dimensions correspond in some measure to the description given by Lacaille in his Journal of the granary he had occupied. The sector having been taken down, and a bottle containing an inscription deposited in a hollow chiseled into the solid rock about three feet below the surface, to mark the spot over which it had been erected, the party quitted Klyp Fonteyn on the 25th of April, and arrived at the Observatory on the 1st of May.

A cursory comparison of the observations having shown that those made at the southern station did not deserve the confidence required in a work of this kind, where the length of a few feet is a quantity of importance, and experience having proved that good observations could not be obtained under a tent in Mrs. De Witt's yard, it was resolved to look out for some solid building close to the station, where the sector could have fair play. Mr. Maclear fixed upon the Raggebay Guardhouse, and the necessary permission having been obtained, a hole was made in the roof, the floor taken up and sunk to the requisite depth, and the sector erected on the 7th of May, one week after the return of the party from Klyp Fonteyn.

From the state of the weather, no observations could be obtained until the 12th, and frequent interruptions afterwards occurred from the same cause; so that six weeks were expended on a a work which,i n favourable weather, might have been accomplished in two.

The sector was dismounted on the 30th of June, and carried back to the Observatory, where it was again set up in the sector-room on the 2nd of July. On examination, it was found to be as perfect as when first received, without the slightest mark of injury.

The author next proceeds to give the reduction of the observations. The barometers employed at Klyp Fonteyn were made by Mr. Thomas Jones, and from a comparison of their results with the Journal kept at the Royal Observatory, it appears that the station at Klyp Fonteyn is 485 feet above the mean level of the sea*. The station in the Guardhouse is close to the sea beach; the feet of the sector tripod could not be more than two or three feet above high-water mark. The chronometer employed at both stations was by Arnold, and beats half-seconds. Its performance at Klyp Fonteyn was good; at the Guardhouse, the reverse. At the former place, its rate was actually obtained by altitudes near the prime vertical; in Cape Town, by means of journeymen pocket-chronometers, carried to and from the transit-clock at the Observatory.

The collimation of the middle wire was deducted from the successive transits of stars in the alternate position of the limb, east and west.

The corrections for aberration, precessions, and nutation were calculated by means of the constant in the Royal Astronomical Society's Catalogue, recomputed for 1838.

The number of stars observed was 40; of which 20 were to the north, and 20 to the south of the zenith at the Cape. The number of observations at Klyp Fonteyn is 464, and at the Guardhouse 669; in all, 1133. It may therefore be supposed that errors of observation are reduced to nothing.

The final results are deduced as follows:—The amplitude being found from the mean of the reduced zenith distances of each star observed at both stations, and each result having a *weight* assigned to it equal to the product obtained by multiplying the least number

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* In the former communication the height of the station is stated to be nearly 400 feet.—Monthly Notices, vol. iv. p. 194. [Phil. Mag. S. 3, vol. xiv. p. 528.]

of observations of the star in question at one station, and in one position of the sector, into the least number for the same star at the other station, the resulting mean amplitude from the stars north of the zenith is 1° 13′ 1″·173, and from the stars south of the zenith, 1° 13′ 14″·961.

By assigning to the amplitude found from each star a weight equal to the quotient of the square of the number of observations of the star by twice the sum of the squares of the errors at both stations, the group to the north of the zenith gives 1° 13′ 14″·173, and the group to the south 1° 13′ 14″·953. The results of the two methods of computation may be regarded as identical; and the stars north of the zenith give the amplitude *less* by 0″·78 than the stars south of the zenith.

It is not easy to assign the cause of this discrepancy. Mr. Maclear inclines to ascribe it to the probable expansion of the tube at Klyp Fonteyn from the high temperature. While the observations were made the thermometer was sometimes as high as 93°, while at the Guardhouse the range was between 57° and 63°. Lacaille (*Mém de l'Acad.* 1752) states that his southern group of stars gave the amplitude greater by 0″·8 than the northern. This near coincidence with Mr. Maclear's result is remarkable.

If the expansion of the tube be the cause of the discrepancy, the mean between the north and south groups is as correct as if no expansion took place. This mean is 1° 13′ 14″·56, with a probable error not exceeding 0″·03.

The axis of the sector on the corn-floor at Klyp Fonteyn was 216 feet (reduced to the meridian) *south* of the centre of the foundation discovered on the 6th of April, and supposed to be Lacaille's sector station. The axis of the sector in the Guardhouse was 45 feet on the meridian *north* of Lacaille's sector station in Mrs. De Witt's yard. Now 261 feet = 2″·56; which added to 1° 13′ 14″·56 gives 1° 13′ 17″·12 for the amplitude of Lacaille's arc. Lacaille's value is 1° 13′ 17″·33*.

