VOL. 3. JANUARY 1828. No. 13.

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# THE PHILOSOPHICAL MAGAZINE AND ANNALS OF PHILOSOPHY:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE, THE LIBERAL AND FINE ARTS, AGRICULTURE, MANUFACTURES, AND COMMERCE.

**NEW SERIES.**

**N° 13.—JANUARY 1828.**

WITH A PLATE,

Illustrative of Dr. PROUT's Paper on the Ultimate Composition of Simple Alimentary Substances.

BY

RICHARD TAYLOR, F.S.A. F.L.S. M. Astr. S. &c.

AND

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

ALERE FLAMMAM.

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## TO CORRESPONDENTS.

Mr. NIXON's Communication has been received, and will appear in our next.—Q. Q. will hear from us in a few days.

We have to acknowledge the receipt of Mr. HAWORTH's paper, which we also hope to insert.

Professor THOMSON's Letter on the Mutual Decomposition of Sulphate of Zinc and Chromate of Potash came too late for our present Number, but will appear in our next.

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*⁂ The Editors request that all Communications intended for immediate insertion may be sent to the care of Mr. Richard Taylor, Printing Office, Red Lion Court, Fleet Street, London, at furthest by the 15th day of the month, or they will be too late to appear in the ensuing Number.*

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NEW SERIES.

No 14.—FEBRUARY 1828.

WITH TWO ENGRAVINGS:

I. Illustrative of Dr. Prout's Paper on the Ultimate Composition of Simple Alimentary Substances.

II. Illustrative of Mr. Nixon's Paper on the Hills of the Penine Chain.

BY

RICHARD TAYLOR, F.S.A. F.L.S. M. Astr. S. &c.

AND

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## TO CORRESPONDENTS.

Mr. E. GIDDY's Communication on the Climate of Penzance, and Mr. MARSHALL's Meteorological Summary from Kendal, will appear in our next.

Mr. BRAYLEY's paper on the *Tshettik* also in our next.

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**NEW SERIES.**

No 15.—MARCH 1828.

BY

RICHARD TAYLOR, F.S.A. F.L.S. M. Astr. S. &c.

AND

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

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## TO CORRESPONDENTS.

Mr. J. Phillips's Paper on the Geology of the Vale of Pickering has been received, and will meet with early attention.

Mr. Tredgold's Theory of the Resistance of Fluids, in our next.

Dr. Forster's Journal of the Flowering of Spring Plants, in our next.

Mr. Maccough's Paper has been received.

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COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE, THE LIBERAL AND FINE ARTS, AGRICULTURE, MANUFACTURES, AND COMMERCE.

## NEW SERIES.

No 16.—APRIL 1828.

WITH TWO ENGRAVINGS:

I. Illustrative of Mr. JOHN PHILLIPS's Remarks on the Geology of the North Side of the Vale of Pickering, Yorkshire.
II. Illustrative of Mr. TREDGOLD's Theory of the Resistance of Fluids.

BY

RICHARD TAYLOR, F.S.A. F.L.S. M. Astr. S. &c.

AND

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


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## TO CORRESPONDENTS.

Mr. GALBRAITH's Paper in our next.

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THE VARIOUS BRANCHES OF SCIENCE, THE LIBERAL AND FINE ARTS, AGRICULTURE, MANUFACTURES, AND COMMERCE.

NEW SERIES.

No 17.—MAY 1828.

WITH AN ENGRAVING,

Illustrative of Dr. WEBER'S Paper on SAVART'S Experiments on the Undulations of Sound.

BY

RICHARD TAYLOR, F.S.A. F.L.S. M. Astr. S. &c.

AND

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

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## TO CORRESPONDENTS.

The Paper "On Single and Erect Vision," in our next Number.

Mr. MARTIN'S "Geological Memoir on a Part of Western Sussex" has been received, and will be noticed in our next.

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## NEW SERIES.

No 18.—JUNE 1828.

WITH A PLATE,

Illustrative of Mr. R. WALKER's Paper on the artificial Production of Cold.

BY

RICHARD TAYLOR, F.S.A. F.L.S. M. Astr. S. &c.

AND

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

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## TO CORRESPONDENTS.

We omitted to mention, in our last notice to Correspondents, that Mr. KINGSTON's promised communications, On the geology of the Bovey district, would be very acceptable.

The Review of Mr. MARTIN's Geological Memoir was prepared for insertion in the present Number; but the length to which the Proceedings of Learned Societies have extended, has compelled us to defer it till our next Number.

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THE

# PHILOSOPHICAL MAGAZINE,

OR

# ANNALS

OF

## CHEMISTRY, MATHEMATICS, ASTRONOMY, NATURAL HISTORY, AND GENERAL SCIENCE.

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BY

RICHARD TAYLOR, F.S.A. F.L.S. M. Astr. S. &c.

AND

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

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

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VOL. III.

NEW AND UNITED SERIES OF THE PHILOSOPHICAL MAGAZINE AND ANNALS OF PHILOSOPHY.

JANUARY—JUNE, 1828.



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## PLATES.



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### Erratum.

P. 363, line 10 from bottom: *for* "Irregular masses of siliceous matter that have obviously invested, either wholly or partially, crystals of iron-ore. Iron-pyrites, quartz or garnet, are of frequent occurrence"—*read* "Irregular masses of siliceous matter that have obviously invested, either wholly or partially, crystals of iron-ore, iron-pyrites, quartz or garnet, are" &c.

THE

# PHILOSOPHICAL MAGAZINE

AND

# ANNALS OF PHILOSOPHY.

[NEW SERIES.]

*JANUARY* 1828.

## I. *On the Theory of Capillary Action, and the Depression of the Mercury in the Tubes of Barometers.* *By* J. Ivory, *Esq. M.A. F.R.S.**

THE elevation and depression of liquids in capillary tubes, that is, tubes of a very small bore resembling a hair, has long occupied the attention of natural philosophers. If we would acquire a precise notion of the present state of this branch of science, we must consider it in two separate points of view. In the first place we are in possession of a mathematical theory which quadrates exactly with the phænomena; but, in the second place, when we direct our attention to the physical foundation of this theory, we find as many opinions as there are different inquirers, and no one account is entirely free from difficulties. My present intention is to make some observations on the first part of the subject, without touching at all on the second, which is reserved for future discussion.

The mathematical theory of the capillary phænomena is not complicated, being derived from two independent principles. The first of these is the fact, that the surface of the same liquid always makes the same angle with the surfaces of solids immersed in it, when the matter of the solids is the same. In the case of glass and water, and generally whenever a solid is *wetted* by a liquid, the two surfaces touch one another, or make an infinitely small angle. In glass and mercury, the two surfaces are inclined to one another in an angle of about 42°.

* Communicated by the Author.

The second principle is the equation of the curve surface of the liquid under the influence of the capillary force. When a liquid is elevated above the general level by capillary action, the surface is always concave upward; on the contrary, it is always convex upward, when the liquid is depressed below the level. In either case, the distance of any point in the curve surface from the level, is proportional to the sum of the curvatures estimated in any two directions at right angles to one another. To speak more precisely, let a normal be drawn to the curve surface at any point; make two planes perpendicular to one another, pass by the normal and intersect the curve surface; put R and R′ for the radii of curvature of the two sections, and $y$ for the distance of the point from the general level; the equation of the surface in the capillary space is,

$$y = \beta \left\{ \frac{1}{R} + \frac{1}{R'} \right\},$$

$\beta$ being a quantity to be determined by experiment. It is a property well known to mathematicians, that the sum of the curvatures is always the same, provided the two sections be at right angles to one another.

A little reflection will show that the two principles we have mentioned fully ascertain every circumstance relating to the capillary phænomena. The equation determines the position of every point in the curve surface above or below the general level; and the other principle limits the extent of the same surface in the capillary space, by making known the inclination of its extreme boundary to the surface of the immersed solid.

It is evident that the equation cannot be verified by direct observation. It was first suggested by the resemblance of the surface of liquids under capillary action to the class of elastic curves treated of in geometry. In reality, there seems to be no circumstance accompanying the greater or less elevation or depression, except a variation of curvature; so that the attention of the inquirer is naturally directed to examine the relation of these two things. There is no other way of proving that the equation accords with nature, but by comparing the mathematical deductions from it with the results obtained by careful and accurate experiments. The most general classification of the capillary phænomena we owe to Dr. Jurin, who makes the quantity of the displaced fluid, which is the proper measure of the capillary force, proportional in all cases to the length of the line of common section of the surfaces of the fluid and solid immersed in it. I do not here allude to the physical cause assigned by Dr. Jurin, which will not bear examination;

amination; but to the law of the phænomena, considered as a general fact allowed to be consonant to experience. Now one of the most curious points in Laplace's theory of capillary action*, is a demonstration, deduced from the equation of the curve surface, which proves that the volume of elevated fluid, or the space made void by the capillary force in the case of a depression, is proportional to the interior periphery of any cylindrical or prismatic tube immersed in the fluid. And it is correct to affirm generally, that what is called Dr. Jurin's theory, is no other than a mathematical consequence flowing from the two principles we have laid down.

There is one set of facts very proper to bring the exact agreement of the mathematical theory with nature to a severe trial. We allude to the depression of the mercury in the tubes of barometers of various diameters. The accuracy requisite in modern philosophical pursuits has drawn the attention of experimentalists to determine the quantity of the depression, in order to derive the true height of the mercury, from the observed height. In tubes from about one tenth of an inch in diameter, to seven or eight tenths, the convex curvature of the surface and the depression are found to vary very quickly and notably; and the comparison of the theory with such a series of connected experiments, cannot but furnish a delicate test of its exactness. Here, however, a difficulty occurs. The mathematical determination of the depression is a problem of great difficulty, which does not yield to the methods of investigation usually employed by analysts, and which has not yet been solved in a satisfactory manner. The remainder of this article is an attempt to overcome this difficulty.

I put $y$ for the vertical ordinate of a point in the convex surface of the mercury, or the distance below the general level; $r$ for the distance of the same point from the axis of the tube; and $z$ for the sine of the inclination of the vertical section of the curve surface to $r$, or to the horizon: according to what is taught in geometry, $\frac{dr}{dz}$ is the radius of curvature of the vertical section; and $\frac{r}{z}$, the radius of curvature of the section at right angles to the vertical section: and hence we obtain from the principles laid down,

$$\frac{dz}{dr} + \frac{z}{r} = 4\beta^2 y,$$

$$\frac{dy}{dr} = \frac{z}{\sqrt{1-z^2}},$$

* *Suppl. à la Théorie de l'Action Capillaire*, p. 10.



$4\beta^2$ being

$4\beta^2$ being a quantity to be found by experiment in tubes of any given matter.

In the glass tubes of which barometers are made, $4\beta^2$ is very nearly equal to 496, and $\beta$ to 7; and the value of $z$ at the surface of such tubes, or the sine of the inclination of the mercury to the horizon, may be taken equal to 0·735. These numbers, which are very convenient, accord nearly with the results of the best experiments, or at least approach to them within the limits of the errors to which all such experimental determinations are liable.

Instead of the curve surface actually formed in the tube, I shall now consider another similar surface, of which the linear dimensions are increased fourteen times, or in the proportion of $2\beta$ to 1. If $y$ represent the vertical ordinate of this new curve, and $y$ stand for $2\beta r$, we shall have these equations, viz.

$$\frac{dz}{dx} + \frac{z}{x} = y$$

$$\frac{dy}{dx} = \frac{z}{\sqrt{1-z^2}}.$$

Exterminate $y$, and put $t = \frac{x}{2} = \beta r$; then

$$\frac{ddz}{dt^2} + \frac{dz}{t\,dt} - \frac{z}{t^2} = \frac{4z}{\sqrt{1-z^2}}.$$

It may be proper to observe here, that the depression is the value of the vertical ordinate, when $z$ and $x$ vanish together, in which case also $\frac{z}{x} = \frac{dz}{dx}$. Hence the depression is equal to $\frac{2z}{x}$ or $\frac{2dz}{dx}$, that is, to $\frac{z}{t}$ or $\frac{dz}{dt}$, when $z$ and $t$ are both evanescent.

Next assume,
$$z = qtc^{\int \omega\,dt},$$
$c$ being the base of Napier's logarithms, and $q$ a constant quantity, which is no other than the depression. Substitute the values of $z$ and its fluxions in the last equation, leaving untouched the radical on the right-hand side; then,

$$\omega^2 + 3\frac{\omega}{t} + \frac{d\omega}{dt} = \frac{4}{\sqrt{1-z^2}}$$

$$\frac{dz}{dt} = z\left(\omega + \frac{1}{t}\right).$$

In the foregoing operations $z$ and $\omega$ are considered as functions of $t$: but I shall now suppose that $z$ is a function of $t$, and $\omega$ a function of the independent variables $z$ and $t$. In the last equation we must therefore write $\frac{d\omega}{dt} + \frac{d\omega}{dz}\cdot\frac{dz}{dt}$ for $\frac{d\omega}{dt}$;

$\frac{d\omega}{dt}$; which being done, and the value of $\frac{dz}{dt}$ substituted, we shall get this equation in partial fluxions, viz.

$$\omega^2 + \omega \frac{d\omega}{dz} z + \frac{d\omega}{dz} \cdot \frac{z}{t} + 3\frac{\omega}{t} + \frac{d\omega}{dt} = \frac{4}{\sqrt{1-z^2}}, \qquad (1)$$

to which we must join the value of $\frac{dz}{dt}$, viz.

$$\frac{dz}{dt} = z\left(\frac{1}{t} + \omega\right). \qquad (2)$$

I next write the equation (1) in this manner,

$$\frac{1}{t} \cdot \frac{d.\,\omega z^3}{z^2 dz} + \frac{d\omega}{dt} + \frac{d.\,\omega^2 z^2}{2z\,dz} = \frac{4}{\sqrt{1-z^2}};$$

and assume, $\omega = \varrho + Az^2 + Bz^4 + \&c.$

$\varrho$, A, B, &c. being functions of $t$. Substitute this value of $\omega$, and expand the radical on the right-hand side of the equation; then, by equating the coefficients of the like powers of $z$, we shall get,

$$\begin{aligned} &\varrho^2 + 3\frac{\varrho}{t} + \frac{d\varrho}{dt} = 4 \\ &4\varrho A + 5\frac{A}{t} + \frac{dA}{dt} = 2 \\ &6\varrho B + 7\frac{B}{t} + \frac{dB}{dt} = \frac{3}{2} - 3A^2 \\ &\&c. \end{aligned} \qquad (3)$$

These equations give us,

$$\begin{aligned} \varrho &= t - \frac{t^3}{6} + \&c. \\ A &= \frac{t}{3} - \frac{t^3}{6} + \&c. \\ B &= \frac{3}{16}t - \frac{7}{48}t^3 + \&c. \\ &\&c. \end{aligned}$$

There is another consequence of the first of the equations (3) which it is necessary to notice. Put

$$\lambda = c^{\int \varrho dt};$$

then we readily derive from the equation mentioned,

$$\frac{dd\lambda}{dt^2} + 3\frac{d\lambda}{t\,dt} = 4\lambda;$$

and by integrating in a series, without introducing any arbitrary constants, which the present purpose does not require, we shall get,

$$\lambda = 1 + \frac{t^2}{2} + \frac{t^4}{2^2.3} + \frac{t^6}{2^2.3^2.4} + \frac{t^8}{2^2.3^2.4^2.5} + \&c.$$

If

If $l$ be the diameter of the tube, then $t = \beta r = \frac{7}{2} \cdot l$, and $t^2 = 12{\cdot}25\, l^2$: thus we have,

$$\begin{aligned}\lambda = 1 &+ 6{\cdot}125\, l^2 \\ &+ 12{\cdot}505\, l^4 \\ &+ 12{\cdot}756\, l^6 \\ &+ 7{\cdot}819\, l^8 \\ &+ 3{\cdot}193\, l^{10} \\ &+ 0{\cdot}931\, l^{12} \\ &+ \&c.\end{aligned} \qquad (A)$$

In all tubes a few terms of this series will give the value of $\lambda$ with sufficient exactness.

Substitute the value of $\omega$ that has been found in the equation (2); then

$$\frac{dz}{z\,dt} = \frac{1}{t} + \varrho + Az^2 + Bz^4 + \&c.$$

The quantities $\varrho$, A, B, &c. evidently vanish when $t$ is equal to zero; let us then inquire what are their values when $t$ is infinitely great. When $t$ is infinite, the equation (1) becomes simply,

$$\frac{d \cdot \omega^2 z^2}{2\,z\,dz} = \frac{4}{\sqrt{1 - z^2}};$$

hence,

$$\omega = \frac{2\sqrt{2 - 2\sqrt{1 - z^2}}}{z},$$

or

$$\omega = 2 + \frac{z^2}{4} + \frac{7 \cdot z^4}{64} + \&c.$$

Thus it appears that while $t$ increases from zero to be infinitely great, $\varrho$, A, B, &c. increase from zero to the finite quantities 2, $\frac{1}{4}$, $\frac{7}{64}$, &c. The terms in the values of $\omega$, and of $\frac{dz}{dt}$, which are omitted, do not, therefore, affect the approximation except in a very limited degree. The same conclusion may be obtained another way; namely, by solving the equations (3) inserieses of the descending powers of $t$. If this be done, the parts of $\varrho$, A, B, &c. independent of $\frac{1}{t}$, will agree with the values above assigned.

Put $2a$ for the value of $\omega$, or of $\frac{dz}{z\,dt}$, when $t$ is infinite; then,

$$a = \frac{\sqrt{2 - 2\sqrt{1 - z^2}}}{z} = 1 + \frac{z^2}{8} + \frac{7z^4}{128} + \&c.$$

Subtract $a\varrho$ from both sides of the equation before found, then,

$$\frac{dz}{z\,dt} - a\varrho = \frac{1}{t} - (a-1)\,\varrho + Az^2 + Bz^4 + \&c.:$$

and,

and, by making,

$$A' = A - \frac{\varrho}{8} = \frac{5}{24}\,t - \frac{7}{48}\,t^3 + \&c.$$

$$B' = B - \frac{7\varrho}{128} = \frac{17}{128}\,t - \frac{35}{256}\,t^3 + \&c.$$

we get, $$\frac{dz}{z\,dt} - a\varrho = \frac{1}{t} + A'z^2 + B'z^4 + \&c.$$

The left side of this equation is equal to zero when $t$ is infinite; for $a\varrho$ then becomes $2a$. From this it follows that A′, B′, &c. are evanescent both when $t$ is equal to zero and when it is infinite. The terms on the right side, except the first, do not therefore increase indefinitely, but always remain inconsiderable.

Assume, $$\frac{dz}{az} = \frac{d\sigma}{\sigma}:$$

then, $$\sigma = z - \frac{z^3}{16} - \&c.$$

$$z = \sigma + \frac{\sigma^3}{16} + \&c.$$

$$a = 1 + \frac{\sigma^2}{8} + \frac{9\sigma^4}{128} + \&c.:$$

then, by dividing all the terms of the foregoing equation by $a$, and introducing $\sigma$ for $z$, we shall get,

$$\frac{d\sigma}{\sigma\,dt} - \varrho = \frac{1}{a}\cdot\left\{\frac{1}{t} + A'\sigma^2 + \left(B' + \frac{A'}{8}\right)\sigma^4 + \&c.\right.$$

Next put, $$\frac{d\sigma}{\sigma\,dt} - \varrho = \frac{ds}{s\,dt} \because \sigma = s\,c^{\int\varrho\,dt} = \lambda . s.$$

Let $a'$ stand for the same function of $s$ that $a$ is of $\sigma$, or of $\lambda s$: then, making,

$$A'' = A' = \frac{5}{24}\,t - \frac{7}{48}\,t^3 + \&c.$$

$$B'' = B' + \frac{A'}{8} = \frac{61}{384}\,t - \frac{119}{768}\,t^3 + \&c.$$

we shall get,

$$\frac{a'ds}{s\,dt} = \frac{a'}{a}\cdot\left\{\frac{1}{t} + A''\lambda^2 . s^2 + B''\lambda^4 . s^4 + \&c.\right.$$

The fraction $\frac{a'}{a}$ must next be reduced to a series of the powers of $s$. Now,

$$\frac{a'}{a} = \frac{1 + \frac{1}{8}s^2 + \frac{9}{128}s^4 + \&c.}{1 + \frac{1}{8}\lambda^2 s^2 + \frac{9}{128}\lambda^4 s^4 + \&c.}:$$

and,

and, by division,

$$\frac{a'}{a} = 1 - t\,P.\,s^2 - t\,P'.\,s^4 - \&c.$$

$$P = \frac{\lambda^2 - 1}{8t} = \frac{t}{8} + \frac{5}{96}\,t^3 + \&c.$$

$$P' = \frac{7\lambda^4 + 2\lambda^2 - 9}{128\,t} = \frac{t}{8} + \frac{41}{384}\,t^3 + \&c.$$

The series for $\frac{a'}{a}$ must next be substituted, and, after having multiplied and reduced, we shall get,

$$\frac{a' d s}{s\,d t} = \frac{1}{t} + A'''\,s^2 + B'''\,s^4 + \&c.$$

$$A''' = A''\,\lambda^2 - P = \frac{t}{12} + \frac{t^3}{96} + \&c.$$

$$B''' = B''\,\lambda^4 - P' - A''\,\lambda^2.\,Pt = \frac{13}{384}\,t + \frac{23}{768}\,t^3 + \&c.$$

The principle of all this analysis lies in this, That, the left side of the equation being equal to zero when $t$ is infinite, all the terms on the right side except the first must be evanescent both when $t$ is zero and when it is infinite. These terms, therefore, remain always inconsiderable, and of no value except in some tubes of small diameter. In order to exhibit the equation in the most simple form possible, I finally put,

$$\frac{a'\,d s}{s} = \frac{d u}{u},$$

$$u = s + \frac{s^3}{16} + \&c.$$

$$s = u - \frac{u^3}{16} - \&c.:$$

and, by introducing $u$ for $s$,

$$\frac{d u}{u\,d t} = \frac{1}{t} + A^{iv}\,u^2 + B^{iv}\,u^4 + \&c.$$

$$A^{iv} = A''' = \frac{t}{12} + \frac{t^3}{96} + \&c.$$

$$B^{iv} = B''' - \frac{A'''}{8} = \frac{3}{128}\,t + \frac{11}{384}\,t^3 + \&c.$$

We must now integrate, and for this purpose write the last equation in this manner,

$$\frac{d.\frac{u}{t}}{d t} = \frac{A^{iv}}{t}\,u^3 + \frac{B^{iv}}{t}\,u^5 + \&c.:$$

then assume, $\frac{u}{t} = q + Q\,u^3 + Q'\,u^5\,\&c.$

$q$ being

$q$ being a constant, and Q, Q′ &c. functions of $t$. By differentiating,

$$\frac{d.\frac{u}{t}}{dt} = \frac{dQ}{dt}u^3 + \frac{dQ'}{dt}u^5$$

$$+ \frac{du}{u\,dt}(3\,Q\,u^3 + 5\,Q'\,u^5 + \&c.)$$

Substitute the value of $\frac{du}{u\,dt}$, and equate the coefficients of the two equal quantities, then

$$\frac{d.Qt^3}{t^3\,dt} = \frac{A^{iv}}{t},$$

$$\frac{d.Q't^5}{t^5\,dt} = \frac{B^{iv}}{t} - 3\,A^{iv}\,Q:$$

and hence,
$$Q = \frac{t}{48} + \frac{t^3}{576} + \&c.$$

$$Q' = \frac{t}{256} + \frac{3\,t^3}{1024} + \&c.$$

These coefficients being known, we have the value of the constant $q$, which it is most convenient to arrange according to the powers of $t$, viz.

$$q = \frac{u}{t} - t\left(\frac{u^3}{48} + \frac{u^5}{256}\right)$$

$$- t^3\left(\frac{u^3}{576} + \frac{3\,u^5}{1024}\right).$$

It has already been noticed that the depression is the value of $\frac{z}{t}$ when $z$ and $t$ are both evanescent. But it is obvious that the vanishing fractions, $\frac{z}{t}$ $\frac{\sigma}{t}$, $\frac{s}{t}$, $\frac{u}{t}$ have all the same limit: wherefore $q$ is the depression. It does not however belong to the surface of the mercury in the tube, but to a surface increased fourteen times in its linear dimensions. To get the real depression we must therefore divide by $2\beta$. Now $t = \beta r = \frac{\beta}{2}\,l$, and $t^2 = 12{\cdot}25\,l^2$; and we thus obtain the following formula for the real depression,

$$q = \frac{u}{49\,l} - l\left(\frac{u^3}{192} + \frac{u^5}{1024}\right)$$

$$- l^3\left(\frac{12{\cdot}25}{2304}u^3 + \frac{36{\cdot}75}{4096}u^5\right).$$

The symbol $u$ stands for a known function of $s$; but in practice it will be most convenient to have $q$ expressed immediately

 in

in terms of $s$. The necessary operations being performed, we finally obtain,

$$q = \frac{1}{49\,l} \times \left(s + \frac{s^3}{16} + \frac{5\,s^5}{256} + \frac{119\,s^7}{12288} + \frac{393\,s^9}{65536}\right)$$
$$- l \times \left(\frac{s^3}{192} + \frac{s^5}{512}\right). \qquad \text{(B)}$$

The term multiplied by $l^3$ being omitted, as it does not affect the value of $q$ in the fifth decimal place. In order to find $s$, we must compute $\sigma$, for which purpose we readily obtain this series,

$$\sigma = z - \frac{z^3}{16} - \frac{z^5}{128} - \frac{35\,z^7}{12288} - \frac{137\,z^9}{98304} + \&c.:$$

and $z$ being 0·735, we get $\sigma = 0{\cdot}70805$. Then

$$s = \frac{{\cdot}70805}{\lambda},$$

$\lambda$ being computed by the formula (A).

As the diameter $l$ decreases, $s$ approaches indefinitely to $\sigma$, and $u$ to $z$; so that, for tubes of an extremely small bore, the depression is,

$$q = \frac{z}{49\,l} = \frac{{\cdot}015}{l}.$$

On the other hand, the diameter of the tube increasing, $s$ decreases very rapidly; and when the diameter is half an inch, or greater, all the powers of $s$ are inconsiderable, and we have simply,

$$q = \frac{s}{49\,l} = \frac{{\cdot}70805}{49\,l \cdot \lambda} = \frac{{\cdot}01445}{l \,.\, \lambda}.$$

The first term of $q$ in the formula (B) being the expansion of a known function, it may be continued to any degree of approximation; but the terms set down are sufficient even for very small tubes. Suppose $l = \frac{1}{20}$, thus $\lambda = 1{\cdot}01539$, $s = {\cdot}69736$, and

$$q = \frac{{\cdot}72274}{2 \cdot 45} - \frac{{\cdot}002}{20} = {\cdot}29490.$$

Next, let $l = \frac{1}{2}$, then $\lambda = 1{\cdot}5460$, $s = {\cdot}1997$, and

$$q = \frac{{\cdot}1997}{24 \cdot 5} = 0{\cdot}0815.$$

I have subjoined a Table of the depressions in tubes of various bores, and have added the experimental determinations published by Lord Charles Cavendish; by which it will be seen that the agreement of the theory with experiment is very satisfactory.

*Depression*

*Depression of the Mercury in the Tubes of Barometers.*

| Diameter. inches. | Depression. inches | Observed by Ld. Cavendish. |
|---|---|---|
| 0·05 | 0·29490 | |
| ·10 | ·14026 | 0·140 |
| ·15 | ·08628 | ·092 |
| ·20 | ·05811 | ·067 |
| ·25 | ·04077 | ·050 |
| ·30 | ·02919 | ·036 |
| ·35 | ·02110 | ·025 |
| ·40 | ·01534 | ·015 |
| ·45 | ·01117 | |
| ·50 | ·00815* | ·007 |
| ·60 | ·00431* | ·005 |
| ·70 | ·00228 | |
| ·80 | ·00119 | |

Dec. 14, 1827. J. Ivory.

## II. *Heights of some of the principal Beds of Ingleborough Hill and Moughton Fell, Yorkshire.* By John Nixon, *Esq.*

*To the Editors of the Philosophical Magazine and Annals.*

Gentlemen,

In the xith volume of the Annals of Philosophy, I proposed to the geologist the measurement, by one barometer only, of the heights and dip of strata known to be nearly horizontal, and furnished at the same time the requisite instructions and tables. From the numerous measurements of this description made by me in different parts of the north-west of Yorkshire, I beg to transmit you a selection of such as may serve to determine the thicknesses and dip of the principal beds of Ingleborough Hill and Moughton Fell.

Having carefully measured by trigonometry the heights of the summits and other elevated parts of the hills, as well as of different places at their bases, barometrical observations were

* In the Supplement to the Encyclopædia, Article Fluids, there is an arithmetical blunder in each of these numbers, as will appear by having recourse to the formula. I was obliged to notice this point on a former occasion, in order to stop the triumph of an antagonist, who has stuck to my skirts for a period of at least six years, and who seems to have great confidence in the efficacy of the adage, *If one way will not do, another will.*

 regularly

regularly made in the course of the day at the lowest and highest of the trigonometrical stations within reach, in order to ascertain and correct the error of the measurements by the barometer. When the station at the base could not conveniently be revisited, the observation at the superior one, together with their difference of altitude, served to determine the fall or rise of the barometer.

The following list, comprising the chief beds of the Ingleton Fells, is arranged conformably to their order of superposition.

A 1. Various beds of grits; Yorkshire paving grits (very siliceous), micaceous sandstones (flagstones and roofing slates) with beds of shale, bearing on some hills three seams of coal.

2. Very rough millstone grit (240 feet thick on Pen-y-gent).

B. The top lime of the Ingleton Fells and the upper part of Wharfdale. It is of very variable thickness, and contains lead in Dod and Settron Fells.

C. 1. A thick bed of various shales with grits and flagstone beds interstratified. It contains a seam of coal on Noughtberry hill at the Garsdale, King's-cross, and ——— pits.

2. Various beds of paving grits, flagstone beds, and shales. In the lower half of this section are two or three very thin beds of limestone.

3. Yorkshire paving grit and shale.

D. 1. Limestone shale containing beds of Dent black marble.

2. Gray limestone without shale, &c. Lead has been found in this bed south-west of the summit of Ingleborough, and is at present procured on the north-west side of the hill.

3. (In some places) a very thin bed of clayshale containing a slender seam of coal.

E. Grauwacke, (clayslate, &c.?)

*Heights of different points of these sections, with their bearings and distances from the summit of Ingleborough.*

A. 1 & 2. Height of the *station* on the loftiest point of Ingleborough, 2374 feet above the Irish Sea.

B. Height of the upper surface of the top lime on the summit of Simon Fell (part of Ingleborough) one mile E.N.E., 2125 feet.

C. 1. Height of the under surface of the top lime (at a spring) half mile E.S.E., 2062 feet;—(at a spring dividing Simon Fell from Ingleborough) half mile E.N.E., 2061 feet;—(on Simon Fell) $1\frac{1}{4}$ mile E.N.E., 2035 feet.

feet. Height of some quarries of grit slate, $\frac{3}{4}$ mile S., 2090 feet;—(on Simon Fell, east side), $1\frac{1}{4}$ mile E. by N., 1968 feet.

C. 2. Height of the upper surface of a thin (15 to 20 feet) bed of limestone on Simon Fell, $1\frac{3}{4}$ mile E., 1608 feet. Height of another bed of limestone near the last, 1479 feet.

3. Quarry of thin grit slate, $1\frac{3}{4}$ mile E., 1427 feet.

D. 1. Height of the upper surface of the Dent marble bed, $1\frac{1}{2}$ mile S. by W. (in the direction of Newby), 1467 feet;—$1\frac{1}{2}$ mile S. by E., 1368 feet;—$1\frac{1}{2}$ mile S.S.E., 1395 feet;—(on Simon Fell) $1\frac{3}{4}$ mile E.S.E., 1350 feet;—(near Selside) 2 miles E., 1270 feet;—(on Park Fell, part of Simon Fell,) $2\frac{1}{4}$ miles N.E., 1166 feet.

E. Height of the grauwacke (in the Ribble under New-Inn Bridge near Harton), $4\frac{1}{4}$ miles E.S.E., 740 feet;—(near the blue slate quarries at the south-east end of Moughton Fell) $4\frac{1}{2}$ miles S.E., 1116 feet. (This is the loftiest point of the denuded grauwacke.) At Hunterstyle (near Cromack) 3 miles S.E., 1047 feet;—(under the huge grauwacke boulders near Austwick) $3\frac{3}{4}$ miles S.S.E., 705 feet;—(in Clapham beck, a patch a little below the recently discovered cave in the limestone,) 3 miles S. by E., about 600 feet;—(in the Greta, 3 miles above Ingleton,) $1\frac{1}{4}$ mile N.W., about 750 feet.

*Dip of the beds above the grauwacke.*—A comparison of the heights of C. 1. and especially of D. 1. to the *south* and *north* of the station on Ingleborough, indicates the dip of these beds to be northerly. Near Newby the height of D. 1. is 1467 feet; and on Park Fell, $3\frac{2}{3}$ miles to the N.E. 1166 feet. A fall of 300 feet in a distance of 19,360 feet is equal to a dip of $0° 53' 16''$; but as the line of maximum depression is probably more to the north, its angular value will exceed 1°.—(At the marble quarries east of Dent and N.N.E. of the *station*, the height of this bed was found to be 1030 feet*.)

*Dip of the grauwacke beds.*—In the Greta, 80° N.E.;—at Hunterstyle, and at the south-east end of Moughton Fell, 45° S. by E.;—on Swartmoor, 80° northerly.

*Thicknesses of the beds.*—A. 2. and perhaps a portion of 1. about 234 feet on Ingleborough.

B. The top lime is about 80 feet thick on Ingleborough and Simon Fell.

* Visually estimated the dip appears to be most rapid to the south of the summits of Ingleborough and Whernside.

C. On

C. On Ingleborough, Simon Fell, and Park Fell, about 700 feet thick.

D. As the upper surface of the grauwacke, &c. apparently diversified with hill and dale previous to the deposition of the superincumbent beds, is not a regular plane, the thickness of this section must vary in conformity. In some parts of Wharfdale it is very probably upwards of 1200 feet in thickness.

I have the honour to be, Gentlemen,
Your most obedient servant,

Leeds, Dec. 8, 1827. JOHN NIXON.

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## III. *On the Thermal Waters of the Alps.* *By* R. BAKEWELL, *Esq.*

*To the Editors of the Philosophical Magazine and Annals.*

Gentlemen,

WHEN we approach a range of lofty mountains, like that of the Pennine Alps, and observe the calcareous strata on the outer part of the range, bent and contorted in various directions; when we further observe beds of limestone and puddingstone alternating and placed in an elevated position, as we advance to the central part of the range; and that the beds of granite in the central part are frequently vertical; we feel assured that their present contorted or vertical position is not the original one. The opinions of geologists have been much divided respecting the cause or causes that have elevated mountains and given a vertical position to beds that once formed the bottom of the ocean. Those who maintain that subterranean heat has expanded and broken the solid crust of the globe, and has raised from vast depths the ancient bed of the ocean, appeal to a cause that is known to exist, and which seems sufficient to explain most of the various appearances which alpine regions present.

In opposition to this theory, it is asserted that there are no remaining vestiges of the action of subterranean fire in the Alps;—but this I am convinced is erroneous. It is true that from near the source of the Rhone, to the foot of the Little St. Bernard, there does not occur any known rock of a volcanic character, with the doubtful exception of some rocks in the valley of Sass, and in the Valorsine. I have examined various parts of this range on the northern side of the highest mountains in the Alps, along a line of one hundred and twenty miles; and though I could discover no indications of the action of subterranean heat in the rocks themselves, I was greatly surprised to observe, the numerous thermal springs that are abundantly

abundantly gushing out at the feet of the primary mountains, near the junction of the mica-slate, or the dark schist passing into mica-slate with the lowest calcareous beds of that vast series of limestone strata, which forms the outer ranges of the Alps. Numerous as these hot springs are on the northern side of the Alps, and not unfrequent on the southern side also, it appeared to me remarkable, that they had hitherto been regarded as isolated phænomena; and their geological position had not been noticed. It is true, some of the warm springs in the Valais and in Savoy had been long known and visited, but the greater number have been discovered since Saussure published his *Voyages dans les Alpes;* and it appears probable, that they would every where be found near the junction of the primary and secondary rocks, were it not for *eboulements* that have covered them with a heap of ruins, or that torrents from the glaciers have mixed with them, and reduced their temperature. Since I visited Savoy in 1821 and 1822, another considerable warm spring has been discovered near the village of Chamouni, at the foot of a glacier; and in 1820 several thermal springs were discovered in that branch of the Alps which extends to Grenoble.

I shall here briefly enumerate the principal known thermal waters in the Pennine Alps, and add some observations and inferences, which I trust will be acceptable to several of your readers.

Naters *in the Haut Valais.*—The warm spring rises under a rock of mica-slate on the north side of the Rhone. The temperature when I visited the place was 86° Fahrenheit, but it is variable from the intermixture with surface-water. At the time of the great earthquake of Lisbon in 1755, the mountain above the spring, I was informed, opened and threw out a considerable quantity of hot water.

Leuk *in the Haut Valais,*—situated in a deep gorge on the northern side of the Rhone. There are twelve springs, varying in temperature from 117° to 126°. These springs have been long known, and are visited by patients from various parts of Europe.

The Valley of Bagnes *in the Bas Valais.*—The warm springs in this valley were buried under a heap of debris from the fall of part of a mountain, which destroyed the baths, the village of Bagnes and one hundred and twenty inhabitants in the year 1545. The name of the valley is obviously derived from the baths. The temperature of the water unknown.

Chamouni.—The thermal waters at this place have been discovered since I visited Chamouni in 1821. I have received no account of the temperature; baths have recently been erected.

erected. The situation is near the junction of mica-slate with the lowest beds of secondary limestone.

St. Gervaise,—situated on a deep gorge on the north-east side of Mont Blanc. The thermal water rises near the junction of mica-slate and limestone. The temperature 94° to 98°. This spring was discovered about the year 1806: it is very copious. Baths have recently been erected, and are much frequented.

Aix les Bains *in Savoy;* the temperature from 112° to 117°. The thermal waters rise in great abundance from two springs situated at the foot of a lofty calcareous mountain, and are near the bottom of the great calcareous formation that forms the outer range of the Alps: there are also numerous hot springs in the vicinity, which the Sardinian government will not allow to be opened. Of the mode of douching at these baths, I have given a particular account in the first volume of my Travels in Savoy, Switzerland, and Auvergne. The thermal waters of Aix were well known to the Romans.

Moutiers *in the Tarentaise.*—The thermal waters rise in great abundance from the bottom of a nearly perpendicular mass of limestone. From the position of this rock, and its connection with those on the opposite side of the valley, in which the hot springs rise, I have no doubt that it is the lowest calcareous bed in that part of the Alps; but its junction with mica or talcose slate is not here seen. The thermal waters of Moutiers contain about two per cent of saline matter, chiefly common salt. The process of extracting it, I have described in the Philosophical Magazine, vol. lxiii. p. 86.

Brida *in the Tarentaise.*—The thermal waters of Brida were noticed in the ancient records of Savoy, but they were covered during a sudden inundation of the valley, and their situation was concealed for many years. In the summer of 1819, another inundation, occasioned by the breaking down of the side of a glacier, laid open the spring again. The rock from which the spring rises, is a greenish talcose slate passing into mica-slate: it is in junction with limestone. The temperature of the water is from 93° to 97° Fahrenheit. The geological position of this spring is more obvious than that of any of the other thermal waters which I visited, being situated close to the steep bank of the river Doron, where both the rocks are laid bare. There are some warm springs on the opposite bank of the river which rise in limestone; but the temperature is lower, owing to an intermixture with common water.

Saute de Pucelle, or *Virgin's Leap.*—There is a very copious thermal spring rising from the bottom of a perpendicular rock near the Isere, between the town of Moutiers and St. Maurice

St. Maurice at the foot of the Little St. Bernard; but owing to the difficulty of access to it, I did not visit it, to ascertain its temperature.

Beside the above thermal waters in the Pennine Alps, various thermal springs were discovered in the adjacent Alps, near Grenoble, in the year 1820: and it seems probable, that a series of these springs might be found, were proper search made, extending westward to the thermal waters of the Pyrenees; for in this line we should approach the southern border of the volcanic district of France. On the Italian side of the Pennine Alps there are also thermal waters: the warm baths of Cormayeur and of St. Didier are situated almost immediately under the southern escarpment of Mont Blanc. I was prevented by the weather from examining the geological position of these springs: their temperature is stated to be 94° of Fahrenheit *.

The inference that may be drawn from the geological position of these thermal waters near the junction of the calcareous beds with mica-slate, or the dark schist which passes into mica-slate, is, that the waters do not rise from the upper strata, but spring out of the lower or primary rocks; and as they break out near the feet of the highest range of the Alps, that extend from the northern side of the Simplon through the Valais and Savoy into France, we may with much probability infer, that these mountains are situated over or near to one common source of heat, by the agency of which they were originally elevated, and their beds placed in a position nearly vertical. This inference is in some degree supported by the well attested fact, that the districts where the hot springs are situated are subject to great and frequent convulsions, particularly in the upper valley of the Rhone. In the year 1755 and 1756, at Brieg, Naters, and Leuk, the ground was agitated by earthquakes every day from the 1st to the 27th of February; some of the shocks were so violent, that the steeples of the churches were thrown down, the walls split, and many houses rendered uninhabitable: many of the springs were dried up, and the waters of the Rhone were observed to boil. At three different times the inhabitants abandoned their houses and fled for safety into the fields. It has been before mentioned, that the mountain above the warm spring at Naters opened during the time of the great earthquake at Lisbon, and threw out hot water; at the same period

* Nearly all the thermal waters in the Alps emit sulphureous vapours, and are slightly saline, except the waters of Leuk, which have the highest temperature, and are inodorous and free from saline impregnation.

the warm saline springs at Moutiers ceased to flow for forty-eight hours. When the water returned, the quantity was said to be increased, and the saline impregnation was weaker. Former and more formidable agitations of the earth are recorded in the Haut Valais, particularly in the district where the principal hot springs are situated. The last earthquake of consequence in the Valais took place in January 1803.

I am informed that several of the retired valleys on the Italian side of the Alps, at the foot of the central chain, are subject to earthquakes, during which the ground has opened or sunk down in various parts, though these effects have been too local to excite attention at a distance. From these facts it seems as reasonable to infer that the thermal waters of the Alps owe their high temperature to subterranean fire, as that the hot springs in countries that have formerly been volcanic, derive their warmth from an internal unextinguished, but quiescent source of heat. No person who has attentively examined the lofty granitic plain to the west of Clermont Ferrand in France, and observed the granite in various parts pierced through by ancient volcanos that have poured currents of lava over its surface, or seen other parts where the granite itself has been changed by its contiguity to subterranean fire, or upheaved and intermixed with volcanic rocks;—no one, I say, who has observed this, can doubt that the hot springs of Mont Dor and Vichy derive their high temperature from a source of heat situated beneath the granite mountains, though ages have passed away since the volcanos of that country have been in an active state: and the only proof of the present existence of subterranean fire in Auvergne, is to be found in the hot springs themselves*. Nor can any adequate reason be assigned for attributing the high temperature of the thermal waters in the Alps, to any other cause than to a source of subterranean fire under these mountains,—a cause which is sufficient also to have produced their original elevation. It is however proper to state, that in some of the moun-

* I visited the extinct volcanos of Auvergne in the spring of 1822; and in 1823 I published an account of my observations in the second volume of my Travels, accompanied with cuts, and a section and outline of the country near Clermont, which, as far as I know, was the first attempt to render in this manner the structure of this volcanic district intelligible to the general reader. I discovered the bones of large mammalia in the fresh-water limestone under the volcanic tufa of Mount Gergovia,—a fact at that time unknown to Messrs. Brongniart, Cordier, and Brochant, to whom I mentioned it on my return through Paris. I was therefore surprised to see it intimated in the Quarterly Review for October 1827, that Dr. Daubeny and Mr. Scrope were the only Englishmen who had given an account of the extinct volcanoes in France.

tains

tains of the Alps the temperature may be slightly increased by a cause hitherto unnoticed. In the upper part of the secondary formations covering the granite, there are beds of gypsum, and this gypsum is anhydrous; but when exposed to air and moisture, it combines with water, and passes to the state of common gypsum: and during this combination we may suppose heat to be evolved, but the process must be extremely slow, and the heat evolved must be totally inadequate to raise the temperature of powerful streams to 126°. Saussure found the temperature of the water in the lower part of the salt mines of Bex, which are situated in the vicinity of gypsum, to be four degrees of Reaumur higher than the mean temperature of the earth. It is not improbable, though Saussure was not aware of the circumstance, that this small increase of temperature in the mines of Bex might be partly owing to the combination of water with gypsum: however, an increase of temperature it is well known, is observed in deep mines, far removed from the gypsum formation.

In reply to what I have advanced respecting the thermal waters in the Pennine Alps, it may be said that few thermal springs have been yet discovered in the northern range of the Alps which form the Bernese Oberland; but the difference in the geological structure of the two ranges will, I conceive, be sufficient to explain why hot springs are more rare in the latter, than in the southern range. Most of the highest mountains in the Bernese Alps are covered with secondary strata; and the valleys are chiefly excavated in these strata, or in enormous beds of sandstone and conglomerate, that form a thick intervening mass between the surface and the primary rocks, sufficient to obstruct the rise of thermal waters; for it has before been stated, that all the thermal waters in the Pennine Alps issue from the primary rocks, or near their junction with the lowest calcareous strata.

Hampstead, Dec. 8, 1827. ROBERT BAKEWELL.

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## IV. *On the Integration of Linear Differential Equations having Constant Coefficients and last Term any Function of the Indeterminate Quantity x.* *By* JOHN HERAPATH, *Esq.*

[Concluded from vol. ii. p. 425.]

WE shall now proceed to a few EXEMPLIFICATIONS of the formulæ.

*Ex.* 1. Suppose $n = 2$ then (17) becomes

$$y = e^{r_1 x} \{c_1 + \int e^{(r-r_1)x} \{ c + \int \mathrm{X} e^{-rx}$$

 the

the same as we have given in vol. ii. p. 420; for $A = -r - r_1$ by the well-known property of equations.

*Ex.* 2. Putting $n = 3$ our (17) gives

$$y = e^{r_2 x}\Big\{ c_2 + \int e^{(r_1 - r_2)x} \Big\{ c_1 + \int e^{(r - r_1)x} \Big\{ c + \int X e^{-rx} \qquad (19)$$

just as we should have it from (6) by putting for A its value $-r - r_1 - r_2$ and for $r_1$, which is a root of the reduced quadratic from the equivalent cubic, $r_1 - r$ found by (16). The solution however here given, exhibits to advantage the great superiority of our general investigation. Had we been contented with the process of successive eduction at first adopted, it would have been almost impossible to develop the simple and elegant law which reigns in the exponents.

*Ex.* 3. To carry on the reduction of the general formulæ to differentials of the 4th, 5th, &c. orders would be superfluous, the application being so very obvious. Let us therefore suppose that any two of the roots in (19) as $r_1$ and $r_2$ are equal. In this case because $dx$, $dx^2$, are understood as factors respectively to $c_1$, $c$, (19) is

$$y = e^{r_2 x}\Big\{ c_2 + c_1 x + \int^2 e^{(r - r_1)x} \Big\{ c + \int X e^{-rx}$$

or $$y = c_2 e^{r_2 x} + c_1 x e^{r_2 x} + \frac{c e^{(r - r_2)x}}{(r - r_2)^2},$$

supposing $X = 0$.

*Ex.* 4. Let us now imagine that all three roots are equal. Our (19) under these circumstances, it is evident gives

$$y = c_2 e^{rx} + c_1 x e^{rx} + \frac{c x^2 e^{rx}}{2} + e^{rx} \int^3 X e^{-rx},$$

or when $X = 0$

$$y = c_2 e^{rx} + c_1 x e^{rx} + \tfrac{1}{2} c x^2 e^{rx},$$

in which as before all three arbitrary constants appear. We have therefore the solutions directly with the full complement of arbitrary constants without any of the refined artifices with which D'Alembert found it necessary to aid the half-guessed imperfect solutions of Lagrange.

*Demonstration of the Completeness of the preceding general Solution.*

Because the roots $r, r_1, r_2, r_3, \ldots$ will admit of a great number of combinations of their differences, besides those we have assumed, it may reasonably be imagined that our general formula (17) or (18) not containing the whole, cannot be complete. Our attention will, therefore, now be directed to the consideration of this point. The formula most convenient for this

this object is (18), which is precisely the same as (17) but under a different form.

Now that part of (18) which consists of the sums of the exponentials clearly remains the same, whatever commutation takes place in the order of the roots: we must therefore apply our attention to the part affected with the successive integral signs. Taking the lowest integral sign, and integrating by parts,

$$\int e^{-rx} X = -\frac{e^{-rx}}{r}\left\{X + \frac{dX}{r} + \frac{d^2x}{r^2} + \frac{d^3X}{r^3} + \ldots\right.$$

of which the general term is

$$-\frac{e^{-rx} d^z X}{r^{z+1}}$$

Hence employing the sign $\sigma\!\!-$ to signify *produces*, *gives*, &c.

$$\int e^{(r-r_1)x}\int e^{-rx} X \;\sigma\!\!-\; -\int \frac{e^{-r_1 x} d^z X}{r^{z+1}}.$$

And because the part under the integral sign is of the same form as before, the general term of the quantity will be $\frac{e^{-r_1 x} d^{2z} X}{r_1^{z+1} r^{z+1}}$. Therefore

$$\int e^{(r-r_1)x}\int e^{-rx} X \;\sigma\!\!-\; \frac{e^{-r_1 x} d^{2z} X}{r_1^{z+1} r^{z+1}}.$$

Pursuing the same course we at length have

$$e^{x r_{n-1}}\int e^{(r_{n-2}-r_{n-1})x}\ldots\int e^{-rx} X \;\sigma\!\!-\; \frac{d^n X}{r^{z+1} \cdot r_1^{z+1} \ldots r_{n-1}^{z+1}}$$

taking no notice of the algebraic sign. This may be regarded as the general term of the last number, and hence as the index of the form it assumes when all the integrations have been executed. But this term would manifestly have the same value and form in whatever order the roots $r$, $r_1$, $r_2$, ... be taken; and therefore the member itself would have the same value and form, and consequently the solution we have given comprehends all the varieties which can arise from the different arrangements that may be given to the roots, and is therefore the complete solution.

### *Failure of Lagrange's Method.*

Having established the accuracy and completeness of our own method, we will now transfer our attention to that proposed

posed by Lagrange, which has been received and relied on as correct by the scientific world for nearly half a century. That we may treat this method with perfect justice and fairness, we propose to work out as directed the identical example with which it is in all the works of the present day illustrated, and then show its absurdity by direct differentiation. The equation usually taken is one of the third order,

$$d^3y + Ad^2y + Bdy + Cy = X. \qquad (20)$$

Suppose $e^{rx}$, $e^{r_1x}$, $e^{r_2x}$, or $y_1$, $y_2$, $y_3$ be the three partial solutions when $X = 0$; then if we take

$$y = pe^{rx} + p_1e^{r_1x} + p_2e^{r_2x} \text{ or } = py_1 + p_1y_2 + p_2y_3$$

$p$, $p_1$, $p_2$ being instead of arbitrary constants arbitrary functions of $x$, these arbitrary functions may, it is conceived, be subjected to certain restrictions, and yet satisfy the conditions of (20). For instance in the value of $dy$ in

$$dy = p\,dy_1 + p_1\,dy_2 + p_2\,dy_3 + y_1\,dp + y_2\,dp_1 + y_3\,dp_2$$

the first three terms are the same as would result if $p$, $p_1$, $p_2$, were constant coefficients; and Lagrange therefore assumes that he may put

$$y_1\,dp + y_2\,dp_1 + y_3\,dp_2 = 0$$

as one condition for determining the functions $p$, $p_1$, $p_2$, which leaves

$$dy = p\,dy_1 + p_1\,dy_2 + p_2\,dy_3$$

This being a second time differentiated becomes

$$d^2y = pd^2y_1 + p_1d^2y_2 + p_2d^2y_3 + dy_1dp + dy_2dp_1 + dy_3\,dp_2$$

which for similar reasons Lagrange again decomposes into

$$d^2y = p\,d^2y_1 + p_1d^2y_2 + p_2d^2y_3,$$

and

$$dy_1dp + dy_2dp_1 + dy_3dp_2 = 0$$

Differentiating once more we have

$$d^3y = pd^3y_1 + p_1d^3y_2 + p_2d^3y_3 + d^2y_1\,dp + d^2y_2dp_1 + d^2y_3dp_2$$

Substituting the several values of $y$, $dy$, $d^2y$, $d^3y$ for these quantities in the proposed equation there results

$$\begin{aligned} &p\,\{d^3y_1 + Ad^2y_1 + Bdy_1 + Cy_1\} \\ &+ p_1\{d^3y_2 + Ad^2y_2 + Bdy_2 + Cy_2\} \\ &+ p_2\{d^3y_3 + Ad^2y_3 + Bdy_3 + Cy_3\} \\ &+ d^2y_1dp + d^2y_2dp_1 + d^2y_3dp_2 = X \end{aligned}$$

But because the parts multiplied by $p$, $p_1$, $p_2$ are assumed to to satisfy each, the equation proposed deprived of its last term X they separately vanish, and leave

$$d^2y_1dp + d^2y_2dp_1 + d^2y_3dp_2 = X$$ which, together with

$$dy_1dp + dy_2dp_1 + dy_3dp_2 = 0$$

and $$y_1dp + y_2dp_1 + y_3dp_2 = 0$$

furnishes three several equations from which two of the quantities $dp$, $dp_1$, $dp_2$ may be eliminated, and the value of the third $p$, $p_1$, $p_2$ be obtained by a simple integration. And this performed with the others will afford us the values of $p$, $p_1$, $p_2$; and of course, according to Lagrange, the complete solution of the differential equation.

In order to investigate this with every possible regard to generality, let as before $r$, $r_1$, $r_2$ be the roots of the equivalent algebraic equation

$$r^3 + Ar^2 + Br + C = 0;$$

and let $$y_1 = e^{rx},\ y_2 = e^{r_1x},\ y_3 = e^{r_2x}$$

which give $$dy_1 = re^{rx},\ dy_2 = r_1e^{r_1x},\ dy_3 = r_2e^{r_2x}$$

and $$d^2y_1 = r^2e^{rx},\ d^2y_2 = r_1^2e^{r_1x},\ d^2y_3 = r_2^2e^{r_2x}$$

Hence the three equations give

$$r^2e^{rx}dp + r_1^2e^{r_1x}dp_1 + r_2^2e^{r_2x}dp_2 = X$$

$$re^{rx}dp + r_1e^{r_1x}dp_1 + r_2e^{r_2x}dp_2 = 0$$

$$e^{rx}dp + e^{r_1x}dp_1 + e^{r_2x}dp_2 = 0$$

from which eliminating $dp_2$ we obtain

$$(r^2-rr_2)e^{rx}dp + (r_1^2-r_1r_2)e^{r_1x}dp_1 = X$$

$$(r-r_2)e^{rx}dp + (r_1-r_2)e^{r_1x}dp_1 = 0$$

and then eliminating $dp_1$ from these

$$(r^2-rr_2-rr_1+r_1r_2)e^{rx}dp = X$$

or $$p = \frac{c+\int Xe^{-rx}}{r^2-rr_2-rr_1+r_1r_2}.$$

In the same way the other coefficients $p_1$, $p_2$ are found; but as this one will answer our present purpose, we shall not trouble ourselves to determine them. From this coefficient we obtain,

$$py_1 = ace^{rx} + ae^{rx}\int Xe^{-rx}$$

putting $a$ for the numeral reciprocal. This is one value that must satisfy the proposed equation. Now in this value $r$ being

being a root of the equivalent algebraic equation, the part $ace^{rx}$ must vanish when the successive differentials of the value are taken and respectively multiplied by A, B, C to answer the conditions of the given differential.

Therefore the part $ae^{rx}\int Xe^{-rx}$ must also satisfy the differential, that is

$$Cae^{rx}\int Xe^{-rx} = Cae^{rx}\int Xe^{-rx}$$

$$Bd.ae^{rx}\int Xe^{-rx} = Bare^{rx}\int Xe^{-rx} + BaX$$

$$Ad^2.ae^{rx}\int Xe^{-rx} = Aar^2e^{rx}\int Xe^{-rx} + AarX + AadX$$

$$d^3.ae^{rx}\int Xe^{-rx} = ar^3e^{rx}\int Xe^{-rx} + ar^2X + ardX + ad^2X$$

or $\quad (r^2 + Ar + B)\,aX + (r + A)\,adX + ad^2X = X$

the other four terms vanishing in consequence of $r$ being a root. Putting for A its value $= -r - r_1 - r_2$, and for B its value $= rr_1 + rr_2 + r_1r_2$ according to the theory of algebraic equations, and restoring the value of $a$, the above equation becomes

$$r_1r_2X - (r_1 + r_2)\,dX + d^2X = (r^2 - rr_2 - rr_1 + r_1r_2)\,X$$

or $\quad d^2X - (r_1 + r_2)\,dX - (r^2 - rr_2 - rr_1)\,X = 0$

a linear equation of the second order with respect to X. This equation therefore limits X to a particular form (namely $e^{bx}$ $b$ being determined from $b^2 - (r_1 + r_2)\,b = r^2 - rr_2 - rr_1$), whereas it ought to remain indefinite. Consequently the solution which requires this limitation cannot be a solution to the equation proposed having X unlimited.

In looking round for the cause of Lagrange's failure we perceive that it must arise from the limitations which his equations of condition introduce. He assumes that the functions $p$, $p_1$, $p_2$ are arbitrary, and hence thinks the limitations he has given them will not affect the accuracy of the solution. If, however, we consider that $e^{rx}$ is not a solution of the equation proposed, but of this equation deprived of its term X, it is obvious that the functional factor $p$ has certain conditions to fulfil, namely, to render the solution $e^{rx}$ of

$$d^3y + Ad^2y + Bdy + Cy = 0$$

a solution of the equation

$$d^3y + Ad^2y + Bdy + Cy = X$$

and if at the same time we consider that the difference between the solutions of the former and latter equation depends entirely

entirely on the influence of the quantity X, it is clear that $p$ which transforms one solution into the other must also depend on X, and for this reason cannot be arbitrary. Consequently the very first assumption of Lagrange is erroneous. and the arguments which he founds on this assumption productive of error. It is most extraordinary that mathematicians have for upwards, I believe, of half a century suffered this to pass unobserved and uncorrected; and, as if to crown their conduct with absurdity, have actually praised Lagrange's method as the most perfect and general that could be given.

*Verification of our own Solution.*

The simple manner in which we have, step by step, deduced our general formula of solution cannot, we presume, leave a doubt as to its truth. But since we have shown the failure of Lagrange's method by differentiating one of his examples, it is but fair that we should apply the same test to a similar example given by our own method.

It is obvious by (12) and (16) that

$$d\mathrm{X}_t = \mathrm{X}_{t-1} e^{-x\mathrm{R}_{t-2}} = \mathrm{X}_{t-1} e^{x(r_{n-2} - r_{n-1})}$$

and in the equation just above (17), that

$$y = e^{xr_{n-1}} \mathrm{X}_n$$

Therefore putting $n = 3$ we have

$$dy = r_2 e^{r_2 x} \mathrm{X}_3 + e^{r_1 x} \mathrm{X}_2$$

$$d^2y = r_2 e^{r_2 x} \mathrm{X}_3 + (r_2 + r_1) e^{r_1 x} \mathrm{X}_2 + e^{rx} \mathrm{X}$$

$$d^3y = r_2^3 e^{r_2 x} \mathrm{X}_3 + (r_2^2 + r_2 r_1 + r_1^2) e^{r_1 x} \mathrm{X}_2 + (r_2 + r_1 + r) e^{rx} \mathrm{X}_1 + \mathrm{X}$$

If we multiply these values of $d^2y$, $dy$, $y$ by $\mathrm{A}(= -r_2 - r_1 - r)$, $\mathrm{B}(= r_2 r_1 + r_2 r + r_1 r)$, $\mathrm{C}(= r_2 r_1 r)$ respectively, it is clear, $r_2$ being a root of the equivalent algebraic equation, that the sum of the left-hand column will $= 0$. It is also obvious from the value of A, that the two terms which constitute the third column $= 0$. But the numeral part of the second column, putting for A and B their values, is plainly

$$r_2^2 + r_2 r_1 + r_1^2 - (r_2 + r_1) \cdot (r_2 + r_1 + r) + r_2 r_1 + r_2 r + r_1 r = 0$$

So that all the terms vanish except X, and leave X = X as it should be.

We might here develop some other properties of our solution, both as it regards integration and even the doctrine of algebraic equations; and we might show how to extend a similar method to linear equations having variable coefficients; but these things we think it better to reserve for another opportunity, more especially as we contemplate investigating the latter subject on much more general principles than we have employed in that of which we have been treating.

JOHN HERAPATH.

Errata in the preceding part of this paper, printed in the last Number of the Philosophical Magazine and Annals.

Page 424, last line, *for* $R^{t}{}_{-2}$ *read* $R_{t-2}$

425, in (16) for $r + R_1 + R_2$ read $r + R + R_1 + R_2$

425, in (18) for $e^{xrn-1}$ read $e^{xr}n-1$

## V. *On the Occultation of* β Scorpii, *September* 25, 1827. *By* E. Riddle, *Esq.*

*To the Editors of the Philosophical Magazine and Annals.*

Gentlemen,

THE double star *β Scorpii* has been occulted almost every lunation during the present year; but only the occultations in May, July, and September, occurred at such times as admitted of their being seen in this country. I happened to see that which took place on September 25th, at about six o'clock in the evening; and as it was not seen by any other person that I have met with, and was attended by some circumstances which I believe were unexpected, it seems desireable to ascertain, if possible, whether it was witnessed by any one else in this neighbourhood, and to what extent the phænomena differed from those which I observed.

Perhaps the insertion of this notice in the Philosophical Magazine may elicit a communication on the subject from some gentleman who observed it with means superior to mine, as I had merely a 30-inch achromatic, with a power of about 80.

The stars are about 14″ apart, the smaller one the more northerly; and though the elements of the occultations of both stars are given in the Nautical Almanac, and both marked there as visible at Greenwich, where I observed, which was in lat. 51° 28′ 53″ N., long. 8″ W., and about 120 feet below the level of the axis of the transit instrument, at the Royal Observatory, *the larger star was not occulted at all.* When I first turned the telescope on the moon, I saw both the stars very distinctly, a little to the eastward of the southern enlightened horn. The smaller one vanished behind the dark limb, a little before it reached the horn. I expected every instant as the larger one approached the enlightened edge, that it would be occulted by some of the luminous protuberances near the horn; but it glided steadily along, appearing in my telescope, nearly if not quite in contact with the enlightened limb. *It never was*

*was invisible;* I never lost sight of it, till I saw both it and the smaller one quite clear of the moon.

But the larger one was so nearly occulted here, that it must have been quite so at a very small distance to the north-eastward; and it would be curious, and not quite useless as connected with the question of lunar parallax, if it should be found that the occultation was observed where the star's apparent path was an accurate tangent to the moon's limb.

I am, Gentlemen, yours, &c.

Greenwich Hospital, Dec. 10, 1827. E. Riddle.

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## VI. *On the Crystalline Form of some Salts. By Mr.* E. F. Teschemacher.

*To the Editors of the Philosophical Magazine and Annals.*

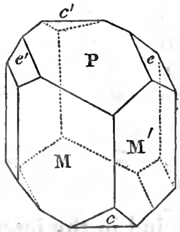
Gentlemen,

OBSERVING a paper in your Journal of December last, on some new double chromates, I beg to remark, that during an investigation which I made last winter, of some of the combinations of chromic acid with the different metals, I met with the same salts; and by an examination of their crystallographical character arrived at the same results as Mr. Henry Stokes, with regard to the combinations of sulphate of nickel and sulphate of zinc with chromate of potash, which, as they have not been before described, it may be interesting to detail.

Struck with the similarity in form of the crystals deposited by the solution containing nickel with some crystals of sulphate of nickel and potash given to me by Mr. H. J. Brooke, and described by him in the Annals of Philosophy, New Series, for December 1823; I subjected some very brilliant crystals of this new salt to an examination by Dr. Wollaston's reflective goniometer, which gave the following measurements.—Primitive form an oblique rhombic prism.

Chromo-sulphate of Nickel and Potash.

| | | |
|---|---|---|
| P on M or M′ | 101° | 37′ |
| P on *e* . . . | 154 | 30 |
| M on M′ . . | 107 | 37 |




Sulphate of Nickel and Potash.

| | | |
|---|---|---|
| P on M or M′ | 102° | 15′ |
| P on *e* or *e*′. | 154 | 32 |
| M on M′ . . | 109 | 10 |

A small difference in the meeting of the angles of these two salts

 will

will be observed. From this similarity of form I concluded this new salt to be a sulphate of nickel and potash combined with a small quantity of chromic acid, which is now further confirmed by comparing the analysis of the former salt made by Mr. Cooper, and inserted also in the same paper of Mr. H. J. Brooke, with that of the new salt made by Mr. Stokes.

| Sulphate of Nickel and Potash. | | Chromo-sulphate of Nickel and Potash. | |
|---|---|---|---|
| Sulphuric acid . . | 18·95 | Sulphuric acid . . | 18·260 |
| Oxide of nickel . | 8·77 | Oxide of nickel . | 8·200 |
| Potash . . . . . . | 10·24 | Potash . . . . . . | 9·862 |
| Water . . . . . . | 12·04 | Water . . . . . . | 12·700 |
| | | Chromic acid . . | 0·978 |
| | 50· | | 50· |

Primitive form of both, an oblique rhombic prism.

| Chromo-sulphate of Zinc and Potash. | | Sulphate of Zinc and Potash. | |
|---|---|---|---|
| P on M . . . . | 101° 47′ | P on M . . . | 102° 20′ |
| P on *e* . . . . . | 154 31 | M on M′ . . | 108 40 |
| M on M′ . . . . | 108 45 | | |

This latter salt, the sulphate of zinc and potash, I formed by adding pure potash to a solution of sulphate of zinc, separating the precipitate, and allowing the clear solution still containing oxide of zinc to crystallize. Not having made the analysis of this latter salt, I am unable to compare it with the analysis of the chromo-sulphate of potash and zinc as given by Mr. Stokes.

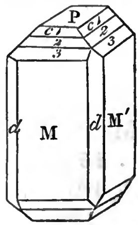
These are further instances in which the combination of minute quantities of matter vary the measure of the angles of crystals, and which show the value of Dr. Wollaston's goniometer in immediately detecting their existence.

I take this opportunity of sending you the measurements of the crystals of two other substances, which I am not aware have been previously given.

Hematin.

| | |
|---|---|
| P on M . . . . | 90° |
| M on M′ . . . | 90 |
| M on *d* . . . . | 135 |
| M on $c^1$ . . . . | 122 10′ |
| $c^2$ . . . . | 118 15 |
| $c^3$ . . . . | 116 15 |

Primitive form a square prism.

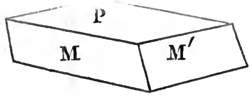



This substance was found in the interior of a piece of logwood ready formed in a crystalline mass; I merely dissolved the

the crystalline part in spirit of wine and obtained the hematin in regular crystals.

Tartrate of Strontian. Primitive form an oblique rhombic prism.

P on M . . . . 92° 35′
M on M′ . . . . 125 20




This salt was formed by adding a solution of tartrate of potash to one of nitrate of strontian.

On platina wire with borax before the blowpipe the chromo-sulphate of nickel and potash gives a transparent light brown glass; the chromo-sulphate of zinc and potash a transparent yellowish-green one. Yours, &c.

Barnsbury Park, Dec. 10, 1827. E. F. Teschemacher.

## VII. *Remarks on* Mr. J. Taylor's *Rain-gauge. By* B. Bevan, *Esq.*

*To the Editors of the Philosophical Magazine and Annals.*

Gentlemen,

IN the last Number of your Magazine, at page 406, is a description of a rain-gauge by Mr. J. Taylor, in which a reference is made to my self-registering gauges, as described in a former Number*.

From the tenor of the paper, it may be supposed that the gauge invented by Mr. Taylor was an improvement upon mine. Now setting aside as far as may be, a partiality for our own inventions, I am not able to see in what respect it can be denominated a *registering* instrument: it may serve, and doubtless will, to point out the total quantity collected in the intervals of inspection, and so will the old simple floating index; but neither of them will show the *time* of *commencement*, the *duration*, and the *rate* of raining, for each shower,—which was effected by my gauges for some years: and although it was necessary to wind up the clock, and replace the paper on the cylinder once a week, it could not be considered any very objectionable trouble, considering the information it afforded; the whole operation might possibly occupy about five minutes, and as this occurred but once a week, it was not quite equal to the proportion of one minute per day.

I am aware, that to a gauge, such as is described by Mr. Taylor, additional apparatus might be applied, so as to render it a proper registering instrument; but at present it does not seem to possess any great superiority to the common floating gauges.

* See Philosophical Magazine and Annals, vol. ii. p. 74.

The

The metals used by me in the construction of the receiver and conducting tubes, were, plate iron and tin, painted, to prevent oxidation; and they were found to last several years.

The above remarks are not intended to discourage improvements in such instruments, but to prevent an erroneous impression upon your readers.

I am, Gentlemen,

Your obedient servant,

Leighton, Dec. 10, 1827. B. Bevan.

[We think that Mr. Taylor's expressions can hardly be considered as intending to propose his instrument to supersede Mr. Bevan's, or to be an improvement upon it.

He describes his object as indeed a different one, which may suit others as well as himself, and which is simply to have a rain-gauge which will record quantities between periods of observation,—if the term registering be objected to,—and without the care or management of persons skilled in such matters.

Mr. Taylor seems to us to acknowledge the merits of Mr. Bevan's instrument, and does not appear to have thought of any rivalry between the two inventions.—Edit.]

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## VIII. *Reply to* F.R.S. L.'s *Remarks on Compound Interest.*

*To the Editors of the Philosophical Magazine and Annals.*

Gentlemen,

YOUR correspondent F. R. S. L. in his "Remarks on the Principle of Compound Interest," seems to have bewildered himself sadly in his attempt to defend Dr. Young's paper on that subject; nor is it in my power to extricate him out of the labyrinth. "It is," he says, "lawful to receive $\frac{5}{365}$*l.* each day that 100*l.* is in the hands of the borrower, but at the end of the year it is only lawful to receive 5*l.* for the use of the 100*l.* without any interest on the interest."—All this is true if the interest is payable *yearly;* but if it is made payable at shorter intervals, what law restrains the lender from improving that interest during each interval, like any other part of his capital? The borrower pays only at the rate of 5*l.* per cent; nor does the lender improve his money at a higher rate, the advantage in his favour arising only from the more frequent opportunity of adding the interest to his capital. From the nature of compound interest, it is evident that the value of an annuity payable at shorter intervals than one year must vary less and less from the value of the same annuity payable yearly,

yearly, as the term is lengthened; so that the values of the perpetuity in both cases must differ from each other by less than any assignable quantity, and therefore must be equal, agreeable to Dr. Price's theorems, and so far prove the correctness of his solution: but this plain and obvious truth appears to F.R.S.L. to be a *revolting paradox;* nor is it to be wondered at, considering his method of reasoning on the subject.

In attempting to prove that Dr. Price employs two different rates of interest in his method of computing, it is observed, that "the discount on ten shillings, receivable at a certain moment as a payment of the half yearly annuity, is greater in the computation than the discount on ten shillings receivable at the same moment as half an annual payment."

Now as the present value of ten shillings, or a moiety of 1*l.* to be received at the end of six months, and the present value of ten shillings to be received at the same time as the half-yearly payment of the annuity, are expressed by the same fraction $\frac{\cdot 5\cdot 35}{1\cdot 025}$, it would, I believe, puzzle any person, however well acquainted with the subject, to produce different results in this case.

At the conclusion of your Correspondent's remarks, Mr. Morgan's documents respecting the probabilities of human life are said to be "greatly calculated to encourage the popular delusion of the improved healthiness and greater longevity of the people of this kingdom." Had he indeed asserted, as Dr. Young states in his paper in the Philosophical Transactions, that only one in 1500 died in the year among the members of the Equitable Society, this accusation would have been just; but I should be glad to know in what part of his works has Mr. Morgan been so absurd? In the Nosological Table at the end of his Treatise on Annuities he represents the number of deaths in each year to be in the proportion of 1930 to 151,754, or as 1 to 78 nearly. By what rule in arithmetic these numbers are made to be in the proportion of 1 to 1500, I shall not stop to inquire.

F.R.S.

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## IX. *On the ultimate Composition of simple alimentary Substances; with some preliminary Remarks on the Analysis of organized Bodies in general.* By William Prout, *M.D. F.R.S.**

[With two Engravings.]

THE present being the first of several communications on the same subject which I hope to have the honour of lay-

* From the Philosophical Transactions, for 1827, Part ii.

ing

ing before the Royal Society, a few observations on the origin and object of the whole series may not be deemed irrelevant.

Many years ago I published an anonymous paper, containing some views, at that time new, connected with the doctrines of chemical proportions*. Though this paper, for reasons which need not be here stated, was drawn up and published in a very hasty and imperfect manner, it attracted some notice: and the views therein advanced gradually gained ground, and at present appear to be generally admitted in this country†. When this paper was published, it was my intention to have pursued the subject further, but I soon found my progress obstructed by insuperable difficulties. The first and chief of these was the want of accurate data; and the infinity of objects comprehended by chemistry prevented the hope of acquiring, by individual exertion, however unremitting, a sufficiency for the establishment of general laws. Professional duties still further limited my exertions, and at length obliged me to relinquish chemistry in general, and confine my attention solely to the chemistry of organized substances; a subject that has occupied the greater portion of my leisure hours for the last ten or twelve years.

Organic chemistry is confessedly one of the most difficult departments of the science; and though much has been done, and more attempted on the subject, it is yet in a very imperfect and unsatisfactory state; and it must be frankly admitted that physiology and pathology have derived less advantage from this most promising and really powerful of the auxiliary sciences, than might have been expected. To explain this perhaps would not be difficult; but as the explanation would be misplaced here, I shall merely observe, that, dissatisfied with the old modes of inquiry, I determined to attempt a different one, and keeping in view the notions I had originally formed respecting chemical combinations, proposed to myself to investigate the modes in which the three or four elementary substances entering into the composition of organized bodies are associated, so as to constitute the infinite variety occurring in nature.

With these views my first object was to determine the exact composition of the most simple and best defined organic compounds, such as sugar, and the vegetable acids; a point that had been several times before attempted, but, as it ap-

* Annals of Philosophy, vi. 321, and vii. 111. (O. S.) The object of the second Paper was simply to correct some oversights in the first.

† Dr. Thomson's Chemistry, and his attempt to establish the first principles of chemistry by experiment. Also, Dr. Henry and Mr. Brande's Elements of Chemistry, &c.

peared to me, without complete success. About the same time also albumen and other animal products, as urea, lithic acid, &c. were examined with similar views. The subject of digestion, however, had for a long time occupied my particular attention; and by degrees I had come to the conclusion, that the principal alimentary matters employed by man, and the more perfect animals, might be reduced to three great classes, namely, the *saccharine*, the *oily*, and the *albuminous:* hence, it was determined to investigate these in the first place, and their exact composition being ascertained, to inquire afterwards into the changes induced in them by the action of the stomach and other organs during the subsequent processes of assimilation. In conformity with this plan, the object of the present communication is the consideration of the first class or family above mentioned, namely, the *saccharine.*

*Preliminary observations on the analysis of organized substances.*

Vegetable substances contain at least two elements, hydrogen and carbon; and most generally three, hydrogen, carbon, and oxygen. Animal substances are still more complicated; and besides the above three, usually involve a fourth element, namely, azote, to which they appear to owe many of their peculiar properties. These general facts have been known ever since the elements themselves have been recognized as distinct principles, though the determination of the exact proportions in which they enter into any particular substance, has always proved a most difficult problem. To enumerate all that has been done on this subject would be loss of time; and it need only be mentioned, that all idea of separating the different elements from one another, so as to obtain them *per se*, has been long since abandoned, if indeed it was ever entertained; and the general principle on which the analysis of organic products has been usually conducted, has been to obtain their elements in the form of binary compounds, either by destructive distillation, as was formerly practised; or by combining the elements with some other element with which they possessed the property of forming definite binary compounds, from the quantity and known composition of which, those of the original elements might be readily obtained by calculation. For this latter purpose oxygen has been the principle usually employed, which, as is well known, forms water with hydrogen, and carbonic acid gas with carbon; two compounds not only as well understood as any in chemistry, but likewise, from their physical properties, well adapted for the purpose. When azote is involved, other means must be adopted, which will be fully considered hereafter.

The modes in which chemists have attempted to combine oxygen with the hydrogen and carbon of vegetable substances have differed very considerably. The illustrious Lavoisier attempted their union by burning the substance at once in oxygen gas, a method subsequently followed by Saussure and others. Afterwards the metallic oxides were employed for the purpose; and Berzelius in particular informs us, that so early as 1807 he had tried the oxide of lead, but did not succeed with it*. In 1811, Gay Lussac and Thenard published the analysis of different organic substances made by means of the chlorate of potash; and, considering the nature of the apparatus they employed, they obtained admirable approximate results†. Berzelius, in 1814, published an elaborate paper on the subject of vegetable analysis, in which he likewise employed the chlorate of potash, but in quite a different manner; and to this celebrated chemist I believe we are indebted for the improvement subsequently adopted by most of his successors, of introducing the mixture of the substance to be analysed, and the oxide, into a narrow tube, and submitting the different portions of it to heat in succession. The results of Berzelius were in general more accurate than those of his predecessors, especially as far as related to the quantity of carbon, but his method was not well adapted for determining the proportion of hydrogen‡. In 1816, Gay Lussac seems to have thought of employing the oxide of copper for the purposes of analysis§, the introduction of which undoubtedly constituted one of the greatest improvements hitherto made in organic analysis; and the use of which has continued to the present time, and will perhaps never be entirely superseded. The oxide of copper has however some disadvantages, which it is one object of the present remarks to point out; another is, to propose a form of apparatus free from most of the objections to which those hitherto in use have been more or less liable.

There are two methods of arriving at the quantity of water formed during the combustion of an organized substance; either actually to collect and weigh it, as Berzelius did, or to estimate the quantity by the loss of weight sustained by the tube after the combustion. The latter in general is the best method, and was that adopted by me from the first: it has since been followed by Dr. Ure, and others‖. Whichever method is adopted, it obviously becomes necessary that no

* Annals of Philosophy, iv. 403. † *Récherches Physico-chimiques,* ii. 265.
‡ Annals of Philosophy, iv. 323. § *Annales de Chimie,* xcvi. 306.
‖ Phil. Trans. 1822. p. 457.

extraneous

extraneous water be present; but all pulverulent substances, and oxide of copper among the rest, are more or less hygrometric, and rapidly attract moisture from the atmosphere. This circumstance seems to have struck the French chemists, and it occurred to me at a very early period. Dr. Ure, however, was the first who published a method of obviating this difficulty; and his method, if this were the only difficulty to contend with, is capable of considerable precision. But there is another, and perhaps still more troublesome property, possessed by the oxide of copper, in common with many other powders, namely, that of condensing air as well as water *; and these two difficulties, taken together in conjunction with another mentioned in a note below, render great precision almost out of the question †. To conquer these, every means

* See Saussure's paper on the absorption of the gases by different bodies. Annals of Philosophy, vi. 241. Also Gilbert's *Annalen der Physick*, xlvii. 112.

† As I am unwilling that so much labour should be lost, particularly as it may be of some use to other inquirers, I have thrown into the form of a note a few of the principal circumstances connected with the inquiry mentioned in the text. In my earlier experiments, tubes of iron, copper, &c. were employed instead of glass, and charcoal instead of spirits, as the medium of heat; and during this period most of the modifications of apparatus which have been since proposed as novelties or improvements, were tried and rejected. I first took the hint of employing a spirit lamp from Mr. Porrett, and was certainly among the first that did so employ it. Various forms of lamps were tried, but at length I was induced to relinquish the use of the horizontal apparatus for the vertical one[a]; and this, I have no hesitation in saying, is by far the best form of apparatus hitherto proposed for the substances to which it is adapted; nor would any other have been required by me, at least, had it not been for the properties of the oxide of copper alluded to in the text, which render this and all other forms of apparatus depending on the employment alone of that substance perfectly useless when great accuracy is required. It has been objected to the lamp, that the heat produced by it is not sufficient; but this is a mistake; at least I have never met with any substance that resisted its action, provided the oxide of copper was well shaken up in the tube, or, if necessary, taken out of the tube and retriturated, and afterwards exposed to heat a second time, one or other of which *ought to be done in all instances*, whatever be the medium of heat employed; for no ordinary heat will induce the oxide to part with its oxygen to a combustible substance at some distance off, and not immediately in contact with itself. A great heat is also attended with some disadvantages, and among others, that of causing the oxide to adhere together in hard and solid masses, which thus becomes more difficult to be removed from the tube, and much less adapted for future experiments. In general, organized bodies are more difficult of combustion, and require more heat than crystallized ones. The lamp described in the text I have only recently employed, and it answers the purpose in all respects better than any I have yet seen.

With respect to the sources of error above mentioned, it was found that

[a] Described in the Annals of Philosophy, xv. 190 (O. S.); and more completely in Dr. Henry's Chemistry, ii. 167, ninth edition.

that could be thought of, as likely to succeed, were tried, but without effect, and I was obliged to relinquish the matter in despair, and endeavour to contrive some other mode of analysis that should be free from these difficulties altogether. After a good deal of consideration I was induced to adopt a method which had occurred to me long before, but which I had never put in execution. This method is very simple, and founded on the following well known principles.

When an organic product containing three elements, hydrogen, carbon, and oxygen, is burnt in oxygen gas, one of three things must happen. 1. The original bulk of the oxygen gas *may remain the same*, in which case *the hydrogen and oxygen in the substance must exist in it in the same proportions in which they exist in water;* (for it is well known that oxygen gas by being converted into carbonic acid gas is not altered in its bulk): or, 2. The original bulk of the oxygen may be *increased*, in which case the oxygen must exist in the substance in a *greater* proportion than it exists in water; or 3. The original bulk of the oxygen gas may be *diminished*, in which case the *hydrogen must predominate.*

Hence it is obvious, that in the first of these cases the composition of a substance may be determined by simply ascertaining

200 grs. of the oxide of copper, recently ignited, gained, after ten or fifteen minutes, exposure to the air, a quantity varying from ·02 to ·05 gr. one half of which, or even more, was acquired before it became cold; that is to say, before it had cooled down to 100°, considerably above which point it began to acquire weight. Of the increase of weight above mentioned, it was found that about $\frac{1}{3}$, or $\frac{1}{4}$, (for the proportion varied from causes that I could not discover,) was due to the condensation of air, the rest was due to moisture. The oxide I employed was perfectly pure, and prepared by exposing metallic copper to heat. Dr. Ure states, "that 100 grs. of the oxide prepared from the nitrate of copper exposed to a red heat merely till the vapours of nitric acid were expelled, absorbed in the ordinary state of the atmosphere from ·1 to ·2 gr. in the space of an hour or two, and about half that quantity in a very few minutes."

In determining the quantity of water formed by the oxide of copper in the usual manner, there is yet another difficulty to contend with, to which we have alluded above, and which we shall here briefly notice. It has been stated that complete combustion seldom or never takes place during one exposure to heat, and that in many cases the oxide ought to be removed from the tube and retriturated. Now it was found, almost invariably, that during the second exposure to heat, the tube, instead of *losing* additional weight, actually became *heavier*, sometimes to the amount of ·03 gr. though often much less than this. I was a good deal puzzled to account for this anomaly at first, but believe that it arose chiefly from the reoxydation of the partially reduced oxide, by the air of the atmosphere.

Since this paper was read before the Royal Society, I have observed one or two other singular facts connected with this subject, which will be noticed when we come to speak of the analysis of substances containing azote.

taining

taining the quantity of carbonic acid gas yielded by a known quantity of it; while in the other two, the same can be readily ascertained by means of the same data, and by noting the excess or diminution of the original bulk of the oxygen gas employed. Such are the simple and universally admitted principles on which the following method of analysis is founded; the only novelty in which, therefore, is the form of the apparatus; and of this it is hoped the following summary sketch, and annexed figures, will convey every requisite information.

Fig. 1. Plate I. represents a front view or elevation of the whole apparatus in the act of being employed. A B is a platform, two feet square, surrounded by a ledge about 2½ inches high, for preserving any mercury that may chance to fall about, and furnished with four adjusting screws (of which two, C C, are sectional views), by means of which it may be placed perfectly horizontal. Into this platform, in the manner represented, are fixed perpendicularly four square pillars, D E, D E, about four feet and a half high, at the top of which is placed another small platform, F F, about four inches wide, and which may be fixed or removed at pleasure by means of the brass pins *a b*, *a b*. I I are glass tubes graduated with the utmost care to hundredths of a cubic inch, and which are cemented at bottom into semicircular iron tubes inclosed in the blocks K K (as represented by the dotted lines). These iron tubes project a little below the wood at the lower part, where they are furnished with iron stop-cocks, S S, for drawing off the mercury when it may be necessary. Into the other end of these semicircular tubes are likewise cemented the glass tubes L L (of smaller dimensions, and a little longer than the tubes I I), and forming with them, when taken together, inverted syphons. The smaller tubes, L L, are represented as surmounted by funnels, R R, furnished with stop-cocks, the object of which is to permit the mercury to flow into them with any velocity that may be required. On the tops of the larger tubes, I I, are cemented the vertical stop-cocks, M M, of which fig. 2. Plate II. is a sectional view on a larger scale, and which renders the construction so obvious, that perhaps no further remark is necessary, than merely stating that the cup, *a*, is filled with oil, and that the plug, *b*, which is square at the upper part, and adapted to a key, is furnished with a shoulder, on which the screw-cap, *c*, rests, and by means of which it may be tightened at pleasure*. On

* These syphons are fixed together independently of the general frame-work, and may be removed at pleasure by taking out the pins *c d*, *c d*, and *e f*, *e f*. This admits of their being replaced by others of different sizes. Those of a larger size have balls near the top, as represented

On the platform, F F, (fig. 1.) is a thin piece of wood, capable of being raised or depressed at pleasure, by means of the screws, O O; on this the lamp Q is placed, which may thus be placed at any distance that may be required from the tube, P. Fig. 3. is an enlarged view of this lamp: it consists of two reservoirs, *d e*, for holding the spirit, connected together by means of the tube, *f*, into which are placed, at the distance from one another of about $\frac{1}{3}$ of an inch, a number of vertical burners, *g g*, &c. about $\frac{1}{12}$ of an inch in diameter, and $\frac{3}{4}$ inch long, and *made as thin* as possible, with the view of preventing the conduction of the heat. These burners are each furnished with a few threads of cotton, and are bent a little alternately like the teeth of a saw, in order that their flame may envelope the tube, P, (fig. 1.) more completely. *h* is a cover for the wick of the lamp when not in use. The tube, P, (fig. 1.) is of green or bottle glass, moderately stout, and about $\frac{1}{5}$ of an inch internal diameter. It is fixed between the horizontal parts of the vertical cocks, M M, so as to be perfectly air-tight; and when required, the whole, or any part of it, may be heated by means of the lamp, Q, at the pleasure of the operator.

When the apparatus is to be employed, both the syphon gasometers, I L, I L, are to be filled with quicksilver, and a small green glass retort, containing the requisite quantity of chlorate of potash, (and which had been previously heated so as to completely expel the common air, and to fill it with oxygen gas,) is to be attached to one of the cocks, as represented in fig. 4, by means of a caoutchouc tube. Heat is then to be applied, and any quantity of oxygen gas that may be required, introduced into the tube, I. After the whole has acquired the temperature of the atmosphere, the exact quantity of the gas is to be accurately noticed, as well as the state of the barometer and thermometer at the same time. The tube, P, containing the substance to be analysed, is then to be firmly fixed between the cocks, M M,* and subjected to heat, during which the oxygen gas is to be transferred from one syphon to another, through the red hot tube, with any velocity that may be required, and which may be regulated by means of one of the

sented by the dotted lines, and may contain as much as 20 cubic inches of gas. It much facilitates the process of determining the exact quantity of gas contained in the apparatus, to have both legs of the syphon graduated, which may be easily done so as to obviate the effects of capillary attraction when the tubes are not both of the same calibre.

* I have tried various modes of connecting the tube so as to insure its being air-tight. Caoutchouc answers very well; but the best substance I have hitherto employed are slips of thin moistened hogs' bladder, tied on very tightly with fine *dry* twine. The twine is then to be moistened also, and the whole kept in this state till the end of the experiment.

the stop-cocks of the funnels, R R, and the stop-cock, S, of the opposite syphon.

Such is a general view of the apparatus, and the principles of its operation: but perhaps a few practical remarks on some of the circumstances to be attended to during its employment, may not be deemed superfluous.

The substance to be analysed may be placed in a small tray made of platina foil, and introduced alone into the tube P, and gradually submitted to the action of heat and oxygen gas; but this does not answer well with organic compounds, as a portion of them is apt to escape combustion. Another method is to mix the substance with pure siliceous sand, and to retain the mixture in the centre of the tube by means of asbestos. But this method often fails, except there be about an inch of the oxide of copper at each end of the tube, which must be kept red hot during the experiment, and in this case it succeeds completely with many substances. Another method, and that which the most generally succeeds, is to mix the substance with peroxide of copper, to heat these together in the tube in the first place, and afterwards to open the other stop-cock and send the oxygen gas through the ignited and partially reduced oxide, by means of which it again becomes peroxidized; and any portion of the substance that had escaped complete combustion in the first part of the experiment, is now completely burnt. This last method is also that employed when it is required to determine the quantity of carbonic acid gas yielded by a given quantity of any substance; only in this case, of course, oxygen gas is not required, and the contents of the tube P, must be taken out and well triturated, and subjected to heat a second time. If it should be required to analyse the gas formed, one method of removing it from the tube I, is represented at fig. 5; and others will readily occur to the practical chemist.

The following are some of the advantages of this apparatus, and mode of analysing organic compounds. In the first and chief place, *there is nothing to be apprehended from moisture.* Whether the substance to be analysed be naturally a hydrate, or in whatever state it may be with respect to water, the results will not be affected; and the great problem, whether the hydrogen and oxygen exist in the substance in the proportions in which they form water, or whether the hydrogen or oxygen predominates, will be equally satisfactorily solved, and that (of course within certain limits), independently of the weight of the substance operated on*. When however it is the object

* It is to be observed, that, throughout the experiments, great care is taken

ject to ascertain the quantities of carbonic acid gas and water yielded by a substance, it is, of course, necessary to operate on a known weight; but this being once determined, there is no fear, as in the common methods, of exposing the substance to the atmosphere as long as may be necessary. The hygrometric properties of the oxide of copper, as well as its property of condensing air, are also completely neutralized; for the whole, at the end of the experiment, being left precisely in the same state as it was at the commencement, the same condensation of course must take place, and any little differences that may exist are rendered quite unimportant from the bulk of oxygen gas operated on, which of course should, in all instances, be considerably greater than that of the carbonic acid gas formed. Another advantage of this method is, that more perfect combustion is insured by it than by any other that I am acquainted with. There is also no trouble of collecting or estimating the quantity of water,—a part of the common process attended with much trouble, and liable to innumerable accidental errors besides those already mentioned, and which there is no method of obviating or appreciating: here, on the contrary, the results are obtained in an obvious and permanent form, and, from the ease with which they are thus verified, comparatively very little subject to error.

It need scarcely be stated, that the form and principles of this apparatus render it well adapted for many other chemical operations besides the analysis of organized substances. Such, for example, as the reduction of oxides by hydrogen, and a variety of others that will readily occur to the practical chemist.

[To be continued.]

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## X. *Notices respecting New Books.*

*Account of a New Work, entitled, A Treatise on the Steam-Engine; Historical, Practical, and Descriptive. By* John Farey, *Engineer. London. Published by* Longman, Rees, and Co. in 4to. Price Five Guineas.

CONSIDERING the great extension which has been given to all branches of our national manufactures, by the application of steam power to expedite laborious operations, it is surprising that no competent engineer has hitherto undertaken to publish on this

taken that the gases are *saturated with moisture:* the errors from this cause are thus rendered definite, and are easily corrected by tables calculated for the purpose from the most accurate data, and which will be given in a subsequent communication.

subject,

subject, in a detailed manner, to be instructive to men of business and practitioners. It is evident, from the repeated instances of dreadful explosions, failures, and misapplications of capital, which have happened (even recently), that engineers as a body are not yet sufficiently informed on this branch of their art. Whilst on the other hand, the continued success of the best engineers proves that real skill does exist amongst them, and may be expected to be attainable by others, with study and proper instruction.

As a means of such instruction, the present work cannot fail to produce public good.

The importance of a scientific guide to students and young practitioners cannot require to be pointed out. Merchants, manufac turers, and men of business, who must call in the aid of engineers to construct engines and apply steam power for them, may easily gather as much knowledge from such a work as will enable them to distinguish between those who are competent to execute what is required, and others who will be likely to lead them into similar errors to those by which so many have suffered. And it will be a most important aid to practical engineers, in directing those studies by which they may render themselves competent to execute what they undertake.

At present, many who are well acquainted with the mechanical structure of steam-engines, and are even accustomed to make them in established forms which they have copied from others, have only very imperfect and even incorrect notions of their operations when in use; they are in the condition in which a physician would be who had only studied the anatomical structure of dead animals, without much knowledge of the physiology and pathology of their living functions. Such engineers often succeed very well in their ordinary business, and acquire a reputation which their knowledge will scarcely bear out, when they are required to depart from their established models, and make such modifications of their parts as is necessary for applying steam power to new purposes.

To obtain these useful results from a publication, it must be composed with great care by a practitioner of competent knowledge and experience; for a self-sufficient guide in an unknown road would be worse than no guide at all: and but little could be expected from literary men, who can have no other acquaintance with the subject than what they may acquire in collecting information from others, merely for the purposes of publication.

Mere descriptions of steam-engines will not prove of any very great utility, even if accurate, and however they may be elucidated by drawings of such extent as can be added to any publication: for to general readers such descriptions will prove tediously minute, and to practical men they will afford no information which is not already habitually imprinted in their memory. To form an instructive guide, the descriptive part must be accompanied by accurate statements of the actual and relative magnitudes of all the parts, and of the quantities of matter and of action that each part is required to possess, together with such a complete development of the principles on which their operations depend, as can only be obtained from a very long and intimate acquaintance with the subject, and a natural habit of observation exercised in an extensive field.

Mr. Farey has been long known as one of those who possess the knowledge required for such an undertaking; and the great length of time which has elapsed since this work was first promised to the public, must have given ample time for its execution in a careful manner.

We have not had time to give this volume the examination that the subject demands, but the objects and general tenor of it may be gathered from the following extracts from the Preface.

"The object of the present treatise is to give a historical, practical, and descriptive account of the steam-engine, and its application to useful purposes.

"The historical part is intended to form a complete account of the invention, from its first origin, to its present state of perfection, from which the statesman and political economist may observe the influence that the adoption of steam power has exercised upon our national prosperity and advancement, during a century past, and may form an opinion of the future advantages to be expected from more extended applications of the same principles.

"The practical and descriptive part is intended to form a course of instruction for professional students, in the practice and principles of making and using steam-engines. At present, such students are left to form and digest their own crude and imperfect observations; and for want of a scientific guide, their conclusions are liable to be tinctured with many erroneous notions and false assumptions. The same part is also intended to form a manual, which, by the aid of tables and theorems for calculation, will facilitate the practice of experienced professional engineers; and will tend to perfect the practice of those engineers and others, who require to employ steam-engines, and apply them to various purposes; but who do not possess a complete knowledge of their construction, and therefore require information, which it has hitherto been difficult to attain. The principles of mechanical and chemical action, upon which the ooeration of steam-engines depends, and the application of those principles to practice, will be also embodied in this part.

"Another part will contain a record and brief explanation of all the speculative projects which have been proposed for the improvement of steam-engines; so as to exhibit to mechanical inventors the various ideas which have been suggested for that object: the instruction that they may obtain from other parts of the work, together with the history of the circumstances from which the great points of invention have originated, may enable them to make some further improvements.

"To attain these objects is not an easy task; for it is only in the course of an active professional practice, that sufficient information is to be obtained; and all competent engineers are too much engaged to find leisure for a literary occupation. The author could not have undertaken such a work, if he had not formed the plan, and collected materials for its execution, at his first entrance into business; and as as it became known that he had such an object in view, he has continually received contributions of information from other engineers, who wished to promote the undertaking; and, with very few exceptions, this feeling has been general in the profession. In this way the author became personally acquainted with two great authorities in this branch of mechanics, the late Mr. Watt and Mr. Woolf, and received from them a full knowledge of the origin and progress of their respective inventions, and of the principles which they followed in applying those inventions to useful practice.

"At the commencement of his professional studies in the years 1805 and 1806, the author felt the want of a guide of this kind; and after carefully studying Dr. Robison's article in the *Encyclopædia Britannica*, and M. Prony's *Architecture Hydraulique*, and finding them insufficient for that purpose, he determined to preserve notes of all the observations and investigations, by which he should acquire his own knowledge of the construction and principles of operation in steam-engines and other machines, and their various applications; in the expectation that at some future period a useful publication might be formed from those materials. Professional avocations have long since occupied all the time which might have been devoted to such an object, but it has never been abandoned; and if the sale of the present work should prove sufficient to induce the publishers to undertake others of a similar nature, the author has an accumulation of notes, which in the course of years he may find time to arrange in a corresponding form with the present work.

"In the year 1815, the author drew up a descriptive article on the steam-engine for Dr. Rees' *Cyclopædia*; but the plan of that work, and the limited number of engravings, rendered it necessary to avoid details, which must constitute the great value of a practical treatise. Since the publication of that article in 1816, the want of a correct manual has been still more felt, from the great and increasing extension of the use of steam power, and the author was advised by his friends in the profession to resume his original project; this he undertook to do in 1820, and the bulk of the historical part was written, and most of the plates were engraved by the late Mr. Lowry, in the next year; but the author being obliged to reside at a distance from London, by engagements which left no leisure for this object, the impression has been carried on at intervals, and the publication of the first volume has been unavoidably protracted until the present time.

"To instruct students, it is necessary to state such elementary propositions in mechanics, as have a direct application to the subject of steam-engines; for this purpose a series of definitions are given in an Introduction to the present volume. These definitions have been formed from a full examination of the works of the best writers on the theory of mechanics, viz., Belidor, Emerson, Smeaton, Hutton, Banks, Gregory, Robison, Young, and others. The author has endeavoured to preserve their modes of reasoning, and the mathematical accuracy of their conclusions, without employing the language of geometrical or algebraical analysis; but all quantities are represented in numbers, and their proportions established by the ordinary processes of arithmetic: this plan has been adopted, in order to render the principles very apparent to those who are not accustomed to any other mode of calculus. This part of the work is intended to give practical men an exact knowledge of the true principles upon which their operations ought to be conducted; and other parts, to show the means of applying those principles to their daily practice, in the construction and use of steam-engines.

"To readers who are conversant with mathematical investigations, the mode of stating the propositions will appear to leave them without sufficient demonstration; but the principles which it is intended to explain and define (rather than to demonstrate) have been so well established in mathematical evidence, as to leave no doubt of their truth; and such readers may be referred to the mathematical writers whose principles have been adopted.

"Dr. Gregory's Treatise of Mechanics, in 2 volumes, 8vo., contains the best collection of mathematical investigations; and the fullest and clearest exposition of the principles will be found in Dr. Young's Course of Lectures on Natural Philosophy, in 2 volumes, 4to.; also Dr. Robison's articles on Mechanical Philosophy, in the *Encyclopædia Britannica*, reprinted by Dr. Brewster, in 4 volumes, 8vo. To the young student, a previous mathematical course is strongly recommended: the best guides are, Martin's System of Mathematical Institutions, in 2 volumes, 8vo.; Dr. Hutton's Course, in 3 volumes, 8vo.; and Dr. Gregory's Mathematics for Practical Men.

"One great object of the present work is to furnish practical engineers with a series of rules for calculating all proportions and quantities, which can be required to be known for the construction and use of steam-engines: these rules have been deduced from very numerous observations made upon steam-engines and mills of all kinds and of all magnitudes. In each case the observations have been very carefully compared, and assorted in series, according to the similarity of circumstances, and then such formulæ deduced from them, as would give results corresponding equally well with all parts of the series. The construction of these various formulæ has been a work of great labour, of which very little appears, because only the results of the investigations are retained in the form of an arithmetical rule. The greater part of these rules have been formed by the author for his own use, in professional practice; and having undergone the test of continual application during a course of several years, and received frequent corrections, he is justified in claiming some confidence iu their accuracy.

"The principles which regulate the proportions of the different quantities which are to be computed by each rule, are stated in the most concise terms which could be chosen, without using algebraical substitutions; these have been avoided throughout the work, because the methods of algebra and fluxions are only necessary to investigate the formulæ, whereby computations may be performed in numbers, by by the processes of ordinary arithmetic; and it is sufficient for practical use, to have rules which will give the required results.

"The method of performing each calculation by the sliding-rule is added, and will tend to facilitate the computations. This valuable instrument was introduced into considerable use amongst engineers by Mr. Watt, and only requires a good collection of formulæ to become of universal application. The author hopes that what he has done will contribute to extend the use of that excellent mode of computation amongst the profession.

"The history of the invention of the steam-engine and its application, is divided into chapters.—The first of which contains an account of the various projects and attempts which were made, during the seventeenth century, to obtain a moving power from fire; and a description of the first working engine which was invented by Mr. Savery for raising water, but it never came into extensive use.

"The second chapter is on the invention, principle, and construction of the fire-engine of Newcomen, which was the first engine brought into important use, and it is still very extensively employed. This subject is treated at length, and rules are laid down for the proportions of its parts.

"The third chapter is on the various applications of Newcomen's invention, which were made during the first half-century after its invention.

"The fourth chapter is on the introduction of cast-iron into the construction of machinery, and the application of the fire-engine to the manufacture of iron.

"The fifth chapter is a history of the origin and progress of Mr. Watt's invention of his first steam-engine for pumping water; with an account of that engine, and of the rules which he established for the proportions of its parts.

"The sixth chapter gives an account of the first application of the steam-engine to give continuous circular motion to mills; with a complete description of the principle, operation, and structure of Mr. Watt's rotative engine for that purpose; and the dimensions of several standard engines made by himself, which have been in use for years, and which perform as well as any modern engines which depend upon the same application of steam.

"The seventh chapter is a treatise on the construction and use of the sliding-rule, and its application to the purposes of calculations relative to steam-engines.

"The eighth chapter is a collection of rules for calculating the proportions and dimensions for all parts of Mr. Watt's rotative steam-engines.

"The ninth chapter is in continuation of the history of the invention of the steam-engine, and describes those modifications in the form of Mr. Watt's engine which were proposed and executed by his cotemporaries.

"The present volume concludes at that part of the history of the invention of the steam-engine, when it had been brought to such a degree

gree of perfection that all its principles of action were fully developed and realized in practice; and, although the art of constructing these machines has been very greatly improved since Mr. Watt's time, and their applications widely extended, no important inventions have been established in practice, except high-pressure engines, and particularly those of Mr. Woolf, which are still used in the same form as he first made them.—The remainder of the subject will consist of technical descriptions of the structure of such steam-engines as are now in use, and as they are made by the best engineers; this, with their applications to various purposes, and the principles which should be followed in making such applications, will form the subject of another volume."

## XI. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

Nov. 15.—DR. KIDD and Dr. Richardson were respectively admitted Fellows of the Society.

The Croonian Lecture, by Sir E. Home, Bart. V.P. was read, entitled, " On the muscles peculiar to organs of sense in particular quadrupeds and fishes."

The author selected for the subjects of this lecture the peculiarities in the muscular structure of the tongue of the Xariffa or *Camelopardalis Giraffa*, an inhabitant of Soudan in Africa; and a muscle belonging to the eye of the *Cobitis anableps*, a fish inhabiting the rivers of Surinam, and called by the natives " the four-eyed fish."

The tongue of the Giraffa, besides being an organ of taste, has many properties of the elephant's proboscis. The latter is incapable of elongation it is true, while the former may be extended to seventeen inches in length. The author observes, that some mechanism must exist by which this elongation can be performed, but that an opportunity of examination after death would be requisite to decide on its nature. The tongue of reindeer offers the same analogy, which however he has not been able to trace for want of time. The chameleon can dart out its tongue to the extent of twelve inches, and, for this purpose, as well as to direct its motion, it has a conical bone inclosed in a muscular tube, the fibres of which are circular, and aid by their pressure to make the bone slide forwards. The Xariffa wants the receptacle for water, which the camel and dromedary possess, it being needless for it as it lives on succulent plants; neither has it the padded hoof to fit it for travelling in sand, but two toes defended by a horny covering,

vering, to enable it to climb rocky ground without stumbling. Its long neck consists of only seven bones, being the same number that occurs in the human skeleton. The tongue is every where smooth and slightly adhesive; it is spotted, but the spots are not raised. Its favourite food is the Acacia tree, of that species now called *Acacia Xariffa:* it has a pleasant flavour both boiled and raw, and its twigs are succulent. The tongue being much exposed to the sun, has a black *rete mucosum*, to prevent blistering. Drawings, by Mr. Cross, exhibit the mode in which it lays hold of the branches of trees. It drinks milk, first rinsing out its mouth with a portion, and rejecting that so employed. It chews the cud, its body being then recumbent and its head and neck erect.

The organ of vision of the *Cobitis anableps* is very remarkable. The specimens examined by the author were furnished by Dr. Muttlebury, of Bath. He first exposes the errors of Artedi and his followers, in their description of this animal; and states, that the cornea being removed, the iris is exposed, having an appearance of two pupils; but on more accurate examination this effect is seen to arise from two lateral projecting portions, folding over each other in the middle, thus dividing the aperture into two. They do not however unite; and in some specimens they leave the pupil entire, only very narrow in the middle, forming an oval, broad above and narrow below; usually, however, they leave two distinct apertures. The crystalline, instead of being spherical, is not even circular, having a small projection at the lower edge, directly behind the smaller aperture. When examined in the microscope, a small bundle of muscular fibres is seen coming from the capsules of the vitreous humour in the lower part, and entering that of the crystalline just at the disc where the smaller curve joins the large one, the action of which is to bring the lower mammary process of the lens downwards and backwards into the centre of the lower apertures in the iris; thus constituting a complete organ for vision at near distances, independent of the part of the lens opposed to that large aperture, which is destined for more distant objects.

The author regards this structure as destined to a similar purpose with that of the *marsupium* in birds, viz. to obviate a difficulty arising from a want of motion in one direction in the eye-ball. He considers that by its means also, the fish, when lying with its eye-ball above the surface of the water, may enjoy distinct vision both in air and water by the motions of the crystalline and eye-ball, combined with the adapting power of the two apertures of the iris to a circular form.

A paper was read, entitled, "Experiments to determine the difference

difference in length of the seconds' pendulum in London and Paris; by Capt. E. Sabine, Roy. Art., F.R.S."

The author commences this paper by a brief statement of the existing state of the determinations of standards of length in the two countries; and he observes that an attempt made by M. Arago in 1817 and 1818, to bring into immediate comparison the standards of the two countries, proved inconclusive, from the rates of the pendulums not having been obtained with sufficient exactness. The author having obtained from His Grace the Master-General of the Ordnance a general leave of absence from his military duties, so long as he could be usefully employed in scientific pursuits, conceived he could in no way better satisfy the condition than in carrying into effect this purpose. Accordingly, being provided with two pendulums,—one made by M. Schumacher, another the property of the Board of Longitude,—he set out for Paris, whither the pendulums were forwarded to him. The comparison was made in Paris at the Royal Observatory, in the Salle de la Meridienne, on the spot in which M. Biot's measurement had been made, and every proper facility and assistance was afforded him. The coincidence clock was compared every twelfth hour by M. Mathieu, with the transit clock of the Observatory. On the 27th of April, the weather having set in mild and steady, the experiments were begun. The results are stated in the form of appended tables, of which a detailed account is given.

Each of the pendulums, when not used in observing coincidences, was employed in determining its rate by a journeyman-clock or counter,—a method used by Messrs. Freycinet and Duperrey; but which the author thinks inferior to that of coincidences, though capable of giving good results. The particulars of these are given in two of the tables. From all the experiments in conjunction, it appears that the numbers of vibrations performed in a mean solar day at Paris (reduced as usual) by the two pendulums, were respectively 85922·06 and 85933·83.

The pendulums and apparatus were reconveyed to London, early in September, by water; and the rates again determined at Mr. Browne's house in Portland-place, by means of that gentleman's excellent clocks and transit observations made by Capt. Sabine. The precautions used are fully detailed; and the observations, which are also appended in a tabular form, the author being assisted by M. Quetelet, of Brussels. They give as a final result 85933·29 and 85945·85 for the numbers of vibrations respectively made by each in a mean solar day, similarly reduced for London.

As a final result of the whole operation, the author regards

12$^{s}$·00

$12^s \cdot 00$ as the acceleration of the seconds' pendulum in passing from Paris to London. The same acceleration deduced from a comparison of M. Biot's and Capt. Kater's direct measurements of the seconds' pendulum, in Paris and in London, comes out $11^s \cdot 76$; or conversely, the length of the seconds' pendulum observed by the former in London transferred to Paris, by an assumed retardation of $12^s$, gives a length differing from M. Biot's by $0^{in} \cdot 00023$. Borda's agrees within $0^{in} \cdot 00079$ with M. Biot's; and Capt. Kater's, so transferred, holds very nearly a mean between the two, but approaches rather nearer to Biot's than to Borda's.

Nov. 22.—W. A. Mackinnon, Esq. was admitted a Fellow of the Society.

A paper was read, entitled, "On a peculiarity in the structure of the *ductus communis choledochus* and of the pancreatic duct in man; by John Davy, M.D. F.R.S."

The peculiarity noticed by the author, consists of a valvular apparatus formed by delicate angular processes of the mucous coat of the lower part of the *ductus communis choledochus* and of the pancreatic duct; which he detected by slitting open these tubes under water, and washing off the adhering mucus. The effect of this structure is to prevent any retrograde motion in the fluids conducted by these tubes: and although a very delicate probe may be made to descend through the ducts with facility, its passage in the opposite direction is arrested by the *sacculi* which are formed by the processes of the inner membrane. This structure is not met with either in the sheep or in the ox, in which animals there is no junction of the biliary and pancreatic ducts.

Another paper by Dr. Davy was read, entitled, "Observations on the action of the mineral acids on copper, under different circumstances."

In prosecuting the researches into the slow operation of electro-chemical agency on the alloys of copper, which was the subject of his former paper published in the Philosophical Transactions, the author was induced to examine the action of the mineral acids on copper, under different circumstances. When atmospheric air was excluded, dilute sulphuric acid, into which a bar of polished copper was immersed, had dissolved at the end of three months only a very minute quantity of that metal, and the bar was slightly tarnished with the black oxide of copper. A similar result was obtained with dilute muriatic acid; but dilute nitric acid dissolved a larger portion of the metal, and the bar was encrusted with black oxide. When the vessels in which similar experiments were made, were covered only with glass, so as to retard evapora-

 tion

tion but not to exclude the air, at the end of eight months the sulphuric acid was found saturated with copper, and the bar covered with a thin crust of black oxide. With nitric acid there was also a considerable deposition of protoxide of copper on the bar, together with a little crystallized sub-nitrate, and a very minute quantity of metallic copper. With the muriatic acid, depositions similar to those with the nitric acid took place, the submuriate being very abundant, and crystallizing as in the native specimens of this mineral from Peru.

The author considers the complicated results produced by the presence of atmospheric air as referrible to electro-chemical action, arising from the reaction upon each other of the combinations formed.

A paper was likewise read, entitled, "On the structure of the knee-joint in the *Echidna setosa* and the *Ornithorhynchus paradoxus*; by G. Knox, M.D.F.R.S. E., communicated by Sir James MacGregor, F.R.S."

After a short review of the labours of comparative anatomists on the animals which are the subject of this memoir, the author describes a peculiarity of structure which was discovered by his brother in the knee-joint of the *Echidna*, consisting of an extension of the *ligamentum adiposum*, or re-duplication of the synovial membrane transversely across the whole joint, dividing it into two cavities which have no distinct communication with each other. The articular surfaces of the upper cavity are the patella and the anterior portions of the condyles of the os femoris, while the lower are formed by the inferior and superior surfaces of these condyles, the upper surface of the tibia, and the semi-lunar cartilages. In the *Ornithorhynchus paradoxus*, the double fold of the synovial membrane extends only half-way across the joint, thus constituting an intermediate link of gradation between the *Echidna* and Man, in whom the *ligamentum adiposum* is wholly within the joint.

Nov. 30.—At the Anniversary Meeting of the Royal Society on St. Andrew's day,—after the names had been read of all Members deceased in the preceding year, and before the Medals were delivered, Mr. Davies Gilbert (President,) addressed the Society to the following effect:

Among the names now read, that of His Royal Highness the Duke of York demands our first attention.

We have in common with the whole nation to deplore the loss of an illustrious personage, who has rendered most essential services to his country by discharging the duties of a high

high military office during a period more arduous than any other in the modern history of Europe. But on topics so remote from our habitual pursuits it would be useless for me to dilate. Justice is done to His Royal Highness by the high station which his memory holds in the opinion and in the estimation of the public.

We have also to lament the loss of two of our Fellows, connected with the Society in the relation of Vice Presidents—the Earl of Morton, and the Bishop of Carlisle.

The first,—in addition to his own merits possessing a strong hereditary claim to our regards, as the descendant of Lord Morton who presided over the Society about sixty years ago,—assisted in our labours; and contributed his aid, with Lord Macklesfield and other distinguished persons, in 1752, to assimilate our Style or Calendar to that used by the continental nations of Europe. To the individual of whom death has now deprived us, we owe much gratitude for his uninterrupted countenance and attention during a long series of years: and in the *Transactions* for 1821 will be found a communication, by Lord Morton, of a curious fact in physiology.

Dr. Samuel Goodenough, late bishop of Carlisle, has ever sustained the character of a sound and elegant scholar. Entrusted with the education of distinguished personages, and having qualified them for the first situations in the state, he fairly and honourably ascended to the summit of ecclesiastical preferment. To classical and theological learning, Dr. Goodenough added a very intimate knowledge of natural history, as is manifested by a communication to the Linnean Society, where his labours have thrown a steady light over an extensive genus of aquatic plants, left by all former botanists in obscurity and confusion. The memory of Dr. Goodenough will long be cherished with affection and with esteem by all who had the honour of his acquaintance either in his public or in his private life.

If it were usual to advert in a particular manner to each Member of whom the Society has been deprived within the last year, there has not been read perhaps the name of a single person, on whom one might not dilate with a melancholy pleasure and satisfaction.—Mr. Canning, pre-eminent throughout the world; The Bishop of Winchester, senior Wrangler of his year, and tutor to Mr. Pitt; Mr. Cline; The Marquis of Hastings; The Duke of Gordon;—all men of literature or science. But as these and other individuals have not been connected in any peculiar relation to the Society, nor shared in its labours, we have only to mention them with regret.

gret.—Colonel Beaufoy may be considered an exception. That gentleman is well known to have devoted his time and attention to practical astronomy; and having confined himself to certain departments, in these he is said to have greatly excelled. His observations of stars occulted by the moon, and of eclipses of Jupiter's satellites, are believed to form the most complete series any where to be found; and such observations are very essential for completing the theories of these respective bodies. —And here it is not irrelevant to mention, that, by the liberality of his son, (professionally engaged in other pursuits and in other countries,) the instruments so well used by Col. Beaufoy are now bestowed on the Astronomical Society.

Two Fellows of the Society demand, however, our special notice—The Rev. Abram Robertson, and The Rev. John Hellins.

Dr. Robertson first appeared in the town of Oxford as a practitioner of medicine: but his abilities and mathematical attainments soon attracted notice. He was induced to become a member of the University; was admitted into orders; received a chaplainship of Christ Church; and became lecturer in geometry, first to the College, and afterwards to the University. In due course Dr. Robertson became professor of mathematics, and finally astronomer at the Radcliffe Observatory. We have in our *Transactions* various proofs of Dr. Robertson's diligence and abilities. Two papers "On a demonstration of the laws given by Sir Isaac Newton for expanding a binomial." This expansion, justly esteemed of the utmost importance as the foundation of every other, and as developing the whole system of fluxions, has received various elaborate demonstrations, the great author having simply contented himself with the annunciation. Among those of the same date none are more clear or satisfactory than the one given by Dr. Robertson: nor does it detract from his merit, that subsequently others have been devised in the general expansion of functions, more concise, and perhaps more immediately urgent of conviction on the mind. We have a third paper "On the precession of the equinoxes." A fourth, showing "A direct method of computing the excentric from the mean anomaly." And a fifth, demonstrating "A theorem in spherical trigonometry, given by the late Dr. Maskelyne." But the great work of Dr. Robertson is his Treatise on Conic Sections;—following the geometric method of Apollonius among the ancients, and of Hamilton in our own times, by deriving all the properties from the cone itself. As an academic book for the instruction of young men this may well be stated as too extensive,

extensive, and requiring more time than can now be allotted to any one branch of mathematical science. As a monument to the author's fame, it promises to remain for ages.

The Rev. John Hellins was one of those extraordinary men, who, deprived of early advantages, have elevated themselves by the force of genius and of industry to a level above most persons blessed with regular education. Mr. Hellins at one time computed for the Nautical Almanac. He afterwards assisted at Greenwich. And what is now perhaps almost unknown, he furnished the late Mr. Windham with all the calculations and tables on which that gentleman brought forward his new military system, as minister of war, in 1806. Mr. Hellins applied himself with great industry to some of the most useful branches of pure mathematics. No less than nine communications appear in our *Transactions*. "On the summation of series."—"On the conversion of slowly-converging series into others of swifter convergency."—"On their application to computing of logarithms, and to the rectifying of circular arcs."—"On the roots of equations." And in 1798, "On a method of computing with increased facility the planetary perturbations:" for the last he was honoured with your Copley medal. Retired to a small living in Northamptonshire, Mr. Hellins became a pattern of philosophic calmness and content

> Far from the madding crowd's ignoble strife,
> His sober wishes never learn'd to stray.

He seems to have said

> Curtatis decimis, modicoque beatus agello,
> Vitam secrete in rure quietus agam.

I have known Mr. Hellins above forty years, and I can testify to his virtues. It once happened that, through the late Dr. Maskelyne, I had nearly obtained for him the Observatory at Dublin. The failure cannot however be lamented, since Brinkley was appointed in his stead.

Although death has deprived us of several eminent persons at home; yet undoubtedly the greatest loss to science must be sought for this year in our foreign list. We find there the names of Bode, of Volta, and of La Place.

Professor Bode is known to every one by his magnificent Cælestial Atlas in twenty large plates, containing 17,000 stars laid down and catalogued with a degree of accuracy unknown to former times; and with an elegance and beauty that may never be excelled. This book appeared about thirty years ago; and the author is said to have employed himself up to very recent times in the cultivation of his favourite science, and

and in giving to the public some books explanatory of the elements, and others more profound; but all in a language little known in this country. He has now departed this life full of years, and with a reputation commensurate with his age.

Professor Volta has enjoyed the rare and enviable felicity of founding a new science. Mr. Galvani had indeed observed the extraordinary effects of peculiarly modified electricity, in exciting the nerves and muscles of frogs: but misled by the physiological hypothesis of a nervous fluid acting intermediately between the sentient principle and the material frame, he hastily concluded that the nervous fluid was now within his reach; and the appearances were denominated Animal Electricity. Nor can we perhaps justly blame Galvani for the generalization that he had formed. Volta himself adopted it for some time, as appears from his paper communicated in 1793: but further experience convinced him that the whole might be explained by electricity chemically produced; and this opinion has been satisfactorily demonstrated by the invention of his pile. Mr. Volta communicated a paper to the Royal Society as early as the year 1782, "On a method of detecting minute quantities of electricity." In 1791 he was elected a Fellow on the Foreign list. And in 1793 Mr. Volta transmitted to the Royal Society, through Mr. Cavallo, the account of Mr. Galvani's discoveries and of his own; which obtained for him the reward of your Copley medal. And in 1800 the *Transactions* were again enriched by a paper from Mr. Volta "On the electricity excited by mere contact."—To pursue the history of Voltaic or chemical electricity any further, would be to detail the successful labours of our own countrymen. Here the pile has been modified into the much more convenient and efficient form of the plates and trough which, in the hands of Sir Humphry Davy, have produced effects equally astonishing and important: and enlarged by the gentleman who now sits on my left hand*, the plates have exhibited such energies as were previously not even contemplated. Galvani has not affixed his name to the science of which he is in a great degree the parent, and which has continued to exercise his genius up to the extremity of a long life: but he has had the satisfaction of witnessing the continually increasing brightness of a flame first kindled by himself, and which he has never ceased to fan.

I now approach La Place. But it cannot be expected that I should give more than a very slight sketch of this extraordinary man.

La Place appears to have commenced his illustrious career

* Mr. Children.

in

in consort with a philosopher distinguished by the sagacity which enabled him to seize the clues leading to all the recesses of modern chemistry, by the indefatigable industry and acumen exerted in exploring every chamber of the labyrinth, and by the unfortunate period in which his lot was cast: a period devoted to an unrestrained action of the worst passions of the human heart,—which could have alone arrested, by a violent death, the progress of Lavoisier. Deprived of his friend and associate in chemical pursuits, La Place appears to have devoted the whole energies of his powerful mind to a science the most abstruse and difficult, but the most sublime of all that are placed within the reach of human intellect. Astronomy, so far as two bodies were alone concerned, had attained absolute perfection by the discoveries of Kepler and by the demonstrations of Newton.

The elliptic orbits, areas proportional to the times, and the squares of the periods, as the distances cubed, left nothing to be desired. But when the regular motion of a heavenly body round its primary is disturbed by a third, the circumstances are widely different. Sir Isaac Newton had indeed sufficiently shown that the principles of gravity and inertia were adequate to solving the problems of three bodies: but in a field so vast, the exertions of no one man, not even those of Newton, were sufficient for its entire cultivation. The labour of others, —of Bernouilli, of Clairaut, of Euler, of Mayer, of La Place, were required in aid; but still pursuing the system and plan dictated by their great master.

To describe the first important discovery of La Place, it will be necessary to premise some particulars. The problem of three bodies requires, From the momentary direction and intensity of the disturbing force, expressed in the generality applicable to all parts of the orbits, to infer the effects produced (through the medium of integration) in any finite time. It is probable that no effort of the human intellect could ever have attained this object, but for the expedient of imputing to the orbit a physical existence, and consequently a liability to variation in all its parts. The larger axis equal to twice the mean distance, the lesser axis indicative of the excentricity, the line of the apsides, and the inclination of the orbit and the nodes in respect to the orbit of the disturbing body. All these variations are expressed by expansions the most elaborate and complicated. But La Place has the glory of discovering, that after including every term of the expansion which involves the second powers of the excentricities, the larger axes, and consequently the mean distances, remain unchanged. All the larger terms in expanded series indicative of

perturbations

perturbations re-establish themselves in short intervals: but others of less magnitude are extended by the peculiar form of their coefficients through periods extremely long. Those measure what had previously been termed secular equations, supposed to vary as the squares of their distances from some assumed epoch. Since the *Méchanique Celeste* has enlightened the world, empirical equations have disappeared; and others corresponding with the true principles of rotary movements have assumed their place. Among these, as similar in their nature, may be included the two great equations of Jupiter and Saturn, each corresponding to the attractive power of the other planet, as is ascertained by the periods and distances of their satellites.

Descending to the peculiar system of our earth, La Place has deduced from gravity all the complicated inequalities of the lunar movements, and some of these involving the distance of the sun in comparison with that of the moon. The solar parallax has been derived from the lunar, with a degree of accuracy greater than can probably be obtained from observations on Mars, or even from that rare phænomenon, a transit of Venus.

Arrived at the earth itself, this illustrious philosopher has investigated with peculiar care the precession of the equinoxes,—an element of the utmost importance in all astronomical researches, caused partly by a displacement of the ecliptic from planetary attraction; but in a much greater degree by the attraction of the sun and moon on the oblate figure of the earth: the compression entered therefore as a main ingredient into these inquiries, to be considered under every probable variation of internal densities. The theory of compression has been perfected by La Place: but data were still wanting to reconcile all the anomalies caused by depositions or formations dependent on causes, to us, apparently fortuitous. These data, we hope, are either now attained, or at the least brought within our reach, by the ingenuity and perseverance of a gentleman who hears me *, in adapting to practical use the reciprocating property in pendulums between the axis of suspension and the centre of oscillation; a property long known, but never before applied. And by the unhoped-for accuracy recently attained in geodetical operations, by the use of measures possessing within themselves the power of adjustment for heat and cold, under the care of that distinguished individual who is at this moment conducting the trigonometrical survey of Ireland †.

* Capt. Kater. † Col. Colby.

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The attention of our great philosopher was by a natural progress carried on to a consideration of the tides, which exhibit phænomena in apparent direct opposition to their cause; and which remained utterly incapable of solution till the discoveries of Newton disclosed the powers of centrifugal and centripetal forces. La Place, in the investigation of this most interesting, curious, and important problem, has not only taken into account the declinations and parallaxes of both the great luminaries, but also the oscillations of fluids in transmitting motion as connected with their depths. And hence he has been enabled to form a probable conjecture respecting the depths of our great oceans. From investigating the laws of Nature as displayed by the heavenly bodies, La Place descended to a subject not less difficult or intricate,—the action of particles on each other in capillary attraction, and in the transmission of sound. And in a separate work he has exhausted, by the most ingenious expedients, a subject scarcely less profound than either of the former,—the doctrine of probabilities. I do not notice separate memoirs and communications to Societies, most of which are embodied in his subsequent works. He is said to have contemplated a rigorous mathematical survey of the atomic theory, as developed by definite proportions. But the fate common to humanity has interposed. La Place has lived sufficiently long for his own fame: but no extension of his life would have satisfied the expectation or the demands of science. The time is not arrived, nor can I presume to assign to this extraordinary man his rank among those that are no more. Yet, speaking from this place and on this occasion, I will say, that Newton holds, and ever must hold, the highest station in the fane of philosophical renown. That to him

Non viget quicquam simile aut secundum.

But although the second niche must remain unoccupied, yet one approximating to that of Newton will hereafter become the elevated station of La Place.

### *On delivering the Royal Medal for Sir* HUMPHRY DAVY.

It is with feelings the most gratifying to myself, that I now approach to the award of a Royal medal to Sir Humphry Davy; and I esteem it a most fortunate occurrence, that this award should have taken place during the short period of my having to discharge the duties attached to the office of President; having witnessed the whole progress of Sir Humphry Davy's advancement in science and in reputation, from his first attempts in his native town, to vary some of Dr. Priestley's experiments on the extrication of oxygen from marine vegetables, to the point of eminence which we all know him to have reached.

It is not necessary for me more than to advert to his discovery of nitrous oxide; to his investigation of the action of light on gases; on the nature of heat; to his successful discrimination of proximate vegetable elements; nor to his most scientific, ingenious, and useful invention, the safety-lamp,—an invention reasoned out from its principles, with all the accuracy and precision of mathematical deduction.

The particular series of discoveries for which the Royal medal has been awarded, are those which develop the relation between electricity and chemistry.

Soon after Sir Humphry Davy had been seated at the Royal Institution by an invitation from Count Rumford, an invitation founded on his first production,—A paper on the nature of heat,—our late President began his experiments and investigations on electric chemistry: a most powerful Voltaic apparatus was fortunately placed at his disposal; and in his hands electric chemistry soon became the most important branch of practical science: important from its immediate energies and powers; but much more so from the general laws of nature, which it has laid open to our view.

A new acidifying principle, or supporter of combustion, was discovered, possessing the same negative electric properties as oxygen. Muriatic acid disclosed its real composition. The oxymuriates were transferred to their proper class. The alkalies were reduced into metals; and the earths were proved to be similar oxides. But in the progress of these experiments a discovery was made, surpassing all the wonders attributed to alchemy. Three basins were arranged in a straight line, each containing water, and to the middle basin some neutral salt was added. The three were connected by moistened syphons of asbestos: the opposite piles of a Voltaic battery were then applied to the extreme vessels; and in a short time the neutral salt disappeared from the middle basin, and its constituent parts were found separated; the acid attracted to the positive pile of the battery, the alkali to the negative. This astonishing result, followed up by other experiments, led to the conclusion that chemical energies may be increased, diminished, or even inverted, by the superinduction of electric powers homogeneous with or dissimilar from their own. This metastasis in the hands of physiological inquirers promises to conduct them to discoveries of the utmost importance in the functions of life. I flatter myself that it is now actually in such hands.

The

The principle of varying or modifying chemical energies by those of electricity has been applied by the invention, in a manner the most philosophical, and on a scale the most extensive.

The copper sheathing of ships and vessels had been found to corrode in the short period of a single voyage, being converted into an oxide through the medium of some acid, or at least of a decompounded substance, occupying the negative extremity of the electric scale. The copper must therefore be positive in respect to tbe body decomposed or attracted. A reference was made by the Government to the Royal Society, with the hope of discovering some remedy for this most serious evil. Grounded on a perfect knowledge of chemical and of electric powers, it immediately occurred to the illustrious discoverer of their relations one to the other, that if a substance more positive than copper, and in contact with it, could be exposed to the corroding action, that the copper would, by induction, be rendered less positive, and therefore indisposed to combine with any other negative body.

Experiments the most satisfactory were then made on a small scale; and in consequence of their success, plates of zinc, and afterwards of iron, were applied to ships' bows; and the copper has been fully and completely protected. The theory and the experiments have been confirmed in the most ample manner. A defect has indeed occurred in practice, from the over success of protection. The induction of negative powers to the copper has gone too far; they have caused it to act on the compounds in an opposite direction, by attracting to itself the earths and alkalies, thus affording attachments to the marine vegetables which the copper was intended to prevent. This appears to me, however, susceptible of a cure. I am sufficiently advanced in years to remember the American-revolution war. Ships were then first sheathed with copper: they were preserved clean from weeds, nor was the copper corroded: but the ships were fastened together by iron bolts, and these, to the utter astonishment of every one, decayed; and the ships became unable to sustain the ordinary straining in gales of wind. For some time the effect could not be traced to its cause, for galvanism was then unknown; but at last bolts made of bronze were substituted for those of iron, and immediately the copper failed. When the theory has therefore been modified by experience on the principle of these empiric trials during the American war, I cannot hesitate in predicting complete practical success; with full glory to the illustrious individual who deduced the practice from theory, and with ample advantage to all those who may then bring the practice into beneficial use.

Sir Humphry Davy having last year communicated a paper to the Society in continuation of his former inductions and generalization on chemical and electric energies, there cannot be a doubt but that the only obstacle against his then receiving a Royal medal, on the first occasion that the Society had it to bestow, was his occupying this chair. That obstacle, unhappily for science, no longer exists; and the Royal Society take this earliest opportunity of testifying their high estimation of these talents and of these labours which all Europe admires. We trust and hope, although our late President has been induced by medical advice to retire from the agitation of active public stations, that his most valuable life will be long spared; and that energies of mind may still be displayed to this Society and to the civilized world, equal to those which have heretofore rendered immortal the name of Davy.

### *On delivering the Medal for* M. Struve.

In no science has progressive dilatation and expansion been equally manifested as in astronomy. Limited at first to observing the phases of the moon; to conjecturing the return of eclipses after certain periods; to arranging religious festivals, or the times for agricultural labour by the heliacal rising and setting of stars,—it has ultimately extended to these stars themselves.

A most eminent individual of our own times may be considered as the parent of sidereal astronomy. The late Sir William Herschel, having doubted the extent of the solar system, leapt boldly beyond its comparatively narrow bounds, and laid open to our view suns mutually revolving round each other at distances from whence the orbit of the earth can subtend no more than a physical point. The same piercing eye distinguished between clustered stars, and spaces shining from collections of nebulous matter, destined perhaps in the course of ages to condense into a more solid form. These sublime pursuits are now actively continued, I am most happy to say, in our own country by a son worthy of such a father, and by a gentleman who, uniting energy, ability, and perseverance, allows no one to run before him in whatever he undertakes*.

The same subject is also investigating on the continent of Europe, with the attention and assiduity that it so well deserves,

* Mr. South.

serves, especially by M. Struve, director of the Observatory at Dorpat. Having obtained the use of a powerful refracting telescope, this indefatigable astronomer has, in the short space of four years, alone and unassisted, produced this work,—A Catalogue of Double and Multiple Stars, to the amount of 3063 stars, "*Catalogus Novus Stellarum Duplicium et Multiplicium,*"—all laid down with an accuracy commensurate to the great labour and attention bestowed on them. The Royal Society have not hesitated in marking with their highest approbation such labour, such attention, and such ability, displayed by an individual who has thus established strong claims for gratitude on the astronomical world, and from a continued exertion of whose energies much more may with confidence be expected.

*On delivering the Copley Medal to Dr.* WILLIAM PROUT.

The science of chemistry, like that of astronomy, may reckon its different periods and distinct elevations. The decomposition of neutral salts; the discovery of gases; the decomposition of water; the application of voltaic energies, by the skilful hands of our late President; the atomic theory, with definite proportions. But nature is inexhaustible, and much more remains to be done. Although the ultimate elements of animal and vegetable substances are known to be few; yet their proximate elements, produced by unions of the former in different proportions and in different manners, continue indefinite. Besides the amylaceous, the saccharine, and twenty other principles widely diffused through the vegetable kingdom, there remain the essential oils and specific secretions; each resolvable into the few ultimate elements, and so readily changing the Proteus-like appearance of their proximate forms, as to baffle all but the most delicate and refined attempts to investigate their real properties and discriminating qualities, —qualities induced through the agency of life, and perhaps involving substances not subservient to the laws of gravity and inertia; the essential attributes of ordinary matter.

Much progress has indeed been made in separating particular substances from their combinations with others, to the great improvement of pharmacy and medicine.

But the Royal Society have viewed with peculiar satisfaction a new and accurate mode of analysis described by Dr. Prout, and founded on the most evident and simple principles; promising not merely to disentangle any one particular combination, but to afford an insight into all the products created by living chemistry. They have hastened, therefore, to stamp with the highest mark of their approbation, as well the mode of of analysis itself, as the specimens of what, in the hands of Dr. Prout, it has already performed: and not doubting, but that by the exertion of such talents, such ingenuity, and such labour, their satisfaction will from year to year be continually increased.

*On delivering the Copley Medal to Lieutenant* HENRY FOSTER.

Of all the accidental discoveries ever made by man, the most unexpected and extraordinary, as well as most useful in its consequences, appears to have been the magnetic needle. No one could have thought it within the range of possibility to devise any plan, by means of which a ship in the midst of a wide ocean, surrounded with perfect darkness, and tossed by the winds and by the waves, might yet be able to ascertain its course with the same certainty as in open day, and under circumstances the most favourable. Yet the simple experiment of a child floating a magnetized needle on a cork, directly led to this important discovery. The variation must, without doubt, have been immediately observed. Columbus is said to have first noticed, and with astonishment and dismay, that the variation increased as he proceeded on his great voyage of discovery towards the West. Magnetism, a subject at once so curious and so useful, was attended to with such care, that the secular change of variation in the same place did not long escape notice. The dip was early known; and more than a century ago observations were made on the daily change of direction, and on a supposed relation which it bore to the appearance of northern lights, and to other natural phænomena. Nor were theories wanting:—some utterly gratuitous, as that conjectured by the celebrated Dr. Halley, who supposed arbitrary points of attraction, and an internal earth or revolving nucleus. Other theories, although given by less eminent men, appear to be more conformable to the true principles of generalizing by induction; as that quoted by Mr. Foster, from Derham in his Physico-theology. But the accuracy of modern experiment was wanting. The method of counting vibrations to ascertain intensity had not then occurred; nor were instruments, in all probability, to be procured that were accurately made, or of much delicacy of motion. In recent times, and by a member of this Society *, we have seen the local attraction of ships compensated on the most scientific principles; terrestrial direction neutralized; and the line of action, at least of diurnal variation, ascertained.

And we have seen phænomena little less astonishing than

* Mr. Barlow.

the one displayed by the original discovery of magnetism itself, in the connection that it bears to electricity and in the induction from rotary motion. It was important, however, that experiments should be repeated in different places, and especially in those which are most difficult of access, but situated near to the magnetic pole.

Lieutenant Henry Foster, well known to this Society by the cooperation he afforded to Captain Basil Hall in determining the number of vibrations made by an invariable pendulum near the equator, and at several other stations; having shared in the dangers of Captain Parry's second voyage, eagerly seized the opportunity afforded by a winter residence at Port Bowen, on the eastern side of Prince Regent's Inlet, in lat. 73° 14′, to ascertain the rate of an invariable pendulum, to conduct an elaborate course of experiments on magnetism; and in addition to these, observations on refraction.

One is utterly astonished at the magnitude of these labours, and at the accuracy and care with which they were conducted, (as is manifest from internal evidence,) in a situation where comfort and ease were unattainable, and where peculiar difficulties presented themselves at every step. It is impossible for me to give an abstract of Mr. Foster's most ample and detailed communication: I must refer every scientific inquirer to the paper itself. Among its important contents are:—The amount and times of daily variation attributable of course to the sun, but including in one series the action apparently caused by the moon.—The line on which a needle being directed the daily variation ceases.—A refutation of the supposed connection between tremors of the needle and aurora borealis.—The amount and times of daily variation in the dip; with a deduction from thence, according to the known law of the cosines, to the periodical change in horizontal intensity.—And a legitimate conclusion from all these facts, that the magnetic axis of the earth may probably describe a small curve, compounded of circles attributable to the sun and moon, of two or three minutes' radius round its mean place, which will solve the change of dip, of diurnal variation and of horizontal intensity; and may account for the secular variation in a manner similar to that which explains the precession of the equinox.

The Royal Society are of opinion that they do no more than strict justice, in awarding their Copley medal to the author of these observations and deductions; and not without a hope that by so doing, public attention may be more strongly drawn towards an officer possessing such abilities, energies, and preseverance *.

* Never were expectations more speedily or more amply gratified; for on

After the delivery of the Medals, the Society proceeded to the election of a Council and Officers for the year ensuing; when on examining the lists, the following was found to be the state of the ballot:

Members of the old Council to continue.—Davies Gilbert, Esq. M.P. Capt. Francis Beaufort, R.N.; John George Children, Esq.; Sir Humphry Davy, Bart.; John F. W. Herschel, Esq. M.A.; Sir Everard Home, Bart. V.P.; Capt. Henry Kater, V.P.; John Pond, Esq. A.R.; William Prout, M.D.; William Hyde Wollaston, M.D. V.P.; Thomas Young, M.D. Foreign Sec.

Members of the Society chosen into the Council.—Francis Baily, Esq.; The Rev. William Buckland, D.D.; Charles Lord Colchester; John Wilson Croker, Esq.; William Henry Fitton, M.D.; The Rev. Edmund Goodenough, D.D.; John Guillemard, Esq.; John Ayrton Paris, M.D.; Peter Mark Roget, M.D.; Capt. Edward Sabine, Roy. Art.

Officers for the ensuing year:

*President:* Davies Gilbert, Esq. M.P.—*Treasurer:* Capt. Henry Kater.—*Secretaries:* Peter Mark Roget, M.D.; Capt. Edw. Sabine.

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## ASTRONOMICAL SOCIETY.

*Nov.* 9.—Mr. Baily presented a paper "On the right ascension of $\gamma$ *Cassiopeæ.*" As this paper is a short one, and of an interesting nature we shall give it nearly in the words of the author:

"On comparing the Catalogue of Stars, recently published by this Society, with the Catalogue of 100 principal fixed stars given by Mr. Pond, at the end of the Nautical Almanac for 1829, I was struck (he says) with the considerable difference which appears in the Æ of $\gamma$ *Cassiopeæ:* Mr. Pond making the Æ of that star upwards of one second (in time) more than the Catalogue printed by this Society. At first I imagined that some error might have crept into the calculations of the Society's Catalogue, notwithstanding they were made by two computers, independent of each other, and afterwards revised by our indefatigable secretary, Mr. Stratford. I therefore, for my own satisfaction, went through the whole computations myself, and was pleased to find that there was not the slightest difference in the results. I next reduced the whole of the observations of that star made by Mr. Pond at the Royal Observatory at Greenwich, and found them to agree very nearly with the result, deduced by Mr. Taylor, who makes the mean of 10 observations to be $= 0^h\ 46^m\ 13^s,23$ reduced to Jan$^y$. 1, 1825: whereas the Society's Catalogue gives only $0^h\ 46^m\ 12^s,13$ on January 1, 1825; being a difference, as already stated, of $1^s,1$. Bradley has

on the very day that the medal was delivered, His Royal Highness the Lord High Admiral was graciously pleased to advance Mr. Foster to the situation of a commander, and to assign him a ship for a voyage of scientific investigations in the Southern hemisphere.

5 observa-

5 observations of this star, and Piazzi has 46. It is scarcely possible that Bradley should be in error one second of time: and still less probable that the mean of 46 observations by Piazzi should be erroneous to such an amount. The result of Bradley's observations (after allowing for the effect of precession) differs 8″,5 in space from those made by Piazzi: which quantity, divided by 45 (or the interval of years between the two observers), will make the annual proper motion of the star, if it really have any, about 0″,19 in space. But, this is not confirmed by the recent observations of Mr. Pond; and we must look elsewhere for a solution of the difficulty. That the position of a star of the third magnitude should be so undecided at the present day that its Æ cannot be satisfactorily depended on, to a second of time, does not speak much in favour of modern astronomy; and shows us that a great deal still remains to be done towards establishing the *fundamental* parts of the science. * * * Amongst other suggestions I have imagined that it might arise from a typographical error; and, in fact, if we suppose a misprint of 10″ in the Æ of $\gamma$ *Cassiopeæ* in Piazzi's Catalogue, that is, if we read Æ=11° 11′ 17″,6 instead of 11° 11′ 7″,6, the whole difficulty will vanish, and the results of the observations of Bradley, Piazzi, and Pond, will agree to the greatest exactness. But, we are scarcely warranted in making such an alteration without a reference to the original observations. Some suspicion however is excited that the printing is not strictly correct, from the circumstance that Piazzi considers the annual proper motion in Æ, as deduced from Bradley's observations, to be = 0; which would agree with the amended reading as here suggested: but which does not accord at all with the present reading in the Catalogue, since the annual proper motion is, as I have already observed, in such case = − 0″,19.

"Before I close these remarks, I would observe that there are also differences in the Æ of three other principal stars (besides that of $\beta$ *Scorpii,* which is acknowledged to be an error in the Greenwich Catalogue) to which I am desirous of calling the attention of the Society; and for which I can, by no means, account. These are $\zeta$ *Ursæ Majoris* and $\beta$ *Cephei* (both stars of the third magnitude) and *c Draconis,* a star of the fifth magnitude. The two last differ, as in the case of $\gamma$ *Cassiopeæ,* above a second in time from the Catalogue of Mr. Pond: but $\zeta$ *Ursæ Majoris* differs as much as 1ˢ,4. The case of this last star is the more remarkable, since the observations of Bradley and Piazzi correspond with wonderful exactness; there being a difference of only one second, in *space,* between them, after a lapse of 45 years: whereas from the time of Piazzi to the year 1825, a period of only 25 years, there appears, from Mr. Pond's observations, to be a difference of upwards of 20″.

"The whole of the computations relative to the positions of these stars, I have frequently repeated, and can assure the Society that there is no error in the results as printed in their Catalogue. Time only, and further observations, can clear up these apparent difficulties."

A paper was read "On double object-glasses; by M. Littrow."

The first part of this communication is devoted to the derivation of the equations expressing the conditions proposed by Mr. Herschel for the destruction of the aberration of sphericity in a thin double object-glass, by the ordinary processes in use among the German geometricians for such investigations. The author states himself to have entered on this investigation with a view to disseminate a knowledge of the theory alluded to in his own country; but being induced thereby to resume some former investigations of his own, he takes the opportunity to communicate to the Society his own principal results.

Taking for granted the well-known expression of Euler for the aberration of a lens of two surfaces, and developing it in descending powers of the distance of the radiant point from the lens, he obtains expressions, from which, by proper management, and substitution of similar quantities for a second lens, he derives the two final equations (A) and ($z$) demonstrated by Mr. Herschel. He then proceeds to the main object of his paper. This may be briefly stated to be the embodying of the relations expressive of the refraction of a ray through any four spherical surfaces, however situated, (provided they have a common axis), in trigonometrical equations, in which no quantity is regarded as small, and of course nothing neglected. These equations are in themselves sufficiently simple when undeveloped, and in that state may very readily be applied to determine whether any proposed construction of an object-glass really does satisfy the essential geometrical conditions of a perfect telescope, by producing a rigorous union of different coloured rays, and rays incident on different parts of the object-glass. Accordingly the author instances their application to a construction recently proposed by a German optician, as of peculiar excellence. In this construction the indices of refraction of the crown and flint lenses being respectively 1·53 and 1·60, and their dispersive ratio 0·25, the thickness of the crown lens 0·01, that of the flint 0, and the lenses being supposed in contact, the radii of the surfaces are

| | | |
|---|---|---|
| For the crown...... | 1st surface (convex) | 0·69281 |
| | 2nd surface (convex) | 2·255319 |
| For the flint........ | 1st surface (concave) | 1·543030 |
| | 2nd surface (concave) | 5·768005 |

Substituting these data in his equations he finds that they satisfy sufficiently well the conditions of achromaticity, but that they are very far from being entitled to the same encomium when the spherical aberration is considered.

But when the question is inverted, and the problem is, not to try whether a proposed construction be good or not, but, *à priori*, to determine what is best, the equations in question, though simple enough in their trigonometrical form, become complicated by the algebraic developments their direct analytical resolution necessitates. Now the essence of M. Littrow's proposed method is to do away with all this development, so far as it tends to produce complication; and after preparing the equations in the most convenient manner the case will allow, to substitute for their direct algebraical, an an *indirect numerical*, solution, by the well known and ready method of trial and error, a method perfectly correct in theory as well as easy in practice.

M. Littrow exemplifies this theory by applying it to that particular case when the indeterminate problem is limited by the condition of Klügel, viz. that the refractions at the first two surfaces shall be as small as possible, in which case the first or crown lens is in fact wholly given, and it only remains to determine the radii of the flint. He takes into consideration the thickness of the former, but not the latter, lens, and neglects also their distance *inter se*. As a numerical instance, he supposes the indices of refraction 1·53 and 1·58, the dispersive ratio $\frac{4}{5}$, and the thickness of the crown lens one hundredth part of its focal length, when, by applying the process described, he finds the following radii:

| | | |
|---|---|---|
| Crown lens.. | 1st surface (convex) ............ | 0·186823 |
| | 2nd surface (convex) ............ | 0·608170 |
| Flint lens .. | 1st surface (concave)............ | 0·407996 |
| | 2nd surface (concave)............ | 0·445808 |

Focal length of the compound lens = 1 ; aperture 0·06495

M. Littrow, however, is by no means of opinion that the condition of minimum refraction here assumed is the best, or in any way essential to a good object-glass ; only it facilitates calculation. He regards it as more advisable to aim at increase of aperture, and for this purpose to assume the first or crown lens equiconvex, which he affirms to be the condition requisite for the attainment of that end. He therefore explains his method of proceeding in this case, and applies it to the same numerical data with those last mentioned, which give for the final results:

| | | |
|---|---|---|
| Crown lens (equiconvex) | Radius of each surface.... | 1·06 |
| Flint lens (biconcave) | Radius of 1st surface .... | 1·04394 |
| | of 2nd surface.... | 3·296512 |

Focal length 3·702231

Aperture.... 0·09973 × focal length

or nearly $\frac{1}{10}$ of the focal length, being a much larger proportional aperture than it has been usual hitherto to give to achromatic telescopes.

The author concludes by stating the reason of his entering into these investigations: viz. an application made to him by an artist of Vienna.

A paper was also read from M. Slawinsky, containing the following observations, made at Wilna. (Longitude $1^h\ 41^m\ 12^s$ East of Greenwich).

1. *Eclipses of Jupiter's Satellites.*

| | | | | Sidereal time. | |
|---|---|---|---|---|---|
| 1825. | Jan. 17 | Im. | 1 .. | $4^h\ 0^m 23^s,7$ | good observation. |
| | Mar. 22 | Em. | 2 .. | 8 51 51 ,9 | very good. |
| | Apr. 23 | Em. | 1 .. | 10 44 25 | good enough. |
| | May 5 | Em. | 1 .. | 12 29 38 ,7 | very good. |
| 1826. | May 8 | Em. | 1 .. | 15 31 56 ,1 | middling. |
| | June 2 | Em. | 2 .. | 16 46 0 ,1 | middling. |
| 1827. | Mar. 18 | Im. | 1 .. | 16 1 14 ,9 | a little doubtful. |
| | Apr. 18 | Im. | 1 .. | 16 46 15 ,2 | passable. |

K 2 2. *Occu*

2. *Occultations.*

| | | | Sidereal time. | |
|---|---|---|---|---|
| 1825. Apr. 1 | *e* Leonis.... | Im. | $10^h\ 8^m\ 55^s,2$ | very precise. |
| | | Em. | 11 15 14 ,2 | a little doubtful. |
| 23 | Star (mag. 8·9) | Im. | 11 22 29 | exact. |
| 1826. Feb. 15 | 208 Tauri | Im. | 6 55 58 ,8 | good. |
| 16 | Saturn | | | |
| | 1st ansa | Im. | 3 11 58 ,8 | good. |
| | 1st limb | Im. | 3 12 8 ,8 | good. |
| | 2nd ansa | Im. | 3 14 48 ,3 | good. |
| 1827. Mar. 18 | λ Libræ...... | Im. | 17 4 55 ,9 | good enough. |

## ZOOLOGICAL SOCIETY.

We extract from an Address delivered by Mr. Children at the Anniversary Meeting of the Zoological Club of the Linnæan Society, on the 29th November last, the following outline of the progress and present prospects of this new, but flourishing, Society.

"From this short sketch of what has passed nnder our immediate observation within these walls in the course of the last twelve months, I turn to what has been doing in another quarter, to which we all look with an interest and anxiety commensurate with the importance attached to the growth and progress of that young but promising child of British energy and science, the Zoological Society. It is a glorious feature in the philosophical character of Great Britain, that whilst in foreign countries, Science owes most of her success to the fostering care of Royal patronage, or the protection of executive power,—*here*, with faint exceptions, 'few and far between,' she relies on her own resources; and, unlike the creeping parasite, raises her head in independent dignity by the individual exertions of her disinterested cultivators, who, loving her for herself, seek only to accelerate her progress, and establish her empire in the human mind on the firm basis of immutable truth. To such an origin the Zoological Society may proudly assert its claim;—not one shilling has been drawn from the public purse for its support: and could it condescend to ask such aid, I for one would raise my voice against the humiliating petition—*Absiste precando, viribus indubitare tuis.* But it has not so forgot its dignity: it has relied solely on the liberal ardour of an enlightened people, and it will still rely on it;—nor will it rely in vain. The spirit of its immortal Founder has gone forth, and will not fail to light up in every heart capable of exalted feelings, some portion of that fire which animated his own; some wish, some sacred hope of treading, with however unequal steps, in the path he has so zealously marked out for them. In saying that not one shilling has been drawn from the public purse for the support of the Zoological Society, I must not be understood as meaning to imply that therefore its welfare is a subject of indifference to the gracious Monarch who wields the sceptre of these kingdoms, or the enlightened individuals whom, in his wisdom, he has summoned to his councils. That the very reverse is the fact, has already been confirmed by the exertion of Royal munificence in favour of the Society, and by its having at its head one of of His Majesty's principal officers of state—a man, whose qualities of head and heart have rarely been equalled, never surpassed; and of whom both the Society and the British nation may honestly be proud. Such a Monarch and such a Minister will never be backward to further the interests of Science, when paramount claims shall happily cease to divert the current of national treasure into other channels, and when increasing prosperity shall relax the strict bands of public œconomy, by which their natural impulses are at present checked and circumscribed. If proof be wanting to support this assertion, we need only turn our eyes a short space northward, for indisputable evidence of the inclination of His Majesty's Government to further the views of the Zoological Society: and it is peculiarly gratifying to me to inform you, that in addition to the ground already allotted for the gardens and *vivaria*, final arrangements have been very recently completed, for the grant of the lake and its islands in the Regent's Park, for the purposes of breeding, rearing, and preserving water-fowl and other aquatic animals; and for a plot of ground for the erection of suitable offices and farm-yards, for breeding and domesticating poultry, &c. The right of *entrée* has also been granted to the Members of the Zoological Society, to the walks and ornamental grounds on the West side of the Regent's Park next to the lake;—all, privileges of essential importance to the Society, and gratifying proofs of the interest that His Majesty's Government takes in its welfare.

"As an accurate and sufficiently minute account of the valuable additions that have lately been made to the Society's Menagerie and Museum appears in the last Number of the "Zoological Journal," it would be superfluous to dwell on them in this place. I shall therefere merely state, that among the latter, stands conspicuous the extensive collection of its lamented founder, the late Sir Thomas Stamford Raffles, particularly rich in those rare animals, only lately known to science, from the eastern islands; as the male and female Proboscis Monkey (*Simia nasica*, Linn.)—a new species nearly allied to it,—the Malay Bear (*Helarctos Malayanus*, Horsf.); different species of *Tupaia*, and of the other new genera *Mydaus, Ictides, Gymnura*, &c. The Birds include most of the splendid species of Sumatra, particularly the gallinaceous fowls. Various new and interesting species are also found among the Fishes, Reptiles, Insects, and Zoophytes. Various other valuable animals have been added by the members and friends of the Society: but the most conspicuous of the late acquisitions is a fine specimen of the Ostrich, graciously presented by His Majesty. In the menagerie and gardens nearly two hundred living animals are exhibited in suitable paddocks, dens, and aviaries; as two beautiful *Llamas*, from the Duke of Bedford and Mr. Robert Barclay; a *Leopard*, the gift of Lord Auckland; *Kangaroos*, a *Russian Bear, Ratel, Ichneumons*, &c. &c.; besides a pair of *Emus, Eagles, Cranes, Gulls, Gannets, Corvorants*; various Gallinaceous Birds, and many others.

"The number of Members, whose names are inscribed in the books of the Zoological Society, amounts this day to 685."

XII. *In-*

## XII. *Intelligence and Miscellaneous Articles.*

### ISOPYRE,—A NEW MINERAL SPECIES.

THE following account of this mineral by W. Haidinger, Esq. F.R.S. E. &c. as well as the four following notices, we copy from the last number of Professor Jameson's Journal.

1. *Description.*—Regular forms not observed. Very pure masses of considerable size, often nearly two inches in every direction, occur imbedded in granite.

Cleavage none. Fracture conchoidal; highly perfect, where the mineral is pure; of lower degrees of perfection, where there are foreign admixtures in it.

Lustre vitreous, often considerable. Colour grayish-black and velvet-black, occasionally dotted with red, as in the heliotrope. Streak pale greenish-gray.

Opake, or very faintly translucent on the thinnest edges, with a dark liver-brown tint.

Brittle. Slight action on the magnetic needle.

Hardness =5·5...6·0. Specific gravity =2·912.

2. *Observations.*—Several specimens of the species of isopyre are preserved in the cabinet of Mr. Allan. Some of them are quite pure, and have no rock attached to them; others are imbedded in a kind of granite, chiefly consisting of quartz, crystals of which often penetrate the dark-coloured mass of the isopyre. Some of the specimens were procured by Mr. Allan three years ago, on a journey through Cornwall, in which I had the pleasure of accompanying him, from a miner in St. Just; others were given to Mr. Allan by Mr. Joseph Carne of Penzance, whose collection of minerals is particularly rich in the products of the western districts of Cornwall. The west of Cornwall is certainly the native country of the isopyre, but I am unable at present more accurately to indicate its locality, as I then considered the substance actually to be, what it was called, *black opal*, and, as such, much less interesting than it proved on more attentive examination, and omitted to take a note of the exact locality.

The resemblance of the isopyre to obsidian, or to what might be supposed to be the appearance of opal, when of a black colour, is very considerable; only the lustre of isopyre is less bright and glassy than that of obsidian. It is also very much like certain varieties of iron slag, and in fact it would be difficult to suspect the mineral not to be a product of the same kind of fusion which we are capable of producing in our own furnaces, if it were not associated with crystals of quartz, or did not contain, as in one of Mr. Allan's specimens, small imbedded crystals of tin-ore and of tourmaline. In allusion to this appearance, and also on account of the perfect similarity of a globule melted before the blowpipe, with the fragment employed in the experiment, I propose the trivial name of *Isopyre*, for designating the mineral, from ισος (equal) and πυρ (fire). The similarity of properties is even preserved in regard to magnetism,

netism, the globule obtained by exposing a fragment of the mineral to the blast of the blowpipe being magnetic, as well as the fragment itself, and even in a higher degree.

From the description given * of the Tachylite of Breithaupt, this mineral should much resemble the isopyre. Its specific gravity is much lower, being only 2·5. . .2·54, so as to preclude the possibility of their belonging to the same species. It occurs in basalt and wacke at Sæsebuehl, near Gœttingen, likewise only massive.

Dr. Turner has analysed the isopyre, and finds its composition to be

| | |
|---|---|
| Silica | 47·09 |
| Alumina | 13·91 |
| Peroxide of iron | 20·07 |
| Lime | 15·43 |
| Peroxide of copper | 1·94 |
| | 98·44 |

OSMELITE,—A NEW MINERAL SPECIES.

Professor Breithaupt, of Freyberg, gives the following account of this substance:

The name of this mineral is derived from οσμη (smell) and λιθος (stone). Its characters are as follows: Colour grayish-white, which passes into a tint between smoke and yellowish-gray. Planes, which have been exposed to the weather, have their colour changed into dark hair-brown. It consists of thin prismatic concretions, either scopiformly or stellularly arranged, and these again collected into coarse granular concretions, forming massive portions. Cleavage visible only in one direction, owing to the thinness of the prismatic concretions, which indeed pass into fibrous. Its form is conjectured to be rhomboidal; is strongly translucent: it feels rather greasy: its hardness, owing to the fibrous structure, is difficult to determine. It appears, however, from some trials on the file, to be intermediate between that of fluor-spar and apatite. Specific gravity = 2·792 to 2·833.

It gives out, in the common temperature of a room, a distinct clayey smell, which is increased by breathing on it, or when brought from a warm to a cold place. In the mouth it tastes like clay, and appears as if it would dissolve like clay, although no change takes place.

This species is distinguished from the zeolites by its greater specific gravity. It approaches to tabular spar in hardness and specific gravity, but in no other characters.

It occurs superimposed on calcareous spar, mixed with datolite, —in veins in trachyte, in a hill at Niederkirchen, near Wolfstein, on the Rhine.

HYDROSILICITE,—A NEW MINERAL SPECIES.

Dr. Kuh, in his inaugural discourse, entitled "*De Hydrosilicite, nova fossilium specie*, Berlin, 1826," informs us, that he found, in the

* Leonhard, 2d edit. p. 781.

serpentine

serpentine of Frankenberg in Silesia, along with chrysoprase, opal, and pimelite, a mineral which he names Hydrosilicite. It is white, without lustre, feels greasy, translucent, fracture even, soft, does not adhere to the tongue, amorphous, and appears to be almost entirely composed of pure silica and water.

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RUSSIAN PLATINA-SAND.

Professor Breithaupt has given the annexed mineralogical examination of this substance.

I was favoured by M. Schwetzau with a quantity of the platina-sand, washed out of the sand of Nijniotaguilsk, in the government of Perme, in Siberia. Of this Siberian sand there are two kinds: the one is ferriferous, and contains platina; the other, which is purer and more quartzy, afforded principally remarkably fine wash-gold.

The platina-sand, even at first glance, appears composed of grains of different kinds. I separated, by the eye, the following minerals: 1. *Platina*. 2. *Gold*. 3. *Irid-osmine*. 4. *Silver-white flat grains*. 5. *Iserine, or magnetic iron-sand*.

The grains, from their appearance, could not have rolled far, and must have been found at no great distance from the place of their origin, for many of them are very sharp-edged, or even bristled with points.

1. *Platina-grains*.—I attempted to separate these from the iserine grains, by means of the magnet; but was surprised to find that not only the iserine, but also many of the platina-grains, adhered to it. I found that some of the platina-grains were magnetic, others not: hence these two kinds are probably varieties of two distinct species.

First species: *Common Platina*.—It is the same with the platina brought from America by Humboldt, and possesses the following characters:

Colour *platina-gray*, which is different from steel-gray. On concave places there is observed a yellowish appearance.—The grains are angular and bristled, seldom blunt-edged; the crystals are hexahedral, and grouped, as in silver-glance. Hardness =70·—8·5*. Is perfectly malleable. Specific gravity 17·001—17·608. A large American specimen in the Wernerian cabinet was 16·914. It is well known that the native platina is always lighter than that prepared by chemical means.

Second species: *Ferruginous Platina*.—The colour is *platina-gray*, but darker than in the preceding species. In hollows in the specimens, the surface is tarnished, from dark brown to black, as in meteoric iron. The grains and crystals have the same forms as in the former species.—Hardness =8·0—8·5. Malleable, but not so completely so as in the first species. Specific gravity 14·666—15·790. It is magnetic, and in some grains not only repels, but also attracts. It is distinguished from the former species by lower

* Scale of hardness here used is that of Breithaupt, in his Mineralogy, 7 = that of glassy actynolite, 8 = that of adularia, 9 = quartz.

specific

specific gravity, less perfect malleability, and its affording, by chemical trials, a considerable portion of iron.

2. *Gold.*—I found few grains of gold in the platina-sand: these were partly *gold-yellow*, partly *grayish-yellow*. Is Werner's grayish-yellow gold, gold combined with platina?

3. *Irid-osmin.*—This species, which is a compound of iridium and osmium, presents the following characters:

The colour is not steel-gray, as is generally believed, but a middle colour between whitish lead-gray and common lead-gray. It occurs crystallized in low hexagonal prisms, which have an axotomous cleavage. Hardness =8·0—8·75. Is imperfectly malleable. Specific gravity =17·969—18·571.

It would be desirable to have iridium and osmium again examined. Iridium will probably be found to possess a higher specific gravity than platina, and probably belong to the tessular system. The osmium, on the contrary, appears to belong to the electro-negative metals, which possess a hexagonal crystallization, such as arsenic, tellurium, and antimony.

4. *Silver-white flat grains.*—They appear to be palladium.

*Concluding Remark.*—In the portion of platina-sand I examined, the large half was ferruginous platina, the smaller common or true platina. The remaining grains composed about $\frac{1}{200}$th part of the whole.

In the Phil. Mag. and Annals for November, we gave a notice of Professor Ossann's having discovered various new metals in the Uralean platina. From the following account, by the same Professor, it appears that there are several varieties of it.

The platina, from ore of the Urals, is more varied in character than that found in America.—I have already been enabled to distinguish four different sorts, and I am told there are still more. One of the kinds, that which is most abundant, is sold at the mint in Petersburg. It consists of grains of different descriptions. Small grains can be separated by means of the magnet, resembling the magnetic grains in the platina of Brazil. The other grains are partly of a lighter and darker lead-gray colour, and about a line in diameter,—partly of a gold-yellow colour; and some are small, flattish, and shining metallic. In the following analysis I used the bluish-gray coloured grains. The following results were obtained in soluble matter:

| | |
|---|---|
| Palladium | 1·64 |
| Rhodium | 11·07 |
| Platina | 80·87 |
| Copper | 2·05 |
| Iron | 2·30 |
| Sulphur | 0·79 |
| Trace of Iridium. | |
| Residuum | 0·11 |
| | 98·83 |

*Poggendorf's Journal.*

## NEW PHÆNOMENA OF VAPOUR OBSERVED BY CLEMENT DESORMES.

This philosopher communicated, on the 4th of December, to the Royal Academy of Sciences, some singular results relative to steam. "When compressed in a boiler, and issuing in a violent and hissing jet, through an orifice made in a pretty large plate, if a flat disk of metal be presented to it, at a little distance from the orifice, the disk is strongly repelled; but if it be brought near and placed against the plate, as if to close the orifice, although the steam issues on all sides like artificial fire-works, and presses against the disk more than before, not only is the disk not driven away, but it adheres to the plate even when the jet is directed downwards. It remains suspended in opposition to its gravity, and can be detached only by force. The same result takes place in an experiment with the wind which issues from the large bellows of a furnace."

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## NOTICE OF A FIRE-BALL:—BY THE REV. S. E. DWIGHT.

This meteor appeared on Saturday evening, March 21, 1813, a little before ten o'clock. The sky was extensively overcast, yet the covering was every where thin; and in the North where the meteor appeared, in various tracts of considerable extent, the stars were in full view. I was standing on a platform on the north side of the house, where I could survey the whole tract of sky over which the meteor passed. When the light first broke upon me, I was looking eastward, and for a moment supposed it to be a flash of very vivid lightning; but from its continuance was led almost instantly to look to the luminary whence it proceeded. The following are the observations which I made at the time with regard to it.

1. The meteor, when I first saw it, was about 35° above the horizon; and from the course of the fence near which I stood, I judged its direction, at that time, to be about N. 20° E.

2. Its figure was nearly that of an ellipse, with the ends in a slight degree sharpened or angular.

3. The length of the transverse diameter appeared to be about equal to the apparent diameter of the moon when on the meridian; and that of the conjugate, about three-fourths of the transverse.

4. The colour of the body resembled that of the moon, but was evidently more yellow.

5. A trail of light was formed behind it of considerable length, perhaps of ten or twelve degrees. It was broadest near the body, and decreased in breadth very slowly for about two-fifths of its length; after which it was an uniform stripe of light, about as wide as the apparent diameter of the planet Venus. The direction of the tail was coincident with that of the transverse diameter.

6. The ball was much more luminous than the tail, so that the end of the ball connected with the tail was scarcely less distinct in its form than the opposite end.

7. The illumination was so powerful, that all the objects around me

me cast distinct shadows, though less strongly marked than when the moon is at the full.

8. Numerous sparks, of the apparent size of the smaller stars, but much more brilliant, were continually issuing from the ball of the meteor, and after descending a little distance, soon disappeared.

9. The length of time, in which the body was visible, was about eight, or possibly ten seconds.

10. A short time before its disappearance,—say one or two seconds,—three much larger sparks, or luminous fragments, were thrown from the body at the same moment. Two of these were apparently as large as the planet Venus; the third was still larger. These three were the last pieces which I saw leave the body. Their paths were at first nearly parallel with that of the meteor, yet beneath it. From this direction, however, they all deviated constantly and rapidly in parabolic curves, until they seemed falling perpendicularly towards the earth. Each fragment became less and less distinct, until it disappeared. The largest of the three continued visible until it was within about 20 degrees of the horizon.

11. The meteor itself disappeared as suddenly as if, in one indivisible moment, it had passed into a medium absolutely opaque, or as if, at a given moment, it had left the atmosphere; but a few moments afterwards there was a distinct and somewhat extensive illumination over that part of the sky for about a second, as if the light of the departing luminary had been reflected from some unknown surface to the earth.

12. When the meteor disappeared, it was about 30° above the horizon, and, as I judged from the course of the fence, in the direction of N. 45° E., or 25° eastward of the place where I first saw it. I concluded that the direction of its path was probably from W. by S. to E. by N. It was obviously going from me; its path making an angle with the optic axis of about 60°.

13. Not less than eight minutes, nor more than ten, after the disappearance of the meteor, there was a report very loud and heavy, accompanied with a very sensible jar. Though mistaken for thunder by those who did not see the meteor, it did not much resemble either thunder or the report of a cannon; but was louder, shorter and sharper than either, and was followed by no perceptible echo.

14. A friend of mine, who was in Berlin at the time, about 23 miles due N. of New Haven, saw the meteor distinctly, but made no particular observations concerning it. His estimate of it accorded generally with mine, but it appeared to him larger, more elevated, and somewhat more to the East in its apparent place.—I could not learn that the fragments which fell from it were discovered.—*Silliman's Journal*, vol. xiii. p. 35.

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## ON THE AURORA BOREALIS OF 26TH SEPT.: BY DR. FORSTER.

Boreham, Nov. 25, 1827.

Having seen the remarkable aurora which occurred on the 26th of September last, slightly mentioned in several of the journals, but no where accurately described, I send you the following brief notes on this

this remarkable phænomenon, which I made during its continuance through the night of the 25th, as it was seen by me and by others from the neighbourhood of Chelmsford in Essex.—I was passing the night at Boreham, and about nine o'clock P.M. I first noticed a very remarkable light in the North, the sky became intensely red in the N.N.E. and N., and afterwards very light all over the northern hemisphere. Clouds, however, soon obscured it for a time, and I returned to the house. At midnight on looking out of window, towards New Hall, I noticed the sky to be remarkably luminous, and on going out perceived every object as clearly as on a bright moonlight night. In the North the most brilliant streams of light shot up into the zenith from the horizon, generally in irregular diverging radii, whose centre appeared to be at nearly N.N.W. by N.; but other streamers of white light in other directions becoming alternately brilliant and faint appeared, while in E.N.E. and N.E. the whole sky seemed of a bright yellowish light: the red colour which I perceived at nine o'clock had quite disappeared, and the white light continued to prevail till it was lost in the day-light of the following morning. It is recorded that on two former occasions, one about twenty years since, and the other early in the eighteenth century, bright auroras happened on the same day; namely, 25th of September. I should like to have authenticated particulars of this fact, as it would establish a most remarkable coincidence of dates with the occurrence of this rare and beautiful phænomenon.

With regard to the aurora of the last 25th of September above described, I may observe, that I never saw distant objects so clearly on the brightest moonlight night as I did on that occasion at midnight; but its light more resembled that of the break of day than of the moon, and hence the name of Aurora has not unaptly been applied to this meteor. T. Forster.

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## LIST OF NEW PATENTS.

To R. Wheeler, of High Wycomb, for improvements on refrigerators for cooling fluids.—Dated the 22nd of November 1827.—6 months allowed to enrol specification.

To W. J. Dowding, of Poulshot, Wiltshire, for improvements in machinery for rollering wool from the carding-engine.—22nd of November.—2 months.

To J. Roberts, of Wood-street, Cheapside, and G. Upton, of Queen-street, Cheapside, for improvements on Argand and other lamps.—24th of November.—6 months.

To J. A. Fulton, of Lawrence Pountney-lane, Cannon-street, London, for a process of preparing or bleaching pepper.—26th of November.—6 months.

To J. Apsey, of John-street, Waterloo-road, Lambeth, for an improvement in machinery to be used as a substitute for the crank.—27th of November.—2 months.

To J. Jenour, jun., of Brighton-street, Pancras, for his cartridge or case, and method of more advantageously inclosing therein shot or other missiles for loading fire-arms.—28th of November.—6 months.

To

To T. Bonnor, of Monkwearmouth Shore, Durham, merchant, for improvements on safety-lamps.—4th of December.—6 months.

To W. Fawcett, of Liverpool, and M. Clarke, of Jamaica, for improved apparatus for the better manufacture of sugar from the canes. —4th of December.—6 months.

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## SCIENTIFIC BOOKS, &c.

A small work, entitled "The Circle of the Seasons," has just been published by Mr. T. Hookham, of Bond-street, in which the average day of flowering of most of our common garden plants throughout the year is noticed, under its respective day; and a short account of other phænomena is added, with some popular observations on the weather, &c. &c. The work is in one small volume 12mo., and intended for a daily companion to the popular botanist out of doors.

Polariscope,—an instrument for observing some of the most interesting phænomena of polarized light. Made and sold by W. Cary, Strand. The principle of this instrument was, we believe, first discovered and described by Biot, and its form we observe is nearly the same as one which has been described by Herschel. We conceive that Mr. Cary has rendered an acceptable service to science by making these instruments for sale, as it will place the important and interesting subject of polarized light within the reach of a greater number of inquirers than would otherwise have had an opportunity of studying it.

A Tabular View of Volcanic Phænomena, comprising a list of the burning mountains that have been noticed at any time since the commencement of historical records, &c. &c. By Charles Daubeny, M.D.F.R.S. Professor of Chemistry in the University of Oxford. This is an extremely useful accompaniment to the author's work on Volcanos. It consists of three parts: 1st, A list of the countries in which volcanos occur; 2ndly, A chronological list of volcanic phænomena; 3rdly, A view of the comparative heights of volcanic mountains. The just estimation in which Dr. Daubeny's work is held, is of itself a sufficient recommendation of the present tabular view, which in addition to the information already mentioned, contains an account of the geological nature of the various volcanic mountains.

A new weekly Medical Journal, under the title of "The London Medical Gazette," being a journal of medicine and collateral sciences, was published on Saturday, December 8.

A short series of Popular Lectures on the Steam-engine, by Dr. Lardner, Professor of Natural Philosophy in the new University, is announced for publication in a few days. The author professes to have stripped the subject of all its technicalities, and to have presented it in such a form that readers totally unacquainted with mechanical science may readily comprehend the construction and operation of the steam-engine, as well as the most interesting circumstances connected with the history of its invention and progressive improvement.

METEORO-

## METEOROLOGICAL OBSERVATIONS FOR NOVEMBER 1827.

### *Gosport.—Numerical Results for the Month.*

Barom. Max. 30·40 Nov. 5. Wind NW.—Min. 29·28. Nov. 29. Wind N.W.
Range of the mercury 1·12.
Mean barometrical pressure for the month . . . . . . . . . 29·964
——— for the lunar period ending the 18th instant . . . . . . 29·872
——— for 14 days with the moon in North declination . . . 30·057
——— for 15 days with the moon in South declination . . . . 29·687
Spaces described by the rising and falling of the mercury . . . 6·790
Greatest variation in 24 hours 0·730.—Number of changes 19.
Therm. Max. 62° Nov. 13. Wind N.—Min. 27° Nov. 22. Wind N.
Range 35°.—Mean temp. of exter. air 48°·42. For 30 days with ⊙ in ♏ 50·60
Max. var. in 24 hours 19°·00—Mean temp. of spring water at 8 A.M. 54°·82

### De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air on five different days . . . . . . . . . . 100°
Greatest dryness of the air in the afternoon of the 2nd . . . . 51
Range of the index . . . . . . . . . . . . . . . . . . . . 49
Mean at 2 P.M. 70°·0—Mean at 8 A.M. 79°·2—Mean at 8 P.M. 81·2
—— of three observations each day at 8, 2, and 8 o'clock . . 76·8
Evaporation for the month 0·70 inch.
Rain near ground 1·835 inch.—Rain 23 feet high 1·690 inch.
Prevailing Wind N.W.

### *Summary of the Weather.*

A clear sky, 1½; fine, with various modifications of clouds, 9; an overcast sky without rain, 14; foggy 1½; rain, 4.—Total 30 days.

### *Clouds.*

| Cirrus. | Cirrocumulus. | Cirrostratus. | Stratus. | Cumulus. | Cumulostr. | Nimbus. |
|---|---|---|---|---|---|---|
| 15 | 10 | 27 | 1 | 15 | 19 | 12 |

### *Scale of the prevailing Winds.*

| N. | N.E. | E. | S.E. | S. | S.W. | W. | N.W. | Days. |
|---|---|---|---|---|---|---|---|---|
| 6 | 3½ | 3 | 2 | 4 | 1 | 2½ | 8 | 30 |

*General Observations.*—This month has been calm and dry, excepting four or five days, and very mild for the season till the 20th; afterward it was mostly cold, foggy, and an extremely damp air.

At 20 minutes part 8 P.M. on the 4th instant, two coloured *paraselenæ* appeared, one on each side of, and 22° 39′ distant from, the moon's centre, as determined by the star marked ι (Iota) in the left heel of the constellation Auriga, which star was conspicuous through, and apparently centered in the paraselena on the northern side of the moon. These mock-moons were formed in a turbid-looking broad band of *cirrostratus* as it slowly passed the moon's horizontal rays.

In the afternoon of the 9th there was a curious appearance of nascent *cumuli* in a long connected range between Gosport and Portsdown Hill; they had the appearance of whitish columns of steam moving eastward near the surface of the earth with a gentle wind from N.W.

On the 11th, 18th, 19th, 25th, and 26th, the index of the hygrometer advanced to the extreme moisture point, or 100 degrees, the whalebone on these days being expanded to its utmost extent, by the deposition of dew and thick haze thereon. The dew deposited in the rain-gauge in the nights of the 17th and 18th amounted to *four hundredth* of an inch in depth.

From 11 till 12 P.M. on the 18th, a faint aurora borealis appeared in the horizon between the N.W. by N. and N. by E. points. A mild lemon-coloured

coloured light, and the segment which it described above the horizon, indicated the phænomenon, whose altitude did not exceed five degrees; nor was it accompanied by any perceptible coruscations during the hour of observation.

In the mornings of the 22nd and 23rd the ice lay pretty thick on the ground, and icy efflorescences appeared on the inside of the glass windows (being the first time this autumn), which were succeeded by a frosty haze on the latter day, and a light shower of snow in the evening. This was the earliest fall of snow observed here during the last twelve years. Several showers of snow also fell in Northumberland on the 21st, and much more fell between London and Manchester on the 22nd and 23rd.

The mean temperature of the atmosphere for November varies considerably in different years, sometimes to ten or twelve degrees. Sudden changes from a temperate air to several degrees below the freezing point, as was the case the latter part of this month, are found so powerful on some delicate constitutions, as to cause a general languor and a painful sensation of the flesh, which are mostly experienced at the setting in of piercing northerly and easterly winds immediately after mild currents of air from the opposite points of the compass. By some attention to this subject, it may be ascertained that colds and their calamitous train of consequences at the close of autumn and beginning of winter are more frequently produced by sitting in wet clothes and shoes, or in a brisk current of air when warm; or by going out of a hot room for any length of time in the dewy or foggy night air,—circumstances which seldom fail in obstructing the insensible perspiration of the body, than by a uniform decrease of temperature, however low, with the precaution of wearing suitable winter clothing.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are two *paraselenæ*, three lunar halos, an aurora borealis, two meteors, lightning in the evening of the 8th, and three gales of wind, two from the South, and one from the West.

REMARKS.

*London.*—Nov. 1. Fine day: some rain in the evening. 2, 3. Fine. 4. Foggy morning: fine day. 5. Cloudy. 6. Fine. 7, 8. Cloudy. 9. Drizzly. 10. Cloudy. 11, 12. Fine. 13—15. Cloudy. 16. Rainy. 17. Cloudy. 18. Foggy morning. 19. Cloudy. 20. Gloomy: cloudy. 21. An extraordinarily high tide this morning about 4 o'clock, doing very considerable damage: day fine. 22. Fine: a fall of snow about 4 P.M. covering the ground generally. 23. Cloudy: clear night. 24. A considerable fall of snow during the night. 25. Gloomy: calm: a gradual thaw. 26. Gloomy. 27. Fine. 28. White frost: fine, having rain in the night. 29. Fine. 30. Cloudy.

*Penzance.*—Nov. 1, 2. Fair. 3—5. Fair: showers. 6—9. Misty. 10. Fair. 11. Fair: rain. 12. Fair. 13, 14. Misty. 15—17. Rain. 18. Fair. 19, 20. Clear. 21. Fair: hail-showers. 22. Hail-showers. 23. Rain. 24. Showers. 25. Misty. 26. Fair. 27, 28. Rain. 29. Fair. 30. Misty. —Rain-gauge ground level.

*Boston.*—Nov. 1. Fine. 2. Stormy. 3, 4. Cloudy. 5. Fine. 6. Rain. 7. Cloudy: rain, A.M. 8, 9. Cloudy. 10—12. Fine. 13, 14. Cloudy. 15. Foggy. 16, 17. Cloudy. 18. Cloudy: rain A.M. 19, 20. Cloudy. 21, 22. Fine: plenty of ice for the first time this season. 23, 24. Fine: snow, P.M. 25, 26. Cloudy. 27. Foggy. 28. Fine. 29. Rain. 30. Cloudy.

RESULTS.

Winds, N. 2: NE. 1: E. 1: SE. 3: SW. 2: W. 3: NW. 13: var. 1.

| | |
|---|---|
| *London.*—Barometer: Mean height for the month ........ | 30·154 inch. |
| Thermometer: Mean height for the month ................. | 43° |
| Evaporation ........................................................ | ·83 inch. |
| Rain .................................................................. | 1·32. |

*Meteoro-*

*Meteorological Observations by Mr. Howard near London, Mr. Giddy at Penzance, Dr. Burney at Gosport, and Mr. Veall at Boston.*

| Days of Month, 1827. | Barometer. | | | | | | | Thermometer. | | | | | | | Wind. | | | | Evapor. | | Rain. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | London. | | Penzance. | | Gosport. | | Boston | London. | | Penzance. | | Gosport. | | Bost. | Lond. | Penz. | Gosp. | Bost. | Lond. | Gosp. | Lond. | Penz. | Gosp. | Bost. |
| | Max. | Min. | Max. | Min. | Max. | Min. | 8½ A.M. | Max. | Min. | Max. | Min. | Max. | Min. | 8½ A.M. | | | | | | | | | | |
| Nov. 1 | 30·01 | 29·99 | 30·00 | 30·00 | 30·03 | 30·01 | 29·57 | 52 | 45 | 54 | 46 | 49 | 44 | 38 | NW. | NW. | N. | NW. | ... | ... | ·03 | ... | ... | ... |
| 2 | 30·29 | 30·01 | 30·00 | 29·98 | 30·10 | 29·83 | 29·40 | 51 | 34 | 55 | 46 | 55 | 38 | 47 | NW. | NW. | NW. | NW. | ... | ... | ... | ... | ... | ... |
| ○ 3 | 30·36 | 30·29 | 30·10 | 30·06 | 30·10 | 30·10 | 29·70 | 53 | 43 | 54 | 50 | 57 | 47 | 43·5 | W. | W. | W. | calm | ... | 0·10 | ... | ... | ... | ... |
| 4 | 30·50 | 30·36 | 30·18 | 30·15 | 30·25 | 30·17 | 29·72 | 54 | 45 | 55 | 49 | 58 | 46 | 48·5 | W. | W. | W. | calm | ... | ... | ... | ... | ... | ... |
| 5 | 30·50 | 30·46 | 30·25 | 30·20 | 30·40 | 30·32 | 29·77 | 56 | 43 | 55 | 50 | 60 | 51 | 52 | W. | W. | NW. | W. | ... | ... | ·04 | ... | ... | ... |
| 6 | 30·46 | 30·36 | 30·30 | 30·30 | 30·34 | 30·26 | 29·75 | 53 | 48 | 56 | 50 | 58 | 47 | 47 | NW. | W. | NW. | calm | ... | ·10 | ... | ... | ... | 0·12 |
| 7 | 30·36 | 30·27 | 30·16 | 30·04 | 30·18 | 30·13 | 29·77 | 53 | 42 | 55 | 49 | 60 | 49 | 46 | NE. | W. | NW. | E. | ... | ... | ·03 | ... | ... | ·17 |
| 8 | 30·27 | 30·07 | 29·92 | 29·86 | 30·07 | 30·06 | 29·70 | 48 | 44 | 54 | 52 | 55 | 49 | 43 | N. | S. | NW. | calm | ... | ... | ... | ... | 0·015 | ... |
| 9 | 30·10 | 30·07 | 29·76 | 29·72 | 29·86 | 29·78 | 29·46 | 49 | 43 | 55 | 52 | 59 | 48 | 46 | SE. | SE. | NW. | calm | ... | ·10 | ·02 | ... | ·025 | ... |
| 10 | 30·10 | 30·09 | 30·02 | 30·00 | 30·02 | 29·90 | 29·50 | 53 | 47 | 55 | 51 | 57 | 49 | 48·5 | NW. | NW. | NW. | N. | ... | ... | ... | ... | ... | ... |
| ☾ 11 | 30·15 | 30·09 | 30·10 | 30·08 | 30·00 | 29·95 | 29·45 | 57 | 32 | 55 | 49 | 58 | 42 | 53 | NW. | W. | NW. | calm | ... | ... | ... | ... | ... | ... |
| 12 | 30·31 | 30·15 | 30·16 | 30·12 | 30·17 | 30·11 | 29·75 | 58 | 43 | 55 | 53 | 54 | 46 | 39 | NW. | NW. | N. | NW. | ... | ·04 | ... | 0·540 | ... | ... |
| 13 | 30·31 | 30·27 | 30·20 | 30·16 | 30·12 | 30·11 | 29·74 | 60 | 48 | 55 | 54 | 62 | 51 | 54 | NW. | NW. | N. | W. | ... | ... | ·14 | ... | ·025 | ... |
| 14 | 30·27 | 29·98 | 30·04 | 30·00 | 30·06 | 29·94 | 29·66 | 57 | 33 | 54 | 51 | 53 | 47 | 48 | E. | NW. | E. | SE. | ... | ... | ·18 | ... | ·300 | ·11 |
| 15 | 29·98 | 29·67 | 29·92 | 29·54 | 29·74 | 29·51 | 29·36 | 48 | 36 | 53 | 47 | 52 | 49 | 42·5 | SW. | W. | N. | calm | ... | ·06 | ·03 | ... | ·400 | ·25 |
| 16 | 29·99 | 29·67 | 29·46 | 29·40 | 29·62 | 29·34 | 29·13 | 47 | 33 | 49 | 42 | 54 | 46 | 46 | SE. | SE. | S. | E. | ... | ... | ·12 | 0·085 | ·085 | ... |
| 17 | 30·24 | 28·99 | 29·70 | 29·56 | 29·84 | 29·70 | 29·46 | 53 | 35 | 55 | 45 | 56 | 48 | 40·5 | SE. | SE. | SE. | calm | ... | ... | ... | 0·350 | ... | ... |
| 18 | 30·40 | 30·24 | 30·00 | 29·90 | 30·10 | 29·97 | 29·70 | 50 | 46 | 56 | 47 | 57 | 48 | 46 | N. | SE. | E. | calm | ... | ·10 | ... | ... | ... | ·15 |
| ● 19 | 30·40 | 30·35 | 30·08 | 30·04 | 30·17 | 30·16 | 29·86 | 47 | 45 | 56 | 54 | 54 | 47 | 47·5 | SE. | E. | NE. | calm | ·47 | ... | ... | ... | ... | ... |
| 20 | 30·37 | 30·35 | 30·10 | 30·10 | 30·14 | 30·05 | 29·70 | 47 | 40 | 54 | 50 | 51 | 44 | 46·5 | SW. | E. | N. | calm | ... | ... | ... | ... | ... | ... |
| 21 | 30·37 | 30·10 | 30·08 | 30·06 | 30·12 | 30·07 | 29·80 | 41 | 24 | 49 | 49 | 44 | 29 | 35 | NW. | NE. | N. | NW. | ... | ·05 | ... | ... | ... | ... |
| 22 | 30·10 | 29·92 | 30·00 | 29·96 | 29·92 | 29·73 | 29·54 | 36 | 19 | 45 | 33 | 42 | 27 | 28·5 | NW. | NE. | N. | NW. | ... | ... | ... | ... | ... | ... |
| 23 | 30·30 | 29·84 | 29·74 | 29·54 | 29·68 | 29·56 | 29·55 | 31 | 20 | 50 | 38 | 40 | 30 | 28 | var. | NW. | N. | NW. | ... | ... | ... | ... | ... | ... |
| 24 | 30·20 | 29·84 | 29·90 | 29·86 | 29·97 | 29·79 | 29·55 | 34 | 24 | 46 | 40 | 38 | 30 | 28·5 | NW. | NW. | NE. | NW. | ... | ·05 | ... | ... | ·200 | ... |
| ☽ 25 | 30·48 | 30·20 | 30·04 | 30·04 | 30·02 | 29·92 | 29·70 | 37 | 36 | 50 | 40 | 46 | 42 | 34 | S. | NW. | NE. | calm | ... | ... | ... | 0·760 | ... | ... |
| 26 | 30·55 | 30·48 | 30·22 | 30·20 | 30·32 | 30·20 | 29·96 | 48 | 25 | 50 | 46 | 48 | 39 | 34·5 | NW. | NW. | NE. | calm | ... | ... | ... | ... | ... | ... |
| 27 | 30·55 | 30·25 | 30·22 | 30·12 | 30·30 | 30·18 | 29·96 | 41 | 28 | 53 | 47 | 51 | 42 | 40·5 | NW. | S. | S. | calm | ... | ·05 | ... | ... | ... | ... |
| 28 | 30·25 | 29·59 | 30·00 | 29·50 | 30·01 | 29·65 | 29·67 | 41 | 36 | 54 | 48 | 51 | 46 | 35 | S. | SW. | S. | E. | ... | ... | ·60 | ... | ·610 | ... |
| 29 | 29·69 | 29·59 | 29·54 | 29·50 | 29·50 | 29·28 | 29·10 | 50 | 36 | 55 | 48 | 53 | 47 | 41·5 | NW. | W. | NW. | SE. | ... | ... | ... | 1·430 | ... | ·59 |
| 30 | 29·59 | 29·27 | 29·52 | 29·44 | 29·48 | 29·44 | 29·20 | 54 | 48 | 56 | 49 | 56 | 49 | 41·5 | S. | SW. | SE. | calm | ·36 | ·05 | ·13 | ... | ·175 | ... |
| Aver.: | 30·55 | 29·27 | 30·30 | 29·40 | 30·40 | 29·28 | 29·60 | 60 | 19 | 56 | 33 | 62 | 27 | 42·3 | | | | | ·83 | 0·70 | 1·32 | 3·165 | 1·835 | 1·39 |

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

*FEBRUARY* 1828.

## XIII. *On the mutual Decomposition of Sulphate of Zinc and Chromate of Potash. By* T. Thomson, *M.D. F.R.S. L. & E. Professor of Chemistry in the University of Glasgow.*

*To the Editors of the Philosophical Magazine and Annals.*

Gentlemen,

IN the last Number of your Journal (for December 1827) I observed a paper by Mr. Henry Stokes "On some new double chromates." The three compound salts which he describes as double chromates, in fact contain merely a small quantity of chromate of potash, or rather of chromic acid in a state of mechanical mixture. They are the three salts which I have described in my First Principles of Chemistry, vol. ii. under the names of Potash-sulphate of zinc, p. 435,
Potash-sulphate of nickel, p. 434,
Potash-sulphate of copper, p. 436,
as any one may satisfy himself by comparing my descriptions and analyses of these salts with those of Mr. Stokes.

I shall take this opportunity of rectifying the account which I have given of chromate of zinc in my First Principles of Chemistry, vol. ii. p. 357. The salt which I describe as a chromate of zinc is in reality a dichromate of zinc, for it contains exactly twice as much oxide of zinc as I have assigned it. This mistake I discovered soon after the publication of my *First Principles*. And it induced me to examine carefully what takes place when solutions of sulphate of zinc and chromate of potash are mixed in the atomic proportions. The result is curious, and will be sufficiently understood by the following statement. If we mix together solutions containing

16 atoms chromate of potash ......= 200
16 atoms sulphate of zinc ......... = 164——364.

 There

There will be deposited

| | | | |
|---|---|---|---|
| | | 6 atoms dichromate of zinc......... | = 102 |
| By carefully concentrating the mother liquor we obtain | 1st, | 5 atoms bichromate of potash... | = 95 |
| | 2nd, | 4 atoms potash-sulphate of zinc. | = 85 |
| | 3rd, | 6 atoms sulphate of potash...... | = 66 |
| | 4th, | 1 atom bisulphate of potash.... | = 16 |
| | | Total.... | 364 |

All these salts are conceived to be anhydrous, merely to simplify the numbers.

The practical chemist will easily understand how I proceeded. I evaporated the liquid as carefully as possible, picking out the crystals as they formed, and examining them. I easily recognized bichromate of potash, potash-sulphate of zinc, and sulphate of potash, by their forms. I did not perceive any crystals of bisulphate of potash, I merely inferred the formation of this salt from the impossibility of accounting in any other way for the formation of the other four salts from the atoms which I had mixed, unless the formation of that salt also be admitted. The reader will not suppose that I succeeded in separating the whole of these four salts from each other, I merely endeavoured to determine the nature of all the salts formed. The rest was the result of an obvious calculation.

From Mr. Stokes's experiments it would appear that similar salts are formed when sulphate of nickel or sulphate of copper is mixed with chromate of potash in the atomic proportions. This I have not hitherto had occasion to investigate. But it will probably turn out that my chromate of nickel and chromate of copper are in fact, dichromates. This I shall endeavour to ascertain as soon as I have leisure.

I am, &c.

Dec. 14, 1827. THOMAS THOMSON.

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## XIV. *On the Measurement by Trigonometry of the Heights of the principal Hills in the Vicinity of Dent, Hawes, and Sedbergh, in Yorkshire.* By JOHN NIXON, *Esq.**

THREE years ago I commenced, and in the course of the present summer I succeeded in completing, the trigonometrical measurement of the heights of a group of hills of the *Penine* chain. Different observers have generally furnished from similar operations results so extremely discordant, that it

* Communicated by the Author.

it becomes necessary, in order to inspire confidence, to give every needful particular respecting the bases, signals, instruments, and methods of calculation.

*Bases.*—From the third volume of the Trigonometrical Survey of England and Wales, are extracted the following distances between stations, &c. placed on hills forming part of the above group.

| | | |
|---|---|---|
| Ingleborough to the Calf | . . . . . . . | 77,615 feet. |
| Ditto | Shunnor Fell . . | 82,397 |
| Ditto | Pen-y-gent Rock | 32,124 |
| Ditto | Whernside . . . | 22,435 |
| Shunnor Fell to the Calf | . . . . . . . | 59,487 |

On the Calf were found the stakes which had supported the theodolite used in the Ordnance Survey. Shunnor Fell was not visited, but the signal now on its summit has most probably been erected over the proper station-mark. In the centre of the pile of stones yet remaining on the highest point of Whernside, there stood some years ago a signal-staff known to have been erected by Colonel Mudge's party. The rock on Pen-y-gent now cut up, and forming part of the boundary wall of Horton, was not situated exactly on the summit of the fell. The station on Ingleborough could not be satisfactorily identified, but the highest point of the mountain corresponds very nearly with it in its bearing and distance from the old hut, as given in the account of the survey. Assuming as a base for the calculations the distance of the *summit* of Ingleborough to that of Pen-y-gent, which we shall call 32,160 feet, the errors of such of the distances as are comparable with those of the Ordnance Survey will be;

| | | |
|---|---|---|
| Ingleborough to the Calf | . . . . . . . | +32 feet. |
| Ditto | Shunnor Fell . . | −22 |
| Ditto | Whernside . . . | + 6 |
| Shunnor Fell to the Calf | . . . . . . . | +18 |

*Signals.*—Where stones were at hand, small towers five to eight feet high with bases six to nine feet in diameter were erected. When the summit of the hill was destitute of rocks, the (pyramidal) signals were formed of large sods. These signals were invariably placed on the loftiest point of the hill, determined in doubtful cases by resorting to a well-adjusted twelve-inch telescopic level.

*Description of the Theodolite.*—With the exception of a few observations made by a common four-inch theodolite, the horizontal angles were measured by a six-inch theodolite, constructed in the autumn of 1826, by Mr. P. Lealand, London.

 The

The excellent twenty-inch telescope of this instrument is furnished with cylindrical rings, a delicate spirit-level, and very fine adjustable (steel?) cross-wires.—It is supported in the Ys of a cradle (common to theodolites of this description) having a horizontal axis four inches in length. A clamp and tangent screw, fitted to one of the supports of this axis, serve to give a slow vertical motion to the telescope*. The lower of the six-inch horizontal circles is divided (on silver) into thirds of a degree, capable of being read off to half-minutes by means of the two opposite verniers of the upper circle and a pair of double magnifiers. The latter are very conveniently fixed to a ring moveable about the rim of the under circle. The upper circle is also provided with a clamp and tangent screw for slow horizontal motion, and has a couple of spirit-levels placed at right-angles to each other, which with the four screws of the parallel plates enable the observer to render the plane of contact of the two circles parallel to the horizon. On the upper parallel plate is fixed a third clamp and tangent screw, intended, no doubt, to bring the cross-wires of the telescope, the circles being clamped together, with some particular degree coincident with the zero of one of the verniers, exactly on the observed object.

When the ground at the station proved impenetrable, heavy stones were piled against the feet of the tripod (set up with its legs at an angle of 45° to the horizon), in such a manner as to render it fixed and inflexible. The summit of the hill being swampy, the legs (set up more erect) were forced below the surface of the turf to some depth, and large stones were heaped on the ground immediately under the centre of the instrument †.

Either of the levels of the upper circle being brought exactly over any two of the screws of the parallel plates, the latter served to place its bubble at the mark. The circle being then moved half round, one half of the deviation of the bubble from its mark was corrected by the two screws, and the other by the adjusting nuts of the level. The level at right angles having undergone a similar verification and correction, the plane of contact of the two circles would consequently be rendered parallel to the horizon‡. The intersection of the cross-

* The instrument has no vertical arch.

† Noughtberry Hill excepted, the ground at the theodolite-stations proved hard and firm.

‡ The correctness of this adjustment was confirmed when the upper circle could be moved quite round without displacing the bubble of the level of the telescope, previously brought to its mark.

wires

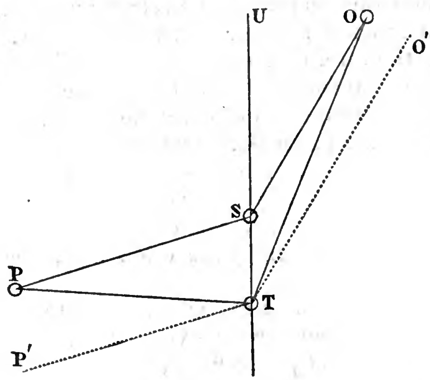
wires of the telescope (or line of collimation) was known to be parallel to the cylindrical rings encircling its tube, when the telescope could be made to revolve in its Ys without causing the point of intersection to deviate from the distant well-defined object which it had bisected. After this adjustment, the line of collimation, provided the artist had made the axis of the cradle parallel to the plane of contact of the two circles, and the Ys at right angles to that axis, would necessarily move in a vertical plane. As the remote signals could not be distinctly seen on interposing the horizontal wire of the telescope, the following method of observation with the vertical wire *only* was resorted to. The point of intersection of the wires being made to accurately bisect an object at a moderate distance, one wire was rendered perfectly vertical by turning the telescope to the right or left within its Ys, until either extremity of the wire, on elevating or depressing the telescope, would also pass through the middle of the object observed. Thus adjusted, it would evidently be superfluous to bisect *constantly* with any particular point of the vertical wire. To guard, however, against the possibility of the wire not being perfectly straight, a point, visually estimated to be equi-distant, from some particular particle of dust, &c. adhering to it, and the point of intersection by the other wire, was invariably brought on the signal.

The graduations being numbered from 0° to 360°, it follows, that the readings from the one vernier should differ exactly 180° from those given by the opposite one; or it will prove either that the vertical axis does not coincide with the centre of the divided circle, or that the divisions of the latter are incorrect. As both sources of error may exist, it will be proper to register for calculation the mean of the two readings. The order of numeration of the graduations being from left to right, the angle between any two of the observed signals is immediately found from a similar register, being equal, either to the difference of their respective readings, or to 360° minus that difference, according as it falls short of, or exceeds 180°.

The description of the instrument and its adjustments has been so ample, that the mode of observing with it will be too evident to require pointing out. It may, however, be needful to remark, that notwithstanding the length of the vertical axis, which is nearly four inches, it was found that a variation in the degree of force with which the two circles were clamped, caused a sensible deviation in the direction of the line of collimation; a source of error which it was endeavoured to obviate by always applying the *utmost* force to the clamp. Having completed single observations of the whole of the signals, it would,

would, without doubt, have materially increased their accuracy to have repeated them (circumstances permitting) with the telescope reversed in position; but such was the inconvenient situation of the tangent screw of the upper circle, as to render the method impracticable*. To make a second set of observations with the telescope in its original position was the only alternative.

As *data* for the reduction of the observations to the centre of the base of the signal, the telescope was pointed so as to bisect the latter, and the reading of the verniers in addition to the distance of the centre of the circles to that of the signal registered in the usual manner. In order to comprehend the nature of the reduction, let S in the annexed figure represent




the centre of the signal, T that of the divided circle, U the prolongation of the straight line passing through S and T, and O the object observed. Make TO′ parallel to SO; then, as the angle USO equals that of UTO′, the angle USO must exceed the observed angle UTO by the angle OTO′, equal to SOT. It is also clear that the observed angle, whether acute or obtuse, to the left or right of the line UST, will always be in defect. To calculate the angle SOT we have given the angle STO, the side ST, and (approximatively) that of SO. Then, as the graduations of the circle are numbered from left to right, the angle at T between U (or S) and an object O to the right, will be equal to the reading of O *minus* that of U; and we must add the angle of correction SOT to O, in order to make the registered readings coincide with those of another theodolite placed over S, the reading for U being alike by the two instruments. When the object (P) is to the left of the line UST, it will form with it at T an angle

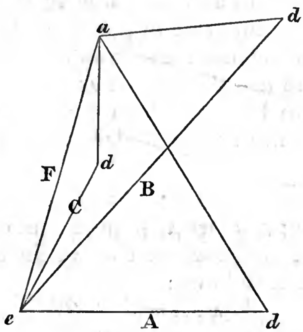
* I have since discovered that, by unfixing the clamp on the upper parallel plate, the circles might have been moved half-round in azimuth.

equal

equal to U minus P, and the correction must be applied to P with the *negative* sign, in order to make USP greater than UTP by the angle of correction SPT. These corrections being applied to the readings for the different signals, we form a new register, whence we can immediately find by subtraction (as before) the angular distance of any two of the signals as observed from S in lieu of T.

On referring to the distances of some of the more remote signals from the theodolite-stations as measured from different bases, discrepancies, amounting in some instances to upwards of 100 feet, will be remarked. These differences were occasioned in a great measure by the signals having a diameter too inconsiderable for the distance, so that the vertical wire in lieu of bisecting, appeared to efface them*. The formula adopted for obtaining a proper mean of such of the different measurements of a distance as were remarkably discordant, requires to be explained.