The author remarks, in conclusion, "that although this work does not clear up the anomaly of Lacaille's arc, it redounds to the credit of that justly distinguished astronomer, that, with his means, and in his day, his result from 15 stars is almost identical with that from 1133 observations on 40 stars, made with a powerful and celebrated instrument. Our field of inquiry is now limited to the terrestrial measure, which every friend to science must wish to see undertaken without delay, as a portion of a greater arc to extend so far as to neutralise local attractions, and leave no doubt upon the true curvature of this portion of the southern hemisphere."

The Longitude of Madras, computed from Moon-culminating Observations. By Edward Riddle, Esq.

Mr. Riddle having undertaken, at the request of Mr. Baily, to compute the longitude of the Madras Observatory from a mass of

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* In the *Fundamenta Astronomiæ* (page 184), Lacaille states that the amplitude deduced from the observations corrected for errors subsequently discovered in the divisions of the sector, and recomputed, is 1° 13′ 17″·5.

corresponding moon-culminating observations made there and at Greenwich, Cambridge, and Edinburgh, in 1834, 5, 6, and 7—observations which have not before been used for the purpose,—has availed himself of the occasion to enter at considerable length into the practical details of the method of computing the longitude; and has given all the requisite formulæ, with examples of their application from the observations under discussion.

The general result of the computations, of which a further account appears in the Monthly Notices for April, is that the longitude of Madras from Greenwich is,

h	m	s			
5	20	54·9	by 54	observations at	Greenwich and Madras
		53·9	.. 56	,, ,,	Cambridge and Madras
		58·0	.. 65	,, ,,	Edinburgh and Madras.

Ephemeris and Elements of the third Comet discovered by Galle. By Mr. Rumker, Superintendent of the Observatory at Hamburg. Communicated by Dr. Lee.

Approximate elements of the same comet have been received from Professor Schumacher.

It has been remarked that these elements resemble those of a comet observed in China in 1097, and computed by Burckhardt; and of one which appeared in 1468, of which observations are recorded by Pingré. Supposing the three appearances to have been of the same comet, the periodic time is thus about 371 years.

On the Present State of our Knowledge of the Parallax of the Fixed Stars. By the Rev. R. Main.

This paper was in part read.

May 8.—Among the presents announced at this meeting, was a 7-feet Newtonian Reflecting Telescope, the work of the late Sir William Herschel, and presented by him to his sister, Miss Caroline Herschel; in whose name, and that of the President, it was now presented to the Society.

The reading of Mr. Main's paper on the Present State of our Knowledge of the Parallax of the Fixed Stars, was resumed and concluded.

This memoir was read to the Council of the Society at their meeting in January of the present year; the object of it being a review of the parallax of the *Cygni*, recently obtained by Professor Bessel. In presenting it as a memoir, to be read before the Society, the author determined to allow it to remain in its original form of a report addressed to the Council; feeling that, if it were given in any other shape, his discussion of the results of eminent contemporaneous astronomers on the subject of annual parallax might seem presumptuous. He recommends to the notice of astronomers a very complete historical summary of astronomy, as connected with this subject, by Fockins (which work was printed at Leyden in 1835), entitled *Commentatio Astronomica de Annuali Stellarum Parallaxi*, which, he remarks, very materially assisted him in the prosecution of the historical part of his work. The author proposes the following divisions of his report:—

1. Abstracts of theoretical papers which have appeared on the subject of annual parallax.

2. A statement of the results of observations which have been made since the time of Bradley for the purpose of detecting parallax.

3. A review of the results of contemporaneous astronomers on this subject.

4. A discussion of Bessel's observations and results.

Under the first division, he gives abstracts of the following papers:—

A Memoir by Clairaut.—Mémoires de l'Académie, 1739, p. 358.

A Memoir by Sir W. Herschel.—Phil. Trans. 1782, lxxii. P. I. p. 82.

A Memoir by Schubert.—Berliner Astronomisches Jahrbuch, 1796, p. 113.

A Memoir by Sir J. Herschel on the Detection of Parallax by the Variation of the Angle of Position of Double Stars.—Phil. Trans, 1826, P. II. p. 266; and 1827, P. I. p. 126.

A Memoir by Struve, forming one of the introductory chapters to his large work on double stars.

A discussion by Bessel of Bradley's Observations of Right Ascensions of Stars differing by nearly 12^h.—Fundamenta Astronomiæ, Introduction.