In order to find the distance between the point *e* and the inaccessible object *a*, measure from *e* in different directions




any number of bases, as ABC, and observe at the two extremities of every base the inclination of that base to a line passing through the point of observation in the direction of the inaccessible object. With these *data* we shall be able to calculate from each base an independent measure of the required distance;—but supposing the observations to have been subject to a small uncertainty, limited, for instance, to one minute, required the probable error of each of the respective *calculated* distances?

By trigonometry, 180° *minus* the sum of the angles taken at the ends of any one of the bases will be equal to the subtense of that base at *a*. Then if we restrict ourselves in our calcu-

* At a distance of 60,000 feet a tower 6 feet in diameter appears under an angle of only 21″.

lations

lations to the use of logarithms, treating them in the notation as natural numbers, F (or the distance required) will be equal to the sum of the base and sine of the angle opposite F, *minus* sine of the angle subtended at $a$ by the base.—Here we may observe that the value of the angle at $e$ affects the calculation merely as it serves to determine that of the unobserved angle $a$.

Admitting that *one* only, as well as *both* of the angles $d\,e$, may have been inaccurately observed, the subjoined list will include every possible arrangement of the angular errors together with the consequent alteration of the correct value of the unobserved angle $a$:

| | | | |
|---|---|---|---|
| I. Make the error of | $d + 1'$, and | $e + 1'$; whence | $a = -2'$; |
| II. | $+1$ | $0$ | $-1$; |
| III. | $0$ | $+1$ | $-1$; |
| IV. | $+1$ | $-1$ | $0$. |

As the log. sines increase from 0° to 90°, and diminish from 90° to 180°, it will be necessary to divide our investigations into two classes.

*Class* I.—The angles at $a$ and $d$ being both acute. Call $a'$ the difference of the log. sine of $a$ and the log. sine of $a + 1'$ (or of $a - 1'$); and $d'$ the corresponding difference of $d$ and $d + 1'$ (or of $d$ and $d - 1'$). Then as the correct length of F has been shown to be equal to $A + \sin d - \sin a$, its value with an arrangement of the angular errors as given in No. I. will become

$= A + \overline{\sin d + d'} - \overline{\sin a - 2a'}$; or simply

$= F + d' + 2a'$; whence $x$ or the lineal error required will be $= d' + 2a'$. For the whole class of the angular errors the values of $x$ will be as follows:

I. $x \ldots = d' + 2a'$
II. $\ldots\ldots\; d' + a'$
III. $\ldots\ldots\; a'$
IV. $\ldots\ldots\; d'$.

Hence the probable error of calculation will be equal to the mean of the above four quantities, or to $a' + \frac{3d'}{4}$.

*Class* II.—When $a$ is obtuse. Selecting No. I. of the arrangement of the possible errors of observation, the calculated length of F becomes

$B + \overline{\sin d + d'} - \overline{\sin a + 2a'}$; whence $x$ will be equal to $d' \backsim 2a'$, and its value for the complete arrangement of errors,

I. $\ldots x = d' \backsim 2a'$
II. $\ldots\ldots\; d' \backsim a'$
III. $\ldots\ldots\; a'$
IV. $\ldots\ldots\; d'$:

Consequently

Consequently when either $a$ or $d$ are obtuse, the mean error of calculation will be

$a' - \frac{d'}{4}$, when $a'$ is greater than $d'$;

$\frac{3d' - 2a'}{4}$, when $d'$ is greater than twice $a'$;

$\frac{2a' + d'}{4}$, when $d'$ is greater than $a'$ but less than $2a'$.

$x$ as thus expressed is the logarithm of the natural number by which we must multiply the correct distance, in order to obtain its value as affected by the mean of the possible errors of observation arranged conformable to our list. Consequently if we call 1 the correct distance, then will the error of measurement be equal to this natural number *minus* 1. Now if the distance as derived from one base A shall be liable to an error of 10 feet, and the same distance, as deduced from another base B, be uncertain to 20 feet, then must the claim to accuracy of A exceed that of B in the reciprocal ratio of 10 to 20 or as 10 to 5.

When we have given F′, F″, F‴ or the required distance as calculated from the same number of bases, together with the *reciprocals* of $x - 1$ corresponding to the respective bases, which call $x'$, $x''$, $x'''$, the correct value of F may be considered as equal to

$$\frac{\overline{F' \times x'} + \overline{F'' \times x''} + \overline{F''' \times x'''}}{x' + x'' + x'''}.$$

In practice it will be most convenient to take out the log. difference for 1′ of the sines of the angles $d$ and $e$, noting them + or − according as the angle is acute or obtuse. A type of calculation is subjoined.

Base A; Noughtberry Hill to Ingleborough . . 44106 feet.
B Ditto Dod Fell . . . . 19347

A. Noughtberry 62° 19′ 8″
Ingleborough 55 26 53 log. diff. of 1′ = +0·0000870 = $d'$,
Berkin ( 62 13 59) . . . . . . +0·0000665 = $a'$.

B. Noughtberry 145° 40′ 35″
Dod Fell . . 23 30 40 log. diff. of 1′ = +0·0002904 = $d'$,
Berkin (10 48 45) . . . . . . . +0·0006610 = $a'$.

Berkin to Noughtberry Hill by base A . . 41053 feet.
Ditto Ditto B . . 41142

Arithmetical mean . . . . . 41097·5

The value of $x$ for the base A will be $a' + \frac{3d'}{4}$, = 0·0001317, of which the natural number *minus* 1 is ·000303; and the reciprocal of the latter 330033.

The value of $x$ for the base B will be $a' + \frac{3d'}{4}, = 0{\cdot}0008788$, of which the natural number *minus* 1 is ·002026, and the reciprocal of the latter 49358.

The corrected distance will therefore be equal to

$$\frac{\overline{41053 \times 330033} + \overline{41142 \times 49358}}{330033 + 49358} = 41064{\cdot}5 \text{ feet.}$$

We might now proceed to investigate what proportion of the difference of 180°, and the sum of the three observed angles of a triangle should be assigned to each angle, were not the differences in the present survey, as appears from the following list, too trivial to require such nicety of correction.

| | | | | | |
|---|---|---|---|---|---|
| No. 1. | −0′ 40″ | No. 5. | −2′ 35″ | No. 9. | +1′ 54″ |
| 2. | +0 19 | 6. | +1 18 | 10. | −0 6 |
| 3. | −0 17 | 7. | −1 37 | | |
| 4. | −0 35 | 8. | −0 12 | | |

Mean difference 57″ or 19″ per angle.

In the succeeding register of the observations, the second column contains the reading from the nearest vernier; the third column that from the opposite one, suppressing the degrees. The two next columns give the minutes and seconds of the corresponding readings for the second set of observations.

*At Dod Fell.*

| | | | | | Reduction to Centre. |
|---|---|---|---|---|---|
| Lovely Seat | 265° 5′ 30″ | 6′ 0″ | 5′ 0″ | 5′ 45″ | −0′ 34″ |
| Shunnor Fell | 248 46 30 | 47 0 | 46 0 | 46 30 | −0 28 |
| Noughtberry Hill | 182 42 0 | 42 0 | 42 0 | 41 30 | −0 10 |
| Berkin | 159 11 15 | 11 0 | 11 0 | 10 30 | +0 6 |
| Bow Fell | 191 1 30 | 1 30 | 1 0 | 1 0 | −0 9 |
| Pen-y-gent | 66 41 0 | 40 30 | 41 0 | 40 30 | +0 32 |
| Whernside | 138 51 30 | 51 0 | 51 0 | 50 45 | +0 20 |
| Colm | 151 36 30 | 36 0 | 36 30 | 36 0 | +0 10 |
| Blea Moor | 139 58 0 | 57 15 | 58 15 | 57 15 | +0 30 |
| Ingleborough | 110 40 45 | 40 0 | 41 0 | 40 30 | +0 24 |
| Wildboar Fell | 215 20 0 | 20 30 | 20 0 | 20 30 | −0 15 |
| Cam Fell | 116 37 30 | 36 30 | .... | .... | +1 42 |
| Cotter Fell* | 232 20 0 | 20 0 | .... | .... | −0 19 |
| The Signal | 354 0 0 | 6 feet distant. | | | |

*At Whernside.*

| | | | | | |
|---|---|---|---|---|---|
| Colm | 241° 15′ 30″ | 15′ 30″ | .... | .... | 0′ 0″ |
| The Calf | 277 42 30 | 42 30 | .... | .... | +0 10 |

* There was no signal on this hill.

Bow

| | | | | | Reduction to Centre. |
|---|---|---|---|---|---|
| Bow Fell | 303° 26′ 30″ | 26′ 30″ | ...... | ...... | +0′ 25″ |
| Shunnor Fell | 336 56 15 | 55 30 | ...... | ...... | +0 14 |
| Noughtberry | 343 17 30 | 17 0 | ...... | ...... | +0 36 |
| Lovely Seat | 348 2 30 | 2 0 | ...... | ...... | +0 14 |
| Whaw Fell | 358 40 15 | 40 0 | ...... | ...... | +0 41 |
| Dod Fell | 15 39 15 | 38 0 | ...... | ...... | +0 19 |
| Wildboar Fell | 308 30 30 | 31 0 | ...... | ...... | +0 15 |
| Pen-y-gent | 70 57 30 | 56 30 | ...... | ...... | −0 4 |
| Ingleborough | 119 53 0 | 52 30 | ...... | ...... | −0 36 |
| Graygreth | 189 56 0 | 55 30 | ...... | ...... | −0 41 |
| The Signal | 241 0 0 | 4·5 feet distant. | | | |

*At Pen-y-gent.*

| | | | | | |
|---|---|---|---|---|---|
| Whernside | 85° 49′ 30″ | 49′ 0″ | 51′ 30″* | 50′ 30″ | 0′ 0″ |
| Ingleborough | 54 5 30 | 5 0 | 6 30 | 6 30 | +0 14 |
| Colm | 83 22 0 | 21 30 | 23 30 | 22 30 | +0 1 |
| Dod Fell | 138 21 30 | 21 30 | 23 0 | 22 30 | −0 18 |
| Bow Fell | 108 53 30 | 53 0 | 55 0 | 54 0 | −0 5 |
| Noughtberry | 117 9 0 | 8 30 | 10 30 | 10 0 | −0 9 |
| Wildboar Fell | 119 28 0 | 27 0 | 29 0 | 29 0 | −0 5 |
| Blea Moor | 101 38 0 | 37 30 | 39 0 | 37 30 | −0 6 |
| Shunnor Fell | 139 28 30 | 28 30 | 29 30 | 27 30 | −0 9 |
| Lovely Seat | 147 38 0 | 37 30 | 38 30 | 38 30 | −0 10 |
| The Signal | 265 49 0 | 4 feet distant. | | | |

*At Ingleborough Hill.*

| | | | | | |
|---|---|---|---|---|---|
| Berkin | 3° 52′ 0″ | 52′ 0″ | 51′ 30″ | 51′ 30″ | +0′ 31″ |
| Colm | 14 9 30 | 10 0 | 9 30 | 9 30 | +0 47 |
| The Calf | 20 27 0 | 27 0 | ..... | ..... | +0 12 |
| Whernside | 36 23 0 | 22 30 | 23 0 | 22 30 | +0 21 |
| Pen-y-gent | 135 44 0 | 44 0 | 44 0 | 44 30 | −0 57½ |
| Dod Fell | 83 59 30 | 60 0 | 58 30 | 59 0 | −0 23 |
| Noughtberry | 59 19 0 | 19 30 | 19 0 | 19 30 | −0 6 |
| Blea Moor | 60 21 30 | 21 30 | 22 0 | 21 0 | −0 11 |
| Graygreth | 350 32 30 | 32 30 | ...... | ...... | +1 9 |
| Bow Fell | 38 30 30 | 30 30 | ...... | ...... | +0 7 |
| Lovely Seat | 72 36 30 | 36 30 | ...... | ...... | −0 9 |
| Shunnor Fell | 63 59 30 | 60 0 | (Fell misty) | | −0 5 |
| The Signal | 230 30 0 | 9 feet distant. | | | |

*At Noughtberry† Hill.*

| | | | | | |
|---|---|---|---|---|---|
| Dod Fell | 224° 17′ 0″ | 16′ 30″ | ...... | ...... | +0′ 29″ |
| Pen-y-gent | 267 5 0 | 5 30 | ...... | ...... | +0 24 |
| Ingleborough | 307 38 30 | 38 0 | ...... | ...... | +0 26 |

* On the point of making the second set of observations one of the feet of the tripod was disturbed.

† *Rubus Chamæmorus.*



Blea

| | | | | | Reduction to Centre. |
|---|---|---|---|---|---|
| Blea Moor | 305° 47′ 30″ | 47′ 0″ | .... | .... | +1′ 14″ |
| Whernside | 328 5 0 | 5 15 | .... | .... | +0 38 |
| Colm | 354 36 30 | 36 0 | .... | .... | +0 15 |
| Berkin | 9 58 0 | 57 30 | .... | .... | +0 4 |
| The Calf | 56 12 15 | 11 45 | .... | .... | −0 15 |
| Bow Fell | 60 7 30 | 7 0 | .... | .... | −0 42 |
| Wildboar Fell | 92 14 30 | 13 30 | .... | .... | −0 30 |
| Cotter Fell | 112 29 0 | 28 30 | .... | .... | −0 27 |
| Shunnor Fell | 137 37 30 | 36 30 | .... | .... | −0 28 |
| Lovely Seat | 155 53 0 | 53 0 | .... | .... | −0 21 |
| Whaw Fell | 282 38 0 | 37 30 | .... | .... | +2 43 |
| The Signal | 197 30 0 | 6 feet distant. | | | |

The angles at the following stations were measured by a four-inch theodolite (much out of repair) reading off to one minute. The graduations were numbered precisely as those of the six-inch theodolite.

*At Whaw Fell.*

| | | | |
|---|---|---|---|
| Dod Fell | 291° | 30′ | 0″ |
| Noughtberry | 192 | 42 | 0 |
| Blea Moor | 54 | 32 | 0 |
| Whernside | 73 | 33 | 0 |
| Cam Fell | 328 | 40 | 0 |
| Bow Fell | 160 | 58 | 0 |
| Ryssell | 135 | 30 | 0 |
| Snays Fell | 291 | 58 | 0 |
| Bear's Head | 274 | 20 | 0 |
| Ten End | 265 | 14 | 0 |

*At Blea Moor.*

| | | | |
|---|---|---|---|
| Dod Fell | 26° | 6′ | 0″ |
| Cam Fell | 42 | 28 | 0 |
| Snays Fell | 14 | 28 | 0 |
| Whaw Fell | 349 | 0 | 0 |

*At Colm.*

| | | | |
|---|---|---|---|
| Whernside | 235° | 52′ | 0″ |
| Ingleborough | 272 | 17 | 0 |
| Graygreth | 313 | 19 | 0 |

*At Ryssell.*

| | | | |
|---|---|---|---|
| Blea Moor | 305° | 56′ | 0″ |
| Bow Fell | 200 | 24 | 0 |
| Whaw Fell | 285 | 17 | 0 |

*At Bear's Head.*

| | | | |
|---|---|---|---|
| Dod Fell | 167° | 49′ | 30″ |
| Noughtberry | 205 | 33 | 30 |
| Whaw Fell | 189 | 56 | 30 |

*At Ten End.*

| | | | |
|---|---|---|---|
| Bear's Head | 267° | 16′ | 0″ |
| Cam Fell | 22 | 30 | 0 |
| Blea Moor | 47 | 58 | 0 |
| Whaw Fell | 57 | 41 | 0 |
| Noughtberry | 80 | 8 | 0 |

*Calculation of the Distances.*

| | | Feet. |
|---|---|---|
| Ingleborough to Pen-y-gent | | 32160 |
| Ingleborough | 99° 20′ 18″ | 22441 |
| Pen-y-gent | 31 44 16 | 42096 |
| Whernside | 48 55 26 | |

| | | Feet. |
|---|---|---|
| Ingleborough | 76° 23′ 55″ | 44098 |
| Pen-y-gent | 63 3 9 | 48082 |
| Noughtberry | 40 32 56 | |
| Ingleborough | 22 56 55 | 44111 |
| Whernside | 136 35 9 | 25026 |
| Noughtberry | 20 27 56 | |

Pen-y-gent

| | | | | Feet. |
|---|---|---|---|---|
| Pen-y-gent | 31° | 19′ | 46″ | 48086 |
| Whernside | 87 | 39 | 35 | 25023 |
| Noughtberry | 61 | 0 | 39 | |
| | | | | |
| Ingleborough | 51 | 44 | 24 | 46068 |
| Pen-y-gent | 84 | 15 | 49 | 36354 |
| Dod | 43 | 59 | 47 | |
| | | | | |
| Ingleborough | 47 | 33 | 58 | 46068 |
| Whernside | 104 | 13 | 24 | 35095 |
| Dod | 28 | 10 | 38 | |
| | | | | |
| Whernside | 55 | 18 | 4 | 35096 |
| Pen-y-gent | 52 | 31 | 45 | 36356 |
| Dod | 72 | 10 | 11 | |

*Mean Distances.*

Ingleborough to Noughtberry...44105
Ingleborough to Dod Fell ......46068
Pen-y-gent to Noughtberry......48084
Pen-y-gent to Dod Fell .........36355
Whernside to Noughtberry......25024
Whernside to Dod Fell .........35095

| | | | | |
|---|---|---|---|---|
| Pen-y-gent | 21° | 12′ | 2″ | 48091 |
| Dod | 115 | 59 | 59 | 19350 |
| Noughtberry | 42 | 47 | 59 | |
| | | | | |
| Ingleborough | 24 | 39 | 5 | 44110 |
| Dod | 72 | 0 | 6 | 19345 |
| Noughtberry | 83 | 20 | 49 | |
| | | | | |
| Whernside | 32 | 21 | 7 | 25031 |
| Dod | 43 | 50 | 20 | 19339 |
| Noughtberry | 103 | 48 | 33 | |

Mean distance of Dod to Noughtberry...... } ......19345

| | | | | |
|---|---|---|---|---|
| Ingleborough | 38 | 51 | 57 | 77647 |
| Noughtberry | 108 | 33 | 4 | 51393 |
| Calf | 32 | 34 | 59 | |
| | | | | |
| Whernside | 65 | 35 | 11 | 56420 |
| Noughtberry | 88 | 6 | 0 | 51404 |
| Calf | 26 | 18 | 49 | |

Mean distance of the Calf to Noughtberry ......... } ...51398
The Calf to Pen-y-gent .........95894

| | | | | |
|---|---|---|---|---|
| Ingleborough | 121° | 32′ | 46″ | 32250 |
| Pen-y-gent | 29 | 16 | 18 | 56210 |
| Colm | 29 | 10 | 56 | |
| | | | | |
| Ingleborough | 22 | 12 | 42 | 32283 |
| Whernside | 121 | 23 | 21 | 14296 |
| Colm | 36 | 23 | 57 | |

| | | | | Feet. |
|---|---|---|---|---|
| Ingleborough | 69° | 48′ | 28″ | 32267 |
| Dod | 40 | 55 | 27 | 46230 |
| Colm | 69 | 16 | 5 | |
| | | | | |
| Ingleborough | 45 | 8 | 45 | 32260 |
| Noughtberry | 46 | 57 | 49 | 31288 |
| Colm | 87 | 53 | 26 | |
| | | | | |
| Pen-y-gent | 54 | 59 | 25 | 56230 |
| Dod | 84 | 55 | 8 | 46237 |
| Colm | 40 | 5 | 27 | |
| | | | | |
| Pen-y-gent | 33 | 46 | 57 | 56222 |
| Noughtberry | 87 | 30 | 51 | 31291 |
| Colm | 58 | 42 | 12 | |
| | | | | |
| Whernside | 102 | 2 | 21 | 14284 |
| Noughtberry | 26 | 30 | 45 | 31294 |
| Colm | 51 | 26 | 54 | |
| | | | | |
| Whernside | 134 | 23 | 26 | 14276 |
| Dod | 12 | 45 | 1 | 46221 |
| Colm | 32 | 51 | 33 | |
| | | | | |
| Dod | 31 | 5 | 17 | 46268 |
| Noughtberry | 130 | 19 | 16 | 31335 |
| Colm | 18 | 35 | 27 | |

*Mean Distances.*

Ingleborough to Colm .........32265
Pen-y-gent ..........................56221
Whernside ..........................14285
Noughtberry ......................31302
Dod ...................................46239

| | | | | |
|---|---|---|---|---|
| Ingleborough | 45° | 49′ | 27″ | 23444 |
| Whernside | 70 | 2 | 55 | 17888 |
| Graygreth | 64 | 7 | 38 | |
| | | | | |
| Ingleborough | 23 | 36 | 45 | 23439 |
| Colm | 41 | 2 | 0 | 14301 |
| Graygreth | 115 | 21 | 15 | |
| | | | | |
| Whernside | 51 | 20 | 26 | 17890 |
| Colm | 77 | 27 | 0 | 14311 |
| Graygreth | 51 | 12 | 34 | |

*Mean Distances.*

Ingleborough to Graygreth......23442
Whernside ..........................17889

| | | | | |
|---|---|---|---|---|
| Ingleborough | 55° | 26′ | 53″ | 44141 |
| Noughtberry | 62 | 19 | 8 | 41053 |
| Berkin | 62 | 13 | 59 | |
| | | | | |
| Ingleborough | 80 | 6 | 36 | 44157 |
| Dod | 48 | 30 | 4 | 58080 |
| Berkin | 51 | 23 | 20 | |

| | | Feet. |
|---|---|---|
| Dod | 23°30′ 40″ | 58182 |
| Noughtberry | 145 40 35 | 41142 |
| Berkin | 10 48 45 | |

*Corrected mean Distances.*

Ingleborough to Berkin .........44149
Dod ....................................58092
Noughtberry ......................41065

| | | |
|---|---|---|
| Ingleborough | 97° 12′ 33″ | 55981 |
| Pen-y-gent | 54 47 42 | 67971 |
| Bow Fell | 27 59 45 | |
| Ingleborough | 20 48 32 | 55981 |
| Noughtberry | 112 27 52 | 21520 |
| Bow Fell | 46 43 36 | |
| Ingleborough | 45 28 15 | 55998 |
| Dod | 80 20 8 | 40495 |
| Bow Fell | 54 11 37 | |
| Pen-y-gent | 23 3 40 | 67951 |
| Whernside | 127 30 1 | 33550 |
| Bow Fell | 29 26 19 | |
| Pen-y-gent | 29 28 1 | 67993 |
| Dod | 124 19 49 | 40503 |
| Bow Fell | 26 12 10 | |
| Whernside | 39 50 56 | 33580 |
| Noughtberry | 92 0 48 | 21530 |
| Bow Fell | 48 8 16 | |
| Whernside | 72 12 1 | 33576 |
| Dod | 52 9 42 | 40480 |
| Bow Fell | 55 38 17 | |

*Mean Distances.*

Ingleborough to Bow Fell ......55987
Pen-y-gent ..........................67972
Whernside ...........................33568
Noughtberry ......................21525
Dod Fell ..............................40493

| | | |
|---|---|---|
| Pen-y-gent | 33° 38′ 2″ | 87582 |
| Whernside | 122 25 56 | 57475 |
| Wildboar Fell | 23 56 2 | |
| Pen-y-gent | 18 53 39 | 87672 |
| Dod | 148 38 43 | 54561 |
| Wildboar Fell | 12 27 38 | |
| Whernside | 67 7 56 | 57516 |
| Dod | 76 28 40 | 54506 |
| Wildboar Fell | 36 23 24 | |
| Whernside | 34 46 51 | 57566 |
| Noughtberry | 124 7 45 | 39670 |
| Wildboar Fell | 21 5 24 | |

| | | Feet. |
|---|---|---|
| Dod | 32° 38′ 18″ | 54436 |
| Noughtberry | 132 3 44 | 39545 |
| Wildboar Fell | 15 17 58 | |

*Corrected mean Distances.*

Whernside to Wildboar Fell ...57522
Dod ....................................54494
Pen-y-gent ..........................87613
Noughtberry ......................39616

| | | |
|---|---|---|
| Ingleborough | 71° 43′ 30″ | 82363 |
| Pen-y-gent | 85 22 15 | 78465 |
| Shunnor Fell | 22 54 15 | |
| Ingleborough | 27 36 34 | 82326 |
| Whernside | 142 56 2 | 63300 |
| Shunnor | 9 27 24 | |
| Ingleborough | 19 59 12 | 82407 |
| Dod | 138 5 4 | 42163 |
| Shunnor | 21 55 44 | |
| Pen-y-gent | 53 38 13 | 78480 |
| Whernside | 94 0 49 | 63352 |
| Shunnor | 32 20 58 | |
| Pen-y-gent | 22 19 0 | 78541 |
| Noughtberry | 129 29 7 | 38643 |
| Shunnor | 28 11 53 | |
| Whernside | 38 42 49 | 63377 |
| Dod | 109 54 38 | 42158 |
| Shunnor | 31 22 33 | |
| Noughtberry | 86 40 32 | 38619 |
| Dod | 66 4 20 | 42180 |
| Shunnor | 27 15 8 | |

*Corrected mean Distances.*

Ingleborough to Shunnor ......82375
Pen-y-gent ..........................78500
Whernside ...........................63359
Noughtberry ......................38628
Dod ....................................42168

| | | |
|---|---|---|
| Ingleborough | 63° 6′ 49″ | 80969 |
| Pen-y-gent | 93 31 51 | 72354 |
| Lovely Seat | 23 21 20 | |
| Ingleborough | 13 17 12 | 80899 |
| Noughtberry | 151 46 2 | 39303 |
| Lovely Seat | 14 56 46 | |
| Ingleborough | 11 22 31 | 81008 |
| Dod Fell | 154 24 2 | 36978 |
| Lovely Seat | 14 13 27 | |

Pen-y-gent

| | | Feet. |
|---|---|---|
| Pen-y-gent | 61° 47′ 49″ | 72300 |
| Whernside | 82 54 27 | 64213 |
| Lovely Seat | 35 17 44 | |
| | | |
| Pen-y-gent | 30 28 36 | 72316 |
| Noughtberry | 111 13 0 | 39345 |
| Lovely Seat | 38 18 24 | |
| | | |
| Whernside | 27 36 27 | 64188 |
| Dod Fell | 126 13 6 | 36870 |
| Lovely Seat | 26 10 27 | |
| | | |
| Dod Fell | 82 23 18 | 36873 |
| Noughtberry | 68 24 35 | 39305 |
| Lovely Seat | 29 12 7 | |

*Corrected mean Distances.*

Ingleborough to Lovely Seat ...80959
Whernside ........................64200
Pen-y-gent ........................72321
Noughtberry ....................39320
Dod ................................36887

| | | |
|---|---|---|
| Noughtberry | 111° 48′ 56″ | 46333 |
| Dod | 49 38 0 | 56456 |
| Cotter Fell | 18 33 4 | |

Pen-y-gent to Cotter Fell ......92115

| | | |
|---|---|---|
| Ingleborough | 75° 21′ 51″ | 28252 |
| Pen-y-gent | 47 31 48 | 37058 |
| Blea Moor | 57 6 21 | |
| | | |
| Ingleborough | 23 37 33 | 28250 |
| Dod | 29 17 13 | 23144 |
| Blea Moor | 127 5 14 | |
| | | |
| Pen-y-gent | 15 31 27 | 37063 |
| Noughtberry | 38 42 50 | 15860 |
| Blea Moor | 125 45 43 | |
| | | |
| Pen-y-gent | 36 43 55 | 37056 |
| Dod | 73 16 54 | 23140 |
| Blea Moor | 69 59 11 | |
| | | |
| Dod | 42 43 31 | 23149 |
| Noughtberry | 81 31 15 | 15880 |
| Blea Moor | 55 45 14 | |

*Mean Distances.*

Ingleborough to Blea Moor ....28251
Pen-y-gent ........................37059
Dod ..................................23144
Noughtberry ......................15870

| | | |
|---|---|---|
| Whernside | 15° 22′ 57″ | 20418 |
| Noughtberry | 45 25 17 | 7604 |
| Whaw Fell | 119 11 46 | |
| | | |
| Noughtberry | 58 23 14 | 7593 |
| Dod | 22 49 16 | 16671 |
| Whaw Fell | 98 47 30 | |

| | | Feet. |
|---|---|---|
| Whernside | 16° 58′ 8″ | 20434 |
| Dod | 20 58 52 | 16655 |
| Whaw Fell | 142 3 0 | |

*Mean Distances.*

Noughtberry to Whaw Fell...... 7598
Whernside ..........................20426
Dod .....................................16668
Shunnor Fell ......................45068
Lovely Seat..........................44290
Bow Fell ..............................27603
Ingleborough .....................37356
Blea Moor .......................... 9371
Berkin .................................41407
Colm ...................................29836
Pen-y-gent ..........................40818

| | | |
|---|---|---|
| Blea Moor | 16° 22′ 0″ | 14348 |
| Dod | 23 19 29 | 10211 |
| Cam Fell | 140 18 31 | |
| | | |
| Blea Moor | 53 28 0 | 14343 |
| Whaw Fell | 85 52 0 | 11555 |
| Cam Fell | 40 40 0 | |
| | | |
| Whaw Fell | 122 36 0 | 7618 |
| Blea Moor | 25 28 0 | 14926 |
| Snays Fell | 31 56 0 | |
| | | |
| Whaw Fell | 72 33 0 | 19821 |
| Noughtberry | 85 0 0 | 18981 |
| Ten End | 22 27 0 | |
| | | |
| Whaw Fell | 63 24 0 | 19829 |
| Cam Fell | 81 25 0 | 17931 |
| Ten End | 35 11 0 | |
| | | |
| Whaw Fell | 81 39 0 | 27998 |
| Noughtberry | 82 44 0 | 27925 |
| Bear's Head | 15 37 0 | |
| | | |
| Whaw Fell | 17 8 0 | 28011 |
| Dod Fell | 140 45 0 | 13042 |
| Bear's Head | 22 7 0 | |

*Mean Distances.*

Ten End to Whaw Fell .........19825
Bear's Head .......................28004

| | | |
|---|---|---|
| Whaw Fell | 80° 58′ 0″ | 26028 |
| Blea Moor | 78 23 0 | 26243 |
| Ryssell | 20 39 0 | |
| | | |
| Whaw Fell | 25 28 0 | 25984 |
| Bow Fell | 69 39 0 | 11916 |
| Ryssell | 84 53 0 | |

Mean distance of Whaw Fell to Ryssell......... } ......26006

[To be continued.]

XV. *On*

## XV. On Mr. Herapath's *Method for the Integration of Linear Equations with constant Coefficients.*

THE integration of linear equations with constant coefficients engaged the attention of analysts very early in the progress of the Integral Calculus. The subject has been treated of so often and so ably, that we can hardly expect any addition to our knowledge of it either new in principle, or very interesting in the details. It would not therefore have been necessary to notice particularly the communications of Mr. Herapath published in the last two Numbers of this Journal, if he had not fallen into an inadvertence respecting Lagrange's investigation, of which it seems proper to advise the public.

In pp. 22, 23, of the last Number of this Journal, Mr. Herapath applies Lagrange's process to the equation,

$$\frac{d^3y}{dx^3} + A\frac{d^2y}{dx^2} + B\frac{dy}{dx} + Cy = X.$$

The calculation is given at length in Lacroix's Treatise on the Integral Calculus, 2nd edit. vol. ii. pp. 326, 327. If $r, r_1, r_2$ stand for the roots of the equation

$$r^3 + Ar^2 + Br + C = 0,$$

the complete integral is as follows, viz.

$$y = \frac{e^{rx}\left(c + \int Xe^{-rx}\,dx\right)}{r - r_1 \,.\, r - r_2}$$

$$+ \frac{e^{r_1x}\left(c_1 + \int Xe^{-r_1x}\,dx\right)}{r_1 - r \,.\, r_1 - r_2}$$

$$+ \frac{e^{r_2x}\left(c_2 + \int Xe^{-r_2x}\,dx\right)}{r_2 - r \,.\, r_2 - r_1}.$$

Of these three parts Mr. Herapath has calculated only the first, which he says must separately satisfy the proposed equation. Now in this assertion, of which no proof is given, lies his mistake. If every part had contained only one root, it might have been rightly inferred that it must separately satisfy the differential equation; but, as the case stands, there is no ground for the assertion. If Mr. Herapath will substitute the two remaining parts of the integral in the differential equation, in like manner as he has substituted the first part, he will find, on collecting the three results, that there is no failure in Lagrange's method.

*α β.*

XVI. *On*

XVI. *On the Error imputed by* Mr. Herapath *to* Lagrange *in his Method of integrating Linear Differential Equations. By* A Correspondent.

*To the Editors of the Philosophical Magazine and Annals.*

Gentlemen,

YOUR Correspondent Mr. Herapath has endeavoured to prove that Lagrange has fallen into an error in his integration of linear differential equations, and expresses his surprise that mathematicians have for upwards of half a century suffered this to pass unobserved. Skilful mathematicians will not take Mr. Herapath's word for his bold assertion without examination, and will then easily detect the fallacy of his reasoning: but as your readers are not all of them mathematicians of competent knowledge, it will perhaps not be useless to point out Mr. Herapath's error.

Mr. H. has deduced the following equation (page 24.)

$$(r^2 + Ar + B)\, aX + (r + A)\, a\, dX + a\, d^2X = X$$

$a$ being $\dfrac{1}{r^2 - r r_2 - r r_1 + r_1 r_2} = \dfrac{1}{(r - r_1)(r - r_2)}$.

This equation is quite incomplete. Mr. H. having employed that part of $y$ which depends on the root $r$, only; all the terms arising from those parts of $y$ which depend on $r_1$ and $r_2$ are left out. A and B involve the three roots equally, and their values may be substituted in the above terms on the left-hand side of the equation as Mr. H. does, and we obtain for them the following

$$r_1 r_2 aX - (r_1 + r_2)\, a\, dX + a\, d^2X$$

Call $a'$, $a''$ the values corresponding to $a$ for the two other roots $r_1$, $r_2$, so that $a' = \dfrac{1}{(r_1 - r)(r_1 - r_2)}$ and $a'' = \dfrac{1}{(r_2 - r)(r_2 - r_1)}$.

The terms which are, consequently, to be added to make Mr. H.'s equation complete will be

$$r r_2 a'X - (r + r_2)\, a'\, dX + a'\, d^2X$$

$$r r_1 a''X - (r + r_1)\, a''\, dX + a''\, d^2X,$$ and the aggregate of all terms will give this equation:

$$(r_1 r_2 a + r r_2 a' + r r_1 a'')X - [(r_1 + r_2)a + (r + r_2)a' + (r + r_1)a'']dX + (a + a' + a'')\, d^2X = X.$$

It will now be easily seen that $a + a' + a'' = 0$,

$(r_1 + r_2)\, a + (r + r_2)\, a' + (r + r_1)\, a'' = 0$ and

$r_1 r_2 a + r r_2 a' + r r_1 a'' = 1$ and that, consequently, the equation becomes $X = X$, as it ought to be. This is the same proof

proof of the correctness of Lagrange's solution that Mr. Herapath employs to establish the truth of his own.

I am, Gentlemen,

Your obedient servant,

4, Pancras-Lane. F. R. S.

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## XVII. *On the ultimate Composition of simple Alimentary Substances; with some preliminary Remarks on the Analysis of organized Bodies in general.* By William Prout, *M.D. F.R.S.*

[Concluded from page 40.]

### *Of the Saccharine principle.*

IN the following observations, the word *Sugar*, is used in its ordinary acceptation; but the extended sense in which the term *saccharine principle* is employed, requires a few remarks.

Messrs. Gay Lussac and Thenard were induced to conclude, from their experiments on organized products, that when the hydrogen and oxygen of a substance exist in it in the proportions in which they form water, the substance is neither acid nor alkaline, as in sugar, starch, gum, &c.; that when the oxygen exceeds this proportion, the substance possesses acid properties; that when it is less, an oily or resinous character*. These conclusions are true to a certain extent, but by no means universally so, as will be shown hereafter. I shall however adopt this general distribution of organized substances so far, as to confine my attention at present to those substances in which the first peculiarity above mentioned exists; and as sugar, on account of its crystalline form, appears to constitute the most perfect and definite of these substances, I have thought it best entitled to give a name to the whole class, or family, and hence have included, under the term *saccharine principle*, all those substances, whatever their sensible properties may be, into the composition of which the hydrogen and oxygen enter in the proportions in which they form water. Now it will be found, that the substances thus constituted may generally be employed as aliments; and as they are chiefly derived from the vegetable kingdom, they may be considered as representing vegetable aliments, properly so called; hence, *saccharine principle* and *vegetable aliment* may be regarded as synonymous terms, and they will be so employed throughout the present inquiry.

* *Récherches Physico-chimiques,* ii. 321.

As

As a subject of general interest to chemists, as well as of considerable importance in the present inquiry, I shall also attempt to investigate the composition of a few of the compounds of the saccharine principle with oxygen, or what are usually denominated the *vegetable acids.*

### *Of Sugars.*

Many analyses of sugar have been published by different chemists, no two of which agree with each other. These discrepances have doubtless arisen from various causes, though one cause has probably been some real or accidental difference in the composition of the sugars employed*. How many distinct varieties of sugar exist I do not pretend to know, but there are at least two, (independently of the sugar of milk, manna, &c. which belong to another series,) and probably there are several others; and it is to the mixture or combination of these in different proportions, and the frequent presence of foreign bodies, that a good deal of the confusion respecting the composition of sugar has undoubtedly risen.

*Cane Sugar.*—The strongest and most perfect sugar that I am acquainted with, is sugar candy carefully prepared from cane sugar. This, purified by repeated crystallizations from water and alcohol, and deprived of the little hygrometric

* Some years ago I published an analysis of sugar, in which the proportions of carbon to water were stated to be to one another as 40 : 60. I was not aware at that time of the differences existing among sugars, and the results given were founded on the analysis of a specimen of remarkably fine looking sugar candy, a quantity of which I had purchased and kept by me for several years for the purposes of experiment[a]. At length my stock became exhausted, and I was surprised to find on analysing other specimens, that they in general contained upwards of one per cent. more of carbon than what I had before examined. This induced me to recur to the notes of my former experiments, but I could detect no material error in them; and though I readily admit that the apparatus I then employed was much less susceptible of accuracy than what I now use, I cannot help thinking that the candy itself was partly in fault, and that it was prepared from an imperfect sugar, probably from the East Indies.

There was also another circumstance which contributed to mislead me, not only in this but in all my other results, viz. an inaccuracy in the weight usually assigned to atmospheric air, at least as regarded *my weights.* I have long suspected the perfect accuracy of this datum as settled fifty years ago by Sir G. Shuckburgh, and have been accustomed for some time past to make an allowance for it; but I was not aware till recently of its exact amount, when I was induced to undertake a series of experiments on the subject, which I hope shortly to lay before the public.

---

[a] See Annals of Philosophy, iv. 424 (N. S.) I do not distinctly remember whether, at the time this paper was published, some of the original sugar candy existed or not, but I had then only made one or two experiments on the sugar of commerce.

 moisture

moisture that usually adheres to it by exposure for some time to a temperature of 212°, was found to be composed of

Carbon ...... 42·85
Water........ 57·15

Now, all the finest and purest specimens of loaf sugar of commerce that I have yet examined, give, when similarly treated, precisely the same results. They may therefore be considered as identical in their composition with sugar candy*. Cane sugar appears to undergo no change whatever at the temperature of boiling water; but at about 300° it begins to melt, and assume the form of a dark-brown liquid. In one experiment, after exposure to this temperature for seven hours, it lost ·6 per cent. only of its weight, but its properties seem to have been permanently injured. Berzelius however has shown that, on being combined with lead, sugar parts with about 5·3 per cent. of water without undergoing decomposition; for he has likewise shown that it may be obtained again from the lead in its original state. This saccharate of lead I have several times formed, and once by accident I obtained it iu the state of beautiful crystals.

*Sugar of Honey.*—The *lowest*† well-defined sugar that I have yet examined, was first obtained from Narbonne honey, by means of a process formerly pointed out by me for obtaining diabetic sugar in a state of purity‡. This, deprived of its hygrometric moisture by being placed under a receiver with sulphuric acid for several days, was found to consist of

Carbon....... 36·36
Water....... 63·63

This sugar in the ordinary state of the atmosphere usually contains more water than indicated by this analysis; that is to say, generally about 64·7 per cent. On the other hand, on exposure to a temperature considerably below that of boiling water, it rapidly loses about 3 per cent. of water, and begins to assume the fluid form: kept at the temperature of boiling water for 30 hours, it lost in one experiment upwards

* Dr. Ure states that he has found sugar to contain upwards of 43 per cent. of carbon; but no such specimen has occurred to me, though I by no means deny its existence. Indeed I have hitherto met with no sugar as it occurs in commerce, yielding more than 42·5 per cent. of carbon, and frequently it contains considerably less than this.

† In commerce, these imperfect sugars are denominated *weak* or *low* sugars, which last epithet is here employed in this sense.

‡ Med. Chirurg. Trans. viii. 537. I have little doubt that honey contains a still lower sugar, and which is incapable in this country (at least during a great part of the year), of assuming the solid form. This is probably the liquid sugar of Proust.

of

of 10 per cent. of its original weight, became of a deep brown colour, and seemed to be partially decomposed *.

*Sugar* prepared from *starch* evidently belongs to this variety, as is sufficiently indicated both by its sensible properties and composition. The same is true in general of *diabetic sugar*, and probably also of the *sugar of grapes, figs, &c.* When pure, all the varieties of this sugar are beautifully white, crystallize in spherules, and are permanent under the ordinary circumstances of the atmosphere.

Between these two extremes, sugars occur of almost every grade, as the following table will show.

| | Carbon. | Water. |
|---|---|---|
| Pure sugar candy . . . . . | 42·85 | 57·15 |
| Impure† sugar candy . . . | 41·5 to 42·5 | 58·5 to 57·5 |
| East India sugar candy (*v*) | 41·9 | 58·1 |
| English refined sugar . . . | 41·5 to 42·5 | 58·5 to 57·5 |
| East India refined sugar (*v*) | 42·2 | 57·8 |
| Maple sugar (*v*) . . . . . | 42·1 | 57·9 |
| Beet-root sugar (*v*) . . . . | 42·1 | 57·9 |
| East India moist sugar (*v*) | 40·88 | 59·12 |
| Sugar of diabetic urine . . | 36 to 40 ? | 64 to 60 ? |
| Sugar of Narbonne honey | 36·36 | 63·63 |
| Sugar from starch . . . . . | 36·2 | 63·8 |

On some of these it may be necessary to make a few remarks. The *sugar candies* of the shops frequently contain minute quantities of foreign fixed bodies, such as lime, &c., as well as others of a destructible character. Both the specimens of *India sugar candy* I examined were obviously impure to the eye, being of a brown colour and deliquescent; they contained, among other things, traces of potash. The *East India refined sugar* was perfectly white, but rather soft and friable, and it did not possess the fine and brilliant grain

* I observed that after this sugar had been cautiously melted it might be preserved in the state of a transparent fluid, if placed in a perfectly dry atmosphere, as under the receiver of an air-pump with sulphuric acid; but that in a few hours after exposure to the air, it began to grow opaque and assume the crystalline form, by attracting moisture. Is not this precisely analogous to the deterioration which is known to take place in the sugars of commerce? See Mr. Daniell on this subject in the Royal Institution Journal, vol. xxxii. Dr. Ure supposes that this deterioration depends on the absorption of oxygen; but I have hitherto met with no sugar containing an excess of oxygen.

† In these results *fixed* bodies only have been allowed for, and those marked (*v*), as occurring in commerce, are probably subject to slight variations in their composition.

of the best refined sugars of commerce. For a specimen of the *maple* sugar I was indebted to Mr. Faraday; this, when I received it, was very impure and deliquescent, but by treating it by the process above alluded to, a portion was separated that differed but little in its appearance from cane sugar. The *beet-root sugar* was made and refined in France; it was perfectly white, but rather soft and fine in the grain. The *East India moist sugar* was of a very low kind, and known in commerce by the name of *Burdwan sugar;* it was deprived of its hygrometric moisture before analysis by exposure to sulphuric acid under a receiver. The *diabetic sugar* was prepared as above; the results given were obtained many years ago, and I have had no opportunity of repeating the analysis with the present apparatus; I believe however that diabetic sugars in general belong to the honey variety. The *sugar of starch* was prepared by myself in the usual manner.

### *Of Amylaceous Principles.*

Before we proceed to consider the analysis of amylaceous bodies, a few remarks on the nature of these and similar substances may not be deemed improper. It has been known from the very infancy of chemistry, that all organized bodies, besides the elements of which they are essentially composed, contain minute quantities of different foreign bodies, such as the earthy and alkaline salts, iron, &c. These have been usually considered as mere mechanical mixtures accidentally present; but I can by no means subscribe to this opinion. Indeed, much attention to this subject for many years past has satisfied me that they perform the most important functions; in short, that organization cannot take place without them. This point will be more fully investigated hereafter: at present it is sufficient merely to observe, that many of those remarkable changes which crystallized bodies undergo on becoming organized, are more apparent than real; that is to say, their chemical composition frequently remains essentially the same; and the only points of difference that can be traced, is the presence of a little more or less of water, or the intimate mixture of a minute portion of some foreign fixed body. There is no term at present employed which expresses this condition of bodies, and hence, to avoid circumlocution, I have provisionally adopted the term *merorganized**, (μέρος *pars* vel *partim*,) meaning to imply by it that bodies on passing into this state become partly, or to a certain extent, organized. Thus starch I consider as *merorganized* sugar, the two substances having, as we shall see

* I am indebted to my friend Mr. Lunn for this term.

presently,

presently, the same essential composition, but the starch differing from the sugar by containing minute portions of other matters, which, we may presume, prevent its constituent particles from arranging themselves in the crystalline form, and thus cause it to assume totally different sensible properties*.

*Wheat Starch.*—The most perfect form of the amylaceous principle is undoubtedly that derived from wheat. This has been analysed by different chemists with very different results. MM. Gay Lussac and Thenard state that they found it to contain as much as 43·55 per cent. of carbon; while Dr. Ure informs us that he only found 38·55 per cent. The following observations will sufficiently explain these differences.

A very fine specimen of wheat starch, which had been prepared expressly at my desire without the addition of the colouring matter commonly added to the starch of commerce, and which had been kept in a dry situation for many months, was found, in the ordinary columnar form in which it usually occurs, (abstracting foreign matters,) to consist of

| | |
|---|---|
| Carbon...... | 37·5 |
| Water ...... | 62·5 |

One hundred parts of the same specimen reduced to a state of fine powder, and subjected to a temperature between 200° and 212°, for the space of twenty hours†, lost, in a mean of two

* When this subject first occupied my attention many years ago, I was at a loss to form any notion of the *modus operandi* of these minute admixtures of foreign bodies, except the mechanical one mentioned in the text, viz. that they operated by being interposed, as it were, among the essential elements of bodies, and thus by weakening or modifying their natural affinities. But the admirable paper, published by Mr. Herschel, in the Philosophical Transactions for 1824, "On certain motions produced in fluid conductors when transmitting the electric current," appeared to throw an entire new light on the subject. The facts brought forward in this paper are of the most important kind, and seem to me to be evidently connected with a principle of a more general character, which when completely developed, will lead to the most unexpected results. "That such minute proportions of extraneous matter," says Mr. H. "should be found capable of communicating sensible mechanical motions and properties of a definite character to the body they are mixed with, is perhaps one of the most extraordinary facts that has yet appeared in chemistry. When we see energies so intense exerted by the ordinary forms of matter, we may reasonably ask what evidence we have for the imponderability of any of the powerful agents to which so large a part of the activity of material bodies seem to belong?"

Any substance may be supposed capable of performing the part of a merorganizing body; but, in a certain point of view, *water* appears to constitute the *first* and *chief*, at least in organized substances.

† I have reason to believe from other experiments that six or eight hours, or even less, of steady exposure to the boiling temperature, will sometimes reduce both starch and arrow root, and even gum, to this state of desiccation.

two experiments, 12·5 parts, and on being analysed in this state gave Carbon...... 42·8
Water....... 57·2

which very nearly coincides with what by calculation it ought to have given, on the supposition that the loss of weight was owing to the escape of water, a circumstance indeed of which there could have been little doubt. Starch however in this state still retains water, a portion of which may be separated by subjecting it to higher temperatures. Thus, after having been exposed as above for twenty-four hours to the temperature of 212°, on being further submitted to a temperature between 300° and 350° for six hours longer, it lost 2·3 per cent. more, and analysed in this state gave very nearly

Carbon...... 44
Water....... 56

It had now acquired a slight yellow colour, and seemed to have suffered some change in its properties; hence, this is probably nearly the utmost quantity of water that starch is capable of parting with, short of decomposition.

*Arrow root.*—This is another variety of the amylaceous principle, of which, like sugar, there seems to be a great variety. The specimen on which the following experiments were made was remarkably fine, and free from adventitious matters. It had been kept in the same drawer with the starch before mentioned, and under precisely similar circumstances of the atmosphere was found to consist of (abstracting foreign matters)

Carbon...... 36·4
Water ...... 63·6.

One hundred parts, in the above state, exposed for twenty hours to a temperature between 200° and 212°, lost fifteen parts. Hence its composition, when thus dried, was very nearly the same as that of wheat starch similarly exsiccated; or it consisted of Carbon ...... 42·8
Water ...... 57·2.

On being subjected to the full temperature of 212° for six hours longer it lost 3·2 per cent. more, and was then reduced to a state similar to that of starch dried between 300° and 350°, or it consisted very nearly of

Carbon...... 44·4
Water...... 55·6

When subjected to the temperature of 300° and 350° for six hours longer, it lost 1·38 per cent. more of its weight, but became of a deeper yellow colour than starch similarly exposed, and consequently showed greater marks of decomposition. Hence, this form of the amylaceous principle, like the sugar of honey before mentioned, seems to part with the whole of the

the water not essential to its composition at the temperature of 212°, or even perhaps below this point if exposed for a period sufficiently long.

It may not be deemed superfluous to notice here very briefly two or three circumstances resulting from the above analyses, which, though their importance may not be seen at present, should be constantly borne in mind, as they will enable us hereafter to throw light on many points connected with organization, which otherwise would be inexplicable.

In the first place, the identity of composition between the sugar of honey and arrow root, under the ordinary circumstances of the atmosphere, seems to show that the differences among the varieties of the amylaceous principles are precisely analogous to those existing among sugars, or in other words, that there are *low* starches as well as *low* sugars. Whether arrow root be the lowest that exists, I am unable to say; but I have met with none lower; and have reason to believe that the greater portion of the other varieties of the amylaceous principle known to exist, like the varieties of sugars above given, are intermediate in their composition between arrow root and wheat starch. The same remarks apply to other merorganized principles.

In the second place, the identity of composition between wheat starch and cane sugar, and between the sugar of honey and arrow root above mentioned, seems to show that, though merorganized bodies are not actually capable of assuming the crystalline form, yet that the original tendency among their essential elements to combine in certain proportions (and perhaps to assume certain forms) still continues to operate, though in a mitigated degree, and thus to exert, as it were, a feeble *nisus*, or endeavour toward the maintenance of certain definite modes of existence.

Thirdly, and lastly, crystallized bodies usually part with their water of crystallation with difficulty, and when they do, it is commonly *per saltum*, or in definite quantities. Merorganized bodies, on the other hand, retain water so feebly at all points, that within certain limits this fluid may be readily separated, or made to combine with them in every proportion. And this appears to be true, not only with respect to water, but with other substances capable of combining with merorganized bodies. It may be remarked also in general, that *low* varieties of principles resemble merorganized bodies in these and some other respects; thus, they usually part readily with all the water not essential to their composition at the temperature of 212°, or even less (provided they be submitted to it

 long

long enough,) above which point they rapidly undergo decomposition, &c.

*Lignin, or the Woody Fibre.*

Messrs. Gay Lussac and Thenard first showed that the hydrogen and oxygen in this principle exist in it in the proportions in which they form water, a result fully confirmed by my experiments. The variety of forms in which lignin occurs in different woods is so great, that an examination of them all would be quite out of the question; I therefore selected two, viz. the woods of the *Box* and *Willow*, which appeared to present the greatest contrast; the one being among the densest, the other the lightest of the woods. These were both treated exactly in the same manner; that is to say, they were first reduced to the form of a coarse powder by rasping, then well pulverized in a Wedgwood mortar, and afterwards sifted. Being by these means reduced to the form of impalpable powders, they were boiled in repeated portions of distilled water till that fluid came off unchanged: a tedious process, requiring several days to accomplish perfectly. After this they were similarly treated with alcohol, and finally again with distilled water. They were now exposed to the atmosphere, when in a dry and favourable state; and when they ceased to lose weight were submitted to analysis, and found to consist of (abstracting foreign matters)

| | Box. | Willow. |
|---|---|---|
| Carbon | 42·7 | 42·6 |
| Water | 57·3 | 57·4 |

A known weight of each was then exposed for twenty-four hours to a temperature of 212°, and afterwards for six hours longer (by means of an oil bath) to a temperature between 300° and 350°; and at the end of this time they were found to have lost, per cent.

| Box. | Willow. |
|---|---|
| 14·6 | 14·4 |

Analysed in this state of desiccation, they were found to consist of

| | Box. | Willow. |
|---|---|---|
| Carbon | 50·0 | 49·8 |
| Water | 50·0 | 50·2 |

showing that the loss of weight arose from the escape of water. These latter results nearly agree with those of MM. Gay Lussac and Thenard, as obtained from the analyses of the woods of the *Oak* and *Beech*, and seem to show beyond a doubt, that the composition of all of them is similar, or that they consist of equal weights of carbon and water; to which simple analogy this important principle probably owes its stability.

*Lignin* undoubtedly exists in many other forms besides the woody

woody fibre; indeed it appears to constitute the skeleton or ground work on which most organic processes in the vegetable kingdom are carried on. To illustrate its properties as an *aliment*, the only point of view in which we have to consider it here, I shall briefly quote the experiments of Professor Autenrieth, of Tubingen, who showed some years ago, that by proper management this principle might be rendered capable of forming bread. The following was the method he employed for this purpose. In the first place, every thing that was soluble in water was removed by frequent maceration and boiling. The wood was then reduced to a minute state of division; that is to say, not merely into fine fibres, but actual powder; and after being repeatedly subjected to the heat of an oven, was ground in the usual manner of corn. Wood thus prepared, according to the author, acquires the smell and taste of corn flour. It is however never quite white, but always of a yellowish colour. It also agrees with corn flour in this respect, that it does not ferment without the addition of leaven, and in this case sour leaven of corn flour is found to answer best. With this it makes a perfectly uniform and spongy bread; and when it is thoroughly baked, and has much crust, it has a much better taste of bread than what in times of scarcity is prepared from the bran and husks of corn. Wood flour also, boiled in water, forms a thick tough trembling jelly, like that of wheat starch, and which is very nutritious*.

It may be remarked that all the preceding principles are capable of being converted into oxalic acid by the action of the nitric acid, and into sugar by the action of dilute sulphuric acid.

### *Acetic Acid, or Vinegar.*

This principle seems to have been more or less used as an aliment, either by accident or design, in every age and country. There have been various analyses of it published, by different chemists; but it is singular, that although some of them have given its exact composition, no one seems to have been struck with the most remarkable peculiarity of its composition†, viz. that

* See the Edinburgh Magazine for November 1817, p. 313, where an account is also given of the Lapland mode of making bread from the bark of trees, as described by Von Buch. It is not improbable that during the above processes the lignin combines with water, and forms an artificial starch.

† Berzelius, in his paper On the definite proportions, in which the elements of organic nature are combined, assigns to vinegar this composition. See Annals of Philosophy, v. 174 (O.S.) Dr. Thomson also, in the last edition of his Chemistry, gives the same composition; though in his more recent work he has assigned to it another proportion of hydrogen.

that the hydrogen and oxygen exist in it in the proportions in which they form water. Some experiments which I made many years ago appeared to render this probable; but from the difficulties attending the analysis of this acid, and the uncertainty arising from the properties of the oxide of copper formerly stated, I was unable to satisfy myself completely on the subject. On repeatedly burning, however, a very fine specimen of the acetate of copper, in a given bulk of oxygen gas, with the apparatus described at the commencement of this paper, it was found that the volume of the gas underwent no change, and hence, that the above opinion was correct.

Acetic acid, freed from non-essential water, I find to be composed of

| | |
|---|---|
| Carbon . . . . . | 47·05 |
| Water . . . . . | 52·95 |

results which almost exactly agree with those of other chemists.

*Sugar of Milk.*

The sugar of milk employed in these experiments was prepared by myself in the usual manner, and rendered as pure as possible by repeated crystallizations. It was then freed from its hygrometric moisture by confinement under a receiver with sulphuric acid, and was found to consist of

| | |
|---|---|
| Carbon . . . . . | 40 |
| Water . . . . . | 60 |

results almost exactly agreeing with those of Berzelius.

*Manna Sugar.*—The saccharine principle existing in manna has been long known to possess peculiar properties. That employed in the following analysis was separated by means of alcohol in the manner commonly described in chemical books, and was obtained in a state of perfect purity by repeated crystallizations from that fluid. It was then dried at 212°, and in this state was found to consist of

| | |
|---|---|
| Carbon . . . . . | 38·7 |
| Water . . . . . | 61·3 |

results very different from those of M. Theodore de Saussure*. This sugar seems to part with hygrometric water only at the temperature of boiling water; but a few degrees above this point it begins to suffer decomposition, and at 250° it assumes, without melting, the form of a brown powder, and acquires a strong empyreumatic odour.

*Gum Arabic.*—A very fine specimen of gum arabic reduced to powder, and analysed as it existed under the ordinary cir-

* See *Bibliothéque Britannique*, 1814; also Annals of Philosophy, vi. 424.

cumstances

cumstances of the atmosphere, was found (abstracting foreign matters) to consist of

| | |
|---|---|
| Carbon . . . . . | 36·3 |
| Water . . . . . | 63·7 |

One hundred parts of the same gum, exposed to a temperature between 200° and 212°, for upwards of twenty hours, lost 12·4 parts. Hence its composition thus dried would be nearly

| | |
|---|---|
| Carbon . . . . . | 41·4 |
| Water . . . . . | 58.6 |

results confirmed almost exactly by actual analysis.

The same gum, further exposed to a temperature between 300° and 350° for six hours longer, assumed a deep brown colour, and seemed to have suffered decomposition, though it lost in weight only 2·6 per cent. more. Hence, gum probably parts with the whole of the water not essential to its composition at the temperature of 212°, provided it be exposed for a sufficient time to this degree of heat.

Substances belonging to this series appear in general to be of a weak or *low* kind, though they are probably very numerous. They may be readily distinguished by being converted into saclactic acid by the action of nitric acid.

*The vegetable Acids.*

*Oxalic Acid.*—Many years ago I ascertained that this acid in the crystallized state consists of

| | |
|---|---|
| Carbon . . . . . | 19·04 |
| Water . . . . . | 42·85 |
| Oxygen . . . . . | 38·11 |

a composition assigned to it long since by other chemists, and now I believe generally admitted, except by Dr. Thomson, who informs us that he has met with a specimen containing as much as half its weight of water*. I have examined a great many specimens with the view of verifying this result, but hitherto have not been successful.

*Citric Acid.*—This and all the following acids, except the *malic*, were analysed at the same period as the oxalic acid above mentioned, and the results have been recently verified. I find the crystals of citric acid to consist of

| | |
|---|---|
| Carbon . . . . . | 34·28 |
| Water . . . . . | 42·85 |
| Oxygen . . . . | 22·87 |

This composition has been approached very nearly by several chemists; but no one, so far as I know, has given it exactly.

* Attempt to establish the first principles of chemistry by experiment, ii. 103.

*Tartaric*

*Tartaric Acid* in crystals is composed of

| | |
|---|---|
| Carbon | 32·0 |
| Water | 36·0 |
| Oxygen | 32·0 |

a composition assigned to it by Dr. Thomson in his work just quoted.

*Malic Acid.*—I am not acquainted with any analysis of malic acid except that of M. Vauquelin*, which has not, I believe, obtained much confidence among chemists, chiefly on account of the large proportion of hydrogen which he assigns to it. The acid I employed was obtained from the berries of the mountain ash by a process very similar to that of Mr. Donovan. It was not analysed *per se*, but in combination with lead, with lime, and with copper, and was found, abstracting water not essential to its composition, to consist of

| | |
|---|---|
| Carbon | 40·68 |
| Water | 45·76 |
| Oxygen | 13·56 |

This acid, in many points of view, may be regarded as one of the most interesting and important of all the vegetable acids.

*Saclactic Acid.*—The unexpected composition of this acid induced me to investigate its properties more fully than I had otherwise intended. What I first employed was obtained from the sugar of milk, and hence was tolerably pure, though not perhaps completely so. Latterly, I have preferred that prepared from gum, which, though exceedingly impure as first obtained, may be easily and completely purified by the following simple process.

Add ammonia in slight excess to the impure acid, and afterwards as much boiling distilled water as will dissolve the saclactate formed. Filter the solution while boiling hot, and then evaporate it very slowly nearly to dryness. The saclactate of ammonia will be separated in the form of crystals, which are to be washed with cold distilled water till they become quite white and pure. They are now to be again dissolved in distilled water, and the boiling saturated solution permitted to drop from a filter into cold diluted nitric acid. This latter of course decomposes the saclactate, and precipitates the saclactic acid in a state of perfect purity. Thus obtained, this acid was found to consist of

| | |
|---|---|
| Carbon | 33·33 |
| Water | 44·44 |
| Oxygen | 22·22 |

* *Ann. de Chimie et de Physique*, tom. vi. 337.

results

results differing a little from those of other chemists, who probably did not take the necessary pains to obtain this acid in a perfectly pure state.

In conclusion, I wish to observe, that I purposely abstain at present from making any further observations on the preceding results than those already given. I do this for several reasons: in the first place, such observations will appear with greater effect, when the whole of the facts in my possession are laid before the public; and secondly, I consider that data which lead to such important conclusions as these appear to do, cannot be too firmly established; I therefore, in the mean time, earnestly invite chemists in general to repeat them, and thus either to confirm them, or point out their errors; and for the sake of those who may be inclined to take this trouble, I shall close this part of the subject with the following remarks: 1. The multiples of hydrogen, carbon, and oxygen, are assumed in the preceding calculations as 1 : 6 : 8. 2. The results given are, on all essential points, the means of many experiments, the differences among which are either inappreciable, or at most vary from ·01 to ·03 of a cubic inch in from 5 to 8 cubic inches of carbonic acid or oxygen gas; the greatest differences in general being, for obvious reasons, found among merorganized bodies; and hence the analyses of these are usually stated to the first decimal figure only. 3. As rules to be observed, I would say, that a single result should never be registered, nor a single calculation made, till the operator has made himself complete master of his apparatus, and carefully studied the nature of the substance to be analysed; for different substances often require very different management: that two or three results should never be relied on; the minute quantities here sought can be only obtained, like those of astronomy, by repeated observations: and lastly, the utmost care should be taken that the substances operated on be *pure*, a point of greater importance, and frequently of more difficult accomplishment than any other, and one that has caused me more trouble than all the rest put together.

## XVIII. *On the Means of ascertaining the Purity of Sulphate of Quina.* By R. Phillips, *F.R.S. L. & E. &c.*

THE great demand which has arisen for this important medicine, and the high price at which it is necessarily sold, have excited some, who are careless as to the means by which they acquire gain, to sophisticate it in a vast number of ways, and by every means which talent misapplied could suggest.

Having

Having repeatedly of late been requested to examine various samples of sulphate of quina, I thought it might be useful to state the several modes which may be employed for that purpose: and I make the present communication with the greater confidence, because I have received the very able assistance of my friend Mr. John T. Barry, of Lombard-street, to whose chemical skill, and the opportunity of frequently applying it, I am indebted for the greater number of hints and facts detailed in this paper.

Pure sulphate of quina has the form of minute fibrous crystals, it is inodorous, and its taste is bitter. If certain vegetable products, such as starch or sugar, be mechanically mixed with it, they may possibly be observed by merely inspecting the preparation with a glass.

1st. If the sulphate of quina be mixed with a considerable proportion of foreign matter, it may probably be detected by dissolving the salt in question in about three hundred times its weight of water,—say one grain in about five fluid drams of boiling distilled water. On cooling, pure sulphate of quina will be deposited in feathery crystals in twenty-four hours, if there be no adulteration.

2dly. As indirect, but as good collateral evidence, the taste of sulphate of quina of known good quality may be compared with that of another sample. Thus when pure, a grain of sulphate of quina will render nearly a pound and a half of water, or 10,500 grains, sensibly bitter.

3rdly. The alkalies either pure or their carbonates, if but slightly in excess, always occasion precipitation at ordinary temperatures in a solution of sulphate of quina containing only 1-1000dth of its weight, or less than one grain in two fluid ounces of water.

4thly. A solution of tannin occasions a very sensible precipitate in an aqueous solution of sulphate of quina, containing only 1-10,000dth of its weight of the salt, provided there be no acid in excess. Kino is that form of tannin which best answers the purpose. It is however to be observed, that the salts of morphia, cinchonia, strychnia, &c. are similarly affected by tannin; but they are not likely to be mixed with sulphate of quina.

5thly. Sulphate of quina suspected to contain sugar, gum, or other substances soluble in cold water, may be tried by digesting the same portion of the salt in small and successive portions of water to saturation. If the sulphate of quina be pure, and the solutions all properly saturated, they will have the same taste and specific gravity; and similar portions will yield by evaporation equal quantities of solid residuum.

6thly.

6thly. A repetition of the above process, substituting alcohol for water, answers for extracting resin and some other substances, because sulphate of quina is soluble in alcohol to only a limited extent.

7thly. If a white substance insoluble in cold water be found in the sulphate of quina, heat the mixture to about 170° of Fahrenheit. This will render starch soluble, and its presence may be determined by the addition of an aqueous solution of iodine, which will immediately occasion a blue colour, and eventually a blue precipitate. The iodine should be added in very small quantity.

8thly. Sulphate of quina has been adulterated with ammoniacal salts. These are rendered obvious by adding a little of the suspected salt to a solution of potash. If any ammoniacal salt be present, ammoniacal gas will be readily detected, either by the smell, or by holding over the mixture a piece of turmeric paper, or a bit of glass moistened with acetic acid.

9thly. To ascertain whether sulphate of quina contains any earthy salts, such as sulphate of magnesia or sulphate of lime; burn a portion of it in a silver or platina crucible, or even in a clean tobacco-pipe. Any earthy salt, or any matter indestructible by heat, will of course remain in the vessel.

10thly. To ascertain that the sulphate of quina contains the proper quantity of sulphuri cacid and quina, dissolve a little in pure muriatic or nitric acid, and add a solution of muriate or nitrate of barytes: 60 parts should give about 17·3 to 17·4 of sulphate of barytes; or the method may be varied without the trouble of drying the precipitate. Dissolve 60 grains of sulphate of quina in water slightly acidulated with muriatic or nitric acid; add a solution of 18 grains of nitrate of barytes, and separate the precipitated sulphate of barytes by filtering. If nitrate of barytes be now added to the clear solution, it should still occasion slight precipitation, for 60 of sulphate of quina contain 5·8 gr. of sulphuric acid, equivalent to 19·1 of nitrate of barytes.

This test is only to determine that there is no crystallized vegetable matter uncombined with sulphuric acid in the sulphate of quina; the detection of earthy or alkaline sulphates has already been provided for.

11thly. Sulphate of quina should lose not more than from 8 to 10 per cent of water by being heated till deprived of its water of crystallization. Mr. Barry informs me that he once examined a sample which contained more than 40 per cent of water in excess diffused through it.

## XIX. *On the Phænomena of Water-spouts.* By Mr. James Main*.

IN sailing from Madras to China, the usual course is through the Straits of Malacca and Sincapore. On the passage from Pulo-penang (*i. e.* the Island of areca-nut trees) to Malacca, the prospect from the ship was interesting: to the right, the thinly inhabited island of Sumatra was distinctly seen covered by impervious jungle, with lofty and thickly growing woods, even to the water's edge. To the left, the kingdoms of Quida, Malay, and Siam, on the continent of Asia, presented the same aspect; but, unlike many parts of the torrid zone, beautifully verdant and refreshing to the eye which had just left the sultry atmosphere and comparatively thinly planted coast of Coromandel. During the passage through those scenes, the navigation was tedious, and the weather uncommonly unsettled; the heat of the air was generally from 75 to 85 by Fahrenheit's thermometer. Occasional and insufferably hot gleams of sunshine, raised from the sea; and especially from the wood-covered and damp surface of the adjacent lands, a most copious evaporation; surcharging the air, and suddenly forming vast accumulations of clouds, all highly charged with electricity. In all directions lightning descended, followed by heavy and impetuous rain. The surrounding air and clouds converging to the spaces thus left unoccupied, generated various and contrary currents of air, wheeling the clouds in violent commotion; partial tornados were consequently created: these by their vertiginous course affected the adjacent and surrounding vapours, drawing them into the vortices; the grosser parts of this whirling body of vapour naturally inclined to the centre of the tornado, and there coalescing, formed the aqueous column called a water-spout.

The first appearance of this phænomenon is the lower end of the column impending from the base of a dark cloud in the shape of an inverted cone, in a somewhat waved direction, descending gradually to the sea or earth. When it happens to descend on the former, it is plainly discernible that a considerable body of air below and around the spout partakes of its dinetical motion; because the water is violently agitated therewith for a considerable time before the point of the cone reaches the surface; and during its approach, a cylindrical body of thick spray of much greater diameter than the spout, is seen raised from the waves, and appears to meet it in its descent; and when in collision, the agitation is extreme. The contact continues for ten or twenty minutes, according to the size of the spout;

* Communicated by the Author.

and

and when exhausted, the lower end becomes broken, less defined, and shrinking as it were upwards, disappears as it began.

Some travellers have reported, that water-spouts rise out of the sea; having observed them, perhaps, only when the spout had descended and was in contact with the surface; or it might happen, that in such a case the tornado might be first generated below; and before the clouds were acted on, a column of spray would be seen to rise and approach the clouds, before a column of water is formed in them of sufficient density to descend. Similar appearances are sometimes seen on shore, when columns of dust, or, as the writer has often witnessed, even cocks of hay are carried up into the air by whirlwinds to a great height.

So frequent were these meteorous appearances during this part of the voyage, that on one occasion (when in sight of "The Rabbit and Coney," two small islands in the Straits, well known to navigators), no less than five water-spouts were seen from the ship at the same instant! some of which were falling on the land, and others on the sea;—the nearest to the ship being computed to be at the distance of five miles. The writer, who attentively observed these phænomena, used an excellent perspective glass, and on one occasion had an opportunity of seeing one while the sun shone upon it (about 7 A.M.), which appeared to be tubular; as the sun's rays were reflected in a double parallel stripe along that part of the spout on which they shone, viz. one stripe reflected from the exterior side next the sun, and another less bright from the interior surface (as was supposed) of the opposite side. This, however, is only a surmise; but from similar motions of fluids which may be seen at any time, there may be reason for supposing that the spout is not solid: for instance, if a hole be made in the bottom of any vessel, and plugged up with the end of a stick; let the vessel be filled with water and the plug withdrawn, the water in escaping through the aperture soon assumes a circular or rather a spiral motion, the descending column draws, or more properly speaking, admits after it an inverted cone of air, reaching from the surface of the water to the hole at which it escapes.

The idea of preventing the approach (for these phænomena are seldom stationary) of a water-spout upon a ship (which might produce dreadful consequences), by discharging great guns towards it, is reasonable; and if it did not approach too rapidly, no doubt would be effectual; because the concussion from the gun would counteract and break the sweeping current of the tornado, and thereby dissipate the motion-embodied fluid, and of course prevent its effects.

Many ideas occur to the beholder of such "elemental strife." Moses hath told us, that soon after the creation "there went up a mist from the earth, which watered the whole face of the ground;" and this necessary alternation of the ascent and descent of the element of water has continued ever since. The aspiring agency of heat, both from the earth and from that daily borrowed of the sun, is apparently the cause of the ascent of humidity; yet without the assistance of some other no less powerful agent, it is difficult to account for all the phænomena connected with the ascent and descent thereof. For though most copious between the tropics, and in our summer months, the lowest degree of temperature even within the arctic circle does not materially check evaporation, and the assumption by aqueous vapour of an invisible state in the atmosphere takes place as well in winter as summer. We find when the atmosphere is most fully charged with aqueous vapour, and has its temperature above the dew point, and consequently not a cloud to be seen; when almost all of it is exhaled from the surface of the earth; when vegetables and even animals languish,—the barometer is high: but when this sustaining power becomes diminished or withdrawn, a change takes place, a sensible precipitation of moisture (erroneously called dew) is felt, the air becomes turbid, clouds are formed, lightning is seen, thunder heard, and rain descends. The barometer, eased of its burden, sinks to the point of rain, or snow, or storm; and to this succeeds "a showery time," which continues till the atmosphere is again charged with that power which keeps its vapour in solution, and restores its transparency. Is electricity this agent?

These meteorous changes, so interesting to mankind, always suggest and force upon us the wish to obtain an efficient weather-glass; such an one as would be affected by the existence and motions of those powers which cause the changes in the atmosphere which are out of the reach of our perceptions. Perhaps an electrometer which would indicate the quantity, but especially the character and *direction* of that subtile fluid (if such an instrument could be constructed), would be of the greatest service in foreshowing the changes of the weather.

The above description and remarks are from actual observation. No reference has been made to any book, or other authority on the subject; and whether the writer has been deceived by appearances, and been led to erroneous conclusions as to the cause of them, must be left to the judgement of the reader.

6, Union Row, Chelsea,
April 21, 1827.

## XX. *On the Nursing Pouch or Chamber of the* Chama concamerata *of Gmelin. By* John Edw. Gray, *Esq. F.G.S. &c.**

WALCH in the 12th volume of the *Naturforscher Journal*, of Berlin, described and figured (*t.* 1. *f.* 7.) a very curious shell, which Chemnitz has since named *Chama concamerata.* It has been considered as very rare; but having lately been brought to this country in considerable abundance, —through the kindness of Mr. Pratt, of Bath, I have been enabled to examine several specimens which had the dried animal in them: and as the formation of the cavity, which is itself an anomalous structure in Mollusca, and the use to which it appears to be applied by the animal, has not hitherto been described, I shall proceed briefly to describe them.

The cavity is formed by a folding-in of the middle of each of the valves; and this folding-in does not appear to take place till the shell has arrived at the middle period of its growth. It is marked externally by a groove formed by the coming together of the sides of the fold; the parietes of the cavity are thin, and marked with the same lines of growth as the valves themselves; the cavities or folds of both the valves are exactly opposite, and when the valves are closed, they meet so as to form a nearly closed chamber, which, although in the internal cavity of the shell, is completely external to the animal; and the contents of the cavity, like when the head is in a double nightcap, are on the outside of the shell. Indeed, the manner in which the cavity is formed may be easily understood by folding a piece of paper to the shape.

The chamber appears to be used by the animal as a *nursing pouch* to contain its eggs; for in all the specimens which I have seen, the cavity of each of the valves contained a group of oval, crumpled, pellucid bodies, adhering together, which, when they were soaked in water under the glass of a microscope, dilated, became semi-transparent, regularly oval, and had all the appearance of the eggs of bivalve Mollusca; but they are rather large for the size of the shell.

This is indeed a remarkable anomaly; for most *Conchiferæ*, as was described by Lister, aërate their eggs in their gills, and then emit them into the water to the protection of Nature. Some freshwater species, as the *Cyclades*, indeed, appear to keep them in the concave part of their shell, so as to be viviparous. I do not know of any instance but the above, where a pouch appears to have been formed for their protection; and what is more remarkable, the other species of the genus *Cardita*, to which Lamarck has referred this shell, are undoubtedly destitute of any thing of the kind.

* Communicated by the Author.

XXI. *On*

## XXI. *On an Apparent Violation of the Law of Continuity.* *By* P. M. Roget, *M.D. F.R.S.**

It cannot fail to strike every philosophical observer of nature, that the greater number of changes which take place in the universe are effected only in a gradual manner; that each event is connected by insensible differences, both with the one which immediately precedes, and the one which immediately follows it; and that we may hence infer that the whole series of mutations which constitute any observed change, is perfectly uninterrupted and continuous. With regard to those changes which appear at first sight to be more suddenly effected, a careful analysis of the whole of the phænomena, and a more minute subdivision of the time during which they occur, will enable us to perceive the operations of the same general principle of successive and intermediate gradation of condition necessarily existing between opposite states of being. Thus, the apparently sudden impulse communicated to a ball by the explosion of gunpowder is, in reality, as much the result of a gradual communication of motion, during which the ball passes successively, though with great rapidity, through every intermediate degree of velocity, as in the case of the slowest increase of motion, which it might acquire by the constant operations of the greatest accelerating force. It was by extending these views to a variety of subjects of physical science, that Leibnitz was led to the conclusion, that the same principle pervaded every department of nature, and that *the Law of Continuity*, as he called it, was the law of the universe. *Natura non operatur per saltum* was his motto; implying that every thing that is executed in nature is done by indefinitely small degrees. Boscovich has assumed this principle as the foundation of his ingenious and profound theory of natural philosophy, and deduced from it a variety of important corollaries and conclusions. The law of continuity, says that philosopher, consists in this, that any quantity, whilst passing from one magnitude to another, must pass through all the intermediate magnitudes of the same kind: or, according to the law of continuity, all changes in nature are produced by insensible and infinitely small degrees; so that no body can in any case pass from motion to rest, or from rest to motion, without passing through all possible intermediate degrees of motion.

* From the Scientific Gazette, Nos. 19 and 20, for November 5 and 12, 1825.—As this work has long since been discontinued, and was of confined circulation, we think Dr. Roget's interesting paper will be acceptable to the readers of the Philosophical Magazine.—Edit.

The

The simpler illustrations of this law are obvious, and must readily occur to those who seek for them; but it is important to familiarize the mind to them, with a view of preparing ourselves for the inquiry to what extent the principle admits of being carried, and whether it be really, as it is pretended, a universal law. It appears to obtain without exception in all those physical changes of situations, qualities, and conditions, which are connected with one another by their mathematical relations. All the forces and powers of nature vary by a continuous gradation from one period of time to another, or undergo differences of degree at different parts of space, without suffering any abrupt transition at any one point. Continuity is an attribute common to the law of gravitation, which acts at a distance, as well as to the laws of the corpuscular forces, of which the sphere of operation is limited to insensible distances, and which give rise to the phænomena of cohesion, of elasticity, and of all chemical combinations and decompositions. It belongs equally to the forces concerned in the phænomena of electricity and of magnetism. As the law of each of these forces may be expressed by some mathematical function of the distance at which they operate, so the same continuity must continue to pervade and characterize all the effects which can result from them, either when exerted singly, or in combination. The motions of bodies may in every case be regarded as the aggregate of the motions of every one of their particles; and the motions of any single particle, as well as of an aggregate of particles, produced by a continuous force, must be itself continuous, not only with regard to augmentations or diminutions of velocity, momentum, mechanical force, &c., but also with regard to the spaces they traverse, and the lines they describe in space. No deflection from a perfectly rectilineous course can take place but by infinitely small degrees; or, to express it in geometrical language, the line of motion cannot pass into any curve, of which that line is not the tangent at the point where the change of direction begins; far less, therefore, can a motion in one right line, suddenly pass into a motion in another right line, which forms any angle whatsoever with it. All angular motions are therefore excluded by the law of continuity, according to which abrupt transitions, either of velocity or direction, are physical impossibilities. The apparent violations of this law in the effects of the collision of elastic bodies, or their reflection after impinging on hard surfaces, and even the phænomena of the refraction and reflection of the rays of light, are shown by Boscovich to be really all in perfect conformity with the law of continuity.

Not

Not content with establishing the universality of this law by induction, Boscovich has endeavoured to prove it by other arguments, of a still more positive nature, derived from abstract considerations. This universality, he says, arises necessarily from the very nature of continuity. The limit which joins the precedent and the consequent of any thing is common to both, and is therefore indivisible. Thus, a superficies separating two solids has no thickness, and is that in which a transition from the one to the other occurs; a line, dividing two portions of a continued superficies, has no breadth; a point, discriminating two segments of a continued line, has no dimension of any kind. So it is with regard to time: for the limit of two consecutive portions is common to both and indivisible; and as every change of a variable quantity from one magnitude to another must be made in time, so every change must participate in the continuity of time. But to every moment of time, a certain magnitude of the variable quantity corresponds, and the limit of two moments of time is common and indivisible; therefore the limits of two magnitudes, corresponding to these two moments, must, in like manner, be common and indivisible. Moreover, it is impossible for any quantity to have two magnitudes at the same time, and when continually varying, that it shall have the same magnitude at different times; much more impossible, therefore, that in the limit of two moments of time it shall have two magnitudes, the one course pending to the precedent, and the other to the consequent moment; or shall not have gone through the intermediate magnitudes in the intermediate moments of time. For the same reason, a body cannot have two velocities at the same time, and therefore cannot have two velocities in the limit common to two moments of time; and when continually changing its velocity, cannot have the same velocity in different moments of time, but must go through all the intermediate velocities in the intermediate moments of time. Hence, then, in passing from the magnitude 8 to the magnitude 12, the variable quantity passes through the magnitudes 9, 10, 11. The increase or diminution of temperature, for example, goes on gradually; the mercury in the thermometer rises or falls progressively, passing through every intermediate degree from one point of the scale to another. Now as this reasoning is unaffected by any considerations of the hardness, softness, elasticity, or other physical property of bodies, the universality of the law, as resulting from the nature of continuity, is a truth independent of such considerations. From these arguments, which I have given as stated by his able expositor Professor Robison, Boscovich has concluded that the law of

continuity

continuity is essentially universal, and that a breach of it is metaphysically, as well as physically, impossible.

The severest test to which the validity of this mode of reasoning can be put, is to apply it to the pure mathematical relations of space and quantity, to which, if the law of continuity have any necessary existence in the nature of things, it ought to apply with the greatest rigour. We find, accordingly, that all the changes of magnitude in those quantities of which the value is dependent on that of certain other quantities, accompany corresponding changes in these latter quantities, in a manner strictly conformable with the law of continuity.

[To be continued.]

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## XXII. *On the Titaniferous Iron-slag of Königshütte in Upper Silesia, and on the Probability of its containing Tantalium. By Prof.* Hünefeld, *of Greifswalde**.

ALTHOUGH the investigations of Wollaston, Walchner, Rose, Du Ménil, Cordier, Vauquelin, Peschier, Berzelius, Zinken, Schrader, Karsten, and others, have shown that titanium is widely distributed, it has been nowhere found in great quantities. Wollaston found it in 1822 in the slag of the great iron-works at Merthyr Tydvil in Wales, in regular pale copper-coloured cubes†; and Walchner found the same in the slag of the pea-iron-ore of the High-furnace of Kandern in Baden‡; and Karsten, before this, in those of the Königshütte§. Karsten's observation has been but rarely mentioned, and has been especially neglected by Berzelius. It seems to me that it is particularly calculated to extend our knowledge of titanium (at least for the German chemists); and this has induced me to give an account of the titaniferous slag of the Königshütte, which I examined in 1824 at Breslau. I must however premise, that I had then no opportunity of combining the investigation, as to quantity, with that as to the nature of the substances.

The slag containing titanium, which was given to me for examination by my friend M. Müller, was thickly covered and filled with pale copper-coloured cubes of titanium; and I found that Dr. Wollaston's description was applicable to them in all respects. Peschier, it is well known, has declared it to be titanate of iron, of which, however, too little proof has been offered for it to be admitted as a fact ||.

* From Schweigger's *Jahrbuch*, N. S. Band xx. p. 332.
† See Philosophical Magazine, vol. lxii. p. 18. and lxiii. p. 15.
‡ Ibid. vol. lxvi. p. 124. § Karsten's *Archiv.* iii. 524.
|| Walchner has declared himself decidedly against this opinion, and

A portion of the slag was digested in aqua regia: the greater part of it was dissolved, with evolution of sulphuretted hydrogen gas, whilst a considerable number of the small cubes of titanium remained undissolved, having a perfect metallic lustre. But besides these, there remained in the residuum a blackish powder, containing grains and minute laminæ, nearly of the colour of silver, and having a metallic lustre, to which I shall return in the sequel. Another portion of the slag was ignited with nitre, in order to oxidate the titanium. The melted mass dissolved in water gave a beautiful green solution, which, when exposed to the air, soon assumed first a deep red, and then a dark violet colour, and at last lost all colour *, whilst portions of the protoxides of manganese and iron were deposited. Subsequently, this deposit (marked *a*) and the sides of the vessel became covered with fine crystalline iridescent laminæ and needles, probably titanate of potash precipitated by the substances having attracted carbonic acid from the air.

The residuum of the dissolved mass in the porcelain crucible was washed and dried, by which it assumed the colour of iron-rust. It was partly dissolved in muriatic acid, and thereby gave a solution of iron, and left a blackish powder (*b*). The filtered strongly alkaline re-acting liquid was evaporated, and mixed with nitric acid (*c*). After standing somewhat longer, it gave crystals of nitre, and then others of an indistinct form. They were mostly white, somewhat opalescent, not easily soluble grains of salt, cracking and decrepitating between the teeth, and which, with salt of phosphorus, were melted before the blowpipe into a clear bead, which within the flame received no colour, either by itself or mixed with tin. The solution was rendered somewhat turbid by caustic ammonia, and white flakes were deposited. Boiled and dissolved with potash, and acidulated with muriatic acid, infusion of galls precipitated it of a dark yellow; but hydrosulphuret of potash and ferroprussiate of iron gave no precipitate; which leads me to suppose that those grains of salt were *tantalate* of potash.

The fluid *c* had the following properties. With an alcoholic infusion of galls, a yellowish-orange precipitate was formed, which was dissolved on being heated; and when it was concentrated, it gave a brown liquid, which remained clear on cooling.

shown that the iron between the laminæ of the cubes of pure titanium had been mechanically interposed; whence (like Wollaston) he also explains its magnetism.

* The cracks of the crucible contained a beautiful orange-red efflorescence in needles, which were not examined. Were they manganesate of potash, or sulphuret of titanium with sulphuret of potassium? (Vid. Berz. *Jahresber.* v. 134.)

cooling. Sulpho-cyanate of potash dissolved in alcohol produced a red colour; with a greater addition, a dark rose colour; but with impure sulpho-cyanate of potash (still containing ferroprussiate of potash), a greenish white precipitate; with the ferroprussiate of potash itself a dark grass green precipitate, which was in part dissolved in the remaining liquid, giving it the same colour; and it was also dissolved by nitric acid. This last solution turned gradually brownish green *. Another solution made with muriatic acid, gave, by contact with a cylinder of zinc, the usual indications of titanium.

The black powder *b* was digested in aqua regia, which extracted the iron and manganese, leaving however a considerable proportion of a black powder, which under the burnisher gave an iron-gray streak of a metallic lustre, became inflamed even before it arrived at a red heat, and yielded a whitish substance, which, with a few deviations, bore the characters of *tantalic acid.* These deviations were probably produced by a small portion of manganese, which was indicated in the trial by the blowpipe.

I subsequently obtained, through the friend above mentioned, a larger quantity of slag containing titanium. It had a still greater quantity of large and small regular cubes of titanium, which were affixed on the outside as well as to the inner cavities of the slag. They had nearly all the same regular form and the rose copper colour, except a few very small groups, which tended somewhat to an orange tint, and had but a faint brilliancy (sulphuret of titanium?). But besides these visible crystallizations of titanium, the slag had some other interesting substances upon it.

1. Granules of metal melted into it of the shape of beans or spheres, weighing from two to thirty grains.

2. Cavities of different sizes, the sides of which, when filed, had a fine lustre as of steel.

3. Several grains of metal as it were sprinkled in the slag, of a globular or oblong shape, with a metallic lustre, and a colour between that of silver and that of tin. In their chemical properties they resembled the small lustrous metallic leaves which have been mentioned above; they scratched glass, gave a shining powder, and seemed therefore to be *tantalium*, such as we have found it hitherto described.

4. Melted grains of metal, partly globular, with a tint verging upon the colour of brass.

5. Portions of melted slag, of a dark rose red nearly. They

* See Pfaff's *Neue Versuche über das Verhalten der Titansäure gegen verschiedene reagentien* (New Experiments on the Habitudes of Titanic Acid with different re-agents); *Jahrb.* b. xv. p. 372; and Pfaff's *Analyt. Chem.* b. ii. p. 523.

all showed, on being filed, a fine metallic lustre, were very hard and tough, but not malleable, some scratching glass distinctly, others but feebly.

No. 1.—The metallic granule was attracted by the magnet, showed, when filed, a bright steel-lustre, which continued in the air; it was very hard, not ductile, but after many blows with a hammer, flew into several pieces, and showed a fine-grained fracture. These fragments could not be melted even in the most intense flame of the blow-pipe; they only became slightly diminished in lustre. Treated with muriatic acid, they yielded odorous hydrogen; the muriatic acid dissolved iron, the granule turned black, and after a longer digestion crumbled into a black dust. In aqua regia, a little more was dissolved; about five-sixths remained, which, when washed and polished with a glass stick, gave a streak of a metallic lustre similar to molybdena. When moistened, it gave a smell of hydrogen gas, such as is given by manganese. Thus it remains undecided whether this quality belongs to a small residuum of manganese, which however is probably the case, since the black powder treated with salt of phosphorus yielded a pearl of a faint amethyst colour, whilst otherwise nearly the whole covered the pearl of salt of phosphorus with a metallic pellicle; and this result remained unchanged on the experiment being continued. Treated with soda and borax, no other re-action took place but that of the salt being covered with a metallic pellicle, which remained unchanged even after the pearl had been repeatedly touched with solution of caustic potash, and a scrap of tin added to it. If, however, the granule of slag had not been sufficiently treated with aqua regia, the black metallic powder gave a blueish green substance with soda, with salt of phosphorus, a yellowish brown pearl in the external flame, a greenish one becoming clear on cooling in the inner flame, and which therefore contained iron and manganese.

After several experiments made with the black powder, I could perceive, at least in this manner, no indication of titanium; but another granule, also treated with aqua regia, left undissolved small rose copper coloured crystals of titanium. Another piece of a melted granule in the titaniferous slag gave on the filed surface visible particles of titanium, as it were sprinkled among it. The granule just described could be filed like the other portions described in what follows, adhering a little to the file.

No. 2.—These parts proved to be almost entirely the same as the former, but were a little harder, and yielded more of the black metallic powder.

No. 3.—They scarcely lost any thing in aqua regia, preserved their

their lustre, and did not crumble. Similar granules remained behind in another experiment with the same solvent, scratched glass, and when beaten and broken with a hammer, made red hot with caustic potash, and treated with the blowpipe apparatus as well as with humid re-agents, did not give any indications of titanium. A portion, however, having been melted with potash, and acidulated with nitric acid, the solution remained for several days exposed to the air, and being afterwards filtered, was by means of a solution of galls precipitated rather abundantly, of a dirty orange colour; whilst hydrosulphuret of potash produced a scarcely perceptible turbidity (*tantalium?*).

No. 4.—The brass colour was only superficial, the mass underneath almost entirely resembling Nos. 2 and 3.

No. 5.—The dark rose red, yellowish, and metallic colour of these pieces extended deeper in the last-mentioned portions; they evidently contained titanium, were more difficultly filed, were magnetic, and might contain a mixture of iron and titanium on the surface, one of tantalium and iron, and some titanium itself further in.

The black powder obtained from the portion of slag of the other mass, was ignited with carbonate of soda; the mass, which when cold appeared white with blueish green margins, was dissolved in water, in which the green colour, which is the property of manganese, disappeared. A white, somewhat loose and flaky powder was precipitated. The remaining fluid, mixed with muriatic acid, when treated with oxalate and ferroprussiate of potash, gave a yellowish red precipitate, small in quantity. The fluid, when exposed to the atmosphere, turned to a fine grass-green, without the least deposit of cyanuret of iron; it was not acid. An infusion of galls produced a dirty yellowish white precipitate, which after some time became grayish white, and subsequently yellowish green. Sulphocyanate of potash produced a reddish deposit. Neither the solution nor the precipitate gave any indications of titanium on being treated with salt of phosphorus; nor even after a short contact with a cylinder of zinc, tin or iron, muriatic acid having first been added to the solution. But on mixing another portion of the dissolved mass with nitric acid, drying it and treating it with salt of phosphorus and tin before the blowpipe, I obtained, by adding a little oxide of iron, a glass of a hyacinth colour, which by a greater addition of the evaporated solution, turned to a violet blue, and became almost transparent.

On comparing these experiments with the slag, with the known chemical properties of the substances, it is evident that they contained not only an abundance of *titanium* (as has been shown

shown before by Karsten), but probably also *tantalium*. As both these metals are so scarce, especially *tantalium*, I have thought it of use to call the public attention to this slag.

It is not improbable but that the titanium originated by reduction from prototitanate of iron and manganese, and the tantalium from the tantalates of the same bases (*tantalite*). It would be a very meritorious performance, and highly important to the geology of Silesia, if any one would institute a strict examination of the iron ore which yielded this slag, as well as of its locality, and the process of smelting it undergoes. Perhaps by so doing some pure tantalium, or combinations of that metal, as well as of titanium (and perhaps wolfram), may be found in larger or smaller quantities. It need scarcely be mentioned, that titanium is found in the Riesengebirge of Silesia, especially in the Iser (*Iserin*) in the shape of *nigrin*.

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## XXIII. *Notices respecting New Books.*

THE following are the Contents of those Parts lately published, of the Philosophical Transactions, and of those of the Linnean and Astronomical Societies:

*Philosophical Transactions for* 1827.—*Part* II.

On a new form of the differential thermometer, with some of its applications. By William Ritchie, A.M.—On the structure and use of the submaxillary odoriferous gland in the genus *Crocodilus*. By Thomas Bell, Esq.—On the permeability of transparent screens of extreme tenuity by radiant heat. By William Ritchie, A.M.—On the derangement of certain transit instruments by the effects of temperature. By Robert Woodhouse, A.M.—On some of the compounds of chromium. By Thomas Thomson, M.D.—Rules and principles for determining the dispersive ratio of glass; and for computing the radii of curvature for achromatic object-glasses, submitted to the test of experiment. By Peter Barlow, Esq.—On the change in the plumage of some hen-pheasants. By William Yarrell, Esq.—On the secondary deflections produced in a magnetized needle by an iron shell in consequence of an unequal distribution of magnetism in its two branches. First noticed by Captain J. P. Wilson. By Peter Barlow, Esq.—On the difference of meridians of the royal observatories of Greenwich and Paris. By Thomas Henderson, Esq.—On some observations on the effects of dividing the nerves of the lungs, and subjecting the latter to the influence of voltaic electricity. By A. P. W. Philip, M.D.—On the effects produced upon the air cells of the lungs when the pulmonary circulation is too much increased. By Sir Everard Home, Bart.—Theory of the diurnal variation of the magnetic needle, illustrated by experiments. By S. H. Christie, Esq. M.A.—On the ultimate composition of simple alimentary substances; with

with some preliminary remarks on the analysis of organized bodies in general. By William Prout, M.D.

Presents received by the Royal Society:—Meteorological Journal.

*Transactions of the Linnean Society of London.—Vol. 15th. Part* II.

Some account of a collection of cryptogamic plants from the Ionian Islands. By Robert Kaye Greville, LL.D.—Description of a new genus belonging to the natural family of plants called *Scrophularinæ*. By Mr. David Don, Libr. L.S.—On *Boswellia* and certain Indian *Terebinthaceæ*. By Henry Thomas Colebrooke, Esq.—The natural history of *Oiketicus*, a new and singular genus of Lepidoptera. By the Rev. Lansdown Guilding, B.A.—Observations on the tracheæ of birds; with descriptions and representations of several not hitherto figured. By William Yarrell, Esq.—On two new genera of land tortoises. By Thomas Bell, Esq.—Of the insect called *Oistros* by the ancients, and of the true species intended by them under this appellation: in reply to the observations of W. S. MacLeay, Esq., and the French naturalists. To which is added, a description of a new species of *Cuterebra*. By Bracy Clark.—A review of the genus *Combretum*. By Mr. George Don.—Description of a new genus of plants belonging to the order *Nymphæaceæ*: in a letter to H. T. Colebrooke, Esq. By Nath. Wallich, M.D.—Observations and experiments, made with a view to ascertain the means by which the spiders that produce gossamer effect their aërial excursions. By John Blackwall, Esq.—Descriptions of two quadrupeds inhabiting the South of Africa, about the Cape of Good Hope. By Andrew Smith, M.D.—An account of a pair of hinder hands of an orang otang, deposited in the collection of the Trinity-House, Hull. By John Harwood, M.D.—On systems and methods in natural history. By J. E. Bicheno, Esq., Sec. L.S.—An account of a new species of Pinus, native of California. By David Douglas.—Remarks on the Antilope Chickara: in two letters addressed to the Secretary. By Robert Hills, Esq.—Extracts from the minute-book of the Linnean Society, &c. &c.

*Memoirs of the Astronomical Society, Vol. 3rd. Part* I.

Observations made in the Island of Cuba. By the late Don José Joaquin de Ferrer, of Cadiz, member of the Phil. Acad. Boston. Comprehending, 1. Observations of the comet of 1807, with the determination of the elements of its orbit; 2. Observations of the lunar eclipse, Nov. 14, 1807; 3. Observations of the comet of 1813, with the determination of the elements of its orbit, together with remarks on its magnitude, and that of the comet of 1811; 4. Observations, and computations of the elliptic orbit, of the comet of 1811.—On the longitude of Port Bowen, by the method of moon-culminating stars. By Lieut. H. Foster, R.N.—Approximate places and descriptions of 295 new double and triple stars, discovered in the course of a series of observations with a 20-feet reflecting telescope: together with some observations of double stars previously known. By J. F. W. Herschel, Esq. Pres.—Notice respecting some errors common to many tables of logarithms. By Charles Babbage, Esq., For. Sec.—Astronomical observations

observations made at Bushey Heath (north latitude 51° 37′ 44″,3; west longitude, in time, from Greenwich $0^h$ $1^m$ $20^s$,93), in the years 1825 and 1826. By Colonel Mark Beaufoy: comprehending, 1. Transits of the moon and moon-culminating stars; 2. Occultations of stars by the moon; 3. Lunar eclipses; 4. Eclipses of Jupiter's satellites.—On a new application of the method of determining the time by observations of two stars when in the same vertical, to the case of *Polaris*, when so situated with respect to any other circumpolar star in the course of its diurnal revolution below the pole. By Dr. Tiarks.—On the passage of the comet of *Bootes* over the disc of the sun on the 18th of November 1826. In a letter from M. Gambart to J. F. W. Herschel, Esq. Pres.—On a new period of eclipses. By James Utting, Esq.—On an appearance, hitherto unnoticed, in the nebula of *Orion*. By John Pond, Esq., Astronomer Royal.—Notice of a comet discovered by M. Flaugergues at Viviers, March 29th, 1826. Extracted from a letter from M. Flaugergues to F. Baily, Esq., Pres.—Astronomical observations: I. Observations taken at Stargard, and the Paramatta observatory, New South Wales, in the years 1825 and 1826. By Dr. Rumker. 1. Of the great comet of 1825; 2. Of the comet in *Leo* in 1825; 3. Of the lunar eclipse in May 1826—of an occultation during the eclipse—and of Mars near the opposition in 1826. II. Observations of the solar eclipse in November 1826, taken at Bushey Heath. By Lieut. George Beaufoy, R.N. III. Observations of a comet in *Eridanus*, and of *Ceres*, *Pallas* and *Vesta*, near their oppositions in the year 1826, taken at Padua. By Professor Santini. IV. Observations of the eclipses of Jupiter's satellites, taken at the Madras observatory, in the years 1817—1825. By John Goldingham, Esq. V. Observations taken at Calcutta in the year 1822; and an extract of a letter from Major J. A. Hodgson to Dr. Gregory. 1. Of the transit of Mercury over the sun's disc in November, 1822; 2. Of occultations of stars by the moon; 3. Extract of a letter relative to the mode adopted for determining the times of the observations of Jupiter's satellites, recorded at p. 440, vol. ii. of the Mem. Ast. Soc.—Report of the council of the society to the seventh annual general meeting.—Address delivered at a special general meeting of the Astronomical Society of London, on April 11, 1827, &c.

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## XXIV. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

Dec. 6, 1827.—THOMAS Henry Hall, Esq., and William Phillips, Esq., were admitted into the Society.

A paper was read, entitled, "On the Corrections in the Elements of Delambre's Solar Tables, required by the Observations made at the Royal Observatory at Greenwich; by G. B. Airy, Esq., M.A., Lucasian Professor of Mathematics in the University of Cambridge, communicated by J. F. W. Herschel, Esq."