Under the second head he gives, briefly, the results obtained by Piazzi, Calendrelli, Brinkley, Pond, Bessel, and Struve, referring to the following works:

For Piazzi and Calendrelli:—

Memoirs of the Italian Society, vol. xii.

Calendrelli's Opusculi Astronomichi, vol. for 1806.

Zach's Monthly Correspondence, vols. xviii. and xix.

For Brinkley and Pond:—

Phil. Trans., vols. for 1810; 1817; 1818, P. II.; 1821, P. II.; 1823, P. I.; 1824, P. I. and II.

Memoirs of the Astronomical Society, vol. i. P. II.

Transactions of the Royal Irish Academy for 1815, vol. xii.

The Introduction to the Konigsberg Observations for the year 1816.

The Dorpat Observations, vols. i., ii., and iii.

Under the third head, the following memoirs on the subject are considered:—

On the Parallax of *α Aquilæ*, by Mr. Henry Taylor, Astronomer to the East India Company at Madras.

On the Parallax of *α Lyræ*, by the Astronomer Royal.—Mem. Ast. Society, vol. x.

On the Parallax of α^1 and α^2 *Centauri*, by Professor Henderson, Mem. Ast. Society, vol. xi.

On the Parallax of Sirius, by Professor Henderson*. Not yet printed in the Society's Memoirs.

[* An abstract of Prof. Henderson's paper will be found in Phil. Mag. Third Series, vol. xvi. p. 148.]

Of these memoirs, it is sufficient to say that Mr. Henderson's parallax of *α Centauri* is discussed at some length; and the result is, that there seems a strong probability of a sensible parallax in this remarkable star, which is strongly recommended to the attention of southern astronomers.

Lastly, in discussing Bessel's parallax of 61 *Cygni*, the author proceeds as follows * :—

That the indication of a parallax in the agreement between the fourth and fifth columns of the preceding tables may be rendered more evident, a graphical projection of them is added. The time being set off by proportional spaces on the line of the abscissæ, inclined lines are drawn through the origin (Jan. 1, 1838), whose ordinates represent the effects of Bessel's correction of the proper motion with contrary signs. From these lines the above differences are set off in the direction of ordinates to the abscissa. The curve, therefore, which passes through the extremities of these ordinates, represents the periodical effect of parallax; and accompanying curves being given, exhibiting the true effect of Bessel's assumed value of the constant, the agreement between the two is shown to be most complete. In the case of measures of distance from star (*a*), the maximum and minimum and vanishing ordinates of the curve are shown with almost as much regularity by the observed differences as by the assumed parallax; and in the second case, though the agreement is not so close, yet the general law of the curve of sines is well preserved.

In arguing on the evidence afforded by the foregoing tables and graphical illustrations, the author concludes, not only that a real parallax has been detected, but that its amount is very approximately given in Bessel's investigation, who is enabled, by repeating the same process, to diminish at pleasure the residual errors of the determination. This feature separates completely this from all former attempts, in some few of which an amount, rather greater than the limiting probable errors, would seem to announce a parallax, of which the evidence is yet so slight as to leave the mind quite unsatisfied of its existence; while the uncertainty of its amount (supposing its existence to be proved) prevents its application to ulterior objects in sidereal astronomy.

To the memoir are annexed two appendices, the first of which contains the investigation of formulæ for computing the coefficients of the constant of parallax in the two cases; and also for finding the variation in the angle of position of two stars very near each other, one of which is affected by parallax.

The second contains a translation of the most important parts of Bessel's description of his Heliometer, from the *Astronomische Nachrichten*, vol. viii. No. 189.

An extract was read from a letter from Professor Bessel to the President, stating that the observation on 61 *Cygni* had been con-

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* Bessel's letter is contained in vol. iv. No. 17, of the Notices of the Astronomical Society [and in Phil. Mag., Third Series, vol. xiv. p. 68.].

tinued through the last year to the end of March 1840, and that the most probable values of the parallax resulting from the measured distances of the double star from each of the two stars of comparison, (*a*) and (*b*), are as follows:—

188 obs. $61-a$.... $0''\cdot3584-0''\cdot0756\,k$; weight, 64·66
214 .. $61-b$.... $0\ \cdot3289-0\ \cdot0276\,k$; ... 78·89

(k being a small indeterminate correction depending on the effects of temperature on the micrometer-screw).

The sums of the squares of the errors cannot, on the supposition of a vanishing yearly parallax, be made less than —

$61-a$... $12\cdot7282-3\cdot2445\,k+0\cdot6330\,k^2$
$61-b$... $15\cdot6507-1\cdot6094\,k+1\cdot7029\,k^2$;

but on assuming the annual parallax to have the value before assigned to it, they become —

$61-a$.. $4\cdot4245+0\cdot2579k\quad 0\cdot2627k^2=4\cdot3614+0\cdot2637(k+0\cdot489)^2$
$61-b$.. $7\cdot1171-0\cdot1768k+1\cdot6426k^2=7\cdot1123+1\cdot6426(k-0\cdot0\cdot54)^2$

On deducing the value of k from the observations, those of the first star give, therefore, $k=-0\cdot489$; and those of the second $k=0\cdot054$. M. Bessel shows that the last of these values is more deserving of confidence than the first; nevertheless, the true value of the correction is still very doubtful, and it is accordingly left indeterminate.

Assuming the relative parallax of (*a*) and (*b*) to be equal, and having due regard to the mean error of both results, the most probable value of the annual parallax is found

$$0''\cdot3483-0''\cdot0533\ k$$

and its mean error $\pm0\ \cdot0141$.

This result is greater by $0''\cdot0347$ than was found from the first series of observations.

Assuming $k=0$, it results from this determination, that the distance of the star 61 *Cygni* from the sun is 592,200 times the mean radius of the earth's orbit—a distance which light would require 9¼ years to pass through.

He divides the two sets of observations of the distance of 61 *Cygni* from the comparison stars (*a*) and (*b*), into separate groups, monthly and half-monthly, and takes the mean of the measures in each group as corresponding to the mean of the days in that group. These means are compared with Bessel's mean distances, which he has derived from the solution of his equations, and the differences tabulated. The effect of Bessel's assumed parallax is then computed for each mean day, and the resulting numbers placed in an adjacent column. The agreement between the computed effect of parallax and the above-mentioned differences is remarkably close, especially for the measures of distance of the star (*a*), in which the solution of the equations shows the error of the assumed proper motion to be very small indeed.

The following are the tabulated monthly groups:—

Results from the Measures of Distances of 61 *Cygni from Star* (*a*).

Mean day.	Mean of each group.	Bessel's mean distance.	Differences.	Effect of a parallax of 0″·369.
1837.				
August.... 23.	461″·406	461″·609	+0″·197	+0″·212
September 14.	·709	...	+ ·100	+ ·100
October .. 12.	·649	...	+ ·040	− ·057
November 22.	·395	...	− ·214	− ·258
December 21.	·287	...	− ·322	− ·317
1838.		...		
January .. 14.	·233	...	− ·376	− ·318
February 5.	·386	...	− ·223	− ·264
May...... 14.	·854	...	+ ·245	+ ·238
June...... 19.	·969	...	+ ·360	+ ·332
July ...... 13.	·825	...	+ ·216	+ .332
August.... 19.	·760	...	+ ·151	+ ·227
September 19.	·649	...	+0·040	+0·073

Results from the Measures of Star (*b*).

Mean day.	Mean of each group.	Bessel's mean distances.	Differences.	Effect of a parallax of 0″·261.
1837.				
August.... 22.	706″·504	706″·291	+0″·213	+0″·133
September 15.	·479	...	+ ·188	+ ·196
October .. 12.	·419	...	+ ·128	+ ·227
November 22.	·186	...	− ·105	+ ·187
December 20.	·283	...	− ·008	+ ·100
1838.		...		
January .. 11.	·177	...	− ·114	+ ·014
February.. 9.	·084	...	− ·207	− ·100
March .... 12.	705·900	...	− ·391	− ·196
May ...... 13.	706·206	...	− ·985	− ·209
June...... 20.	·414	...	+ ·123	− ·102
July ...... 21.	·446	...	− ·155	+ ·016
August .. 22.	·588	...	+ ·297	+ ·133
September 5.	·498	...	+ ·207	+ ·175
22.	·712	...	+0·421	+0·212

June 12.—The following papers were read :—

Continuation of the Investigation for the correction of the Elements of the Orbit of Venus. By Mr. Glashier, of the Royal Observatory, Greenwich.

This paper is a continuation of Mr. Main's investigations, communicated to the Society in June 1837 and April 1838, and printed in vols. x. and xi. of the Memoirs. Mr. Main corrected the orbit from Mr. Airy's observations of 1833–36. Mr. Glashier recomputed the observations of 1836 (for errors of assumed semi-diameter), and computed the observations of 1837 and 1838, and gives corrections of the elements of the orbit for all the years : then, substituting the observations, he deduces the residual errors (as compared with Lindenau's tables), which he finds not so small as might have been expected from so fine a series of observations. The author, however, feels confident that the results are correctly derived, from the pains taken to ensure accuracy: and the whole is given in a detailed shape, in order that any suspected error may be more readily detected.