The author was desired by the Board of Longitude to examine the discordancies between the right ascension of the sun, as observed at Greenwich

Greenwich since the erection of the new transit instrument, and as computed by the solar tables of Delambre, which are used in the computation of the Nautical Almanac, with a view to the discovery of the errors in the elements of those tables. The result of the comparison at first indicated the necessity of a correction of the epochs of the sun's longitude, and of the longitude of the perigee, and perhaps also of the equation of the centre. But upon pursuing the examination through a series of years, it became manifest that some other source of irregularity existed, and that this could be no other than an erroneous estimate of the masses of some of the planets, especially of Venus and of Mars. A more critical examination showed that there was also an error in the assigned mass of the moon. It was found necessary in these investigations to take into account an error which occurred in the tables with regard to the secular motion. It results from the author's researches that the epochs for 1816, and those for 1821 to 1825, ought to be increased respectively by 4″,734 and 5″,061; that of the perigee increased by 46″,3, and the greatest equation of the centre diminished by 0″,84; the mass of Venus should be reduced in the proportion nearly of 9 to 8, and that of Mars nearly in the proportion of 22 to 15. On a comparison of these results with those which have been derived from an examination of some of Dr. Maskelyne's observations as given by Burckhardt in the *Connaissance des Tems* for 1816, they are found on the whole to agree in the most satisfactory manner.

Dec. 13.—A paper was read, entitled, "On the measurement of high temperatures; by James Prinsep, Esq., Assay Master of the Mint at Benares, communicated by P. M. Roget, M.D. Sec. R.S."

In the commencement of this paper the author describes the many abortive endeavours of former experimenters to obtain instruments for the accurate admeasurement of high temperatures, and afterwards describes several attempts of his own to effect this very desirable object. In the course of his inquiries a remarkable fact presented itself in the change which occurred in an index constructed on the compensation-principle, and formed by two slips of metal, the one of silver, the other of gold, originally quite pure, and united without any alloy. In the course of a few years, although it had never been subjected to a very high temperature, the surface of the gold became converted into an alloy of silver, the impregnation extending gradually to a considerable depth in the gold, and destroying the sensibility of the instrument to changes of temperature.

After trying various plans, he gave the preference to the one founded on the following principles: namely, that the fusing points of the pure metals are fixed and determinate; that those of silver, gold, and platina, comprehend a very extensive range of temperature; and that between these three fixed points in the scale, as many intermediate ones as may be required, may be obtained by alloying the three metals together in different proportions. When such a series of alloys has been once prepared, the heat of any furnace may be expressed by the alloy of least fusibility which it is capable of melting. The determinations afforded by a pyrometer of of this kind will, independently of their precision, have the advantage of being identifiable at all times and in all countries; the smallness of the apparatus is an additional recommendation, nothing more being necessary than a little cupel, containing in separate cells the requisite number of pyrometric alloys, each of the size of a pin's head. The specimens melted in one experiment need only to be flattened under the hammer in order to be again ready for use. For the purpose of concisely registering the results, the author employs a simple decimal method of notation, which at once expresses the nature of the alloy, and its correspondence with the scale of temperature. As the distance between the points of fusion of silver and of gold is not considerable, the author divides this distance on the scale into ten degrees; obtaining measures of each by a successive addition of 10 per cent of gold to the silver, the fusion of which, when pure, marks the point of zero, while that of gold is reckoned at 10 degrees. From the point of fusion of pure platina to that of pure gold, the author assumes 100 degrees, adding to the alloy which is to measure each in succession, one per cent of platina. The author then enters into a detailed account of the method he employed for ensuring accuracy in the formation of the requisite series of alloys, and of various experiments undertaken to ascertain their fitness as measures of high temperatures. The remainder of the paper contains the recital of the author's attempts to determine, by means of an apparatus connected with an air-thermometer, the relation which the fusing point of pure silver bears to the ordinary thermometric scale.

A paper was likewise read, entitled, "On Alimentary Substances; by Sir George Smith Gibbes, M.D. F.R.S."

After a few remarks on the very scanty assistance which physiology and pathology have yet derived from the chemistry of organized substances, notwithstanding the great improvements which have been lately introduced in the methods of analytical research, the author maintains the proposition, that all alimentary substances, whether belonging to the animal or vegetable kingdoms, retain some principles originally the result of vitality, which they communicate to other bodies, and thus constitute their food. He considers the appearances of the infusory animalcula, and more especially of the *Monas Termo,* during the decomposition of alimentary substances as favouring this hypothesis. In support of this argument, he adduces experiments by Dr. Ingenhouz, himself, and others, from which he infers that these animalcula perform some definite function during the growth of vegetables.

From the circumstance of chloride of lime preventing the development both of vegetation and of infusory animalcula, the author infers that these animalcula are instrumental in the production and growth of plants.

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## LINNÆAN SOCIETY.

Dec. 4.—Read a paper "On the Locomotive Power of the Snail, by Mr. James Main." The author describes as the species which have chiefly come under his notice the following:—*Limax maximus: ater:*

*ater: lichenivora: rufus: mutabilis: tenax:* and *agrestis.* The belly of the snail being perfectly smooth, there are no appendages to do the office of feet; and the whole of the body moves at once, and not in parts successively. By placing the animal on a piece of glass, Mr. Main was enabled to observe a muscular motion; but this, instead of being from head to tail, was directly the reverse, so that the animal's motion cannot be caused by impulses in the direction of its progress. He gives, in conclusion, two conjectures as to the cause of the animal's motion; namely, 1st, that the body is moved forward by the retromissive discharge of slime, which being emitted simultaneously from every part of the under surface, he conceives may exercise a force adequate to the propelling of the animal:—or 2ndly, from its power of forming its lower surface into segments of circles along the whole of its length; and thus by assuming a vertical vermicular action on the plane of the sustaining surface, impelling the body forward by alternate contraction and expansion.

As dry air deprives the animal of motion, Mr. Main is inclined to consider the first surmise the more probable.

Read an extract from a letter from Dr. Rigby to Mr. R. Taylor, dated Berlin, on the ova of the *Hirudo medicinalis*;" with some specimens.

Read also "An account of *Margarōdes,* a new genus of insects found in the neighbourhood of Ants' Nests; by the Rev. Lansdown Guilding, B.A., F.L.S. &c." Mr. Guilding, after quoting Dr. Nugent (Geol. Trans. vol. v. p. 463), who states that the *ground pearl* (improperly supposed to be fossil) is found in prodigious quantity in the furrows of the land in Antigua when newly turned up, and suggests that it may be the production of an insect, informs us that he has succeeded, by watching some that he preserved in moist marl, in detecting the insect which issued from them. He conceives it to be a parasite on the ants, whose formidable numbers in the dry islands, they are calculated to keep down. The entire want of a mouth is remarkable in this new insect, the food being absorbed by tubes in the fore claws. It also possesses the extraordinary power of throwing out long filaments, when in dry situations, supposed to be for preserving itself by obtaining moisture. Its scales effervesce and disappear in nitric and muriatic acid; sulphuric turns them black; and vinegar slowly decomposes them. In flame they burn like horn. Mr. G. is uncertain at present what station is to be assigned to this insect.

Dec. 18.—Three new species of Land Tortoise were exhibited by Mr. Bell:—*Testudo pardalis, actinodes.* and *tentoria,* descriptions of which are given in the number of the Zoological Journal just published.

Read a portion of Dr. Hamilton's "Commentary on the *Hortus Malabaricus.*"

Jan. 15, 1828.—Some specimens of *Ianthinæ* were exhibited by L. W. Dillwyn, Esq., washed ashore in July last in Oxwick bay near Swansea, many of them picked up alive and yielding a beautiful dye. Specimens of the *Medusa Vellela* and *M. Navicula* were found with them.

 Read

Read "Descriptions of three new species of Plants, natives of New Granada, by M. Gondot, Professor of Botany at Bogota: the plants are,

1. *Sessea corymbiflora* foliis obovatis attenuatisque, floribus corymbosis.—In woods near Bogota.

2. *Cinchona Muzonensis* foliis ovato-oblongis acutis basi attenuatis, stipulis revolutis, paniculâ brachiatâ, corollis albis, limbo imberbi.—In great forests near the city of Muzo.

3. A plant of a Genus nearly akin to *Theobroma*, from which it differs chiefly in habit, in the form of the calyx, and the structure of the stamens. Monadelphia Dodecandria. *Büttneriocæ*, Brown. "Arbuscula foliis digitatis, quinatis."—Forests near Muzo. Called by the inhabitants the Cacao *Montaras* or *Symoron*. The poor people mix the fruit with the cultivated cacao.

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## GEOLOGICAL SOCIETY.

Dec. 7.—John Braddich, Esq. of Boughton-Mount near Maidstone; G. W. Featherstonhaugh, Esq. of Duanesburgh, New York; Arthur Kett Barclay, Esq. of Grosvenor Place; and Lord Francis Leveson Gower, of Albemarle Street, were elected Fellows of the Society.

A paper was read, "On the Geology of Quebec and its Vicinity, by J. T. Bigsby, M.D. F.L.S. G.S." &c. &c.

The author, who acknowledges the assistance he has derived from the manuscripts of Lieut. Skene, R. E., first describes the tract, on the eastern termination of which the city of Quebec is situated, as an oblong ridge of about seven miles and a half in length, and in average width about one mile and a half; subsiding on the north-west by steep and rocky slopes, into rich meadows; whilst on the south-east it advances in the form of cliffs towards the northern bank of the St. Lawrence.

Various rivers traverse the district above mentioned, nearly from north to south, of which the most considerable are the St. Charles and the Montmorenci. On the southern bank of the St. Lawrence, Point Levi is the most conspicuous promontory; and to the west of it the country is intersected by several streams running from south to north.

*Diluvium.*—The districts above mentioned are partially covered with boulders of gneiss, granite, syenite, and labrador felspar; the greatest quantities of which are found on and near Cape Diamond, Point Levi, and Point Montmorenci; whilst occasional deposits of clay, gravel, and sand, including organic remains, the author supposes to be of diluvial origin,—and not produced by the operation of any existing watercourses.

The rocks of this region repose upon each other in the following descending order:—1st. A slaty series, composed of shale and grauwacke, occasionally passing into a brown limestone, and alternating with calcareous conglomerate in beds, some of which are charged with fossils.—2nd. A conchiferous brown and black limestone sometimes based upon a calcareous conglomerate.—3rd. *Gneiss.* The author's chief reason for considering the slaty-series as superior to the limestone,

limestone, is, that the latter is in some situations in immediate contact with gneiss; while in others it passes into beds of the first series above mentioned; of which the conglomerates contain organic remains derived from the conchiferous limestone.

1. The *slaty-series* occupies the whole of the southern shore of the St. Lawrence, the Island of Orleans, and a considerable portion of the north bank of the river, including the ridge upon which Quebec is placed. In that neighbourhood the mass of the deposit consists of a black and brown slaty limestone, inclined at high angles, in some instances nearly vertical; and alternating with semi-crystalline limestone, and various conglomerates. The limestone contains several varieties of crystallized carbonate of lime, intermixed with quartz crystals, and occasionally traversed by seams of bituminous matter. Near Cape-Rouge, and on the plains of Abraham and Kilgraston, some of the strata consist of red and greenish clay-slate. In the calcareous conglomerates, organic remains are mixed with fragments of clay-slate, and the beds alternate with compact gray limestone and quartzose layers. Between Quebec and Cape-Rouge, boulders of primary rocks, and fragments of compact grauwacke, are buried deep in the red schist.

The channels of the various streams east and west of Quebec, afford instructive sections, which, according to the author, prove these slaty deposits to be more recent than the conchiferous limestone.

On the south side of the river St. Lawrence, the slaty limestone of Quebec is no longer seen, but several new beds of conglomerate present themselves; one of the lowest of which contains trilobites, encrinites, corallines, and other fossils associated with vegetable impressions, probably *fuci* and *amansiæ*. In the schistose beds near the mouth of the Etchemin are thin seams of coal; and at the village of St. Henry the slate is so compact as to be used for hones.

2. The horizontal conchiferous limestone occupies a zone from two to three miles in breadth, on the north of the slaty tract, and included between the slate and a mountainous range of gneiss. It is exposed in the beds of all the rivers which flow southwards into the St. Lawrence, and its characters are well developed at the falls of the Montmorenci and the St. Charles, and at the quarries of Beaufort. The organic remains consist of several species of trilobites, orthoceræ, terebratulæ, encrinites, ammonites, &c. On the Montmorenci the beds are from eighteen inches to two feet in thickness, nearly horizontal, and of a blackish-brown colour; in one situation they pass into a subjacent calcareous conglomerate, whilst in other places the limestone itself contains large blue nodules, and reposes immediately upon gneiss. At Beaufort-quarries, ledges of fetid limestone alternate with calcareo-bituminous shale, containing organic remains similar to those noticed on the Montmorenci.

From the characters and fossils of the limestone above described, the author regards it as the same with the *calcaire intermediaire* of D'Aubuisson, or the equivalent of the "Carboniferous-limestone" of English geologists.

Dec. 21.

Dec. 21.—Henry Holland Stutzer, Esq. River-Terrace, Islington, was elected a Fellow of the Society.

The reading was begun of a paper "On a group of Slate-Rocks in Yorkshire, between the rivers Lune and Wharfe, from near Kirby Lonsdale to near Malham, by John Phillips, Hon. Mem. of the Leeds and Yorkshire Philosophical Societies."

1828. Jan. 4.—John Murray, Esq. Jun. of Albemarle Street; Henry Tuffnell, Esq. of Christchurch, Oxford; The Right Hon. Viscount Cole, of Christchurch, Oxford; R. C. Fergusson, Esq. M.P., of Craigdarroch, Dumfriesshire, and of Great Cumberland Street; John Phillips, Esq. of York, Hon. Mem. of the Yorkshire, Leeds, and Hull Philosophical Societies; and John Gurdon, Esq. of Assington Hall, Suffolk,—were elected Fellows of the Society.

The reading of Mr. Phillips's paper begun at the last meeting, was concluded.

The object of this paper is to describe the geological structure and relations of a group of rocks which the author characterizes as "aberrant from the slate district of Cumberland," and extending about fifteen miles towards the east under the summits of Greygarth, Ingleborough, and Pen-y-gent,—a tract remarkable for the variety and singularity of its geological appearances, among which the proofs of dislocation are peculiarly striking and important.

To this description a sketch is premised of the slate-series of the Lakes of Westmorland and Cumberland, where the rocks are grouped in three principal divisions, the lowest consisting of dark soft slate much contorted, with fine-grained gneiss beneath it passing into granite. The second division occupies a country of very different aspect from that of the slate. The mountain-ranges being marked by abrupt precipices, as at Helvellyn, Langdale Pikes, and the Lakes of Ulswater, &c. and composed of brecciated argillaceous rocks containing calcareous spar, green-earth, and calcedony, with greenstone and other forms of trap. On the south of this chain is a tract of transition limestone, containing caryophyllia, productæ, spiriferæ, and other fossils; and this is covered by a third zone of slate, the most recent rock of the country, usually divisible into rhomboidal blocks, of which two principal varieties are observable, alternating with each other; the one homogeneous and fissile, and containing organic remains sparingly distributed, of the genera trigonia, pecten, gryphea, turritella terebratula;—the other more granular and micaceous. This formation is in some cases succeeded by red conglomerate, but more commonly by mountain-limestone, the lowest beds of which contain numerous pebbles of slate and quartz; and above the limestone are the carboniferous rocks, including the millstone grit and the upper coal-measures. The highest strata known in the country, consist of the new red sandstone, placed in an unconformable position above the coal formation.

The tract, which is the more immediate object of this paper, extends from the valley of the Lune in an easterly direction, to that of the Wharfe. Along its middle, from Casterton Fells to a few miles east of of the Ribble, ranges an almost continuous line of argillaceous rocks, generally fissile, and belonging to the third division of slates above mentioned. This tract is bounded on the north by the elevated strata that support the summits of Greygarth and Pen-y-gent; and on the south (in consequence of great dislocations) by millstone grit and the coal measures. If the rivers Lune and Wharfe are included, no fewer than nine streams cross the district from north to south, and exhibit very distinctly the structure and relations of the rocks; the greater number of the streams cutting through the limestone and millstone grit, exposing the subjacent slate, and finally passing off on the depressed strata of the coal measures. The author describes in detail, the phænomena presented in these several sections, and illustrates his observations by sectional views and sketches.

The structure of the country is very well displayed in the course of the Ribble, where the slate first appears on the north, beneath parallel bands of limestone; while on the south, the carboniferous strata, of which the northern portion is horizontal, decline at a high angle, thus indicating a vertical dislocation of about four hundred feet. Besides this fault on the southern verge of the slate, another still more important one may be traced in a parallel direction across the valley of Ribbles-dale, and over Malham Moor; by which, strata have been brought into opposition, that in their original place were separated by a thickness of more than five hundred feet. Various facts are stated by the author in proof of this derangement, and descriptive of the phænomena produced by it.

The author subjoins to his descriptions some remarks on the stratification of slate, and on the difficulty of discriminating between the planes of general stratification, or dip, and those of the cleavage effected by a blow,—the latter of which are often disposed at considerable angles to those of the dip. He is disposed to think, that in the fissile granular varieties of slate approaching to sandstone, the laminæ of cleavage may really be those of deposition; since the surfaces are frequently coated with mica, and the fossil remains are in a disposition parallel to them. Besides this more general cleavage, however, the slate is also traversed by other planes, oblique to those of cleavage, and less conspicuous, to which the quarry-men give the name of "Bate." The direction of these planes, though nearly alike in limited spaces, is found to vary considerably in different portions of the same tract; and even the better-defined planes of the ordinary cleavage are seldom parallel to each other throughout any great extent of country.

A collection of fossil vegetables, chiefly from the Jarrow and Felling collieries in the Northumberland and Durham coal-field, presented to the Society by William Hutton, Esq. of Newcastle-upon-Tyne, with drawings describing the plants according to the system of M. A. Brongniart, was accompanied with some remarks by the donor, comprised in a catalogue.—The collection consists of Calamites, Sigillaria, Sagenaria, Stigmaria, Filices, Sphænophyllum, Asterophyllum, &c.; also several specimens of undescribed confervæ, leaves, stems, &c.

A notice was read on the occurrence of "Chlorophæite" in basaltic dykes

dykes in Northumberland; and of carbonate of strontian in the lead measures at Fallowfield near Hexham, by William Hutton, Esq. of Newcastle-upon-Tyne.

The author discovered "Chlorophæite" in a basaltic dyke near the river Coquet, about two miles N.E. of Felton, in the form of small nodules, which upon fracture exhibit the changes of colour and appearance mentioned by Dr. MacCulloch, who first found this mineral in the Isle of Rum. This substance has also been observed by the author at Coaley Hill near Newcastle, in the steatitic or earthy form, and but rarely crystallized.

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## ASTRONOMICAL SOCIETY.

Dec. 14.—A letter was read from Maj. Hodgson the Surveyor-General of India, dated from Calcutta, April 18, 1827, accompanied by two copious lists of astronomical observations. The *first* contained a series of transits of the moon and some of the principal fixed stars, as observed at his own house (situated 5 seconds in time, east of the Flag-staff of Fort William) from November 18, 1826 to March 20, 1827, both inclusive. The observations are above 300 in number: and the stars observed are not those which are usually denominated moon-culminating stars, but consist of some of the principal stars only; and many of these situated occasionally at very considerable distances from the moon in declination. In fact, the list of moon-culminating stars has never yet been computed early enough to enable it to be sent to such distant places in sufficient time for the observations. It is therefore to be feared that correspondent observations of many of the stars observed by Major Hodgson may not be found at the observatories in Europe. The results of the transits are regularly computed by Major Hodgson, agreeably to a printed form, where every step of the process is minutely laid down, and of which he has forwarded to the Society two examples. It is Major Hodgson's intention to continue his observations for one whole year at least, and to transmit them to the Society, together with the daily computations showing the results. The instrument employed was a 31-inch transit by Dollond; but he is in expectation of a more powerful one shortly from England. The *second* list contained a series of upwards of 200 observations of the eclipses of Jupiter's satellites taken by gentlemen in the civil service, and by officers of the Bengal army, at different times and at different places. The dates extend from July 18, 1795 to April 11, 1827: and the names of the observers are given, together with the length, the aperture, and the magnifying power, of the telescope employed. Major Hodgson states that he shall transmit several others as soon as he can collect them: for (he observes) "observations of this kind have been of much use in determining geographical positions in this widely extended country; and the probability of obtaining correspondent observations through the medium of the Astronomical Society, will excite travellers and surveyors to take advantage of all opportunities of making observations of the eclipses of the satellites of Jupiter, and of the culmination of the moon and stars."

A paper

A paper was also read, entitled "On the computation of the geocentric places of the planets for ephemerides:" by J. F. Littrow.

The usual mode of computing the geocentric places of the planets for ephemerides consists in instituting a first computation of their heliocentric positions, which are then reduced to geocentric by well known formulæ. To avoid the very laborious calculations entailed by this process, Prof. Gauss devised a method much more expeditious, and which gives at once the geocentric right ascensions and declinations, as well as the distance of the planet from the earth, in terms of the co-ordinates of the earth and planets at the instant of observation. The equations expressive of these relations are stated by M. Littrow, and are extremely simple. By them, the right ascension ($\alpha$), the declination ($\delta$), and mutual distance ($\varrho$) are given directly as follows:

$$\tan\alpha = \frac{Y+y}{X+x}$$

$$\tan\delta = \frac{Z+z}{X+x}\cdot\cos\alpha \qquad (1)$$

$$\varrho = \frac{Z+z}{\sin\delta}$$

X, Y, $Z$ being the co-ordinates of the earth, and $x, y, z$ of the planet.

Now it is obvious, that if we neglect the perturbations (on which neglect it will be observed the application of this method mainly depends), all these co-ordinates are, ultimately, functions respectively of the mean longitudes of the earth and planet, and their values may therefore be tabulated as such; and the tables, once constructed, will last till the secular variations of the elements of the planetary orbits shall have so changed them as to produce an error surpassing that which can be tolerated in an ephemeris.—These tables once computed, the foregoing equations reduce the annual computations to the utmost brevity. The only point then to consider is the construction of the tables themselves.

The expressions of the co-ordinates in terms of the mean longitude are made to depend on the true longitude as the independent variable, by means of six constant quantities for each planet. These are, 1st, the respective inclinations of the plane of the planet's orbit to the solstitial and equinoctial colures and the earth's equator; and, 2ndly, the plane angles made by the line of intersection of the planet's orbit, and the ecliptic respectively with its line of intersection with the solstitial and equinoctial colures and equator. The author states the method of computing these constants from the known elements of the orbit, and gives their actual numerical values for all the planets with their secular variations.—These once known, and the true longitude, as a preliminary step, computed for every mean longitude, he states the equations by which the co-ordinates are formed from them for every given value of the longitudes; and, finally, embodies the whole result of his computations in a table stating their values for each of the planets, and for intervals of 4° for Mercury and Venus, 3° for Mars, 2° for Jupiter, Saturn, and Uranus, and for each day in the year for the Sun.

The publication of the 1st Part of the 3rd Volume of the Memoirs of the Society, was announced by the President.

At the conclusion of the business of the evening, An instrument contrived by Mr. Henry Atkinson, of Newcastle-upon-Tyne, to illustrate some of the phænomena of rotation, was submitted to the inspection of the members, by Mr. Riddle.—It consists of a flat circular disk, through the centre of which passes a small endless screw, having a cup in one of its extremities. When the cup is *below* the centre of gravity, and set on a point on which the disk (inclined to the horizon) is made to rotate in its own plane, the whole rotating plane performs at the same time, a slow revolution round the perpendicular to the horizon, which passes through the point of rotation; and the revolution of the plane is in the *same* direction as the rotation. When the point of rotation is *above* the centre of gravity, the plane moves slowly round in a direction *contrary* to that of rotation, affording an apt *illustration* of *precession.* When the point of rotation is *in* the centre of gravity, the position of the revolving plane continues permanent.

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ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

At a special meeting of this Society, assembled for the express purpose, the following Address of Congratulation was unanimously adopted.

*To Davies Gilbert, Esq. M.P. &c. &c.*

Sir,—At the last annual meeting of this Society, over which you have presided from its first institution, and which is so deeply indebted, in its origin and advancement, to your zeal and ability, we ventured to express a hope that your continued exertions, and successful labours in the wider fields of philosophy and science, might be crowned with honour in a higher sphere, by your being elected to the office of President of the Royal Society of London.

It is gratifying to us to see that our wishes are fulfilled; and we most sincerely congratulate you on being chosen to fill that chair, which having been the seat of Newton, may be deemed the throne of intellectual eminence.

That two of our countrymen, natives of this remote province, almost of the same spot, should have been raised in succession to the dignity of this station, is to us a source of pleasure and of pride.

As members of this Society, we rejoice at an appointment which forms for us, though at an humble distance, a connecting link with the most renowned Society of our country, and brings us within the influence of its notice and support; for although your enlarged mind will lead you equally to protect and encourage every branch of science and knowledge, since they all mutually assist each other, blending their various colours into one ray of intellectual light; yet we shall indulge the feeling, that Geology has, as it were, a filial claim upon your regard, and will experience that kind and fostering care which may fairly be asked for a science, now only in its infancy.

While

While we request you to accept our congratulations, we deeply lament the occasion of our illustrious townsman's resignation.

Be pleased, Sir, to take an early opportunity of conveying to Sir Humphry Davy our condolence on his illness, and our sincere wishes for his restoration to perfect health, and to the exercise of those faculties which he has employed to the noblest purposes—the enlargement of the boundaries of human knowledge, and the gaining for his name the most enviable kind of immortality (such is his own estimate) "that which is connected with the gratitude and blessing of his fellow-creatures."

May the enjoyment of health enable you, Sir, to continue for many years in the discharge of the high situation in which you have been so honourably placed, with happiness to yourself, with satisfaction to your associates, and with benefit to the world.

(By order of the Royal Geological Society of Cornwall,)

JOSEPH CARNE, Treasurer,

Penzance, Dec. 22, 1827. Chairman of the Meeting.

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## ROYAL ACADEMY OF SCIENCES OF PARIS.

May 28.—The following are the titles of the works or manuscripts received by the Academy at this sitting:—Description of a new steam-apparatus for boats; by M. Tourasse.—New facts relative to the therapeutic employment of the Pyrothonide, by M. Rauque.—Notice respecting comets; by M. Courbon, surgeon.—New notice on the bursting of steam-boilers; by M. Tabareau.—New theory of the phænomena of vision; by M. Plagge.

M. Delessert read a letter from M. Brunel on the subject of the accident which happened at the Thames Tunnel.—M. Thenard, on the part of the Commissioners, read a favourable account of the memoir by M. Isidore Boullay.—M. Bonastre had presented a memoir On the compounds of the oil of cloves and of pimento with the alkalies, and several salifiable bases. The principal fact contained in this memoir is, that these oils, which do not redden litmus, combine nevertheless with salifiable bases; but M. Chevreul, reporter of the Academy, stated, that this fact not being accompanied with the necessary details and precision, is devoid of the interest which it may hereafter possess. M. Bonastre was recommended to continue his researches.

June 4.—The Minister of the Interior communicated the edict of the King, by which the nomination of M. Cassini, jun., as a free academician, is confirmed.—M. Girard presented a notice respecting hydraulic mortars, made with fossil argillaceous sands.—M. Dulong, in the name of the Commission, made a report relating to the compression of liquids.—M. Cordier read the first part of a memoir On the temperature of the interior of the earth.—M. Bonnard read a memoir On the regularity of the geognostic facts which exist in the territory of Arkose in the East of France.

June 11.—At this, which was a public sitting, M. Cuvier read An historical eulogium respecting M. Hallé.—M. Dupin read Statistical researches respecting the canals of the north and south of France.

France.—M. Cuvier read An historical eulogium respecting M. Corvisart; and M. Cordier, a memoir On the temperature of the earth.

June 18.—The following are the titles of the memoirs or manuscript works presented to the Academy:—Notice respecting two mussels and a toad, which were taken alive from a well which had been filled up for one hundred and fifty years; by Dr. Quenin, mayor of Orgon.—A sealed packet, containing An account of a new process for breaking calculi; by M. Cazenave.—A sealed packet, containing New chemical researches; by MM. Quesneville and Julia Fontenelle.—Description of a method by which boats may go up rivers alone and without expense or danger; by M. Anatasi.—Design for a new pump for fire-engines; by M. Stolz.—New considerations respecting the boilers of steam-engines; by M. Tabareau.—On certain organs of the *Hymenoptera, Diptera*, &c.; by M. Robineau-Desvoydi.—Researches respecting the vibration of several sonorous bodies, and particularly elastic cords; by M. Cagniard de la Tour.—Notice respecting the extinct volcanoes of the South of France, the eruptions of which occurred since the deposition of the second freshwater formation; by M. Marcel de Serres.—On the elementary principles relating to the division of territories.—The Minister of the Interior announced his having sent the Academy a collection of the fossil bones found in the grottoes of Osselles.—M. Geoffroy-Saint-Hilaire sent from Montélimart several particulars which he had collected respecting the scientific establishments of the South of France.—M. Blainville, in the name of the Commission, read An account of an extremely curious memoir, by MM. Raspail and Robineau-Desvoydi, but which appears to require new researches.—M. Brochant gave a favourable account of the new memoir by M. Bonnard On the territory of Arkose.—M. Constant Prévost read a memoir, entitled An examination of the geological question, "Whether the continents which we inhabit have been repeatedly covered by the ocean?"

June 25.—MM. Raspail and Robineau-Desvoydi announce that they possess myriads of *Alcyonellæ*, and sent a packet of them.—M. Lacroix gave a favourable account of M. Denaix's work, entitled A methodical comparative geographical essay.—M. Cuvier read a memoir On the *Scarus* of the ancients.—M. Berthier read A notice of four memoirs which he presented to the Academy on some new mineral species.—M. Roger communicated some results which he had obtained respecting the height of Mont-Blanc.—M. Raspail read a memoir, entitled A physiological analysis of the *Spongilla friabilis* of Linnæus.

The Academy decided that the place vacant by the death of M. Ramond should be supplied.

July 2.—M. Pons, of Florence, informed the Academy that he had discovered on the 20th of June, a small comet, invisible to the naked eye, in the constellation of *Cassiopæia;* and according to a letter from Marseilles, M. Gambart also observed it at 2 o'clock in the morning of the 21st of June.—M. Beudant reported, in the name of the Commissioners, very favourably with respect to four memoirs

memoirs which M. Berthier had presented to the Academy at one of its late sittings.—M. Prévost read an extract from a new memoir on geology.

July 9.—The Minister of the Marine forwarded Observations on various subjects, which had been sent to him from New Holland, by M. Durville.—M. Rembielinski presented a memoir, entitled *Description des courbes productionelles.*—M. Velpeau read a memoir, entitled *Récherches sur l'œuf humain.*—M. Sérullas read a memoir already noticed in the *Annales.*—M. Cordier, in the name of a Commission, gave a favourable account of the memoir lately read by M. Prévost.—The Commission for the presentation of candidates for the place of Foreign Associate, vacant by the death of Volta, was composed of MM. Arago, Fourier, Legendre, Cuvier, Thenard, and Desfontaines.—A secret Committee of the Section of Mineralogy presented the following list of candidates for the place vacant by the death of M. Ramond: MM. Bonnard, Berthier, and Constant Prévost.

July 16.—The lamented death of M. Fresnel was announced to the Academy.—M. Keller, engineer in the Marine, requested permission to deposit a sealed packet.—M. Boucharlat also sent a sealed packet, containing the results of his experiments upon ammonia, and an analysis of a memoir On the possibility of considering ammonia as an hydracid as well as an alkali.—M. Cuvier reported respecting the fossil bones found in the cavern of Oisselles near Besançon.—M. Berthier was elected a member of the Academy.—M. Andreossy presented an historical notice of the giraffe.—M. Turpin read a memoir On the structure and reproduction of the truffle compared with those of the toadstool.

July 23.—M. Arago communicated several new experiments on bromine, by M. Aug. de la Rive.—M. Cordier finished the reading of his paper On the temperature of the earth.—M. Turpin concluded his memoir On the reproduction of the truffle.—M. Dutrochet communicated some observations on *Endosmosis* and *Exosmosis.*

July 30.—M. Roger sent the Elements of the calculation by which he determined the height of Mont-Blanc.—M. Thenard, in the name of a Commission, reported that the manuscripts of M. Reineck, who died at Ancennis, contained nothing worthy of being printed: the examination was made at the request of the Minister of the Interior.—M. Collard de Martigny communicated an announcement of the principal consequences deduced from his researches into the alterations produced in the quantity and composition of the blood and lymph, by complete abstinence from liquid and solid food.—M. Binet read a memoir On the determination of the orbits of planets and comets.—MM. Robiquet and Colin read a second memoir on madder.—M. Savart presented a new memoir On the vibrations of solid bodies. — At a secret sitting, the President, in the name of the Commission of the 9th of July, presented the following candidates for the vacant place of Foreign Associate. In the first rank, Dr. Thomas Young, of London; and afterwards,

terwards, in alphabetical order, MM. Bessel, of Kœnigsberg; Blumenbach, of Gottingen; Robert Brown, of London; Leopold de Buch, of Berlin; Dalton, of Manchester; Olbers, of Bremen; Œrsted, of Copenhagen; Plana, of Turin; and Soemmering, of Frankfort.

Aug. 6.—The following memoirs were received at this sitting:—On the use of chloride of lime in disinfecting the air in which silkworms are kept, by M. Bonafous; On the propagation of heat in a triangular prism, by M. Ostrogratzki; On an artificial nose from the skin of the forehead, by M. Delpech; On the motion of fluids in the atmosphere, by M. Le Chevallier.—Dr. Young was elected Foreign Associate of the Academy.— M. Geoffroy-Saint-Hilaire presented the head of a young giraffe, in which it was seen that the bony nucleus of the horn in the young animal is separated from the forehead by a distinct suture. There were read a memoir On some electrical phænomena produced by the pressure and cleavage of crystals, by M. Becquerel; On the compression of gases, by M. Despretz; On a new theory of sonorous vibrations, by M. Cagniard-Latour; On the operation for the artificial pupil, by M. Faure.—M. Molard gave a favourable account of tachygraphy, and of the tachytype, invented by M. Conti.

Aug. 13.—Titles of memoirs received;—Memoir in continuation of the history of quina, cinchonia and kinic acid, by M. Henry; Remarks on the weight and dimensions proper for the sails of machines in order to produce the desired effects, by M. Landormy; A third notice on the motion of fluids, by M. Le Chevallier; Memoir on two cases of luxation of the cervical vertebræ with compression of the spinal marrow, by M. Barny.—M. Chevreul read a notice On the discovery of phocenic acid in the alkanet.—M. Lisfranc read a memoir On *rhynoplastie*, and presented an individual who had successfully undergone the operation.—M. Geoffroy-Saint-Hilaire communicated Some observations on a polydactyl horse, with toes separated by membranes.—M. Savart read a memoir On vibrations. The Academy received at this sitting two verbal reports: one by M. Freycinet, On M. Balbi's ethnographical atlas; the other by M. Silvestre, on M. Francœur's work On the teaching of linear drawing.

Aug. 20.—The ordonnance of the King confirming the nomination of M. Berthier was received.—A letter from M. Pons announced the discovery of a new comet.—M. Fossombroni communicated a report respecting the Cæsarean operation lately performed in a hospital at Florence.—Titles of memoirs received: Researches and observations upon false consecutive aneurism, by M. Breschet; Discoveries on the treatment of scrofulous affections, and a process for healing *fistula in ano* (a sealed packet), by M. Deygallières; New observations on yellow fever, by M. Leymerie.—M. Cassini, in the name of a Commission, gave a favourable account of M. Turpin's researches on the reproduction of truffles.— M. Pouillet read a memoir On electro-magnetism.—M. Chabrier read An extract of some new observations on the progressive motion of animals.

Aug.

Aug. 27.—The Academy received a memoir from M. Madelaine On steam-engines.—M. Chevreul gave a very unfavourable account of a new process, proposed by M. Ratieuville, for dyeing wool of a blue colour.—The following papers were also read: Researches on the determination of the series which represent the functions given in one part only of their extent, by M. Brisson; A memoir on the dyeing of woollen with prussian blue, by M. Raymond; A memoir on the formation of sulphuric æther, by MM. Dumas and Boullay; Researches on the human ovum, by M. Velpeau.—M. Fréderic Cuvier gave a favourable verbal account of the Baron Gérando's work On the education of the deaf and dumb.

Sept. 3.—The Minister of the Interior transmitted the ordonnance of the King, by which the nomination of Dr. Young was confirmed. —M. Deygalières sent two Observations on scrofulous diseases, cured according to the principles of his new method.—M. Tournal, junior, of Narbonne, announced the discovery of caverns containing fossil bones, near that city.—M. Lalanne presented an instrument which he names *sécateur perspectif.*—Mr. Perkins read a memoir On new high-pressure machines.—M. Dumeril gave an account of the researches of M. Velpeau on the human ovum.—M. Chevreul, in the name of the Commission, made a favourable report respecting the memoirs of M. Sérullas.—M. Blainville began the reading of a report on the work of M. Jacobson On the reproduction of bivalves. —M. Parseval presented a memoir On the integration of linear equations.—M. Cauchy read a memoir On the determination of the series of Lagrange, and the conditions of convergence.

Sept. 10.—M. Rambur, a physician of the department of Inare and Loire, sent a description of a monstrous child which had two bodies and only one head.—M. Blainville made a verbal communication respecting the organization of a species of *Terebratula.*—M. Navier read the report respecting M. Clement's memoir On the escape of elastic fluids and safety valves, &c.—MM. Julia Fontenelle and Poisson read a notice respecting a new kind of paper made from liquorice.—M. Boisduval read a memoir On the monograph of the tribe *Zyneides.*

Sept. 17.—M. Raspail announced that he had discovered in the subterraneous logs of typha, a fecula possessing very peculiar characters, which he details.—M. Milne Edwards deposited a sealed packet.—M. Haldat sent two memoirs, one On diffraction, and the other On magnetism in motion.—The following memoirs were read: On the general system of the internal navigation of France, by M. Brisson; A second memoir, by M. Cauchy, On the application of the calculus of residuums to physico-mathematical questions; Researches into the vertebral organization of the lower order of animals, by M. Robineau Desvoidy.—On the magnetic action exerted in all bodies by the influence of a very powerful magnet, by M. Becquerel.

Sept. 24.—M. Gendrin, a physician, sent some researches into the heat of hot springs.—M. Mozembas, of Bordeaux, sent a memoir On the means of constructing lightning conductors with œconomy.

nomy.—M. Tournal sent a second letter On the caverns containing fossil bones, which he had discovered.—M. Raspail read a memoir On the *Alcyonellæ* of marshes.—M. Gerard, in the name of a Commission, gave a favourable account of the last memoir by M. Vicat.—M. Dugez read a memoir On a monstrous conformation in the heart of a new-born infant.—M. Chevreul gave a detailed account of the recent labours of MM. Robiquet and Colin On the colouring matters of madder.—M. Villermet read a memoir entitled, *Statistique des conceptions*.—M. Velpeau communicated new researches On the human ovum.

Jan. 7, 1828.—Mr. Ivory was nominated a correspondent, in the section of Geometry.

## XXV. *Intelligence and Miscellaneous Articles.*

### ON THE FLUIDITY OF SULPHUR AND PHOSPHORUS AT COMMON TEMPERATURES:—BY MR. FARADAY.

I PUBLISHED some time ago a short account of an instance of the existence of fluid sulphur at common temperatures; and though I thought the fact curious, I did not esteem it of such importance as to put more than my initials to the account. I have just learned through the *Bulletin Universal* for September, p. 178, that Signor Bellani had observed the same fact in 1813, and published it in the *Giornale di Fisica*, vol. vi. (Old Series.) I also learn, by the same means, that M. Bellani complains of the manner in which facts and theories, which have been published by him, are afterwards given by others as new discoveries; and though I find myself classed with Gay-Lussac, Sir H. Davy, Daniell, Bostock, &c. in having thus erred, I shall not rest satisfied without making restitution,—for M. Bellani, in this instance, certainly deserves it at my hand.

Not being able to obtain access to the original journal, I shall quote M. Bellani's very curious experiments from the *Bulletin*, in which they appear to be fully described. "The property which water possesses, of retaining its fluid state, when in tranquillity, at 10° or 15° below its freezing point, is well known: phosphorus behaves in the same manner; sometimes its fluidity may be retained at 13° (centigrade?) for a minute, an hour, or even many days. What is singular is, that, though water cooled below its freezing point, congeals easily upon slight internal movement, however communicated, phosphorus, on the contrary, sometimes retains its liquid state even at 3°, even though it be shaken in a tube or poured upon cold water. But as soon as it has acquired the lowest temperature which it can bear without solidifying, the moment it is touched with a body at the same temperature, it solidifies so quickly, that the touching body cannot penetrate its mass. If the smallest morsel of phosphorus is put in contact with a liquefied portion, the latter infallibly solidifies, though it be only a single degree below the limit of temperature necessary;—this does not always happen when the body touching is heterogeneous.

"Sulphur

"Sulphur presented the same phænomena as phosphorus; fragments of sulphur always produced the crystallization of cold fluid portions. Having withdrawn the bulb of a thermometer which had been plunged into sulphur at 120°, it came out covered with small gobules of sulphur, which remained fluid at 60°; and having touched these one after another with a thread of glass, they became solid: although several seemed in contact, yet it required that each should be touched separately. A drop of sulphur, which was made to move on the bulb of a thermometer, by turning the instrument in a horizontal position, did not congeal until nearly at 30°; and some drops were retained fluid at 15° *i. e.* 75° of Reaumur below the ordinary point of liquefaction."—*Quarterly Journal*, Jan. 1828, p. 469.

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### ELEMENTARY NATURE OF BROMINE.

Iodine colours a solution of starch blue, bromine renders a similar solution orange colour. M. A. de la Rive added a few drops of bromine to a solution of starch coloured blue by iodine, and obtained a compound which gave two distinct colours with starch—one brown, the other yellow; the difference of colour corresponding with the two bromides of iodine described by M. Balard. These compounds of iodine and bromine, dissolved in a solution of starch, were subjected to the voltaic pile: immediately the yellow solution became blue about the negative pole and orange about the positive pole, indicating the separation and places of the iodine and bromine. Thus the smallest quantity of iodine may be discovered in bromine; but when the experiment was resorted to, to prove whether the idea thrown out, that bromine was a compound of chlorine and iodine, was founded in fact or not, it gave no such indication, and a solution of bromine in starch electrified for a long time together, gave no appearance of iodine. Hence M. de la Rive concludes, that bromine contains no iodine, but is an element analogous to iodine and chlorine. When bromine and iodine are combined, the former passes to the positive pole, and is consequently more negative than the latter; which accords with the observation of M. Balard, that it should occupy a place between chlorine and iodine. According to the *Bulletin Universel*, when the letter to M. Arago, containing an account of the facts above referred to, was read to the Academy of Sciences, that body decided that the assertion of M. Dumas, that bromine was a compound of chlorine and iodine, should be considered as retracted, and that it should be so entered upon the *proces-verbal* of the sitting.—*Ibid.* p. 466.

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### QUANTITY OF BROMINE IN SEA WATER.

One hundred pounds of sea water, taken up at Trieste, treated by chlorine, ether, &c. according to M. Balard's process, produced five grains of bromide of sodium, or 3·278 grains of bromine. It would appear, that in the sea water of Trieste, the bromine is unaccompanied by any iodine; and the same is the case, according to M. Hermbstadt with the waters of the Dead Sea. In the water of the Mediterranean, on the contrary, iodine always appears with the bromine.—*Ibid.* p. 466.

### SALE OF BROMINE.

The discoverer of bromine, M. Balard, has been enabled, by his improvements, to prepare that peculiar body in quantities sufficient to permit its sale. It may be obtained at his shop, Rue Argenterie at Montpellier, or at M. Quesneville's manufactory of chemical substances, at Paris. The price is four francs the gros (about 60 grains), fourteen francs the half ounce, and twenty-three francs the ounce.—*Ibid.*

### PREPARATION OF IODOUS ACID.

M. Pleischl says that, in preparing this acid, three parts of chlorate of potash with one of iodine are to be used, and not equal parts, according to M. Sementini; and also that it is indispensable to cool the receiver considerably during the whole operation.—*Ibid.*

### NEW BORATE OF SODA.

M. Payen lately presented to the Society of Pharmacy a new borate of soda, which will advantageously be substituted for calcined borax. It crystallizes in regular octahedrons, is harder than common borax, and is almost as sonorous as cast iron: its fracture is vitreous, and rather undulated. When immersed in water, the crystals become opake, and retain their opacity in dry air.

This borate differs but little from common borax, except in containing less water of crystallization. It is more convenient for soldering copper than common borax, because it does not swell so much.—*Journal de Pharmacie*, Dec. 1827. p. 624.

### ANALYSIS OF A SALIVARY CONCRETION.

This concretion was analysed by M. Lecanu: it weighed 0·45 gramme, its form was oval and slightly rough in the surface: when broken it presented two distinct layers; the central one was hard, compact, and of a gray colour; the outer one was friable and perfectly white. It yielded by analysis:

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|---|---|
| Phosphate of lime ........... | 75 |
| Carbonate of lime ........... | 20 |
| Animal matter and loss ........ | 5 |
| | 100 |

This salivary concretion differs, then, in its composition from those previously analysed by MM. Wollaston, Laugier, and Henry jun.;—from the first, by the presence of carbonate of lime; from the second, by the absence of carbonate of magnesia; and from the third, by the absence of carbonate and phosphate of magnesia and common salt. It contains the same principles as the salivary concretions of the horse, elephant, and cow, analysed by M. Lassaigne.—*Ibid.* p. 626.

### HAIDINGERITE,—A NEW MINERAL SPECIES.

This substance, so called in honour of M. Haidinger, is an ore of antimony which is found near Chazelles in Auvergne. The ore was rejected,

rejected, because it yields metal of so bad quality that manufacturers could not use it. A portion of the ore was sent to M. Berthier, who gives the following account of it. Haidingerite has not yet been met with in regular forms, but some cavities exhibit rudiments of prismatic crystals, having the appearance of those of common sulphuret of antimony: this new mineral occurs generally in confused laminated masses, mixed with quartz, white ferruginous carbonate of lime, and cubic pyrites. Its colour is iron gray, and its surface is frequently covered with iridescent tints. It has generally less splendour than sulphuret of antimony, without any shade of blue. It does not obey the magnet. It has not been procured in pieces sufficiently pure to admit of its specific gravity being ascertained. It readily melts before the blowpipe, and is quickly acted upon by cold muriatic acid, and gives out pure sulphuretted hydrogen gas, and is totally dissolved, except some pyrites and quartz; no sulphur is deposited.

By analysis;—taken the mean of several experiments, it yielded

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| Sulphur | 28·3 |
| Antimony | 48·3 |
| Iron | 14·9 |
| Zinc | 00·3 |
| Sulphuret of Iron | 03·2 |
| Quartz | 03·2 |
| | 98·2 |

or independently of the gangue, it consists of

| | | | | |
|---|---|---|---|---|
| Sulphur | 30·3 | or | Sulphuret of Antimony | 71·5 |
| Antimony | 52·0 | | Protosulphuret of Iron | 25·5 |
| Iron | 16·0 | | Sulphuret of Zinc | 00·5 |
| Zinc | 00·3 | | | |
| | | | | 97·5 |
| | 98·6 | | | |

*Ann. de Chim. et de Phys.* tom. xxxv. p. 351.

The above affords an excellent illustration of the complex views of foreign chemists with respect to atomic composition. M. Berthier considers it as composed of

| | | |
|---|---|---|
| 18 atoms of Sulphur, | or | 4 atoms of Sulphuret of Antimony. |
| 4 atoms of Antimony, | | 3 atoms of Protosulphuret of Iron. |
| 3 atoms of Iron. | | |

Adopting the simpler views generally entertained in this country, the mineral in question is constituted of

| | | | |
|---|---|---|---|
| 3 atoms of Sulphur | or | 2 atoms of Sulphuret of Antimony | 120 |
| 2 atoms of Antimony | | 1 atom of Protosulphuret of Iron | 44 |
| 1 atom of Iron | | | |
| | | | 164 |

Edit.

## BROMIDE OF SELENIUM:—BY M. SÉRULLAS.

Selenium combines with bromine in several proportions; but five parts of the former and one part of the latter appear to form the most intimate compound. To obtain bromide of selenium, the latter in

 powder

powder is to be put into a moderately large tube, and the bromine is to be poured upon it. Combination takes place rapidly with a noise similar to that produced by immersing red-hot iron in water; much heat is given out, and the mixture becomes instantaneously solid: the compound has a red brown colour, similar to that of iodide of phosphorus; and some parts are yellowish like chloride of iodine; it gives out vapour when exposed to the air, which has an odour similar to that of chloride of sulphur. It is entirely soluble in water, except some particles of selenium, which precipitate. The filtered solution is colourless when there is no uncombined bromine; it is strongly acid, and consists of selenic and hydrobromic acids. The solution when saturated by potash, gives, by evaporation, a crystallized mixture of seleniate and hydrobromate: on the addition of muriatic acid, either before or after saturation with potash, the selenium is precipitated in particles of a fine red colour.

When a bar of iron or zinc is kept immersed in the solution of bromide of selenium, rapid action occurs, and especially with the zinc; hydrogen gas is abundantly evolved, and the metallic bar is covered with selenium, which adheres in the same manner as one metal precipitated by another; by agitation the selenium is detached, and bubbles of hydrogen gas, which had also been retained, are given out. The selenium being separated by filtration, the hydro-bromate of zinc may be decomposed by carbonate of potash, by which carbonate of zinc is precipitated and hydro-bromate of potash remains in solution. This action of zinc or iron, in the solution of bromide of selenium, is similar to that which occurs when either of these metals is put into a mixture of selenic and muriatic acids; the selenium is deoxidized, and hydrogen evolved by the decomposition of water. Is the whole of the hydrogen evolved, or is part of it employed in reducing the selenic acid? Or is there, as with the metals, a voltaic circuit formed? A slight smell of decayed cabbage is perceptible, which is one of the characteristics of oxide of selenium. When bromide of selenium is strongly heated, a portion of it sublimes, and assumes a yellow appearance, the remainder is decomposed into bromine and selenium.—*Ibid.* p. 349.

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ELECTRICITY OF GASES AND THE ATMOSPHERE.

M. Pouillet concludes from numerous experiments: 1st, That gases give out electricity, when they combine together, or when they unite with solid or fluid bodies; and in these combinations, oxygen always gives out positive electricity, and the combustible body, whatever it may be, gives out negative electricity; and reciprocally, when a compound is decomposed, each of the elements then requiring the electricity which it had given out, is in the opposite state of electricity. This reciprocity shows in what consists the difference of the nascent state of a body from its ultimate condition.

2ndly. The action of vegetables upon the oxygen of the air is one of the most permanent and powerful causes of atmospheric electricity; and when it is considered on one hand that a gramme (about 15·5 grains) of charcoal, in becoming carbonic acid, gives out sufficient electricity

electricity to charge a Leyden jar; and on the other hand, that the charcoal which is contained in vegetables does not give out less electricity than charcoal which burns freely, one may conclude, says M. Pouillet, as my direct experiments tend to prove, that over a surface of vegetation 100 metres square, more electricity is produced in a day, than is necessary to charge the strongest electrical battery.—*Ibid.* p. 420.

## ROSE-COLOURED PETROSILEX FROM SAHLBERGH IN SWEDEN.

M. Berthier has analysed this substance. Its characters are, that it is compact, homogeneous; its fracture is very fine-grained, waxy; its colour is deep flesh red; it is very translucent, and capable of receiving a fine polish. By the blowpipe it melts into a white enamel; but it is much less fusible than felspar. When it is calcined in a strong white heat, it does not soften, nor does it alter in colour or appearance. When strongly heated in a charcoal crucible in a porcelain furnace, its angles are rounded, it becomes milk-white, and resembles chalcedony. It loses only about 1-200dth of its weight in this high temperature—a loss which is to be attributed to hygrometric moisture. It yielded by analysis

| | |
|---|---|
| Silica | 79·5 |
| Alumina | 12·2 |
| Soda | 06·0 |
| Magnesia | 01·1 |
| Oxide of iron | 00·5 |
| | 99·3 |

*Ibid.* t. xxxvi. p. 22.

## NONTRONITE,—A NEW MINERAL.

This mineral occurs in the arrondissement of Nontron, in the northern part of the department de la Dordogne, which contains a mine of manganese of considerable importance, near the village of Saint Pardoux, in heaps of which this mineral was found. This substance is disseminated throughout the ore in kidney-shaped masses, which are usually very small, and are rarely as large as the fist. This mineral is compact, of a straw or canary yellow colour, with a shade of green; its fracture is uneven: it is opake, unctuous to the touch, and very tender; its consistence is that of clay, and it is easily scratched with the nail; it takes a fine polish, and assumes a resinous lustre by being rubbed with very soft bodies. It flattens and becomes lumpy under the pestle, and is not reduced to powder; it has not the argillaceous smell, nor any action upon the magnet. When immersed in water it immediately disengages numerous bubbles of air; it becomes translucent on the edges without softening or losing its form, as clays do; and if after some hours it is taken from the water and weighed, after being well wiped, it is found to have gained about 1-10th of its weight of water. When heated in a glass tube it loses water at a low temperature, and becomes of a dirty red colour. When strongly calcined in a crucible, it assumes a similar appearance, and its

its weight is diminished 19 to 21 per cent. After calcination it is sensibly magnetic.

Muriatic acid acts very readily upon it; the solution does not contain the smallest trace either of manganese, protoxide of iron, or of alkali: it contains peroxide of iron, alumina, and magnesia; the insoluble portion is gelatinous, and is composed of silica, soluble in the alkaline solutions, and sometimes mixed with a small quantity of clay, when the mineral has not been picked with sufficient care.

By analysis this substance yielded

| | |
|---|---|
| Silica | 44· |
| Peroxide of Iron | 29· |
| Alumina | 3·6 |
| Magnesia | 2·1 |
| Water | 18·7 |
| Clay | 1·2 |
| | 98·6 |

*Ibid.* p. 22.

ANALYSIS OF SOME VEGETABLE PRODUCTS.

M. F. Marcet has analysed some vegetable bodies: the method employed was that proposed by M. Gay-Lussac of using peroxide of copper. Common starch yielded

| | |
|---|---|
| Carbon | 43·7 |
| Oxygen | 49·7 |
| Hydrogen | 6·6 |
| | 100· |

Roasted starch gave

| | |
|---|---|
| Carbon | 35·7 |
| Oxygen | 58·1 |
| Hydrogen | 6·2 |
| | 100· |

It appears therefore that roasted starch contains much more oxygen and less carbon than common starch. *Hordein* is the name given by Proust to a substance separated from barley flour. It yielded

| | |
|---|---|
| Carbon | 44·2 |
| Oxygen | 47·6 |
| Hydrogen | 6·4 |
| Azote | 1·8 |
| | 100· |

M. Marcet remarks that some chemists have considered this substance as a modification of starch, and others as analogous to sawdust. Dr. Thomson appears to regard it as of the same nature as the parenchyma of the potato: this latter substance gave M. Marcet

| | |
|---|---|
| Carbon | 37·4 |
| Oxygen | 58·6 |
| Hydrogen | 4· |
| | 100· |

It

It appears therefore that hordein differs very materially from this substance as well as from saw-dust, of which the analysis is thus given by MM. Gay-Lussac and Thenard:

| | |
|---|---|
| Carbon | 52· |
| Oxygen | 42·4 |
| Hydrogen | 5·6 |
| | 100· |

M. Marcet therefore considers hordein a peculiar substance, but most resembling starch. The azote which it contains may, he thinks, be derived from the presence of gluten.

The following are the results of M. Marcet's analysis of gluten and yeast:

| | Gluten. | Yeast. |
|---|---|---|
| Carbon | 55·7 | 30·5 |
| Oxygen | 22·0 | 57·4 |
| Hydrogen | 7·8 | 4·5 |
| Azote | 14·5 | 7·6 |
| | 100· | 100· |

*Ibid.* p. 27.

## NEW CHLORIDE OF MANGANESE.

M. Dumas obtained this compound by putting a solution of manganesic acid into contact with sulphuric acid and fused common salt. Water and the new chloride are formed; the former is retained by the acid, the latter volatilizes in a gaseous form, with a greenish tint, and when passed into a tube cooled to 5° or 4° of Fahrenheit, it condenses into a liquid of a brownish-green colour. The most simple process appears to be to form a common green chameleon, to convert it into red chameleon by sulphuric acid, and to evaporate the solution, which will give a residue consisting of sulphate and manganesate of potash. This mixture acted upon by concentrated sulphuric acid produces the solution of manganesic acid, into which the common salt is to be thrown in small pieces until the vapours which rise are colourless; the latter effect is a sign that all the manganesic acid is decomposed, and that muriatic acid only is produced. This chloride of manganese corresponds in proportions to the manganesic acid; it is readily formed and examined, but not easily preserved. An analogous compound is obtained when a fluoride is used instead of the chloride, but a sufficient quantity for examination has not yet been procurable.—*Ibid.* xxxvi. 81.

## ON THE POWER OF WATER AND BROMINE IN CONDUCTING ELECTRICITY.

M. de la Rive found, as had been previously ascertained by M. Balard, that pure dry bromine did not conduct the electricity of a voltaic battery, consisting of sixty pairs of plates very strongly charged, a delicate galvanometer being the test. A similar experiment was then made with pure water contained in a glass capsule, and communicated

cated with the battery and galvanometer by platina wires; and the deviation of the needle was scarcely sensible. A few drops of bromine were then added to the water, which soon imparted a yellow colour to the water; being now included in the voltaic circuit, the galvanometer needle was deviated 70°, and an abundant disengagement of gas took place from the platina wires. There were oxygen and hydrogen in the usual proportions, showing that water only had been decomposed. From these experiments it results, that a body which conducts voltaic electricity very imperfectly, namely, pure water, may be rendered a good conductor by holding in solution a very minute quantity of a perfectly non-conducting substance, namely, bromine: the same fact occurs with iodine, and iodine and water.

*Ibid.* xxxv. p. 161.

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## EFFECTS OF HEAT UPON SULPHUR.

According to M. Dumas fused sulphur begins to crystallize between 226° and 228°; between 230° and 284° it is as liquid as a clear varnish, and of an amber colour; at about 320° it begins to thicken, and acquire a red colour; on increasing the heat, it becomes so thick, that it will not pour. This effect is most marked between 428° and 572°; the colour is then a red brown: from 572° to the boiling point it becomes thinner, but never so fluid as at 248°; the deep red brown colour continues till it boils. When the most fluid sulphur is suddenly cooled, it becomes brittle; but the thickened sulphur similarly treated, remains soft, and more soft as the temperature has been higher. Thus at 230° the sulphur was very liquid and yellow; and cooled suddenly by immersion in water, it became yellow and very friable; at 374° it was thick, and of an orange colour; but by cooling became at first soft and transparent, but soon friable and of the ordinary appearance: at 428° it was red and viscid; and when cooled, soft, transparent, and of an amber colour; at the boiling it was deep brown red colour; and when cooled very soft, transparent, and of a red brown colour. It is not necessary, as is sometimes stated, to heat the sulphur a long time to produce this effect; all depends upon temperature. The only precaution necessary is, to have abundance of water, and divide the sulphur into small drops or portions, that the cooling may be rapid. If it be poured in a mass, the interior cools slowly, and acquires the ordinary hard state. When the experiment is well made at 446°, the sulphur may be drawn into threads as fine as a hair, and many feet in length.

*Ibid.* xxxvi. p. 83.

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## PEROXIDE OF BARIUM.

M. Quesneville recommends the following process for preparing this compound.—Put nitrate of barytes into a coated earthenware retort, to which a tube is to be attached, for the purpose of conveying the liberated gases to a water-trough. The heat is to be continued until pure oxygen gas comes over, and the operation is then to be stopped. The peroxide of barium thus obtained falls to pieces in water without producing heat; when boiled in water, oxygen gas is evolved,

ved, and by a strong heat it is reduced to protoxide. Sulphuric acid evolves no nitric acid, nor does nitric acid produce nitric oxide when mixed with it. The protoxide of barium evidently becomes peroxide by combining with the nascent oxygen of the decomposed nitric acid.

*Ibid* xxxvi. p. 108.

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## ON CERTAIN HABITUDES OF SEPIÆ.

Portsmouth, Aug. 15, 1827.

Gentlemen,—A fisherman this morning brought me a large bunch of the eggs of the Sepia, or common cuttle-fish, fastened to a sea weed. The young were alive in the fluid contained in the membranaceous coverings, and their motions could be readily seen by removing the outer or dark membrane. As I wished to preserve them for a specimen to be added to a long series of bottles containing the natural history, anatomy, &c. of this fish, which I have presented to the museum of the Portsmouth Institution (the eggs being larger than any I had before obtained), I laid the specimen on a mahogany table while I prepared some spirit. On returning in a few minutes, I found the young Sepiæ had (with one exception) burst the membranes near the stem-like process, and were swimming about in the fluid which escaped during the time of my removing them by means of a broad spatula into a glass of salt water. I was surprised by the amazing strength with which these minute fish (not bigger than a large pea) ejected a current of water, followed by the dark-coloured fluid peculiar to them, through an extent of at least half a yard, and with such force as to be quite unpleasant to the face. I have deemed it worth while to notice these facts, as it is uncommon to obtain the eggs at the state of perfection, even here on the sea coast. The little creatures lived in my window for several hours, exhibiting all the voracious propensities of the full-grown ones. I am, &c.,

Henry Slight, Surgeon,
Librarian to the Portsmouth Institution.

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## NOTICE OF AN ERROR IN GALBRAITH'S MATHEMATICAL TABLES AND FORMULÆ.

In the practical problems, at page 167, for computing the deflection of beams, fixed at one end and loaded at the other with any given weight, an erroneous rule is copied from Mr. Barlow's Treatise on the Strength of Timber. When errors of this nature are circulated in books of such general practical utility, it becomes an important although an ungracious duty, to point them out.

The rule should be—

Multiply the tabular value of E by the breadth, and cube of the depth, both in inches.

Multiply also the cube of the length in inches, by the given weight, and that product again by 16.

Divide the latter product by the former for the deflection sought.

N.B. The tabular value of E is four times the weight in pounds of the modulus of elasticity.

B. Bevan.

ERRORS IN HUTTON'S LOG. TABLES (FIFTH EDITION).

| | | | |
|---|---|---|---|
| For Log. Tang. of 0° 44′ | 13″ | —8·1193361 | read 8·109 &c. |
| | 14 | —8·1194998 | read 8·109 &c. |
| | 15 | —8·1196634 | read 8·109 &c. |
| | 16 | —8·1198269 | read 8·109 &c. |
| | 17 | —8·1199904 | read 8·109 &c. |

J. Nixon.

CRYSTALLIZATION OF PHOSPHORUS.

By the fusion and careful refrigeration of a large quantity of phosphorus, M. Frantween has obtained very fine crystals of an octahedral form, and as large as a cherry-stone.

CHEMICAL EXAMINATION ON ANCIENT INSTRUMENTS, &c.

M. Vauquelin has analysed a poignard blade, formed entirely of copper; a mirror found to consist of 85 parts of copper, 14 tin, and 1 of iron per cent; and a blue colour found in a tomb, gave by analysis, silica 70, lime 9, oxide of copper 15, oxide of iron 1, soda and potash 4. A blue colour similar to this both in tint and composition, was found in the bottom of a furnace in which copper had been fused at Romilly.—*Bull. Univ.* A. vii. 264.

LONDON INSTITUTION.

Encouraged by the success of the Royal Institution, the managers of the London Institution propose, during the present season, to try an experiment which they trust will prove acceptable to the proprietors.

The library and theatre of the London Institution will be prepared during eight evenings for a soirée or converzatione, to which proprietors and their friends will be admitted under certain regulations. Coffee and tea will be supplied in the library, where it is intended to introduce all new scientific inventions or discoveries, that can be exhibited by models or drawings. New literary works, original drawings, and scarce books, will constitute another source of interest and information. This plan will be attended with the advantage of introducing learned foreigners and men of science to a complete view of the establishment, and to a social conversation with its managers and proprietors. At some period during the evening an adjournment will take place to the Theatre, where it is intended to give a short discourse or lecture, either honorary or professional, on any new machinery, or on any chemical, philosophical, or literary novelty that may be considered by the managers to deserve the attention of the Institution.

The following is a notice of the series of lectures for the season, to be delivered in the Theatre of this Institution:—

Course I.—On the motive forces of the arts: illustrated by models of machines, steam-engines, &c.; by Norton Webster, Esq. To commence on Monday, the 4th of February, at seven o'clock in the evening.

Course

Course II.—On the phænomena and history of igneous meteors and meteorites: illustrated by a series of transparent paintings of meteors, and an extensive collection of meteorites; by E. W. Brayley, jun. Esq. A.L.S. To commence on Saturday, the 9th of February, at one o'clock in the afternoon.

Course III.—On music, particularly as to vocal music (illustrated by voices), by Samuel Wesley, Esq. To commence on Tuesday, the 4th of March, at one o'clock in the afternoon.

Course IV.—On poetry and the drama in general, and on Milton and Shakspeare in particular, by John Thelwall, Esq. To commence on Thursday, the 6th of March, at seven o'clock in the evening.

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## LIST OF NEW PATENTS.

To R. W. Winfield, of Birmingham, for his improvements in tubes or rods produced by a new method of manufacturing and in the construction only, and for manufacturing the same, with various other improvements, into parts of bedsteads and other articles.—Dated the 4th of December 1827.—6 months allowed to enrol specifications.

To J. Meadon, of Millbrook near Southampton, for improvements on wheels for carriages.—4th of December.—6 months.

To S. Wilkinson, of Holbeck, Yorkshire, for improvements in mangles.—4th of December.—6 months.

To Maurice de Jough, of Warrington, cotton-spinner, for improvements in machines adapted for spinning, doubling, twisting, roving, or preparing cotton, &c.—4th of December.—6 months.

To T. Tyndall, of Birmingham, for improvements in the manufacture of buttons, and in the machinery for manufacturing the same: communicated from abroad.—4th of December.—6 months.

To D. Ledsam and W. Jones, of Birmingham, for improvements in machinery for cutting sprigs, brads, and nails.—4th of December.—6 months.

To J. Robinson, of Merchants-row, Limehouse, for an improvement in the manufacture of brushes of certain descriptions, and in the manufacture of a material and the application thereof to the manufacture of brushes and other purposes.—4th of December.—6 months.

To Paul Steenstrup, of Basing-lane, London, esquire, for improvements in machinery for propelling vessels, and other purposes.—11th of December.—6 months.

To J. H. Sadler, of Hoxton, Middlesex, for improvements in power-looms.—13th of December.—6 months.

To R. Rewcastle, of Newcastle-upon-Tyne, for an improved method of ballasting ships or vessels.—13th of December.—6 months.

To R. Stein, of Regent-street, Oxford-street, for an improvement in applying heat to the purpose of distillation.—13th of December.—6 months.

To F. B. Geithner, of Birmingham, for improvements on castors for furniture, &c.—13th of December—6 months.

To H. Peto, of Little Britain, for an apparatus for generating power.—13th of December.—6 months.

To

To J. A. Berrollas, of Nelson-street, City-road, for a method of winding up a pocket watch, or clock, without a key, which he calls "Berrollas's keyless watch or clock;" and also a certain improvement to be applied to his late invented detached alarum watch.—13th of December.—2 months.

To Lieut. A. M. Skene, of Jermyn-street, for an improvement in propelling vessels, and for working under-shot water-mills.—13th of December.—6 months.

To J. L. Stevens, of Plymouth, for a new method of propelling vessels by the aid of steam or other means, and for its application to other purposes.—18th of December.—6 months.

To T. Tyndall, of Birmingham, for improvements in the machinery for making nails, brads, and screws: communicated from abroad.—18th of December.—6 months.

To J. George, of Chancery-lane, esquire, barrister-at-law, for his invention for preserving decked ships or vessels, so as to render them less liable to dry-rot, and for preserving goods on board such ships and vessels from damage by heat.—18th of December.—6 months.

To T. S. Holland, of the city of London, esquire, for combinations of machinery for generating and communicating power and motion, applicable to propelling of fixed machinery, as also floating bodies, carriages, and other locomotive machines.—19th of December.—6 months.

To W. Harland, M.D., of Scarborough, for improvements in apparatus for propelling locomotive carriages, which improvements are also applicable to other useful purposes.—21st of December.—6 months.

To C. A. Furguston, of Mill Wall, in the parish of All Saints, Poplar, mast-maker, and J. Falconer Atlee, of Prospect-place, Deptford, for their improvements in the construction of made masts.—22nd of December.—6 months.

To W. Hale, of Colchester, for his improvements in machinery for propelling vessels.—27th of December.—6 months.

To W. Gossage, of Leamington Priors, Warwickshire, for improvements in the construction of cocks for the passage of fluids.—2nd of January, 1828.—6 months.

To T. Botfield, of Hopton Court, Salop, for improvements in making iron, or in the method or methods of smelting and making of iron.—2nd of January.—4 months.

To J. Hall, jun., of Ordsall near Manchester, for improvements in dyeing piece-goods by machinery.—2nd of January.—2 months.

To J. Cl. Daniell, of Stoke, Wilts, for improvements in dressing cloths, and in the machinery applicable for that purpose.—2nd of January.—6 months.

To W. Morley, of Nottingham, for improvements in and additions to machinery now in use for making lace or net.—9th of January.—6 months.

To J. A. Hunt Grubbe, of Stanton Saint Bernard, Wilts, clerk, for a transmitting heat wall for the ripening of fruit.—9th of January.—6 months.

To J. Gilbertson, of Hertford, for an improvement in the construction

tion of furnaces, by which they consume their own smoke.—15th of January.—2 months.

To C. Hooper, of Spring Gardens, in the parish of Marston Bigott, Somersetshire, for an improved machine for shearing and cropping woollen and other cloths.—15th of January.—2 months.

To J. Evans the younger, of Moreton Mills, near Wallingford, Berks, for improvements on steam engines.—15th of January.—6 mon.

To J. Blades, of Clapham, Surrey, for an improvement in the waterproof stiffening for hats: communicated from abroad.—15th of January.—6 months.

To W. Newton, of Chancery-lane, for an improved surgical chair-bed with various appendages.—15th of January.—6 months.

To G. D. Harris, of Field-place, near Stroud, Gloucestershire, for improvements in dressing and preparing woollen yarns, and in cleansing, dressing, and finishing woollen cloths, &c. and in the apparatus for performing the same.—15th of January.—6 months.

To J. Falconer Atlee, of Prospect-place, Deptford, for improvements on bands or hoops for securing made and other masts, bowsprits and yards, and applicable to other purposes.—15th of January.—6 months.

To W. Erskine Cochrane, esquire, of Regent-street, for improvements in certain apparatus for cooling, and other purposes.—15th of January.—6 months.

To J. Taylor Beale, of Church-lane, Whitechapel, and G. Richardson Porter, of Old Broad-street, for their new mode of communicating heat for various purposes.—19th of January.—6 months.

To W. Percivall, of Knightsbridge, for improvements in the construction and application of shoes without nails to the feet of horses and certain other animals.—19th of January.—6 months.

To G. Jackson, of Saint Andrew, Dublin, for improvements in machinery for propelling boats and other vessels, which improvements are also applicable to water-wheels and other purposes.—19th of January.—6 months.

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## METEOROLOGICAL OBSERVATIONS FOR DECEMBER 1827.

### *Gosport.—Numerical Results for the Month.*

Barom. Max. 30·58 Dec. 28. Wind NE.—Min. 29·07. Dec. 1. Wind W.
Range of the mercury 1·51.

| | |
|---|---|
| Mean barometrical pressure for the month . . . . . . . . . | 29·810 |
| ——— for the lunar period ending the 18th instant . . . . . | 29·763 |
| ——— for 15 days with the moon in North declination . . . | 29·693 |
| ——— for 15 days with the moon in South declination . . . . | 29·834 |
| Spaces described by the rising and falling of the mercury . . . | 8·900 |

Greatest variation in 24 hours 0·560.—Number of changes 26.
Therm. Max. 57° Dec. 5. Wind W.—Min. 31° Dec. 29. Wind N.
Range 26°.—Mean temp. of exter. air 47°·82. For 30 days with ☉ in ♐ 47·50
Max. var. in 24 hours 17°·00—Mean temp. of spring water at 8 A.M. 53°·87

### De Luc's *Whalebone Hygrometer.*

| | |
|---|---|
| Greatest humidity of the air on nine different days . . . . . . . . . | 100° |
| Greatest dryness of the air in the afternoon of the 6th : . . . | 55 |
| Range of the index . . . . . . . . . . . . . . . . . . | 45 |

Mean

Mean at 2 P.M. 78°·2—Mean at 8 A.M. 83°·0—Mean at 8 P.M. 85·5
—— of three observations each day at 8, 2, and 8 o'clock . . . 82·2
Evaporation for the month 0·60 inch.
Rain near ground 5·625 inch.—Rain 23 feet high 5·115 inch.
Prevailing Wind S.W.

*Summary of the Weather.*

A clear sky, 3; fine, with various modifications of clouds, 9; an overcast sky without rain, 7½; foggy 2; rain, 9½.—Total 31 days.

*Clouds.*

| Cirrus. | Cirrocumulus. | Cirrostratus. | Stratus. | Cumulus. | Cumulostr. | Nimbus. |
|---|---|---|---|---|---|---|
| 15 | 8 | 28 | 0 | 14 | 12 | 23 |

*Scale of the prevailing Winds.*

| N. | N.E. | E. | S.E. | S. | S.W. | W. | N.W. | Days. |
|---|---|---|---|---|---|---|---|---|
| 1½ | ½ | 1 | 1 | 4 | 10½ | 8 | 4½ | 31 |

*General Observations.*—The weather to the 24th inclusive was remarkably mild for the season, but windy and wet, five inches and a quarter of rain having fallen; and on several days before the winter solstice it was remarkably dark, from there being no opening in the prevailing strata of clouds to admit the passage of the solar rays.

Early in the morning of the 12th there was a thick fog, which gave out a very strong sulphurous smell: the 29th was also distinguished by a cold dense fog throughout the day and night.

The last spring tides were high in Portsmouth harbour, and along the southern coast, also at Liverpool, and along the western coast, in consequence of so great a fall of rain, with accompanying south and south-west gales. The highest spring tide was a few minutes before 12 P.M. on the 18th, although it was the first after the change of the moon. The rain on the 18th, 19th, and 20th continued fifty hours without intermission.

On the 21st and 22nd, thunder storms, with lightning and hail, were experienced in several places in Hampshire.

On the 25th a parhelion appeared at mid-day on the western side of the sun, when the *cirrocumulus* and attenuated *cirrostratus* clouds were beautifully tinged with faint prismatic colours thirty degrees distant from the sun's centre.

The *maximum* temperature of the external air occurred in the nights of the 3rd, 7th, 9th, 13th, 15th, 17th, 21st and 23rd, instead of in the days. These frequent occurrences rendered the circumstance remarkable and deserving an explanation. It seems to be influenced by the sun's position in the zodiac at this time of the year; the temperature of the ground at and near the surface; and the inosculation of the sea air with that over the land in the evening, just before the coming on of rain from the western side of our meridian. 1st. The sun's meridional altitude in this latitude in December varies from 16 to 17¾ degrees, and from so great an obliquity of his rays, or so small an angle, that from this altitude they make with the horizon, only three or four degrees of thermometrical heat are produced near the earth's surface, more than the temperature of the ground. 2ndly. The temperature of the atmosphere in a wet December like the present, depends very much on the temperature of the ground, and the quality of the prevailing vapours, which, if humid and accompanied with mild currents of air, then the temperatures of the ground and air are found to coincide nearly. 3rdly. If under these circumstances of temperature and condensing state of the lower medium of the atmosphere, the sea air should unite with the land air on the approach of rain after sunset, on any day in December or January on which the temperature of the air has

has been rather low, there is a great probability, from the sea air being warmer than the land air, that the *maximum* temperature will happen in the night instead of in the day.

Long solar lights and shades were very conspicuous half an hour before sunset on the 30th, and moved round the sun, in a westerly direction, like the spokes of a large wheel in slow motion. This singular appearance was no doubt caused by a brisk wind blowing against the eastern side of a hemispherical *cumulus* cloud (on whose dense surface the sun's horizontal rays had previously impinged and were reflected) so as to give it a revolving motion.

These reflected rays repeatedly varied in length, perhaps from the uneven surface of the cloud. A short time before they disappeared, they were nearly equidistant, their angular distance from each other in at least a semicircle, was about twelve degrees; and each of the horizontal rays reflected to a distance of nearly sixty degrees. The sun being near the horizon, the lower rays could scarcely be traced: but the phænomenon certainly exhibited a grand and gratifying appearance.

The mean temperature of the external air this month, is 5¼ degrees higher than the mean of December for the last twelve years, and is also for that period unprecedented.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one parhelion, one large lunar halo in the night of the 26th, which set with the moon; four meteors, one rainbow; lightning and thunder in the night of the 20th; and fourteen gales of wind, or days on which they have prevailed; namely, one from the East, three from the South, seven from South-west, and three from the West.

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REMARKS.

*London.*—Dec. 1. Cloudy. 2. Drizzly. 3. Fine. 4. Cloudy: drizzly. 5. Cloudy. 6. Fine. 7. Stormy night. 8, 9. Fine. 10. Rain: drizzly. 11. Foggy morning: rainy night. 12. Cloudy: rain at night. 13. Cloudy: foggy morning. 14. Rainy. 15. Cloudy: rainy night. 16. Fine morning: heavy shower at 3 P.M. 17. Cloudy. 18. Rain: boisterous night. 19. Rainy. 20. Fine. 21, 22. Cloudy. 23. Fine. 24. Rain. 25—27. Fine. 28. Fine day: foggy night. 29. Gloomy: dense fog all day. 30. Fine. 31. Fine: rain at night.

*Penzance.*—Dec. 1, 2. Showers. 3. Fair. 4, 5. Misty. 6. Clear. 7. Clear, fair: rain at night. 8. Clear. 9. Misty: rain. 10. Rain. 11, 12. Rain: fair. 13. Clear. 14. Rain: fair. 15. Rain. 16. Clear: hail-showers. 17. Fair: rain. 18, 19. Rain. 20. Hail-showers. 21, 22. Rain. 23. Fine: rain. 24. Rain: fair. 25. Clear. 26. Fair. 27. Clear. 28. Fair. 29—31. Clear.—Rain-gauge ground level.

*Boston.*—Dec. 1. Cloudy: rain early A.M. 2. Rain. 3. Cloudy. 4. Cloudy: rain early A.M. 5. Fine. 6. Stormy. 7—9. Fine. 10. Cloudy. 11. Fine: rain P.M. 12. Misty: rain at night. 13. Cloudy: rain early A.M. 14. Rain: rain early A.M. 15—17. Fine. 18. Cloudy: rain P.M. 19. Cloudy. 20—21. Fine. 22. Fine: rain P.M. 23. Fine. 24. Cloudy. 25. Fine: rain A.M. 26. Cloudy. 27—29. Fine. 30. Cloudy. 31. Fine.

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RESULTS.

Winds, NE. 1: SE. 4: SW. 14: W. 3: NW. 9.

| | |
|---|---|
| *London.*—Barometer: Mean height for the month ........ | 30·038 inch. |
| Thermometer: Mean height for the month ................ | 44·274 |
| Rain ........................................................ | 3·60 |
| Evaporation ................................................ | 1·24 inch. |

*Meteoro-*

*Meteorological Observations by Mr. Howard near London, Mr. Giddy at Penzance, Dr. Burney at Gosport, and Mr. Veall at Boston.*

| Days of Month, 1827. | Barometer. | | | | | | | Thermometer. | | | | | | | Wind. | | | | Evapor. | | Rain. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | London. | | Penzance. | | Gosport. | | Boston | London. | | Penzance. | | Gosport. | | Bost. | Lond. | Penz. | Gosp. | Bost. | Lond. | Gosp. | Lond. | Penz. | Gosp. | Bost. |
| | Max. | Min. | Max. | Min. | Max. | Min. | 8½ A.M. | Max. | Min. | Max. | Min. | Max. | Min. | 8½ A.M. | | | | | | | | | | |
| Dec. 1 | 29·31 | 29·21 | 29·30 | 29·26 | 29·14 | 29·07 | 28·54 | 54 | 42 | 52 | 50 | 56 | 45 | 46 | W. | NW. | W. | S. | ... | ... | ... | ... | ... | 0·24 |
| 2 | 29·91 | 29·31 | 29·50 | 29·34 | 29·26 | 29·16 | 28·70 | 45 | 42 | 50 | 46 | 50 | 46 | 44 | NW. | NW. | NW. | calm | ... | ... | ·15 | 0·520 | ... | ·11 |
| ○ 3 | 29·96 | 29·91 | 29·72 | 29·70 | 29·80 | 29·62 | 29·28 | 60 | 40 | 53 | 46 | 52 | 46 | 44·5 | NE. | NW. | N. | calm | ... | 0·05 | ·09 | ... | 0·090 | ·10 |
| 4 | 30·15 | 29·96 | 29·88 | 29·84 | 29·92 | 29·80 | 29·26 | 54 | 49 | 54 | 50 | 55 | 50 | 53 | W. | W. | W. | W. | ... | ... | ·03 | ... | ... | ·10 |
| 5 | 30·15 | 30·10 | 30·00 | 30·00 | 29·98 | 29·92 | 29·40 | 49 | 47 | 55 | 50 | 57 | 47 | 53·5 | W. | W. | W. | W. | ... | ... | ·03 | ... | ·030 | ... |
| 6 | 30·44 | 30·15 | 30·10 | 30·00 | 30·14 | 29·83 | 29·30 | 48 | 31 | 49 | 46 | 50 | 35 | 45·5 | NW. | NW. | NW. | NW. | ... | ·10 | ... | ... | ... | ... |
| 7 | 30·44 | 30·05 | 30·08 | 29·86 | 30·23 | 30·00 | 29·77 | 54 | 34 | 49 | 42 | 52 | 48 | 38 | SW. | W. | SW. | NW. | ... | ... | ·03 | ... | ·260 | ... |
| 8 | 30·13 | 30·05 | 29·86 | 29·84 | 29·95 | 29·82 | 29·84 | 48 | 28 | 52 | 46 | 51 | 37 | 43 | NW. | N. | NW. | NW. | ... | ... | ... | ... | ... | ... |
| 9 | 30·13 | 29·81 | 29·70 | 29·60 | 29·86 | 29·67 | 29·55 | 54 | 32 | 54 | 42 | 54 | 52 | 35 | SE. | SW. | S. | calm | ... | ·05 | ... | ... | ·060 | ... |
| 10 | 29·81 | 29·68 | 29·36 | 29·26 | 29·60 | 29·36 | 29·15 | 55 | 44 | 55 | 50 | 56 | 46 | 52·5 | SW. | SW. | SW. | SW. | ... | ... | ·06 | ... | ·070 | ... |
| ☾ 11 | 29·68 | 29·63 | 29·26 | 29·26 | 29·46 | 29·37 | 28·96 | 52 | 35 | 50 | 47 | 54 | 42 | 44 | SW. | W. | S. | W. | ·48 | ... | ·83 | 1·645 | ·830 | ... |
| 12 | 29·72 | 29·63 | 29·30 | 29·26 | 29·33 | 29·30 | 29·06 | 49 | 37 | 48 | 42 | 51 | 39 | 42·5 | SW. | W. | E. | calm | ... | ·10 | ·15 | ... | ·120 | ·14 |
| 13 | 29·72 | 29·68 | 29·56 | 29·52 | 29·64 | 29·50 | 29·00 | 47 | 37 | 54 | 40 | 45 | 41 | 42·5 | NW. | NW. | NW. | calm | ... | ... | ·02 | ... | ·180 | ·31 |
| 14 | 29·86 | 29·68 | 29·40 | 29·30 | 29·54 | 29·32 | 28·95 | 51 | 42 | 54 | 42 | 53 | 45 | 45 | SE. | W. | S. | NW. | ... | ... | ·40 | ... | ·770 | ·12 |
| 15 | 29·88 | 29·86 | 29·56 | 29·54 | 29·66 | 29·58 | 29·26 | 54 | 47 | 54 | 45 | 55 | 45 | 43 | NW. | SW. | SW. | NW. | ... | ·05 | ·30 | 0·840 | ·140 | ·35 |
| 16 | 30·39 | 29·88 | 29·90 | 29·70 | 29·94 | 29·66 | 29·16 | 47 | 34 | 50 | 44 | 51 | 39 | 44 | SW. | W. | W. | NW. | ... | ... | ·02 | ... | ... | ... |
| 17 | 30·39 | 30·00 | 30·06 | 30·06 | 30·17 | 30·03 | 29·66 | 52 | 38 | 52 | 44 | 52 | 47 | 38 | SW. | SW. | SW. | NW. | ... | ... | ·08 | ... | ·370 | ... |
| ● 18 | 30·00 | 29·80 | 29·74 | 29·66 | 29·82 | 29·70 | 29·33 | 54 | 51 | 53 | 49 | 55 | 52 | 51 | SW. | SW. | SW. | NW. | ... | ... | ·33 | ... | ·475 | ... |
| 19 | 29·80 | 29·75 | 29·46 | 29·42 | 29·61 | 29·57 | 29·05 | 54 | 50 | 52 | 49 | 55 | 46 | 54·5 | SW | SW. | SW. | SW. | ... | ·05 | ·34 | 1·785 | ·750 | ·16 |
| 20 | 29·87 | 29·75 | 29·52 | 29·42 | 29·56 | 29·50 | 29·00 | 49 | 38 | 50 | 43 | 50 | 42 | 45 | SW | W. | SW. | W. | ... | ... | ... | 0·500 | ·120 | ... |
| 21 | 29·75 | 29·60 | 29·62 | 29·44 | 29·68 | 29·36 | 29·22 | 48 | 37 | 50 | 42 | 53 | 48 | 37 | SW. | SW. | SW. | W. | ... | ... | ·07 | ... | ·180 | ... |
| 22 | 30·09 | 29·60 | 29·58 | 29·52 | 29·50 | 29·42 | 28·90 | 54 | 41 | 50 | 44 | 54 | 43 | 44 | SW. | W. | W. | W. | ... | ·05 | ·38 | 1·105 | ·370 | ... |
| 23 | 30·09 | 30·01 | 29·90 | 29·80 | 30·03 | 29·83 | 29·42 | 52 | 38 | 55 | 44 | 54 | 46 | 42 | NW. | S.W. | NW. | W. | ... | ... | ·06 | ... | ·330 | ·09 |
| ☽ 24 | 30·52 | 30·01 | 30·06 | 29·74 | 29·94 | 29·77 | 29·34 | 49 | 38 | 55 | 47 | 56 | 40 | 51 | SW. | W. | SW. | N. | ... | ... | ·10 | 0·500 | ·110 | ... |
| 25 | 30·62 | 30·52 | 30·24 | 30·20 | 30·36 | 30·30 | 29·85 | 48 | 42 | 51 | 44 | 51 | 46 | 43 | NW. | W. | W. | W. | ·46 | ·05 | ... | 0·025 | ... | ·17 |
| 26 | 30·62 | 30·58 | 30·30 | 30·30 | 30·42 | 30·40 | 30·00 | 51 | 41 | 52 | 48 | 53 | 44 | 47 | SW. | W. | SW. | calm | ... | ... | ... | ... | ... | ·09 |
| 27 | 30·81 | 30·58 | 30·34 | 30·30 | 30·46 | 30·38 | 30·00 | 50 | 31 | 52 | 48 | 53 | 42 | 44 | SW. | W. | SW. | calm | ... | ... | ... | ... | ... | ... |
| 28 | 30·81 | 30·71 | 30·40 | 30·38 | 30·58 | 30·57 | 30·33 | 40 | 31 | 50 | 48 | 47 | 35 | 33 | NW. | E. | NE. | calm | ... | ·05 | ... | ... | ... | ... |
| 29 | 30·71 | 30·58 | 30·36 | 30·34 | 30·47 | 30·40 | 30·15 | 38 | 30 | 50 | 46 | 43 | 31 | 30 | NW. | SE. | NW. | calm | ... | ... | ... | ... | ·030 | ... |
| 30 | 30·58 | 30·14 | 30·12 | 30·06 | 30·27 | 30·16 | 30·04 | 40 | 31 | 50 | 45 | 45 | 40 | 30·5 | SE. | SE. | SE. | calm | ... | ... | ... | ... | ... | ... |
| 31 | 30·14 | 29·75 | 29·66 | 29·64 | 29·87 | 29·66 | 29·56 | 41 | 34 | 50 | 47 | 51 | 46 | 37 | SE. | S. | S. | NW. | ·30 | ·05 | ·13 | 0·660 | ·340 | ... |
| Aver.: | 30·81 | 29·21 | 30·40 | 29·26 | 30·58 | 29·07 | 29·38 | 60 | 28 | 55 | 40 | 57 | 31 | 43·3 | | | | | 1·24 | 0·60 | 3·60 | 7·580 | 5·625 | 1·98 |

THE

# PHILOSOPHICAL MAGAZINE

AND

# ANNALS OF PHILOSOPHY.

[NEW SERIES.]

*MARCH* 1828.

## XXVI. *On the Regular or Platonic Solids.* *By* DAVIES GILBERT, *F.R.S. &c.*

*To Mr. Richard Taylor.*

Sir,

THE following trifle, extracted from my memorandum-book of many years standing, may perhaps (as a mere curiosity) be favoured with a place in your Journal. It exhibits an easy method of deriving these bodies from a modern discovery in spherics; and it illustrates the extreme generalization attached to algebraic expressions.

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Montucla and other writers on mathematics, attribute to Albert Girard, about the year 1629, the discovery of this very curious property of the sphere:—That if the whole surface of the sphere, or what may be considered as its subtense from the centre, be represented by eight right angles; then will the subtense from the centre of any figure, on the surface of the sphere bounded by arches of great circles, equal the excess of the spherical angles above the plane angles formed by the chords of the sides. A discovery applied in recent times with great advantage to all geodetical operations conducted on an extensive scale.

Now as three planes at the least must meet at each solid angle, or point of every solid; it is obvious that no regular equal and similar planes of any figures exceeding the pentagon in number of sides, can possibly meet in one point or solid angle. Thus the angle between two sides of an hexagon being $1\frac{1}{3}$ right angle, three of these would make up 4 right angles, and consequently the sides would expand themselves into one plane. The same would take place with four squares and with six equilateral triangles.

The solid angles or points of all solids bounded by regular equal and similar sides, must then consist either of three, four, or five equilateral triangles. Of three squares or tetragons, or of three pentagons. And consequently the number of these finite bodies cannot exceed five.

That five such actually exist, is thus proved.

When three equilateral triangles meet in a point, the three equal spherical angles must, to complete space, amount each to one-third of four right angles, or to 120°. The three angles therefore of each spherical triangle will equal four right angles, or be two right angles above the two right angles of a plane triangle. The solid angle therefore at the centre of each spherical triangle will subtend one quarter part of the surface of the sphere, and the solid must be a tetraëdron.

If four equilateral triangles meet at a point, the angle of each spherical triangle must, for the same reason, be in this case a right one. Consequently, the three angles of each spherical triangle will equal three right angles, or be one right angle in excess of the three angles of a plane triangle. The solid angle therefore, at the centre of each spherical triangle, will subtend one-eighth part of the surface of the sphere, and the solid must be an octaëdron.

If five equilateral triangles meet in a point, the angle of each spherical triangle must be one-fifth of four right angles, or 72°; that is, four-fifths of one right angle: the three angles of each spherical triangle will amount, therefore, to two right angles and two-fifths of a right angle, exceeding the two right angles of a plane triangle by two-fifths of a right angle, or 36°, being the twentieth part of eight right angles; the solid angle therefore at the centre of each spherical triangle is one-twentieth part of the spherical surface, and the solid must be the icosaëdron.

If three squares or tetragons meet in a point, each spherical angle must be $1\frac{1}{3}$ right angle, and the four spherical angles of the tetragon will amount to $5\frac{1}{3}$ right angles, exceeding $(2n-4)$ right angles, or the four right angles of a plane tetragon by $1\frac{1}{3}$ right angle, or by one-sixth of eight right angles: the solid must therefore be the hexaëdron or cube.

If three pentagons meet in a point, each spherical angle must be $1\frac{1}{3}$ right angle, as in the last instance; and the five spherical angles of the pentagon will be $6\frac{2}{3}$ right angles, exceeding the $(2n-4)$ right angles of the plane pentagon by $\frac{2}{3}$ds of a right angle, or by $\frac{1}{12}$th of eight right angles. The solid must therefore be the dodecaëdron.

The

The same results may be obtained analytically in a manner exhibiting the extensive generality of algebraic language.

Let $x$ = the number of regular similar and equal planes bounding the solid body.

$y$ = the number of sides in each plane.

$z$ = the number of planes, and consequently of spherical angles, meeting at each point.

Then $\frac{8 \text{ right angles}}{x}$ = the solid angle subtended by each side of the solid from the centre of the sphere.

But since all the spherical angles meeting at any point of the solid must in every instance equal four right angles to fill space; each spherical angle will be equal to $\frac{4 \text{ right angles}}{z}$ and all the spherical angles of a side of the solid $= \frac{y}{z} \times 4$ right angles. But the plane angles of each side of the solid = $(2y-4)$ right angles. Consequently, the excess of the spherical angles above the plane angles $= (4\frac{y}{z} - 2y + 4)$ right angles. And this equals the solid angle subtended by each side of the solid from the centre of the sphere. Consequently, $(4\frac{y}{z} - 2y + 4)$ right angle $= \frac{8 \text{ right angles}}{x}$. $8z = 4xy - 2xyz + 4xz$, and $x = \frac{4z}{2y+(2-y) \times z}$.

Now it is obvious, from the nature of the quantities represented by $x$, $y$, and $z$, that they must in all cases be integral and positive. But since $x = \frac{4z}{2y+(2-y) . z}$. The denominator $2y+(2-y)$ must be also affirmative. To ascertain therefore the limit which $y$ cannot exceed without making the expression negative, put $2y+(2-y) . z = 0$. Then $y = 2 + \frac{4}{z}$.

If $z$ were infinite $y = 2$ and it attains its next integral value;

when $z = 6$ ......... $y = 3$;
when $z = 5$ ......... $y = 3$;
when $z = 4$ ......... $y$ may be 4;
when $z = 3$ ......... $y$ may be 6;
when $z = 2$ ......... $y$ at its maximum infinite: therefore any finite number may be taken for it.

If $z$ less than 2, the highest value of $y$ is negative.

 If

If therefore $z =$ any number from infinity to 2.

$y$ may be 2. Indicating that the sphere is divided into any number of parts by great circles (meridians) passing through the same two poles.

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$z = 6$ $y$ at its maximum 3, $x$ infinite. Hence the sphere may be covered by an infinite number of equilateral triangles meeting with 6 angles at each point.

$z = 5$, $y$ at its maximum $= 3$ $x = 20$. Hence the sphere may be covered by 20 equilateral triangles meeting 5 in a point.

$z = 4$, $y$ at its maximum $= 4$, $x$ infinite. Hence the sphere may be covered by an infinite of squares or tetragons meeting 4 in a point.

$y = 3$, $x = 8$. Hence the sphere may be covered by 8 equilateral triangles meeting 3 in a point.

$z = 3$, $y$ at its maximum $= 6$, $x$ infinite. Hence the sphere may be covered by an infinite number of hexagons meeting 3 in a point.

$y = 5$, $x = 12$. The spheres may therefore be covered by 12 pentagons meeting 3 in a point.

$y = 4$. $x = 6$. The sphere may therefore be covered by 6 squares or tetragons meeting 3 in a point.

$y = 3$, $x = 4$. The sphere may therefore be covered by 4 equilateral triangles meeting 3 in a point.

$z = 2$ $y$ at its maximum infinite $x$ any number whatever. Appearing to indicate that a sphere being divided by a mathematical plane into two equal parts, may have that plane bounded by any regular polygon, when 2 angles only will meet in a point.

$y = 2$ The plane appears to contract itself into a right line.

The

The quantities may be arranged in the following manner:

| | | |
|---|---|---|
| $x = 4$ | $y = 3$ | $z = 3$ |
| $x = 6$ | $y = 4$ | $z = 3$ |
| $x = 8$ | $y = 3$ | $z = 4$ |
| $x = 12$ | $y = 5$ | $z = 3$ |
| $x = 20$ | $y = 3$ | $z = 5$ |
| $x$ infinite | $y = 3$ | $z = 6$ |
| | $y = 4$ | $z = 4$ |
| | $y = 6$ | $z = 3$ |

$x$ any number whatever, $y = 2$, $z =$ the same number as $x$.

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## XXVII. *On the Ellipticity of the Earth as deduced from Experiments with the Pendulum.* By J. Ivory, *M.A. F.R.S.**

IN the *Conn. des Tems* 1830, lately published, there is a notice of experiments made with an invariable pendulum, by M. Duperrey, in the course of a voyage round the world. The places of observation are not numerous, and one only, namely, Toulon, is a new station. But, in the present state of this inquiry, no less instruction is to be reaped from the repetition of the experiments at the old stations, than from the extension of our knowledge by new observations. If we could compare the lengths of the pendulum determined by different observers on the same spot, the errors to which such experiments are liable would become known, and we should be able to estimate the degree of precision with which the figure of the earth can be investigated by this mode of experimenting. An attentive examination of all that has been accomplished in this research will clearly prove that the high expectations of accuracy which were at first entertained from it, have not been realized in practice. It is remarkable that the pendulum-experiments within 30° of the equator are very irregular; while in higher latitudes, if we except Drontheim, the results obtained are not chargeable with inconsistency in any great degree. Two causes only can be imagined in order to account for the irregularity near the equator: either the experiments must be erroneous, or gravity must be very unequally distributed in that quarter of the globe†.

* Communicated by the Author.

† Captain Sabine's experiments have lately appeared in this Journal, corrected from an error in estimating the divisions of the level of his repeating circle. The corrections are excessively trifling, so much so that it seemed hardly worth while to call the attention of the public to the circumstance, as the experiments must be considered as remaining *in statu quo*. That very great irregularity prevails in these experiments, and indeed in all those made near the equator, is a fact that all will allow; but the cause has not been ascertained in a satisfactory manner.

The

The pendulum which beats seconds at Paris, at the temperature of 15° Cent., being represented by unit, M. Duperrey has determined the relative lengths, at the same temperature and at the level of the sea, at the five following stations:

| | Latitude. | Relative Length. |
|---|---|---|
| Toulon . . . . . | 43° 7′ 20″ N. | 0·99950585 |
| Isle of Ascension | 7 55 48 S. | 0·99729881 |
| Isle of France. . | 20 9 23 | 0·99789022 |
| Port Jackson . . | 33 51 40 | 0·99871430 |
| Falkland Islands | 51 31 44 | 1·00025995 |

Adopting 39·12929 for the length of the pendulum at Paris in English inches, at the temperature 62° Fahr., the following table shows the lengths of the pendulums at the stations of M. Duperrey, in English measure, at the standard temperature, and likewise the errors according to my formula published in this Journal for October 1826.

| | Observed Pendulum. | Computed Pendulum. | Excess of Calculation. |
|---|---|---|---|
| Toulon. . . . . . | 39·10996 | 39·11071 | +·00075 |
| Port Jackson . . | 39·07899 | 39·07637 | —·00261 |
| Isle of France . . | 39·04674 | 39·03643 | —·01031 |
| Ascension . . . . | 39·02360 | 39·01566 | —·00796 |
| Falkland Islands | 39·13945 | 39·13940 | —·00005 |

The pendulums in this table at Ascension and the Isle of France coincide very nearly with the lengths previously determined by Captain Sabine and M. de Freycinet. From some cause or other, they are both anomalous, and they are treated as such by M. de Freycinet and M. Duperrey, being left out in computing the ellipticity which they adopt. But the other three pendulums fall within the limits laid down in this Journal for October 1826, and increase the number of instances that come under my formula to 31, out of 40 the total number of experiments that have been made. My formula was originally constructed from 26 experiments; and all the pendulums since determined by M. de Freycinet, Mr. Foster, and M. Duperrey, agree with it within the prescribed limits, except in the three instances of the islands of Mowi, Guam, and the Isle of France, which are greatly irregular and irreconcileable with the other experiments. It is therefore very probable that the mean ellipticity of the earth will ultimately be found to approach very near $\frac{1}{300}$, as deduced from my formula.

At

At Port Jackson the pendulum determined by M. Duperrey is less than the length found by M. de Freycinet, and approaches nearer to the pendulum at Paramatta, to which it ought to be very nearly equal: and this proves the justness of the remarks I made relative to this point at pp. 351, 352, of this Journal for November 1826.

Comparing the results obtained by M. Duperrey and M. de Freycinet at the same stations and on the same spot, the pendulum of the former observer at the Falkland Islands is longer than that of the latter by ·00233; and, at Port Jackson and the Isle of France, the pendulums of the former are respectively less than those of the latter by ·00145 and ·00095. We may therefore conclude, that such experiments are liable to an error amounting from ·002 to ·003 in the length of the pendulum, or from two to three vibrations in a mean solar day. We are sure that the error may amount to the quantity mentioned; but it may be much greater. At Ascension the difference between the pendulums of Captain Sabine and M. Duperrey is very small.