An Account of some Experiments made with an Invariable Pendulum, at the Cape of Good Hope. By T. Maclear, Esq.

When Mr. Maclear was appointed Astronomer at the Cape of Good Hope, he was desirous of repeating the experiments there with an invariable pendulum; but it was some time before he had sufficient leisure to prosecute this measure. The pendulum had been previously swung in London by Mr. Baily, and also after its return to this country. The method of proceeding in such cases is so similar, and has been so often described, that it is unnecessary to enter further on that part of the subject. The result of these experiments shows, that, on the assumption that the pendulum made 86400 vibrations in London, in a mean solar day, at the temperature of 62° *in vacuo*, and at the mean level of the sea, it made only 86332·92 vibrations, under the same circumstances, at the Cape of Good Hope; which is almost identical with the experiments of Mr. Fellows, and differing very little from the experiments of Captain Foster and Freycinet. A new pendulum, consisting of a thick brass bar, without any bob, and furnished with *four* knife edges, is about to be forwarded to Mr. Maclear, which he proposes to swing at the principal stations of the triangulation that is now carrying on in that colony.

An Account of some Experiments made with three Invariable Pendulums, by Lieut. Murphy, R.E., during the late Expedition down the Euphrates. By Mr. Baily.

When Colonel Chesney undertook the expedition down the Euphrates, three invariable pendulums were placed under his care, for the purpose of their being swung at positions more *inland* than had been hitherto practised. Two of these pendulums (iron and copper) belonged to this Society, and the other (brass) to the Admiralty; and they are the same that were taken out by the late lamented Captain Foster. They had been previously swung in this country, before the expedition just mentioned, by Mr. Baily, and also subsequent thereto. Only two places presented favourable opportunities for swinging these pendulums during the expedition: the first, at Port William, near Bir, on the Euphrates; and the other, at Bussora. The experiments were made by Lieutenant Murphy, and were conducted with his usual caution and ability: the details are recorded in printed skeleton forms, with which he was furnished previous to his departure; but none of the computations were made till after Lieutenant Murphy's decease. The reductions have since been made by Mr. Baily, on the same data as those already mentioned in the seventeenth volume of the Memoirs of this Society. On the assumption that each of these pendulums made 86400 vibrations in a mean solar day in London, at the temperature of 62° *in vacuo*, and at the mean level of the sea, it is found that they respectively made the following number of vibrations at the two stations above-mentioned, viz.

Pendulums.	Port William.	Bussora.
Brass	86340·66	86318·98
Copper	86341·30	86317·26
Iron	86338·96	86317·66
Mean =	86340·31	86318·26

The Elements of the Annular Eclipse of the Sun that will happen on October 8, 1847. By Mr. George Innes.

These elements have been deduced from Carlini's "Solar Tables" (1832), and Burckhardt's "Lunar Tables" (1812). The time of apparent conjunction in right ascension at Greenwich will be Oct. 8^d 19^h 28^m 36^s·593; at which time the sun's apparent semidiameter will be 16′ 2″·529, and the moon's augmented semidiameter, 14′ 43″·968;

Greenwich mean time.	Sun's right ascension.	True right ascension.	Declination south.	Equation of time.	Semi-diameter.
18 hours	195° 19′ 31″·390	194° 6′ 41″·844	62′ 20″·693	12′ 29″·507	16′ 2″·512
19 do.	195 21 59·783	194 8 59·511	63 17·975	12 30·245	16 2·524
20 do.	195 24 28·183	194 11 17·180	64 15·247	12 30·930	16 2·536
21 do.	195 26 56·616	194 13 34·914	65 12·525	12 31·597	16 2·546
21^h 6^m 37^s·755......	195 27 13·036	194 13 40·125	65 18·861	12 31·670	16 2·549
22 hours	195 29 25·054	194 15 52·614	66 9·797	12 32·271	16 2·566

Greenwich mean time.	Moon's true longitude.	Longitude north.	Equatorial horizontal parallax.	Horizontal semidiameter.	True right ascension in arc.	Declination south.
18 hours	193° 54′ 30″·843	22° 49″·641	53° 54′·130″	14′ 41″·300	192° 57′ 44″·323	5° 8′ 47″·322
19 do.	194 24 59·768	25 32·833	53 54·197	14 41·316	193 26 45·329	5 17 42·208
20 do.	194 54 28·771	28 16·446	53 54·214	14 41·320	193 64 27·307	5 26 35·565
21 do.	195 23 57·790	30 59·751	53 54·325	14 41·351	194 22 50·127	5 35 27·710
21^h 6^m 37^s·755	195 27 13·036	31 17·610	53 54·326	14 41·351	194 25 55·127	5 40 34·132
22 hours	195 54 26·993	33 42·861	53 54·409	14 41·428	194 51 13·894	5 44 18·646