In deducing the ellipticity of the earth, M. Duperrey combines his own experiments with M. de Freycinet's, exclusively of those made by other observers. The results he obtains vary between the limits $\frac{1}{266}$ and $\frac{1}{290}$. Upon the whole he concludes that the two hemispheres of the earth are similar, and he fixes definitively upon $\frac{1}{288}$, or $\frac{1}{290}$, as the ellipticity common to both. But on examining the calculations of M. Duperrey, it will be found that the ellipticity he adopts, depends entirely on the equatorial pendulum determined by M. de Freycinet at Rawak. If we suppose that the pendulum at Rawak errs in excess, which is very probable, and diminish its length by correcting the possible error, the ellipticities will come out of less quantity in all the combinations in which they were before equal to $\frac{1}{288}$ or $\frac{1}{290}$. Wherefore, although M. Duperrey finds five combinations of his experiments in which the ellipticity is $\frac{1}{288}$ or $\frac{1}{290}$; yet, whether its real value be equal to either of those numbers, or to a less fraction, will depend entirely upon the error that may exist in the pendulum at Rawak. Captain Sabine has deduced the same ellipticity, viz. $\frac{1}{288}$, by many combinations of his own experiments and those made in England and France; and I have already remarked that this uniformity of result is occasioned by the pendulums at the Islands of Ascension and St. Thomas, which enter into all the computations. If these two stations be left out in any, or in all, the combinations, the ellipticity will no longer be the same as before, but a less fraction. The arriving at the same result by a multiplicity of arithmetical operations

rations is in reality a play of calculation caused by one common datum, or several common data; and therefore it does not furnish independent proofs in favour of the number in question. There must, in all probability, be some error in the pendulum at Rawak; and we know that the two pendulums of Captain Sabine are anomalous, involving an irregularity much greater than usually occurs in such experiments; from which considerations we must conclude that the evidence for the ellipticity $\frac{1}{288}$ rests but on slender foundations.

It is reasonable to think that every new set of experiments with the pendulum should be joined to the stock we already possess, in order to add to the number of unexceptionable experiments, and to correct such as are of doubtful authority. By proceeding in this manner our *data* for obtaining an exact knowledge of the figure of the earth and of the distribution of gravity on its surface, would continually increase and improve in accuracy; and, by applying proper methods of calculation, we might hope to bring this great question to a satisfactory solution. But if every new set is to be taken by itself, we may, by making arbitrary combinations of the experiments, be led into great error, and to entertain speculations that have little foundation in nature.

The number of experiments made with the pendulum by good observers at present amounts to 40, contained in the subsequent table; but of these, six, placed last in the table, are decidedly anomalous, and must be separated from the rest. In setting aside these six experiments I do not now proceed on my own opinion; I follow the example of M. de Freycinet and M. Duperrey. There remains 34 experiments which may be compared together; and we have now to inquire, What mode of calculation must be adopted in order to obtain a result entitled to the greatest confidence the case will admit of. On reflection it will appear that we cannot expect to attain what we wish for by the method of the least squares as usually applied, nor by the modification of that method I used in this Journal for October 1826. These more direct methods are useful in bringing out a first approximation; but it seems necessary to correct the approximate elements thus obtained by the methods used with success in so many problems of astronomy. It is on these principles that the following investigation is conducted.

If we put $39 + \delta$ for the pendulum in English inches at any station; $\lambda$ for the latitude; and $39 + \Delta$ for the equatorial pendulum; we shall have this equation,

$$\Delta + f \sin^2 \lambda = \delta + e, \qquad (1)$$

*e* being

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[NEW SERIES.]

*MARCH* 1828.

## XXVI. *On the Regular or Platonic Solids.* *By* DAVIES GILBERT, *F.R.S. &c.*

*To Mr. Richard Taylor.*

Sir,

THE following trifle, extracted from my memorandum-book of many years standing, may perhaps (as a mere curiosity) be favoured with a place in your Journal. It exhibits an easy method of deriving these bodies from a modern discovery in spherics; and it illustrates the extreme generalization attached to algebraic expressions.

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Montucla and other writers on mathematics, attribute to Albert Girard, about the year 1629, the discovery of this very curious property of the sphere:—That if the whole surface of the sphere, or what may be considered as its subtense from the centre, be represented by eight right angles; then will the subtense from the centre of any figure, on the surface of the sphere bounded by arches of great circles, equal the excess of the spherical angles above the plane angles formed by the chords of the sides. A discovery applied in recent times with great advantage to all geodetical operations conducted on an extensive scale.

Now as three planes at the least must meet at each solid angle, or point of every solid; it is obvious that no regular equal and similar planes of any figures exceeding the pentagon in number of sides, can possibly meet in one point or solid angle. Thus the angle between two sides of an hexagon being $1\frac{1}{3}$ right angle, three of these would make up 4 right angles, and consequently the sides would expand themselves into one plane. The same would take place with four squares and with six equilateral triangles.

The solid angles or points of all solids bounded by regular equal and similar sides, must then consist either of three, four, or five equilateral triangles. Of three squares or tetragons, or of three pentagons. And consequently the number of these finite bodies cannot exceed five.

That five such actually exist, is thus proved.

When three equilateral triangles meet in a point, the three equal spherical angles must, to complete space, amount each to one-third of four right angles, or to 120°. The three angles therefore of each spherical triangle will equal four right angles, or be two right angles above the two right angles of a plane triangle. The solid angle therefore at the centre of each spherical triangle will subtend one quarter part of the surface of the sphere, and the solid must be a tetraëdron.

If four equilateral triangles meet at a point, the angle of each spherical triangle must, for the same reason, be in this case a right one. Consequently, the three angles of each spherical triangle will equal three right angles, or be one right angle in excess of the three angles of a plane triangle. The solid angle therefore, at the centre of each spherical triangle, will subtend one-eighth part of the surface of the sphere, and the solid must be an octaëdron.

If five equilateral triangles meet in a point, the angle of each spherical triangle must be one-fifth of four right angles, or 72°; that is, four-fifths of one right angle: the three angles of each spherical triangle will amount, therefore, to two right angles and two-fifths of a right angle, exceeding the two right angles of a plane triangle by two-fifths of a right angle, or 36°, being the twentieth part of eight right angles; the solid angle therefore at the centre of each spherical triangle is one-twentieth part of the spherical surface, and the solid must be the icosaëdron.

If three squares or tetragons meet in a point, each spherical angle must be $1\frac{1}{3}$ right angle, and the four spherical angles of the tetragon will amount to $5\frac{1}{3}$ right angles, exceeding $(2n-4)$ right angles, or the four right angles of a plane tetragon by $1\frac{1}{3}$ right angle, or by one-sixth of eight right angles: the solid must therefore be the hexaëdron or cube.

If three pentagons meet in a point, each spherical angle must be $1\frac{1}{3}$ right angle, as in the last instance; and the five spherical angles of the pentagon will be $6\frac{2}{3}$ right angles, exceeding the $(2n-4)$ right angles of the plane pentagon by $\frac{2}{3}$ds of a right angle, or by $\frac{1}{12}$th of eight right angles. The solid must therefore be the dodecaëdron.

The

The same results may be obtained analytically in a manner exhibiting the extensive generality of algebraic language.

Let $x$ = the number of regular similar and equal planes bounding the solid body.
$y$ = the number of sides in each plane.
$z$ = the number of planes, and consequently of spherical angles, meeting at each point.

Then $\frac{8 \text{ right angles}}{x}$ = the solid angle subtended by each side of the solid from the centre of the sphere.

But since all the spherical angles meeting at any point of the solid must in every instance equal four right angles to fill space; each spherical angle will be equal to $\frac{4 \text{ right angles}}{z}$ and all the spherical angles of a side of the solid $= \frac{y}{z} \times 4$ right angles. But the plane angles of each side of the solid = $(2y - 4)$ right angles. Consequently, the excess of the spherical angles above the plane angles $= (4\frac{y}{z} - 2y + 4)$ right angles. And this equals the solid angle subtended by each side of the solid from the centre of the sphere. Consequently, $(4\frac{y}{z} - 2y + 4)$ right angle $= \frac{8 \text{ right angles}}{x}$. $8z = 4xy - 2xyz + 4xz$, and $x = \frac{4z}{2y + (2 - y) \times z}$.

Now it is obvious, from the nature of the quantities represented by $x$, $y$, and $z$, that they must in all cases be integral and positive. But since $x = \frac{4z}{2y + (2-y) . z}$. The denominator $2y + (2 - y)$ must be also affirmative. To ascertain therefore the limit which $y$ cannot exceed without making the expression negative, put $2y + (2 - y) . z = 0$. Then $y = 2 + \frac{4}{z}$.

If $z$ were infinite $y = 2$ and it attains its next integral value;

when $z = 6$ ......... $y = 3$;
when $z = 5$ ......... $y = 3$;
when $z = 4$ ......... $y$ may be 4;
when $z = 3$ ......... $y$ may be 6;
when $z = 2$ ......... $y$ at its maximum infinite: therefore any finite number may be taken for it.

If $z$ less than 2, the highest value of $y$ is negative.



If

If therefore $z =$ any number from infinity to 2.

$y$ may be 2. Indicating that the sphere is divided into any number of parts by great circles (meridians) passing through the same two poles.

---

$z = 6$ $y$ at its maximum 3, $x$ infinite. Hence the sphere may be covered by an infinite number of equilateral triangles meeting with 6 angles at each point.

$z = 5$, $y$ at its maximum $= 3$ $x = 20$. Hence the sphere may be covered by 20 equilateral triangles meeting 5 in a point.

$z = 4$, $y$ at its maximum $= 4$, $x$ infinite. Hence the sphere may be covered by an infinite of squares or tetragons meeting 4 in a point.

$y = 3$, $x = 8$. Hence the sphere may be covered by 8 equilateral triangles meeting 3 in a point.

$z = 3$, $y$ at its maximum $= 6$, $x$ infinite. Hence the sphere may be covered by an infinite number of hexagons meeting 3 in a point.

$y = 5$, $x = 12$. The spheres may therefore be covered by 12 pentagons meeting 3 in a point.

$y = 4$ . $x = 6$. The sphere may therefore be covered by 6 squares or tetragons meeting 3 in a point.

$y = 3$, $x = 4$. The sphere may therefore be covered by 4 equilateral triangles meeting 3 in a point.

$z = 2$ $y$ at its maximum infinite $x$ any number whatever. Appearing to indicate that a sphere being divided by a mathematical plane into two equal parts, may have that plane bounded by any regular polygon, when 2 angles only will meet in a point.

$y = 2$ The plane appears to contract itself into a right line.

The

The quantities may be arranged in the following manner:

| | | |
|---|---|---|
| $x = 4$ | $y = 3$ | $z = 3$ |
| $x = 6$ | $y = 4$ | $z = 3$ |
| $x = 8$ | $y = 3$ | $z = 4$ |
| $x = 12$ | $y = 5$ | $z = 3$ |
| $x = 20$ | $y = 3$ | $z = 5$ |
| $x$ infinite | $y = 3$ | $z = 6$ |
| | $y = 4$ | $z = 4$ |
| | $y = 6$ | $z = 3$ |

$x$ any number whatever, $y = 2$, $z =$ the same number as $x$.

## XXVII. *On the Ellipticity of the Earth as deduced from Experiments with the Pendulum.* By J. Ivory, *M.A. F.R.S.**

IN the *Conn. des Tems* 1830, lately published, there is a notice of experiments made with an invariable pendulum, by M. Duperrey, in the course of a voyage round the world. The places of observation are not numerous, and one only, namely, Toulon, is a new station. But, in the present state of this inquiry, no less instruction is to be reaped from the repetition of the experiments at the old stations, than from the extension of our knowledge by new observations. If we could compare the lengths of the pendulum determined by different observers on the same spot, the errors to which such experiments are liable would become known, and we should be able to estimate the degree of precision with which the figure of the earth can be investigated by this mode of experimenting. An attentive examination of all that has been accomplished in this research will clearly prove that the high expectations of accuracy which were at first entertained from it, have not been realized in practice. It is remarkable that the pendulum-experiments within 30° of the equator are very irregular; while in higher latitudes, if we except Drontheim, the results obtained are not chargeable with inconsistency in any great degree. Two causes only can be imagined in order to account for the irregularity near the equator: either the experiments must be erroneous, or gravity must be very unequally distributed in that quarter of the globe†.

* Communicated by the Author.

† Captain Sabine's experiments have lately appeared in this Journal, corrected from an error in estimating the divisions of the level of his repeating circle. The corrections are excessively trifling, so much so that it seemed hardly worth while to call the attention of the public to the circumstance, as the experiments must be considered as remaining *in statu quo*. That very great irregularity prevails in these experiments, and indeed in all those made near the equator, is a fact that all will allow; but the cause has not been ascertained in a satisfactory manner.

The

The pendulum which beats seconds at Paris, at the temperature of 15° Cent., being represented by unit, M. Duperrey has determined the relative lengths, at the same temperature and at the level of the sea, at the five following stations:

| | Latitude. | Relative Length. |
|---|---|---|
| Toulon . . . . . | 43° 7′ 20″ N. | 0·99950585 |
| Isle of Ascension | 7 55 48 S. | 0·99729881 |
| Isle of France. . | 20 9 23 | 0·99789022 |
| Port Jackson . . | 33 51 40 | 0·99871430 |
| Falkland Islands | 51 31 44 | 1·00025995 |

Adopting 39·12929 for the length of the pendulum at Paris in English inches, at the temperature 62° Fahr., the following table shows the lengths of the pendulums at the stations of M. Duperrey, in English measure, at the standard temperature, and likewise the errors according to my formula published in this Journal for October 1826.

| | Observed Pendulum. | Computed Pendulum. | Excess of Calculation. |
|---|---|---|---|
| Toulon. . . . . . . | 39·10996 | 39·11071 | +·00075 |
| Port Jackson . . | 39·07899 | 39·07637 | —·00261 |
| Isle of France . . | 39·04674 | 39·03643 | —·01031 |
| Ascension . . . . | 39·02360 | 39·01566 | —·00796 |
| Falkland Islands | 39·13945 | 39·13940 | —·00005 |

The pendulums in this table at Ascension and the Isle of France coincide very nearly with the lengths previously determined by Captain Sabine and M. de Freycinet. From some cause or other, they are both anomalous, and they are treated as such by M. de Freycinet and M. Duperrey, being left out in computing the ellipticity which they adopt. But the other three pendulums fall within the limits laid down in this Journal for October 1826, and increase the number of instances that come under my formula to 31, out of 40 the total number of experiments that have been made. My formula was originally constructed from 26 experiments; and all the pendulums since determined by M. de Freycinet, Mr. Foster, and M. Duperrey, agree with it within the prescribed limits, except in the three instances of the islands of Mowi, Guam, and the Isle of France, which are greatly irregular and irreconcileable with the other experiments. It is therefore very probable that the mean ellipticity of the earth will ultimately be found to approach very near $\frac{1}{300}$, as deduced from my formula.

At

At Port Jackson the pendulum determined by M. Duperrey is less than the length found by M. de Freycinet, and approaches nearer to the pendulum at Paramatta, to which it ought to be very nearly equal: and this proves the justness of the remarks I made relative to this point at pp. 351, 352, of this Journal for November 1826.

Comparing the results obtained by M. Duperrey and M. de Freycinet at the same stations and on the same spot, the pendulum of the former observer at the Falkland Islands is longer than that of the latter by ·00233; and, at Port Jackson and the Isle of France, the pendulums of the former are respectively less than those of the latter by ·00145 and ·00095. We may therefore conclude, that such experiments are liable to an error amounting from ·002 to ·003 in the length of the pendulum, or from two to three vibrations in a mean solar day. We are sure that the error may amount to the quantity mentioned; but it may be much greater. At Ascension the difference between the pendulums of Captain Sabine and M. Duperrey is very small.

In deducing the ellipticity of the earth, M. Duperrey combines his own experiments with M. de Freycinet's, exclusively of those made by other observers. The results he obtains vary between the limits $\frac{1}{266}$ and $\frac{1}{290}$. Upon the whole he concludes that the two hemispheres of the earth are similar, and he fixes definitively upon $\frac{1}{288}$, or $\frac{1}{290}$, as the ellipticity common to both. But on examining the calculations of M. Duperrey, it will be found that the ellipticity he adopts, depends entirely on the equatorial pendulum determined by M. de Freycinet at Rawak. If we suppose that the pendulum at Rawak errs in excess, which is very probable, and diminish its length by correcting the possible error, the ellipticities will come out of less quantity in all the combinations in which they were before equal to $\frac{1}{288}$ or $\frac{1}{290}$. Wherefore, although M. Duperrey finds five combinations of his experiments in which the ellipticity is $\frac{1}{288}$ or $\frac{1}{290}$; yet, whether its real value be equal to either of those numbers, or to a less fraction, will depend entirely upon the error that may exist in the pendulum at Rawak. Captain Sabine has deduced the same ellipticity, viz. $\frac{1}{288}$, by many combinations of his own experiments and those made in England and France; and I have already remarked that this uniformity of result is occasioned by the pendulums at the Islands of Ascension and St. Thomas, which enter into all the computations. If these two stations be left out in any, or in all, the combinations, the ellipticity will no longer be the same as before, but a less fraction. The arriving at the same result by a multiplicity of arithmetical operations

rations is in reality a play of calculation caused by one common datum, or several common data; and therefore it does not furnish independent proofs in favour of the number in question. There must, in all probability, be some error in the pendulum at Rawak; and we know that the two pendulums of Captain Sabine are anomalous, involving an irregularity much greater than usually occurs in such experiments; from which considerations we must conclude that the evidence for the ellipticity $\frac{1}{288}$ rests but on slender foundations.

It is reasonable to think that every new set of experiments with the pendulum should be joined to the stock we already possess, in order to add to the number of unexceptionable experiments, and to correct such as are of doubtful authority. By proceeding in this manner our *data* for obtaining an exact knowledge of the figure of the earth and of the distribution of gravity on its surface, would continually increase and improve in accuracy; and, by applying proper methods of calculation, we might hope to bring this great question to a satisfactory solution. But if every new set is to be taken by itself, we may, by making arbitrary combinations of the experiments, be led into great error, and to entertain speculations that have little foundation in nature.

The number of experiments made with the pendulum by good observers at present amounts to 40, contained in the subsequent table; but of these, six, placed last in the table, are decidedly anomalous, and must be separated from the rest. In setting aside these six experiments I do not now proceed on my own opinion; I follow the example of M. de Freycinet and M. Duperrey. There remains 34 experiments which may be compared together; and we have now to inquire, What mode of calculation must be adopted in order to obtain a result entitled to the greatest confidence the case will admit of. On reflection it will appear that we cannot expect to attain what we wish for by the method of the least squares as usually applied, nor by the modification of that method I used in this Journal for October 1826. These more direct methods are useful in bringing out a first approximation; but it seems necessary to correct the approximate elements thus obtained by the methods used with success in so many problems of astronomy. It is on these principles that the following investigation is conducted.

If we put $39 + \delta$ for the pendulum in English inches at any station; $\lambda$ for the latitude; and $39 + \Delta$ for the equatorial pendulum; we shall have this equation,

$$\Delta + f \sin^2 \lambda = \delta + e, \qquad (1)$$

*e* being

$e$ being the error of observation, and $f$ a quantity which is the same at all points of the earth's surface. The equation (1) being general, if we add all the like equations at the 34 stations we propose to include in our investigation, we shall get

$$34\Delta + f\Sigma(\sin^2\lambda) = \Sigma(\delta) + \Sigma(e).$$

Now I have found,

$$\Sigma(\sin^2\lambda) = 15{\cdot}9316,$$
$$\Sigma(\delta) = 3{\cdot}72872;$$

and hence by dividing all the terms by 34, we get,

$$\Delta + {\cdot}4686 f = {\cdot}10966, \qquad (2)$$

neglecting the term containing $e$, which must be very small, both because the errors must in some degree compensate one another, and on account of the great divisor 34. The equation (2) must be very exact, provided we take care not to enhance its error by introducing small divisors.

Again, when the ellipticity is $\frac{1}{288}$, we have $f = {\cdot}202$ nearly, as appears from the calculations of Captain Sabine; and, when the ellipticity is $\frac{1}{300}$, $f$ is nearly ·208, according to my formula in this Journal for October 1826. The mean is ·205, which cannot err from the true value of $f$ more than $\pm$ ·003. Now it is evident that this small error will not affect the product $f\sin^2\lambda$ near the equator, and so long as $\sin^2\lambda$ is less than 0·2. I have therefore computed the values of $\Delta$ by means of the formula,

$$\Delta = \delta - f\sin^2\lambda,$$

on the supposition that $f = {\cdot}205$, for all the stations in the table where $\sin^2\lambda$ is less than 0·2, as follows:

| | $\Delta$ | |
|---|---|---|
| Maranham...... | ·01173 | |
| Bahia........... | ·01398 | |
| Rio Janeiro..... | ·01264 | |
| Trinidad....... | ·01188 | |
| Madras......... | ·01289 | |
| San Blas....... | ·01066 | |
| | 6)·07378( | ·01230, mean of 6. |
| Rawak......... | ·01479 | |
| Sierra Leone... | ·01550 | |
| Jamaica ......... | ·01560 | |
| | 9)·11967( | ·01330, mean of 9. |

The inequality of the several values of $\Delta$ is very remarkable; in particular the three stations placed last, greatly exceed the mean of the first six. But whatever be the cause of the irregularity, it cannot arise from the approximate value assigned to $f$. The mean of the nine stations must be an approximation sufficient for our present purpose. Let ·01330 be substituted for $\Delta$ in the equation (2), then $f = \cdot 2057$, which will serve for a first value of $f$. Now put

$$a = \cdot 01330, \qquad \Delta = a + s,$$
$$b = \cdot 2057 \qquad f = b + \tau,$$

$s$ and $\tau$ being the corrections of the approximate quantities: then since $\Delta$ and $f$ must satisfy the equation (2), and $a$ and $b$ likewise satisfy the same equation, we get,

$$s + \cdot 4686\, \tau = 0.$$

The formula (2) is one of the equations of the method of the least squares applied to all the 34 experiments; and in order to find the other equation of the same method, let all the terms of equation (1) be multiplied by the coefficient of $f$: then,

$$\Delta \sin^2 \lambda + f \sin^4 \lambda = \delta \sin^2 \lambda + e \sin^2 \lambda;$$

by substituting the values of $\Delta$ and $f$,

$$s \sin^2 \lambda + \tau \sin^4 \lambda = \sin^2 \lambda\, (\delta - a - b \sin^2 \lambda) + e \sin^2 \lambda:$$

and the sum of the like equations for all the 34 stations will be the equation sought, viz.

$$s\, \Sigma\, (\sin^2 \lambda) + \tau\, \Sigma\, (\sin^4 \lambda) = \Sigma\, .\, \big(\sin^2 \lambda\, (\delta - a - b \sin^2 \lambda)\big),$$

the term containing $e$ being neglected. Now,

$$\Sigma\, (\sin^2 \lambda) = 15\cdot 9316,$$
$$\Sigma\, (\sin^4 \lambda) = 10\cdot 4381;$$

wherefore,

$$15\cdot 9316 s + 10\cdot 4381\, \tau = \Sigma\, .\, \big(\sin^2 \lambda\, (\delta - a - b \sin^2 \lambda)\big):$$

and, by a former equation,

$$15\cdot 9316\, s + 7\cdot 4654\, \tau = 0;$$

consequently,

$$2\cdot 973\, \tau = \Sigma\, .\, \big(\sin^2 \lambda\, (\delta - a - b \sin^2 \lambda)\big).$$

The sum on the right-hand side consists of thirty-four terms, which must be separately calculated by substituting for $\delta$ and $\sin^2 \lambda$ their values at the several stations. The computation being made, and all the results combined according to their respective signs, I have found

$$2\cdot 973\, \tau = - \cdot 00061.$$

Hence, $\tau = - \cdot 00020$; $s = - \cdot 4686\, \tau = + \cdot 00009$;

$$f = \cdot 2055, \qquad \Delta = \cdot 01339.$$

If

If therefore $l = 39 + \delta$, be the length of the pendulum at the latitude $\lambda$, we have this general formula, viz.

$$l = 39{\cdot}01339 + {\cdot}2055 \sin^2 \lambda,$$

$$\text{ellipticity} = {\cdot}00865 - \frac{{\cdot}2055}{39{\cdot}01339} = {\cdot}00338 = \frac{1}{295{\cdot}5}.$$

| Stations. | Latitude. | Observed Pendulum. | Computed Pendulum. | Excess of Calculation. | Observers. |
|---|---|---|---|---|---|
| | | inches. | inches. | | |
| Falkland Islands | 51° 31′ 44″ S. | 39·13945 | 39·13935 | —·00010 | Duperrey. |
| Ca. Good Hope | 33 55 15 | 39·07817 | 39·07739 | —·00078 | Freycinet. |
| Port Jackson... | 33 51 40 | 39·07899 | 39·07719 | —·00180 | Duperrey. |
| Paramatta ...... | 33 48 43 | 39·07622 | 39·07702 | +·00080 | Sir T. Brisbane. |
| Rio Janeiro .... | 22 55 22 | 39·04374 | 39·04457 | +·00083 | Hall and Foster. |
| Bahia ............ | 12 59 21 | 39·02433 | 39·02377 | —·00056 | Sabine. |
| Maranham ...... | 2 31 43 | 39·01213 | 39·01379 | +·00166 | Sabine. |
| Rawak .......... | 0 1 34 | 39·01479 | 39·01339 | —·00140 | Freycinet. |
| Sierra Leone ... | 8 29 58 N. | 39·01997 | 39·01787 | —·00210 | Sabine. |
| Trinidad......... | 10 38 56 | 39·01888 | 39·02041 | +·00153 | Sabine. |
| Madras .......... | 13 4 9 | 39·02338 | 39·02390 | +·00052 | Goldingham. |
| Jamaica ......... | 17 56 7 | 39·03503 | 39·03288 | —·00215 | Sabine. |
| San Blas ....... | 21 32 24 | 39·03829 | 39·04109 | +·00280 | Hall and Foster. |
| Formentera .... | 38 39 56 | 39·09424 | 39·09361 | —·00063 | Biot. |
| New York ...... | 40 42 43 | 39·10120 | 39·10081 | —·00039 | Sabine. |
| Toulon .......... | 43 7 20 | 39·10996 | 39·10941 | —·00055 | Duperrey. |
| Figeac .......... | 44 36 45 | 39·11322 | 39·11476 | +·00154 | Biot. |
| Bourdeaux...... | 44 50 26 | 39·11303 | 39·11557 | +·00254 | Biot. |
| Clermont ...... | 45 46 48 | 39·11809 | 39·11893 | +·00084 | Biot. |
| Paris ... ......... | 48 50 14 | 39·12929 | 39·12929 | +·00056 | Biot. |
| Dunnose ........ | 50 37 24 | 39·13614 | 39·13618 | +·00004 | Kater. |
| Dunkirk.......... | 51 2 10 | 39·13773 | 39·13762 | —·00011 | Biot. |
| London .......... | 51 31 8 | 39·13929 | 39·13932 | +·00003 | Kater. |
| Arbury Hill .... | 52 12 55 | 39·14250 | 39·14174 | —·00076 | Kater. |
| Clifton .......... | 53 27 43 | 39·14600 | 39·14605 | +·00005 | Kater. |
| Leith ............ | 55 48 41 | 39·15554 | 39·15454 | —·00100 | Kater. |
| Portsoy ......... | 57 40 59 | 39·16159 | 39·16016 | —·00143 | Kater. |
| Stockholm ...... | 59 20 34 | 39·16541 | 39·16546 | +·00005 | Suanberg. |
| Unst ............ | 60 45 28 | 39·17146 | 39·16985 | —·00161 | Kater. |
| Drontheim...... | 63 25 54 | 39·17456 | 39·17778 | +·00322 | Sabine. |
| Hammerfest ... | 70 40 5 | 39·19475 | 39·19636 | +·00161 | Sabine. |
| Port Bowen ... | 73 13 39 | 39·20347 | 39·20177 | —·00170 | Foster. |
| Greenland ...... | 74 39 19 | 39·20328 | 39·20428 | +·00100 | Sabine. |
| Spitzbergen ... | 79 49 58 | 39·21464 | 39·21248 | —·00216 | Sabine. |
| Ascension ...... | 7 55 48 S. | 39·02385 | 39·01730 | —·00655 | Sabine & Duperrey. |
| Isle of France . | 20 9 40 | 39·04721 | 39·03780 | —·00941 | Freycinet & Dup^y. |
| St. Thomas...... | 0 24 41 N. | 39·02074 | 39·01339 | —·00735 | Sabine. |
| Galapagos ...... | 0 32 19 | 39·01717 | 39·01341 | —·00376 | Hall. |
| Guam ........... | 13 27 51 | 39·03023 | 39·02453 | —·00570 | Freycinet. |
| Mowi ............ | 20 52 7 | 39·04737 | 39·03946 | —·00791 | Freycinet. |



This

This table shows the errors of the formula. The greatest error is at Drontheim, and it is nearly as much with the ellipticity $\frac{1}{288}$. The next greatest error is at San Blas; but this would be considerably diminished by adopting the length found by Captain Foster, viz. 39·03881, instead of the mean of the lengths found by that gentleman and Captain Hall, which stands in the table. The excess at Galapagos is little greater than the defect at Drontheim; and as we know that the former pendulum was determined in unfavourable circumstances, the experiment seems to be sufficiently well represented by the formula. We may therefore affirm that, if we adopt Captain Foster's pendulum at San Blas, the formula represents 35 experiments out of 40, within the limits of the probable errors: for the difference between the observed and the calculated lengths is less than what would arise from an error of 2 vibrations in a mean solar day, except at Sierra Leone, Jamaica, San Blas, Bourdeaux, and Spitzbergen, where the error is $2\frac{1}{2}$ vibrations; and at Drontheim and Galapagos, where it amounts to $3\frac{1}{2}$ and 4 vibrations. But at the five anomalous stations, the errors are between 6 and 10 vibrations in a day.

Without laying any stress on the mode of investigation we have here followed, it cannot, I think, be denied that the ellipticity $\frac{1}{293\cdot3}$ represents the observations much better than $\frac{1}{288}$; it ought therefore to be adopted until another shall be found, if it be possible, that will represent the same observations still better, or until future experimental researches shall have corrected and enlarged our present knowledge on this subject. What other reason can be alleged for preferring one ellipticity to another?

But if the experiments can be represented within the limits of the probable errors, what becomes of the splendid speculation about local attraction, which connects the apparent inequality of gravity with the variation of the soil. It is evident that we can have no measure of the excess or defect of gravity caused by the attraction of the adjacent matter, if the errors of observation be sufficient to account for the irregularity in the length of the pendulum. This theory too is quite uncertain, so long as there remains any doubt about the exact quantity of the ellipticity. At Maranham and Trinidad, Captain Sabine makes the observed pendulums gain upon the calculated ones upwards of 4 vibrations in a mean solar day; in my table the acceleration is only $1\frac{1}{2}$, which may be attributed to the inaccuracy of experiment. According to the same gentleman,

tleman, the observed pendulum at Spitzbergen falls short of the calculated one $3\frac{1}{2}$ vibrations in a day;—in my table the defect is only $2\frac{1}{2}$. The density of the matter near the surface of the earth, varying at the different stations of the pendulum, must in some degree influence the time of vibration: but the speculation is premature; and there is good reason to think, at least in far the greater number of cases, that the effect in question will never be separated from the unavoidable errors of observation.

What has just been said will not, however, apply to the five anomalous experiments, all of which show an excess of gravity far surpassing the ordinary measure of experimental error. Besides, at two of the stations the pendulums have been verified by two independent experiments varying little in the results. It is remarkable that in all the five cases, and at Galapagos, there is an excess of gravity; and, according to my table, there is a like excess, though much less in quantity, at Rawak, Sierra Leone, and Jamaica. On the other hand, there appears to be a defect of gravity at Maranham, Trinidad, and San Blas. Thus there are nine instances of excess, and only three of defect; and we may suspect that there is some cause of error, different from local attraction, tending to make these tropical pendulums longer than they should be. At the Isle of France, the excess of gravity is no less than 10 vibrations a day; yet this is a small island, surrounded on all sides by an extensive sea; and we might infer, *à priori*, that a pendulum placed upon it very little above the level of the sea, instead of being accelerated, would be retarded by the great defect of density in the waters of the ocean. But the purpose of this paper is to deduce from the experiments the consequences that necessarily flow from them by a strict investigation; and I shall refrain from entering into the region of conjecture and opinion.

Feb. 11, 1828. J. Ivory.

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## XXVIII. *The Climate of Penzance, Cornwall.—Meteorological Results of the Temperature, Wind and Weather deduced from diurnal Observations made at Penzance for 21 Years: (the Thermometrical Observations made at 8 a.m. and 2 p.m.) to which are added the Maxima, Minima, and Media of the Register Thermometer for 7 Years, with the Inches of Rain fallen during that Period.* By Edward Collins Giddy, *Curator of the Cabinet of the Royal Geological Society of Cornwall.*

January

## JANUARY.

| Years | Thermom. Max. | Thermom. Min. | Thermom. Mean | Winds N | NE | E | SE | S | SW | W | NW | P. Wind | Weather N° of days | Wet | Dry |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1807 | 52 | 37 | 47,0 | 1 | 6 | 2 | 5 | 2 | 0 | 4 | 11 | NW | 31 | 9 | 22 |
| 1808 | 50 | 26 | 41,5 | 1 | 4 | 0 | 1 | 2 | 6 | 11 | 6 | W | 31 | 18 | 13 |
| 1809 | 52 | 33 | 45,0 | 1 | 3 | 4 | 8 | 1 | 6 | 4 | 4 | SE | 31 | 19 | 12 |
| 1810 | 53 | 19 | 41,0 | 3 | 6 | 4 | 5 | 5 | 7 | 0 | 1 | SW | 31 | 10 | 21 |
| 1811 | 50 | 22 | 37,5 | 1 | 4 | 7 | 2 | 4 | 5 | 0 | 8 | NW | 31 | 14 | 17 |
| 1812 | 52 | 30 | 42,0 | 4 | 5 | 2 | 1 | 3 | 4 | 2 | 10 | NW | 31 | 16 | 15 |
| 1813 | 51 | 30 | 41,5 | 3 | 3 | 7 | 3 | 3 | 2 | 2 | 8 | NW | 31 | 12 | 19 |
| 1814 | 51 | 28 | 37,5 | 9 | 5 | 3 | 5 | 3 | 2 | 1 | 3 | N | 31 | 18 | 13 |
| 1815 | 50 | 26 | 40,0 | 4 | 10 | 0 | 3 | 3 | 3 | 0 | 8 | NE | 31 | 18 | 13 |
| 1816 | 52 | 32 | 43,5 | 2 | 1 | 2 | 5 | 2 | 4 | 3 | 12 | NW | 31 | 18 | 13 |
| 1817 | 54 | 32 | 47,0 | 7 | 0 | 1 | 2 | 4 | 2 | 12 | 3 | W | 31 | 19 | 12 |
| 1818 | 55 | 34 | 46,5 | 3 | 0 | 2 | 2 | 4 | 3 | 11 | 6 | W | 31 | 23 | 8 |
| 1819 | 55 | 38 | 47,0 | 0 | 2 | 1 | 3 | 7 | 6 | 8 | 4 | W | 31 | 23 | 8 |
| 1820 | 56 | 22 | 40,0 | 1 | 7 | 5 | 4 | 4 | 2 | 2 | 6 | NE | 31 | 14 | 17 |
| 1821 | 55 | 26 | 45,5 | 0 | 8 | 4 | 5 | 7 | 0 | 7 | 0 | NE | 31 | 16 | 15 |
| 1822 | 50 | 37 | 45,5 | 4 | 4 | 1 | 0 | 2 | 2 | 7 | 11 | NW | 31 | 16 | 15 |
| 1823 | 54 | 30 | 40,5 | 3 | 6 | 3 | 11 | 1 | 5 | 2 | 0 | SE | 31 | 17 | 14 |
| 1824 | 54 | 32 | 46,0 | 3 | 8 | 3 | 1 | 0 | 3 | 7 | 6 | NE | 31 | 14 | 17 |
| 1825 | 54 | 34 | 46,0 | 0 | 7 | 3 | 0 | 3 | 3 | 8 | 7 | W | 31 | 14 | 17 |
| 1826 | 55 | 27 | 43,0 | 4 | 2 | 8 | 10 | 0 | 2 | 1 | 4 | SE | 31 | 8 | 23 |
| 1827 | 54 | 25 | 44,5 | 2 | 4 | 4 | 3 | 0 | 3 | 8 | 7 | W | 31 | 19 | 12 |
| For 21 years. | Max. | Min. | Mean | N | NE | E | SE | S | SW | W | NW | P. Wind | N° of days | Wet | Dry |
| | 56 | 19 | 43,0 | 56 | 95 | 66 | 79 | 60 | 70 | 100 | 125 | NW | 651 | 335 | 316 |

## FEBRUARY.

| Years | Thermom. Max. | Thermom. Min. | Thermom. Mean | Winds N | NE | E | SE | S | SW | W | NW | P. Wind | Weather N° of days | Wet | Dry |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1807 | 56 | 34 | 46,5 | 2 | 4 | 0 | 1 | 1 | 8 | 2 | 10 | NW | 28 | 15 | 13 |
| 1808 | 50 | 28 | 41,0 | 4 | 5 | 3 | 1 | 1 | 3 | 8 | 4 | W | 29 | 12 | 17 |
| 1809 | 54 | 34 | 47,0 | 0 | 3 | 0 | 2 | 0 | 16 | 0 | 7 | SW | 28 | 18 | 10 |
| 1810 | 56 | 26 | 45,0 | 0 | 5 | 3 | 2 | 5 | 7 | 5 | 1 | SW | 28 | 15 | 13 |
| 1811 | 54 | 41 | 45,5 | 0 | 0 | 0 | 7 | 3 | 9 | 4 | 5 | SW | 28 | 22 | 6 |
| 1812 | 54 | 37 | 47,0 | 4 | 0 | 0 | 3 | 4 | 7 | 5 | 6 | SW | 29 | 22 | 7 |
| 1813 | 52 | 34 | 46,0 | 0 | 1 | 1 | 4 | 4 | 7 | 6 | 5 | SW | 28 | 21 | 7 |
| 1814 | 52 | 32 | 42,0 | 1 | 5 | 2 | 8 | 2 | 2 | 4 | 4 | SE | 28 | 12 | 16 |
| 1815 | 56 | 36 | 49,5 | 1 | 0 | 0 | 6 | 3 | 10 | 4 | 4 | SW | 28 | 18 | 10 |
| 1816 | 56 | 26 | 44,0 | 0 | 6 | 0 | 1 | 9 | 1 | 5 | 7 | S | 29 | 10 | 19 |
| 1817 | 56 | 40 | 49,0 | 0 | 3 | 0 | 1 | 0 | 4 | 11 | 9 | W | 28 | 8 | 20 |
| 1818 | 58 | 34 | 48,5 | 3 | 0 | 1 | 4 | 6 | 2 | 3 | 9 | NW | 28 | 15 | 13 |
| 1819 | 55 | 38 | 47,5 | 3 | 1 | 0 | 1 | 4 | 4 | 7 | 8 | NW | 28 | 18 | 10 |
| 1820 | 56 | 30 | 44,0 | 4 | 8 | 2 | 1 | 6 | 5 | 0 | 3 | NE | 29 | 11 | 18 |
| 1821 | 52 | 36 | 43,5 | 2 | 1 | 13 | 6 | 2 | 0 | 3 | 1 | E | 28 | 3 | 25 |
| 1822 | 53 | 38 | 47,0 | 2 | 1 | 0 | 2 | 4 | 12 | 4 | 3 | SW | 28 | 11 | 17 |
| 1823 | 52 | 32 | 44,0 | 2 | 3 | 0 | 1 | 0 | 9 | 7 | 6 | SW | 28 | 22 | 6 |
| 1824 | 58 | 34 | 47,0 | 1 | 7 | 4 | 3 | 2 | 2 | 9 | 1 | W | 29 | 21 | 8 |
| 1825 | 56 | 36 | 47,5 | 1 | 0 | 5 | 6 | 0 | 5 | 7 | 4 | W | 28 | 12 | 16 |
| 1826 | 56 | 40 | 49,0 | 0 | 1 | 0 | 1 | 2 | 13 | 10 | 1 | SW | 28 | 19 | 9 |
| 1827 | 54 | 30 | 38,0 | 1 | 8 | 13 | 2 | 0 | 1 | 2 | 1 | E | 28 | 7 | 21 |
| For 21 years. | Max. | Min. | Mean | N | NE | E | SE | S | SW | W | NW | P. Wind | N° of days | Wet | Dry |
| | 58 | 26 | 45,5 | 31 | 62 | 47 | 63 | 58 | 127 | 106 | 99 | SW | 593 | 312 | 281 |

## MARCH.

| Years | Thermom. Max. | Thermom. Min. | Thermom. Mean | Winds N | NE | E | SE | S | SW | W | NW | P. Wind | Weather Nº of days | Wet | Dry |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1807 | 54 | 30 | 42,0 | 2 | 15 | 4 | 3 | 2 | 0 | 1 | 4 | NE | 31 | 4 | 27 |
| 1808 | 54 | 30 | 42,0 | 0 | 6 | 10 | 12 | 2 | 0 | 0 | 1 | SE | 31 | 7 | 24 |
| 1809 | 60 | 32 | 46,5 | 0 | 4 | 12 | 1 | 2 | 7 | 0 | 5 | E | 31 | 7 | 24 |
| 1810 | 58 | 30 | 47,0 | 0 | 4 | 2 | 9 | 0 | 6 | 3 | 7 | SE | 31 | 12 | 19 |
| 1811 | 61 | 37 | 48,5 | 2 | 3 | 14 | 1 | 3 | 2 | 5 | 1 | E | 31 | 7 | 24 |
| 1812 | 60 | 28 | 44,0 | 6 | 3 | 5 | 5 | 0 | 7 | 1 | 4 | SW | 31 | 17 | 14 |
| 1813 | 62 | 28 | 49,0 | 6 | 6 | 0 | 1 | 3 | 4 | 3 | 8 | NW | 31 | 11 | 20 |
| 1814 | 58 | 30 | 42,5 | 0 | 7 | 4 | 3 | 0 | 8 | 2 | 7 | SW | 31 | 12 | 19 |
| 1815 | 62 | 38 | 51,5 | 0 | 0 | 0 | 2 | 12 | 3 | 4 | 10 | S | 31 | 21 | 10 |
| 1816 | 54 | 32 | 45,0 | 5 | 3 | 5 | 0 | 1 | 10 | 2 | 5 | SW | 31 | 16 | 15 |
| 1817 | 60 | 32 | 48,5 | 5 | 1 | 2 | 4 | 4 | 5 | 2 | 8 | NW | 31 | 18 | 13 |
| 1818 | 58 | 36 | 47,0 | 3 | 0 | 1 | 2 | 3 | 4 | 8 | 10 | NW | 31 | 23 | 8 |
| 1819 | 60 | 38 | 48,5 | 1 | 10 | 3 | 0 | 3 | 2 | 3 | 9 | NE | 31 | 10 | 21 |
| 1820 | 61 | 30 | 46,0 | 1 | 8 | 3 | 1 | 2 | 4 | 6 | 6 | NE | 31 | 9 | 22 |
| 1821 | 56 | 40 | 50,0 | 2 | 0 | 1 | 5 | 2 | 5 | 9 | 7 | W | 31 | 17 | 14 |
| 1822 | 56 | 44 | 50,0 | 2 | 1 | 1 | 4 | 5 | 10 | 6 | 2 | SW | 31 | 14 | 17 |
| 1823 | 54 | 38 | 46,0 | 5 | 0 | 2 | 6 | 0 | 7 | 6 | 5 | SW | 31 | 13 | 18 |
| 1824 | 62 | 36 | 47,0 | 4 | 7 | 0 | 0 | 4 | 3 | 5 | 8 | NW | 31 | 18 | 13 |
| 1825 | 60 | 38 | 48,5 | 1 | 8 | 5 | 5 | 2 | 4 | 4 | 2 | NE | 31 | 17 | 14 |
| 1826 | 66 | 38 | 49,0 | 3 | 7 | 5 | 2 | 6 | 1 | 3 | 4 | NE | 31 | 8 | 23 |
| 1827 | 55 | 36 | 48,5 | 2 | 1 | 0 | 2 | 1 | 1 | 18 | 6 | W | 31 | 18 | 13 |
| For 21 years. | 66 | 28 | 47,0 | 50 | 94 | 79 | 68 | 57 | 93 | 91 | 119 | NW | 651 | 279 | 372 |

## APRIL.

| Years | Thermom. Max. | Thermom. Min. | Thermom. Mean | Winds N | NE | E | SE | S | SW | W | NW | P. Wind | Weather Nº of days | Wet | Dry |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1807 | 68 | 32 | 49,0 | 0 | 5 | 1 | 5 | 7 | 5 | 3 | 4 | S | 30 | 9 | 21 |
| 1808 | 60 | 34 | 46,0 | 5 | 1 | 1 | 4 | 3 | 3 | 2 | 11 | NW | 30 | 7 | 23 |
| 1809 | 58 | 34 | 46,0 | 4 | 7 | 1 | 1 | 0 | 6 | 2 | 9 | NW | 30 | 14 | 16 |
| 1810 | 66 | 34 | 51,5 | 1 | 2 | 3 | 6 | 4 | 10 | 2 | 2 | SW | 30 | 8 | 22 |
| 1811 | 62 | 32 | 51,0 | 1 | 3 | 3 | 5 | 7 | 6 | 2 | 3 | S | 30 | 10 | 20 |
| 1812 | 60 | 36 | 49,5 | 7 | 1 | 3 | 8 | 5 | 0 | 2 | 4 | SE | 30 | 9 | 21 |
| 1813 | 64 | 35 | 51,5 | 3 | 7 | 5 | 4 | 1 | 2 | 4 | 4 | NE | 30 | 10 | 20 |
| 1814 | 62 | 43 | 53,5 | 3 | 0 | 2 | 4 | 12 | 5 | 1 | 3 | S | 30 | 16 | 14 |
| 1815 | 66 | 40 | 52,5 | 6 | 6 | 6 | 3 | 1 | 0 | 0 | 8 | NW | 30 | 8 | 22 |
| 1816 | 68 | 38 | 50,0 | 6 | 4 | 2 | 8 | 2 | 2 | 2 | 4 | SE | 30 | 12 | 18 |
| 1817 | 62 | 39 | 50,5 | 6 | 10 | 6 | 3 | 1 | 0 | 1 | 3 | NE | 30 | 1 | 29 |
| 1818 | 64 | 38 | 51,5 | 1 | 4 | 9 | 3 | 7 | 1 | 2 | 3 | E | 30 | 14 | 16 |
| 1819 | 66 | 44 | 54,5 | 3 | 1 | 9 | 4 | 5 | 2 | 5 | 1 | E | 30 | 14 | 16 |
| 1820 | 66 | 40 | 53,0 | 6 | 3 | 7 | 1 | 0 | 3 | 1 | 9 | NW | 30 | 7 | 23 |
| 1821 | 64 | 45 | 53,5 | 0 | 4 | 0 | 6 | 1 | 5 | 3 | 11 | NW | 30 | 18 | 12 |
| 1822 | 64 | 40 | 50,5 | 9 | 2 | 2 | 3 | 4 | 5 | 2 | 3 | N | 30 | 10 | 20 |
| 1823 | 58 | 42 | 49,0 | 2 | 6 | 5 | 4 | 2 | 2 | 5 | 4 | NE | 30 | 12 | 18 |
| 1824 | 60 | 40 | 50,5 | 3 | 2 | 0 | 11 | 5 | 2 | 5 | 2 | SE | 30 | 17 | 13 |
| 1825 | 66 | 44 | 55,5 | 0 | 8 | 5 | 2 | 3 | 2 | 7 | 3 | NE | 30 | 11 | 19 |
| 1826 | 61 | 46 | 52,0 | 6 | 1 | 1 | 2 | 0 | 4 | 7 | 9 | NW | 30 | 10 | 20 |
| 1827 | 60 | 41 | 51,5 | 2 | 1 | 3 | 6 | 1 | 2 | 8 | 7 | W | 30 | 10 | 20 |
| For 21 years. | 68 | 32 | 51,5 | 74 | 78 | 74 | 93 | 71 | 67 | 66 | 107 | NW | 630 | 227 | 403 |

**MAY.**

| Years | Thermom. Max. | Thermom. Min. | Thermom. Mean | Winds N | NE | E | SE | S | SW | W | NW | P. Wind | Weather N° of days | Wet | Dry |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1807 | 72 | 46 | 56,5 | 5 | 0 | 5 | 5 | 6 | 5 | 3 | 2 | S | 31 | 14 | 17 |
| 1808 | 71 | 44 | 58,0 | 1 | 0 | 2 | 7 | 8 | 11 | 1 | 1 | SW | 31 | 13 | 18 |
| 1809 | 72 | 44 | 59,5 | 1 | 0 | 2 | 5 | 10 | 5 | 3 | 5 | S | 31 | 8 | 23 |
| 1810 | 66 | 44 | 55,5 | 8 | 0 | 5 | 10 | 1 | 5 | 1 | 1 | SE | 31 | 8 | 23 |
| 1811 | 72 | 50 | 59,5 | 3 | 1 | 2 | 2 | 7 | 10 | 3 | 3 | SW | 31 | 20 | 11 |
| 1812 | 70 | 50 | 58,5 | 1 | 6 | 5 | 1 | 11 | 1 | 0 | 6 | S | 31 | 14 | 17 |
| 1813 | 68 | 46 | 58,5 | 2 | 1 | 0 | 5 | 9 | 3 | 7 | 4 | S | 31 | 15 | 16 |
| 1814 | 68 | 44 | 56,0 | 1 | 3 | 12 | 2 | 4 | 2 | 2 | 5 | E | 31 | 5 | 26 |
| 1815 | 71 | 52 | 60,5 | 9 | 0 | 1 | 2 | 8 | 3 | 6 | 2 | N | 31 | 16 | 15 |
| 1816 | 68 | 42 | 56,5 | 1 | 3 | 6 | 1 | 2 | 3 | 4 | 11 | NW | 31 | 14 | 17 |
| 1817 | 64 | 44 | 55,0 | 10 | 3 | 1 | 3 | 2 | 2 | 5 | 5 | N | 31 | 16 | 15 |
| 1818 | 70 | 48 | 59,0 | 2 | 5 | 11 | 0 | 3 | 4 | 3 | 3 | E | 31 | 7 | 24 |
| 1819 | 74 | 50 | 59,5 | 4 | 6 | 2 | 4 | 3 | 3 | 4 | 5 | NE | 31 | 16 | 15 |
| 1820 | 72 | 46 | 59,0 | 1 | 2 | 3 | 4 | 12 | 5 | 0 | 4 | S | 31 | 17 | 14 |
| 1821 | 62 | 46 | 55,5 | 1 | 3 | 3 | 5 | 4 | 0 | 9 | 6 | W | 31 | 15 | 16 |
| 1822 | 69 | 43 | 58,5 | 4 | 1 | 8 | 7 | 7 | 2 | 2 | 0 | E | 31 | 9 | 22 |
| 1823 | 70 | 52 | 58,0 | 1 | 2 | 4 | 3 | 5 | 9 | 4 | 3 | SW | 31 | 13 | 18 |
| 1824 | 72 | 44 | 57,0 | 4 | 7 | 6 | 1 | 3 | 1 | 3 | 6 | NE | 31 | 15 | 16 |
| 1825 | 67 | 48 | 60,0 | 4 | 4 | 3 | 4 | 7 | 3 | 2 | 4 | S | 31 | 16 | 15 |
| 1826 | 71 | 47 | 59,0 | 1 | 21 | 1 | 1 | 0 | 0 | 2 | 5 | NE | 31 | 5 | 26 |
| 1827 | 63 | 46 | 55,0 | 0 | 2 | 2 | 4 | 4 | 6 | 7 | 6 | W | 31 | 17 | 14 |
| For 21 years. | 74 | 42 | 58,5 | 64 | 70 | 84 | 76 | 116 | 83 | 71 | 87 | S | 651 | 273 | 378 |

**JUNE.**

| Years | Thermom. Max. | Thermom. Min. | Thermom. Mean | Winds N | NE | E | SE | S | SW | W | NW | P. Wind | Weather N° of days | Wet | Dry |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1807 | 70 | 52 | 61,0 | 4 | 2 | 0 | 2 | 4 | 4 | 1 | 13 | NW | 30 | 3 | 27 |
| 1808 | 72 | 52 | 62,5 | 2 | 1 | 4 | 3 | 3 | 4 | 2 | 11 | NW | 30 | 5 | 25 |
| 1809 | 75 | 45 | 61,0 | 6 | 3 | 1 | 2 | 0 | 7 | 3 | 8 | NW | 30 | 9 | 21 |
| 1810 | 72 | 54 | 63,0 | 0 | 3 | 0 | 9 | 2 | 5 | 1 | 10 | NW | 30 | 7 | 23 |
| 1811 | 74 | 51 | 62,0 | 7 | 4 | 1 | 0 | 2 | 11 | 1 | 4 | SW | 30 | 14 | 16 |
| 1812 | 72 | 54 | 61,0 | 2 | 1 | 5 | 2 | 1 | 4 | 7 | 8 | NW | 30 | 9 | 21 |
| 1813 | 72 | 52 | 62,5 | 12 | 1 | 7 | 0 | 2 | 3 | 5 | 0 | N | 30 | 7 | 23 |
| 1814 | 74 | 50 | 61,5 | 8 | 2 | 2 | 2 | 6 | 3 | 3 | 4 | N | 30 | 4 | 26 |
| 1815 | 74 | 56 | 63,0 | 1 | 3 | 5 | 2 | 4 | 3 | 3 | 9 | NW | 30 | 11 | 19 |
| 1816 | 76 | 52 | 61,5 | 6 | 1 | 1 | 1 | 6 | 1 | 5 | 9 | NW | 30 | 8 | 22 |
| 1817 | 76 | 52 | 62,5 | 5 | 2 | 4 | 2 | 9 | 2 | 5 | 1 | S | 30 | 11 | 19 |
| 1818 | 77 | 58 | 67,5 | 4 | 0 | 7 | 0 | 1 | 3 | 7 | 8 | NW | 30 | 8 | 22 |
| 1819 | 72 | 56 | 61,0 | 6 | 2 | 1 | 1 | 3 | 4 | 7 | 6 | W | 30 | 17 | 13 |
| 1820 | 78 | 51 | 63,0 | 7 | 2 | 4 | 2 | 0 | 2 | 1 | 12 | NW | 30 | 8 | 22 |
| 1821 | 67 | 50 | 59,5 | 4 | 5 | 5 | 8 | 1 | 0 | 6 | 1 | SE | 30 | 9 | 21 |
| 1822 | 78 | 58 | 66,0 | 4 | 0 | 3 | 8 | 4 | 2 | 6 | 3 | SE | 30 | 10 | 20 |
| 1823 | 66 | 51 | 59,5 | 6 | 0 | 5 | 6 | 0 | 0 | 10 | 3 | W | 30 | 13 | 17 |
| 1824 | 70 | 55 | 62,5 | 3 | 7 | 3 | 3 | 5 | 4 | 2 | 3 | NE | 30 | 13 | 17 |
| 1825 | 75 | 52 | 65,5 | 3 | 6 | 2 | 2 | 5 | 3 | 5 | 4 | NE | 30 | 13 | 17 |
| 1826 | 80 | 58 | 68,5 | 1 | 4 | 7 | 9 | 1 | 0 | 0 | 8 | SE | 30 | 4 | 26 |
| 1827 | 70 | 53 | 60,0 | 1 | 6 | 0 | 3 | 1 | 3 | 4 | 12 | NW | 30 | 14 | 16 |
| For 21 years. | 80 | 45 | 62,5 | 92 | 55 | 67 | 67 | 60 | 68 | 84 | 137 | NW | 630 | 197 | 433 |

## JULY.

| Years | Thermom. Max. | Thermom. Min. | Thermom. Mean | Winds N | NE | E | SE | S | SW | W | NW | P. Wind | Weather Nº of days | Wet | Dry |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1807 | 75 | 58 | 66,0 | 1 | 2 | 2 | 1 | 5 | 10 | 7 | 3 | SW | 31 | 6 | 25 |
| 1808 | 80 | 59 | 68,5 | 0 | 4 | 1 | 5 | 2 | 12 | 2 | 5 | SW | 31 | 12 | 19 |
| 1809 | 74 | 53 | 63,5 | 7 | 5 | 3 | 2 | 1 | 3 | 1 | 9 | NW | 31 | 9 | 22 |
| 1810 | 74 | 54 | 63,0 | 3 | 0 | 0 | 2 | 3 | 14 | 5 | 4 | SW | 31 | 17 | 14 |
| 1811 | 76 | 56 | 66,0 | 4 | 3 | 6 | 0 | 3 | 7 | 2 | 6 | SW | 31 | 6 | 25 |
| 1812 | 76 | 56 | 64,0 | 1 | 4 | 3 | 3 | 2 | 8 | 4 | 6 | SW | 31 | 11 | 20 |
| 1813 | 74 | 54 | 64,0 | 3 | 0 | 1 | 2 | 2 | 9 | 6 | 8 | SW | 31 | 14 | 17 |
| 1814 | 76 | 58 | 66,0 | 4 | 0 | 1 | 2 | 8 | 3 | 2 | 11 | NW | 31 | 11 | 20 |
| 1815 | 74 | 53 | 66,0 | 5 | 2 | 6 | 2 | 3 | 2 | 1 | 10 | NW | 31 | 12 | 19 |
| 1816 | 74 | 54 | 61,5 | 6 | 0 | 1 | 1 | 2 | 3 | 8 | 10 | NW | 31 | 18 | 13 |
| 1817 | 74 | 56 | 63,0 | 5 | 0 | 0 | 3 | 5 | 4 | 11 | 3 | W | 31 | 19 | 12 |
| 1818 | 78 | 59 | 68,0 | 9 | 0 | 6 | 0 | 3 | 2 | 6 | 5 | N | 31 | 9 | 22 |
| 1819 | 78 | 57 | 67,5 | 7 | 5 | 0 | 4 | 2 | 2 | 5 | 6 | N | 31 | 4 | 27 |
| 1820 | 77 | 60 | 66,0 | 2 | 1 | 6 | 4 | 7 | 1 | 5 | 5 | S | 31 | 9 | 22 |
| 1821 | 71 | 56 | 63,5 | 6 | 3 | 1 | 2 | 3 | 3 | 7 | 6 | W | 31 | 10 | 21 |
| 1822 | 71 | 56 | 64,0 | 3 | 0 | 1 | 4 | 3 | 4 | 9 | 7 | W | 31 | 22 | 9 |
| 1823 | 70 | 54 | 61,5 | 2 | 0 | 0 | 1 | 2 | 3 | 17 | 6 | W | 31 | 20 | 11 |
| 1824 | 71 | 60 | 67,0 | 5 | 4 | 3 | 2 | 2 | 6 | 5 | 4 | SW | 31 | 13 | 18 |
| 1825 | 84 | 60 | 69,5 | 3 | 7 | 4 | 13 | 1 | 2 | 0 | 1 | SE | 31 | 2 | 29 |
| 1826 | 75 | 60 | 68,5 | 2 | 2 | 2 | 5 | 0 | 9 | 3 | 8 | SW | 31 | 9 | 22 |
| 1827 | 72 | 58 | 65,5 | 1 | 3 | 1 | 4 | 0 | 3 | 7 | 12 | NW | 31 | 11 | 20 |
| For 21 years. | 84 | 53 | 65,5 | 79 | 45 | 48 | 62 | 59 | 110 | 113 | 135 | NW | 651 | 244 | 407 |

## AUGUST.

| Years | Thermom. Max. | Thermom. Min. | Thermom. Mean | Winds N | NE | E | SE | S | SW | W | NW | P. Wind | Weather Nº of days | Wet | Dry |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1807 | 75 | 57 | 65,5 | 3 | 0 | 0 | 4 | 9 | 6 | 6 | 3 | S | 31 | 11 | 20 |
| 1808 | 74 | 54 | 66,0 | 1 | 8 | 0 | 0 | 4 | 9 | 3 | 6 | SW | 31 | 8 | 23 |
| 1809 | 73 | 53 | 62,5 | 0 | 0 | 0 | 1 | 4 | 14 | 3 | 9 | SW | 31 | 18 | 13 |
| 1810 | 70 | 53 | 62,0 | 3 | 0 | 2 | 1 | 5 | 6 | 3 | 11 | NW | 31 | 13 | 18 |
| 1811 | 70 | 54 | 61,5 | 4 | 0 | 1 | 0 | 4 | 7 | 7 | 8 | NW | 31 | 11 | 20 |
| 1812 | 72 | 54 | 64,5 | 8 | 1 | 4 | 3 | 6 | 2 | 1 | 6 | N | 31 | 5 | 26 |
| 1813 | 72 | 55 | 63,5 | 8 | 6 | 1 | 0 | 1 | 5 | 5 | 5 | N | 31 | 8 | 23 |
| 1814 | 74 | 56 | 64,0 | 1 | 0 | 1 | 0 | 6 | 7 | 6 | 10 | NW | 31 | 10 | 21 |
| 1815 | 74 | 58 | 65,0 | 6 | 0 | 4 | 0 | 1 | 9 | 6 | 5 | SW | 31 | 9 | 22 |
| 1816 | 70 | 56 | 62,0 | 10 | 2 | 5 | 0 | 4 | 3 | 4 | 3 | N | 31 | 12 | 19 |
| 1817 | 72 | 53 | 62,0 | 4 | 0 | 1 | 1 | 10 | 3 | 5 | 7 | S | 31 | 25 | 6 |
| 1818 | 76 | 58 | 66,0 | 11 | 4 | 9 | 0 | 0 | 1 | 3 | 3 | N | 31 | 6 | 25 |
| 1819 | 79 | 56 | 69,0 | 4 | 2 | 10 | 4 | 2 | 5 | 1 | 3 | E | 31 | 7 | 24 |
| 1820 | 72 | 50 | 63,0 | 5 | 2 | 1 | 2 | 2 | 9 | 7 | 3 | SW | 31 | 20 | 11 |
| 1821 | 72 | 58 | 65,0 | 2 | 0 | 0 | 11 | 2 | 4 | 9 | 3 | SE | 31 | 14 | 17 |
| 1822 | 76 | 54 | 63,0 | 3 | 2 | 0 | 7 | 2 | 7 | 8 | 2 | W | 31 | 10 | 21 |
| 1823 | 68 | 55 | 61,0 | 2 | 0 | 1 | 2 | 4 | 9 | 10 | 3 | W | 31 | 25 | 6 |
| 1824 | 72 | 58 | 64,0 | 3 | 4 | 1 | 4 | 0 | 2 | 11 | 6 | W | 31 | 18 | 13 |
| 1825 | 73 | 60 | 65,5 | 2 | 2 | 3 | 7 | 0 | 2 | 6 | 9 | NW | 31 | 13 | 18 |
| 1826 | 76 | 59 | 67,5 | 1 | 4 | 2 | 2 | 2 | 5 | 8 | 7 | W | 31 | 9 | 22 |
| 1827 | 69 | 55 | 65,0 | 3 | 13 | 1 | 1 | 1 | 2 | 4 | 6 | NE | 31 | 15 | 16 |
| For 21 years. | 79 | 50 | 64,5 | 84 | 50 | 47 | 50 | 69 | 117 | 116 | 118 | NW | 651 | 267 | 384 |

## SEPTEMBER.

| Years | Thermom. Max. | Thermom. Min. | Thermom. Mean | Winds N | NE | E | SE | S | SW | W | NW | P. Wind | Weather N° of days | Wet | Dry |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1807 | 68 | 44 | 57,0 | 6 | 3 | 0 | 0 | 3 | 7 | 0 | 11 | NW | 30 | 14 | 16 |
| 1808 | 68 | 42 | 59,0 | 6 | 5 | 3 | 1 | 3 | 4 | 1 | 7 | NW | 30 | 15 | 15 |
| 1809 | 74 | 48 | 59,5 | 0 | 6 | 0 | 4 | 0 | 4 | 5 | 11 | NW | 30 | 14 | 16 |
| 1810 | 72 | 52 | 58,0 | 5 | 3 | 4 | 3 | 5 | 4 | 0 | 6 | NW | 30 | 5 | 25 |
| 1811 | 70 | 49 | 60,0 | 1 | 1 | 13 | 4 | 3 | 2 | 0 | 6 | E | 30 | 8 | 22 |
| 1812 | 72 | 54 | 61,5 | 8 | 1 | 6 | 1 | 1 | 3 | 6 | 4 | N | 30 | 9 | 21 |
| 1813 | 68 | 49 | 60,0 | 3 | 3 | 7 | 2 | 3 | 4 | 5 | 3 | E | 30 | 13 | 17 |
| 1814 | 68 | 50 | 61,0 | 0 | 1 | 15 | 2 | 4 | 3 | 1 | 4 | E | 30 | 12 | 18 |
| 1815 | 74 | 52 | 63,0 | 0 | 6 | 5 | 2 | 4 | 8 | 4 | 1 | SW | 30 | 11 | 19 |
| 1816 | 68 | 49 | 60,0 | 2 | 3 | 3 | 1 | 3 | 4 | 7 | 7 | W | 30 | 12 | 18 |
| 1817 | 70 | 50 | 60,0 | 1 | 1 | 14 | 5 | 4 | 2 | 1 | 2 | E | 30 | 8 | 22 |
| 1818 | 72 | 52 | 61,0 | 4 | 0 | 2 | 3 | 5 | 5 | 6 | 5 | W | 30 | 17 | 13 |
| 1819 | 74 | 48 | 63,0 | 3 | 4 | 6 | 2 | 5 | 7 | 1 | 2 | SW | 30 | 14 | 16 |
| 1820 | 70 | 48 | 59,0 | 6 | 1 | 3 | 7 | 0 | 4 | 5 | 4 | SE | 30 | 13 | 17 |
| 1821 | 71 | 54 | 62,0 | 1 | 0 | 0 | 2 | 3 | 11 | 6 | 7 | SW | 30 | 16 | 14 |
| 1822 | 68 | 47 | 59,0 | 2 | 2 | 6 | 8 | 1 | 7 | 2 | 2 | SE | 30 | 9 | 21 |
| 1823 | 67 | 50 | 59,0 | 1 | 0 | 5 | 3 | 2 | 2 | 11 | 6 | W | 30 | 13 | 17 |
| 1824 | 68 | 49 | 61,0 | 6 | 0 | 1 | 8 | 1 | 6 | 5 | 3 | SE | 30 | 18 | 12 |
| 1825 | 72 | 57 | 64,0 | 2 | 4 | 1 | 3 | 8 | 3 | 5 | 4 | S | 30 | 10 | 20 |
| 1826 | 70 | 56 | 63,0 | 2 | 1 | 5 | 8 | 1 | 3 | 6 | 4 | SE | 30 | 7 | 23 |
| 1827 | 68 | 53 | 60,0 | 1 | 6 | 5 | 1 | 1 | 5 | 4 | 7 | NW | 30 | 13 | 17 |
| For 21 years. | 74 | 42 | 60,0 | 60 | 51 | 104 | 70 | 60 | 98 | 81 | 106 | NW | 630 | 251 | 379 |

## OCTOBER.

| Years | Thermom. Max. | Thermom. Min. | Thermom. Mean | Winds N | NE | E | SE | S | SW | W | NW | P. Wind | Weather N° of days | Wet | Dry |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1807 | 68 | 42 | 56,5 | 3 | 5 | 0 | 0 | 9 | 6 | 4 | 4 | S | 31 | 11 | 20 |
| 1808 | 60 | 46 | 50,5 | 4 | 0 | 2 | 0 | 3 | 3 | 3 | 16 | NW | 31 | 17 | 14 |
| 1809 | 66 | 45 | 53,0 | 0 | 0 | 5 | 15 | 7 | 0 | 3 | 1 | SE | 31 | 3 | 28 |
| 1810 | 65 | 38 | 55,5 | 3 | 3 | 5 | 6 | 2 | 7 | 0 | 5 | SW | 31 | 13 | 18 |
| 1811 | 64 | 40 | 58,5 | 0 | 1 | 0 | 1 | 10 | 15 | 1 | 3 | SW | 31 | 13 | 18 |
| 1812 | 67 | 42 | 54,5 | 5 | 0 | 0 | 0 | 10 | 4 | 4 | 8 | S | 31 | 18 | 13 |
| 1813 | 68 | 37 | 53,5 | 3 | 0 | 6 | 5 | 5 | 7 | 0 | 5 | SW | 31 | 15 | 16 |
| 1814 | 62 | 42 | 53,0 | 0 | 1 | 4 | 7 | 8 | 2 | 3 | 6 | S | 31 | 11 | 20 |
| 1815 | 65 | 44 | 55,0 | 0 | 4 | 2 | 4 | 6 | 6 | 2 | 7 | NW | 31 | 19 | 12 |
| 1816 | 68 | 42 | 56,0 | 6 | 1 | 2 | 9 | 5 | 4 | 2 | 2 | SE | 31 | 17 | 14 |
| 1817 | 58 | 40 | 50,0 | 7 | 5 | 9 | 2 | 2 | 0 | 0 | 6 | E | 31 | 10 | 21 |
| 1818 | 65 | 49 | 59,0 | 1 | 1 | 8 | 4 | 9 | 5 | 2 | 1 | S | 31 | 18 | 13 |
| 1819 | 68 | 38 | 54,5 | 4 | 6 | 1 | 2 | 4 | 2 | 2 | 10 | NW | 31 | 15 | 16 |
| 1820 | 62 | 45 | 53,0 | 2 | 1 | 7 | 4 | 0 | 4 | 0 | 13 | NW | 31 | 17 | 14 |
| 1821 | 64 | 46 | 55,5 | 5 | 1 | 0 | 3 | 3 | 10 | 5 | 4 | SW | 31 | 18 | 13 |
| 1822 | 62 | 47 | 54,5 | 3 | 0 | 0 | 7 | 9 | 3 | 8 | 1 | S | 31 | 20 | 11 |
| 1823 | 64 | 44 | 54,0 | 0 | 1 | 1 | 9 | 4 | 1 | 11 | 4 | W | 31 | 19 | 12 |
| 1824 | 64 | 42 | 55,0 | 2 | 2 | 0 | 3 | 7 | 5 | 3 | 9 | NW | 31 | 26 | 5 |
| 1825 | 65 | 46 | 58,0 | 5 | 2 | 1 | 1 | 5 | 1 | 9 | 7 | W | 31 | 12 | 19 |
| 1826 | 64 | 50 | 58,0 | 0 | 0 | 1 | 4 | 4 | 3 | 7 | 12 | NW | 31 | 7 | 24 |
| 1827 | 64 | 45 | 55,5 | 1 | 3 | 0 | 10 | 3 | 5 | 4 | 5 | SE | 31 | 22 | 9 |
| For 21 years. | 68 | 37 | 55,5 | 54 | 37 | 54 | 96 | 115 | 93 | 73 | 129 | NW | 651 | 321 | 330 |

## NOVEMBER.

| Years | Thermom. Max. | Min. | Mean | Winds. N | NE | E | SE | S | SW | W | NW | P. Wind | Weather. Nº of days | Wet | Dry |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1807 | 57 | 30 | 42,0 | 1 | 5 | 2 | 2 | 0 | 5 | 5 | 10 | NW | 30 | 19 | 11 |
| 1808 | 56 | 40 | 49,0 | 0 | 1 | 6 | 7 | 2 | 3 | 6 | 5 | SE | 30 | 14 | 16 |
| 1809 | 54 | 28 | 46,0 | 1 | 7 | 6 | 4 | 0 | 1 | 1 | 10 | NW | 30 | 9 | 21 |
| 1810 | 57 | 36 | 46,0 | 0 | 6 | 0 | 1 | 1 | 10 | 3 | 9 | SW | 30 | 21 | 9 |
| 1811 | 58 | 38 | 49,0 | 7 | 3 | 2 | 0 | 0 | 7 | 0 | 11 | NW | 30 | 20 | 10 |
| 1812 | 58 | 32 | 49,0 | 0 | 6 | 3 | 5 | 7 | 4 | 0 | 5 | S | 30 | 14 | 16 |
| 1813 | 62 | 40 | 48,5 | 0 | 1 | 5 | 4 | 6 | 2 | 4 | 8 | NW | 30 | 16 | 14 |
| 1814 | 56 | 36 | 47,0 | 1 | 5 | 1 | 2 | 8 | 2 | 2 | 9 | NW | 30 | 11 | 19 |
| 1815 | 59 | 32 | 46,0 | 6 | 2 | 9 | 1 | 2 | 4 | 1 | 5 | E | 30 | 10 | 20 |
| 1816 | 55 | 36 | 46,0 | 6 | 6 | 2 | 3 | 1 | 3 | 1 | 8 | NW | 30 | 16 | 14 |
| 1817 | 58 | 40 | 53,0 | 1 | 1 | 0 | 1 | 8 | 10 | 2 | 7 | SW | 30 | 11 | 19 |
| 1818 | 62 | 48 | 55,0 | 1 | 0 | 2 | 7 | 6 | 8 | 2 | 4 | SW | 30 | 20 | 10 |
| 1819 | 58 | 34 | 47,0 | 7 | 6 | 1 | 0 | 3 | 2 | 2 | 9 | NW | 30 | 21 | 9 |
| 1820 | 58 | 38 | 49,0 | 3 | 8 | 4 | 5 | 2 | 5 | 0 | 3 | NE | 30 | 18 | 12 |
| 1821 | 60 | 41 | 52,0 | 2 | 0 | 0 | 4 | 7 | 9 | 8 | 0 | SW | 30 | 21 | 9 |
| 1822 | 58 | 42 | 51,5 | 1 | 1 | 0 | 0 | 8 | 10 | 9 | 1 | SW | 30 | 22 | 8 |
| 1823 | 58 | 42 | 50,5 | 1 | 1 | 12 | 0 | 7 | 5 | 2 | 2 | E | 30 | 10 | 20 |
| 1824 | 58 | 44 | 52,0 | 0 | 0 | 0 | 4 | 1 | 8 | 6 | 11 | NW | 30 | 24 | 6 |
| 1825 | 57 | 38 | 50,5 | 4 | 2 | 0 | 0 | 0 | 3 | 9 | 12 | NW | 30 | 25 | 5 |
| 1826 | 56 | 36 | 48,5 | 5 | 7 | 0 | 1 | 0 | 1 | 5 | 11 | NW | 30 | 13 | 17 |
| 1827 | 56 | 40 | 52,0 | 0 | 4 | 0 | 3 | 4 | 0 | 12 | 7 | W | 30 | 21 | 9 |
| For 21 years. | Max. | Min. | Mean | N | NE | E | SE | S | SW | W | NW | P. Wind | Nº of days | Wet | Dry |
| | 62 | 28 | 49,0 | 47 | 72 | 55 | 54 | 73 | 102 | 80 | 147 | NW | 630 | 356 | 274 |

## DECEMBER.

| Years | Thermom. Max. | Min. | Mean | Winds. N | NE | E | SE | S | SW | W | NW | P. Wind | Weather. Nº of days | Wet | Dry |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1807 | 50 | 29 | 43,5 | 3 | 0 | 0 | 8 | 3 | 3 | 5 | 9 | NW | 31 | 13 | 18 |
| 1808 | 54 | 27 | 42,5 | 2 | 6 | 4 | 7 | 1 | 1 | 5 | 5 | SE | 31 | 12 | 19 |
| 1809 | 54 | 34 | 45,0 | 2 | 0 | 0 | 0 | 0 | 8 | 5 | 16 | NW | 31 | 23 | 8 |
| 1810 | 53 | 32 | 45,0 | 1 | 2 | 4 | 1 | 1 | 8 | 3 | 11 | NW | 31 | 21 | 10 |
| 1811 | 54 | 29 | 44,0 | 4 | 5 | 0 | 2 | 0 | 13 | 0 | 7 | SW | 31 | 20 | 11 |
| 1812 | 54 | 26 | 41,0 | 1 | 8 | 10 | 3 | 3 | 0 | 2 | 4 | E | 31 | 6 | 25 |
| 1813 | 54 | 30 | 46,5 | 3 | 9 | 4 | 5 | 2 | 1 | 5 | 2 | NE | 31 | 16 | 15 |
| 1814 | 58 | 34 | 48,0 | 2 | 1 | 2 | 2 | 5 | 6 | 7 | 6 | W | 31 | 20 | 11 |
| 1815 | 52 | 22 | 44,0 | 2 | 3 | 1 | 0 | 0 | 11 | 4 | 10 | SW | 31 | 16 | 15 |
| 1816 | 54 | 30 | 44,0 | 1 | 2 | 2 | 2 | 5 | 3 | 11 | 5 | W | 31 | 23 | 8 |
| 1817 | 58 | 30 | 44,0 | 1 | 5 | 1 | 0 | 5 | 2 | 3 | 14 | NW | 31 | 24 | 7 |
| 1818 | 58 | 34 | 45,5 | 5 | 6 | 3 | 7 | 2 | 4 | 2 | 2 | SE | 31 | 10 | 21 |
| 1819 | 56 | 26 | 43,5 | 3 | 6 | 1 | 0 | 3 | 7 | 2 | 9 | NW | 31 | 20 | 11 |
| 1820 | 56 | 28 | 46,5 | 1 | 7 | 1 | 3 | 1 | 8 | 7 | 3 | SW | 31 | 11 | 20 |
| 1821 | 55 | 38 | 48,0 | 0 | 0 | 0 | 2 | 4 | 16 | 2 | 7 | SW | 31 | 29 | 2 |
| 1822 | 53 | 28 | 42,0 | 1 | 6 | 10 | 4 | 0 | 2 | 6 | 2 | E | 31 | 19 | 12 |
| 1823 | 54 | 41 | 48,0 | 3 | 0 | 1 | 0 | 2 | 3 | 16 | 6 | W | 31 | 19 | 12 |
| 1824 | 57 | 39 | 50,5 | 3 | 0 | 1 | 3 | 0 | 7 | 12 | 5 | W | 31 | 28 | 3 |
| 1825 | 56 | 31 | 47,0 | 3 | 2 | 0 | 5 | 1 | 3 | 6 | 11 | NW | 31 | 16 | 15 |
| 1826 | 58 | 38 | 49,0 | 9 | 3 | 0 | 1 | 2 | 4 | 5 | 7 | N | 31 | 15 | 16 |
| 1827 | 54 | 42 | 49,5 | 0 | 1 | 0 | 2 | 1 | 7 | 15 | 5 | W | 31 | 21 | 10 |
| For 21 years. | Max. | Min. | Mean | N | NE | E | SE | S | SW | W | NW | P. Wind | Nº of days | Wet | Dry |
| | 58 | 22 | 46,0 | 50 | 72 | 45 | 57 | 41 | 117 | 123 | 146 | NW | 651 | 383 | 268 |

| Months | Thermom. | | | Winds. | | | | | | | | | Weather. | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Maxima | Minima | Media | N | NE | E | SE | S | SW | W | NW | Prevailing wind | Partial showers | Showery days | Hail showers | Snow showers | Snowy days | Misty days | Partial rain | Rainy days | Number of days | Wet days | Dry days |
| Jan. | 56 | 19 | 43,0 | 56 | 95 | 66 | 79 | 60 | 70 | 100 | 125 | NW | 61 | 23 | 23 | 20 | 7 | 32 | 93 | 76 | 631 | 335 | 316 |
| Feb. | 58 | 26 | 45,5 | 31 | 62 | 47 | 63 | 58 | 127 | 106 | 99 | SW | 52 | 22 | 25 | 11 | 0 | 33 | 104 | 65 | 593 | 312 | 281 |
| Mar. | 66 | 28 | 47,0 | 50 | 94 | 79 | 68 | 57 | 93 | 91 | 119 | NW | 67 | 14 | 25 | 8 | 0 | 16 | 98 | 51 | 651 | 279 | 372 |
| April | 68 | 32 | 51,5 | 74 | 78 | 74 | 93 | 71 | 67 | 66 | 107 | NW | 68 | 18 | 27 | 0 | 0 | 15 | 64 | 35 | 630 | 227 | 403 |
| May | 74 | 42 | 58,5 | 64 | 70 | 84 | 76 | 116 | 83 | 71 | 87 | S | 72 | 11 | 7 | 0 | 0 | 21 | 125 | 37 | 651 | 273 | 378 |
| June | 80 | 45 | 62,5 | 92 | 55 | 67 | 67 | 60 | 68 | 84 | 137 | NW | 70 | 6 | 5 | 0 | 0 | 14 | 86 | 16 | 630 | 197 | 433 |
| July | 84 | 53 | 65,5 | 79 | 45 | 48 | 62 | 59 | 110 | 123 | 135 | NW | 65 | 10 | 1 | 0 | 0 | 30 | 98 | 40 | 651 | 244 | 407 |
| Aug. | 79 | 50 | 64,5 | 84 | 50 | 47 | 50 | 69 | 117 | 116 | 118 | NW | 88 | 14 | 0 | 0 | 0 | 22 | 110 | 33 | 651 | 267 | 384 |
| Sept. | 74 | 42 | 60,0 | 60 | 51 | 104 | 70 | 60 | 98 | 81 | 106 | NW | 86 | 16 | 5 | 0 | 0 | 13 | 97 | 34 | 630 | 251 | 379 |
| Oct. | 68 | 37 | 55,5 | 54 | 37 | 54 | 96 | 115 | 93 | 73 | 129 | NW | 79 | 38 | 17 | 1 | 0 | 16 | 100 | 70 | 651 | 321 | 330 |
| Nov. | 62 | 28 | 49,0 | 47 | 72 | 55 | 54 | 73 | 102 | 80 | 147 | NW | 86 | 30 | 22 | 3 | 0 | 26 | 107 | 82 | 630 | 356 | 274 |
| Dec. | 58 | 22 | 46,0 | 50 | 72 | 45 | 57 | 41 | 117 | 123 | 146 | NW | 84 | 35 | 43 | 13 | 0 | 16 | 100 | 92 | 651 | 383 | 268 |
| For 21 years. | Maximum | Minimum | Medium | N | NE | E | SE | S | SW | W | NW | Prevailing wind | Partial showers | Showery days | Hail showers | Snow showers | Snowy days | Misty days | Partial rain | Rainy days | Number of days | Wet days | Dry days |
| | 84 | 19 | 54,5 | 741 | 781 | 770 | 835 | 839 | 1,145 | 1,104 | 1,455 | NW | 878 | 237 | 200 | 56 | 7 | 254 | 1,182 | 631 | 7,670 | 3,445 | 4,225 |

*The Maxima, Minima, and Media of the Register Thermometer for seven Years, with the Number of Inches of Rain fallen during that Period.*

| Years. | January. | | | | February. | | | | March. | | | | April. | | | | May. | | | | June. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Register Thermom. | | | Rain in inches | Register Thermom. | | | Rain in inches | Register Thermom. | | | Rain in inches | Register Thermom. | | | Rain in inches | Register Thermom. | | | Rain in inches | Register Thermom. | | | Rain in inches |
| | Max. | Min. | Med. | | Max. | Min. | Med. | | Max. | Min. | Med. | | Max. | Min. | Med. | | Max. | Min. | Med. | | Max. | Min. | Med. | |
| 1821 | 55 | 26 | 45,0 | 2,900 | 52 | 32 | 41,5 | 0,520 | 56 | 34 | 46,5 | 4,205 | 64 | 37 | 50,0 | 2,300 | 62 | 40 | 50,5 | 3,385 | 67 | 43 | 56,5 | 1,560 |
| 1822 | 50 | 34 | 44,0 | 2,42,5 | 54 | 36 | 45,5 | 2,440 | 58 | 39 | 49,5 | 2,800 | 65 | 36 | 48,0 | 2,380 | 69 | 42 | 55,5 | 1,340 | 78 | 52 | 64,0 | 2,240 |
| 1823 | 54 | 27 | 39,0 | 8,625 | 52 | 29 | 43,0 | 6,965 | 55 | 35 | 46,0 | 3,205 | 58 | 38 | 47,0 | 1,980 | 70 | 44 | 55,0 | 2,770 | 66 | 44 | 55,5 | 1,940 |
| 1824 | 54 | 30 | 44,5 | 2,00,5 | 58 | 34 | 46,0 | 3,280 | 62 | 32 | 45,0 | 5,740 | 60 | 36 | 45,0 | 2,600 | 72 | 38 | 53,0 | 3,95,5 | 70 | 47 | 57,0 | 4,660 |
| 1825 | 55 | 32 | 45,0 | 3,375 | 57 | 35 | 45,5 | 2,700 | 61 | 35 | 46,0 | 3,610 | 67 | 36 | 48,0 | 1,005 | 67 | 48 | 55,0 | 4,085 | 75 | 47 | 58,0 | 1,800 |
| 1826 | 55 | 26 | 41,0 | 3,075 | 56 | 36 | 47,0 | 5,295 | 66 | 34 | 46,5 | 2,310 | 61 | 33 | 50,5 | 1,110 | 71 | 38 | 54,0 | 0,600 | 80 | 40 | 62,0 | 0,660 |
| 1827 | 54 | 24 | 42,0 | 4,420 | 54 | 27 | 38,0 | 1,600 | 56 | 32 | 47,0 | 5,265 | 61 | 36 | 48,0 | 1,360 | 63 | 40 | 52,0 | 5,315 | 71 | 47 | 58,0 | 2,160 |
| | Max. | Min. | Med. | Inches | Max. | Min. | Med. | Inches | Max. | Min. | Med. | Inches | Max. | Min. | Med. | Inches | Max. | Min. | Med. | Inches | Max. | Min. | Med. | Inches |
| | 55 | 24 | 43,0 | 26,825 | 58 | 27 | 44,5 | 22,800 | 66 | 32 | 46,5 | 27,135 | 67 | 33 | 48,5 | 12,735 | 72 | 38 | 53,0 | 21,450 | 80 | 40 | 59,0 | 15,020 |

| Years. | July. | | | | August. | | | | September. | | | | October. | | | | November. | | | | December. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1821 | 71 | 51 | 60,5 | 1,660 | 73 | 55 | 63,0 | 4,470 | 72 | 49 | 60,5 | 4,520 | 64 | 41 | 54,0 | 5,770 | 60 | 40 | 51,0 | 5,170 | 56 | 36 | 47,5 | 9,500 |
| 1822 | 71 | 52 | 61,0 | 9,070 | 77 | 48 | 61,0 | 2,200 | 68 | 45 | 59,0 | 1,865 | 63 | 43 | 53,5 | 5,315 | 58 | 39 | 50,0 | 7,255 | 54 | 28 | 40,5 | 2,545 |
| 1823 | 70 | 49 | 59,0 | 3,030 | 68 | 48 | 59,5 | 7,085 | 67 | 46 | 57,5 | 3,645 | 64 | 37 | 51,5 | 8,210 | 58 | 38 | 48,5 | 3,705 | 55 | 40 | 46,5 | 6,295 |
| 1824 | 71 | 50 | 61,0 | 3,180 | 72 | 49 | 61,0 | 2,600 | 68 | 40 | 51,5 | 4,165 | 64 | 39 | 54,0 | 5,965 | 58 | 36 | 50,5 | 6,910 | 57 | 32 | 48,0 | 6,470 |
| 1825 | 84 | 49 | 63,0 | 0,310 | 73 | 52 | 62,0 | 3,140 | 72 | 46 | 58,5 | 2,730 | 65 | 38 | 54,0 | 5,090 | 57 | 35 | 47,5 | 5,820 | 56 | 29 | 45,5 | 5,285 |
| 1826 | 75 | 52 | 64,0 | 1,240 | 76 | 52 | 63,0 | 2,115 | 70 | 46 | 60,0 | 4,315 | 65 | 43 | 56,0 | 2,840 | 56 | 34 | 46,0 | 4,280 | 58 | 36 | 50,0 | 4,400 |
| 1827 | 73 | 52 | 61,5 | 2,250 | 70 | 50 | 59,0 | 2,865 | 68 | 48 | 58,0 | 2,820 | 63 | 42 | 54,5 | 6,105 | 56 | 33 | 50,5 | 3,165 | 55 | 40 | 49,0 | 7,580 |
| | Max. | Min. | Med. | Inches | Max. | Min. | Med. | Inches | Max. | Min. | Med. | Inches | Max. | Min. | Med. | Inches | Max. | Min. | Med. | Inches | Max. | Min. | Med. | Inches |
| | 84 | 49 | 61,0 | 20,740 | 77 | 48 | 61,5 | 24,475 | 72 | 40 | 58,0 | 24,060 | 65 | 37 | 54,5 | 39,295 | 60 | 33 | 49,0 | 36,305 | 58 | 28 | 46,5 | 42,075 |

*Annual Results for 7 Years.*

| Months | Register Thermometer | | | Rain in inches | Years | Register Thermometer | | | Inches of rain fallen in each year | Wet days | Dry* days |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Max. | Min. | Med. | | | Max. | Min. | Med. | | | |
| Jan. | 55 | 24 | 43,0 | 26,825 | 1821 | 73 | 26 | 52,5 | 46,020 | 186 | 179 |
| Feb. | 58 | 27 | 44,5 | 22,800 | 1822 | 78 | 28 | 53,0 | 41,875 | 172 | 193 |
| Mar. | 66 | 32 | 46,5 | 27,135 | 1823 | 70 | 27 | 51,0 | 57,455 | 196 | 169 |
| Apr. | 67 | 33 | 48,5 | 12,735 | 1824 | 72 | 30 | 51,5 | 51,470 | 225 | 141 |
| May | 72 | 38 | 54,0 | 21,450 | 1825 | 84 | 29 | 52,0 | 38,950 | 161 | 204 |
| June | 80 | 40 | 59,0 | 15,020 | 1826 | 80 | 26 | 53,5 | 32,240 | 114 | 251 |
| July | 84 | 49 | 61,0 | 20,740 | 1827 | 73 | 24 | 51,5 | 44,905 | 188 | 177 |
| Aug. | 77 | 48 | 61,5 | 24,475 | .. | .. | .. | .. | .. | .. | .. |
| Sept. | 72 | 40 | 58,0 | 24,060 | .. | .. | .. | .. | .. | .. | .. |
| Oct. | 65 | 37 | 54,5 | 39,295 | .. | .. | .. | .. | .. | .. | .. |
| Nov. | 60 | 33 | 49,0 | 36,305 | .. | .. | .. | .. | .. | .. | .. |
| Dec. | 58 | 28 | 46,5 | 42,075 | .. | .. | .. | .. | .. | .. | .. |
| | Max. of 7 years | Min. of 7 years | Med. of 7 years | Inches of rain fallen in 7 years | | Med. of the Max. | Med. of the Min. | Med. of 7 years | Average quantity of rain for 7 years | Average wet days for 7 years | Average dry days for 7 years |
| | 84 | 24 | 52,0 | 312,915 | | 75,5 | 27,1 | 52,0 | 44,702 | 177,3 | 187,5 |

The thermometers I use are two double registers,—one made by Newman, and the other by Cary,—which are placed in a north-eastern aspect, in such a manner as not to be acted upon by reflected heat.

My rain-gauge is placed in my garden at the level of the ground, entirely free from the influence of buildings or trees. The gauge is exactly six inches diameter at the edge of the brass rim at the top. The basin is made of pewter $2\frac{1}{2}$ inches deep. The tube attached to the basin is three inches long, tapered to about $\frac{1}{8}$th of an inch at the point. The measure is a glass cylinder graduated to a quarter of an inch of water, or $1784\frac{8}{10}$ grains, nearly 1784·796 grains.

Penzance, Jan. 18th, 1828. EDWARD C. GIDDY.

* Dry days are those on which no fall whatever takes place,—not the slightest shower.

XXIX. *Description*

## XXIX. *Description of New Succulent Plants. By* A. H. Haworth, *Esq. F.L.S. &c.*

*To the Editors of the Philosophical Magazine and Annals.*

Gentlemen,

HAVING finished another new Decade, viz. the eleventh, of new Succulent Plants, which may now commence a second century of them, I lose no time in forwarding it to you, as under; hoping it may soon find a place in your very useful Annals.

The Decade contains, as usual, a regular and scientific distribution and description of ten distinct species of Succulent Plants; all, as far as my long experience enables me to determine, perfectly new and unrecorded. And no less than eight of these fine plants are from our old and most productive source, the Cape of Good Hope; where Mr. Bowie, detected them, and succeeded in transmitting them to the royal gardens of Kew, where they are now flourishing, and where the descriptions were made from the living plants.

Of the native countries of the two remaining plants which constitute the Decade, I am not quite so certain. But if they are not both from North America, one of them at least, viz. the *Yucca*, is probably from thence, as the greater part of its noble genus has been found there.

I remain, Gentlemen, yours, &c.

Chelsea, Sept. 1827. A. H. Haworth.

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*Decas undecima Plantarum Novarum Succulentarum.*

Classis et Ordo. Pentandria Pentagynia.

*Genus*, Sedum *Auctorum.*

*Sectio*, * Acutifolia *Nob.*

*viridulum.* S. (small green) foliis suberectis lineari-subulatis
1. viridibus uno latere submucronulatis.

Nova species, ex horto Chelseiano, communicante amico Dom. Anderson.

*Obs.* Patriam nescio, nisi Europæam seu Americanam, vix Asiaticam. Sub dio cum affinibus viget.

*Obs.* *Sedo virenti* affine; et *S. recurvato* Willd. simillimum, at foliis paulò majoribus planioribus viridioribus, et sine dubio obtusioribus, potiúsve ad apicem quasi à latere inferiore obliquè truncatis. Minus est quàm *Sedo anopetalo*, atque viridius.

*Genus,*

*Genus*, CURTOGYNE *Nob. in Revis. Pl. Succ. p.* 8.

*undosa.* C. (reflexing-leaved) foliis ovato-linguiformibus cris-
2. pis, ramulorum florentium retroflexis.

*Habitat* ad Caput Bonæ Spei, ubi in locis natalibus invenit amicus Dom. Bowie, misitque ad regium hortum Kewensem.

*Floret* Aug. G. H. ♄.

*Obs.* Simillima *Crassulæ albæ Sims in Bot. Mag.* at major, floribus ut in illâ. Subpedalis ramosa, ramis florentibus, calamo sæpè crassioribus, et terram versus decumbentibus, apicibus assurgentibus. *Folia* arctè amplexicaulia decussata numerosa viridia: infra flores imbricatim retroflexa, atque in aëre aperto sæpè rufo-cincta: in ramis junioribus longiora magísque crispatim undosa, et sæpè sesquiunguicularia. Etiam simulat *Crassulam undosam Nob.* at major, magísque undosa: sed pòst id locarem.

*Obs.* *Squamula* ordinaria ad lentem quadratim truncata retusa pallida at cerina. *Styli*釋 internè (lentis ope) cultrati et puberulo-ciliati.

Classis et Ordo. PENTANDRIA PENTAGYNIA.

PURGOSEA* (TURGOSEA *Nob. in Revis. Succ. p.* 14.) *Crassula Thunb.*

*Inflorescentia* spicato-thyrsoidea. *Corolla* 5-petala, *petalis* basi læviter imbricantibus, infra apicem mucronulatis. *Germina* intùs plana, extùs gibba, *squamulis* ordinariis emarginatis.

*Sectio*, LINGUÆFOLIA, foliis lorato-linguæformibus subacutis crassis.

*pertusula.* P. (small impress-dotted) foliis lanceolatis recur-
3. vulis suprà subimpresso-punctatis; bracteis summis cordatis integris, scapo paniculato.

*Habitat* ad Cap. B. Sp. ubi invenit Dom. Bowie.

*Florebat* in regio horto Kewensi, Oct., Nov., &c. A.D. 1824. G. H. ♃.

*Obs.* *T. pertusæ* simillima, at plùs duplò minor. *Folia* radicalia subbiuncialia 4-5 lineas lata crassa pallidè viridia, rubro sæpè infernè suffusa, minutè cartilagineo-serrulata; ad lentem parcéque (florendi tempore) impresso-punctata: (nec incurva semicylindracea impresso-punctata 4-5-uncialia, 9 lineas lata, ut in *T. pertusâ.*) *Scapus* infernè longè ramosus, ramis simplicibus, vix puberulus. *Bracteæ* numerosissimæ usque ad 30 paria,

* A voce πύργος, *Turris*: wrongly printed *Turgosea*, in *Revis. Succ.*

30 paria, cordato-lanceolatæ expansæ internodiis longiores; imæ remotæ, superiores pedetentim magìs approximatæ. *Flores* pauciores in singulo fasciculo quàm in *C. pertusâ*, pedunculo sæpè longiore. *Corolla* nivea *petalis* erectis sed ultra medium revoluto-recurvis. *Styli* basi collecti erecti, supernè recurvo-patentes breves pallidè virides, *stigmate* (per lentem) hemisphærico limpido sive albo.

*Obs.* The specific character of *C. pertusa* will now require to be altered as follows:—Foliis lorato-acuminatis incurvis semicylindraceis, bracteis superioribus ovato-lanceolatis cartilagineo-serrulatis adscendentibus; scapo thyrsiformi.

*Obs.* *T. pertusa* is my ancient *Aloë pertusa*, taken from Commeline (Vide *Revis. Pl. Succ.* 15 & 201, A.D. 1821); and it is also *Crassula corymbulosa* of the same work, *p.* 10, (described from a bad and dead specimen); and likewise *Crassula corymbulosa* of *Link's Hort. Berl.* A.D. 1821.

Classis et Ordo. DECANDRIA PENTAGYNIA.

*Genus*, COTYLEDON *Auctorum.*

*Sectio*, * LATIFOLIA.

*cuneiformis.* C. (short, wedge-leaved) brevicaulis: ramosa:
4. foliis confertis obovato-cuneatis mucronatis subfarinoso-albis.

*Habitat* ad Cap. B. Sp. ubi invenit Dom. Bowie, ante ann. 1824.

*Obs. Caudex* brevis fruticosus valdè ramosus. *Folia* subindè (per culturam) virescent, sed sæpè farinoso-alba. *C. crassifoliæ Nob.* similis, at multoties humilior. *Flores* non vidi.

*Sectio nova*, VILLOSULÆ.

*interjecta.* C. (fleshy-stemmed) foliis angustè oblongis acutis
5. inflexo-canaliculatis; caudice brevi valido.

*Habitat* ad Cap. B. Sp. Dom. Bowie. G. H. ♄.

*Flores* non vidi.

*Obs. Caudex* teres adhuc 4-uncialis, tuberoso-ramosus carnosus. *Folia* incurva crassa carnosa glaucescente-cinerea triuncialia, quinque lineas lata; subtus convexa, basi semiteretia, sine carina, *C. spuriæ* proxima, sed altior, *foliis* brevioribus crassioribus angustioribus, magísque canaliculatis, et sine dubio incurvis, nec recurvis. In *C. spuria*, *folia* 4-5 uncias longa pe-

 tiolo

tiolo desinentia, 9 lineas lata recurva, et spatulato-lanceolata. Viget in regio horto Kewensi, sed *flores* non vidi.

Classis et Ordo. HEXANDRIA MONOGYNIA.

*Genus,* YUCCA *Auctorum,* et *Nob. in Suppl. Pl. Succ.* 31, charactere novo ampliori.

*Sectio,* * * FILIFERÆ *Nob. in loco.*

*puberula.* Y. (pubescent, thready) acaulis: foliis lorato-lan-
6. ceolatis patentibus glaucis; fibris marginalibus pauculis fulvis: ramulis florigeris densè puberulis.

*Habitat* fortè cum cæteris affinibus in Americâ Septentrionali. Communicavit florentem mense Augusti A.D. 1827, amicus Dom. Sweet, *Horti Britannici; Geraniacearum; Cistinearum; Floræ Australasiæ Iconum, &c. &c.* utilissimus Auctor.

*Obs.* In sectione FILIFERÆ inter *Yuccam glaucescentem Nob.* et *Y. recurvam Salisb. Parad. Lond.* 31. speciem hanc conspicuam pro certo locarem. *Folia* inflexo-concavula, deorsùm ad costam magìs magísque incrassescentia. *Scapus* affinium quadripedalis ramosus corymbosus, infernè magìs bracteolatus; *floribus* lævibus ovato-globulosis, pendulis lacteis. *Petala* lanceolata, interiora longiora, ferè duplò, atque quàm aliis latiora.

*Obs.* I believe Mr. Sweet received the fine plant above described, in bloom, from Mr. Millar's nursery at Bristol, the day before he brought it to me: and we may soon expect a figure of it in his beautiful *Hardy Flower Garden.*

*Genus,* HAWORTHIA *Duval in Cat. Pl. Succ. in Horto Alençonio,* A.D. 1809.

*Sectio,* MARGARITIFERÆ.

*clariperla.* H. (small, bright-pearled) foliis subulato-acutis
7. undique præclari-perlatis, perlis subtùs majoribus.

*Habitat* ad Cap. B. Sp. Etiam viget apud amicum Dom. Hitchin, Nordovici, qui nuper viventem communicavit.

*Obs.* Inter *H. attenuatam* et *H. fasciatam* apparet; priorem valdè simulat sed minorem, perlis longè pulchrioribus et clarioribus, et subtùs sæpè fasciatim serialibus; foliis longè brevioribus sive minùs attenuatis. Fortè *H. fasciatæ* magìs affinis, præcipuè in magnitudine. *Flores* ut in affinibus. Ab *H. attenuatâ* distinguitur,

distinguitur, perlis undique magìs extantibus pulchrioribus; et ab *H. fasciatâ*, foliis undique (nec infrà solùm) perlatis.

Classis et Ordo. Dodecandria Trigynia.

*Genus*, Euphorbia *Auctorum.*

*Sectio*, Florispinæ *Nob.*

*pentagona.* E. (Cape, 5-angled) erecto-decumbens: dodran-
8. talis: spinis subsemuncialibus.