Obliquity of the Ecliptic	23° 27′ 2″·514
Sun's semi-diameter	16 2·529
——— horizontal parallax ...	8·5925
——— latitude	+ 0·179

On the Comparison of the Neapolitan Standard Yard with the Standard Yard of this Society. By Mr. Simms.

The Neapolitan Government having directed Mr. Simms to construct a new standard scale, on a principle similar to that which was made for this Society, the object of the present communication was to place on record the results of the comparisons that were made with the centre yards of each scale. It appears that 215 comparisons were made on nine several days by three different persons; and the mean of the whole showed that the centre yard of the Neapolitan scale was longer than the standard yard of the Society's scale, by ·002680 of an inch: the greatest differences from the mean being +·000520 and −·000365 of an inch. This new scale is marked No. 6; it being the sixth scale of this construction that has been made.

On the Difference of Longitude between the Observatories of Madras and the Cape of Good Hope, deduced from Moon-culminating stars. By T. Maclear, Esq.

The observations extend from February 19, 1834, to October 10, 1837, both inclusive, and contain all the corresponding observations within that period, except two which appear to be erroneous; and amount to seventy in number. Of these, only three were of the second limb. The result of the whole shows that the difference of longitude is $4^{h}\ 7^{m}\ 1^{s}{\cdot}56$, with a probable error of $\pm\ 0^{s}{\cdot}53$.

LXXXII. *Intelligence and Miscellaneous Articles.*

ON THE LIMIT TO THE ACTION OF CERTAIN CHEMICAL REAGENTS.

M. P. HARTING having examined the action of certain chemical reagents, has given the following statement as the result.

1. The sensibility of *starch* as a reagent for *iodine*.

Iodide of potassium slightly acidulated with nitro-muriatic acid, tested by a diluted solution of starch, gave the following results.

1. Containing 1-500dth iodine gave a black precipitate, the upper surface was brownish yellow.

2. Containing 1-1000dth iodine gave nearly the same colour.

3. Containing 1-2000dth iodine gave the precipitate the same colour, but the solution slightly coloured.

4. Containing 1-3000dth iodine, precipitate bluish black, the solution nearly clear.

5 and 6. Containing 1-4000dth to 1-5000dth iodine, precipitate bluish black, the solution quite clear.

7 to 11. Containing 1-10,000dth to 1-40,000dth iodine, very dark blue.

12 and 13. Containing 1-50,000dth to 1-60,000dth iodine, blue with a shade of violet.

14 and 15. Containing 1-80,000dth to 1-100,000dth iodine, the upper stratum violet blue, the under stratum violet.

16. Containing 1-120,000dth iodine, the upper stratum violet, the under stratum rose-colour.

17. Containing 1-150,000dth iodine, the whole precipitate rose-colour, with a shade of violet.

18 and 19. Containing 1-200,000dth to 1-250,000dth iodine, rose-colour, the upper surface only with a slight shade of violet.

20 to 22. Containing 1-300,000dth to 1-400,000dth iodine, the whole precipitate of a rose-colour.

23 to 25. Containing 1-450,000dth to 1-550,000dth iodine, the upper stratum of the precipitate was slightly rose-colour, the under stratum was white.

The action took place immediately on the addition of the starch as far as No. 19. The following numbers required some period to elapse first: it required some hours in the solution of 1-500,000dth to 1-550,000dth before any effect was produced.

2. The sensibility of reagents *on acids.*

For sulphuric acid, specific gravity 1·829, containing 75·83 per cent. real acid.

Syrup of violets did not detect less than 1-250th sulphuric acid of the above specific gravity, or 1-310th real acid.

Paper stained by Brazil wood was acted upon by 1-10,000dth, or 1-12,500dth real acid.

Paper stained by tincture of red cabbage was reddened by 1-15,000dth, or 1-18,750dth real acid.

Paper stained by logwood was changed to a golden yellow colour by 1-50,000dth, or 1-62,500th real acid.

Paper stained by litmus was immediately reddened by 1-20,000dth, or 1-25,000dth real acid, and after some hours was slightly reddened by 1-50,000dth, or 1-62,500dth real acid.

Carbonate of potash occasioned a slight effervescence with 1-250th, or 1-310th real acid.

For phosphoric acid.—Brazil wood paper and paper stained with red cabbage detected 1-10,000dth of anhydrous phosphoric acid.

Litmus paper was immediately reddened by 1-10,000dth, and after some hours by 1-30,000dth phosphoric acid.

Peculiar reagents for various acids.

For free sulphuric acid.—A concentrated solution of chloride of calcium occasioned a precipitate, after some hours, in a solution containing 1-310th of real sulphuric acid.

A solution of acetate of lead gave a precipitate with 1-50,000dth real acid.

A solution of chloride of barium gave a precipitate with 1-75,000dth.

For combined sulphuric acid.—Acetate of lead produced a precipitate in a solution of sulphate of soda containing 1-36,000dth acid.

Chloride of barium in a solution of the same salt containing 1-45,000dth acid.

For nitric acid.—By means of hydrochloric and gold leaf 1-240th of nitric acid, spec. grav. 1·32 was detected: the gold leaf was dissolved in 24 hours.

For phosphoric acid.—Acetate of lead produced an immediate precipitate with 1-10,000dth anhydrous acid, and with 1-20,000dth, after remaining for half an hour.

Lime water produced exactly the same effect.

Chloride of barium did not occasion a precipitate in less than 1-10,000dth.

For arsenious acid.—Lime water in excess produced a precipitate in a solution containing 1-4000dth of this acid.

Ammoniacal solution of oxide of copper detected 1-8000dth.

Sulphate of copper and ammonia detected 1-12,000dth.

The two last reagents occasioned precipitates in still more diluted solutions, but the precipitates did not possess their characteristic green colour.

Hydrosulphuric acid produced a precipitate in 1-30,000dth.

Ammonia nitrate of silver formed a yellow precipitate with 1-30,000dth; with a more diluted solution the colour of this precipitate was not sufficiently apparent.

3. The sensibility of reagents *for metals and their oxides.*

For free alkalies in general.—Turmeric paper detected the presence of 1-3000dth caustic alkali.

Paper stained with red cabbage detected the presence of 1-7500dth caustic alkali.

Brazil wood paper was coloured slightly violet with 1-20,000dth.

Litmus paper reddened by acetic acid was distinctly rendered blue by 1-80,000dth.

Hydrate of potash contains 16 per cent. of water, therefore the quantity of real potash detected by the above reagents was 1-3600dth, 1-9000dth, 1-24,000dth, and 1-95,000dth.

For potash.—An alcoholic solution of chloride of platinum occasioned a precipitate in a solution of nitrate of potash containing 1-205th of this base; a solution containing 1-200dth was not precipitated by it.

A very concentrated solution of tartaric acid produced a precipitate with 1-220th, but none with 1-230th. The sensibility of these reagents was tried at a temperature of 59° F.

For lime.—Oxalate of ammonia produced a cloudiness after a few minutes in a solution of chloride of lime containing 1-400,000dth of lime.

For baryta.—Fluo-silicic acid produced a slight precipitate in a solution of chloride of barium containing 1-3800dth of baryta.

A solution of sulphate of soda produced in half an hour a cloudiness in a solution containing 1-71,000dth.

For magnesia.—A solution of phosphate of soda indicated in 24 hours the presence of 1-200,000dth of magnesia in a solution of sulphate of magnesia. This reagent must be very concentrated, and a quantity equal to the solution examined, must be added. These conditions are absolutely necessary, as otherwise this reagent will not indicate the presence of even 1-1000dth magnesia in solution. This is probably the reason that M. Roth fixes the delicacy of this reagent at 1-4000dth of magnesia.

A solution of ammonia produced after some minutes a slight precipitate in a solution containing 1-6000dth of magnesia.

For protoxide of iron.—Tincture of galls and a solution of ferroprussiate of potash, acidulated with a few drops of hydrochloric acid, indicated after some minutes the presence of protoxide of iron in a solution containing 1-440,000dth of crystallized sulphate of iron.

For peroxide of iron.—Tincture of galls indicated the presence of 1-300,000dth of peroxide of iron in a solution of sulphate of peroxide of iron by rendering it of a slight violet colour.

A solution of ferroprussiate of potash indicated the presence of 1-420,000dth of the same salt.

For copper.—A solution of ammonia gave after several hours a slight blue colour to a solution of sulphate of copper containing 1-9400dth of oxide of copper.

A solution of prussiate of potash rendered the presence of 1-78,000dth of the same salt visible.