*Habitat* ad Cap. B. Sp. ubi invenit Dom. Bowie, ante ann. 1825. G. H. ♄.

*Obs. Caudex* adhuc pentagonus, diametro semunciali, supernè glaucescens, sulcis validis, et concinnè lineolâ exaratâ impressis. *Spinæ* adhuc trilineares ultráque, florigeræ ut in *Euphorbiâ enneagonâ Nob.* cui simillima, et antè id locarem.

*Obs.* Caudices in horto caldario, decumbentes at grossi, apicibus assurgentibus, *spinis* citiùs marcescentibus; et postea decidentibus affinium more. *Flores* affinium, sed perfectos non examinavi.

Classis et Ordo. Dodecandria Monogynia.

Rülingia *Ehrhart.* Portulacca *Linn.*

*intermedia.* R. (thready, intermediate) foliis densissimè nu-
9. merosis expansis planis, extùs convexis; apice retuso-deltoidibus; filamentis axillaribus tortilibus fulvis.

*Habitat* ad Cap. B. Sp. ubi invenit Dom. Bowie, circa ann. 1823. G. H. ♄.

*Obs. R. polyphyllæ Nob. in Suppl. Pl. Succ. p.* 63. simillima, at duplò plúsve minor, seu angustior, sed fortassè elatior. Etiam simulat *R. filamentosam Nob.* at latior, foliis numerosioribus confertioribus pallidioribus, pilis axillaribus fulvicantibus, nec albis.

*Obs. Surculi* sæpiùs simplices subtrilineares, diametro semunciali, incomptè piliferi. *Folia* arachnoideo-incompta, sordida, sive fusco- seu rufo-viridia. *Flores* affinium.

Classis et Ordo. Syngenesia Polygamia Necessaria.

*Genus*, Cineraria *Auctorum.*

*vestita.* C. (cottony) subacaulis: lanato-vestita: foliis multi-
10. fariis subtereti-spatulescentibus planiusculis.

*Obs. Caudex* crassus, 2-3-uncialis, *ramulis* e terrâ pullulantibus et demùm cæspitosè crescentibus.

*Folia* 3-4-uncialia varianter depresso-teretiuscula succulenta, niveo lævi atque nitenti tomento, uti caudices eleganter vestita, *Kleinii tomentosi Nob.* exacto more, cui forsan ob multifaria folia affinis, sed non resinosa ut in illâ. *Foliorum* majorum apices sæpè plùs minùs impressi potiùs quàm planatim-spatulescentes. *Flores* non vidi, sed secundùm Dom. Bowie, corymbosi, et ut in *Cinerariâ.*

*Obs. Cinerariam cacalioidem* cui forsan simillima appareat proxima.

P.S.—The *Epiphyllum truncatum Nob. Sup. Pl. Succ. p.* 85. has, since that publication appeared, been figured on *t.* 696. of the *Botanical Register;* and my friend Mr. Hood, about the end of November last sent me, from his fine collection at South Lambeth, a specimen, in bloom, of this beautiful plant. And the following is a description of its remarkable flower:

*Flos* in medio apicis ramulorum terminalis, solitarius triuncialis subincurvulus elegantissimus, tubo cum germine subrectangulatim curvato. *Germen* nudum virescens truncatim semunciale, subpyriforme, utroque carinulato, seu lineâ protuberante. *Corollæ* tubus proprius satis crassus roseus nitens, distanter petalino-squamosus; et basi, calyculatus cum *foliolis* reflexis oblongo-ovatis omninò corallinis coccineis, sed infernè violascentibus. *Petala propria*, numerosa, ringenti-reflexa, calycinis foliolis valdè similia. *Stamina* numerosa collecta capillacea nitentia alba, apicem versus incurvula, petalisque parùm humiliora. *Antheræ* (in nostro exemplo) elegantes stramineæ, sed exiguæ; et (fortassè per Novembris frigorem) sine fœcundante farinâ.

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## XXX. *On the supposed Subsidence of the German Ocean.* *By* A Correspondent.

HAVING only just met with No. 9, of Vol. II., for last September, of the Philosophical Magazine, it may be useless, probably, (from the disputed point having been settled,) to recommend that the following experiment should be tried, in order to decide the hypothesis of Mr. Robberds, on the former level of the German Ocean, viz.: A surveyor should be employed to take an *exact* level from high water mark of the present day—not the very highest tides, but a mean spot of spring-tides—up to the furthest spot recorded in Domesday, where a salina was situated, and then measure *down* to the marine deposition, which, according to Mr. Robberds, is found under an alluvial deposit. One composed of fresh

fresh water and marine shells intermixed. This *deposit** may naturally be allowed to mark the highest tide of those days. And if it is found that the *present* level of high water is below the more ancient one in the valleys, it is proved at once that the ocean has receded at that particular district; but if the levels are found to be nearly the same, it will be proved that the sea has been excluded, and thus set the question completely at rest: at present I lean towards the latter opinion, from circumstances in some degree similar on the Kentish coast near Hythe. The sea was known to have come much nearer the hills formerly, than it now does; but the level of the Military Canal proves that the level of high water has not decreased, and that the change has taken place by the formation, by the ocean, of natural barriers of shingle, over which it very rarely flows, and even then in such small quantities as to be harmless; whilst the lower parts of Romney Marsh are liable to inundations to a great extent whenever a breach is made in Dymchurch, or the other walls: thus adding proof that the sea has not decreased on this coast, but is kept out by natural and artificial boundaries.

It would add to the proofs, if the mean of the *low* water level was also carried on, and the *bottom* of the ancient sandy deposit bored to, and measured.

Sittingbourne, Feb. 4, 1828. J.

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## XXXI. *On the Measurement by Trigonometry of the Heights of the principal Hills in the Vicinity of Dent, Hawes, and Sedbergh, in Yorkshire.* By JOHN NIXON, *Esq.*

[Concluded from p. 95.]

THE vertical angles were measured by the horizon-sector, described in the Philosophical Magazine, vol. lix. p. 130.

At Noughtberry Hill the instrument stood upon a large square board screwed firmly to the tripod of the larger theodolite, and at Pen-y-gent it was placed upon the well-built wall crossing the summit of the hill; but at the other stations a flag firmly fixed upon the signal-tower reduced to an altitude of about four feet, formed the rude yet incomparable stand.

Formerly the minute error of adjustment of the levels, which was found to vary with the temperature, was ascertained when on the point of setting out for the station, and a register of the thermometer kept contemporary with the observations. To this plan two objections existed: the error of adjustment might

* I mean the upper surface of the one Mr. Robberds mentions, in speaking of the soil of the meadows and marshes; as "the lowest bed of their soil is sand in which marine shells are found."

have

have varied on the route; and the thermometer would rarely indicate the precise temperature of the levels, necessarily exposed on bright days during a pair of observations alternately to the sun and the shade of the instrument. Some time ago the original levels were replaced by two superior ones (by Dollond), with tubes so nearly cylindrical as to require little or no correction for temperature*. To avoid the other source of error, when the instrument had acquired at the station the temperature of the air, the telescope was placed within its Ys at a slight angle of *elevation*†. The left index being then *levelled* and the angle of elevation read off, the telescope was inverted, and the right index similarly levelled and read off. The telescope was subsequently taken out of the Ys, and reversed in position, when the indices were once more levelled and read off. With the levels correctly adjusted, the angle of inclination would be the same by the four readings; or if the sum of the two angles read off before the telescope was reversed, equalled that of the two succeeding measurements, a compensation of error would exist; otherwise one-fourth of their difference would give the mean error of the levels, additive to, or subtractive from angles of elevation, according as the second pair of readings exceeded or fell short of the first pair. Thus at Dod Fell the readings were:

| | | | |
|---|---|---|---|
| Left index and level ... | $3' \ 30''$; | reversed ... | $4' \ 15''$ |
| Right ...................... | 3 45 | ............... | 3 50 |
| Sums .................. | 7 15 | ............... | 8 5 |
| Difference ...... | 0 50 | | |

Hence $12''\cdot5$ ($=\frac{50}{4}$) is the mean error of the levels additive to the elevations. The inclination of the telescope, as given by the left level, is $3' \ 52''\cdot5$; by the right one, $3' \ 47''\cdot5$; of which the minute difference may arise from errors of division and reading off. When the temperature had varied considerably in the course of the observations, the verification of the adjustment was repeated on their termination, and the mean of the two values registered as the proper correction.

In addition to this variable error is the constant one arising from an inequality in the size of the cylindrical rings; the one at the object end of the telescope exceeding the other by a quantity which affects the elevations with an error of at least

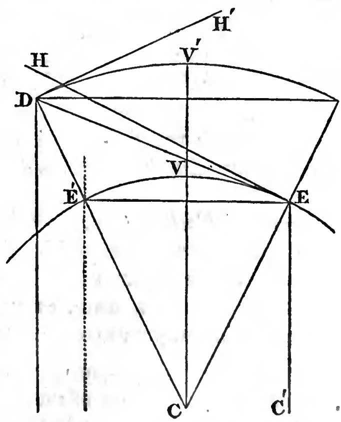
* The bubbles are displaced one-eighth of an inch for a change of inclination of $5''$, the angle to which the graduations (having a radius of about 15 inches) are read off.

† Had there been no risk of mistaking an elevation for a depression in the readings with the telescope reversed, it might have been preferable to have placed the telescope in a *horizontal* position.

$-11''$.

—11″. As the observed refractions come out *negative* for terrestrial arcs not exceeding 5′, it is to be suspected that this error, especially as it was determined by a method liable to some objection, may have been undervalued. In the beginning of the year, the graduated arcs (which had become bent from improper packing) were repaired, and the divisions renewed. At the same time, fine cross lines were substituted for the delicate filament extended diagonally from the original vertical wire to the horizontal one; an alteration which may account in part for the refractions alluded to appearing negative;—the horizontal line being probably depressed, in order to avoid the other extreme, some little *below* the summit of the observed hill.

With regard to the adjustment of the cross lines, although the line of collimation should in strictness be parallel to the plane of the graduated arcs, as well as to the axis of the tubes of the levels, it would nevertheless require a very gross error in this respect to affect elevations or depressions not exceeding 3°. As the sector is fitted up with two divided arcs (one on each side of the tube), each furnished with a moveable index carrying a spirit-level, the one used with the telescope erect, and the other with it inverted,—it becomes superfluous to render the line of collimation strictly parallel to the cylindrical rings; for the half sum of the readings by the two indices, when corrected for the error of the levels and cylindrical rings, is evidently the true angle. The transverse levels were adjusted by bringing their bubbles to the marks (by means of the nuts), whilst the planes of the arcs appeared to be parallel to a fine plumb-line suspended nearly in contact with them.




As a preface to the register of the observations it may be useful to point out the requisite corrections, and allude to the errors and uncertainties to which they are liable.

Admitting the earth to be a sphere, the difference of altitude or level of any two points on its surface ED will be equal to their difference of distance fom C the centre of the earth, where the vertical of D meets that of E. Through E, perpendicular to its vertical EC, draw the horizontal line EH; and perpendicular

pendicular to DC, draw through D the corresponding horizontal line DH′. The angle formed at E or D by the meeting of a straight line passing through any point, and a horizontal line situated in the same vertical plane, is termed the elevation or depression of that point, according as it lies above or below the horizontal line: thus the angles HED, H′DE are both depressions. To find by trigonometry the difference of altitude of DE, no more data are required than the lineal distance EE′ or DD′, and the depressions HED, H′DE; or, simply, either of them, provided we have also given the terrestrial arc E′VE, or angle E′CE. Through C draw a line perpendicular to the chord of equal altitude or level E′E, which line, from the properties of the circle, will bisect the contained arc (at V). Make EC′ parallel to VC, whence VCE or half the arc will be equal to CEC′. As HEC and E′EC are both right angles, the horizontal line EH will be elevated above the chord of level E′E by the angle HEE′ = CEC′= half arc. It is also similarly demonstrable that the horizontal line DH′ must be elevated above the chord of level DD′ by the half arc DCV′=VCE, and that the angle D′DE, as the chords DD′, EE′ are parallel, will be equal to DEE′ the *angular* difference of level. The depression at the upper station will consequently *exceed* the angular difference of level by the half arc, and the elevation at the lower station will be equally in *defect;* whence the depression must exceed the elevation by the contained arc, and half their sum will be equal to the angle DEE′. When both angles are depressions, that at the upper station (or larger one) is the half arc *plus* DEE′, and the other is the half arc *minus* DEE′, so that their sum equals that of the half arcs, and half their difference the angle DEE′. With this angle, the distance EE′, and the angle E′DE (=90° plus half arc*), we get the side E′D or difference of altitude. When one only of the angles is given, the difference of the half arc and the *depression* (considered as an elevation when the former exceeds the latter), or the *elevation* increased by the half arc, will give the angular difference of level†.

*Error of Collimation.*—When the cylindrical rings of a telescopic-level are unequal in diameter, the line of collimation, supposed to be horizontal, describes, in a revolution in azimuth, the sides of a cone, erect or inverted, according as the line passes below or above the true horizon to which the axis

* In the calculations, E′DE has been considered as a right angle.

† In latitude 54° the log. of the mean value of the half arc in *seconds* may be found by subtracting the constant logarithm 2·308227 from the log. of the distance in feet.

of

of the cone will be perpendicular. This constant error might be ascertained, were there no refraction, by reciprocal observations of the elevations and depressions. Let $d\,e$ denote the true depression and elevation; $d'\,e'$, their values affected by the error of collimation $y$, and $a$ the contained arc; then, as it has been shown that $(e + a) = d$, consequently $\frac{(e' + a) - d'}{2} = \frac{(e + a + y) - (d - y)}{2}$ will be equal to the error of collimation, to be *added* to the observed depressions, and $\frac{d' - (e' + a)}{2}$ the error subtractive. When both angles $d\,D$ are depressions, their sum, it has been demonstrated, will equal the contained arc; hence $\frac{(d' + D') - a}{2}$, or $\frac{a - (d' + D')}{2}$ will give the error of collimation; the depressions being observed in excess in the former case, and in defect in the latter.

When the line of gravity, disturbed by local attraction, is not in the direction of the centre of the earth, the line of collimation supposed to be horizontal, describes in its revolution a plane inclined to the true horizon at an angle equal to the deflection, and cutting it at right angles to the direction of the disturbing cause. The elevations must therefore be in defect in the direction of the attraction, and in excess in the opposite quarter of the horizon by a quantity equal to the inclination of the planes to each other; but in the direction of their intersection the error of collimation will be null. When the deflection ($g$) as measured on the vertical plane passing through two stations, is the same at both and in one direction (or parallel), the elevation and corresponding depression will be equally in excess or defect, and $\frac{(d + g) + (e + g)}{2}$ will exceed $\frac{d + e}{2}$ by $g$; but should the deflections be in opposite directions (or inclined to each other), a compensation of error takes place, $\frac{(d + g) + (e - g)}{2}$ being equal to $\frac{d + e}{2}$. Admitting the line of gravity to be disturbed at one station only, $\frac{(d + g) + e}{2}$ will differ from $\frac{d + e}{2}$ by $\frac{g}{2}$.

*Refraction.*—In the general state of the atmosphere the elevations may be considered as increased, and the depressions equally diminished by a constant ratio of the contained arc ($a$). In this case, if $d$ and $e$ represent, as before, the true depression and elevation, their values $d'$ and $e'$ as affected by the (positive) refraction $r$, will be $d - r$ and $e + r$. Hence $\frac{(e' + a) - d'}{2}$, or $\frac{a - (d' + D')}{2}$, will be the refraction*; but the angular difference of level may be obtained without previously ascertaining the refraction or having given the contained arc, DEE′ being equal to $\frac{e' + d'}{2}$, or to $\frac{d' - D'}{2}$.

When the instrument, from some imperfection in its construction, gives the elevations in excess or defect by an unknown but constant quantity $y$, then will $e'$ be equal, in the former case, to $(e + y + r)$, and $d'$ to $(d - y - r)$; but in the latter case we shall have $e' = (e - y + r)$ and $d' = (d + y - r)$; whence $\frac{d' + e'}{2}$, or $\frac{D' - d'}{2}$, will be the correct angular difference of level†, and $\frac{(a + e') \backsim d'}{2}$, or $\frac{a \backsim (d' + D')}{2}$, the refraction *plus*, or *minus* the constant error of the instrument, according as the elevations are in excess or defect‡. Were the refraction a constant ratio of the arc, its value, unaffected by the instrumental error, might be determined from reciprocal observation on arcs differing in extent. Let $(r \pm y)$ be the observed refraction for the arc $a$, and $(r' \pm y)$ that for the (much) greater arc $a'$; then $(r' \pm y) - (r \pm y)$ will be the true refraction for the arc $(a' - a)$.

Unfortunately for the accuracy of trigonometrical measurements, it will frequently occur, especially on low grounds with a cloudless sky in the spring, that a thin stratum of air in con-

* When the observations are not reduced to the ground, the refraction, calling $h$ the angle subtended by the *sum* of the heights of the eye at the two stations, will be $\frac{(h + a + e') - d'}{2}$, or $\frac{(h + a) - (d' + D')}{2}$.

† When the height of the eye is the same at both stations $\frac{d' + e'}{2}$, or $\frac{d' - D'}{2}$, will still give at once the angular difference of level $l$; for

$$\left.\begin{array}{ll} & l + \frac{a}{2} - r + y + \frac{h}{2} = d' \\ \text{and} & l - \frac{a}{2} + r - y - \frac{h}{2} = e' \end{array}\right\} \text{whence } d' + e' = 2l;$$

$$\text{or} \quad \frac{a}{2} + r - y - \frac{h}{2} - l = D'; \text{ whence } d' - D' = 2l.$$

‡ When $y$ is known, and the observations are not reduced to the ground, the refraction will be equal to

$$\frac{(a + h + e' \pm 2y) - d'}{2}, \text{ or to } \frac{(a + h \pm 2y) - (d' + D')}{2}.$$

tact

tact with the ground at the station is rarer in the heat of the day than the stratum immediately above it, and denser in the morning and evening, than comports with the natural constitution of the atmosphere. The refraction varies in consequence from $\frac{a}{n} + x$ in the morning to $\frac{a}{n} - x$, its amount about noon, and reverts in the evening to the first expression of its value. Hence the refraction on similar days is perpetually changing, and becomes a constant ratio of the arc $\left(\frac{a}{n}\right)$ only twice in the day; viz. in the middle of the forenoon and afternoon, when $x$ or the maximum increment or decrement of the regular refraction resulting from the passage of the ray through the disturbed stratum of air, becomes 0. In such a condition of the atmosphere it would evidently be impossible to ascertain the correct angle for calculation from even *contemporary* reciprocal observations, unless $x$ and its horary variations should have the same value at the same time at both stations.

On a careful consideration of the errors to which the determination of the angular difference of level is liable, we cannot but admit that the least objectionable method of measuring the difference of altitude of two hills is to make the requisite observations at an *intermediate* station. When the latter is equidistant from the former, so complete a compensation of errors (that arising from local attraction excepted) is effected, that it becomes superfluous to be acquainted with the error of collimation (from whatever cause arising), or even the refraction, although variable to a small amount, provided the two observations were nearly contemporary. But if we suppose the two hills AB, and the intermediate station S, to be in a line and equal in altitude, and admit the plumb-line to be drawn 10″ towards A, then shall we make A lower, and B higher than S by about $\frac{\tan. 10'' \times \text{distance}}{\text{radius}}$, and B will be calculated to be higher than A by twice this quantity. As it may frequently be impracticable to have an equidistant station, we must select two (or more) places ST, so situated that the distances SA + TA may be nearly equal to SB + TB. In this latter case the error of collimation and the refraction should certainly be known approximatively; but great errors may be committed in this respect without materially affecting the *difference* of altitude of the two hills. When SA is more nearly equal to SB than TA is to TB, the difference of level is probably more correctly determined from S than from T, unless SA + SB should greatly exceed TA + TB. Hence the following correction:

 Let

Let $f$ denote the reciprocal of $\left(\frac{SA+SB}{2} + SA \backsim SB\right)$, and $g$ the reciprocal of $\left(\frac{TA+TB}{2} + TA \backsim TB\right)$: then if $l\,l'$ represent the difference of altitude calculated from the observations at S and T, the correct value may be considered as equal to $\frac{lf+l'g}{f+g}$. The heights of two or more hills having been similarly determined, it may be useful, in the event of being stationed on one of them, to observe the elevation or depression of the others, with a view to learn the actual refraction. On such occasions it is requisite to have given the instrumental and adjustment errors. Adopting this method, the following refractions were obtained:

| | Arc. | Ref$^n$. |
|---|---|---|
| Gt. Whernside as observed from Pen-y-gent . | 8′ 50″ | + 4″·5 |
| ——————— Dod Fell . . | 10 24 | +25 |
| ——————— Noughtberry | 13 34 | +28 |
| Pendle Hill——————— Noughtberry | 24 42 | + 1·30 |

The refractions deduced from the reciprocal observations are given below:

| | Arc. | Ref$^n$. |
|---|---|---|
| Between Whaw Fell and Noughtberry . . . . . | 1′ 14″ | + 6″ |
| Dod Fell and Noughtberry . . . . . | 3 10 | + 9 |
| Whaw Fell and Whernside . . . . . | 3 21 | − 18 |
| Ingleborough and Whernside . . . . | 3 41 | − 9 |
| Noughtberry and Whernside . . . . . | 4 6 | + 2 |
| Ingleborough and Pen-y-gent . . . . . | 5 16 | − 6 |
| Dod Fell and Whernside . . . . . . | 5 45 | + 4 |
| Pen-y-gent and Whernside . . . . . . | 6 54 | + 11 |
| Noughtberry and Ingleborough . . . | 7 14 | + 3·5 |
| Dod Fell and Ingleborough . . . . . . | 7 31 | − 7·5 |
| Noughtberry and Pen-y-gent . . . . | 7 53 | + 16·5 |

Mean arc 5·″6; Mean refr. + 1″.

As the refraction resulting from observations made some years ago by the same instrument on numerous arcs, several of which were of considerable extent, appeared to be about 1-18th of the arc, the refraction adopted in the calculations, when not determinable from reciprocal observations, has been 1-18th *minus* the constant quantity 18″. In addition to the elevations being observed in defect after the substitution of the cross lines for the filament, two other causes may be assigned for the smallness of the refraction;—the ray, as the hills were lofty and differed little in elevation, would be confined to a stratum of rare air; and the observations were made (a little too early in the year) about the warmest part of the day, when

when the variable part of the refraction is at its *negative* maximum*. The refraction between Noughtberry Hill and the much lower hill Pendle, comes out, it is true, very considerable (1-16·5); but as the day on which the observations were made was of extraordinary clearness, which is generally attributed to the rate of decrement of temperature in the atmosphere being much less than wanted, the refraction would in consequence exceed its mean value.

The register of the observations will require explanation.—The *first* column gives the time at each observation; the *second*, the name of the hill of which the ground at the summit (Pen-y-gent excepted) was observed; the *third*, its elevation E or depression D as read off on the left index; the *fourth*, the half difference of the corresponding reading by the right index ± the constant error of the instrument and the variable one of the levels, forming together what is termed the error of collimation, additive to, or subtractive from the given reading according to its prefixed sign. (Were the graduations perfect and the adjustments of the cross lines and levels unaffected by variation of temperature, the error of collimation should be a constant quantity.) The *last* column contains the difference of level of the ground at the station (Pen-y-gent excepted) and the summit of the observed hill, the latter being higher or lower than the station according as the difference is marked H or L.

*At Whaw Fell.*

Height of Eye 3·5 feet.

| | | | | | Feet. |
|---|---|---|---|---|---|
| Sept. 15, | 11h 0m | Noughtberry Hill | 2° 46′ 20″ E. | −19″ | 372·6H. |
| 1824. | 11 5 | Lovely Seat | 0 25 40 | −11·5 | 377·1 |
| | 11 10 | Ten End | 0 12 5 | − 9 | 82·4 |
| | 11 20 | Bear's Head | 0 20 0 | −11·5 | 184·0 |
| | 11 30 | Dod Fell | 1 11 25 | − 4 | 356·8 |
| | 11 40 | Ingleborough | 0 46 32 | −17·5 | 537·9 |
| | 13 0 | Ingleborough | 0 46 30 | −16·5 | |
| Mar. 29, | 15 25 | Ingleborough | 0 45 45 | +16 | |
| 1825. | 15 50 | Ingleborough | 0 45 50 | +11 | |
| | 15 10 | Bow Fell | 0 45 30 | + 9·5 | 388·8 |
| | 15 40 | Bow Fell | 0 45 20 | +21 | |
| Sept. 15, | 11 45 | Shunnor Fell | 0 35 30 | − 9 | 513·7 |
| 1824. | 12 50 | Shunnor Fell | 0 35 25 | − 6·5 | |
| | 12 0 | Whernside | 1 35 20 | −14 | 579·4 |
| | 12 10 | Berkin | 0 10 25 | −16·5 | 165·6 |
| | 13 25 | Pen-y-gent, Wall | 0 34 40 | −14 | 451·2 |

* The mornings were generally slightly frosty.

Height

Height of Eye 3·5 feet.

| | | | | Feet. |
|---|---|---|---|---|
| Sept. 15, 11h 35m | Blea Moor | 0° 29′ 35″ D. | + 6″·5 | 74·8 L. |
| 1824. 15 30 | Snays Fell | 0 25 30 | +11·5 | 51·5 |
| Mar. 29, 15 15 | Ryssell | 0 4 20 | −16 | 10·7 |
| 1825. | | | | |

As the *minute* error of the levels was unregistered, the greater part of the observations serve merely to verify those made at the other stations. The refraction, although the *filament* was made use of, it was found necessary to consider as 1-·18 *minus* 18″.

*At Ingleborough.*

Height of Eye 4 feet.

| | | | | Feet. |
|---|---|---|---|---|
| June 17, 10h 30m | Shunnor Fell | 0° 7′ 10″ D. | +10″ | 27·2 L. |
| 1822. 13 10 | Lovely Seat | 0 12 45 | +15 | 162·8 |
| | (Call the refraction for the above 1-18th.) | | | |
| July 2, | Colm | 0 16 30 | −21·5 | 122·6 |
| 1827. | Berkin | 0 33 30 | −22 | 376·5 |
| | Noughtberry Hill | 0 17 32 | −22 | 170·5 |
| | Dod Fell | 0 18 28 | −22 | 186·2 |
| | Graygreth | 0 49 2 | −22·5 | 314·3 |
| | Blea Moor | 1 18 5 | −25·5 | 614·9 |
| | Pen-y-gent, Wall | 0 13 35 | −37 | 91·7 |
| | Bow Fell (dim) | 0 14 20 | −37 | 148·4 |
| | Whernside | 0 3 20 E. | +22 | 41·2 H. |

*At Whernside.*

Height of Eye 4 feet.

| | | | | Feet. |
|---|---|---|---|---|
| July 4, 12h 55m | Wildboar Fell | 0° 10′ 15″ D. | − 2″ | 92·0 L. |
| 1827. 13 0 | The Calf | 0 16 32·5 | + 6 | 196·9 |
| 13 5 | Bow Fell | 0 22 40 | + 3·5 | } 191·7 |
| July 6. 14 10 | Bow Fell | 0 22 56 | − 8 | |
| 12 20 | Colm | 0 41 35 | + 1 | } 163·3 |
| July 4. 13 10 | Colm | 0 41 22·5 | +12 | |
| 13 50 | Graygreth | 1 10 35 | + 3·5 | } 356·2 |
| July 6. 12 30 | Graygreth | 1 11 10 | −14 | |
| 13 10 | Shunnor | 0 8 50 | −12·5 | } 64·2 |
| 14 40 | Shunnor | 0 8 52 | −10 | |
| July 4. 13 45 | Shunnor | 0 8 30 | + 2 | |
| 13 37 | Noughtberry Hill | 0 30 52·5 | +14·5 | } 209·0 |
| July 6. 12 45 | Noughtberry Hill | 0 31 35 | − 9 | |
| 13 15 | Lovely Seat | 0 15 50 | − 2·5 | 197·6 |
| 12 40 | Dod Fell | 0 25 20 | − 7·5 | 224·7 |
| 12 0 | Pen-y-gent, Wall | 0 14 33 | −15·5 | 131·0 |
| 12 5 | Ingleborough | 0 9 7·5 | −11 | 41·2 |
| 12 35 | Whaw Fell | 1 40 27·5 | − 4 | 582·2 |

*At Noughtberry Hill.*

Height of Eye 4 feet.

| | | | | | Feet. |
|---|---|---|---|---|---|
| July 5, 1827. | 15h 35m | Great Whernside | 0° 2′ 5″ D. | − 1″ | |
| | 15 45 | Pendle Hill | 0 19 48 | −15 | |
| | 15 25 | Dod Fell | 0 5 5 | − 4·5 | 16·1 L. |
| | 16 5 | Berkin | 0 20 50 | 0 | 205·7 |
| | 16 10 | Whaw Fell | 2 50 55 | −15 | 371·6 |
| | 16 13 | Blea Moor | 1 38 45 | + 6 | 445·6 |
| | 15 0 | The Calf | 0 3 22·5 | − 1 | 14·5 H |
| | 15 20 | Lovely Seat | 0 3 7·5 | − 7·5 | 5·7 |
| | 15 4 | Bow Fell | 0 0 43 E. | + 3·5 | 20·5 |
| | 15 8 | Wildboar Fell | 0 6 35 | +13·5 | 119·6 |
| | 16 15 | Wildboar Fell | 0 6 52 | + 3 | |
| | 15 12 | Cotter Fell | 0 5 5 | + 1 | 122·4 |
| | 15 16 | Shunnor Fell | 0 9 6 | +10 | 143·0 |
| | 15 40 | Pen-y-gent, Wall | 0 1 30 | + 1 | 76·6 |
| | 15 50 | Ingleborough | 0 9 20 | + 5 | 170·5 |
| | 15 55 | Whernside | 0 26 25 | + 6 | 209·0 |
| | 16 0 | Colm | 0 2 12·5 | + 6 | 48·4 |

*At Pen-y-gent.*

Height of Eye above the Wall 9 inches.

| | | | | ″ | Feet. |
|---|---|---|---|---|---|
| July 11, 1827. | 11h 35m | Ingleborough | 0° 6′ 52″ E. | + 8·5 | 91·7 H. |
| | 14 50 | Ingleborough | 0 6 50 | + 7 | |
| | 11 55 | Whernside | 0 7 17 | + 5 | 131·0 |
| | 12 30 | Wildboar Fell | 0 4 52·5 D. | − 6 | 47·6 |
| | 14 5 | Wildboar Fell | 0 5 5 | −11 | |
| | 13 40 | Cotter Fell | 0 5 37·5 | −21 | 47·8 |
| | 13 55 | Shunnor Fell | 0 3 12·5 | −12 | 69·1 |
| | 11 50 | Colm | 0 6 20 | − 4·5 | 30·0 L. |
| | 12 10 | Bow Fell | 0 8 12 | −12 | 53·7 |
| | 12 16 | Noughtberry Hill | 0 9 6 | + 6 | 76·6 |
| | 12 35 | Blea Moor | 0 51 27·5 | − 8 | 520·2 |
| | 12 45 | The Calf | 0 9 12·5 | − 1 | 53·5 |
| | 14 55 | The Calf | 0 9 25 | − 9·5 | |
| | 13 45 | Lovely Seat | 0 8 52·5 | − 8 | 65·9 |
| | 14 35 | Penhill (Wensleydale) | 0 27 5 | −13 | |
| | 14 30 | Great Whernside | 0 2 43 | −11 | |
| | 14 40 | Great Whernside | 0 2 48 | −15 | |

*At*

*At Dod Fell.*

Height of Eye 4 feet.

| | | | | | Feet. |
|---|---|---|---|---|---|
| Aug. 25, | 15h 35m | Great Whernside | 0° 1′ 15″ E. | +16″ | |
| 1827. | 13 40 | Wildboar Fell | 0 3 55 | +20 | 139·0 H. |
| | 13 55 | Cotter Fell | 0 3 40 | +20 | 142·6 |
| | 14 5 | Shunnor Fell | 0 9 10 | +22 | 162·1 |
| | 14 25 | Noughtberry Hill | 0 0 20 | +22·5 | 16·1 |
| | 14 40 | Colm | 0 0 40 | +18·5 | 66·4 |
| | 14 45 | Whernside | 0 18 28 | +20·5 | 224·7 |
| | 15 5 | Ingleborough | 0 9 15 | +26 | 186·2 |
| | 15 20 | Pen-y-gent, Wall | 0 5 15 | +23·5 | 95·0 |
| | 14 15 | Lovely Seat | 0 1 25 D. | −23·5 | 24·9 |
| | 14 20 | Bow Fell | 0 0 35 | −22 | 39·5 |
| | 14 35 | Berkin | 0 16 7 | −20·5 | 186·2 L. |
| | 15 10 | Blea Moor | 1 6 40 | −21 | 429·2 |
| | 15 30 | Penhill (Wensleydale) | 0 25 0 | −16 | |

*Calculation of the* mean *differences of Level of the Stations.*

*Whernside above Ingleborough.*

| | Feet. |
|---|---|
| By reciprocal observation | 41·2 |
| By obs. from Pen-y-gent | 39·3 |
| ——— Noughtberry | 38·5 |
| ——— Dod | 38·5 |
| Ar. mean 39·4; cor. mean | 39·8 |

*Ingleborough above Pen-y-gent, Wall.*

| | |
|---|---|
| By reciprocal obs. | 91·7 |
| By obs. from Whernside | 89·8 |
| ——— Noughtberry | 93·9 |
| ——— Dod | 91·2 |
| Arithm. and corr. mean | 91·7 |

*Ingleborough above Noughtberry Hill.*

| | |
|---|---|
| By reciprocal obs. | 170·5 |
| By obs. from Whernside | 167·8 |
| ——— Pen-y-gent | 168·3 |
| ——— Dod Fell | 170·1 |
| Ar. mean 169·2; cor. mean | 168·9 |

*Ingleborough above Dod Fell.*

| | Feet. |
|---|---|
| By reciprocal obs. | 186·2 |
| By obs. from Whernside | 183·5 |
| ——— Noughtberry | 186·6 |
| ——— Pen-y-gent | 186·7 |
| Ar. mean 185·8; cor. mean | 185·7 |

*Whernside above Pen-y-gent.*

| | |
|---|---|
| By reciprocal obs. | 131·0 |
| By obs. from Inglebro'. | 132·9 |
| ——— Dod Fell | 129·7 |
| ——— Noughtberry | 132·4 |
| Ar. mean 131·5; cor. mean | 131·4 |

*Whernside above Noughtberry Hill.*

| | |
|---|---|
| By reciprocal obs. | 209·0 |
| By obs. from Inglebro' | 211·7 |
| ——— Pen-y-gent | 207·6 |
| ——— Dod Fell | 208·6 |
| Ar. mean 209·2; cor. mean | 209·1 |

*Whernside*

*Whernside above Dod Fell.*

| | Feet. |
|---|---|
| By reciprocal observation | 224·7 |
| By obs. from Inglebro' | 227·4 |
| ——— Noughtberry | 225·1 |
| ——— Pen-y-gent | 226·0 |

Ar. mean 225·8; cor. mean 225·6

*Noughtberry Hill above Dod Fell.*

| | Feet. |
|---|---|
| By reciprocal observation | 16·1 |
| By obs. from Ingleborough | 15·7 |
| ——— Whernside | 15·7 |
| ——— Pen-y-gent | 18·4 |

Ar. mean 16·5; cor. mean 16·3

*Pen-y-gent, Wall, above Dod Fell.*

| | |
|---|---|
| By obs. from Dod Fell | 95·0 |
| ——— Ingleborough | 94·5 |
| ——— Whernside | 93·7 |
| ——— Noughtberry | 92·7 |

Ar. mean 94·0; cor. mean 94·1

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*Heights of the Stations above the Irish Sea at low water.*

| | Feet. | Feet. |
|---|---|---|
| Ingleborough* ......... | 2374·6 | |
| Whernside ............. | 2414·4 | |
| Pen-y-gent, Wall...... | 2283·0 | (ground 2277·0). |
| Noughtberry Hill...... | 2205·5 | |
| Dod Fell†............... | 2188·9 | |

*Calculation of the Height of Whaw Fell.*

| | Feet. | Feet. |
|---|---|---|
| By obs. from Noughtberry Hill ... | 1833·9 | |
| ——— Whernside ............ | 1832·2 | |
| | ——— Cor. mean | 1833·6 |
| By obs. at Whaw Fell of Whernside | 1835·0 | |
| ——— Inglebro' | 1836·7 | |
| ——— Pent-y-gent | 1831·8 | |
| ——— Noughtberry | 1832·9 | |
| ——— Dod Fell | 1832·1 | |
| | ——— Cor. mean | 1833·4 |

Height of Whaw Fell........................................ 1833·5

In the following table are given for each hill its height as determined from the stations at the head of the column;—the claims to accuracy of the different values of the altitude being considered in the calculation of the *mean* to be *reciprocally* as

* See Phil. Mag. for 1823.

† Observations made some years from hills chiefly in Wharfdale, gave, Whernside 2412; Pen-y-gent, Wall 2281·4; Dod Fell 2189·8 feet.

 the

the distance of the station to the hill. The last column contains the heights deduced from the observations at Whaw Fell.

| Hills. | Height by Observation from Whernside. | Ingleborough. | Pen-y-gent. | Noughtberry. | Dod Fell. | Corrected Mean Height. | Height by Observations from Whaw Fell. |
|---|---|---|---|---|---|---|---|
| | Feet. | Feet. | Feet. | Feet. | Feet. | Feet. | Feet. |
| Shunnor Fell | 2350·2 | 2347·4 | 2352·1 | 2348·5 | 2351·0 | 2349·8 | 2347·2 |
| Cotter Fell ... | ...... | ...... | 2330·8 | 2327·9 | 2331·5 | 2329·7 | ...... |
| Wildboar Fell | 2322·4 | ...... | 2330·6 | 2325·1 | 2329·0 | 2326·3 | ...... |
| Colm .......... | 2251·1 | 2252·0 | 2253·0 | 2253·9 | 2255·3 | 2252·4 | ...... |
| Bow Fell ...... | 2222·7 | 2226·2 | 2229.3 | 2226·0 | 2228·4 | 2226·1 | 2222·3 |
| The Calf...... | 2217·5 | ...... | 2229·6 | 2220·0 | ...... | 2221·3 | ...... |
| Lovely Seat... | 2216·8 | 2211·8 | 2217·1 | 2211·2 | 2213·8 | 2214·0 | 2210·6 |
| Graygreth ... | 2058·2 | 2060·3 | ...... | ...... | ...... | 2059·2 | ...... |
| Bear's Head | ...... | ...... | ...... | ...... | ...... | ...... | 2017·5 |
| Berkin......... | ...... | 1998·1 | ...... | 1999·8 | 2002·7 | 2000·0 | 1999·1 |
| Ten End ...... | ...... | ...... | ...... | ...... | ...... | ...... | 1915·9 |
| Ryssell........ | ...... | ...... | ...... | ...... | ...... | ...... | 1822·8 |
| Snays Fell ... | ...... | ...... | ...... | ...... | ...... | ...... | 1782·0 |
| Blea Moor ... | ...... | 1759·7 | 1762·8 | 1759·9 | 1759·7 | 1760·3 | 1758·7 |
| Mean error... | −1·5 | −0·9 | +3·2 | −0·8 | +1·6 | | −2·7 |
| Mean distance | 43,892 | 49,632 | 73,462 | 36,051 | 44,745 | | 32,930 |

At the Bear's Head the apparent elevation of Dod Fell, measured by the 4-inch theodolite, was 43′ 18″, whence the altitude of the former would be 2021 feet. At Ten End the Bear's Head was elevated 40′ 9″, giving 1911 feet for the height of Ten End. The heights of Ryssell and Snays Fell are as yet unconfirmed.

When the mean error of a number of heights determined from any particular station is considerable, it would appear to argue that the altitude of the station had been assumed in excess or defect according to the *sign* and by the *amount* of the error. But if the mean distance of the several hills to the station should greatly exceed the mean of their distances to the other stations, the error, as in the case of Pen-y-gent, may with greater probability be attributed to a false estimate of the refraction. Had we made use of $\frac{a}{18}$ *minus* a smaller quantity than 18″, the discrepancies would have disappeared.

On

On another occasion I shall beg to add the results of a few barometrical measurements, and furnish some geological notices.

Leeds, Dec. 26, 1827. JOHN NIXON.

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## XXXII. *On an Apparent Violation of the Law of Continuity.* *By* P. M. ROGET, *M.D. F.R.S.**

[Concluded from p. 121.]

THE severest test to which the validity of this mode of reasoning can be put, is to apply it to the purely mathematical relations of space and quantity, to which, if the law of continuity have any necessary existence in the nature of things, it ought to apply with the greatest rigour. We find, accordingly, that all the changes of magnitude in those quantities of which the value is dependent on that of certain other quantities, accompany corresponding changes in these latter quantities in a manner strictly conformable with the law of continuity. The same exact correspondence also obtains with regard to the geometric relations of space, such as lines, angles, surfaces and volumes. Thus, all the trigonometrical lines which are functions of the arcs of circles, undergo regular and continuous changes, corresponding to the regular and continuous increase or diminution of the arcs to which they are related. Some, as the sine and cosine, pass from every intermediate magnitude between nothing and a certain limit, which they do not exceed, but to which, when the arc increases uniformly, their last approximations are made with extreme slowness. Arrived at that limit, they again begin to decrease; but at first with extreme slowness, like bodies that are set in motion, from a state of rest, by the action of a moving force. Their progress of diminution is by degrees accelerated; and is again retarded only when they are preparing, as it were, to undergo some other change. By the time they arrive at any of their states, either of maximum or minimum, their rate or velocities of increase or diminution have gradually been extinguished, and reduced to nothing. Some of these lines, as the tangent for example, have no li-

* The former portion of this article was inadvertently reprinted in our last Number, without having been previously submitted to the author; in consequence of which, several inaccuracies in the original (now noticed in p. 206,) were left uncorrected; and the division having been made in an improper place, the last paragraph of the former portion is here repeated.—EDIT.

mits

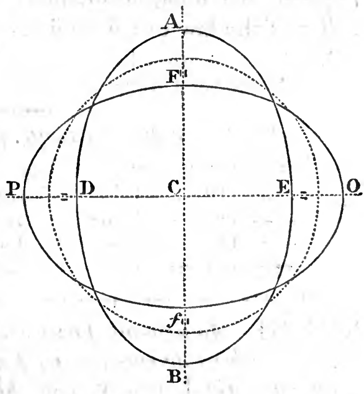
mits to their increase of magnitude, and pass through every possible linear dimension from nothing to infinity, while estimated in one certain direction, characterized by the positive sign. From positive infinity, the transition is made at once to negative infinity, and the subsequent changes consequent upon the further enlargement of the arc, consist in the gradual diminution of the negative tangent; that is, of the line estimated in a direction opposite to that which was assumed as the measure of the original, or positive tangent. This negative tangent passes again, by the continuation of the same process, into the primitive tangent, after having been again reduced to nothing.

By considering, in like manner, the generation and the course of curves of every kind, whether produced by the intersection of lines, moving according to certain laws, or of points whose motions are limited to certain conditions with relation to other lines or curves, we shall find that the same law of continuity obtains, both with respect to the changes of magnitude and the changes of direction. No mathematical quantity changes from positive to negative, or from negative to positive, without either becoming infinite, or being reduced to nothing; and it passes through every possible intermediate degree of magnitude, in its progress from the one to the other. The change from increase to diminution, or the reverse, is never abruptly made; but is always effected by a transition through a condition of momentary quiescence, which is the limit of opposite kinds of changes, and at which instant no actual change takes place.

Such being the universal law which is observed by lines generated according to any given geometric conditions, we should naturally be led, from analogy, to expect that the motions of points, having certain geometric relations to given curves, would also be governed by the same law. Let us take, for example, a small portion of any curve. This portion will have, what is termed, a certain radius of curvature, and the furthest extremity of this radius will be the centre of curvature. In proportion as the curvature is diminished, this radius will increase in length; and the centre of curvature will be removed to a greater distance. Its motion during this gradual change will be along a right line, perpendicular to the tangent of the curve at the point of contact. As the curve in the change we are supposing it to undergo, approaches more and more to a straight line, the centre will recede with great rapidity; till at length, when all curvature is lost, it may be regarded as removed to an infinite distance; and at this

this precise moment it is indifferent whether we consider it as existing on the one side or the other of the tangent, with which the curve has now coalesced. But we may suppose the same power, whatever it be, which has effected this gradual change of curvature, to continue its operation still further. A curvature in the opposite direction will now take place, and the centre of this curvature will be found on the opposite side to that in which it had at first been situated. Its motion, in proportion as the curve continues to follow the same uniform progress of change, will approach the line from a distance infinitely negative. All these successive changes of position in the centre of curvature are perfectly similar to those of the extremity of the tangent, during the increase of the arc of a circle from nothing to the entire circumference of the circle; and are, in both cases, perfectly accordant with the law of geometric continuity.

A case, however, has occurred to me, in which this apparently universal and necessary law seems to be directly violated. Let the ellipse ADBE be the curve which is to be subjected to the operation of the cause, tending, as in the former instance, to diminish its curvature; and let us trace the successive changes which will thereby be occasioned in the position of the two foci, F, *f*. A compressing force, for example, is applied in the direction of the transverse diameter, AC, BC; or, what will produce the same effect, a stretching force is applied in the direction of the conjugate, or shortest diameter, CD, CE. By the action of either of these forces, the form of the ellipse will be brought nearer to that of a circle, till it at length becomes a perfect circle, represented by the dotted circumference. But it assumes this form only for an instant, being merely the passage to another series of ellipses, which, taking their rise from this circle, at first differ but insensibly from it; but afterwards gradually assume every possible degree of compression, until they are ultimately reduced to a straight line at right angles with the transverse diameter of the first set of ellipses. The former represent circles that are elongated, the latter circles that are compressed in



in one particular direction; the two pass into each other by a continuous series, of which the perfect circle is merely a term of transition.

Let us now attend to the changes of position in the foci, corresponding to these transitions. During the gradual change of the first ellipse into the circle, the foci F, *f*, move along the diameter AB, approach nearer and nearer to the centre of the figure C, until they both coalesce in the centre. In the next series of changes, we find them again separating from each other; but instead of moving in the same line as before, we observe them starting off, each in a new direction, CP and CO, precisely at right angles to their former lines of motion, AC and BC; and this change of direction is made abruptly, *per saltum*, and by an angular motion. Are not these changes direct violations of the law of continuity?

As the question which I have now proposed may be thought deserving the attention of such of your philosophical readers as are stimulated by the appearance of paradox to renewed investigation; and as the attempts to solve it may give occasion to the exercise of ingenuity, I shall abstain from suggesting the considerations which might, perhaps, enable us to explain this apparent anomaly, consistently with the universality of the law of continuity.

Bernard-street, Russell-square, November 1, 1825. P. M. Roget.

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*Errata in the last Number.*

Page 118 line 23, *for* "operations," *read* "operation."
—— 118 — 23, *for* "greatest," *read* "gentlest."
—— 119 — 3, *for* "to," *read* "with."
—— 119 — 32, *for* "rectilineous," *read* "rectilineal."
—— 120 — 26, *for* "course pending," *read* "corresponding."

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## XXXIII. *Additional Discussion respecting the Ellipticity of the Earth as determined by Experiments made with the Pendulum. By* J. Ivory, *Esq. M.A. F.R.S.**

IN deducing the figure of the earth from the observed lengths of the pendulum, I have always thought it necessary to leave out a few of the experiments that were inconsistent with the rest. The inconsistency is proved by comparing the pendulums on the same parallel, or nearly on the same parallel; in which latter case a small correction must be made for the difference of latitude. It is evident that, if the

* Communicated by the Author.

pendulums

pendulums when thus compared are excessively irregular, we never can deduce from them, by any mode of investigation whatever, any conclusion respecting the figure of the earth, in which much confidence can be placed. On this ground I always rejected such of the experiments as were irreconcileable with the rest; and I thought this proceeding the more justifiable, both because the experiments set aside were always very few in comparison of the whole number, and because those that remained were brought within the compass of inequality to be expected in experimental quantities. The results obtained on this principle, are already before the public, and therefore need not be particularly mentioned. My present intention is to solve the problem, by taking in all the 40 experiments we at present possess, in the expectation, that by this means some light will be thrown on this subject, although my former mode of solution still appears to be founded on sound principles.

In order to abridge, I shall begin with laying down the general formulas of the mode of solution I follow. Put $39+\delta$ for the pendulum in English inches at any station; $\lambda$, for the latitude; $39+\Delta$, for the equatorial pendulum; and $f$, for the difference between the pendulums at the pole and the equator: then $$\Delta + f \sin^2 \lambda = \delta.$$

Further, let
$$G = \Sigma\,(\delta),$$
$$H = \Sigma\,(\sin^2 \lambda),$$
$$K = \Sigma\,(\sin^4 \lambda),$$
the sums being extended to all the stations of which the number is $n$: then, by taking the sum of all the like equations at the $n$ stations, we get,

$$\Delta + \frac{H}{n} f = \frac{G}{n}. \qquad \text{(A)}$$

Next, $a$ being an approximate value of $\Delta$, find an approximate value of $f$, namely $b$, by substituting $a$ for $\Delta$ in Equat. (A); or, if $b$ be an approximate value of $f$, find, by means of the same equation, a corresponding value of $\Delta$, namely $a$: then, if we put,
$$\Delta = a + s$$
$$f = b + \tau,$$
we shall have,
$$s + \frac{H}{n} \tau = 0. \qquad \text{(B)}$$

Again: Multiply all the terms of the first equation by the coefficient of $f$: then
$$\Delta \sin^2 \lambda + f \sin^4 \lambda = \delta \sin^2 \lambda;$$
and, by substituting the values of $\Delta$ and $f$,
$$s \sin^2 \lambda + \tau \sin^4 \lambda = \sin^2 \lambda\,(\delta - a - b \sin^2 \lambda);$$
and,

and, by adding the like equations at all the stations,

$$\mathrm{H}s + \mathrm{K}\tau = \Sigma\left(\sin^2\lambda\,(\delta - a - b\sin^2\lambda)\right);$$

and, by exterminating $s$,

$$\left(\mathrm{K} - \frac{\mathrm{H}^2}{n}\right)\tau = \Sigma\left(\sin^2\lambda\,(\delta - a - b\sin^2\lambda)\right). \quad \text{(C)}$$

If $\sin^2\lambda$ had exactly the same value at all the stations, the coefficient of $\tau$ in this last formula would be evanescent. In reality this can never be the case: but if all the stations were in higher latitudes than 45°, the coefficient would be so small that a minute variation in the sum on the right-hand side, would produce a great alteration in the values of $s$ and $\tau$; and, considering the uncertainty of the experimental errors, the problem would be in a great degree indeterminate. Were we in possession of no other experiments than those north of Formentera, the figure of the earth would be very ill determined; because all the pendulums might be sufficiently well represented at the same time that the equatorial pendulum is made to undergo considerable variations. The practical solution of this problem requires many accurate experiments between the tropics for finding the exact length of the equatorial pendulum; and likewise many experiments from the tropics to the poles for ascertaining the value of the other element $f$ with the requisite precision.

Now taking all the 40 experiments in the table of my former paper, I have found,

$$\mathrm{G} = 3{\cdot}91529$$
$$\mathrm{H} = 16{\cdot}25069$$
$$\mathrm{K} = 10{\cdot}47158:$$

hence, $n$ being 40, by equat. (A)

$$\Delta + {\cdot}40627 f = {\cdot}09788.$$

In order to find an approximate value of $\Delta$, the six anomalous experiments must be reduced to the equator on the same supposition as before; namely, $f = {\cdot}205$: Then

| | $\Delta$ |
|---|---|
| Galapagos | ·01717 |
| Ascension | ·01995 |
| St. Thomas | ·02073 |
| Guam | ·01912 |
| Mowi | ·02136 |
| Isle of France | ·02277 |
| Sum | ·12110 |
| 9 equatorial values of $\Delta$ in my former paper | ·11967 |
| | 15)·24077(·01605 mean |

By

By substituting ·01605 for $\Delta$ in the foregoing equation, we get $f =$ ·2014. We now have,

$$a = \cdot 01605, \qquad \Delta = a + s,$$
$$b = \cdot 2014, \qquad f = b + \tau:$$

and the equations (B) and (C) will give us,

$$s + \cdot 40627\, \tau = 0$$
$$3\cdot 869\, \tau = \quad (\sin^2 \lambda\, (\delta - a - b \sin^2 \lambda)).$$

Computing now the sum on the right-hand side of the last equation, which sum consists of 40 terms, we shall find,

$$3\cdot 869\, \tau = + \cdot 00307;$$

wherefore, $\tau = + \cdot 00079; \quad s = - \cdot 00032;$
$f = \cdot 2022; \quad \Delta = \cdot 01573:$

And we hence obtain this general formula for the length of the pendulum, viz. $l = 39\cdot 01573 + \cdot 2022 \sin^2 \lambda$,

$$\text{ellipticity} = \cdot 00865 - \frac{\cdot 2022}{39\cdot 01573} = \cdot 00346 = \frac{1}{289}.$$

This result coincides almost exactly with what Captain Sabine has deduced from his experiments. We have next to examine, how the formula agrees with the phænomena. Beginning with the six anomalous stations, there is still an excess of gravity at them all, as will appear by the following errors which must be applied to the observed pendulums to make them equal to the calculated lengths; viz.

| | errors. | | errors. |
|---|---|---|---|
| Galapagos . | —·00144 | Guam ...... | —·00355 |
| St. Thomas | —·00500 | Mowi ....... | —·00599 |
| Ascension .. | —·00427 | Isle of France | —·00746. |

The error at Galapagos is now very small, and, in all probability, smaller than the actual error of observation. At the other five stations the excesses of gravity are still great; and, with respect to the like errors in my table, they appear to be reduced nearly in the proportion of 3 to 4. But if some advantage be gained at the anomalous stations, there is a disadvantage at all the other stations within 50° of the equator on either side. Within these limits all the observed pendulums are uniformly short of the calculated ones, except at Port Jackson and Jamaica, where there is a very small difference of an opposite kind, which may be neglected; and even, with respect to these two stations, there is great reason to think that the observed pendulums are both too long. Nor are the deficiencies trifling, as will appear by the following list of errors, which must be applied to the observed pendulums to make them equal to the calculated lengths:

| | errors. | | errors. |
|---|---|---|---|
| Paramatta . | + ·00212 | Madras .... | + ·00269 |
| Rio Janeiro | + ·00266 | San Blas . . | + ·00418 |
| Bahia ..... | + ·00161 | Figeac .... | + ·00224 |
| Maranham . | + ·00399 | Bourdeaux | + ·00323 |
| Rawak .... | + ·00094 | Clermont . . | + ·00148 |
| Trinidad . . | + ·00376 | Paris ..... | + ·00103 |

About 50° of latitude all the formulas very nearly agree with one another, and with observation. In receding more from the equator, the pendulums are shorter in the formula we are considering, than in the others; but the effect of this is not very perceptible except at the highest latitudes. At Port Bowen and Spitzbergen, the errors are —·00239 and —·00302.

It appears then that, by lengthening the equatorial pendulum, we approach a little nearer to the anomalous experiments, and recede from all the rest within 50° of the equator. The foregoing investigation proves that all the 40 observed pendulums can never be represented, without great discrepancies, by any one ellipticity. In my last formula all the pendulums, except the five anomalous ones, are placed on one and the same surface within the limits of the errors of observation. But no one pendulum within 50° of the equator answers to its proper place in the surface of which the ellipticity is $\frac{1}{289}$. This surface is merely an imaginary mean, the offspring of calculation. It can have nothing to do with what is called local attraction: for it makes an excess of gravity at five particular stations, and, as far as experience enables us to pronounce, a defect of gravity every where else within the limits mentioned.

Feb. 14. J. IVORY.

## XXXIV. *Failure of* Lagrange's *Method of integrating Linear with Constant Coefficients**. *By* JOHN HERAPATH, *Esq.*†

IF we suppose the roots $r, r_1, r_2$ of the equation

$$r^3 + Ar^2 + Br + C = 0$$

to

* It should have been mentioned in my last paper, that the constants in (18) are not precisely the same as in (17), except the first two. In the other terms several factors in the denominators were suppressed and understood in the constants, for brevity and ease of printing.

† Communicated by the Author.

to be all equal, the three equations of condition, p. 23, which are asserted, p. 96, to give the "complete integral" of

$$\frac{d^3y}{dx^3} + A\frac{d^2y}{dx^2} + B\frac{dy}{dx} + Cy = X \qquad \text{(a)}$$

by Lagrange's method will become

$$\begin{aligned} r^2e^{rx}\{dp + dp_1 + dp_2\} &= X \\ dp + dp_1 + dp_2 &= 0 \\ dp + dp_1 + dp_2 &= 0, \end{aligned}$$

giving obviously $X = 0$. That is, the equations of condition, which are said to furnish the "complete integral" of (a), and which therefore ought to give all cases of the integral without limiting the value of X, fix in the above instance the value of this indefinite function to zero.

Any candid mind would not need a more decided proof of absurdity in the principles of Lagrange's method than the above; but as some may think it dealing a little too summarily with a process which has obtained so much celebrity, and may prefer an instance of failure in result to a theoretical exposition of absurdity in principle, I submit the following, taken not from any investigation of mine, but copied from a professed advocate of Lagrange in the last Number of this volume. We are informed, p. 96, that the "complete integral" of (a) is

$$\text{“}\; y = \frac{e^{rx}\{c + \int Xe^{-rx}dx\}}{r - r_1 \,.\, r - r_2}$$

$$+ \frac{e^{r_1x}\{c_1 + \int Xe^{-r_1x}dx\}}{r - r_1 \,.\, r_1 - r_2} \qquad \text{(b)}$$

$$+ \frac{e^{r_2x}\{c_2 + \int Xe^{-r_2x}dx\}}{r - r_2 \,.\, r_1 - r_2}\,.\text{”}$$

Now this being the "complete integral," must give the value of $y$ whatever values be assigned to $r$, $r_1$, $r_2$ and X. Putting for simplicity $X = 0$, and then supposing $r = r_1$ the above equation becomes

$$y = e^{rx}\frac{c - c_1}{0 \times (r - r_2)} + \frac{c^0e^{r_2x}}{(r - r_2)^2};$$

that is, the value of $y$ is infinite unless the arbitrary constants $c$, $c_1$ are equal, or certain functions of $r$, $r_1$. But $c$, $c_1$ being in the strict sense of the word "arbitrary constants," are not necessarily functions of $x$ or $r$, $r_1$; and therefore the infinite value of $y$ remains. We however know from other principles that $y$ may be finite.



Lest

Lest it be supposed that the failure of Lagrange's method may be remedied by putting the pretended "complete integral" under the form of our (17), so as to take away the zero divisors, and thus bring out the complete solution: I shall now examine this point. For the sake of simplicity I will take an equation of the second order only, the lowest to which Lagrange's method can be fairly applied; and which from its having but two roots, and therefore but one difference between them, is peculiarly adapted to exhibit perspicuously the justice or injustice of our observations.

If $r, r_1$ be the roots of the equivalent algebraic equation, the "complete integral" is

$$y = \frac{ce^{rx} - c_1 e^{r_1 x} + e^{rx}\int Xe^{-rx}dx - e^{r_1 x}\int Xe^{-r_1 x}dx}{r - r_1}.$$

And if we put $X = 0$

$$y = \frac{ce^{rx} - c_1 e^{r_1 x}}{r - r_1} = ce^{r_1 x}\int e^{(r - r_1)x}\,dx + c_1 e^{rx}\int e^{(r_1 - r)x}\,dx$$

must be the "complete integral" of

$$\frac{d^2y}{dx^2} + A\frac{dy}{dx} + By = 0.$$

But when $r = r_1$ the latter expression for the "complete integral" is

$$y = (c + c_1)\,x\,e^{rx}$$

which is evidently not the "complete integral," were it only that $c + c_1$ makes but one arbitrary constant. The integral by our formula (17) is

$$y = \{c_1 + cx\}\,e^{rx}$$

in which two arbitrary constants appear.

J. HERAPATH.

P.S. Since writing the above, I have examined with more attention Lagrange's method, and I find that the solution (b), which is professed to be taken from Lacroix's great work, as the "complete integral" by Lagrange's method, is not strictly the integral which this method gives. The result of Lagrange's contains less of absurdity than the above, but nevertheless fails to give the complete solution. I have not found where Lagrange published his method, nor what examples of it he gave. If that of Lacroix be one, it is a curious circumstance that he should fail both in method and deduction.

XXXV. *On*

## XXXV. *On Systems and Methods in Natural History.* By J. E. Bicheno, *Esq., F.R.S., Sec. L.S., &c.**

I PROPOSE to myself on the present occasion to make some observations on Systems and Methods in Natural History; a subject of great importance at all times, but more especially so at present, when new views of arrangement and nomenclature are proposed, and to some extent adopted. Let me not be understood, however, in the general observations which follow, to be opposed to any particular system; my object being to discuss the first principles of arrangement, and to leave others to judge how far they are applicable to the views adopted by any individual systematist.

It has appeared to me that the difficulties of the subject have not been duly appreciated; and the time cannot be unprofitably occupied, if I accomplish no more than to enable us to estimate them. It might even be suspected, from the readiness with which new systems are adopted, that they have a peculiar attraction for ardent minds; as it has not unfrequently happened that young naturalists have found themselves prematurely embarrassed in a subject, which of all others requires not only an extensive acquaintance with the operations of the human mind, but long experience and various practice. The line of argument I propose to employ, must necessarily be somewhat abstract; yet I hope I shall be borne with, since the practical naturalist could make no accumulations to his science, and all his particulars would stand unconnected and discrepant throughout, without the aid of abstract reasoning. Besides, I am anxious to engage the attention of persons accustomed to turn their observations to the operations of the human mind, and to the instruments which it employs to perform its labours; feeling assured that, by obtaining the cooperation of this class of philosophers, we shall have great light thrown upon our subject; and that it will be one means of attracting the notice of those who delight in a large and liberal treatment of science. While they impart to us a philosophical solidity, in which I am apprehensive we are wanting, we may hope to communicate to them a reciprocal benefit, in some of those graces and charms to be derived from the study of Nature, and in which perhaps they may be deficient.

Without undervaluing the study of species, upon which a great deal of our knowledge is built, it cannot be denied that naturalists in general have been too often content with assigning them names, and a place in the systems they have adopted; and

* From the Transactions of the Linnean Society, vol. xv. Part ii. p. 471.

and this they have done without having an ulterior view to their structure and functions, and the relations subsisting amongst them. Much less have they kept in view the end of generalizing the particulars they are accumulating; but they continue to heap together a "*rudis indigestaque moles,*" until they are actually overwhelmed by their materials. To build up science skilfully, the combination should go on with the collecting, or the superstructure will exhibit neither use nor beauty.

Mr. Roscoe has clearly illustrated, the comparative merits of the artificial and natural arrangements in Botany in a former volume of the Transactions*; and has satisfactorily proved, in my estimation, that however admirable and comprehensive the system of Jussieu may be, yet it ought not to supersede the use of the Linnæan arrangement. The two great masters of botanical science propose different ends, and ought not to be regarded as rivals. The President of this Society has also constantly pressed upon the attention of the student the same important fact.

In some respects it is not to be regretted that the absolute sway which the name of Linnæus has had among English naturalists is somewhat abated: for although authority is an extremely useful bond of union, and has in this instance established among us a nomenclature which nothing short of homage to the founder could probably have made current, yet it has brought with it the ordinary evils attendant upon great names. The range of the pupil has been limited by that of the master; and it has been considered a species of heterodoxy to dissent from the established opinions. The danger to be now apprehended is, that those who adopt other arrangements will forget the advantages to be derived from what is old, in their love of that which is new.

In addition to the remarks made by Mr. Roscoe and the President, I would beg leave to suggest to those who adopt new systems,—and in adopting them think it advisable to break up the old orders and genera into many new ones,—that the artificial and natural systems aim at two very distinct objects, which are in some measure incompatible with each other. The one is to make us acquainted with individuals: and the other, founded upon an acquaintance with individuals, to combine them according to their characters, so as to abridge the labour of reasoning, and to enable us to ascend from particular to general truths.

In order to assist us in these investigations, we employ certain words in a peculiar sense. Thus the word *Species*, when

* Trans. Linn. Soc. vol. xi. p. 50.

used

used by naturalists, has a more confined signification than the same word when employed in scholastic language. We have agreed that a species shall be that distinct form originally so created, and producing by certain laws of generation others like itself: whereas all that logicians have meant, is a number of objects bearing a certain resemblance to one another, and on that account denominated by a single appellation, which may be employed to express any one of them. This term is the creature of art, to help us up the first step of generalization. By its assistance we propose to reason upon all the individuals conforming to the law we have laid down, as safely as we can do of any one of them. There is this inconvenience attending the use of it by naturalists, that it assumes as a fact, that which in the present state of science is in many cases a fit subject of inquiry; namely, that species, according to our definition, do exist throughout nature. It is too convenient a term to be dispensed with, even as an assumption; only care should be taken that we do not accept the abstract term for the fact.

It might, for instance, be proposed as a legitimate question, whether the species of some familiar genera, such as *Rosa*, *Rubus*, *Saxifraga*, do not run into one another by imperceptible shades, unappreciable by human sense, in the same manner as certain genera melt and intermingle their characters, so as to render it impossible to circumscribe them. Indeed, the extent to which species-making has been carried in modern times, almost leads to this conclusion. Visible and palpable distinctions are in many cases no longer relied on; and there are many acute naturalists, who, without bringing the subject to the test of experiment, are content to rely on those empirical characters, which can only be perceived by long and familiar experience, and cannot be described by words. The truth is, that all sensible objects have characters which leave impressions upon the mind, without our being capable of embodying them in language. We are all aware of this when we speak of tastes, and tints, and the countenances of our friends. Every-body perceives them, yet nobody can communicate to his neighbour his perception of their differences. Thus botanists speak of certain species of plants differing in appearance, habit, touch, &c.; by which they often mean that they have some indescribable peculiarities about them, which point them out to the practised observer as distinct. A great number of such species may be detected in every modern Flora of a well investigated country; but whether they deserve to be ranked among those which are capable of definition, is a question of great doubt:—that the practice is an inconvenience, none will deny; and if it be much longer continued, will involve

volve in inextricable difficulty all our well known species, make us dependent upon empirical and traditional evidence for our acquaintance with them, and render it impossible to derive instruction from books. In such cases the assumed law ought to be brought to the test of experiment, or the species should be rejected.

Many of our cultivated plants also tend to invalidate the law. Who can refer our *cerealia* and esculent vegetables, in many instances, to their true types? and how few of our old flowers are there, of which the astutest botanist can trace the origin! Domesticated animals afford a still more striking example; and man himself furnishes the most difficult problem of all.

These remarks and examples are, I apprehend, sufficient to show how difficult it is to adopt the term in its strict acceptation; and that however precisely the naturalist has attempted to employ it, he has not succeeded to the extent he has proposed; and that it can only be taken as correct in a vague and general sense, and as a convenient abstraction to relieve him at the first step from the necessity of becoming acquainted with every individual.

The next term of importance to the naturalist upon which the accuracy of his reasoning depends, is that division of his system which he denominates a *Genus*. This is an assemblage of individuals agreeing also in some common characters; but, unlike the word species, it is not previously defined. Thus much indeed has been thought requisite; that in botany these common characters should be taken from the parts of fructification, and in zoology from such parts as are indicative of structure and habits. "A genus should furnish a character, not a character form a genus." We are not here, as in the word species, precluded from inquiry by a previous definition. Though both words are terms of generalization, there is the same difference between them, as instruments of reasoning, as between a definition and a proposition in geometry.

The species includes all the characters which are in the genus, and those likewise which distinguish that species from others belonging to the same genus; and the more divisions we make, as order, family, class, it is intended that the names of the lower should become still the more comprehensive in their signification, but the less extensive in their application to individuals. Naturalists by this invention, which is not exclusively their own, have it in their power to contemplate and reason upon these separate characters, with all their consequences, as if they existed independently of species; as by the use of the word species they are enabled to look at their peculiar

culiar attributes independently of individuals. This faculty of the mind, which is one of the most curious that belongs to it, has given rise in all languages to a multitude of words of the same kind as the names of genera in Natural History; words, which do not express individual existences, but are abstractions of qualities and characters belonging to them *.

All general reasoning in morality, law, politics, and even mathematics, depends for its accuracy upon the proper use of generic and other abstract terms. In mathematics they admit of exact (or I would rather say more exact) previous definition; and hence arises the accuracy of deductions the most recondite and remote in that science. In the other sciences, which are of a speculative and contingent nature, these terms are employed not with the same precision, but seem to be the result of our necessities, borrowed from sensible objects and analogy, and frequently indeed from accidental coincidences. They derive their force rather from the character of the mind that employs them, than from any exact definition they may have received; and it seems impossible to make men use such words in a common acceptation. Hence it is, I apprehend, that knowledge of a speculative kind so soon finds its limits; and where at its outset it has promised such glorious results to mankind, as long as it floated in general propositions, the same subject eludes the grasp of the human faculties when it is attempted to be reduced to exactness, and leaves something always to be desired. We are constantly approximating to the truth, yet never reaching it.

It is sometimes asserted, but not correctly, that Natural History, by the aid of its terms, partakes of the nature of mathematical truth; or that it lies intermediate between that science and speculative knowledge. The situation of the naturalist is rather this. He finds himself placed amidst an infinite number of unknown particulars; and in order to facilitate an acquaintance with them, he at once, without regarding individuals with much minuteness, throws together a number of them, which he calls a species, according to an assumed hypothesis. These he attempts again to combine by certain external characters, and calls them a genus. By these means he is enabled to contemplate and treat of them, without being utterly bewildered in the labyrinth of unarranged individuals. Classification is his *filum Ariadneum*. It was but imperfectly understood by the ancients; and has enabled the moderns to arrive at conclusions with much more expedition than they,

* I would avoid here, and leave the question to be decided by the reader, after he has consulted Locke and Berkeley, whether we have got ideas corresponding to these abstract terms, or whether they are mere signs, like $x$, $y$, and $z$, in algebra.

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and with equal safety. It does that at once which is constantly going on in ordinary language,—the modifications of it to express the classes of external objects. The invention of new terms suited to express new ideas in an abridged and compressed form, is a slow process, and in most cases is the result of convenience. There is no convention to attain the object, because nobody can arrest the subtile means that are employed. But the naturalist being without terms, or at most with so few that they are within his power, attempts to anticipate the slow process usually working in language, and forms at once his instruments of reasoning; and systems and methods can be regarded as no further useful, than as they are assimilated to the ordinary process of abridging the labour of thought adopted by mankind in other subjects of a like nature.

Naturalists err greatly who imagine they are employing terms possessing some new and distinct properties; whereas all they can do is to hold the subjects of natural history together in a loose manner by the use of the words *species*, *genus*, *order*, and *class;* thus presenting certain characters to the mind as separate objects of contemplation by means of abstract terms, of a similar though somewhat more precise import than those which are employed by the rest of mankind in treating general subjects. A stricter use may be made of these words by naturalists than by metaphysicians, because the business of the one is to examine characters and qualities more nicely than the subjects entertained by the other will admit of. Nevertheless, the one cannot employ these abstractions as instruments of reasoning in a different sense from the other. There is no magic about them in the hands of a naturalist more than there is in any of the thousand general terms in the mouths of the vulgar. "Rose" and "Grass" were generic names before the flood, and will continue to be so in spite of systems and methods. The naturalist has attempted only to carry this necessary operation of the mind somewhat further and with more precision, and has thus exposed himself to errors, which the vulgar have escaped. Thus, although there are but two modes of reasoning; namely, by the use of words expressive of an individual and its attributes, or by general words indicative of an aggregation of individuals with their common attributes; yet naturalists have used their terms in a different sense, and have invented additional ones, such as *order*, *tribe*, *cohort*, *family*, *class*, by which they attempt to express with more accuracy larger generalizations than they would do by employing a generic term, and as if they could settle the relative rank of the different groups whose existence they have assumed. Whereas the truth is, that in many instances a class may be equivalent to an order or a genus. These different

ferent gradations, thus strictly aimed at, are gratuitous assumptions with which Nature has nothing to do; and which frequently lead to the establishment of false hypotheses.

[To be continued.]

## XXXVI. *Notices respecting New Books.*

*The Steam-Engine: comprising an Account of its Invention and Progressive Improvement, with an Investigation of its Principles and the Proportions of its Parts for Efficiency and Strength; detailing its Application to Navigation, Mining, impelling Machinery, &c. with 20 Plates and numerous Wood-cuts. By* THOMAS TREDGOLD, *Civil Engineer, Member of the Inst. of Civ. En. &c.* Price 2*l.* 2*s.* 4to. Jos. Taylor.

A BRIEF analysis of the contents of this valuable work will give our readers some idea of its utility, as well as of the immense degree of labour necessary to collect, arrange, and reduce to their elementary principles, the various phænomena concerned in that master-piece of human contrivance, the Steam-engine. In so doing, we propose to follow, in some degree, the author's statement of its contents in his Preface; with the addition of many particulars he has not noticed in that short summary.

The work is in sections. The first contains a short history of the invention and improvement of the steam-engine, the inventors being noticed in the order of the dates of their first essays, commencing with the Marquis of Worcester, and closing with Woolf and Oliver Evans: a short sketch is added of the rapid progress of the application of steam power; and the section concludes with the remark "that the whole tends to prove that the steam-engine, in the highest state of perfection it has yet attained, is entirely of British origin. The remark extends to the discovery of physical principles, as well as of mechanical combinations. No new principle, nor no new combination of principles, has yet been derived from a foreign source, the most perfect of foreign steam-engines being professedly copied from British ones, and not unfrequently manufactured by British workmen."

The second section treats of the nature and properties of steam, its elastic force, expansive force, and motion; first stating the general principles of the equilibrium of heat, the phænomena attending a change of state, and a comparison of the best experiments on the quantity of heat required to convert bodies into vapour or steam, with the necessary formula for the comparison. The elastic force is next considered, with the principles which guided the author in his choice of a formula for a case where empirical means are necessarily employed: and here we remark for the first time a formula for the expansion and expansive force of water, free from the objection of becoming negative in either high or low temperatures. The connection between the boiling point of sea-water and the force of its steam is treated, and a simple formula for the force of vapours is shown to apply to those from various fluids, with the necessary correction

correction of the constants.—The mechanical power of compressed gases is examined; and it is inferred, that the steam of water possesses greater advantages for use in an engine, than the vapour of any other liquid.—Rules are next given to find the volume the steam of a given quantity of water occupies at a given force and temperature; the laws of the mixture of steam and air, as in the air-pump of an engine; and the laws of the motion of steam in the passages, &c. of engines, with the loss by cooling; and a curious inquiry respecting the temperature of condensation which affords the maximum of useful effect in the old atmospheric engines; illustrated by a reference to Mr. Watt's experiments, with a model of one. The section concludes with the investigation of the formula for the escape of steam at safety-valves.

The third section is on the generation and condensation of steam, and the apparatus required; first treating of combustion and combustibles, with an extensive table to show the relation between the heat afforded by bodies, and that which their constituent elements would give: then the conditions favourable to the process of combustion are considered, and the quantity of the gaseous products, with rules for the supply of air, and size of chimneys. The relation between the surface of boiler to be exposed to heat, and the quantity of steam to be generated; and of the capacity of the boiler to the effective power of the engine, are also exhibited, and compared with practice; and then rules given to fulfil the essential conditions. The different species of boilers, fire-places, safety-valves, and other apparatus, are then described; and the section closes with the principles of condensation, the quantity of water required to produce it, and a tabular view of the methods that may be used, referring to the dates of the first use of them in the steam-engine.

The fourth section commences with a popular illustration of the nature and modes of obtaining the power of steam, and their relative advantages; next, proceeding to show how to calculate the utmost effect steam can possibly afford to produce rectilinear motion; and afterwards its effect in rotary engines,—showing the loss of effect in the latter to be constant, and in some cases very considerable. The modes of applying the power of steam are classified, with their combinations; and then the laws which determine the relations of the parts for cylinder engines, so as to afford a maximum of useful effect with a given quantity of steam; the capacity of the air-pump, and the power lost in working it.

The fifth section treats of the proportions, power and constructions of noncondensing engines, usually called high-pressure engines, showing the causes of loss of power, and the means of estimating the quantity of loss; the best proportion for cutting off the stroke when the steam acts expansively; and the theory of combined cylinder expansive engines.

The sixth section treats of condensing engines. After a general view of their nature, and a classification of the methods of construction, the proportions, causes of loss, and the power of the atmospheric engine are investigated; and also in its combination with a separate

separate condenser. The steam-pressure engines of Boulton and Watt, both single and double, is next considered, with the construction of them either for expansive action, or not, and all the causes of loss of power, and the proportions of the parts in detail; also the combined engines of Hornblower and Woolf are treated of in a similar manner.

The seventh section is occupied by the construction of the parts common to most engines; as valves, slides, pistons; with the mode of estimating their friction, methods of moving valves, &c. cranks, parallel motion apparatus, and practical rules for the strength of the parts of engines, with the conditions necessary for safety in boilers, and the mode of estimating the effect of irregular expansion on the strength of boilers of cast-iron,—closing with the methods of joining the parts of engines together.

The eighth section is on equalizing the action, regulating the power to the effect, measuring the quantity of useful effect, and managing the working of engines; describing, with investigation when required, the fly and counter weight to equalize the action, the throttle valve, Field's valve, the conical pendulum, the regulator and the cataract to regulate the power; and the gauges, indicator, and counter, to show the state of the parts, and how by friction the useful effect of any engine may be measured at a trifling expense; and lastly, the circumstances to be chiefly attended to in working the engine.

The ninth section is on the application of the steam-engine to various purposes; first, generally to raising water, then its particular cases, as the drainage of mines, the supply of towns, with a table of the supply to towns in ancient and modern times; secondly, to impelling machinery for manufacturing purposes; thirdly, its application to agricultural purposes; and lastly, its application to steam carriages.

The tenth section is on steam navigation, treating of the longitudinal and lateral stability of vessels, and the resistance of vessels; in the latter inquiry a new theory of resistance is adopted, which appears to give results nearly according with practice. The various modes of propelling vessels are noticed, and those by water-screws and paddle-wheels are investigated, and the modifications pointed out; then the strength of vessels, the power of sails and of currents, and their combination with steam power, followed by the necessary rules for arranging the proportions, power, and effect of vessels, and a tabular view of the vessels, executed at different times by the best manufacturers in this country.

These multifarious inquiries are followed by an extensive table of the properties of steam, and two tables of the proportions of engines for different powers, with the expenditure of water and fuel each requires. The letter-press is illustrated by wood-cuts in the pages, and the whole by twenty plates, each having its description opposite to it; and a copious index, and a full table of contents, renders reference to particulars easy; and, as a whole, this most useful work presents that combination of science with practice, which perhaps no one, except a person having the experience of a workman, as well as an extensive

extensive knowledge of science, could have written,—and such we know to be the case with the author*.

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*The Elements of Geometry, with Notes. By* J. R. Young.

The great excellence of Euclid is well known to consist in the completeness and perfect accuracy, as well as the general simplicity and elegance of his demonstrations, admitting no assumption, no step in the process of his reasoning that has not been previously established, and at the same time, with this requisite, usually adopting the shortest course for arriving at his conclusion. It is not, however, pretended that this admirable work, so long and so deservedly received as the text-book for instruction, is absolutely free from defects. Even the very rigour of his proofs is sometimes the source of so much intricacy and abstruseness, especially in the equally difficult and important branch of the mathematics, the doctrine of proportions, as to form a serious and discouraging obstacle to the progress of the learner. The want of success in every attempt hitherto made to lessen this difficulty, might seem to render the task hopeless. The aim of Mr. Young, however, in the work before us, has been, without impairing the completeness and satisfactory nature of his demonstrations, to contract and facilitate the labour of the student as much as possible by simplicity and conciseness, and also to add to the extent and accuracy of his geometrical knowledge. Nor do we hesitate to recommend his treatise, not certainly as superseding the use of Euclid, but as a useful auxiliary to that great work. His observations on the theory of parallel lines; his demonstration of the converse of every proposition where this is possible, and showing its failure, where it is not; the labour he has bestowed on the doctrine of proportions, as well as his corrections of many errors of preceding geometers, and supplying their defects, together with his minute attention to accuracy throughout,—may be justly considered as rendering his performance valuable, especially to the learner.

The notes are numerous, and most of them important. Among the various errors and defects of modern geometers detected and corrected in them, one instance especially is singular, and deserves to be noticed. Mr. Thomas Simpson, in his Elements of Geometry, has substituted in the place of the seventh proposition of Euclid's sixth book, one which Mr. Young has proved to be absolutely false. This is the more remarkable, as, though the work in which it occurs has been in the hands of geometers more than seventy years, this error has hitherto escaped detection. Even Dr. Robert Simson, the editor of Euclid, though entertaining no very friendly feeling towards his contemporary, suffered it to pass unnoticed. A similar error occurs also in Mr. Leslie's Geometry, Prop. xiv. B. vi. last edition.

As the celebrated propositions of Euclid, (B. i. Prop. 47.) the discovery of which is attributed to Pythagoras, admits of various demonstrations, Mr. Young has given three; the last of which in the notes is concise and simple, though that inserted in the text instead of Euclid's, is inferior to the latter, not only in elegance but in simplicity also.

* His first Essay was a paper in the 46th vol. of the Phil. Mag. p. 15.

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The first eight books of this work now published treat of lines situated in the same plane. The second Part, as announced by the author, will contain the geometry of planes and solids; with an Appendix, and Notes on the Symmetrical Polyedrons of Legendre.

## XXXVII. *Proceedings of Learned Societies.*

### LINNÆAN SOCIETY.

Feb. 5.—READ some account of the Botany of the provinces lately ceded by the Burmese to the Honourable the East India Company; with a description of two new genera of plants: in a letter to H.T. Colebrooke, Esq., F.R.S. & L.S. &c. By Nathaniel Wallich, M.D. F.L.S. F.R.S. E. &c. Superintendent of the Botanic Garden at Calcutta. The author states that his botanical treasures are most extensive; the number of species having long ago surpassed 2000; and that he has never seen any vegetable production equal to his *Amherstia nobilis* when in full bloom. It surpasses all the Indian plants.

*Amherstia.* Diadelphia Decandria—Nat. Ord. Leguminosæ. The flowers of this splendid tree are disposed in pyramidal pendulous clusters 2 feet long, and 10 inches broad at the base. Leaves $1\frac{1}{2}$ foot long, with 8 or 10 pair of oblong pointed pinnæ, which are from 8 to 10 inches long, and of a peculiarly delicate glaucous hue. The racemes are scarlet. The petals are furnished at the apex with a broad yellow spot, having a tubular calyx; and the genus is evidently allied to Heterostemon of Desfontaines.

Dr. Wallich has at length found the *Varnish*-tree of the Burmese, which he constitutes a new genus, and calls it *Melanorrhœa;* Polyandria Monogynia; Anacardiæ, Brown.—Also another singular plant, which he calls *Phytocrene gigantea,* allied to Araliaceæ. The trunk is as thick as a man's thigh, and when divided affords a large quantity of a limpid, tasteless, and very wholesome water.

Feb. 19.—Read a description of a curious Fungus belonging to the Gastromycous order, found near Wrexham by J.T. Bowman, Esq. F.L.S. on decaying oak branches stript of bark. In its earliest stages it is globular: afterwards, from the expansion of the filaments, the sporules are exposed, and the sporangium becomes rugged and broken; from the ripening of the seeds the peridium bursts, and the filaments being set at liberty acquire first a horizontal and then a more erect position, resembling the branches of a palm-tree.

At the above meetings were also read some portions of a paper by J. E. Bicheno, Esq. F.R.S. Sec. L.S. entitled "Remarks on the Flora of Great Britain, as connected with Geography and Geology."

The Author in this paper, instead of attempting to connect plants with particular temperatures, as most authors who have treated the subject have done before, endeavours to show the relation which vegetables have to Geographical and Geological structure. He regards England as the most favourable place to commence such remarks, because of the intimate knowledge we have of its stratification, and also of the stations of all our plants.

In order to assist our inquiries he thinks it necessary to reject all those

those species the introduction of which may be traced to artificial causes, regarding those alone as indigenous which are coeval with the soil. Agriculture, Medicine, and even Religion, have each swelled our list. *Coronopus didyma* is not found in England but at ports, and seems to be a particularly migratory species. It may be found, he says, in almost every latitude where ships frequent;—at Lisbon, Madeira, Rio Janeiro, the Cape, Sydney. He thinks the same disposition may be witnessed in many of the Cruciferæ, and adduces as instances, *Sisymbrium Irio*, and *Thlaspi campestre*. Whether the cause of such phænomena is to be sought for in the structure of the seed, or in the pabulum necessary to the growth of the plant, remains to be determined. The *Blatta orientalis* (common Cock-roach), and the Norway Rat, which has eaten up the old Black Rat, are instances from the animal world, showing a similar tendency.

In order to ascertain whether a species be indigenous, it is of importance to ascertain upon what stratum, or in what places, it terminates its range. Many southern plants he finds terminate their northern range upon old walls, such as *Sisymbrium Irio* and *tenuifolium*, *Dianthus Caryophyllus*, *Teucrium Chamædrys*, &c. Others again abounding in the south, finish their course by attaching themselves to some of the warmer strata, as to mountain limestone, or new red sand-stone. *Hutchinsia petræa* grows in the Olive country so abundantly that it is gathered as a salad. With us it finds only a few localities, and those always on mountain limestone. *Campanula patula* finds its most northern accommodation in England on the new red sand-stone. It has one other station with us on chalk, which is its place in Brittany.

The sea-coast also carries many plants further north, than inland situations can do. The Common Privet for instance, though so abundant in the south on the chalk and gravels, becomes rarer as we proceed to the north. In Scotland it affects the coast, and in Sweden is wholly confined to it. On the other hand, plants which belong to a more northern latitude and to more primitive countries, do not terminate their southern range on our warmer and richer soils. The author suspects *Daphne Mezereum* not to be English, because it is found only on the more recent strata.

The stratum producing the greatest number of rare plants with us is the mountain limestone; and although but a small portion of this rock should appear above the surface surrounded by other rocks of a different structure, it seems to have caught the rarer species. The district of Gower, Brean Down, St. Vincent's Rocks, Orme's Head, are examples.

Next to mountain limestone the Bury sand seems to be a favourite station of unusual productions. *Veronica triphyllos* and *verna*, *Vicia lathyroides*, *Tillæa muscosa*, *Scleranthus perennis*, &c. &c. grow there.

The richest soils are the least productive of rare species. Slaty countries, excepting the highest elevations, are also singularly deficient.

Many of our promontories are remarkable for the rarities they produce

duce. *Erica vagans* terminates its northern range on the serpentine of the Lizard; *Draba aizoides*, in Gower.

The author then proposed to consider the vegetation which is produced by the different strata, commencing with the eastern side of the kingdom, or with the most recent deposits.

The diluvial soil of the Fens, which is composed of decayed vegetables resting on clay, is, when left to nature, soon covered with grasses, to the exclusion of every other plant. Scarcely a single rare species is to be found on it. The water in such soils, on the contrary, abounds in rarities. *Stratiotes aloides, Arundo calamagrostis, Teucrium Scordium, Lactuca Scariola, Senecio paludosus*, &c. &c. are of this number. The author remarked, that the first of these species, though wild in Berkshire, had not been observed to flower there. *Arundo calamagrostis* he regards as peculiarly a Fen plant with us, and that the localities assigned to it elsewhere belong to *A. epigejos*. This locality appears to be on its most southern limit. The most remarkable feature of Fen vegetation is the luxuriance and quantity of Cruciferæ.

Whatever phænomena geologists may discover to induce them to regard the Bury sand as the same with that of Bagshot-heath, the author remarks that their vegetable productions do not correspond; and though he does not assert that we ought to find a similar vegetation on strata of the like nature, yet it so frequently occurs that the rarer plants correspond where strata are alike, that he regards such differences as worth noticing.

The rare plants of the Bury sand are peculiar to itself, while the Bagshot sand is remarkably deficient in uncommon species; the only one of notoriety, he has remarked, being *Agrostis setacea*, which is abundant, and which may be traced upon the sands and gravels of the plastic clay, through Dorsetshire, Hampshire, Surrey, and even to the Commons in the neighbourhood of London, where its range terminates.

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## GEOLOGICAL SOCIETY.

Feb. 1.—The reading of Professor Sedgwick and Mr. Murchison's paper, begun at the last meeting, was concluded.

This paper consists of three divisions: 1st. A brief outline of the general structure of the Isle of Arran. 2d. An account of the section on the N.E. coast of the island. 3d. Concluding remarks explanatory of the probable causes, and geological epochs of the several phænomena. In the 1st division, the authors, considering that the subject has been amply elucidated by Jameson, MacCulloch, and Hendrick, confine themselves to such details as are necessary to make their subsequent description intelligible. In the 2d part, the strata on the N.E. coast are described in great detail, for the purpose of comparison with the corresponding members of the English Series; from whence it appears, that a succession of formations, analogous to the old red sandstone, carboniferous series, and new red sandstone, are exhibited twice over, in an anticlinal section.

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The mineralogical centre of this section is at North Sannox, and the lower red conglomerate is there seen in several situations, rising to the height of about 1000 feet above the sea. 1. This formation is supposed to be identified with the old red sandstone; from its lowest members graduating into grauwacke; from its containing concretionary limestone not distinguishable from the cornstone of Herefordshire; and its being regularly overlaid by the carboniferous series. 2. The middle deposit of the section is clearly referable to the carboniferous series, by its mineralogical structure, by the organic remains in the calcareous beds, which are identical with those of the mountain-limestone; by its containing seams of coal, which have been worked; and by the plants in the shale being of the same species with many of those most abundant in the coal-measures of England. 3. The superior sandstone and conglomerate are of enormous thickness, rising into lofty and precipitous hills upon the coast. These are referred to the new red sandstone, from their position and internal characters; and this classification is confirmed particularly by the structure of the sandstone on the southern coasts of the island. This formation differs however from the new red sandstone of England, not only in being conformable to the beds on which it rests, but also by graduating into the superior parts of the carboniferous order.

In conclusion, the authors endeavour to show, that the great dislocations of the secondary deposits have been produced by an upheaving of the granite; and they state, in corroboration of this opinion, that where the breaks in the strata are greatest, there the granite makes the nearest approaches to them. It is further attempted to be proved, that the granite could not have been in a perfectly fluid state at the period of its elevation, from the fact of its existence in the form of mural and serrated precipices on the flanks of the secondary strata; being in this respect prominently distinguished from the trap of the southern regions of the island, which has, in numberless places, not only penetrated, but overflowed upon the new red sandstone.

Feb. 15.—The Anniversary Meeting of the Society was held this day; and the following Fellows were elected Officers and Council for the year ensuing:—*President:* William Henry Fitton, M.D.: *Vice Presidents:* Arthur Aikin, Esq. F.L.S.; Rev. W. Buckland, D.D. F.R.S. Professor of Mineralogy and Geology in the University of Oxford; Charles Lyell, Esq. F.R.S. & L.S.; Rev. A. Sedgwick, F.R.S. Woodwardian Prof. Camb.—*Secretaries:* W. J. Broderip, Esq. F.L.S.; R. I. Murchison, Esq. F.R.S. & L.S.—*Foreign Secretary:* Henry Heuland, Esq.—*Treasurer:* John Taylor, Esq. F.R.S.—*Council:* J. E. Bicheno, Esq. Sec. L.S.; John Bostock, M.D. F.R.S.; Rev. W. D. Conybeare, F.R.S; John Crawfurd, Esq. F.R.S.; Michael Faraday, Esq. F.R.S.; Davies Gilbert, Esq. M.P. Pres. R.S.; G. B. Greenough, Esq. F.R.S; J. F. W. Herschel, Esq. Sec. R.S.; Leonard Horner, Esq. F.R.S.; Ashhurst Majendie, Esq. F.R.S.; Rev. J. H. Randolph; N. A. Vigors, Esq. F.R.S.; Sir R. R. Vyvyan, Bart. M.P.; Henry Warburton, Esq. M.P. F.R.S.

ASTRONOMICAL

## ASTRONOMICAL SOCIETY OF LONDON.

Jan. 11, 1828.—There was read a paper entitled "Third Series of Observations with a 20-feet reflecting telescope;—containing a Catalogue of 384 new double and multiple stars, completing a first thousand of those objects detected in sweeps with that instrument;—together with Observations of some previously known." By J. F. W. Herschel, Esq., President of the Society.

This paper, as its title imports, is a continuation of the two papers previously communicated by the author on the same subject. The field of discovery in this department of astronomy, though narrowed by the great work recently published by Professor Struve, the author considers as not yet exhausted; since, on an average of the part of the heavens swept by him, not above one in four, of double stars sufficiently remarkable to attract attention in sweeping, have been catalogued by the eminent astronomer last named; not to mention the vast number of interesting close double stars, below the 9th magnitude, which a minuter examination than the nature of his sweeps permits would no doubt produce. The double stars of this Catalogue, he observes, are considerably more *select* than those of his two former ones; those whose distance exceeds 32″ being (except in particular cases) excluded, and the limit of distance being narrowed according to the faintness of the component stars.

The author prefaces his Catalogue with a comparison of the magnitudes habitually assigned to the stars by himself and Professor Struve; from which it appears that on the average, his magnitudes have a denomination about one unit lower than those of that astronomer;—a star (for example) which M. Struve would call of the 9th magnitude, being, in Mr. Herschel's nomenclature, of the 10th. The limit of vision in the Dorpat telescope, he presumes to lie about his average 14th magnitude, though such a determination must necessarily be liable to some latitude. This conclusion he deduces from a series of instances, in which small companions have been seen by him attached to large stars, within the limits of Professor Struve's 4th class, which have escaped the notice of the latter.

The author then states the principle on which he estimates magnitudes below the 6th, which is that of continual bisection of the light; and he cites some experiments, by which it appears that the light of an average star of the 1st magnitude is at least 150 times that of the 6th. He then adduces a series of observations of a considerable number of the closer stars, of M. Struve's Catalogue, by which it appears that the Slough telescope easily defines with its ordinary sweeping power, the generality of M. Struve's stars of the 1st class, and many of those marked by him as *vicinæ*, and even *pervicinæ*; but those which have the epithet *vicinissimæ*, he has not yet succeeded in separating with the highest power (240) usually applied,—which indeed was to be expected. In lieu of M. Struve's classification of double stars, which he considers as enlarging beyond due limits the number of those of the 1st class, he proposes the following system, which in fact very nearly approximates to that originally followed by Sir William Herschel.

 Class

| | | | |
|---|---|---|---|
| Class I. close | ............ | 0″ and below | 1″ |
| Class I. not close | ........ | 1 and below | 2 |
| Class II. | ................. | 2 and below | 4 |
| Class III. | ................. | 4 and below | 8 |
| Class IV. | ................. | 8 and below | 16 |
| Class V. | ................. | 16 and below | 32 |
| Class VI. | ................. | 32 and below | 64 |

So that the limit of distance of stars of the *n*th class shall be $2^n \times 1''$.

The author then subjoins a list of stars common to his two former Catalogues, and to that of Professor Struve, 86 in number; after which he proceeds to describe some singular phænomena observed in the course of his examination of these objects, which explain certain discrepancies between the results of observations of their angles of position on different nights, and which tend to throw light on some obscure points in the theory of vision. He considers it as rendered very probable, by some of the facts adduced, that time is required for light to make an impression on the retina, as well as for the impression made to wear off; and that this time is the less, the brighter the object; and explains by this principle a remarkable degree of unsteadiness and fluctuation observed in the limb of the planet Mars, while small stars in the field remained perfectly tranquil, as well as certain other curious phænomena.

He then adds some observations on the contrasted colours so frequently observed in double stars, and regards them as (at least) in many cases referable to the laws of vision; in virtue of which, a strong light having an excess of the less refrangible rays, will cause a feebler one, in which no such excess exists, to appear of the complementary hue; instances of which, in artificial lights, are adduced. He notices especially the extremely intense red colour of a star of the 8th magnitude, R. A. $4^h\ 41^m$. N.P.D. 61° 47′ (1828.)

These prefatory remarks are terminated by some observations of the 5th star in *trapezio nebulæ Orionis,* pointed out by M. Struve. The author adduces evidence, which he considers as satisfactory, that no such star existed in that situation on the 13th March, 1826. It was observed, however, by M. Struve, to be conspicuous on the 11th Nov. of that year. It is now readily seen in the Slough telescope; and at the time of drawing up the present paper, it was so bright as not to be overlooked with the most ordinary degree of attention. He considers it therefore, if not as a NEW STAR, at least as a variable one of very singular character.

The Catalogue, which follows, is arranged in all respects like the preceding ones published in the Memoirs of this Society, and is followed by a list of about 200 double stars, for the most part found in the same sweeps with the others; but which, occurring in M. Struve's Catalogue, cannot now be regarded as new double stars. Their observed places and estimated angles of position, distances, and magnitudes, are however given, in order to afford ground of comparison between the two Catalogues, of which comparison the results are stated.

PROCEEDINGS

## PROCEEDINGS AT THE FRIDAY EVENING MEETINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN.

Jan. 25.—The Members of this Institution commenced their meetings for the season, on this evening; Mr. Brande occupied the place at the lecture table, and gave an account of the discovery of the three most important vegeto-alkalies, Morphia, Cinchonia, and Quinia; also of their properties, the best methods of preparing them, the methods of detecting impurities in them; illustrating the whole by reference to experiments and specimens.

At the close of the evening he paid a just tribute to the memory of Mr. Daniel Moore, who, a firm friend to the Institution during his life, left it a thousand pounds at his death.

A quantity of the new element bromine was laid upon the library tables, with specimens of Brazilian manufactures, &c. &c.

Feb. 1.—An experimental illustration and explanation of the curious phænomena produced in certain circumstances by a current of air, steam, water, or of any other fluid, was given in the lecture room, by Mr. Faraday. The phænomena were first brought to the notice of scientific men by M. Clement, and consists, as our readers well know, in the apparent adhesion of a flat disc against an aperture in a plane surface, out of which the stream of fluid is passing.—Make a smooth round hole about the eighth of an inch in diameter, through the middle of a large sound bung, cut one of the flat surfaces of the bung smooth with a sharp knife, stick three or four pins upright into that surface, equidistant from each other, and about three quarters of an inch each from the central hole; and then drop between them a disc of paper or card one inch and a half in diameter, so as to lie loosely between the pins over the hole. No effort to blow tbe paper off by forcing air or the breath through the hole in the bung, will succeed; but the stronger the current, the more forcibly will the paper be pressed up against it. Mr. Faraday gave the same explanation of the effect as that given by M. Clement. He stated it to be a pure effect of the momentum of the air between the disc and the cork, and to have no necessary connection with the lateral currents of air which move on to join a stream passing in one direction:—these lateral currents were cut off in some of the experiments, and still the effect remained unabated. He equally denied the effects of friction as having any thing to do with the effect.

Numerous objects of interest were placed upon the library tables, amongst which were several from Ashantee, and especially the scull of an Ashantee slain in the battle of Aug. 1824, which had two occipital bones.

Feb. 8.—Mr. Ainger gave an illustrated account of the origin of Grecian architecture, and of the principles acknowledged in modern times as derived from it, and then applied those principles to certain parts of St. Paul's cathedral; at the same time strongly reprobating the species of tyranny which has resulted from judging a building of one kind by the rules and the taste which have been formed upon others altogether of a different nature.

Presents,

Presents, curious specimens, and new machinery were laid upon the library tables as usual.

Feb. 15.—The subject of the evening was Resonance, or the reciprocation of musical sounds. It was explained and illustrated by Mr. Faraday; but he stated that his information was obtained from Mr. Wheatstone, to whom belonged also the new matter brought forward.

The vibration of one string when another in unison with it was struck, was referred to as an illustration of the nature of reciprocation. The honour of this discovery was given to Messrs. Noble and Pigott, pupils of Dr. Wallace. Reciprocation by undulations communicated through the air, or through solid bodies, was then illustrated, and the nature of stringed instruments explained.

After this, followed matter of a more original nature. It was stated that columns of air could reciprocate to vibrating bodies, when their vibrations could accord with those of the latter; in illustration of which, a flute was made to speak, simply by approaching a vibrating tuning-fork to its embouchoir. Other columns of air were also made to produce intense sound by reciprocating with tuning-forks which were themselves inaudible. An elementary model of an instrument to be constructed upon this principle was also exhibited.

Some musical instruments were then referred to, which had been brought by Sir Stamford Raffles from Java; and which had been lent to the Institution by Lady Raffles, for the evening's illustrations. They consisted of plates of sonorous metal, suspended by their nodal points over pipes of bamboo adjusted to reciprocate to the lowest note produced when the plate was struck. When the apertures of the tubes were covered, and the plates made to sound, the ordinary tones were produced; but upon removing the cover, the air in the tubes instantly reciprocated to the lower notes, and a series of fine rich sounds were produced. This instrument is called the Génder.

A further illustration of the reciprocation of sound was then given on the guimbard or Jews-harp, the different tones of which are found to depend upon the circumstance, that columns of air reciprocate not only when they vibrate in equal times with the original phonic, but also when their vibrations are any multiple of those of the phonic. This fact was illustrated by a syringa, and also by the beautiful performance of M. Eulenstein on the Jews-harp.

Some illustrations of the resonance of the columns of air in the cavities of the ear were then given,—and the subject concluded.

In the library were several beautiful engravings, by Robinson, Turrell, and others. A new perspective instrument by Mr. Turrell, called a Perspectograph, &c. &c.

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## XXXVIII. *Intelligence and Miscellaneous Articles.*

### ANALYSIS OF SOME ALLOYS OF BISMUTH.

M. Laugier has analysed several of these compounds. To analyse an alloy of bismuth and lead, he dissolves it completely in dilute nitric acid, and then pours into the solution one of carbonate of ammonia,

monia, which at first precipitates both oxides in the state of carbonate; and when added in excess, redissolves the carbonate of bismuth. The carbonate of lead is to be washed in the filter with a solution of carbonate of ammonia, in order to dissolve any adhering carbonate of bismuth; the washing is to be finished with warm water, in order to dissolve all the carbonate of ammonia used in the washing.

The alkaline liquor containing the oxide of bismuth, is to be saturated by an acid, and then ammonia is to be added in excess; all the oxide of bismuth is precipitated, and after washing on a filter it is to be dried and weighed.

There is another method, which is perhaps more simple, but not so exact.—Boil the alkaline liquor and evaporate to dryness, wash it and filter in order to collect the oxide of bismuth. But this method is inconvenient, because the oxide of bismuth adheres to the vessel in which the solution is evaporated.

An alloy of equal weights of lead and bismuth gave by analysis 49·1 of the former metal, and 49·6 of the latter; the loss being 1·3 per cent. The nitrate of lead and oxide of bismuth separated by this process were both pure.

The fusible compound of 8 parts bismuth, 5 lead, and 3 tin, analysed with nitric acid, left the oxide of tin on the filter: the oxides of bismuth and lead were separated as above directed; 16 parts yielded bismuth 7·98, lead 4·95, tin 3; the loss of the operation amounting to $\frac{7}{100}$ of the whole.—*Ann. de Chim.* xxxvi. *p.* 333.

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### EXAMINATION OF COPPER.

Some samples of copper which I examined some time since, contained a small portion of silver, without any other impurity which I could detect: lately, some other samples have been put into my hands, in order to determine the cause of their being complained of. The copper was dissolved in dilute nitric acid, used in excess; it contained no lead, and upon the addition of muriatic acid to a very dilute solution, slight precipitation took place, which I at first imagined was occasioned, as in the former case, by the presence of silver. I happened, however, to remember a fact mentioned in conversation some years since, and which I have never met with in any chemical work,—That a solution of bismuth so dilute or so acid that water would occasion no precipitation in it, is decomposed by the addition of common salt or muriatic acid; and this I found to be the case in the present instance: the precipitate was small in quantity, not amounting to one per cent for the copper employed, and it differed from chloride of silver in very readily passing through filtering paper. I found it necessary, in order to determine its quantity, to supersaturate with ammonia, by which the oxide of copper was dissolved, and the oxide of bismuth precipitated. R. P.

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### ON EFFLORESCENCE.

The following observations are by M. Gay Lussac. Many salts when exposed to the air, are well known to effloresce; that is, they fall to powder and lose their water of crystallization; and it is generally supposed

supposed that salts after efflorescence are perfectly anhydrous. Having been long convinced that this opinion is not correct, I have made some experiments upon the principal salts which are efflorescent in a high degree. Crystallized sulphate of soda, exposed to the air even when it is not very dry, readily loses all its water of crystallization. Phosphate of soda becomes readily opake without losing its form. After three months' exposure to the air, it contained on the 18th of July, 7·4 of the 12 proportions of water, which it contains in its usual state. Reduced to powder, and thinly spread upon paper, it contained on the 26th of July 5·65 proportions of water;—again exposed to the air during a hot and dry period, it contained on the 31st of July only 5·65 proportions;—afterwards exposed till the 21st of October, the weather having become colder and more damp, it was found to contain 7·2 proportions of water: some phosphate which had been calcined, absorbed in five days' exposure to the air, nearly half a proportion of water.

Carbonate of soda behaves on exposure very much as the phosphate: it becomes opake, loses much water without altering its form; but I have never found it anhydrous after exposure.

It results from these observations, that some salts completely lose their water of crystallization by efflorescence; but that others retain variable quantities, according to the hygrometric state of the air. I do not assert, however, that the water may not remain in definite proportions; it merely appears that in the phosphate and carbonate of soda, which retains a proportion of water of a certain number,—the seventh for example,—differs but little from that which unites the proportion, immediately above or below.—*Ann. de Chim.* xxxvi. 335.

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## NATIVE PLATINA.

A piece of native platina, weighing about 64 grains, has recently been found in the Russian mines at Hijne-Taguilski. Its shape was round, its surface granulated, and in some places it bore a metallic lustre. Its specific gravity being only 16, it must contain the various alloys met with in platina. It is singular that this specimen was met with in digging an argillaceous stratum.—*Monthly Mag. Feb.* 1828.

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## MANUFACTURE OF ULTRAMARINE.

M. Gay Lussac announced to the Academy that M. Tunel, inspector of gunpowder and saltpetre, had succeeded in the direct formation of ultramarine, and that what he obtains by his process is finer and more brilliant than the natural colour. It was by following the analysis made by M. Clement Désormes, that the inventor accomplished this desirable object. M. Tunel has already been able to supply the public with ultramarine at 25 francs the ounce; the colour having hitherto been sold for 50 or 60 francs the ounce. He hopes that he shall be able to sell it at a still more moderate price. M. Tunel has thought proper to keep his process secret for a certain time.—*Le Globe, Fev.* 9, 1828.

### HEAT GIVEN OUT DURING COMBUSTION.

M. Despretz read at the Academy of Sciences, on the 15th and 22d of October last, a memoir on the heat given out during combustion. By means of a new method of observation, he found that hydrogen is the body, of which a given weight gives out most heat, and the metals least. The result will be opposite if we refer the results to the same weight of oxygen. It is remarkable that carbon, which in burning does not alter the volume of oxygen gas, produces three-fifths of the heat developed by the metals, iron, zinc, and tin, which reduce the oxygen to the solid state. Hence it is in the act of combination that we must seek for the principal cause of the development of heat, and not in the approach of particles.

In his second memoir, M. Despretz has shown that the quantity of heat developed by a certain quantity of a body which burns without changing the volume of the gas, is the same, whatever be the density of the gas.—*Le Globe.*

### INFLAMMABLE GAS ARISING AFTER BORING FOR SALT.

In boring for salt at Rocky Hill, in Ohio, about a mile and a half from Lake Erie, after proceeding to the depth of 197 feet, the auger fell, and salt water spouted out for several hours. After the exhaustion of this water, great volumes of inflammable air issued through the aperture for a long time, and formed a cloud; and by ignition, occasioned by the fire in the shops of the workmen, consumed and destroyed every thing in the vicinity.—*Transactions of the Philosophical Society of New York.*

### INFLAMMABLE GAS FROM SALT MINES EMPLOYED FOR PRODUCING LIGHT.

In the salt mine of Gottesgabe at Rheine, in the county of Tecklenbourg, there has issued for sixty years from one of the pits, (which has on this account been called the *Pit of the Wind,*) a continued current of inflammable gas. The same gas is produced in other parts of the mines. M. Rœders, the inspector of the salt mines, has used this gas for two years, not only as a light, but as fuel for all the purposes of cookery. He collects it in pits that are no longer worked, and conveys it in tubes to the house. It burns with a white and brilliant flame. Its density is about 0·66. It contains only traces of carbonic acid and sulphuretted hydrogen, and therefore should consist of carbonated hydrogen and olefiant gas.—*Brewster's Journal, Jan.* 1828.

### NATURAL GAS LIGHTS AT FREDONEA.

This village, on the shores of Lake Erie, is lighted every night by inflammable gas from the burning springs, as they are called, in its vicinity. Captain Hall has visited this village, and will no doubt give us a good account of it on his return.—*Ibid.*

### IODINE IN CADMIUM.

Iodine is found in the great zinc foundry at Königshute in Upper Silesia, in the cadmium which accompanies the zinc ores.—*Ibid.*

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### ANALYSIS OF THE GREEN IRON ORE AND ARSENIATE OF LEAD.

According to Dr. Karsten, a variety of this ore from the Hollester mines near Siegen, in Rhein Prussia, consists of

| | |
|---|---|
| Oxide of iron ...... | 63·450 |
| Phosphoric acid .... | 27·717 |
| Water ............ | 8·560 |
| | 99·727 |

And according to the same authority, a variety of arseniate of lead from Herrhausen near Siegen, in Rhein Prussia, consists of

| | |
|---|---|
| Oxide of lead ...... | 69·97 |
| Muriatic acid ...... | 0·81 |
| Arsenic acid ...... | 29·22 |
| | 100·00 |

The results of this analysis are not accordant with those obtained by Dr. Wohler.—*Poggendorff's Annalen,* vol. iv. p. 161.

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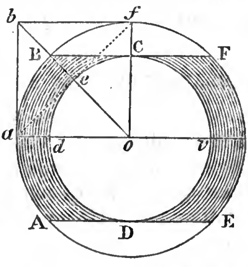
### GEOMETRICAL PROBLEM. BY MR. J. ROBOTHAM.

Required, a square equal in area to the space comprised between the arcs [AB, DC, and EF, DC,] of two concentric circles, and two parallel right lines, tangents to the interior circle. The area of the exterior circle being double that of the interior.




Let ABFE, and $d\,C\,v\,D$ be the two concentric circles. Bisect the quadrant $a\,o\,f$ by the line $o$B. Then by the nature of the problem, the area $a\,B\,d\,e = d\,e\,o = e\,o\,C$. To each of these equals add the area $e$ BC, then will the area of the space $a\,BC\,d =$ the area of the triangle $o$BC. And since $o\,a = o$B, the square, described on $o\,a$, viz. $a\,o\,f\,b = 4$ times the area of the triangle $o$BC = the space AB, DC, and EF, DC. Which may be seen by drawingthe diagonal $a\,f$.

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### SCIENTIFIC BOOKS.

*Just Published.*

The 19th Number of Leybourn's Mathematical Repository; containing twenty mathematical questions, and their answers, selected from an extensive correspondence.—Horæ Arithmeticæ, by Mr. Horner.—On Porisms, by Mr. Galloway.—On Central Forces, by the Rev. Mr. Bromhead.—On Equations, by the Rev. Mr. Hawkes. —On

—On Attraction, by Ch. Fred. Gauss.—Solutions to a curious Problem, by Mr. Lechmutz.—Cambridge Problems, 1821—1826. Together with a set of twenty questions, to be answered in Number XXI.

A Manual of Natural and Experimental Philosophy, beautifully illustrated, has just been published by C. F. Partington, and dedicated to the London Institution. It is well calculated to assist those who attend the Lectures, and not less so to give those who cannot attend a vast mass of useful information.

Observations on the Cruelty of employing Climbing-boys in sweeping Chimneys: and on the Practicability of effectually Cleansing Flues by Mechanical Means. With Extracts from the Evidence before the House of Commons: &c. &c.

*Flora Londinensis.* Nos. 35 and 36 of the New Series.—These Numbers, containing Twelve rare and highly interesting British Plants, complete the new edition of this splendid Work, which contains upwards of 650 accurate Delineations of Indigenous Plants.

A Manual of Electro-Dynamics, chiefly translated from the Manual d'Electricité Dynamique; or, A Treatise on the Mutual Action of Electric Conductors and Magnets of J. F. Demonferrand; with Notes, &c. &c. by James Cumming, M.A. 8vo.

Reports of Medical Cases, selected with a view of illustrating the Symptoms and Cure of Diseases by a reference to Morbid Anatomy; embracing Dropsy, Inflammation of the Lungs, Phthisis, and Fever. By Richard Bright, M.D. F.R.S. &c. 4to. with 16 coloured plates.

Popular Lectures on the Steam Engine, in which its Construction and Operation are familiarly explained; with an Historical Sketch of its Invention and Progressive Improvement. By the Rev. Dionysius Lardner, LL.D. 12mo.

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## *Preparing for Publication.*

Capt. John Ross, K. S. R. N., is about to publish a Treatise on Steam Navigation, Illustrated with Plates, comprehending a History of the Steam-Engine, its Principles, an Explanation of the Terms, and an Account and Examination of the Improvements applicable to Naval Purposes.—A Complete System of Naval Tactics peculiar to Steam Navigation as applicable to Naval Warfare and Commerce, including a comparison of this System with all others.—The Effects of such a System in National Defence.—Rules and Regulations calculated to prevent Accidents by Explosion and Collision.—An Alphabetical Arrangement of Technical Terms used in Steam Navigation; and a Chronological Table of the Progress of the Steam Engine, from the most ancient period of its invention.—The Description and Use of the Royal Clarence Sextant, an Instrument recently invented by the Author, and highly useful in Steam Navigation, for ascertaining the exact distance of Ships from Land, or any object in view: with Tables applying the principle to a Common Sextant.

An Essay on the Application of Mathematical Analysis to the

 Theories

Theories of Electricity and Magnetism.—This Essay will commence with an Exposition of the General Principles common to both Theories; which will be followed by particular Applications of them to many cases not hitherto submitted to Calculation.

In the course of the present month will be published by W. Wood, 428 Strand, the 4th and concluding Part of Haworth's *Lepidoptera Britannica*, with a complete Index to all the species.

Also, the second edition of Wood's *Index Testaceologicus*, or Catalogue of English and Foreign Shells, illustrated with 2300 Figures.

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LIST OF NEW PATENTS.

To J. Weiss, of the Strand, for improvements on instruments for bleeding horses and other animals.—Dated the 26th of January 1828.—6 months allowed to enrol specification.

To Augustus Applegath, of Crayford, Kent, for his improvements in block printing.—26th of January.—4 months.

To Donald Currie, of Regent-street, esquire, for a method of preserving grain and other vegetable and animal substances and liquids. Communicated from abroad.—31st of January.—6 months.

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*Summary, for the Year* 1827, *of the State of the Barometer, Thermometer, &c. in Kendal. By* S. MARSHALL, *Esq.*

| 1827. | Barometer. | | | Thermometer. | | | Quantity of Rain in Inches. | No. of rainy Days. | Prevalent Winds. |
|---|---|---|---|---|---|---|---|---|---|
| | Max. | Min. | Mean. | Max. | Min. | Mean. | | | |
| January | 30·08 | 28·89 | 29·61 | 49° | 9° | 34·59° | 8·630 | 14 | SW. |
| February | 30·40 | 28·89 | 29·51 | 53 | 14 | 33·92 | 2·698 | 5 | N. |
| March | 30·03 | 28·40 | 29·36 | 56 | 23 | 42·93 | 8·676 | 22 | SW. |
| April | 30·13 | 29·42 | 29·77 | 70 | 30 | 47·23 | 2·553 | 14 | SW. |
| May | 29·94 | 29·10 | 29·56 | 69 | 32 | 52·87 | 3·483 | 15 | W. |
| June | 30·14 | 29·27 | 29·69 | 74 | 45 | 56·98 | 4·264 | 17 | W.&SW. |
| July | 30·15 | 29·39 | 29·79 | 74 | 42 | 59·01 | 3·170 | 15 | W. |
| August | 30·18 | 29·10 | 29·81 | 68 | 46 | 56·61 | 5·214 | 12 | NW. |
| Septemb. | 30·20 | 29·14 | 29·78 | 68 | 41 | 55·55 | 3·329 | 16 | SW. |
| October | 30·18 | 28·92 | 29·54 | 63 | 34 | 51·95 | 3·009 | 17 | SW. |
| Novemb. | 30·05 | 29·09 | 29·78 | 55 | 21 | 42·30 | 2·615 | 9 | W. |
| Decemb. | 30·46 | 28·57 | 29·47 | 54 | 24 | 42·53 | 10·365 | 23 | SW. |
| Average | | | 29·63 | | | 48·03 | 58·006 | 179 | |

In comparing the preceding summary with that for 1826, it appears that the barometer has not reached the altitude which it then attained, 30·78, and the mean height for the year is one tenth of an inch less. The heat of the summer months has not equalled that of 1826, the greatest being 74°; whilst in 1826 the maximum was 85°, and the mean 47°·81. The superior mean temperature for 1827 may be accounted for by the weather's being more uniformly mild. From 26th of April to 21st of November, (or a period of nearly seven months) the thermometer was never so low as the freezing point; and from the former date to the end of the year, there have been but eleven

eleven days of frost. We have had thirty-two wet days more in this year than in 1826, and the quantity of rain is greater by 14·926 inches. The writer of these remarks has carefully registered observations on the weather in this town for upwards of five years. He subjoins a summary from 1823 to 1827, both years included. From observations made by the late John Gough, and by John Dalton of Manchester, he inferred the mean annual quantity of rain for Kendal was 51·8 inches. The average for the five years alluded to, will be found to be 57·310 inches. The difference may arise from two causes: —one, the difference of altitude in the places where the observations were made, above the level of the sea; but though that does not exceed many yards, yet even so small a difference will affect the amount of the mean in a series of years. The observations from which the former *mean* was calculated, were taken from twenty different years, though not twenty successive years. Assuming all the observations to be equally correct, the mean deduced from the twenty years is most likely to be the correct one. There are few places where rain-gauges are kept, that have so great a quantity of rain as at Kendal, though it is probable that in many places there are more rainy days within the same period. From a number of observations now in my possession, the annual mean quantity of rain which falls in England, may be stated at 35·2 inches.

| Years. | Mean of Barometer. | Mean of Thermometer. | Inches of Rain. | No. of Rainy Days. | Prevalent Winds. |
|---|---|---|---|---|---|
| 1822 | | | 62·726 | | |
| 1823 | 29·56 | 45°00 | 62·749 | 198 | |
| 1824 | 29·76 | 46·88 | 62·762 | 187 | SW. |
| 1825 | 29·64 | 47·49 | 59·973 | 169 | SW. |
| 1826 | 29·73 | 47·81 | 43·060 | 147 | SW. |
| 1827 | 29·63 | 48·03 | 58·006 | 179 | SW. |
| Mean | 29·66 | 47·04 | 57·310 | 176 | SW. |

## METEOROLOGICAL OBSERVATIONS FOR JANUARY 1828.

### *Gosport.—Numerical Results for the Month.*

Barom. Max. 30·40 Jan. 27. Wind N.W.—Min. 29·17 Jan. 13. Wind S.W.
Range of the mercury 1·23.
Mean barometrical pressure for the month .......................... 29·895
Spaces described by the rising and falling of the mercury............ 6·740
Greatest variation in 24 hours 0·530.—Number of changes 22.
Therm. Max. 56° Jan. 18 and 25. Wind SW.—Min. 29° Jan. 9. Wind NE.
Range 27°.—Mean temp. of exter. air 44°·95. For 31 days with ☉ in ♑ 44·05
Max. var. in 24 hours 17°·00—Mean temp. of spring water at 8 A.M. 52°·57

### De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air several times during the month....... 100°
Greatest dryness of the air in the afternoon of the 27th ............ 63
Range of the index.......................................................... 37
Mean at 2 P.M. 80°·8—Mean at 8 A.M. 87°·4—Mean at 8 P.M. 89·8
—— of three observations each day at 8, 2, and 8 o'clock......... 86·0
Evaporation for the month 0·60 inch.

Rain

Rain near ground 6·710 inch.—Rain 23 feet high 6·135 inch.
Prevaling Wind S.W.

*Summary of the Weather.*

A clear sky, 2; fine, with various modifications of clouds, 9; an overcast sky without rain, 11½; foggy 1; rain, 7½.—Total 31 days.

*Clouds.*

| Cirrus. | Cirrocumulus. | Cirrostratus. | Stratus. | Cumulus. | Cumulostr. | Nimbus. |
|---|---|---|---|---|---|---|
| 16 | 8 | 30 | 1 | 8 | 9 | 19 |

*Scale of the prevailing Winds.*

| N. | N.E. | E. | S.E. | S. | S.W. | W. | N.W. | Days. |
|---|---|---|---|---|---|---|---|---|
| ½ | 3½ | 3½ | 6 | 4 | 7 | 4 | 2½ | 31 |

*General Observations.*—The first part of this month to the 17th was very wet, and generally stormy, 6·385 inches of rain having fallen here; the latter part was dry, with a humid atmosphere. From the 2nd of December last to the 17th instant, upwards of a perpendicular foot of rain fell here, and during that period heavy rain frequently fell throughout England, but chiefly along the southern shores; also along the shores of Ireland and Scotland, in consequence of the S.W. winds and gales having prevailed two-thirds of the time, and having often been crossed by upper winds: the result was an overflowing of the rivers, and a general inundation of the low lands. Even the level roads and lands near the hilly districts in Hampshire, and the adjoining counties, lay under water several days about the middle of the month, which rendered them impassable to foot-passengers.

On the 1st instant, 1·89 inch of rain fell between the hours of 8 A.M. and 6 P.M., accompanied with a brisk gale from the S.E. in the morning, and a very hard gale from the North throughout the afternoon and night. That depth of rain in so short a time is with us unprecedented; but nearly the same depth fell here in 24 hours on the 29th of August 1821.—A more violent gale and rain in the afternoon, (as the latter frequently came down in torrents), have seldom been experienced: and in consequence of the then highly saturated state of the ground, the level roads and fields about the town and neighbourhood lay under water several hours, and carriages were prevented from travelling in the roads till the rush of water from the higher ground had subsided. On the morning of the 5th, icy efflorescences appeared on the inside of the glass windows; and at mid-day a bright parhelion was observed on the eastern side of the sun, 22½ degrees distant from his centre, From the 6th to the 10th the atmosphere presented a snowy appearance, and on the latter day one inch and a half of snow fell here, with a gale from the North-east. Early in the afternoon of the 11th a thick fog came on, but was soon dispersed by a change of wind to the West; and from a sudden rise of temperature the ice and snow were dissolved in four or five hours. While the thaw was thus rapidly going on, and immediately after the fog cleared away, a dense *stratus*, two or three feet in height from the ground, formed in the town and its vicinity, which viewed an hour before sunset from a height of twenty feet, had an unusually whitish appearance, like smoke from a tobacco-pipe. It was no doubt produced so suddenly, by the heat from the ground communicating with the lower atmosphere, whose temperature was only 40 degrees; while the heat of the ground, which had been kept down by the ice and snow, was 13 degrees higher. A wet fog prevailed throughout the day of the 12th: its temperature on the ground at 9 A.M. was 40 degrees; at three feet high, 43 degrees; and at nine and twelve feet high, 46 degrees. At midnight

midnight vivid lightning appeared in the W.S.W. horizon, preceded by two winds crossing each other, the upper one from that point, and the lower one from the South-east. Several black thunder-clouds also appeared at the same time at a great distance to the westward, which, with a rapid fall of the quicksilver in the barometer, indicated an approaching storm. The thunder-storm and hurricane are said to have come on at Plymouth soon after midnight, and to have lasted three hours with increasing violence, so that not only the ships and vessels at that port received considerable damage, but those in the Atlantic Ocean, over which the hurricane came, as since ascertained. At 4 o'clock the following morning a very heavy gale from the S.W. was felt here, accompanied with heavy rain, vivid lightning, and long peals of thunder, from half-past five till half-past six: and in three hours afterward the storm was very awful at Dover, and severely felt in the Straits; so that it was more or less violent along the whole southern coast. It is remarkable that the morning tide here, and at Plymouth, on this occasion, was very nearly as high as any of the following spring tides, notwithstanding it was a neap tide.

The mean temperature of the external air this month is unprecedentedly high; indeed the mean of March in several preceding years was much lower, and it certainly felt more like a spring than a winter month, which has been verified in some measure by the unusual appearance of spring flowers. This may be justly attributed to three co-operating circumstances, viz. the prevalence of warm winds, as may be seen by the above scale; the wet state of the ground, and the consequent humidity of the contiguous air; and the uncommonly high temperature of the ground, from there having been but little frost this winter to diminish its heat. The *maximum* temperature of the air has occurred four times by night instead of in the day.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are two parhelia, one lunar halo, two meteors, and eleven gales of wind, or days on which they have prevailed; namely, one from the North, five from the North-east, two from the South-east, and three from South-west.

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## REMARKS.

*London.*—Jan. 1. Fair. 2. Foggy. 3. Fair. 4. Rain. 5. Snow. 6—9. Cloudy. 10. Fine. 11. Snow. 12. Fog. 13—15. Cloudy. 16. Snow. 17, 18. Rain. 19—21. Fine. 22. Cloudy. 23. Fine. 24. Cloudy. 25. Rain. 27. Fine. 28. Foggy, 29. Rain. 30, 31. Fine.

*Penzance.*—Jan. 1. Heavy rain: fair. 2. Fair. 3. Rain: hail-showers. 4. Hail-showers. 5. Fair: rain. 6. Clear. 7. Rain: blowing strong. 8. Fair. 9. Fair: rain. 10. Rain. 11. Clear. 12. Cloudy: rain: stormy at night. 13. Rain. 14. Cloudy: rain. 15. Rain. 16. Clear: rain. 17. Rain. 18. Rain: clear. 19. Fair. 20, 21, Clear. 22. Cloudy: clear. 23. Clear: misty. 24. Rain. 25. Rain: clear. 26. Clear: 27, 28. Fair. 29, 30. Clear. 31. Rain.—Rain-gauge ground level.

*Boston.*—Jan. 1. Rain. 2. Fine: rain P.M. 3. Fine. 4. Fine: rain P.M. 5—8. Cloudy. 9. Cloudy: snow P.M. 10. Fine. 11. Snow. 12. Fine: rain early A.M. 13. Rain. 14. Cloudy: rain P.M. 15, 16. Cloudy. 17. Rain and stormy. 18. Rain. 19—23. Fine. 24. Cloudy: rain A.M. 25—28. Fine. 29. Cloudy. 30. Fine. 31. Cloudy.

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*Owing to the Discontinuance of* Mr. Howard's *Observations, our Meteorological Table has some deficiencies, which we hope to supply in our next.*

*Meteoro-*

Meteorological Observations by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

| Days of Month, 1828. | Barometer. | | | | | | | Thermometer. | | | | | | | Wind. | | | | Evapor. | | Rain. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | London. | | Penzance. | | Gosport. | | Boston | London. | | Penzance. | | Gosport. | | Bost. | Lond. | Penz. | Gosp. | Bost. | Lond. | Gosp. | Lond. | Penz. | Gosp. | Bost. |
| | 9 A.M. | 10 P.M. | Max. | Min. | Max. | Min. | 8½ A.M. | Max. | Min. | Max. | Min. | Max. | Min. | 8¼ A.M. | | | | | | | | | | |
| Jan. 1 | 29·48 | 29·60 | 29·60 | 29·26 | 29·63 | 29·23 | 29·22 | 47 | 36 | 50 | 47 | 52 | 37 | 43·5 | NE. | NE. | SE. | calm | | ... | | 0·880 | 1·890 | 0·22 |
| ○ 2 | 29·77 | 29·41 | 29·80 | 29·50 | 29·83 | 29·48 | 29·53 | 50 | 45 | 48 | 41 | 51 | 45 | 36 | N. | W. | SW. | calm | | ... | | ... | ·400 | 0·13 |
| 3 | 29·56 | 29·60 | 29·80 | 29·50 | 29·75 | 29·42 | 29·03 | 50 | 34 | 50 | 44 | 48 | 36 | 43·5 | NW. | NW. | NW. | NW. | | 0·05 | | 0·725 | ... | ... |
| 4 | 29·73 | 29·52 | 29·70 | 29·70 | 29·80 | 29·60 | 29·97 | 44 | 33 | 46 | 42 | 46 | 33 | 34 | W. | NW. | S. | NW. | | ... | | ... | ·315 | ... |
| 5 | 29·52 | 29·53 | 29·70 | 29·70 | 29·59 | 29·58 | 29·25 | 35 | 34 | 45 | 41 | 41 | 34 | 38 | W. | W. | NW. | NW. | | ... | | ... | ·020 | ·15 |
| 6 | 29·54 | 29·81 | 29·70 | 29·64 | 29·78 | 29·64 | 29·47 | 39 | 33 | 45 | 34 | 40 | 35 | 37 | W. | W. | NE. | NW. | | ... | | ... | ·020 | ... |
| 7 | 29·86 | 29·74 | 29·50 | 29·50 | 29·83 | 29·75 | 29·76 | 36 | 33 | 45 | 40 | 42 | 39 | 34·5 | SE. | SE. | SE. | calm | | ... | | ... | ... | ... |
| 8 | 29·75 | 29·85 | 29·70 | 29·64 | 29·85 | 29·76 | 29·70 | 38 | 33 | 45 | 42 | 40 | 33 | 37 | SE. | N. | E. | E. | | ... | | 1·020 | ·020 | ... |
| 9 | 29·85 | 29·77 | 29·73 | 29·72 | 29·90 | 29·84 | 29·82 | 35 | 29 | 44 | 40 | 35 | 29 | 35 | SE. | SE. | E. | E. | | ·06 | | ... | ... | ... |
| ☾ 10 | 29·57 | 29·52 | 29·56 | 29·40 | 29·64 | 29·52 | 29·47 | 34 | 28 | 52 | 38 | 37 | 31 | 16·5 | N. | SE. | NE. | calm | | ... | | 0·725 | ·340 | ... |
| 11 | 29·41 | 29·47 | 29·40 | 29·34 | 29·56 | 29·43 | 29·40 | 40 | 36 | 54 | 40 | 48 | 40 | 22 | SE. | W. | E. | calm | | ... | | ... | ·010 | ... |
| 12 | 29·63 | 29·51 | 29·60 | 29·40 | 29·70 | 29·50 | 29·43 | 46 | 42 | 52 | 45 | 51 | 46 | 36 | SW. | SW. | SE. | SW. | | ·04 | | ... | ·800 | ·37 |
| 13 | 29·45 | 29·45 | 29·50 | 29·20 | 29·53 | 29·17 | 28·85 | 47 | 38 | 48 | 45 | 50 | 42 | 43 | W. | NW. | W. | calm | | ... | | 1·250 | ·525 | ·58 |
| 14 | 29·37 | 29·40 | 29·50 | 29·40 | 29·52 | 29·40 | 29·22 | 45 | 35 | 47 | 45 | 47 | 39 | 39 | NW. | N. | SE. | calm | | ... | | 0·885 | ·155 | ·10 |
| 15 | 29·48 | 29·61 | 29·50 | 29·50 | 29·70 | 29·63 | 29·39 | 37 | 31 | 49 | 42 | 42 | 38 | 34 | NE. | W. | NE. | E. | | ... | | 0·430 | 1·120 | ·05 |
| 16 | 29·68 | 29·68 | 29·52 | 29·50 | 29·76 | 29·63 | 29·57 | 40 | 38 | 52 | 44 | 50 | 46 | 32·5 | NE. | S. | E. | E. | | ... | | 0·180 | ·380 | ... |
| ● 17 | 29·68 | 29·93 | 29·74 | 29·60 | 29·95 | 29·84 | 29·70 | 50 | 49 | 54 | 48 | 53 | 50 | 35 | SW. | W. | SE. | E. | | ... | | 0·400 | ·390 | ·50 |
| 18 | 30·04 | 30·06 | 30·00 | 29·90 | 30·14 | 30·07 | 29·66 | 56 | 46 | 55 | 50 | 56 | 45 | 49 | SW. | W. | SW. | calm | | ·10 | | 0·220 | ... | ·47 |
| 19 | 30·13 | 30·13 | 30·06 | 30·04 | 30·22 | 30·20 | 29·70 | 55 | 47 | 54 | 48 | 55 | 47 | 47 | SW. | W. | SW. | W. | | ... | | ... | ... | ... |
| 20 | | | 30·10 | 30·08 | 30·28 | 30·26 | 29·75 | | | 53 | 49 | 52 | 42 | 47 | | W. | SW. | calm | | ... | | ... | ... | ... |
| 21 | | | 30·10 | 30·08 | 30·21 | 30·19 | 29·77 | | | 55 | 48 | 54 | 43 | 43·5 | SW. | W. | SE. | calm | | ·08 | | ... | ... | ... |
| 22 | | | 30·12 | 30·08 | 30·20 | 30·11 | 29·58 | | | 56 | 49 | 55 | 43 | 48 | SW. | W. | SW. | W. | | ... | | ... | ... | ... |
| ☽ 23 | | | 30·20 | 30·20 | 30·35 | 30·26 | 29·72 | | | 52 | 47 | 54 | 42 | 46·5 | W. | W. | W. | calm | | ... | | ... | ... | ·03 |
| 24 | | | 30 20 | 30·18 | 30·35 | 30·31 | 29·85 | | | 52 | 47 | 55 | 49 | 43 | SW. | SW. | SW. | calm | | ·10 | | 0·240 | ... | ... |
| 25 | | | 30·20 | 30·18 | 30·19 | 30·10 | 29·70 | | | 52 | 47 | 56 | 42 | 48 | SW. | W. | SW. | W. | | ... | | 0·160 | ·110 | ... |
| 26 | | | 30·25 | 30·24 | 30·38 | 30·26 | 29·74 | | | 51 | 45 | 51 | 45 | 41·5 | SW. | NW. | W. | W. | | ... | | ... | ... | ... |
| 27 | | | 30·40 | 30·35 | 30·40 | 30·38 | 29·84 | | | 48 | 46 | 53 | 39 | 48 | | NW. | NW. | W. | | ·07 | | ... | ... | ... |
| 28 | | | 30·27 | 30·12 | 30·36 | 30·28 | 29·96 | | | 50 | 44 | 52 | 45 | 36 | SW. | S. | S. | calm | | ... | | ... | ... | ... |
| 29 | | | 30·06 | 30·00 | 30·17 | 30·13 | 29·81 | | | 53 | 45 | 52 | 41 | 42 | W. | S. | S. | calm | | ... | | ... | ... | ... |
| 30 | | | 30·00 | 29·94 | 30·05 | 29·98 | 29·70 | | | 50 | 46 | 50 | 42 | 40 | SW. | SW. | SE. | calm | | ... | | ... | ... | ... |
| 31 | | | 29·86 | 29·84 | 29·96 | 29·94 | 29·53 | | | 52 | 47 | 52 | 49 | 44 | SW. | SW. | SW. | S. | | ·10 | | 0·340 | ·215 | ... |
| Aver.: | | | 30·40 | 29·20 | 30·40 | 29·17 | 30·22 | | | 55 | 34 | 56 | 29 | 39·3 | | | | | | 0·60 | | 7·455 | 6·710 | 2 60 |

THE

# PHILOSOPHICAL MAGAZINE

AND

# ANNALS OF PHILOSOPHY.

[NEW SERIES.]

*APRIL* 1828.

## XXXIX. *A Letter to the Editors relating to the Ellipticity of the Earth as deduced from Experiments with the Pendulum. By* J. Ivory, *M.A. F.R.S.*

Gentlemen,

IN examining the calculations in the articles relative to the ellipticity of the earth, inserted in the last Number of this Journal, I have detected a small arithmetical error, which, although of little moment, it may be proper to correct. The equation at the bottom of p. 170, viz.

$$2{\cdot}973\,\tau = -{\cdot}00061,$$

should be, $$2{\cdot}973\,\tau = -{\cdot}00033:$$

consequently, $\tau = -{\cdot}00011, \quad s = +00005,$

$$f = {\cdot}2056, \qquad \Delta = {\cdot}01335;$$

and the formula for the length of the pendulum,

$$l = 39{\cdot}01335 + {\cdot}2056 \sin^2 \lambda.$$

But the error just corrected is so small as not to affect either the reasoning deduced from the formula at the top of p. 171, or any of the results obtained by it.

It is directed in p. 207, that the two approximate quantities $a$ and $b$, be so taken as to satisfy equation (A): but it is obvious that, although this be convenient, it is not necessary in the method of computation I have followed. Nothing more is required than that $a$ and $b$ nearly satisfy equation (A); in which case the right-hand side of equation (B), will not be equal to zero, but to a small known quantity positive or negative,—a circumstance which makes no essential difference in finding the corrections $s$ and $\tau$.

From the calculations which have been made, we may infer that

that no great confidence can be placed in the ellipticity deduced from any partial combination of the experiments with the pendulum: for we have found that there is considerable uncertainty in the value of that element, even when we avail ourselves of all the experiments we at present possess. This matter will be placed in a clear light by setting before the reader, the values of $\Delta$, or of the excess of the pendulum at the equator above 39 inches, which we have obtained by taking different means of the tropical experiments, viz.

| $\Delta$ | | |
|---|---|---|
| ·01230 | mean of | 6 experiments. |
| ·01330 | | 9 |
| ·01605 | | 15 |

The true value of the equatorial pendulum is therefore very uncertain. If we take the first mean of six tropical experiments, and neglect the other nine, we shall obtain, by combining the 31 remaining experiments, a formula for the length of the pendulum coinciding very nearly with that published in this Journal for October 1826, the ellipticity being about $\frac{1}{300}$. The other two means are the bases of the calculations in the two papers inserted in the last Number of this Journal. A strict scrutiny of the experiments would perhaps bear us out in adopting $\frac{1}{300}$ as the ellipticity that best agreed with the bulk of them; but any ellipticity between $\frac{1}{300}$ and $\frac{1}{296}$ will represent sufficiently well 34 out of the 40 experiments in our possession. The ellipticity $\frac{1}{289}$ does not represent with much accuracy, either the six anomalous experiments, or the remaining 34; and in the present state of our knowledge, we may consider it a thing nearly demonstrated, that this value is too great.

What is most remarkable in the experiments with the pendulum, is their excessive irregularity near the equator. This is well illustrated by the four values of $\Delta$ following, which are experimental quantities, not the results of calculation, viz.

| | Longitude. | $\Delta$ |
|---|---|---|
| Maranham ......... | 44° 21′ W. | ·01173 |
| Rawak............... | 131 1 E. | ·01479 |
| Galapagos ......... | 90 0 W. | ·01717 |
| St. Thomas......... | 6 45 E. | ·02074 |

The longitudes are set down, as all the stations are so near the equator, that they may be reckoned upon it. If the same irregularity shall be found to prevail upon all the parallels, it must be acknowledged that the figure of the earth, deduced from such incoherent data, will lose much of the interest and utility, which are usually attached to it. But no such irregu-

larity

larity has hitherto been experienced beyond the tropics; and even between the tropics there are only a few stations at which so great an excess of gravity has been observed as excludes them from belonging to the same surface as the other experiments. I apprehend it cannot be said at present that the great anomalies alluded to, are so well ascertained as to render any further inquiry unnecessary in regard to their exact quantity, or to the causes which produce them: and the determination of these points in preference to any other, seems to claim the attention of the experimenter; because it must fix in a great degree the complexion of the whole theory. Can we hope to determine an elliptical surface that will represent all the experiments within the limits of the probable errors of observation? Or, must we be content with a mean elliptical figure liable to great discrepancies? These seem at present to be the most interesting questions in this research. The least attention to the position of the anomalous stations on the surface of the globe, will prove how fruitless it would be, to suppose that the experiments can be better represented by a figure different from the elliptical spheroid. Far less can we expect, in the present state of our knowledge, to attain any useful purpose by pushing the theoretical solution of the figure of the earth to quantities of the second order; because the corrections thus introduced must be ultimately determined by the experiments themselves, the uncertainties of which greatly surpass the quantities to be found.

I remain, Gentlemen,

Your obedient servant,

March 12, 1828. J. IVORY.

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## XL. *Remarks on the Geology of the North Side of the Vale of Pickering.* By JOHN PHILLIPS, *F.G.S., Keeper of the Museum of the Yorkshire Philosophical Society.**

[With an Engraving.]

THE principal object of this communication is to explain some of the peculiar appearances which are exhibited along the southern edge of the oolitic hills which margin the Vale of Pickering from Seamer to Helmsley.

My attention was first drawn to a part of this country by a visit to Kirkdale Cave, in company with Mr. Salmond and Mr. Smith, in March 1824. In August of the same year I had further opportunity of examining with the aid of a barometer, the whole line during my walk from York to Scarborough.

* Read to the Yorkshire Philosophical Society, Jan. 1, 1828; and communicated by the Author.

In September 1825, I again traversed the line from Craike by Wass Bank to Helmsley; in September 1826, the Rev. W. V. Vernon and myself twice observed with attention the country about Ebberston and Snainton; and in October 1827, I very carefully reviewed with a barometer in my hand the whole line from Scarborough to Brandsby.

The roads from Craike and Brandsby to Helmsley, and from Helmsley to Scarborough, are lines remarkably well adapted for inquiring into the stratification of the country in question, and slight deviations to the right and left are easily made.

Some results of these several investigations I now beg to submit to the consideration of the Society.

Four strata or formations as usually enumerated, are seen along the north side of the Vale of Pickering.

Kimmeridge clay.
Coralline oolite.
Calcareous grit.
Oxford clay.

Of these the Kimmeridge clay, coralline oolite, and calcareous grit, may be considered as well identified by imbedded fossils with the strata bearing those names in the midland and southern parts of England. The Oxford clay is defined by its position between the well identified calcareous grit and Kelloways rock, and contains besides some ammonites which locally serve to characterize it.

*Kimmeridge Clay.*—It was in 1824 that I first had the pleasure of finding in the Kimmeridge clay, where it had been exposed by cutting the road at the east end of Kirby Moorside, plenty of characteristic pieces of *Ostrea deltoidea*, a fossil which Mr. Smith and myself had always considered to mark this stratum. The clay here is thin, contains minute layers of brown and white sandy lumps, in the upper part of which lies an ammonite like *A. plicomphalus* (Min. Conch.), and covers brown sandy stone incumbent on coralline oolite. The clay rises to the north, and forms the little insular hills which have recently afforded new examples of *Ostrea deltoidea*. From Kirby to Sinnington it retires to the south of the road, but beyond Sinnington forms a hill nearly a hundred and twenty feet high above the stream which there exposes the uppermost beds of the oolitic series beneath. From this point its course is wholly on the south of the road, but so much concealed by the alluvial and diluvial covering, that it would be a difficult matter to represent it on a map with even tolerable accuracy. It is seen on the coast at intervals from Filey to the Speeton Cliffs; but *Ostrea deltoidea* has never yet been found there. Unless at about one mile east of Helmsley, the Kimmeridge clay lies wholly

wholly to the south of the road from Kirby Moorside to Sproxton, one mile south of Helmsley. In this clay hill I found no opportunity of searching for fossils, but about a mile further west, where the road crosses a low insulated woody hill, I had the satisfaction of finding, in a broken clay bank, a layer of *Ostrea deltoidea* in tolerable perfection. This is the fifth example within my own knowledge of this remarkable fossil being found in the Kimmeridge clay of Yorkshire. Its true position here, as at Heddington and other places in the south of England, is very near the top of the oolitic formation which lies beneath; and I have no doubt it would be found in several other places, and in greater plenty, if it were carefully sought after. From the hill above mentioned, the Kimmeridge clay turns away toward Malton, and accompanies the eastern boundary of the coralline oolite.

*The Oolitic Rocks* rise from out of the Vale of Pickering to the north and to the west. Every one accustomed to practical geological investigation will at once recognize in the long slopes from Hambleton and Oswaldkirk, the Terrace of Rievaulx, and the sides of Newton dale, the characters of regular declination. There has been a general mistake respecting the order of succession of the members of the coralline oolite series in this tract of country. It has been generally believed that the uppermost beds of the series are coralline or shelly limestone, such as is seen in the quarries near Scarborough and Malton. But in truth, these oolitic beds are separated from the Kimmeridge clay by a considerable thickness of sandy calcareous and ferruginous beds, containing some fossils analogous to those which lie in the calcareous grit beneath the oolite.

This fact is evident on either the Gilling or Coxwold road to Helmsley. Oswaldkirk bank on the former road shows oolite under a considerable covering of brown sandstone, which continuing beyond Grange is twice covered by detached hills of Kimmeridge clay, but never once exposes the subjacent oolite till we descend to the town of Helmsley (see Plate V. section A). On the Coxwold road, after ascending nearly the whole height of the romantic Wass Bank, we notice the Oxford clay, surmounted by mural precipices of calcareous grit, and at the brow of the hill a quarry of coralline oolite covered by a hundred feet of brown and yellow sandstone. This forms the poor heathy moors to the north-west, and continuing N.E. towards Helmsley, presents to that town an *escarpment on the declining side* which exposes the coralline oolite (see section B).

Here then we first find indications of a denudation of a very interesting character: a denudation on the dipping edge of the

strata

strata which has produced here and there escarpments looking toward the formations which cover their continuous slopes. This principle will, I am satisfied, be admitted by all who carefully consider the facts; and it avoids the impropriety of supposing a dislocation without evidence. According as the denudation has extended more or less toward the "rise" of the strata, more or fewer beds of the oolitic series are exposed: where the denudation has produced the greatest effect, the lowest stratum exposed is Oxford clay.

A similar escarpment is found on the Rievaulx road from Helmsley as on the Oswaldkirk road; but long slopes of coralline oolite and brown sandstone over it cross the road to Bladlam. Hence to the valley of Kirkdale the same brown stone lies on all the higher parts of the road, and in a soft condition covers the oolite in the quarry at the famous cave. A mean of three measures very carefully made in 1824, gave me thirty-one feet for the height of the cave above the bed of the stream. (The statement of eighty feet in Dr. Buckland's account is probably a typographical error.) Nearly fifty feet of oolite are here exposed, and there are about twenty feet of sand and sandstone above. The brown stone continues below Manor-dale to Kirby Moorside, where it is covered by Kimmeridge clay.

In the stream which flows through the heathy common on the road from Kirby Moorside to Sinnington, geologists may observe in a very satisfactory manner the relation of this sandstone to the coralline oolite. The road-bridge over the stream stands on irregular sandstone layers, containing a few fragmented shells. Rising toward the north at a greater angle than the slope of the valley, fresh beds are exposed in succession as we proceed, and at length below six feet of yellow sand we find the uppermost very solid bed of oolite full of *Turritellæ*, *Melaniæ*, spines of *Cidaris* and *Trigonia costata*, as at Helmsley. A few beds lower lies a coarse variety of *Ostrea gregaria?* (Min. Conch.) At about half a mile from the road these observations may be repeated in the large limestone quarries, where the remarkable turbiniferous bed covers the other calcareous layers. Some *Turritellæ* also occur in the lower beds of the rock, which occupies the whole stream and sides of the valley, being covered by the heathy cap of sandstones. In the bank of the stream at Sinnington these sandstone beds are again exposed dipping south, and afford some fossils extremely similar to those which accompany the lower calcareous grit; particularly ammonites and pectines. Beyond Sinnington, the road crosses a hill of Kimmeridge clay one hundred and twelve feet above the stream; then descending

ing almost as many feet, it continues by Wretton and Aislaby on the long slopes of the same sandstone series to Pickering.

It thus appears that from Helmsley to Pickering the effects of denudation on the dipping edge of the oolitic series are scarcely at all observable; the beds declining regularly, and almost without interruption, into the Vale of Pickering. The denudation of Helmsley appears to be connected with the formation of the valleys about that town; but from Pickering eastward, a more extensive operation of similar causes has produced a long range of escarpments crossing the direction of the little existing valleys, but parallel to the general line of the great Vale of Pickering.

The valley descending to the south by the town of Pickering, presents good opportunities of observation. The strata on the western side are much lower than on the eastern, and slope south into the Vale of Pickering, without presenting any escarpment. On the eastern side calcareous grit is quarried beneath the limestone, a thin covering of the upper sandstones mixed with clay lies above that rock, and the whole series, sloping south into the Vale of Pickering, has been subjected to denudation parallel to the range and on the declining side. The effect of this denudation becomes extremely evident as we proceed further toward Scarborough. Before reaching the village of Thornton we cross a hill which appears to me to be formed of the Oxford clay. At Allerston, springs issue from the top of it, beneath the calcareous grit which is found on all the little risings to Ebberston, while the clay fills the valleys. At Ebberston, as at Allerston, springs issue from beneath the calcareous grit, and flow southward over the Oxford clay, in which stratum Mr. Vernon and myself found *Mya V-scripta*, and a peculiar ammonite as at Scarborough (see section C). At Parson's Houses, between Ebberston and Allerston, a well was sunk twenty-one yards through the clay of Ebberston to a brown freestone rock, which I suppose to be Kelloways stone.

Above the line of springs at Ebberston, calcareous grit in its usual characters with calcedonized ammonites occupies a good height in the hill, and is surmounted as usual by the coralline oolite, here full of *Ostreæ* in the lower part, and of *Melaniæ* and *Turritellæ* at the top. This escarpment, showing calcareous grit and coralline oolite, is very distinct above and north of the village of Snainton; and I think may be traced, though less evidently, by Brompton and Wykeham to the old Tower at West Ayton. After this, the long slopes of calcareous grit, descending from Falsgrave Moor and Oliver's Mount, are covered by parallel beds of oolite at Seamer, sloping down into the Vale of Pickering without any denudated edge.

Between

Between these sloping beds of the oolitic range as they continue to Filey Brig, and the projection of chalk from Hunmanby, the Vale of Pickering is much narrowed; and where it opens into the sea exposes such a vast quantity of diluvium as to cover almost all the beds of oolite, and effectually to hide the junction of this series with the Kimmeridge clay. This stratum is seen irregularly in the cliffs peeping out from under its huge load of diluvium, and at length near Speeton is exposed in very decided characters, lying immediately beneath the chalk.

I have no doubt that an impartial examination of this line of country will lead to the adoption of my opinion, that the pseudo-escarpments of the oolitic rocks on the north side of the Vale of Pickering are caused by denudation nearly parallel to their ranges, and that no general dislocation is at all concerned in the appearances. Even a new *zigzag fault* would add nothing to the explanation, the facts remain just as before: sometimes the dip of the strata is uninterrupted till the Kimmeridge clay, as at Sproxton and Kirby Moorside, lies on the oolitic rocks; in other cases the declining surface is interrupted, and a pseudo-escarpment results, exhibiting coralline oolite alone as at Helmsley, or calcareous grit beneath it as at Pickering, or even Oxford clay beneath that as at Ebberston. The strata re-commence no doubt on the south of this last place; but they are not seen, because from the breadth of the denudation the opposite edges are sunk much below the general level of the vale, and covered by diluvial and alluvial accumulations. These accumulations prevent the Kimmeridge clay from being seen in the flat central part of the vale; but on its southern side where the ground rises to the Wolds this stratum may be very well traced. (See Plate IV. section D.)

The inferences from these results connect themselves closely with general views on denudation and diluvial action, but it seems improper to enter into a discussion of them on such an insulated example. This is not the only instance which may be brought forward when the details have been more carefully studied. Till then, I must content myself with referring to the accompanying sections in illustration of these remarks, and have only further to observe that they were all drawn on the spot.—One general explanation will apply to them all:

Plate V.
1. White chalk.
2. Red chalk.
3. Kimmeridge clay.
4. Upper calcareous grit.
5. Coralline oolite.
6. Lower calcareous grit.
7. Oxford clay.

8. Kelloways

8. Kelloways rock, which appears on the northern edge of the oolitic hills, but is not seen at the surface in the Vale of Pickering.

Dec. 1827.

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## XLI. *A new Theory of the Resistance of Fluids, compared with the best Experiments. By Mr.* Thomas Tredgold, *Civil Engineer; Hon. M. Inst. Civ. En., and of the Liter. and Phil. Society of Newcastle-upon-Tyne.**

[With an Engraving.]

1. THE following is an account of some inquiries respecting the resistance of fluids, which were undertaken to enable me to render more effectual assistance to those who consult me on subjects where an accurate knowledge of this resistance is of importance: a further motive to proceed in the research was the extensive application of which it is susceptible among the wants of a commercial nation. With a like sense of its utility it was long ago remarked by Dubuat, "that to determine the resistance which a body in motion experiences in a fluid at rest, is one of the most important problems of general mechanics;" and since that time it has acquired still greater interest, in consequence of the application of a new species of power to propel vessels at sea.

2. Since I commenced the investigation, the French Academy of Sciences have offered a premium for the best series of experiments on the resistance of fluids; and certainly a good series of experiments under a great variation of conditions would be valuable; but when they are not made for the express purpose of determining particular truths, or data, they become of little value in assisting the progress of scientific investigation. The object of an experiment, like that of an equation, is the determination of something unknown; the same principles apply to both cases. The fine discoveries which made chemistry a science, resulted from experiment conducted by the methodical system of the mathematicians, though not in the same technical form.

I have felt the defects of the miscellaneous system of experiment, in attempting to correct and confirm my own inquiries; but have compared them with the best I am acquainted with.

3. When a body is moved forward in a fluid, the parts of the fluid before the body must be displaced; the force required to produce this effect is the measure of the *direct resistance.*

* Read to the Lit. and Phil. Society of Newcastle-upon-Tyne; and communicated by the Author.

4. The vacancy left by the motion of the body is filled by the motion of the fluid; but, with a loss of the force necessary to maintain an equilibrium among the parts of the fluid, equivalent to the force producing this motion in the fluid. The increase of resistance from this cause has been termed the *minus pressure of the fluid.*

5. The sum of the direct resistance, and that due to the deficiency of equilibrium, called the minus pressure, is the total resistance when the friction is neglected.

6. If fluids were devoid of friction they would be perfectly elastic, differing only in the degree of compression produced by a given force; hence, in considering the effect of collision on a fluid, it must be esteemed an elastic body. The coherence and friction of natural fluids render their elasticity in a slight degree imperfect, but not sufficiently so to interfere with the application of rules founded on its perfect elasticity in most of the cases occurring in practice. The total want of elasticity is incompatible with the very nature of a fluid.

7. If a body moving with an uniform velocity in a straight line AB, Plate IV. fig. 1. impinge on a filament of a fluid in which it moves, with a part of its surface inclined in any manner to the direction of the motion, the fluid will be reflected, and the direction of the resultant of the impingeing and reflecting forces will be perpendicular to the surface, and equal to the pressure of a column of the fluid capable of producing the actual velocity of the body. For the fluid being elastic, the restoration of figure produces the reflecting force, and the force in a direction perpendicular to the surface is only half that which would arise from the collision of a non-elastic body.

8. *Of the direct Resistance.*—Let $v$ be the difference between the velocity of the fluid and that of the body, when estimated in the direction of the motion; $g =$ the space described in a second by the velocity due to the force of gravity *in vacuo* $= 32\frac{1}{6}$ feet; $a =$ the angle the portion of the surface, on which the filament of fluid strikes, makes with the direction of the motion: and $h =$ the height of the column of fluid equal to the direct resistance. The velocity of the body is to that of the surface in a direction perpendicular to itself; or in the direction of the resultant as $v : v \sin a$; and $\frac{v^2 \sin^2 a}{2g} =$ the column of fluid whose weight would be equivalent to the perpendicular pressure. This pressure reduced to the direction of the motion is $h = \frac{v^2 \sin^3 a}{2g}$, the forces perpendicular to the direction of the motion being supposed to mutually balance one another.

9. *Of*

9. *Of the Minus Pressure.*—It is obvious, from the property of figures having parallel sides, that the quantity of fluid required to fill the vacancy left behind the body when it moves, is a constant quantity, whatever be the form of the body; therefore the deficiency of pressure will vary only with the velocity the fluid must acquire to follow the retiring surface. But if $c$ be the angle that surface makes with the direction of the motion, the velocity will be $v \sin c$; consequently, the motion of the fluid being established, the resistance from the deficiency will be $\frac{v^2 \sin^2 c}{4g}$.

Combining the two resistances, we have the height of the column $= \frac{v^2}{4g}(2 \sin^3 a + \sin^2 c) + F = H$; where F is the friction. When the body is a cube or a cylinder, with the ends perpendicular to the direction of the motion, the angles become 90°, and neglecting the friction $\frac{3v^2}{4g} = H$.

The height due to the resistance being to the height due to the velocity as 3 : 2.

10. If the height of a column that would balance the friction be $x$ feet, and H be the height producing the motion, then

$$H - x = \frac{v^2}{4g}(2 \sin^3 a + \sin^2 c); \text{ or } x = H - \frac{v^2}{4g}(2 \sin^3 a + \sin^2 c).$$

Assuming that the friction varies as the square of the velocity, and as the surface of the body; and putting $f =$ the height of a column of water whose weight is equal to the friction of one foot of surface when the velocity is one foot per second; then $\frac{f}{s}$ will be the height when distributed over the whole section of the moving body, when $s$ is the area of that section; therefore, if $lp$ be the area of the surface, $\frac{flpv^2}{s} = x$.

11. There is also the friction of the fluid itself to be considered when the velocity is considerable; and without being able to assign a satisfactory reason for it, if we consider the length to be increased by a quantity A$v$, where A is a coefficient depending on the nature of the fluid, the result agrees nearly with experiment, and the value of $x$ including this effect is

$$x = \frac{fpv^2(l + Av)}{s}; \text{ and consequently,}$$

$$v^2\left(\frac{2 \sin^3 a + \sin^2 c}{4g} + \frac{fp(l + Av)}{s}\right) = H.$$

Before attempting to determine the constant quantities from

 experi-

experiment, it will be useful to consider the different cases corresponding to the experiments to be compared.

12. If a plane of given area, and equal thickness, be set at different angles to the direction of the motion, the resistance will be expressed by $\frac{v^2}{4g}\left\{2(\sin^2 a + \cos^2 a) + \sin a + \cos a\right\} = \mathrm{H}$ when the effect of friction is neglected. (See Plate IV. fig. 3.)

When the thickness is inconsiderable, the effect of the edge may be neglected, and then $\frac{v^2}{4g}(2\sin^2 a + \sin a) = \mathrm{H}$.

13. In the case of a parallelepiped, with a wedge-formed prow attached, we have $\sin c = 1$; (see fig. 2.) and,

$$\frac{v^2}{4g}(2\sin^3 a + 1) = \mathrm{H}.$$

Including the friction $\frac{v^2}{4g}\left(2\sin^3 a + 1 + \frac{4gfp\left(l + \frac{b}{4\sin a}\right)}{s}\right) = \mathrm{H}$.

The same equations will apply when a cylinder terminates at one end in a cone.

14. The resistances of curved surfaces may also be computed: for example, let it be the resistance of a sphere;—put $p = 3{\cdot}14159$, and $y =$ the variable radius of the base, and $2py\dot{y} =$ the fluxion of its area. Then the figure being a circle, and $x$ the abscissa measured on the direction of the body's motion (fig. 4.) $\frac{r-x}{r} = \sin a$; and $y\dot{y} = (r-x)\dot{x}$; consequently,

$$\frac{2pv^2}{4g}\left(\frac{2\overline{r-x}^{\,4}\dot{x}}{r^3} + \frac{\overline{r-x}^{\,3}\dot{x}}{r^2}\right) = \mathrm{H} \times 2py\dot{y}.$$

The fluents are,

$$\frac{2pv^2}{4g}\left\{\frac{2(r^5-(r-x)^5)}{5r^3} + \frac{r^4-(r-x)^4}{4r^2}\right\} = \mathrm{H}py^2.$$

And, when $x = r$,

$$\frac{v^2pr^2}{4g}\left(\frac{4}{5} + \frac{1}{2}\right) = \frac{1{\cdot}3\,v^2pr^2}{4g} = \mathrm{H}pr^2;\ \text{or}\ \frac{1{\cdot}3\,v^2}{4g} = \mathrm{H}.$$

The resistance of a sphere is to the resistance of a cylinder of the same diameter as 1·3 : 3, or as 1 : 2·308; or as ·43$\dot{3}$ : 1.

15. If a cylinder have a hemispherical end or prow, then $\sin c$ becomes unity, and substituting this value of it in the equation we have,

$$\frac{v^2}{4g}\left(\frac{4}{5} + 1\right) = \frac{1{\cdot}8\,v^2}{4g} = \mathrm{H}.$$

16. And, if the motion be reversed, or the flat end go forward,

$$\frac{v^2}{4g}\left(2 + \frac{1}{2}\right) = \frac{2{\cdot}5\,v^2}{4g} = \mathrm{H}.$$

17. Ac-

17. According to this investigation we have therefore the following order of resistance.

| | | |
|---|---|---|
| A cylinder with flat ends .................... | 3 | or 1·00 |
| A cylinder with the hind part a hemisphere | 2·5 | or 0·83̇3 |
| A cylinder with the fore part a hemisphere | 1·8 | or 0·60 |
| A sphere .................................... | 1·3 | or 0·433 |

These ratios are likely to be altered a little by the effect of friction; and if the cylinder be reduced in length till it becomes a thin plate (fig. 9. and 10.), a still greater alteration is caused by the interference of the two motions of the fluid. The effect is easily observed by moving differently-formed bodies in water.

18. When a cylinder moves in a direction perpendicular to its axis (fig. 7. and 8.), making $d$ its length, we have $2d\dot{x} =$ the fluxion of its area, and $\frac{y}{r} = \sin a = \sin c$; hence,

$$\frac{v^2}{4g}\left(\frac{4dy^4\dot{y}}{r^3(r^2-y^2)^{\frac{1}{2}}} + \frac{2dy^3\dot{y}}{r^2(r^2-y^2)^{\frac{1}{2}}}\right) = \mathrm{H}\times 2d\dot{x}.$$

The fluents are, when $z$ is the arc of the curve,

$$\frac{2dv^2}{4g}\left(\frac{3r^3z-(r-x)\cdot(3r^2y+2y^3)}{4r^3} + \frac{2r^3-2(r-x)^3-3y^2(r-x)}{3r^2}\right)$$
$$= \mathrm{H}\times 2dx.$$

In this form the equation applies to the curved ends of canal boats having flat bottoms, the curves being usually portions of circles; and an approximate equation in the following form is easily applied.

Let $mr =$ half the breadth of the boat, $r$ being the radius of curvature; then

$$\frac{2dv^2}{4g}\left\{\frac{3z-r(1-m)\cdot(3-4m)\sqrt{2m}}{4} + \frac{r(2-2(1-m)((1-m)^2+3m)}{3}\right\} =$$

$2d\mathrm{H}x$. In the ordinary boats $m = \cdot125$, and therefore,

$$\frac{2dv^2}{4g}\left(\frac{3z-1\cdot1629\,r}{4}\right) = 2l\mathrm{H}x.$$

From these equations I have been enabled to compare some experiments made by Mr. Bevan on the power required to draw canal boats; which will be detailed after treating of the friction of bodies moving in fluids.

When a perfect cylinder is the form of the body, then $r = x$ and $p = 3\cdot14159\,r$, we have,

$$\frac{v^2}{4g}\left(\frac{3p}{8} + \frac{2}{3}\right) = \frac{1\cdot8448\,v^2}{4g} = \mathrm{H}.$$

19. *Of the Friction of Fluids.*—A series of experiments on the

the friction of water were made by Col. Beaufoy*, from whence it appears that the friction of water is very nearly as the square of the velocity; and about ·0032 pounds for one foot of surface moving at the rate of one foot per second. It is somewhat greater than this in slow motions; most likely from the effect of cohesion being sensible in small velocities. The experiments made by the Society for the Improvement of Naval Architecture do not appear to be so nearly in the ratio of the square of the velocity, but at the velocity of four nautical miles per hour the resistance is exactly ·0032 pounds (see Dr. Young's Nat. Phil. ii. p. 229). The head of water equivalent to this resistance is $\frac{0\cdot0032}{62\cdot5} = 0\cdot0000512 = f$.

20. If the resistance from the friction of the fluid itself be neglected, the general equation for water will be

$$v^2 \left\{ \frac{2 \sin^3 a + \sin^2 c}{128\cdot76} + \frac{0\cdot0000512\, p\, l}{s} \right\} = \mathrm{H};$$

or, $$\frac{v^2}{128\cdot76} \left( 2 \sin^3 a + \sin^2 c + 0\cdot0066 \frac{p\, l}{s} \right) = \mathrm{H}.$$

When $\sin a = \sin c = \sin 90°$, the resistance from friction is to the resistance from pressure as $3 : 0\cdot0066 \frac{p\, l}{s}$.

21. The value of the coefficient A, for water is of very little use, neither are there good experiments for determining it; in the resistance of air, however, it becomes of importance. The equation for air, when the length of the body may be neglected, is

$$\frac{v^2}{4g} \left( \sin^3 a + \sin^2 c + \frac{4 g f \mathrm{A}\, p\, v}{s} \right) = \mathrm{H}.$$

And if $n$ be the resistance of the body compared with a plane of equal section when the friction is neglected, the resistance of the plane being unity, the best experiments give $4 g f \mathrm{A} = 0\cdot000000332\, n$. The results of experiments, however, differ considerably; and it is difficult to fix on a datum which it never was their object to determine.

22. *Comparison with Experiments.*—The experiments of Bossut † were made with a parallelepiped four feet in length, two feet in breadth, and two feet draught of water. It was first tried alone, and afterwards with wedge-shaped prows of different degrees of acuteness (see fig. 2.); the time of describing ninety-six feet was observed, when the velocity had become uniform, except in the experiments with the most acute angles

* Dr. Thomson's Annals of Philosophy, vol. vi. p. 281, 1815.

† *Traité D'Hydrodynamique,* tom. ii. p. 394—411.

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in which the time of describing seventy-two feet was observed. Five trials were made with each change of form, except with the most acute prows, which were tried only thrice, and in the latter the results were not so regular as the others. The following table contains the mean resistances as Bossut has collected them, the resistance of the parallelepiped being denoted by 10000.

The equation applying to this case is $\frac{v^2}{128{\cdot}76}\, 2 \sin^3 a + 1 + 0{\cdot}0066 \left( \frac{p\left(l + \frac{1}{2 \sin a}\right)}{s} \right)$; and neglecting the difference between the measures, the resistance must be 10000 when $\sin a = 1$, consequently $0{\cdot}3285 \left( 2 \sin^3 a + 1 + {\cdot}0445 \left( 8 + \frac{1}{\sin a} \right) \right)$ is the expression for other angles.

| Angle with the Direction of the Motion. | Resistance collected from Observation. | Resistance by Calculation. | | Old Theory neglecting Friction *. |
|---|---|---|---|---|
| | | without friction. | with friction. | |
| 90° | 10000 | 10000 | 10000 | 10000 |
| 84 | 9893 | 9891 | 9893 | 9890 |
| 78 | 9578 | 9571 | 9578 | 9568 |
| 72 | 9084 | 9067 | 9083 | 9045 |
| 66 | 8446 | 8417 | 8442 | 8346 |
| 60 | 7710 | 7663 | 7700 | 7500 |
| 54 | 6925 | 6863 | 6913 | 6545 |
| 48 | 6148 | 6068 | 6131 | 5523 |
| 42 | 5433 | 5330 | 5407 | 4478 |
| 36 | 4800 | 4687 | 4777 | 3455 |
| 30 | 4404 | 4170 | 4272 | 2500 |
| 24 | 4240 | 3781 | 3897 | 1654 |
| 18 | 4142 | 3530 | 3662 | 955 |
| 12 | 4063 | 3393 | 3552 | 432 |
| 6 | 3999 | 3341 | 3578 | 109 |

The numbers in the fourth column ought to represent the resistances; and as the difference between these and Bossut's mean results are not greater than the differences among the trials with the same form, in any case we may, I think, con-

* See Bossut's *Hydrodynamique*, tom. ii. p. 411, or Robison's Mechanical Philosophy, vol. ii. p. 295. Vince's Hydrostatics, sect. iii. Dr. Hutton's Course of Mathematics for the Military Academy, vol. ii. Problem xix. p. 353.

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clude that the formula is sufficiently near for any practical purpose, as far as this species of body is concerned.

23. Some experiments were made in a very different manner by Mr. Vince*, and besides being chiefly made with thin planes, they were made by a rotary machine, which rendered it necessary to determine the centre of resistance. This he has done inaccurately; but neglecting its effect on the results, the experiments give the ratio of the height due to the resistance to be to the height due to the velocity as 3 : 2. This is the same as the ratio we have derived from theory (Art. 9.).

The experiments with a thin plane set at different angles may be compared with the equation $\frac{v^2}{4g}(2\sin^2 a + \sin a) = H$; (Art. 12.) which making the resistance at 90° equal 1000, will vary as $\frac{1000\,(2\sin^2 a + \sin a)}{3}$.

| Angle with the Direction. | Ratio by Experiment. | Ratio by Calculation. | Differences. |
|---|---|---|---|
| 90° | 1000 | 1000 | 0 |
| 80 | 963 | 975 | +12 |
| 70 | 915 | 902 | −13 |
| 60 | 820 | 788 | −32 |
| 50 | 660 | 646 | −14 |
| 40 | 506 | 489 | −17 |
| 30 | 330 | 333 | + 3 |
| 20 | 157 | 192 | +35 |
| 10 | 48 | 78 | +30 |

Mr. Vince also tried the resistance of hemispheres moving in water; with the base foremost, and with the spherical side foremost, the ratio was 8339 to 3400. The resistance of a cylinder of the same diameter he found to be 7998; hence making the latter unity we have By experiment; Cylinder 1; base of hemisphere 1·05; round side ·427.
By calculation; Cylinder 1; base of hem. ·835, round side ·6.

24. The experiments of Dubuat† on the pressure of water on bodies moving in it tend to confirm the relation of 2 to 1 between the direct and minus pressure of a column capable of generating the velocity. He has shown that the pressure is different on different parts of a plane surface, and this we might expect; but the whole effect is not altered by the distribution of

* Phil. Trans. Abrid., vol. xviii. p. 250.
† *Principes d'Hydraulique*, tom. ii. partie 3.

of the resisting forces, which will vary in proportion to the facility with which the fluid can escape by reflection from the different parts of the surface.

25. The experiments made by the Society for the Improvement of Naval Architecture*, differ considerably from others, but they are most readily compared in a tabular form. The resistances were taken at a velocity of five nautical miles per hour, which is equivalent to 8·35 feet per second.

| Form of the Body. | Resistance by Experiment. | Ratios. | Calculated Resistance. |
|---|---|---|---|
| | Pounds. | | Pounds. |
| Thin square plane................ | 80·76 | 1·08 | 100·7 |
| Cube................................ | 79·34 | 1·07 | 100·7 |
| Thin round plane................ | 80·64 | 1·08 | 100·7 |
| Cylinder............................ | 74·69 | 1·00 | 100·7 |
| Cylinder and hemispherical end | 56·04 | ·75 | 83·9 |
| The same reversed ............... | 22·28 | ·30 | 60·4 |
| Cylinder terminating in a hemisphere at both ends ...... | 18·53 | ·25 | 43·6 |
| Sphere .............................. | 25·24 | ·34 | 43·6 |

I have no doubt that there is an error somewhere in the mode of trial which gave the resistance stated in the account of these experiments, for they do not agree with others.

26. Col. Beaufoy made some experiments on the resistance of water, which appear to have been conducted in a similar manner. He found the resistance of a square foot, moving at the rate of one foot per second, to be 1·2949 pound; according to my mode of calculation, it should be 1·45 pound.

27. Borda, from an experiment made in sea-water, makes the resistance of a foot of surface $1\frac{3}{4}$ pound Fr., at a velocity of about one foot per second; and Bouguer, as quoted by Robison, makes the resistance 1·44 pound Fr. My mode of calculation gives 1·785 pound Fr. for the resistance of sea-water, to a surface one foot square moving at the rate of one foot (French) per second.

28. Mr. Bevan has given me an account of some trials he made to ascertain the resistance of a canal boat on the Grand Junction Canal. The length of the boat was 69·57 feet; its width, 6·83 feet; floating depth in the water, 0·89 foot; weight, $9\frac{3}{4}$ tons; radius of curvature four times the breadth; and the area of surface in contact with the water, 540 feet. (See fig. 7.)

* Buchanan on Propelling Vessels by Steam. 1816.

The equation rendered applicable to this case is
$v^2 (\cdot 242\, d\,(3\,z - 1\cdot 1629\,r) + 0\cdot 0032\,p\,l) =$ the resistance in pounds; and as $d = \cdot 89$; $z = 13\cdot 8$; $r = 27\cdot 32$, and $p\,l = 540$, it becomes $3\cdot 79\,v^2 =$ the resistance in pounds.

| Velocity in Feet per Second. | Resistance in Pounds by Experiment. | Calculated Resistance. |
|---|---|---|
| Feet. | Pounds. | Pounds. |
| 1·31 | 6·1 | 6·49 |
| 1·98 | 14 | 14·8 |
| 2·93 | 28 | 32·5 |
| 4·3 | 56 | 70·0 |

Considering the complex form, the resistance is expressed very well by the equation, and quite as nearly as a general rule could be expected to give it; and every one is aware of the advantage of being able to anticipate practical effects so nearly.

29. The most important experiments on the resistance of air appear to be those conducted by Dr. Hutton *, for the improvement of the theory and practice of Gunnery. In order to ascertain the effect of giving different inclinations to the same surface, he fixed a rectangular plane 32 inches in area, at different angles to the direction of its motion; the velocity was twelve feet per second.

| Angles with the Direction. | Resistance by Experiment. | Calculated Resistance. | Difference. |
|---|---|---|---|
| 90° | 1000 | 1000 | 0 |
| 80 | 994 | 975 | −19 |
| 70 | 957 | 902 | −55 |
| 60 | 868 | 788 | −80 |
| 50 | 724 | 646 | −78 |
| 40 | 533 | 489 | −44 |
| 30 | 331 | 333 | + 2 |
| 20 | 158 | 192 | +34 |
| 10 | 52 | 78 | +26 |
| 5 | 19 | 34 | +15 |

The resistance of the plane when set at an angle of 90° and moving at the rate of twelve feet per second, was 0·841 ounce. By my mode of calculation it comes out 0·896 ounce. In another set of trials with a triangular plane, the resistance cor-

* Tracts, vol. iii. p. 202.

responding

responding to the same area and velocity was ·846 ounce. The mode adopted by Dr. Hutton to reduce for the edge of the plane is not quite correct, as it obviously has an unequal effect at the different angles. (Art. 12.)

30. In comparing the resistance of bodies of different forms, Dr. Hutton obtained the following ratios:

| | Cone. Angle 25° 42′. | | | | Hemisphere. | |
|---|---|---|---|---|---|---|
| | Vertex. | Base. | Sphere. | Cylinder. | Convex side. | Base. |
| Ratio by Experiment | 126<br>·44 | 291<br>1·02 | 124<br>·435 | 285<br>1·00 | 119<br>·42 | 288<br>1·01 |
| By Calculation | ·388 | ·729 | ·434 | 1·00 | ·6 | ·833 |

It will be remarked, that in air as in water, the hemisphere with the convex side foremost differs most widely from the result of calculation. The sphere is very near, and the near coincidence of the actual resistance, as well as the ratio, is worthy of notice.

The resistance of a ball two inches in diameter was found by experiment to be ·163 ounce at a velocity of twenty-five feet per second. By calculation I find its resistance ·1655 ounce at that velocity,—a difference of only $\frac{1}{65}$ in excess.

31. Col. Beaufoy* made some experiments on the resistance of air on bodies of different shapes, and published a table of the resistances. When the area of the base is one superficial foot the force is in ounces; and taking the line corresponding to a velocity of twelve feet per second, I have added the numbers my mode of computation give in these cases.

| | Cone. Angle 45°. | | Thin Plane. | Cylinder. | Wedge. Angle 45°. | |
|---|---|---|---|---|---|---|
| | Vertex. | Base. | | | Vertex. | Base. |
| Resistance by Experiment | 3·61 | 4·586 | 4·834 | 4·668 | 3·005 | 4·69 |
| Calculated Resistance | 2·294 | 3·36 | 4·032 | 4·032 | 2·294 | 3·36 |
| Differences ... | 1·316 | 1·226 | ·802 | ·636 | ·711 | 1·33 |

In these comparisons the calculated numbers are in defect, and the ratios are not very different.

32. Returning to Dr. Hutton's experiments †, I must next

* Annals of Philosophy for 1815, vol. vi. p. 277.

† Tracts, vol. iii. p. 230, where the tentative formula of Dr. Hutton are given.



show

show how far the formula will apply to great variation of velocity.

The equation for a spherical ball reduced to the form applicable to this case is

$$\frac{v^2}{4g}\left(1\cdot3 + \frac{1\cdot3 \times 0\cdot000000332 \times 4v}{d}\right) = \text{H};$$

and since a cubic foot of air weighs 1·2 ounce at the mean temperature and pressure, and the area of the section of the ball is $d^2 \times \cdot7854$ feet, it becomes

$$\frac{v^2 d^2}{105}\left(1 + \frac{0\cdot000001328\, v}{d}\right) = \text{the resistance in ounces.}$$

| Velocity in Feet per Second. | Resistance to a Ball two Inches Diameter in Ounces. | | |
|---|---|---|---|
| | By Experiment. | By Theory, neglecting Friction. | By Theory, with Friction. |
| 5 | 0·006 | ·0066 | ·00661 |
| 10 | 0·026 | ·02647 | ·026478 |
| 15 | 0·058 | ·0595 | ·05977 |
| 20 | 0·103 | ·106 | ·10664 |
| 25 | 0·163 | ·166 | ·16725 |
| 30 | 0·237 | ·238 | ·24016 |
| 40 | 0·427 | ·425 | ·43012 |
| 50 | 0·676 | ·665 | ·675 |
| 100 | 2·78 | 2·647. | 2·727 |
| 200 | 11·34 | 10·6 | 11·24 |
| 300 | 25·8 | 23·8 | 25·96 |
| 400 | 46·5 | 42·5 | 47·62 |
| 500 | 74·4 | 66·5 | 76·5 |
| 600 | 110·4 | 96·0 | 113·28 |
| 700 | 156·0 | 130·0 | 157·44 |
| 800 | 212·0 | 171· | 211·96 |
| 900 | 280·3 | 215· | 273·32 |
| 1000 | 362·1 | 265·00 | 345· |
| 1100 | 456·9 | 321· | 427·48 |
| 1200 | 564·1 | 383· | 521·24 |
| 1300 | 683·3 | 450· | 625·76 |
| 1400 | 811·5 | 520· | 739·52 |
| 1500 | 947·1 | 595· | 865· |
| 1600 | 1086·9 | 680· | 1007·68 |
| 1700 | 1228·4 | 769· | 1162· |
| 1800 | 1368·6 | 860· | 1326·56 |
| 1900 | 1505·7 | 960· | 1508·72 |
| 2000 | 1637·8 | 1060·00 | 1700· |

In this series of comparisons there are only two places in which the difference amounts to $\frac{1}{10}$; and the one is in excess, the other in defect; and if the resistances were represented by the ordinates

ordinates of two curves, these curves would cross five times at unequal distances in the range.

*Explanation of the Plate.*

Fig. 1. A body moving in the direction AB. The collision and line of reaction of a filament of the fluid from the oblique surface of its fore-part are indicated by faint lines, with dotted lines to represent the composition and reduction of the forces. The motion of the fluid after the body is also shown by lines of a light tint. The dotted rectangle is equal to the oblique parallelogram, but the motion of the fluid is less as the obliquity increases.

Fig. 2. Represents the plan of the body used by the French Academicians in their experiments; the head was altered to different angles, but the form of the after-part remained the same in all their experiments.

Fig. 3. is to show the effect of moving a plane surface in a fluid when placed obliquely in respect to the direction of its motion; and it is obvious that the edge of the plane must have a sensible effect on both the direct and minus-pressure.

Fig. 4. 5. and 6. are, a sphere; a cylinder, with the after-part a hemisphere; and a cylinder with the fore-part a hemisphere; having the characters used for calculation annexed to their respective lines.

From the flat end of fig. 5. the stroke and reaction being in the same direction, the fluid cannot move laterally till the central pressure so far exceed that on the more remote parts as to cause the proper quantity of lateral motion; or till a portion of the fluid accumulates before the body: but the portion accumulated being a fluid, the forces must produce the same effect as if each filament were struck and reflected by the solid plane.

Fig. 7. The plan of a body formed by segments of circles, with the characters annexed to the lines. When the radius is half the breadth, the form becomes a prism terminating in semicylinders at the ends (as fig. 8).

Fig. 9. and 10. are to show how the two motions of the fluid interfere when the fore- and after-part are separated by a thin edge only. In the hemisphere (fig. 9.), the resistance is increased by the fluid displaced by the fore-part preventing the fluid from following behind the body. In fig. 10. the reflected fluid from the fore-part in some degree facilitates the motion of the following fluid.

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It will be evident to any one who examines the preceding paper, that it must have cost me a great deal of labour; and in consequence I was desirous of presenting it to the Royal Society. But finding that I must sacrifice all claim to new theoretical

theoretical investigation in order to secure its appearance in the Transactions of that Society, I chose in preference to send it to Newcastle, and to take this most respectable channel for presenting it to the public, knowing that it will be extensively circulated among men of science, as well as that in these days it does not require the aid of authority to support the cause of truth; while, recollecting the state of hydrodynamical science as it appears in books written for the use of University students, we know that when authority has not truth to propagate, it does not hesitate to teach that which is known to be erroneous.

Having opened a new path in this difficult subject of the motion of fluids, it was not in my nature to stand still; the efflux of fluids, the impulse on bodies placed in a moving fluid, and various other inquiries followed. These, as my health permits, will be presented to the world.

THOMAS TREDGOLD.

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## XLII. *On* Mr. Herapath's *Second Attack on* Lagrange's *Method.*

LAGRANGE's method still charged with failure! In returning to this subject it was incumbent on Mr. Herapath, as a man of an ingenuous mind, to acknowledge the mistake he has already committed; of which he says not a word, although it was so clearly pointed out to him. Who, that felt the spirit of Science within him, could bear the thought of having made a charge of failure in consequence of a mistake of his own, without hastening to apologize to his living readers, and to the illustrious dead from whose fame he had detracted! The former argument, which was to overturn Lagrange's method, originated in the erroneous notion that every one of the three parts of the integral considered, must separately satisfy the differential equation; whereas it is the aggregate of all the three parts which satisfies that equation*. The matter is so very plain,

* At the bottom of p. 23, of this Journal for January, Mr. Herapath gives the value of $p$, which he has computed. Taking this value, it is obvious that his $p y_1$, is the same with the first part of the integral in p. 96 of this Journal for February. The other two parts of the same integral are of course identical with $p_1 y_2$ and $p_2 y_3$, all the parts being derived from one another by interchanging the roots. Mr. Herapath's expression of $y$, viz. $p y_1 + p_1 y_2 + p_2 y_3$, is therefore the very same with the integral in p. 96 of this Journal for February. This being established, however surprising it may appear, yet we must infer, from what he says of the integral just mentioned, and from the very confident postscript to his article, that he is not acquainted with what he has himself computed. The truth is, that care was taken not to change his expressions in the slightest degree. God knows what would have been the consequence, if, in explaining this gentleman's mistakes, any one had presumed to alter one tittle of his formulas!

plain, that it is not easy to account for the mistake; unless indeed his last article help us to a solution of the difficulty.

He is now puzzled how to deduce the case of equal roots from the usual form of the complete fluent. There is some confusion in what he writes, arising from his not distinguishing between the mathematical definition of the complete integral, and a property belonging to it. An algebraic expression which satisfies the differential equation, and contains the requisite number of arbitrary constants, is the complete integral; and it possesses the property of comprehending every possible case, by varying the arbitrary quantities. It is certain that Mr. Herapath's integral of the third order, when we take in all its three parts, does satisfy the differential equation *; it also contains three arbitrary constants; it is therefore the complete integral: but notwithstanding, he contends that it is not general; for he makes it fail, particularly in the case of equal roots. Now this proceeds, not from a failure in any method or in any doctrine, but from a failure in Mr. Herapath, who does not reason correctly in adapting the general expression to the particular case.

Let us take his own example, p. 212, of the last Number of this Journal, viz.

$$y = \frac{ce^{rx} - c_1 e^{r_1 x} + e^{rx} \int X e^{-rx} dx - e^{r_1 x} \int X e^{-r_1 x} dx}{r - r_1}$$

When the two roots are real and unequal, or when they are imaginary, there is no difficulty; but the puzzle is, what must be done when the roots are equal, for then the integral is infinite. For the sake of simplicity, let $r_1 - r = \omega$, then $r_1 = r + \omega$; also put $c_1 = c + h\omega$: then, by substitution,

$$y = \frac{e^{rx+\omega x}(c + h\omega + \int X e^{-rx-\omega x} dx) - e^{rx}(c + \int X e^{-rx} dx)}{\omega}$$

This is only another form of the complete integral, the arbitrary constants being $c$ and $h$; but it has the advantage of bringing out the correct value of $y$ when the roots are equal, or $\omega = 0$. If we substitute the expansions of $e^{\omega x}$ and $e^{-\omega x}$, we shall obtain, supposing $\omega = 0$,

$$y = e^{rx}(h + cx + x\int e^{-rx} X dx - \int e^{-rx} X x d^{x});$$

but, $x\int e^{-rx} X dx - \int e^{-rx} X x dx = \int dx \int e^{-rx} X dx$;

wherefore, $y = e^{rx}(h + cx + \int dx \int e^{-rx} X dx)$,

* See the calculation in this Journal for February last, p. 97.

which

which is the complete integral in its simplest form when the roots are equal. If we suppose $X = 0$, then,

$$y = e^{rx}(h + c\,x).$$

Thus, when we follow a proper method, it appears that the complete integral comprehends all the subordinate cases; which proves the incorrectness of Mr. Herapath's views.

Nothing more was intended by the notice inserted in this Journal for February last, p. 96, but to correct Mr. Herapath's mistake about Lagrange's method. It may now be proper to inform him that there is nothing new in his papers. Euler has integrated the equation of the third order in his *Calculus Integralis*, tom. ii. sect. ii. cap. 3. prob. 149; and at the beginning of the scholium to the problem, he observes: "In genere autem, nulla integralium reductione adhibita, integrale nostræ equationis ita exprimi potest;" and he then sets down the very same expression of $y$ which is contained in Mr. Herapath's equation 19. Euler's words contain the exact description of Mr. Herapath's method, which finds the integral by successive integrations without attempting to reduce it to the form best adapted for use. In prob. 151. of the same chapter, Euler exhibits the unreduced integral of any order indefinitely, being the very same with Mr. Herapath's equation 17, and therefore containing the sum and substance of that gentleman's doctrine. Lastly, in prob. 152. cor. 4. it is observed that this form of the fluent is free from the difficulty about the equal roots, on account of the absence of zero divisors. These accumulated proofs show that there is nothing new in Mr. Herapath's papers, except the extraordinary mistakes they contain.

The history of this problem; the real difficulties attending it; the slips that were *at first* made by geometers of the highest eminence about the case of equal roots; the correction of these slips, and the artifices by which the solution has been completed;—all these are points about which it is fair to presume that Mr. Herapath is at present not well informed.

Some apology is due to the readers of this Journal, for the length of these remarks; but it seemed proper to explain the matter pretty fully; for, unless the writer of this article has been misinformed, Mr. Herapath's solution of this problem has, not much to the credit of British science, been blazed abroad as a great discovery. I now withdraw from any further intermeddling in this trite subject *.

*α β.*

XLIII. *On*

* It is probable that his Postscript relates to this frivolous point, viz. whether the arbitrary constant in $p$ is to be written with the same denominator

## XLIII. *On Systems and Methods in Natural History.* By J. E. Bicheno, *Esq., F.R.S., Sec. L.S., &c.*

[Concluded from p. 219.]

IT was the opinion of Linnæus, and continues to be the opinion of some of his disciples, that genera are actually founded in nature as much as species. "Naturæ opus semper est species et genus." *Phil. Bot.* § 162. "Genus omne est naturale, in primordio tale creatum, hinc pro lubitu et secundum cujuscunque theoriam non proterve discindendum aut conglutinandum." *Ib.* § 159. So the excellent and elegant author of the "Introduction to Physiological and Systematic Botany," says, "A genus comprehends one or more species so essentially different in formation, nature, and often many adventitious qualities from other plants, as to constitute a distinct family or kind no less permanent, and founded in the immutable laws of the creation, than the different species of such a genus. Thus in the animal kingdom a horse, ass, and zebra, form three species of a very distinct genus, marked not only by its general habit or aspect, its uses and qualities, but also by essential characters in its teeth, hoofs, and internal constitution." It was the circumscribing these insulated assemblages of species that Linnæus regarded as the business of the accomplished naturalist.

Those therefore who use the word *genus* in the Linnæan sense, do not employ it with the same meaning as those who regard genera as merely conventional, and subject to be broken down to suit convenience. The latter would do well to employ some other term, else one great object will be lost at which we are aiming;—the keeping together under some one common head those small assemblages of species which in some instances are so obvious, and so important in enabling us to comprehend and discourse of the scheme of nature.

Whether such insulated groupings really exist, it is for the naturalist to determine, and this can be only inferred from a very extensive knowledge; but as long as we are witnesses to

minator as the variable part, as he has himself written it; or without the denominator. Write it how he will, the same egregious blunder still remains; namely, his supposing that every part of the integral must separately satisfy the differential equation. His Postscript is not clear; but two things may be gathered from it: one, that he is possessed of a method for measuring the degrees of absurdity; the other, that he is not well assured what is, or what is not, Lagrange's method, although he has, twice in this Journal, accused it of failure. The truth is, that all his arguments are directed, not peculiarly against Lagrange's method, but against the complete integral, reduced to its simplest form, by whatever method it may have been obtained.

such striking modifications of form as we discover in the genus *Erica*, *Rosa*, *Eriocaulon*, &c., among plants, and in *Vespertilio*, *Strix*, *Scarabæus*, &c., among animals, it would be the height of folly to give up a term so expressive and at the same time so useful, or to transfer its received meaning to some other word which has not been used in the same sense.

As the success of the systematist depends so materially upon the proper use of these abstractions, I shall now proceed to show some distinctions which it is necessary to keep in view while we employ them. We aim, as I said before, at two distinct objects by the use of systems: we use the artificial for becoming acquainted with individuals, and the natural as the means of combining them, and enabling the student to comprehend and speak of the general truths relating to nature by a knowledge of a few particulars.

Division and separation is the end of the artificial system;—to establish agreements is the end of the natural. In one case we reason *à priori;* in the other *à posteriori.* The one is a descending, the other an ascending series. Linnæus understood this distinction when he remarked, "Ordines naturales valent de naturâ plantarum; artificiales in diagnosi plantarum."—"Cavendo in imitando naturam filum Ariadneum amittamus." Nevertheless it has appeared to me that many modern naturalists have not adopted these truths; and that it is the prevalent error of the day to attempt to generalize where they ought to analyse; while their arrangements, called natural, are almost all of them framed with a view to distinguish. Let me not be supposed by these remarks to wish to exclude from the natural system every attempt at diagnosis; for it is obvious, that as the business of the naturalist is to study all the characters, he can no more neglect differences than he can agreements. I only wish to point out the two dissimilar objects we have in view, that they may not be confounded.

M. Decandolle, for instance, whose labours as a systematist are invaluable, seems to overlook this distinction. In his *Regni Vegetabilis Systema Naturale*, he starts from things the least known, to reason on things best known. He begins his comprehensive work with a predicate of the stars; and, proceeding downwards to minerals, comes to plants. Here he employs a series of terms expressive of a natural gradation from the highest to the lowest group, attempting fresh combinations at every stage, and making a place for every thing. Thus he has *class, sub-class, cohort, order, tribe, genus, section, species.* The extraordinary number of these combinations diminishes their value as a work of natural arrangement. It is a difficulty of sufficient amount to establish a few well marked; and

and when they are so multiplied, it may be suspected that many of them are arbitrary and artificial. This attempt at breaking down good orders and genera into many subordinate and loosely defined groups, and encumbering them with names, involves the subject in obscurity, and may well be questioned as contrary to his main design of presenting those comprehensive views which are afforded by a natural system.

Mr. Brown has adopted a different mode in his *Prodromus.* He has attempted to combine no further than his knowledge would warrant, not even employing the terms class or order as the names of his groups. As his object is chiefly synthesis, he keeps his diagnostic characters apart, thus leaving the mind less embarrassed when it is in pursuit of analysis. It must be admitted indeed, that his work cannot be employed with any success by the inexperienced, or even by those who have occupied themselves only in searching for species; but to have made it subservient to this purpose, would have been to have rendered it less beautiful and complete as a work of synthesis. His aphorisms and remarks not being reduced to exact method, "are," as Lord Bacon expresses it, "still in their growth, increasing in bulk and substance."

Now wherever the object of the systematist is to enable his reader to discover species, it is necessary to define at every step; and where natural characters do not present themselves, we must adopt artificial ones. For this purpose large classes are formed, many of which are necessarily artificial. These again are broken up into orders, mostly of an artificial character; and thus the naturalist is led step by step from more comprehensive definitions to less, from class to order, from order to genus, and from genus to species. In this descending series it will be observed that the essential feature is the facility that is afforded for definition. Hence the Linnæan system of botany has succeeded so well, because its author selected chiefly as the ground of his arrangement the number and proportion of parts most obvious and least liable to vary. His classes and orders are avowedly so many assumptions, which practice has shown to be convenient; but when we come to genera, the artificial system falls in with the natural, as Linnæus framed their characters upon resemblances founded in nature.

Now in the natural system this machinery of terms cannot be employed in the same manner. It is an ascending series from the less to the greater predicate. From genera we proceed upwards to orders, and orders we combine into classes. We become more and more general in our characters, instead of more and more definite. Here indeed we ought not to sacrifice, as in the artificial scheme, to convenience; and break up well-defined genera and orders because they contain a large number of species. If we find a large genus, for instance, as *Erica*, agreeing in some well-marked characters of structure, form, station, and properties, it appears contrary to the end proposed by the natural system, to divide and subdivide the species into small groups, and to give each of these the same value as is now possessed by the whole. This is frittering away characters which are essential to the use of a genus, and destroying our power over it when we proceed to generalise. The value of generic terms consists essentially in the distinct conceptions we have of them; but if we go on to multiply them, as is at present the fashion, we render it as impossible to circumscribe them, as it is to parcel out the colours of the rainbow; and instead of making Natural History familiar and popular, it will require the compass of a man's life to master the terms we employ. If indeed the object be to analyse, division may be very convenient, because the inquirer may be otherwise bewildered in the multitude of particulars. It does not follow from hence that the student of the natural system may not avail himself of subordinate groups by whatever characters they may furnish; only the giving them equivalent names, and making them co-ordinate, is destructive, as it appears to me, of his system as a means of general reasoning.

In no department of natural history are the inconveniences arising out of this confusion of analysis and synthesis more felt than in Entomology. The multitude of species included in this kingdom of nature is so great, that it requires the most skilful arrangement to enable the student to determine them: yet it is unquestionably the worst furnished with assistance in this way;—a defect which may be attributed chiefly, I apprehend, to the attempt which both we and our continental neighbours have made to combine the natural with the artificial system. We have aimed at analysis and synthesis at the same time. A comprehensive acquaintance with this infinitely varied tribe can alone enable us to synthesise with safety; and a long period must elapse before we can hope to embrace within our synthesis the whole of the insect world.

In the large views taken by means of the natural system, our business will for ever be the labour of separating what we shall know from that which is unknown. The profoundest knowledge will at last be but a fragment. Some groups of nature are so closely related, that they have been observed from time immemorial. "Whatsoever parteth the hoof and is cloven-footed, and cheweth the cud," comprehends a group of animals so obviously connected, that they must have received a generic appellation from the remotest period. As

knowledge

knowledge has increased, more and more families have been separated: still there is always a remainder of unknown things. Take any natural system, and see if this is not the case. Linnæus in his "Fragments of a Natural Method" professes only to separate from the mass those groups which he saw clearly. Again, his definition of vegetables indicates the same truth: "Vegetabilia comprehendunt Familias septem, *Fungos*, *Algas*, *Muscos*, *Filices*, *Gramina*, *Palmas*:" and then, to include the remainder, he adds, "et *Plantas*;" defining the last thus, "Plantæ dicuntur reliquæ, quæ priores intrare nequeunt familias." *Phil. Bot.* § 78. Take up Jussieu's *Genera Plantarum*: and besides his "Plantæ incertæ sedis," see how he is obliged to dispose at the end of many orders his "Genera affinia," and "Genera nondum satis determinata." This is true inductive philosophy; yet the same author may be suspected of departing from this mode of investigation when he attempts to edge in his remainder under artificial or sweeping characters, as he has done in *Eleagni* and *Junci*, and when, falling in with this modern innovation, he invents a multitude of new orders to embrace every known species of plant.

The mammiferous animals are arranged with more ease according to a natural system, in consequence of their number being comparatively small, and their forms strongly marked. Nevertheless the system of M. Cuvier, in the *Règne Animal*, clearly shows the vain attempt of finding a place for every thing. Nothing can be more satisfactory and beautiful than many of his orders and divisions; yet see how he is compelled to change his ground when he comes to the *Pachydermata*, and to huddle together species very remotely connected. His birds also exemplify the same fact, where his order *Passeres* is made to include all that his other orders will not hold. "Son caractère semble d'abord purement négatif, car il embrasse tous les oiseaux qui ne sont ni nageurs, ni échassiers, ni grimpeurs, ni rapaces, ni gallinacés." Thus it contains the Warblers, the Shrikes, the Goatsuckers, the Crows, the Creepers; birds of the most dissimilar habits, and living upon the most dissimilar food. The Chough is separated widely from the *Corvi*, and *Anthus* from *Alauda*. Now this is what we might expect from the nature of the subject; only it is desirable that the remainder of unknown things should be distinctly avowed, and not reduced to an exact place in the natural system. Jussieu's was the most philosophic mode, which was to place this residue at the end. Linnæus too was very correct when he pronounced his natural orders to be a "Fragment;" and those persons who imagine it to be necessary or advantageous to find a place for every thing, and to divide and split for the purpose of making such places, appear to lose sight of the chief object of the natural system, and to destroy its utility as an instrument of general reasoning.

The French writers in general are prone to combine in their systems the very distinct objects of individualizing and generalizing. They are for ever subdividing where the great aim should be to combine, and thus they detract from the utility of their arrangements for either purpose. It is they who have countenanced the use of *sub-classes*, *cohorts*, *tribes*, *stirpes*, *sub-genera*, and *sub-species*; and they also are the great contributors to the minute division of genera. Strictly speaking, in the natural system we should employ but few terms of the kind alluded to, and those of loose application. For instance, the word *sort* or *group* would as correctly express any natural assemblage of species, as *sub-class*, *race*, *tribe*, *cohort*, or *stirps*; for what do we know of the relative value of the groups attempted to be pointed out by these expressions? And how can we say they are not co-ordinate or commensurate with each other? The great division of cotyledonous plants may, for aught we know, be only equivalent to the order of Grasses; and a genus in some cases seems as distinct as any class, as *Parnassia* and *Linnæa* among plants, and the *Ornithorhynchus* and *Hippopotamus* among animals. Indeed in the recent work of M. Latreille, *Familles Naturelles du Règne Animal*, he has arranged the monotrematous animals in a class by themselves, and has made two orders; in one case, consisting of a single species, the *Ornithorhynchus paradoxus*, and in the other, of two other species before considered as belonging to that genus. Thus it is, as M. Cuvier remarks, that these animals set at naught all our classification by their osteology and mode of bringing forth.

The adoption of these numerous terms, intended to express fixed ideas, must be looked on with suspicion. The terms *species* and *genus* are too well established by custom, and are so clearly the result of convenience, and moreover conform so closely to the ordinary use of these words, that their utility cannot be questioned; but those numerous subdivisions current among our neighbours, and sensibly increasing among ourselves, may well be doubted as unphilosophical language. To each of them is attempted to be assigned a definite value beforehand, and an impracticable degree of precision; and we deceive ourselves by fancying that we can deal with these delicate and fleeting instruments of thought differently from the rest of the world. But are we to attempt to fetter nature by our systems and terms? "Books should follow sciences, not sciences books," says the immortal Bacon; yet the adop-

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tion of systems and technical expressions, which have received their definition beforehand, cannot be employed without the danger of perpetuating false hypotheses, and an apprehension on the part of the ignorant, that these inventions give us some power over nature not belonging to ordinary language.

The more correct mode would be to exclude from the natural method most of these terms, and to employ in their place some convertible words of looser import, as indeed M. Cuvier has to some extent done; such for instance, as group, section, division, to express those larger assemblages of approximations to assigned forms, which are rather predicated than proved; and in many cases to point them out by mere signs, such as are used in printing. Thus, for instance, the word section, or any similar word, might be employed to express the plants severally comprehended in the order *Gramineæ*, the class *Compositæ*, and the division *Monocotyledones;* and where the characters are less definite, the plants pointed at might be assembled under a simple asterisk.

One chief recommendation of the natural system over the artificial, is the liberty which it leaves to the mind. The one shuts it in to the narrowest scope of observation, while the other suffers it to range in search of all the properties belonging to created beings; their functions, their structure, relations and resemblances, affinities and analogies. It is speculative and general truth that the natural system enables us to pursue; and this will never submit to be bound by any fetters which the art of man can invent. Books after all are but a rude mode of holding knowledge together; and language but an imperfect vehicle to convey with precision the just relations of things. At best it bears the image of the earthy, while things themselves bear the image of the heavenly.

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## XLIV. *Examination of a gelatinous Substance found in a damp Meadow;—as a Contribution to the Knowledge of the Meteors called Shooting-Stars. By Dr.* R. Brandes*.

My friend Dr. Buchner communicated some time ago (in Kastner's *Archiv.* v. 182), a treatise on the substance of the meteors called Shooting-Stars, which Kastner has designated by the name of star-jelly. This substance, found in a damp meadow, was of a gelatinous appearance, and was supposed by Dr. Schultes to be *Tremella nostoc.* M. Buchner, however, having examined it, was of a different opinion, not having been able to discover in it any trace of an organic tissue.