Polished iron showed the presence of 1-125,000dth of oxide of copper, or 1-156,000dth of metallic copper, if the solution was acidulated by a drop of nitric acid.

For lead.—A piece of zinc precipitated lead from a solution of the nitrate when 1-3000dth of oxide was present.

An excess of sulphuric acid occasioned a precipitate in a solution of the same salt containing 1-20,000dth oxide.

A solution of chromate of potash occasioned a cloudiness in a solution containing 1-70,000dth of the same oxide.

A solution of hydrosulphuric acid blackened a solution containing 1-350,000dth.

For silver.—Chromate of lead produced a slight red precipitate in a solution of nitrate of silver containing 1-10,000dth of oxide. No reaction took place in a solution containing above 1-20,000dth.

Arsenite of potash produced a decided yellow precipitate with 1-6000dth oxide in solution, but none with 1-20,000dth.

Iodide of potassium indicated the presence of 1-4000dth oxide, but produced no action with 1-30,000dth.

A solution of hydrosulphuric acid precipitated a solution containing 1-35,000dth of oxide.

Chloride of sodium produces a cloudiness in a solution which contained only 1-240,000dth.—*Extracted by E. F. Teschemacher from the Journal für Praktische Chemie, No.* 1, 1841.

ANILIN.

The following are the additional particulars relative to anilin, referred to in p. 281.

Anilin is a base which with acids readily yields fine crystallized salts. When exposed to atmospheric air, it soon becomes yellow and eventually brown, and the same resinous body is formed which is separated from it by distillation. It must therefore be preserved out of the contact of air, and to prevent its action should be quickly distilled. Anilin may contain a little water, from which it is freed by distillation, taking care, when a third of it has been distilled, to change the receiver; by this method only it is obtained perfectly anhydrous. Boiling anilin dissolves sulphur and iodine in large quantity, and deposits them on cooling, in crystals. Nitric acid, under certain circumstances, converts anilin into a blue or green body, which does not appear to be indigo; this substance has, however, been hitherto obtained only uncertainly and in small quantity, as it undergoes fresh decomposition by the nitric acid. Chromic acid produces in solutions of anilin a precipitate which is sometimes of a deep green colour, and at others of a blackish blue: this acid appears to be a good reagent for anilin, since it is readily produced even in weak solutions; when the precipitate is obtained from concentrated solutions it leaves a considerable quantity of oxide of chromium when calcined. Oxymanganate of potash and the salts of anilin suffer mutual decomposition, brown oxide of manganese being deposited. Anilin yielded by analysis,

Hydrogen	7·54
Carbon	78·21
Azote	14·83

Hydrochlorate of anilin is obtained by direct combination and crystallization; it is very soluble in water. It consists of

Hydrochloric acid	27·81	
Anilin	72·19	100·00.

Oxalate of anilin is obtained by mixing a spirituous solution of anilin with oxalic acid. This salt is a white powder, which is to be washed with alcohol and then dissolved in hot water; on cooling fine crystals of the salt are obtained of several lines in length. This salt appeared to consist of

Oxalic acid	25·92	
Anilin	67·64	
Water	6·44	100·00.

L'Institut, No. 356.

Notice by Prof. Dove *respecting the Error in his Letter on the Law of Storms, pointed out by* Sir David Brewster *at* p. 514.

By some inadvertence I have confounded the notice of Reid's work in the Edinburgh Review, 1839, p. 406, with that in the Foreign Quarterly Review, 1839, p. 1. The paragraph mentioned by me occurs in the latter work, at p. 2. Those who have read my memoir "On Barometric Minima," which appeared in Poggendorff's *Annalen*, vol. xiii. p. 596, will be best able to judge of the share which I have had in the explanation of the phænomena of storms. As this memoir appears to be known only in Germany, I may observe that the still important empirical data contained in it, have been combined in my recent article on the law of storms (Poggendorff's *Annalen*, 1840, Jan., p. 1), with those for which we are indebted to Messrs. Reid and Redfield. Which of the theoretical derivations, whether that given by Mr. Redfield, at p. 33 on the storms of the American coast, or that published by me (analogous to the circular polarization of light), agrees best with the totality of the phænomena,—or whether the connexion suspected by Messrs. Reid and Piddington, with the magnetism of the earth, be more probable,—I leave to the opinion of those who may be inclined to test these views collectively.

W. Dove.

June 13*th*.

INDEX

INDEX TO VOL. XVIII.

END OF THE EIGHTEENTH VOLUME.

Printed by Richard and John E. Taylor, Red Lion Court, Fleet Street.