* From Schweigger's *Jahrbuch der Chemie,* N. R. Band xix. p. 389.

Indeed

Indeed he does not decide whether its origin was terrestrial or not; yet he asserts that this substance could be neither a plant nor an animal, (at least not an entire one,) but perhaps the produce of an animal, an *excretion* resembling gum, mucus, &c.; and although he does not deny the possibility of such a body having descended from the atmosphere, he does not think it very probable. Nay, he does not much object even to the comparison of this slimy substance with the manna of the Israelites, which fell from heaven;—he thinks at least that this slimy mass might be as nourishing as oysters. However inclined I may feel to agree with my respected friend on the first two points; viz. that this mass might originate from the excretion of an animal, or a gelatinous meteor, the idea of comparing it with the manna of the Israelites seems to me untenable, there being too great a difference between both the nature of the substances and the places in which they have been found. I was much inclined to ascribe to the substance in question an atmospheric origin, in consequence of the want of organic structure, which M. Buchner ascribes to the mass he examined; in consequence also of the account given by R. Graves of a fiery meteor which had fallen in Massachusetts, in the United States, in a spot where the following morning a gelatinous substance was found*; in consequence, moreover, of my own observations on the existence of atoms of azotized substances in the atmosphere, at least in rain-water, (*Jahrb.* 1826, iii. 253); and ultimately from the account frequently repeated to me by a soldier of the contingent of Lippe, who had fought in the Peninsula, of his having frequently noticed in Spain, while on duty during cold nights, stars to shoot, and perceived in the morning in damp places, where he thought them to have fallen, white gelatinous masses, which were soon decomposed; an opinion which I also mentioned in my treatise on Rain-water.

M. Schwabe, an apothecary at Dessau, published some time after a treatise on the same subject (Kastner's *Archiv.* vii. p. 428), on his having had an opportunity of examining a substance which had been found in a damp meadow, which was likewise gelatinous and of a green colour. Mr. S. recognised this mass distinctly as *Nostoc commune* Vauch. (*Tremella nostoc*, Linn.), having discovered in it through the microscope the structure of this singular *nostoc*. As this mass agreed not only in its external form and place of discovery with that of Buchner, but also in its chemical composition, Mr. S. thought himself justified in assuming that both this substance and that

* Gilbert's *Annalen*, lxxi. 314.

of

of Buchner were of the same nature; since, owing to its peculiar texture, Mr. B. might have overlooked its organic structure. But on comparing more closely the descriptions given by the two naturalists of the substances observed by them, we discover several other differences, besides the one that Mr. Schwabe discovered,—a distinct organic structure; while Mr. Buchner saw none. The substance examined by Mr. S. was of a greenish colour; that of Mr. B. white, like swelled tragacanth. The former on being burned emitted, not an *animal* smell, but one quite peculiar, resembling burning *confervæ*, *rivulariæ*, and *chætophoræ*, and yielded a shining coal, which preserved the skinny form of the pieces employed; and being reduced to ashes, left a residuum of silica, with carbonate, muriate, and sulphate of potash, with traces of phosphate of lime and oxide of iron; but Buchner's substance, on being heated, swelled considerably, diffusing at the same time a strong *animal* smoke; caught fire at last, and left a coal that could not be reduced to ashes, and which contained carbonate of soda and phosphate of lime. However similar Mr. S. may consider his substance to that of Mr. B., the facts here stated seem to invalidate his conclusion as to their identity. I feel, therefore, great pleasure in being able to communicate an examination which I had an opportunity of making last autumn, and which may perhaps throw some light on the point in question.

A friend and fellow-townsman of mine, who has a large meadow near our city, which, lying in the lower part of our salt-valley, has been drained in a slight degree by dint of great labour, and the grass in it improved by the application of salt and ashes, for manure, found in this very meadow a gelatinous substance, and was told by a labourer that he had frequently seen similar ones there; a circumstance which had escaped my friend and me, though both often passing through it.

My friend having brought me this substance for examination, my mind immediately reverted to that examined by Mr. Buchner; and perceiving even outwardly several differences between them, I undertook a close examination of it, as follows.

The substance was of a very clear white colour, representing a strongly swelled mass, which Mr. B. very properly compares to swelled tragacanth; its bulk might be about 2½ cubic inches. On a closer view, I perceived that in several places it was covered with a very fine white skin, which seemed to have burst in the centre only; in such parts the inside had protruded in the shape of a very bulky gelatinous mass. The bursting of the skin had undoubtedly been produced by the swelling of the mass on account of the moisture it had absorbed from the meadow, a tension which the thin membranous covering could not resist. In such parts too the membrane was so concealed by the jelly that it could scarcely be perceived. Nor had the jelly here any distinct form, or any traces of organization. But wherever the mass had remained entire, and although swelled was still inclosed, it showed a vermicular appearance of the thickness of a quill and thicker; whilst in those places where the membrane had burst, the inside projected in lumps of three-quarters of an inch in thickness. This vermicular appearance presented several small slightly indented divisions, and had entirely the figure of an intestine; the back was marked by a tender vessel of a darkish-brown colour, which spread with fine veins towards the front, losing themselves about the middle in small blackish points. By this vessel the back of the mass was entirely contracted, and much extended towards the surface, just like an intestine.

In a dry place the substance gradually shrunk, soon lost its white colour, turned to a brownish yellow hue, became very tough, so that it could be drawn into threads like glue, and dried at last into a horny mass.

Burned in a crucible of platinum it swelled, became gradually black, emitted a strong animal smell, like singed wool, and left 1·2 of grayish white ashes, which were scarcely affected by water for some time, but at length gave a slightly alkaline solution. In nitric acid the ashes were completely dissolved, and ammonia gave a precipitate of phosphate of lime.

Twenty grains of this substance were desiccated on a water-bath. It became hard and tough; and its weight was reduced to four grains. Moistened with water, it in a short time re-assumed its former size and white colour.

One hundred grains of the substance were boiled in three ounces of water; by this it swelled into a tremulous jelly, which was so bulky that it had thickened almost all the water; for the whole being deposited on a loose clean linen cloth, it congealed upon it; and after several hours but little of the liquid had oozed out, which was rendered turbid by protonitrate of mercury and acetate of lead, but not by superacetate of lead. A little of the substance was shaken with alcohol; this acted but little, but separated from it some of the water, and on its bulk being diminished, it likewise lost its transparency.

A solution of ammonia acted but slightly, whether in the cold or by heat; but a solution of caustic potash affected it perceptibly even while cold, and entirely absorbed it when heated:

heated: neutralized by any acid, the substance was again precipitated. The sulphuric, nitric, and muriatic acids also acted upon it while cold, and entirely dissolved it by heat. The nitric acid turned somewhat yellow, the sulphuric acid brown; the muriatic acid remained colourless.

From these experiments it is evident that this substance did not resemble *albumen*, but essentially agreed with *jelly*, and resembled the slime of springs. It consisted of

| | |
|---|---|
| Gelatinous substance | 18·8 |
| Animal ditto | traces |
| Phosphate of lime and phosphate of soda, with an organic acidity | 1·2 |
| Water | 80·0 |
| | 100·0 |

What now is the origin of this substance?

The evident existence of an organic structure will not allow any opinion of its being of an atmospheric formation, but shows on the contrary that its origin must be terrestrial and of an animal kind. Its striking resemblance to an intestine, led me at first to suppose that it might have been the intestine of a bird; but its containing a smooth jelly, and being inclosed in so fine a membrane, compared with the tougher skin of any intestine, which could not have swelled to the degree mentioned, the want of the usual contents of an intestine, &c. left ultimately no room for such a supposition. But the chemical resemblance of this substance to the spawn of frogs, led me to the idea of its being perhaps the spawn of some animal. It could not be that of a frog, but it might be the swelled spawn of a snail, such as are frequently found in damp meadows; as the *Limax rufus, agrestis, stagnalis, &c.* I compared the descriptions given in this respect in Cuvier's Comparative Anatomy; in Oken's Natural History, iii. chap. i. p. 309; in the same author's Natural History for Schools, p. 668; in Goldfuss' *Hanbuch der Zoologie*, i. p. 661, and other works;—in all of which, however, I found little information respecting the object of my research; viz. the spawn of snails. Oken, in his Natural History for Schools, however, remarks, when speaking of *Limax stagnalis*, that " its spawn was a small gelatinous cylinder, of one inch in length and one line in thickness, containing a dozen small yellow eggs; that these rows of eggs generally adhere to some aquatic plants, and at the end of a fortnight or three weeks the young snail crept out of it." Oken further mentions in his Natural History, concerning the *limaces:* " The excremental canal of the sexual bladder is short, ends in the vagina close to the ovarium, much longer as it should

 seem,

seem, in proportion to the penis, and the ovarium connects with it before it enters the vagina. There is, therefore, no doubt that this bladder forms a portion of the female parts, and furnishes perhaps the gelatinous substance for the eggs, especially as this bladder is found in all snails. Their contents, however is solid, soft like pomatum, and red-brown, which has led to the erroneous supposition of its being purple." (Compare also Cuvier on the same subject.) Although the egg-sticks of the *limax*, as mentioned, are very small, it is yet possible to suppose our substance to have been the spawn of a *Limax rufus*, or some other species, since its great bulk chiefly arose from water,—a reason too why its solid contents, compared with its volume, were so small, and those of the water so great; and I was enabled, as we have seen in the experiment with boiling water, to swell it to its actual extent. This supposition was further confirmed, on my finding in a portion of the substance which I had placed in a small cup before the window of my study for a few days, a little naked snail (*limax*) of about a quarter of an inch long. I think, therefore, I may positively assert that the white gelatinous substances which are occasionally found in damp meadows, and frequently pronounced to be the substance of shooting-stars, are not of atmospheric origin, but consist of the spawn of the above-mentioned snails, which, although small in its natural state, and therefore remaining unobserved, assumes, in damp places, by absorbing water, the large bulk and white gelatinous appearance, necessarily attracting the attention of persons who find them in their way; and finally, that its being found only in damp places is owing to the very nature of this spawn.

I doubt whether the real substance of a shooting-star has ever been found. Whoever has observed these meteors, must be convinced that one could not so readily notice the spot where they seem to fall in the darkness of night, as to be able to find out the supposed substance, and as it were be able to say that he has a fallen star in his hand. Before this can be clearly shown, it may yet have to be proved whether any opinion can be formed as to the nature of shooting-stars. Even the observation of the above-mentioned American meteor seems subject to doubt; and it is still a question whether the produce of a fiery meteor could be a gelatinous mass?

Our knowledge of meteors is indeed much increased by the inquiries of Professor Brandes of Breslaw; but still the nature of the substance seems to be enveloped in doubt.

It now only remains for me to consider the apparent differences which seem to exist between the observations of Messrs. Buchner and Schwabe, and my own; which may be

satisfactorily

satisfactorily done in consequence of the precision with which those gentlemen have written their accounts.

The substances they have respectively examined offer, as I have stated above, qualities so different in their nature, as to ascribe to each a different origin; so that I cannot agree with Mr. S. in supposing the substance examined by him to be of the same kind as Mr. B.'s, while I readily admit that the substance he has investigated was a real *tremella*. Mr. B.'s substance, on the contrary, fully agrees with that examined by me. Their chemical contents are quite the same; and the only essential difference between them seems to be, that the substance examined by him no longer exhibited any organic structure; whilst that observed by me still bore the evident traces of an animal production. But if we consider that this snail-spawn had lost all appearance of organic structure wherever the membrane had burst, and especially when it was much swelled with water, I suppose that Mr. B. found this substance so swelled, that it had burst the membrane in every part, so as to cover and obliterate the organic structure entirely. That the mass examined by Mr. B. was actually very much swelled, appears from the fact, that he found only 40·4 of solid substance; whilst the one examined by me still had 20·0. If then the identity of the two cannot be doubted, we must at once ascribe the same origin to both,—that of being the spawn of a snail.—Thus I think I have explained the nature of the substance called star-jelly, and have had the satisfaction of reconciling the differences between the observations of Messrs. B. and S.; having shown that the observations of both these meritorious naturalists were correct, but that the substances were entirely different in their nature.

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## XLV. *Description of a Percussion Rifle, igniting by a Spring instead of a Lock. By Lieut.-Col.* Miller, *F.R.S.**

THE stock of this rifle is made either of iron or bronze†, hollow in the centre, and the barrel is made to screw into it at the breech. The spring acts horizontally, and is screwed to a plate, fixed to the left side of the small of the stock. A cross piece is attached to the fore part of the spring, which passes through the stock behind the breech, and projects a little on the left side. In the cross piece there is a notch, and a button at the end of it;—the trigger moves upon a pivot in the upper part of the stock, and is pressed forward by a spring behind it. The piece is cocked by grasping the small of the stock

* Communicated by the Author. † As manufactured by Nock.

stock with the fingers of the right hand, and pressing the thumb against the button of the spring until the notch comes opposite the trigger. The cap is then put on the nipple, and the spring let down upon it, by again placing the thumb against the button, and pulling the trigger with the middle finger. In that position the spring is allowed to remain, until the piece is about to be used, when it is again cocked and fired off. Fig. 1. represents a side view of the gun; and fig. 2. the action of the spring. A piece of leather is put round the small of the stock, to make it more pleasant to the touch; and a box placed behind it, for holding caps and patches. The power of the spring may be increased or diminished at pleasure, by turning the screw.

This contrivance, it is conceived, produces fire more instantaneously than a common lock, from its having no friction. It is also less liable to get out of order; and a gun can be constructed on this principle, at about half the expense of that now generally in use.

A rifle of this construction was tried at Woolwich in August last, of which the following is the result.

Woolwich, August 1, 1827.

Report of the experiment carried on this day with a percussion rifle, igniting by a spring instead of a lock, proposed by Col. Miller. The rifle was fired by Col. Miller, and at the ranges of 400 and 100 yards, proposed by himself. The powder used was Curtis and Harvey's, brought by Col. Miller. The target was one of 9 feet, of two boards each an inch thick.

Target at 400 yards, balls 28 to the pound.

| | | |
|---|---|---|
| Round 1. | charge 1 drachm; grazed | 100 yards short. |
| 2. | ———————— | 100 yards short. |
| 3. | ———————— | 20 yards short. |
| 4. | ———————— | 20 yards short. |
| 5. | ———————— | 30 yards short. |
| 6. | ——— $1\frac{1}{8}$ drachm; ——— | 30 yards short. |
| 7. | ———————— | over all. |
| 8. | ———————— | struck target 4 feet 7 inches right of bull's eye, 1 foot 9 inches under; ball penetrated 1 inch, and lodged in target. |
| 9. | ———————— | grazed 20 yards short; struck target, and dropped; ball made an impression half its own diameter in depth. |
| 10. | ———————— | grazed 28 yards short. |

Round

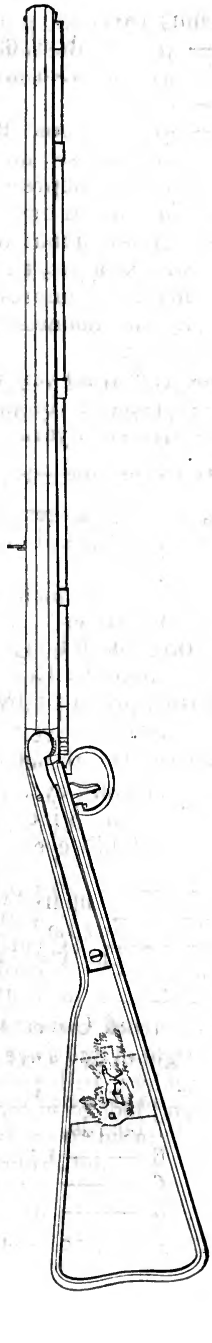

Fig. 1. View of Gun, one-sixth the original size.

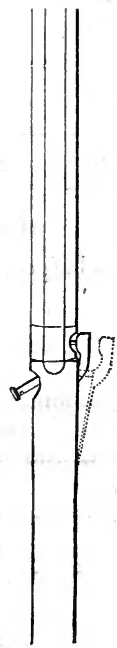

Fig. 2. Action of the Spring, one-fourth the original size.

Round 11. charge $1\frac{1}{4}$ drachm; over.
12. ———————— grazed about 60 yards short.
13. ———————— grazed twice, and struck target.
14. ———————— over.
15. ———————— grazed about 20 yards short, struck target, and dropped; ball made an impression half its own diameter in depth.
16. ———————— grazed about 80 yards short; struck target, and dropped; ball made an impression one-fourth its own diameter in depth.
17. ———————— over.
18. ———————— grazed 20 yards short.
19. ———————— grazed to the right of target.
20. ———————— grazed 3 yards short.

Target at 100 yards distance.

Round 1. charge 1 drachm, through target (hung fire a little) 21 inches above, 7 left of bull's eye.
2. ———————— through, $\frac{1}{2}$ inch under, $1\frac{1}{2}$ left of bull's eye.
3. ———————— through, 9 inches under, 3 right of bull's eye.
4. ———————— through, $1\frac{3}{4}$ inch under, $1\frac{1}{2}$ left of bull's eye.
5. ———————— through, 9 inches under, 5 left of bull's eye.
6. ———————— through, 6 inches under, 8 left of bull's eye.

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Penetrations of Col. Miller's rifle compared with the Government rifle, through elm boards, each $\frac{1}{2}$ an inch thick, steeped in water, and placed in a frame $\frac{3}{4}$ of an inch apart from each other, at the distance of 60 feet.

Col. Miller with his own rifle, and Government powder.

Weight of rifle, 7 lbs. 10 oz.; length of barrel 23 inches.

Round 1. charge 1 drachm, flashed once, barrel just washed; through 7 boards, struck 8 smartly.
2. ———————— 6 ——— split the 7th.
3. ———————— 6 ——— ————
4. ———————— 6 ——— struck 7th.
5. ———————— 6 ——— struck 7th slightly.

Col.

Col. Miller, with his own powder.

Round 1. charge 1 drachm (Not taken, struck near a former shot).
2. ———— through 7 boards, struck 8th slightly.
3. ———— (The same as the first).
4. ———— through 7 boards.
5. ———— 6 boards, struck 7th slightly.

Government rifle with Government rifle powder.

Weight of rifle $9\frac{1}{4}$ lbs.; length of barrel 30 inches; balls 20 to the pound.

Round 1. charge $3\frac{1}{2}$ drachms (4 drachms including priming) through 8 boards, just touched the 9th.
2. ———— 8 boards, split the 9th.
3. ———— 8 boards, splintered the 9th.
4. ———— 8 boards, splintered the 9th.
5. ———— 8 boards, splintered the 9th.
6. ———— 8 boards, split the 9th.

Col. Miller's rifle, 28 balls to the pound.

Round 1. charge $1\frac{1}{4}$ drachm, through 7 boards, struck the 8th.
2. —— $1\frac{1}{2}$ —— through 6 boards, struck the 7th.

Government rifle, 20 balls to the pound.

Round 1. charge 1 drachm, through 5 boards, struck the 6th.
2. —— $1\frac{1}{4}$ —— —— 5 boards, struck the 6th.
3. —— $1\frac{1}{2}$ —— —— 6 boards, struck the 7th.

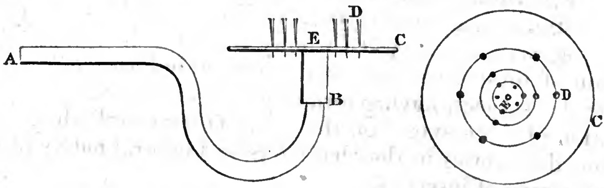
Here ends the Report.—From the preceding experiments, the great superiority of percussion over the flint-lock is very apparent, as the penetration of the percussion rifle is not very much inferior to that of the flint-lock, with a much heavier ball, and four times the quantity of powder. The penetration of the percussion rifle also does not appear to be increased by increasing the charge beyond a drachm; which tends to prove, if any proof were wanting, that moderate charges are the best, and that heavy ones only tend to destroy arms, and an unnecessary expenditure of ammunition, without producing any corresponding effect.

## XLVI. *Account of an improved Form of Apparatus for exhibiting* M. Clement Désormes's *Experiments on Currents of Air, &c.* *By* Robert Younge, *Esq.**

THIS improvement consists in the application of moveable pins on the plate, which enable the operator to vary the size of his discs; and it is presumed involves the explanation of these phænomena. The form of the glass tube permits the effect to be very distinctly seen, particularly when the current is powerful, the edge of the plate being on a level with the sight.

AB is a bent glass tube about six inches long; C is a circular copper plate, having a tube of the same metal attached underneath, which receives the end of the glass tube B.

D are upright moveable pins †, passing through holes in the plate, which are represented in the circular drawing. At E the plate is perforated, to form a communication between the upper side of the plate and the glass tube ‡.




By commencing with a large disc, and gradually reducing its diameter, we may perceive corresponding alterations in the force of the resistance which it offers; and by striking circles of different diameters round the central opening, and leaving three or more smaller holes in each circle for inserting the pins, we may easily apply discs of any diameter; and *when these bear a certain proportion to the diameter of the central opening, they are invariably blown off.*

In No. 2. of the Journal of Science, p. 473, it is observed, "If the issue for the vapour be turned towards the earth, and the disc consequently tend to fall, as well by its own weight, as by the pressure of the vapour, still it will not descend." Might not the amount of this counter-attraction, or overcoming of gravity, be pretty accurately estimated by the application of a series of discs, of different weights, varying according to the diameter of the opening, and weighted in proportion to the power of the current?

* Communicated by the Author.

† The sole use of these pins is to keep the discs over the central opening.

‡ This apparatus was exhibited at the last meeting of the Sheffield Literary and Philosophical Society.

XLVII. *Of*

**XLVII.** *Of the Insect called* Oistros *by the Ancients, and of the true Species intended by them under this Appellation: in reply to the Observations of* W. S. MacLeay, *Esq., and the French Naturalists. To which is added, A Description of a new Species of* Cuterebra. *By* Bracy Clark, *F.L.S., and Foreign Member of the Royal Academy of Sciences of Paris.**

IN the 14th volume of the Transactions of the Linnean Society, is a communication written by my friend W. S. MacLeay, Esq., intended to prove that the fly, intitled *Oistros* by the ancients, was not the insect so named by Linnæus, but that it probably belonged to the present Linnean genus *Tabanus.*

Being of a contrary opinion, I am led once more to address this learned Society, to lay before them the grounds on which it is founded, that naturalists may not incautiously and too hastily adopt the above conclusion, and that they may avoid the confusion which change of names and counter changes always produce in science. I am also led to this undertaking in order to vindicate Linnæus himself, our great master, and such distinguished naturalists as Vallisneri and Reaumur, with whose views on this subject I wholly concur. Nor is the justification of myself wanting as a motive to induce me to re-examine the subject, having formerly sent to this Society a dissertation of some extent on the genus *Œstrus*, unfolding some curious discoveries in the characters and natural habits of this singular race of insects †.

Disputations about the meaning of the ancients, and identifying their descriptions with the modern species of natural history, would perhaps, in a general way, be better avoided in the valuable volumes of this Society, as leading to much desultory and unsatisfactory discussion: practical subjects and didactic facts would perhaps better maintain their reputation. As, however, the Society have in this instance already admitted the discussion, it is but fair and just to allow the reply in the same channel, that the impression, if erroneous, may be removed.

W.S. MacLeay, in the paper alluded to, insists that the οἶστρος of the ancients, and the *Brize* or *Breeze* of the old English poets, is not the *Œstrus* of the moderns; and he infers this from the anatomical characters which some of the ancient authors have left us of their insect. Now, besides the anatomical descriptions to be found in the works of philosophers, there is another mode of identifying the insect; and that is, by the description

* From the Transactions of the Linnean Society, vol. xv. Part ii. p. 402.
† Published in the 3rd volume of the Society's Transactions.

description of the effects it produces upon cattle, and which are so singular, that they have afforded incidents to most rural poets, ancient and modern: and the truth seems to be, that the poets in describing these effects have been true to nature; while the philosophers, being presented with a wrong insect, have only involved the subject in error.

That it is an Italian insect we have the authority of Vallisneri of Padua, who appears to have been the first naturalist who bred the true *Œstrus Bovis* from the grubs found in the backs of the cattle; and for the first time, as far as we possess any record of the subject, saw with certainty the identical object that created so much commotion among them. He applied correctly enough the passages of the ancients which he thought had allusion to this insect. Reaumur followed Vallisneri in these researches, and bred with great difficulty one imperfect specimen of the true *Œstrus Bovis.* Linnæus next followed; but not having ever seen the insect, and not daring to describe from figures merely, or the descriptions of others, he took the large Horse Bot for it,—the *Œstrus Equi* of my enumeration. This error is continued through all the editions of the *Systema Naturæ*, intending all the while, and referring to Vallisneri and Reaumur for, the true *Œstrus Bovis.* Thus, like some of the ancients, he also described a spotted-winged insect for the *Œstrus Bovis;* whereas the true insect has perfectly spotless wings. The true fly cannot be caught in the act of oviposition, from the violent running of the cattle, and the terror they are in at the approach of their enemy.

This makes it more than probable, nay, almost certain, that if Aristotle, Ælian or Pliny described an insect with spotted wings, or with a trunk or proboscis, &c., they knew nothing about the true *Œ. Bovis*, and had been deceived as to the real object of their research. It was indeed much more easy for them to have been presented with one of the numerous host of flies that infest the backs of cattle and lodge on them, than the true *Œ. Bovis.* Their fly may have been a *Tabanus* or an *Asilus*, a *Conops*, or a *Culex*, or any other with spotted wings; for as the true fly cannot be caught in the act of oviposition, it was next to impossible they should have discovered, or been made acquainted with, the true object of such disturbance. Indeed, during these commotions it would be dangerous to approach the cattle, or to remove any thing from their back; and if an insect was caught under any other circumstance, how could it be known that it was the genuine cause of this agitation?

It is in vain now to inquire what precise fly these ancient philosophers might have been presented with, as their testimonies

monies are various, and militate against each other; but none are descriptive of the true fly, which we now fully know. Surely such a conclusion is more natural and just, than to suppose these conflicting descriptions true, and that the poets and common observers were false witnesses.

I now proceed to give what Virgil says respecting the name of it among the ancients, and the tumult it occasions; and of which no sweat-sucking *Tabanus*, *Conops*, or modern *Asilus*, can in any way be the cause.

> "Est lucos Silari circa, ilicibusque virentem
> Plurimus Alburnum volitans, cui nomen Asilo
> Romanum est, Œstron Graii vertêre vocantes:
> Asper, acerba sonans: quo tota exterrita sylvis
> Diffugiunt armenta, furit mugitibus Æther
> Concussus, sylvæque, et sicci ripa Tanagri."
>
> *Georg.* lib. iii. v. 146.

From this admirable description, it is clearly manifest that *Asilus* was the Roman name for the fly which agitates the cattle; and it is equally clear that *Œstros* was the Greek name for it.

Not much weight is due to the observation, that Homer's insect was not the modern *Œstrus*, because he mentions the spring as the season of its appearance, since he also adds, in the same line, ὅτε τ' ἤματα μακρὰ πέλονται, "when the days are long;" nor that Shakespeare did not use the word *Brize* for the same insect, merely because he has assigned its appearance to the month of June, when it more often appears now in July. Indeed the alteration of style will account for this difference. But the same poet uses the word in another place, where the allusion is too distinct to be mistaken:

> "The herd hath more annoyance by the Brize,
> Than by the Tiger." *Troilus and Cressida.*

And again in an old Play, quoted by Archdeacon Nares in his Glossary, the following use of the word occurs,

> "I will put the Brize in 's tail, shall set him gadding presently."

Now if MacLeay or Latreille, who entertains a similar opinion, had ever been as much among cattle on the heaths, as my pursuits have led me, they would have long since obtained a practical acquaintance with the effects produced by these insects, and would not have been led to suppose that the *Tabani*, *Conopses*, or *Culices*, were the object of poetic description, or have made any mistake between the effects of one and the other. When the *Tabani* and *Conopses* have come and settled in great numbers on the back and sides of the animal, he would, as I have often witnessed, scarcely regard them. A

toss

toss of the head, perhaps, towards the part, if they sucked a little too vigorously; or, if they were still more importunate, a lash of the tail, was in general all the notice he would condescend to take of them. But if an *Œstrus* approached, the consternation was indescribable, and the agitation most remarkable; and the object attacked, however lazily he might be disposed from the heat of the weather, or a full belly, would become suddenly as agile as a young deer, and canter away, holding out his tail, and running with a sort of undulatory movement of the back (thereby endeavouring, perhaps, to disappoint the touch and designs of his enemy), till he had obtained his accustomed retreat in the water, or the fly had quitted him,—

> ....................Tossing the foam
> They scorn the keeper's voice, and scour the plain,
> Through all the bright serenity of noon;
> While from their labouring breasts a hollow moan
> Proceeding, runs, low-bellowing, round the hills.—Thomson.

Assuredly no *Tabanus* can produce any effects like these. Unable to account for this extraordinary agitation, I had formerly given way to the notion of some very painful infliction by the *Œstrus:* but I am now led to question this opinion, inasmuch as I can discover no instrument by which this effect can be produced. The shrill sharp sound, which Virgil describes, was, I dare say, not stated without some real ground; and a friend of mine actually informed me, that he was standing in a farm-yard one day near some cattle, when one of these flies entered and approached them, and that he distinctly heard this shrill sound. In confirmation of this account we may remark, that the wing-scale, covering the *halteres*, which has been supposed by Keller to be the organ of sound, is particularly large in this insect; but further than this we dare not assert, but leave the point for future investigation. We know from Linnæus's own account, that the *Œstrus Tarandi*, or Rein-deer Bot, very similar in all respects to the *Œ. Bovis*, makes no sound while depositing its egg; which again brings me into doubt upon this matter.

We next have to observe, in confirmation of the peculiar effects of these insects upon the animals they infest, that those of the *Œstrus* of the Rein-deer, are equally singular and remarkable; and this fact we have from the indefatigable researches of our immortal leader, Linnæus himself. He says, speaking of the *Œ. Tarandi*, in his Lapland Tour, that as he was in bed early one morning, he perceived a very ungrateful smell, and when day-light appeared, "there were standing about

about the cot a thousand of these Rein-deer, driven by old men, boys, dogs, and women, who milked these animals. They appeared to be under the apprehension of some invisible attack: the animals carried their heads aloft, their ears pricked up and extended, beating the ground, and kicking in the air with their feet, as though by enchantment. Then for a while they would be quiet; then, again, they were seen most furious, and this with so general and regular a movement, that no army would have surpassed their exercises in uniformity."

Linnæus further states, in the *Lachesis Lapponica*, respecting the effects produced by this sort of *Œstrus*, that in passing afterwards into the Lapland alps he observed a Rein-deer, which was loaded with his own package, frequently to stop short and become perfectly quiet and motionless as a pillar of stone, or one suddenly struck with catalepsy; the head held straight out, the ears upright, the eyes fixed; nor could he by any ill treatment be induced to proceed; but in a little while he would again resume his march. Where, I would ask, is the *Tabanus*, or *Conops*, that could produce effects like these? or what naturalist, at all acquainted with the operations of Nature herself, could confound the dissimilar effects produced by these several insects?

Linnæus further says, that in the Rein-deer fly he saw the egg held out "like a white mustard-seed" at the end of the abdomen, which, if true, fully confirms the supposition that there can be no infliction.

The *Œstrus hæmorrhoidalis* and *Œstrus Ovis*, in performing their office of ovi-deposit, are also equally irritating and peculiar, as I have shown in the paper above alluded to, in the 3rd volume of the Society's Transactions.

I avail myself of this opportunity in conclusion, to state, in addition to my former remarks on this genus, that it appears to me, as there is no *aculeus* or weapon of infliction at the end of the abdomen of the female of the *Œstrus Bovis*, that the egg is simply thrust down among the hair, till it meets the skin, and that then it is affixed to it by a glutinous liquor secreted at the same time; and that the egg being hatched, the young grub insinuates itself into, and finally through the skin, forming an abscess beneath it. In a somewhat similar manner it is that the ichneumon flies deposit their eggs on the sides of living caterpillars of the *Lepidoptera*, and hatching, perforate their skins, and entering within, live on the parenchyma or pulp of their bodies till matured and fully grown, when they make their way out again and change to the chrysalis.

I may also remark of the *Œstri*, that they appear to be wonderfully

wonderfully kept from such an increase as would be fatal to the animals they feed upon, by the difficulties and imminent hazards they are exposed to in the act of depositing their eggs. The teeth of the horse must destroy, one should imagine, nine-tenths of the *Œ. Equi*, *hæmorrhoidalis*, and *salutiferus*. The *Œstri* seem however, in the hands of Providence, to make a double recompense for the sufferings they occasion; first, by keeping the animals on the alert during hot weather, when they would be often too idly disposed for their welfare; while the few larvæ which succeed in getting into their bodies, appear to benefit them by their local irritations, stimulating the stomach to a quicker digestion of their watery food, and diverting diseases by their counter irritations of the skin and frontal cavities,—thus producing the effect of issues or vesicatories, which are powerful remedies in relieving and in preventing diseases.

I apprehend that I have now sufficiently shown that the *Œstrus* of the ancients could have been no *Tabanus*, and that it is clear Olivier, who appears to have originated this notion, and who was followed by Latreille, was mistaken.

A very extensive enumeration of this genus is seen in a late ingenious publication, the *Systematische Beschreibung* of J. W. Meigen. It is however in some instances not correct; for on carefully examining the *Œstrus lineatus* of this writer, introduced from Villers, it would appear to be that stumbling-block of systematists in entomology, the *Œ. Bovis* of my enumeration*, and not of Linnæus, as he states, who, as we have repeatedly said, described the *Œ. Equi* for this species. The *Œ. pictus* of this author, beautifully figured by Curtis in the British Entomology, *no.* xxvi. *t.* 106, I rather suspect to be the faucial bot of the Stag†.

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As the species of the new genus *Cuterebra* were taken for *Œstri* till I separated them, and are closely allied to them in their habits, I have ventured at the close of this paper to communicate to the Society a new and undescribed species lately received from America, along with some other insects sent me by my nephew, Joseph Clark, from the Illinois.

* The lines on the thorax, and the figure of Villers, undoubtedly confirm it. Meigen's *Œ. Bovis* is the *Œ. Bovis* of my enumeration, under which this should have come as a synonym.

† I may here observe, that a few days since, in visiting the British Museum, I was shown the insect Dr. Leach has called *Œstrus Clarkii*, and find it only a variety, and scarcely that, of the *Œstrus veterinus* of my enumeration.

CUT-

CUTEREBRA FONTINELLA.

C. thorace atro, lateribus albis; abdomine violaceo, ultimis segmentis albis, nigro-punctatis.

White-tailed Cuterebra, or Blue Rabbit Fly.

*Habitat* in Illinoe Americæ Septentrionalis, cuniculos infestans.

DESCR. *Cuterebrâ Cuniculi* dimidio minor; atra, subcylindrica, cum capite parùm latior. *Frons* insuper atra et circa oculos lucida, infra albida, pilosa, utrinque puncto elevato atro. *Oculi* picei. *Thorax* insuper ater, latè per medium et ad latera pilosus, albus, punctis tribus nigris utrinque notatus. *Alæ* obscurè nigro-fuscæ, sulcis valdè puculatæ et rugosæ, corpore longiores; ad basin squamulâ foliaceâ erectâ, magnâ: tympanum halterem tegens magnum, convexum, marginatum. *Abdomen* breve, atrum, lucidum, supernè violaceo resplendens; segmentis duobus postremis hirtis, albidis, punctisque variis atris elevatis, glabris. *Anus* utrinque quasi forcipe prehensorio armatus. *Pedes* atri.

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## XLVIII. *Notices respecting New Books.*

*An Introduction to Geology: comprising the Elements of the Science in its present advanced State, and all the recent Discoveries. By* ROBERT BAKEWELL. *The Third Edition, entirely recomposed, and greatly enlarged. With new Plates, Cuts, &c. Octavo. pp.540.*

THE readers of the Philosophical Magazine, may probably remember, that after the first edition of Mr. Bakewell's Introduction to Geology appeared in 1813, the late Mr. John Farey, a practical geologist and accurate observer, published a very copious analysis of the work, in several Numbers of the Magazine, (see vol. xli. and vol. xlii.*) A second enlarged edition of the Introduction to Geology was soon after published; and it was translated into German by Mr. Fred. Müller, of Friburg. The work has now been several years out of print, and has been frequently called for. We shall extract from the author's Preface a brief account of what has been done in the third edition, in order to present a distinct view of

* Mr. Farey opposed with some warmth, the parts of Mr. Bakewell's work which were at variance with his own opinions on the Geology of Derbyshire; but he concludes his strictures with the following remarks: "I beg however distinctly to state, that I have yet perused no systematic work on Geology, which I think entitled to a general and careful reading, in any degree compared with Mr. Robert Bakewell's 'Introduction to Geology,' and which I sincerely wish may be read and studied by hundreds of persons, with no less care and attention, than I have bestowed upon it. Because I can assure them that it contains more facts concerning our planet, and fewer absurdities and whimsical assertions and theories, than any of the numerous systematic works which have preceded it, in our own language, or I believe in any other."—*Phil. Mag.* vol. xlii. p. 246.

of the progress of geology, and the important discoveries that have recently been made.

"The causes which have retarded the publication of a third edition it is unnecessary to mention: the delay has, I trust, been favourable to its appearance in a very improved state; as I have been collecting materials for it, during several years, having visited almost every situation of much geological interest in our own island, from the Land's End in Cornwall, to the Grampian Mountains in Scotland; and passed part of three years in examining the geology of Savoy, Switzerland, and France. There is scarcely a rock-formation described in the present volume, that I have not examined in its native situation, and compared with the descriptions of former geologists. I have also had opportunities of examining the collections, and of profiting by the communications, of some of the most eminent geologists on the Continent.

"While engaged in these pursuits, I have not been inattentive to the labours of other observers. So numerous and interesting are the discoveries made in geology during the last ten years, that in order to present a concise view of the science in its present advanced state, the 'Introduction to Geology' has been recomposed, and all the chapters are greatly enlarged.

"The following new Chapters have been added:—Chap. II. On Fossil Organic Remains. Chap. IV. On the Principles of Stratification. Chap. X. A Retrospective View of Geological Facts. Chap. XVIII. On the Destruction of Mountains; and on the Bones of Land Quadrupeds, found in Diluvial Depositions and in Caverns. Chap. XIX. On the Formation of Valleys; and on Deluges and Denudations.—The Plates are new, except Plate IV. and part of Plate VII."

"In the course of the present work, I have frequently attempted to elucidate the geology of England, by comparisons with situations I have examined on the Continent, in order to connect the geology of our own island, with that of France, Switzerland, and Savoy.

"By comprising the numerous facts and observations contained in the present volume, within the limits of an elementary work, from the desire to be concise, I may have run the risk of becoming obscure: this I have studiously endeavoured to avoid; my chief aim being to present the reader with a system of geology, which shall explain geological phænomena in a clear and intelligible manner, and as free from technical obscurity as the nature of the subject would admit. In order that the price may not exceed that of the last edition, this work is printed in a smaller type."

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## XLIX. *Proceedings of Learned Societies.*

### LINNÆAN SOCIETY.

March 4. A COMMUNICATION from the Rev. Leonard Jenyns, M.A. F.L.S. was read, On the distinctive characters of two British species of *Plecotus* Geoff. supposed to have been confounded under the name of *Long-eared Bat.*

A new

A new Bat found adhering to the bark of a pollard willow, and which the author names *brevimanus,* is discriminated in this Memoir from the *auritus,* which together with *barbastellus* make up Geoffroy's subgenus *Plecotus* of the Vespertilionidæ.

Spec. Char. *P. brevimanus,* Lesser long-eared Bat, "vellere suprà rufo-fusco, subtùs albescente; auriculis oblongis, capite haud duplò longioribus; trago ovato-lanceolato; caudâ antibrachium longitudine æquanti, apice acuto."

The difference in absolute size, in the relative proportions of the parts, in the colour, and in the apparent habits, seem to require the making it a distinct species.

March 18.—The chair having been taken by A. B. Lambert, Esq. V.P., Edward Forster, Esq. the Treasurer of the Society, communicated to the meeting the afflicting tidings which had arrived during the day, of the decease of Sir James Edward Smith, their eminent and much-beloved President,—an office to which he had been appointedby the annual and unanimous choice of the Society from its first establishment in 1788, till his death. The Society immediately adjourned.

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## GEOLOGICAL SOCIETY.

Feb. 15.—This day being the Anniversary, after the election of Officers and Council, as noticed in our last Number, a Report from the Auditors and Council was read and approved; after which the following Address was delivered from the chair by W. H. Fitton, M.D. F.R.S. &c. the President of the Society.

GENTLEMEN OF THE GEOLOGICAL SOCIETY.

You have just received from your Council a report on the condition of your finances; with a statement of the accessions to your number during the past year, and of the measures adopted for advancing the welfare of the Institution. It remains for me to lay before you a few remarks on the branch of knowledge which the Geological Society is intended to promote: and what I shall offer upon this subject will be confined, in a great measure, to the state of Geology in this country; since neither have my opportunities of acquiring information during the past year enabled me to give, nor does my duty appear to call for, a more extended view;—though such periodical reports in other hands, and on more suitable occasions, have been frequently attended with advantage.

We have had since our last Anniversary to regret the loss of one of our foreign members. Mr. Brocchi, whose death, according to the accounts we have received, took place in Egypt, whither he had been invited to discharge the duties of a mining Engineer, is distinguished in the recent scientific history of Italy by numerous contributions to the Geology of that country;—and his principal work "On the Fossil Conchology of the Subapennine Hills," abounds in valuable observations, and in proofs of accuracy and acuteness in the comparison of the fossil and existing species. His talents, however, were not merely those of an observer;—his general views were always wide

 and

and philosophic; and the style of his writings is considered by competent judges as remarkable for its purity and good taste. But those only, who have had the pleasure of being personally acquainted with Mr. Brocchi, could appreciate his patriotism and philanthropy, the variety of his acquirements, and the spirit and eloquence which rendered his conversation more than commonly instructive.

The printed "Proceedings" of the Society, and the portion of the Transactions published within the year, are the best records of our contributions to geological science during that period: and the volume now in progress will, I trust, be found to have contributed in no small degree to the advancement of inductive Geology. New monsters, it is true, have not been brought to light from the depths of our strata; nor has Zoology been enriched with new genera, by such rare coincidences of genius and good fortune as distinguish the last volume of our Transactions: but the Geology of England has been illustrated by various memoirs, on tracts not previously examined; and by more exact and extended researches on portions of our strata, the general relations of which had been before determined. Correct data have thus been recorded, to which inquirers in other countries may refer, for the purpose of comparison with their own.

I have to congratulate you upon the progress which has been made in the Trigonometrical survey of Ireland; a work designed with all the skill of modern science, and committed for the execution to such hands, and with such instruments, as to leave no doubt of the result. Maps alone, such as this survey will produce, are an acquisition of the first importance to our inquiries; since they form one of the chief and indispensable instruments of geological research:—but with these, in the present case, will be connected a series of observations more strictly geological, which cannot fail to throw great light upon the structure and composition of the country to which the operations extend. The Tract, which I now show you, has been drawn up by one of the principal officers engaged in the Irish survey*, and lithographized for the use of the subordinate surveyors; and it contains so clear and able a system of instruction for their guidance, illustrated by sectional sketches, as greatly to facilitate the task of geological investigation. The surveyors will thus accumulate a series of specimens, the precise places of which will have been recorded in maps upon a very large scale,—on which also the heights above the sea will be determined, in points almost innumerable; while sections are taken in well chosen situations, for the purpose of illustrating more effectually the order of the strata. The ultimate results of operations so well combined, must be equally honourable to those who are engaged in this vast work, and fertile in various and substantial advantage to physical science.

But while the survey of Ireland is in progress, it is to be hoped that that of England will not cease to advance; and that no great delay will take place in the publication of the maps which have been actually prepared by the Ordnance. To geologists who have travelled in England, I need not mention the benefits that our science has derived

* Captain Pringle, of the Royal Engineers.

from

from the maps already engraved; nor dwell upon the misery of plunging, from a tract that we have traversed with the advantage of this guide, into regions where the survey leaves us, lost, as it were, and bewildered from the want of such assistance. The sheets of the Ordnance Survey which I now lay before you, represent a portion of the midland counties, coloured geologically by a gentleman whose activity and accuracy of research have made him minutely acquainted with the stratification of the district around him*; and the maps thus coloured, are probably as complete specimens of geological illustration as ever have been produced. The knowledge of this observer extends with equal precision several miles to the north of the tract here represented; but these sheets, you perceive, are bounded by a right line;—and beyond that line it has not been in his power to extend his colours, because no good map of the adjacent district is in existence. In this instance therefore, and no doubt in numberless other cases, the want of adequate maps may cause the final and irreparable loss of much geological information: And when it is considered, that Geology is but one of many departments of useful inquiry, to which good maps administer,—how much they contribute to the advancement of commerce, and to the comforts and conveniences of life,—it will be unnecessary to urge the enlightened and public spirited persons, to whose hands this great undertaking is committed, to finish with as much promptitude as possible what has been so admirably begun.

The effective establishment in this country of a society for the cultivation of Zoology,—a source of just gratification to all who are interested in the progress of Natural History,—is an event connected very intimately with the advancement of our subject: for to the Geologist it is of great importance to obtain facility of access to cabinets and to living specimens, in elucidation of fossil remains; and to have the privilege of appealing, in doubtful cases, to competent authorities, in what relates to the animal kingdom. But the connection of Zoology with our science, is a field too wide to be discussed upon the present occasion; nor would my own acquaintance with the subject justify my dwelling upon it.

The numerous provincial institutions, which have been recently established for the promotion of useful knowledge, will also materially contribute to the diffusion of a taste for Geology; and will throw new light upon the structure and productions of their respective districts.

I wish that it had been in my power to speak with equal gratification, of the relation in which our subject stands to another principal department of Natural History; but the fossil remains of the vegetable kingdom do not appear to occupy, at present, a just share of the attention of Botanists in England: and hence it has happened, that of the numerous and interesting specimens of fossil plants continually brought to light from our strata, especially within the coal districts, the greater part has been sent for illustration to those naturalists on the continent, whose publications upon this branch of inquiry, are so creditably known to science. Ought we not then

* Mr. Lonsdale of Bath.

to

to imitate the example of those, for whose labours we have so much reason to be grateful; and to reflect, that—if the botanical characters of fossil specimens be obscure, and the investigation of them at present unsatisfactory,—the subject is still comparatively new, and the difficulties such as perseverance and the multiplication of specimens must every day diminish: whilst the views to be derived from the connection of vegetable remains with geology, are scarcely inferior in interest to those already disclosed by the fossil remains of animals? The distribution of plants upon the former surfaces of the globe,—its relation to the epochs of geological deposition,—the variations it may have undergone from change of climate, either by alteration of internal temperature, or of elevation above the sea;—the former existence of vegetation in the more complex forms, in tracts where scarcely any traces of it exist at present,—are subjects which give rise to some of the most important general questions connected with the history of the globe;—and that require for their due consideration such an acquaintance with the characters of fossil vegetable remains, as none but the most skilful and experienced botanists can be expected to possess.

On the Geology of foreign countries, the last year has not been unproductive. A valuable paper on the structure of Jamaica, has been published in our Transactions, by one of the most skilful of our practical observers. We have received a very important contribution of specimens from Captain Franklin and Dr. Richardson, under whose direction the expedition to the northern coast of America has been conducted with so much ability and success;—and a memoir by the latter, on the structure and components of those regions, will soon be read at one of our meetings. Captain Parry also, and Captain Foster, have presented us with a valuable collection of specimens from Spitzbergen, obtained during their late expedition to the north. Captain King, who has enriched our cabinets with specimens from the coasts of Australia, and done so much for other departments of natural history, has recently sent home a collection of rocks obtained during the earlier part of his survey in the Straits of Magellan; and further collections, accompanied with new information, may still be expected from the same indefatigable observer. We have reason also to hope that Geology will not be neglected during the expeditions soon about to sail,—of Captain Boteler, for the survey of the western coast of Africa, and of Captain Foster, for the purpose of determining the longitude of important stations on the shores of the Atlantic.

There is the greater reason to rejoice in the contributions thus given, or to be expected, from the Naval department of the public service, since it has not unfrequently been the reproach of this country, that—possessing colonies, which have dispersed the natives of Britain in every region of the habitable globe, and commerce, that maintains continual intercourse with them,—the benefits conferred by England on the natural history of distant countries, have fallen very far short of what the intelligence and activity of our national character might have afforded. Let us hope, however, that brighter days

days are opening upon us; and that those who are employed in the various departments of our foreign service, will universally feel, that where such frequent opportunities of advancing useful knowledge are likely to occur, an acquaintance with branches of science not immediately essential to professional duty, is strictly accordant with the dignity of the naval and military character.

Among the donations of foreign specimens to our cabinets, there is one of very peculiar interest;—the rich collection of fossil bones and shells presented to us by Mr. Crawfurd from Ava: which has the greater value, as it is one of the first collections of this description, that has made its way into England from our extensive empire in the East, or the adjoining territories. These specimens afford some very striking novelties, both to the Geologist and Zoologist; an account of which, I trust, will soon be laid before you by competent describers.

The last year has produced some valuable publications on the Geology of Volcanoes, which, though not emanating immediately from this Society, are the work of our own members. We are indebted to Dr. Daubeny for a judicious volume, in which he has combined what had been previously published on volcanoes, with much valuable observation of his own. The productions of Mr. Scrope, though his speculative views are not free from objection, are full of originality and talent.—To that especially, which describes the extraordinary volcanic region in the centre of France, illustrated with such effect as to render the task of comparison with other districts easy and inviting, I should have had pleasure in alluding more fully; if an eloquent account of it, in one of our leading journals, were not familiar to us all *: and this, also I believe I do not err in ascribing to an active member of our Institution.

In the speculative department of Geology, nothing has been of late more remarkable, with reference to its history in this country, than the universal adoption of a modified Volcanic theory, and the complete subsidence, or almost oblivion, of the Wernerian and Neptunian hypotheses;—so that what, but a few years since, it was by some considered as hardihood to propose in the form of conjecture, seems now to be established nearly with the evidence of fact. It is no longer denied, that volcanic power has been active during all the revolutions which the surface of the globe has undergone, and has probably been itself the cause of many of them;—and that our continents have not merely been shaken by some mighty subterraneous force, but that strata, originally horizontal, have thus been raised, shattered, and contorted, and traversed, perhaps repeatedly, by veins of fluid matter;—operations which have produced phænomena, so nearly resembling those of recent volcanic agency, that to have so long disputed the identity of their cause, is one of the most remarkable proofs in the annals of philosophic history, of the power of hypothesis in distorting or concealing truth. Whatever, therefore, be the fate of the Huttonian theory in general, it must be admitted, that many of its leading pro-

* Quarterly Review, Vol. xxxv. page 447, &c.

positions

positions have been confirmed in a manner which the inventor could not have foreseen.

The most striking modern support of these correcter views, is due to Von Buch and Humboldt, and to the facts and inferences derived by Dr. MacCulloch from the country which gave birth to Hutton, and to his illustrator, Mr. Playfair, and in which were made the experiments of Sir James Hall. More recently, a series of facts observed by Professor Henslow, in the Isle of Anglesea*, has proved, in the most satisfactory manner, the connection of veins of trap with very high temperature; since the change produced upon the strata, through which the substances now occupying the veins were injected, has approached so nearly to fluidity, as to admit of their crystallization, in forms different from any which the components of the rocks, if they had not been thus acted on, would have afforded. Sir Humphry Davy's experiments on the fluids contained within cavities in crystals †, are another striking and unexpected confirmation of Hutton's views: and our own Transactions, besides various incidental pieces of evidence derived from this country, supply the testimony of an unprejudiced witness to an earthquake on the coast of Chili ‡, which brings almost before the eyes of the reader, the movement and permanent elevation of the land.

Having alluded to Mr. Playfair's support of the volcanic theory, it would be unjust to the memory of that distinguished man, not to mention, that his geological writings have had, indirectly, an effect in accelerating the progress of our subject, the benefit of which we experience at this moment, and probably shall long continue to feel; and which, perhaps, outweighs in value the partial success of the speculations for which he so strenuously contended. He clothed our subject with the dignity of an eloquence most happily adapted to philosophic inquiry, and redeemed the geologist from association with that class of naturalists who lose sight of general laws, and are occupied incessantly with details;—placing him, where he ought to stand, beside the mathematician, the astronomer, and the chemist; and permanently raising our science into an elevated department of inductive inquiry. His mild and tolerant character threw an assuaging influence upon the waves of a controversy, which in his time considerations entirely foreign to science had exasperated into unusual violence: and if, fortunately, there is no longer any trace of this asperity, the change must, in a great degree, be ascribed to the tone of Mr. Playfair's writings, enforced by the manly and consistent tenour of his blameless life.

I cannot, for your sake, regret that the presence of some of those who have had a large share in the foundation of this Institution, prohibits my alluding to their continued and unremitting efforts in support of it.—And the same cause prevents my dwelling on the effects produced, at both our Universities, by the geological instructions delivered there; which have given to the subject an impulse perhaps

* Transactions of the Cambridge Philosophical Society, vol. i. page 406.
† Philosophical Transactions, 1822, page 367, &c.
‡ Geological Transactions, second series, vol. i. page 413.

without

without example in the history of those institutions, and gone far to render natural science a permanent department of general education.

But there is one of our number, whom professional and domestic occupations retain so much in a remote quarter of the country, that we have seldom the gratification of his presence amongst us, though his writings are in all our hands : and it is a duty,—not to Mr. Conybeare, but to the subject, and to ourselves,—to say, that among the more recent causes which have accelerated the progress of Geology in England, the publication of the "Outlines of England and Wales," by him and Mr. Phillips, has had an effect, to which nothing since the institution of this Society, and the diffusion of the geological maps of England, can be compared. It is with peculiar pleasure that this statement can be made in this place; since a large proportion of that work has been derived from our own Transactions, and the authors have long been distinguished members of our Society. Of course their publication is not free from defects and inequalities,—inevitable perhaps in a first edition, composed for the greater part during its progress through the press:—but, regarding it as the first general sketch of a country so complex as our own, it may be said without fear of contradiction, that no equal portion of the earth's surface has ever been more ably illustrated;—nor any geological work produced, which bears more strongly impressed upon it the stamp of original talent for natural science.

The object, however, of our Institution, to adopt the language of the charter, is "to investigate the mineral structure of the Earth;"—not to confine ourselves to the British Islands only, (and even they are best illustrated by comparison,) but to extend our researches if possible, to every part of the globe;—to record the geological phænomena of the most distant countries, as well as of our own,—and from the whole, derive the laws that have regulated the structure of this planet, and still influence the changes which are in progress upon it. It is our good fortune, and the fact is intimately connected with the commercial wealth of our country, that it affords a greater variety of strata and of geological appearances, than most other portions of the civilized world of such limited extent; while the range and variety of our coasts unveil the geological anatomy of England, with an obviousness and convincing facility to the observer, that have greatly accelerated our inquiries. The Geology of England, therefore,—which, with a view only to commercial advantage, and to the comforts and conveniences of life, would have well deserved all the labour that has been bestowed upon it,—acquires a new and more dignified interest, when we reflect that this island is in a great measure an epitome of the globe; and that the observer, who makes himself familiar with our strata, and the fossil remains which they include, has not only prepared himself for similar inquiries in other quarters, but is already, as it were, acquainted by anticipation with what he must expect to find there. If, therefore, I were called upon to state in what manner those who have leisure, health, and talent for such inquiries, can most effectually advance the bounds of our science, and increase the reputation which England has begun to acquire

quire in this department of natural knowledge,—I should say, that it would be,—First, by rendering themselves accurately familiar with the geological phænomena of our own country; and then,—by taking abroad with them the knowledge thus acquired, and comparing the phænomena with those of distant regions; since it is only from the multiplication of such comparisons, that sound general views can be derived.

But even within the British Islands, there still are tracts, and of no small extent, which are comparatively, and a great part of them absolutely, unknown. More than one half of Ireland is in this condition: for the publications of Conybeare and Buckland, Stephens, Weaver, Griffith, and Dr. Berger, comprehend nearly all that has been done in that country. But this subject, as I have already mentioned, has passed into such hands, as will, no doubt, accomplish every thing that can be desired.

In the North and North-west of England, the labours of Otley*, Smith, Professor Sedgwick, and some other inquirers, have already ascertained the principal relations of one of the most important districts; but very little has yet been published upon it. And on the mountainous tracts in Wales, the ancient and very interesting essay of Owen †, and the valuable papers of Mr. Aikin and Professor Henslow, with that of Mr. De la Beche on Pembrokeshire, and of Mr. Martin on the Coal Basin of Glamorganshire ‡,—a tract on which Mr. Conybeare is occupied at present, comprehend nearly every thing that deserves to be mentioned here.

In Scotland also, notwithstanding the graphic and copious illustrations of Dr. MacCulloch, and the mineralogical skill and perseverance of other eminent naturalists who have applied themselves to the Geology of their native country,—no geological map has yet appeared; and a great part of that rich and varied region remains to be explored. But the Society will have pleasure in observing, in the last portion of their Transactions §, that an effective comparison of the more recent strata of Scotland with our English formations has already begun. The memoir of Mr. Murchison on the Brora Coal-field is an excellent specimen of what may be effected in this department of inquiry; and a paper produced at the last meeting by the joint labours of Professor Sedgwick and Mr. Murchison, leaves no doubt that the remaining memoirs which are to be expected from those gentlemen, will throw great light on the comparative geology of that distant portion of our island.

The value, however, of the researches and identification at Brora, goes much further than the mere comparison of a remote tract, with the

* The work here referred to, is a brief but valuable notice, "On the succession of rocks in the district of the Lakes," published in the Lonsdale Magazine, or Provincial Repository, for October 1820:—Vol. I. No. x. pp. 433, &c.

† Dated in 1570:—See Cambrian Register, for 1796, and Geol. Trans. N. S. Vol. I. page 312.

‡ Philosophical Transactions, 1806. page 342.

§ Second Series, vol. ii. p. 293.

the stratification of England: they confirm a suggestion of Dr. Buckland and Mr. Lyell, that the coal formation of that neighbourhood was in reality the equivalent of a portion of our Oolitic strata; and demonstrate the remarkable fact, that the same fossils which in England occur in oolitic limestone, exist there in strata of quartzose sandstone and of shale! The whole series indeed, of the phænomena developed by recent examination in Scotland and the north of England, gives rise to the most interesting speculations on the questions of geological identity, and of the relative value in geology of mineralogical and zoological characters,—which has been so ably treated by Brongniart and other continental writers:—questions, which it is necessary to keep continually in view, and that acquire fresh interest and importance in proportion as we extend our researches to the remoter districts of the world.

To those amongst us who are confined to England, the most useful task perhaps would be, when we have mastered the general relations of our series, to take up some one portion of the subject,—a group of strata, or even a single stratum, or any one of the numberless questions connected with their zoological and mineralogical relations,—and to publish in the form of *Monographs* the results of our inquiries. For it may be stated with confidence, that there is not any one of our strata, however familiarly it may be supposed to be already known, that would not, if thus treated, reward the most elaborate and minute examination.

But those who are deprived of the privilege of travelling even in England, must not suppose that they can be of no service as geologists; or if they belong to our body, that they are thus released from their obligation to be active in our cause: and there are two descriptions of persons,—the resident clergy, and members of the medical profession in the country,—to whom what I am about to say may be more particularly deserving of attention. Such persons, if they have not yet acquired a taste for natural science, can hardly conceive the interest which the face of the country in their vicinity would gain, however unpromising it may appear, by their having such inquiries before them; how much the monotony of life in a remote or thinly inhabited district would thus be relieved; nor how much benefit they might confer on the natural history of their country. Even of those who have made some progress in geological studies, many, I apprehend, are prevented from investigating attentively the tracts where they reside, or from communicating their knowledge, by a belief that the Geology of England itself is sufficiently known already; and that the district, with the phænomena of which they are themselves familiar, would have no interest or novelty for the world at large:—whereas it may be asserted (and it were easy to produce examples from modern researches in some of the counties near London), that there is no district that will not furnish sufficient interest and novelty to an attentive inquirer, not merely to repay his own exertions, but to instruct the most learned, and enlarge the bounds of our science.

To landed proprietors also, it can hardly be known, without some tinge of geological information, how nearly our subject is connected with Agriculture,—with an acquaintance with the nature and correctives of the soil, the supply of water, and facility of effectual drainage, and numberless facts essential to the perfection of rural œconomy; the discovery and supply of stone, for building and the construction of roads, the choice of the line of roads and of canals, and the facility of their execution. All these are but a few of the topics that come strictly within the province of the geologist: and which are so essential to the prosperous management of landed property, that a geological map may perhaps with truth be considered as not less necessary to the country gentleman, than the topographical plan of his estates.

I am fully aware, that much of what I have just said is obvious;—and even familiar, to the greater part of those who hear me:—But my object is to be useful; and I believe that some of those whom these remarks are likely to reach, are not sufficiently acquainted with the practical advantages derivable from our pursuits;—and that others are unconscious of the means within their own power for advancing them.

I shall conclude, Gentlemen, by congratulating you on the good feeling by which the proceedings of this Society have always been characterized; and on the self-command that renders both agreeable and instructive the conversations, (I will not call them discussions—much less debates,) with which it is now our practice to follow up the reading of memoirs at our table; and which have given to our evening meetings a character more like that of social intercourse in a private circle, than of the formal proceedings of a public body. This practice, I know, has been a subject of doubt, to many who wish well to our institution, and do not undervalue the personal character and disposition of our members. But, so long as our conversations are carried on with the urbanity by which they have hitherto been distinguished,—while it is the wish of those who share in them to give or to receive information, and not to shine,—and the object is not victory but truth,—there seems to be no reason to apprehend any very serious injury from the continuance of our geological warfare.

There is still another train of thought connected with our meetings, on which I confess I have sometimes delighted to dwell. The spirit in which they have been conducted has been so kind,—so little tainted with, or rather so perfectly free from, any admixture of the leaven with which from interest or ambition most of the pursuits of life are embittered;—and our duties here have been associated with so many offices of cordiality and friendship;—that when, in after life, the cares and chances of the world may have dispersed those whom I have now the happiness to see around me, I am fond to believe that the remembrance of these evenings will be called to mind with pleasure:—And I feel confident, that, as many of us already derive the chief part of our enjoyments from the friendships to which congenial pursuits have led, the Geological Society will continue to be no less effective, in the production of warm personal attachment, and of manly and ingenuous intercourse among its members, than it has been, in maintaining an active and energetic spirit of research.

March 7.—

March 7.—A Paper was read "On the Geological Relations and Internal Structure of the Magnesian Limestone, and the lower portions of the New Red Sandstone series, in their range through Nottinghamshire, Derbyshire, Yorkshire, and Durham."—By the Rev. A. Sedgwick, M.A. F.R.S. V.P.G.S. &c.

A sketch of the subjects contained in this paper was laid before the Society in 1826 (Nov. 17):—They were resumed in a more systematic and detailed form during two meetings in 1827; and are now terminated by the observations read at the present meeting.

The contents of the Memoir are presented in the following order:

PART I.—§ 1. *Introduction.*—The new red sandstone is considered as one great complex formation, interposed between the coal measures and the lias;—with two calcareous formations subordinate to it, one in the lower part of the series (the *magnesian limestone*), and another in the upper part (the *muschel-kalkstein*). The lower of the two calcareous formations is considered in detail; the upper has not yet been discovered among the British secondary deposits.

§ 2. *External characters of the country through which the Magnesian Limestone ranges.*—The form of the western escarpment is described, and is supposed to exhibit proofs of great denudations; and the general character of the soils resting upon the formation is noticed.

§ 3. *General distribution of the formation.*—The range of the escarpment is given in great detail; some errors of the geological maps are corrected; and in describing the eastern boundary, the enormous masses of diluvium in the county of Durham are briefly noticed.

§ 4. *Outliers.*—Sixteen outliers from the western escarpment are described; the most southern of which is at Conisborough. In addition to these, there are eight detached patches of magnesian limestone on the line of bearing, which are not considered as outliers. The most remarkable of these are seen in the range through Yorkshire.

§ 5. *Relations of the Magnesian Limestone to a succession of Coal Measures.*—In a general point of view these formations must be unconformable, because the overlying beds are extended far beyond the limits of the productive parts of the carboniferous order: and the fact is also proved by actual sections in several parts of Yorkshire and Durham. At the same time there are continuous tracts of country where the want of conformity does not appear, and where the overlying beds seem almost to graduate into the coal measures. Several details are given respecting ancient coal works, in which, in more than one hundred places, the coal had been extracted by *shafts sunk through the magnesian limestone:* and it is asserted that the quality of the coal is never injured by the presence of the overlying formations. Such injury is not only contrary to fact, but seems to be a physical impossibility.

§ 6. *On the Faults affecting the Magnesian Limestone and Coal strata, Trap dykes, &c.*—Examples are given of some great faults which traverse both the carboniferous and the superior formations: but it is remarked that many of the dislocations of the lower order of rocks

rocks do not affect the upper. Respecting the age of the trap dykes of the coal-fields, it is not possible to determine their epoch in comparison of the magnesian limestone, where they range up to the escarpment; and of such dykes there are only two examples; one of which does, and the other does not, pass through the beds of the overlying series.

Part II.—*Internal Structure and great Subdivisions of the Magnesian Limestone.*—Considered as a subordinate part of the *new red sandstone* series, this formation admits of five natural subdivisions, each of which is described in a separate section.

§ 1. *Lower Red-sandstone*, or *Rothe-todte-liegende.*—In Yorkshire this appears generally in the form of a coarse siliceous sandstone, of a reddish tinge. It is associated with incoherent sand, red micaceous shale, and sometimes with variegated marls. In Durham it is generally represented by a yellowish and nearly incoherent sand. In some places it cannot be distinguished from the gritstone beds of the coal measures: but as it commences in the edge of Derbyshire, and is almost co-extensive with the magnesian limestone as far as the mouth of the Tyne, it must on the whole be unconformable to the inferior order. It is, however, of very unequal thickness, and its upper beds are not always parallel to the strata of limestone which rest upon it. In Durham, being of loose texture and pervious to water, it throws the greatest difficulties in the way of mining operations carried on within the limits of the limestone.

§ 2. (a). *Variegated Marls, with irregular Beds of Compact and of Shell Limestone.*—This deposit is not either of great extent or thickness, and is confined to a small part of the escarpment in Nottinghamshire and Derbyshire. It is supposed to be contemporaneous with the following subdivision:

§ 2. (b). *Marl-slate, and Compact Limestone.*—This is much more extensively developed than the preceding formation; and though by no means coextensive with the yellow limestone, derives importance from its constancy of position and from its fossils. Several localities in the county of Durham are described; and among the beds of marl-slate of East Thickley, &c., two or three species of fern have been discovered; and seven or eight species of fish, four of which at least seem to be identical with fish of the *Copper-slate.*

§ 3. *Great central deposit of Yellow Limestone.*—It is subdivided into the following modifications, each of which is described in detail. (1) *Dolomite*, a simple crystalline rock.—(a) *Arenaceous dolomite*, coarse, nearly incoherent, often in minute rhombs.—(b) *Small-grained dolomite.* Many quarries of this variety are described as existing on the back of the deposit, and extending from the neighbourhood of Mansfield to Bramham Moor. The crystalline beds pass into others of mechanical structure, and in some extreme cases contain 20 or 30 per cent of siliceous sand.—(2) *Compact magnesian limestone.*—(3) *Laminated.*—(4) *Earthy.*—(5) *Masses of irregular concretionary structure.*—(6) *Beds or concretionary masses of crystalline limestone without magnesia.* Examples of these are derived from quarries near Ripon, Knaresborough, and Newton Kyme, &c. —(7)

—(7) *Brecciated structure.* This modification abounds on the coast of Durham.—(8) *Small concretionary structure.*—(a) *Irregular.*—(b) *Regular or oolitic.*—(9) *Large globular concretionary structure.*—Of this, four principal modifications are described with minute detail. All these several subdivisions of structure are supposed to have been produced by great internal movements, after the mechanical deposition of the formation.

§ 4. *Lower Red Marl and Gypsum.*—This extends from the edge of Nottinghamshire to the banks of the Wharf; thins off at the two extremities; attains its greatest thickness (perhaps nearly 100 feet) on the right bank of the Ain;—but has not been discovered in Durham or the northern parts of Yorkshire.

§ 5. *Upper thin-bedded Gray Limestone.*—Near Ferry Bridge this contains very little magnesia. In other places it contains subordinate dolomitic beds. It commences at Carlton near Worksop, and ranges without interruption to the left bank of the Wharf. Further north it reappears in several places, under a modified form: and the highest beds on the coast of Durham may perhaps be referred to it; but the classifications are made obscure by the absence of the *lower red marl.*

§ 6. *Great Subdivisions of the new red Sandstone which are superior to the dolomitic series.*—In Nottinghamshire these consist of two principal deposits.—(a) *Upper red sandstone.*—(b) *Upper red marl and gypsum.*—The same subdivisions may be traced near the mouth of the Tees. In the central parts of Yorkshire they are obscured by diluvium.

By way of conclusion,—the deposits described in § 1 and § 2, are supposed to be the equivalents of the *rothe-todte-liegende,* the *kupfer schiefer,* and *zechstein.*—The ore described in § 3, 4, and 5, are in like manner supposed to be the equivalents of the *rauchwacke, asche,* foliated *stinkstein,* breccias, and gypsum, which compose the upper part of the Thuringerwald system. The coincidence, in order, mineralogical character, and organic remains, seems to be nearly perfect. In like manner the two divisions described in § 6. are taken as the respective equivalents of the *bunter sandstein* and *keuper*; and, the enormously thick deposits between the coal measures and the lias, with the exception of the *muschel-kalkstein,* are thus found to admit of the same natural subdivisions in England and in central Germany. Finally, the author speculates about the origin of the dolomitic deposits, and adopts in part the theory which derives them from the mechanical destruction of the rocks of the carboniferous order. He states however two facts (1st, the greater abundance of magnesia than could have been supplied by the dolomites of the carboniferous limestone; 2ndly, the fact that some beds contain a greater proportion of magnesia than is found in true dolomites), which seem to imply that the waters of the ocean had a power of separating carbonate of magnesia from the preexisting rocks, in a manner which is not explained by the mere mechanical hypothesis. Whatever may have been the origin of the whole system; its extent, regular subdivisions, and characteristic organic remains, seem to prove,

prove, that it originated in the long continued and consistent operation of powerful causes, acting simultaneously in distant parts of the earth.

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PROCEEDINGS AT THE FRIDAY EVENING MEETINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN.

Feb. 22.—The progress of printing by machines, from the improvement of the press by Lord Stanhope, to the present time, was briefly traced by Mr. Cowper, who at the same time illustrated the various inventions by drawings, models, and many of the working parts of the present machines: after which a more particular account was given of the machines invented by Mr. Kœnig and perfected by himself and Mr. Applegath*, and of the one recently erected at The Times newspaper office. The extraordinary speed of this machine is such, that 4000 impressions on one side can be taken in one hour. The disposition of the form, inking apparatus, paper rollers, and conveying tapes, were explained upon models and sections.

The library contained a variety of interesting objects, and especially several productions of the fine arts.

Feb. 29.—Mr. S. Solly, jun. gave an illustrated account of the analogies and differences between the skeletons of man and birds. The accordance between the skeletons was first pointed out, and that of man referred to as a standard; and then the departures and differences in different animals, with the effects thereby produced, were explained.

March 7.—Supplementary remarks on the subject of February 15th,—*i. e.* the reciprocation of sound,—were given by Mr. Faraday. New cases of reciprocation were adduced, and illustrated; especially of one column of air by another, and of beats and chords by columns of air. The formation of the grave harmonic was referred to, and cases of its reciprocation mentioned: and the theory before laid down relative to the Jews-harp, experimentally proved by an apparatus consisting of a column of air in a tube which could be lengthened or shortened at pleasure, and before which a Jews-harp was firmly fixed and made to vibrate. This apparatus gave all the sounds which could be produced by the mouth, at the same time that the columns reciprocating to the tongue of the instrument could be measured.

Presents: Works of art, specimens of paper prepared in France from straw, and other objects, were laid upon the library table.

March 14.—Mr. Turrell, the engraver, gave an account of the latest improvements in etching upon steel. He illustrated the processes of laying a ground etching, rebiting, &c.; of ascertaining whether the plates were in a proper state; of preparing peculiar menstruums for biting in steel, and many others which constitute the latest improvements of the arts. He showed the application of etching upon steel to the manufacture of graving tools, which

* We believe that all printing-machines which have hitherto been produced since Mr. Kœnig's original invention, have been varied applications and combinations of his principles.—Edit.

could

could be manufactured only in an inferior manner by any other process; and concluded by explaining the principles and use of a new instrument invented by him, and called a Perspectograph.

Amongst the objects in the library was a very large specimen of native silver from Mexico.

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## ROYAL ACADEMY OF SCIENCES OF PARIS.

Oct. 1. 1827.—M. Julia Fontenelle exhibited a well preserved head of an inhabitant of New Holland.—M. Latreille gave a very favourable account of the monograph, presented by M. Bois-Duval, of the tribe of *Zygenides*, of the order *Lepidoptera*.—M. Geoffroy-Saint-Hilaire, made a very favourable report respecting a notice of a monstrous child, by M. Rambur, physician at Ingrandes.—M. Poisson read a memoir on sonorous bodies; and M. Cauchy stated that he had made similar researches.—MM. Milne Edwards and Audoin presented anatomical researches on the nervous system of the *Crustacea*. M. Cagniard de Latour communicated the results of several new experiments which he had made respecting the vibration of sonorous bodies.

Oct. 8.—M. Tournal communicated some details respecting the fossil bones of the caverns of the department of the Aude.—Dr. Thomas Young thanked the Academy for having recently elected him a Foreign Associate.—M. Rousseau, of Coucy-le-Château, sent a memoir on the improvement of forceps.—M. Parseval sent a supplement to his last Memoir on Analysis.—M. de Senne communicated a work on the operation of tracheotomy.—M. Magendie gave a favourable account of M. Breschet's memoir on false consecutive aneurism of the heart, and true aneurism of the arteries.—M. Mirbel made a favourable verbal report on the botanical part of M. Freycinet's Voyage edited by M. Gaudichaud.—M. Frederic Cuvier read an extract of a work on the development of the bristles of the hedgehog.—M. Cagniard continued his communication respecting his researches on sonorous vibrations.

Oct. 15.—M. Julia Fontenelle sent a sealed packet.—M. Delafuge presented a manuscript entitled: *Nouvelle Jérusalem apocalyptique.*—M. Joseph Anastasi sent a memoir On practical mechanics.—The Academy afterwards heard; A memoir by M. Binet on the solution of indeterminate equations of the first degree.—Researches by M. Despretz On the heat disengaged during combustion.—A memoir by M. Gasparin, On European climates.—Researches by M. Desvoidy on the vertebral organization of the *crustacea*, the arachnida, and insects.—And a work by M. Delpech on the resection of the inferior maxillary bone.

Oct. 22.—M. Champie sent a manuscript containing some new propositions in geometry.—M. Serullas announced that he had formed a bromide of arsenic.—M. Cordier gave a very favourable verbal account of the Geological Essay on the Environs of Issoire, published by MM. Devèse and Bouillet.—M. Lacroix, in the name of the commission, read an analysis of M. Binet's memoir On the determination of the orbit of comets, which was not approved of.—M. Savart read

read an extract of a memoir On the elasticity from bodies.—M. Despretz communicated some new experiments on the heat disengaged by combustion.—M. Cagniard de Latour presented the continuation of his experiments on what he terms *les chocs solides des cordes.*—The Academy decided that M. Fresnel's place should be filled up.

Oct. 29.—M. Coste, captain in the artillery, announced the forwarding of a work containing several mechanical experiments.—M. Fossard sent a manuscript entitled: *Utilité de l'horlogerie.*—The Academy heard a favourable account given by M. Mirbel, of M. Despreaux's work, entitled *Essai sur les Lamenaires des côtes de la Normandie;* and a report by M. Cordier on the researches made by M. Marcel de Serres on the extinct volcanoes of the South of France.—The Academy decided that the place vacant by the death of M. Laplace should not be filled up.—M. Payen read a notice relating to a new crystallized borate of soda, and its employment in the arts.—The Section of Physics afterwards presented, in a secret committee, the following candidates for the vacant place: MM. Savart; Becquerel; Cagniard de Latour, Pouillet, Despretz.

Nov. 5.—M. Bernier sent a memoir on the means of descending to the bottom of water.—The Academy proceeded to a scrutiny of the ballot for the election of a member: of 49 votes, M. Savart had 29, M. Cagniard de Latour 9, M. Pouillet 6, M. Despretz 5.—M. Cordier gave an account of the memoir on thermal waters, presented by M. Gendrin. It results from this memoir, as might be expected, and has been already proved, that thermal waters and common water cool in the same manner. The contrary opinion, however widely spread, is therefore a prejudice.—M. Cauchy communicated some new fundamental propositions on the calculus of remainders.—M. Adolphus Brongniart read a memoir containing new observations on the spermatic granules of vegetables.—M. Girard stated that the memoir lately presented by M. Anastasi was unworthy of any examination.—M. Raspail communicated observations on the motion of the tentacula of the *vorticella.*

Nov. 12.—M. Marcel de Serres sent from Montpellier a sketch of a geognostic map of the department de l'Hérault.—M. de Freycinet read a letter from M. Gaymard, dated New Zealand, containing several details respecting the voyage of the Astrolabe.—M. Girard read a memoir respecting some standards of the ancient Egyptian cubit, lately discovered.—M. Boyer read a favourable account of the memoirs presented by M. Faure respecting the iris and artificial pupils.—M. Barry, physician of Limoges, sent a memoir concerning two cases of luxation of the cervical vertebræ with compression of the spinal marrow, which he stated he had reduced: The commissioners named by the Academy did not approve of it.—M. Coquebert gave a verbal account of the new *Annales de Sciences naturelles,* published at the Havana by M. Ramon de la Sagra.—M. Delpech read a notice respecting his labours relating to *Rhinoplastie.*

Nov. 19.—The President of the Council of Ministers informed the Academy, that a marble bust of M. Laplace would be executed for the Library of the Institute.—M. Jomard presented engravings of four measures

measures of the Egyptian cubit.—M. Dupetit-Thouars gave a verbal report respecting the work, entitled *Disposition méthodique des mousses;* by M. Walker Arnott.—M. Bouillaud read Experimental researches on the functions of the posterior portion of the brain.—M. Willermé read the remainder of his Memoir on the distribution of conceptions and births, in relation to the seasons.—M. Sérullas read Experiments on the iodide of antimony, and laid upon the table some observations relating to the bromide of bismuth.

Nov. 26.—The Minister of the Interior sent a Notice of M. D'Hombres-Firmas respecting the fossil bones found in the environs of Alais.—M. Saint-Hilaire deposited the manuscript of a new work which he proposed to publish under the title of *Flore et Pommone françaises.*—M. Damoiseau, in the name of a Commission, gave an unfavourable account of the chronological tables by the Abbé Lachèvre.—M. Le Gendre verbally announced the recent ingenious labours of M. Jacobi of Konigsberg. This geometer has greatly perfected the important theory of elliptical functions.—M. Dupin, in the name of a Commission, read the first part of a report respecting M. Brisson's Essay on the general system of French navigation.—M. Cagniard de Latour read some New experimental and theoretical researches on the properties of sound.

December 3.—MM. Gauthier de Claubry et Person requested that a sealed note which they had deposited, should be given to the Commissioners, who were to give an account of the Memoir on madder by MM. Robiquet and Collin.—M. Malbouche announced that he had successfully practised the method for the cure of stammering, invented by Mrs. Leigh of New York.—At his request two Commissioners named by the Academy were to attend the experiments.—At the request of the Academy of Inscriptions, two members of the Academy of Sciences were added to the Commission named by them, for examining the Egyptian cubits lately discovered.—M. Duméril, in the name of a commission, reported respecting M. Chabrier's Memoir on the progressive motion of man and animals.—M. Gay-Lussac made a verbal report upon a pamphlet by M. Burridge, On the improvement of civil architecture.—M. Biot read a memoir On the figure of the earth.

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## OBITUARY:—SIR J. E. SMITH.

On Monday, the 17th of March, died at his house in Surrey Street, Norwich, his native city, aged 68, Sir James Edward Smith, M.D. F.R.S. Member of the Academies of Stockholm, Upsal, Turin, Lisbon, Philadelphia, New York, &c. &c. the Imperial Acad. Naturæ Curiosorum, and the Royal Academy of Sciences at Paris, Hon. Mem. of the Horticultural Society; and President of the Linnæan Society, which office he had held from the first establishment of the Society in 1788.

Of this eminent Naturalist and most excellent and amiable man, as time does not permit us in our present Number to give such a notice as is due to his station and his merits, we must fulfill that duty at a future time. We shall now only add, that he had laboured nearly

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up to the day of his decease (though often impeded by sickness), with unabated zeal and success in the advancement of his favourite science; the fourth volume of his "English Flora" having been published but a few days before his death. The former volumes of this masterly work have been noticed by us on their publication *. At the close of the volume which has just appeared, are the following remarks, which will now be read with melancholy interest by the friends and admirers of the much lamented author.

"Several circumstances have caused a long delay in the publication of the present volume, which, if their recurrence should not be prevented, may render the completion of the work, according to its original plan, very precarious. In the mean while, the number of volumes originally proposed is now finished, and the first twenty-three Classes are completed, as well as the first Order of the twenty-fourth, *Cryptogamia Filices,* the only one that required more study and emendation than it has hitherto received.

"Of the remaining Orders, the *Musci* have been detailed in the Latin *Flora Britannica* and *Compendium* of the author, as well as in his *English Botany;* and by other well-known writers, in two editions of the *Muscologia Britannica,* and the *Muscologiæ Hibernicæ Spicilegium.* The monograph of Dr. Hooker on British *Jungermanniæ,* which, with their allies, constitute the next Order to the *Musci,* diffuses a new light over the whole of that Order. The works of Mr. Dawson Turner on *Fuci,* and of Mr. Dillwyn on *Confervæ,* have gone far to exhaust the species of those tribes; an application of scientific principles to the settlement of their genera being all that is wanting. The *Lichen* family, under the controul of the great Acharius, assumes the dignity of an entire and well-arranged Order. The *Fungi,* better discriminated by Withering than by most popular writers, and well explained by the figures of the excellent and lamented Sowerby, are, in their minutest details, exquisitely illustrated by the *Cryptogamic Flora* of the ingenious Dr. Greville, and the accurate publications of Mr. Purton. These, marshalled by the aid of the learned Persoon and others, might possibly have proved less obscure than heretofore. This tribe indeed leads the botanist to the end of his clue, and leaves him in palpable darkness, where even Dillenius was bewildered.

"All these subjects, if not yet brought into perfect daylight, might well, by the help of those brilliant northern lights, Acharius, Fries, and Agardh, have been made more accessible to the student, and more instructive to systematic botanists, by one long accustomed to their contemplation in the wild scenes of Nature, and not unfurnished with remarks of his own. If our bodily powers could keep pace with our mental acquirements, the student of half a century would not shrink from the delightful task of being still a teacher; nor does he resign the hope of affording some future assistance to his fellow-labourers, though for the present, "a change of study," to use the expression of a great French writer, may be requisite "by way of relaxation and epose."

* See Phil. Mag. vol. lxvii. p. 60.

L. *In-*

## L. *Intelligence and Miscellaneous Articles.*

### ANALYSIS OF ALCOHOL, ÆTHER, ETC.

MESSRS. Dumas and Boullay have recently analysed alcohol, and several æthereal preparations. The alcohol used was repeatedly rectified from chloride of calcium; its sp. gr. was 0·7925 at 64° Fahr.; it boiled at 169° at a medium pressure.

The analysis of alcohol agrees, the authors remark, with that deduced by M. Gay-Lussac, from the density of its vapour. The following are the results of the experiments compared with those of calculation.

| | Experiment. | Calculation. |
|---|---|---|
| Carbon | 52·37 | 52·28 |
| Hydrogen | 13·31 | 13·02 |
| Oxygen | 34·61 | 34·70 |
| | 100·29 | 100·00 |

Sulphuric æther was obtained perfectly free from alcohol by rectification from chloride of calcium, until the operation produced no alteration in its properties. Its sp. gr. was 0·713 at 68° Fahr.; it boiled at 93° Fahr. at a medium pressure. The experimental results compared with those of calculation were as follow:

| | Experiment. | Calculation. |
|---|---|---|
| Carbon | 65·05 | 64·96 |
| Hydrogen | 13·85 | 13·47 |
| Oxygen | 21·24 | 21·57 |
| | 100·14 | 100·00 |

There is a slight excess of hydrogen, but the authors consider sulphuric æther as consisting of a volume of olefiant gas and half a volume of the vapour of water.

The oil of wine examined was separated from pure æther by distillation: as it is not volatilized at a very low temperature, it remains almost entirely in the retort; a part is then distilled and afterwards rectified from chloride of calcium and a little potash. Its sp. gr. is 0·9174. It was found to consist of

| | By Experiment. | | By Calculation. |
|---|---|---|---|
| Carbon | 88·36 | 88·80 | 88·94 |
| Hydrogen | 11·64 | 11·20 | 11·06 |
| | 100·00 | 100·00 | 100·00 |

Oil of wine is therefore a carburet of hydrogen, differing in the proportion of its constituents from all the previously known carburets. The calculated result is obtained by supposing it to be formed of four volumes of the vapour of carbon with three volumes of hydrogen gas. The authors observe that this composition necessarily results from the kind of reaction which gives rise to the formation of oil of wine, as elucidated by other experiments.

Sulphovinic

Sulphovinic acid was analysed in combination with barytes. The results are thus stated:

| | | |
|---|---|---|
| Sulphate of barytes.... | 53·30 | 54·00 |
| Sulphurous acid ....... | 14·65 | 14·85 |
| Carbon .............. | 11·32 | 10·33 |
| Hydrogen............ | 1·46 | 1·39 |
| Water .............. | 19·31 | 20· |
| | 100·04 | 100·57 |

The composition of the oily matter, brought to 100, would give

| | |
|---|---|
| Carbon............ | 88·37 |
| Hydrogen ......... | 11·63 |
| | 100·00 |

It is therefore oil of wine. This being admitted, the sulphovinate of barytes is represented by an atom of hyposulphate, two atoms of oil of wine, and five of water; or

| | By Experiment. | By Calculation. |
|---|---|---|
| Hyposulphate of barytes.... | 68·40 | 67·37 |
| Oil of wine .............. | 12·25 | 12·27 |
| Water .................. | 19·65 | 20·36 |
| | 100·30 | 100·00 |

The authors then show that similar results are obtained by analysing the sulphovinates of copper and lead.

The authors consider the theory of ætherification as rendered extremely simple by these analyses: the acid and the alcohol are divided into two parts, one of which produces oil of wine and hyposulphuric acid, and occasions the formation of a certain quantity of water; and the other portion of acid and alcohol furnishes by their action weakened acid and æther. It will be seen, according to this process (say the authors), what would be the nature of the action of peroxide of manganese or chromic acid in the formation of æther. They would lose a portion of their oxygen to form water and oil of wine, thus presenting the formation of hyposulphuric acid. M. Gay-Lussac has in fact stated that this acid is not produced in these kinds of reaction. The formation of hyposulphuric acid is not then necessarily connected with that of æther; it is also difficult to believe that the production of oil of wine is necessary to that of æther, the reactions producing them seeming so independent. If it be admitted, as stated by M. Desfosses, that fluoboric acid gives æther without oil of wine, it seems at least that the necessity is not general; and the authors conclude that the two phænomena have nothing in common.—*Annales de Chimie et de Physique*, Nov. 1827.

I shall probably offer some observations upon the paper from which the above is extracted in the next Number of the Annals, more especially with respect to the notice taken of the experiments of Mr. Faraday and Mr. Hennell. In the mean time I cannot help regretting the great use (if that term can be properly applied) which MM. Dumas and Boullay have made of symbols to represent chemical

mical compounds. Can they, or can any body, believe that the following is a very intelligible mode of describing bisulphovinate of lead?

$$\dot{P}b + 4\dddot{S} + 8H^2C^2 + 3\dot{H}H = \dot{P}b + 2\overset{..}{\dddot{S}}{}^2 + 4H^3C^4 + 5\dot{H}H.$$

R. P.

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LIST OF NEW PATENTS.

To W. Nairn, of Dane-street, Edinburgh, for his improved method of propelling vessels through, or on the water by the aid of steam, or other mechanical force.—Dated the 5th of February 1828.—6 months allowed to enrol specification.

To C. Hitch, of Ware, Hertfordshire, for his improved wall for building.—21st of February.—2 months.

To G. Dickinson, of Buckland Mill, near Dover, for his improvements in making paper by machinery.—21st of February.—4 mon.

To Angelo Benedetto Ventura, of Cirencester-place, Fitzroy-square, for his improvements on the harp, lute, and Spanish guitar.—21st of February.—6 months.

To T. Otway, of Walsall, for an expedient for stopping horses when running away.—21st of February.—2 months.

To D. Bentley, of Pendleton, Lancashire, for an improved method and machinery for bleaching and finishing linen or cotton yarn and goods.—21st of February.—6 months.

To W. Brunton, of Leadenhall-street, for improvements on furnaces for the calcination, sublimation, or evaporation of ores, metals, &c.—21st of February.—2 months.

To J. Levers, of Nottingham, for improvements in the manufacture of bobbin net-lace.—3rd of March.—4 months.

To W. Pownall, of Manchester, for improvements in making healds for weaving.—6th of March.—4 months.

To B. H. Brook, of Huddersfield, for improvements in the construction and setting of ovens or retorts for carbonizing coal for gas-works.—6th of March.—6 months.

To Lieut. W. Roger, of Norfolk-street, Strand, for improvements on anchors.—13th of March.—6 months.

To R. G. Jones, of Brewer-street, Golden-square, for a method, communicated from abroad, of ornamenting china and other compositions, which he denominates Lethophanic, translucid, or opaque china.—13th of March.—2 months.

To G. Scholefield, of Leeds, mechanic, for improvements in or additions to looms for weaving woollen, linen, &c.—13th of March.—6 months.

To N. Gough, of Salford, Manchester, for an improved method of propelling carriages or vessels by steam, &c.—20th of March.—6 months.

To S. Clegg, of Chapel Walks, Liverpool, for improvements in steam-engines, and steam-boilers and generators.—20th of March.—6 months.

*Results of a Meteorological Journal for the Year* 1827, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

*By* WILLIAM BURNEY, *LL.D.*

Latitude 50° 47′ 20″ North—Longitude 1° 7′ West of Greenwich. In time 4′ 28″.

| 1827. Months. | Barometer. Max. | Min. | Media. | Range. | No. of Changes. | Spaces described. | Greatest Variation in 24 hours. | Media at 8 A.M. | Media at 2 P.M. | Media at 8 P.M. | Self-registering Thermometer. Max. | Min. | Media. | Mean Range. | Gt. Var. in 24 hours | Media at 2 P.M. | Media at 8 A.M. | Media at 8 P.M. | Mean Temp. of Spring Water. | De Luc's Hygrometer. Max. | Min. | Mean Range of the Index. | Media at 2 P.M. | Media at 8 A.M. | Media at 8 P.M. | Media at 8,2,&8 o'cl. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | In. | In. | In. | In. | | In. | In. | In. | In. | In. | ° | ° | ° | ° | | ° | ° | ° | ° | ° | ° | ° | ° | ° | ° | ° |
| Jan. | 30·26 | 29·29 | 29·828 | 0·97 | 21 | 7·52 | 0·76 | 29·828 | 29·815 | 29·832 | 54 | 20 | 38·90 | 34 | 18 | 42·16 | 36·68 | 38·68 | 51·62 | 100 | 48 | 52 | 67·7 | 74·1 | 74·0 | 71·9 |
| Feb. | 30·44 | 29·49 | 29·983 | 0·95 | 16 | 5·07 | 0·48 | 29·977 | 29·978 | 29·988 | 56 | 14 | 36·62 | 42 | 21 | 41·11 | 33·86 | 36·39 | 50·12 | 98 | 45 | 53 | 58·6 | 66·3 | 66·1 | 63·7 |
| Mar. | 30·30 | 28·79 | 29·716 | 1·51 | 31 | 11·44 | 0·94 | 29·701 | 29·721 | 29·726 | 60 | 32 | 47·06 | 28 | 24 | 52·13 | 45·77 | 45·32 | 49·54 | 96 | 44 | 52 | 60·7 | 69·6 | 71·0 | 67·1 |
| April | 30·28 | 29·44 | 29·960 | 0·84 | 14 | 4·04 | 0·33 | 29·959 | 29·964 | 29·959 | 70 | 35 | 51·07 | 35 | 22 | 57·17 | 50·43 | 48·50 | 49·78 | 88 | 42 | 46 | 55·5 | 63·6 | 68·8 | 62·6 |
| May | 30·15 | 29·09 | 29·758 | 1·06 | 19 | 5·43 | 0·57 | 29·762 | 29·759 | 29·760 | 72 | 40 | 56·31 | 32 | 21 | 61·58 | 56·74 | 53·13 | 50·22 | 95 | 33 | 62 | 54·1 | 60·1 | 70·9 | 61·7 |
| June | 30·33 | 29·64 | 29·958 | 0·69 | 26 | 4·08 | 0·32 | 29·959 | 29·958 | 29·956 | 75 | 45 | 60·75 | 30 | 24 | 67·27 | 60·87 | 58·30 | 51·35 | 82 | 36 | 46 | 51·4 | 58·0 | 61·3 | 56·9 |
| July | 30·46 | 29·76 | 30·103 | 0·70 | 28 | 4·32 | 0·40 | 30·108 | 30·112 | 30·099 | 80 | 53 | 65·03 | 27 | 25 | 71·84 | 65·06 | 62·97 | 52·87 | 94 | 37 | 57 | 50·7 | 58·8 | 65·6 | 58·4 |
| Aug. | 30·41 | 29·38 | 30·009 | 1·03 | 21 | 4·62 | 0·36 | 30·008 | 30·014 | 30·009 | 77 | 48 | 62·58 | 29 | 23 | 68·52 | 61·42 | 60·74 | 54·35 | 84 | 38 | 46 | 49·6 | 57·4 | 61·5 | 56·2 |
| Sept. | 30·34 | 29·50 | 29·986 | 0·84 | 30 | 3·70 | 0·46 | 29·982 | 29·986 | 29·990 | 73 | 49 | 60·77 | 24 | 24 | 66·27 | 59·56 | 59·53 | 55·25 | 100 | 40 | 60 | 54·6 | 64·6 | 67·5 | 62·2 |
| Oct. | 30·36 | 29·07 | 29·731 | 1·29 | 15 | 7·09 | 0·58 | 29·727 | 29·727 | 29·735 | 68 | 35 | 55·56 | 33 | 21 | 59·35 | 54·23 | 54·26 | 55·71 | 100 | 49 | 51 | 66·2 | 75·8 | 81·0 | 74·3 |
| Nov. | 30·40 | 29·28 | 29·964 | 1·12 | 19 | 6·79 | 0·73 | 29·960 | 29·961 | 29·969 | 62 | 27 | 48·42 | 35 | 19 | 52·30 | 46·83 | 48·43 | 54·82 | 100 | 51 | 49 | 70·0 | 79·2 | 81·2 | 76·8 |
| Dec. | 30·58 | 29·07 | 29·810 | 1·51 | 26 | 8·90 | 0·56 | 29·813 | 29·811 | 29·810 | 57 | 31 | 47·82 | 26 | 17 | 50·81 | 45·74 | 48·00 | 53·87 | 100 | 55 | 45 | 78·2 | 83·0 | 85·5 | 82·2 |
| Aver. | 30·58 | 28·79 | 29·900 | 12·51 | 266 | 73·0 | 0·94 | 29·899 | 29·900 | 29·903 | 80 | 14 | 52·57 | 31·25 | 25 | 57·54 | 51·43 | 51·19 | 52·46 | 100 | 33 | 51·6 | 59·8 | 67·5 | 71·2 | 66·2 |

TABLE (continued.)

| 1827. | Scale of the Winds. | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | North. | North-East. | East. | South-East. | South. | South-West. | West. | North-West. | Total Number of Days. |
| January .. | 5 | 6½ | ½ | 1 | 1 | 3½ | 7½ | 6 | 31 |
| February | 2½ | 13½ | 4 | 1 | 1 | 3½ | ½ | 2 | 28 |
| March ... | 2 | 2 | ... | 1 | 3 | 10 | 6 | 7 | 31 |
| April .... | 4 | 5 | 3 | 5 | 4 | 1 | 4 | 4 | 30 |
| May .. .. | 1 | 3½ | 3 | 2½ | 3 | 10 | 4½ | 3½ | 31 |
| June .. .. | 1½ | 5 | ½ | 1½ | ½ | 13½ | 1 | 6½ | 30 |
| July .. .. | 2½ | 2 | 2 | 2 | 3 | 11 | 6 | 2½ | 31 |
| August .. | 8 | 7½ | 1 | 1 | 3 | 4½ | 2 | 4 | 31 |
| September | 2 | 8 | 1 | 3 | 4 | 6 | 2 | 4 | 30 |
| October .. | 2 | 4½ | 5½ | 5 | 4 | 6 | 2 | 2 | 31 |
| November | 6 | 3½ | 3 | 2 | 4 | 1 | 2½ | 8 | 30 |
| December | 1½ | ½ | 1 | 1 | 4 | 10½ | 8 | 4½ | 31 |
| Results for 1827. | 38 | 61½ | 24½ | 26 | 34½ | 80½ | 46 | 54 | 365 |

| 1827. | Modifications of Clouds. | | | | | | | Weather. | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Cirrus. | Cirrocumulus. | Cirrostratus. | Stratus. | Cumulus. | Cumulostratus. | Nimbus. | A clear Sky. | Fair, with Clouds. | An overcast Sky. | Foggy. | Rain, &c. | Total Number of Days. |
| January .. | 18 | 9 | 30 | ... | 15 | 15 | 19 | 4 | 11 | 11 | ½ | 4½ | 31 |
| February | 8 | 6 | 24 | ... | 14 | 4 | 8 | 4½ | 11½ | 10 | ... | 2 | 28 |
| March ... | 21 | 10 | 31 | ... | 19 | 26 | 22 | 3 | 12½ | 10 | ... | 5½ | 31 |
| April .... | 13 | 9 | 28 | 4 | 17 | 18 | 14 | 4 | 13 | 8½ | ½ | 4 | 30 |
| May .. .. | 20 | 9 | 28 | ... | 22 | 21 | 18 | 3 | 14½ | 7 | ½ | 6 | 31 |
| June .. .. | 17 | 11 | 25 | ... | 25 | 23 | 15 | 4 | 14 | 7 | ... | 5 | 30 |
| July .. .. | 20 | 16 | 23 | 1 | 28 | 22 | 10 | 6 | 15 | 6 | ½ | 3½ | 31 |
| August .. | 21 | 12 | 29 | ... | 25 | 25 | 13 | 5½ | 15½ | 6 | ... | 4 | 31 |
| September | 18 | 10 | 28 | 3 | 24 | 26 | 18 | 3½ | 14 | 6 | ½ | 6 | 30 |
| October .. | 22 | 12 | 31 | 1 | 22 | 19 | 18 | 3 | 10½ | 10 | 2 | 5½ | 31 |
| November | 15 | 10 | 27 | 1 | 15 | 19 | 12 | 1½ | 9 | 14 | 1½ | 4 | 30 |
| December | 15 | 8 | 28 | ... | 14 | 12 | 23 | 3 | 9 | 7½ | 2 | 9½ | 31 |
| Results for 1827. | 208 | 122 | 332 | 10 | 240 | 230 | 190 | 45 | 149½ | 103 | 8 | 59½ | 365 |

| 1827. | Atmospheric Phænomena. | | | | | | | | | Evaporation in Inches, &c. | Rain in Inches, &c. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Anthelia. | Parhelia. | Paraselenæ. | Solar Halos. | Lunar Halos. | Rainbows. | Meteors. | Lightning. | Thunder. | | |
| January .. | ... | ... | 1 | ... | 1 | ... | 3 | ... | ... | 0·70 | 1·000 |
| February | ... | ... | ... | 4 | ... | ... | 4 | ... | ... | 0·90 | 0·820 |
| March ... | ... | 6 | ... | 9 | 2 | 1 | 2 | 1 | ... | 1·95 | 3·145 |
| April .... | ... | 2 | ... | 3 | 3 | ... | 4 | ... | ... | 2·35 | 1·910 |
| May .. .. | ... | 5 | ... | 4 | ... | 1 | 3 | 3 | 3 | 3·05 | 2·125 |
| June .. .. | ... | 2 | ... | 4 | ... | ... | 3 | 1 | ... | 3·00 | 1·660 |
| July .. .. | ... | ... | ... | 2 | 1 | ... | 6 | 1 | 1 | 3·70 | 1·115 |
| August .. | ... | ... | ... | ... | ... | ... | 14 | 1 | 2 | 3·85 | 2·060 |
| September | ... | 1 | ... | 1 | 1 | 4 | 20 | 2 | 1 | 2·85 | 3·835 |
| October .. | ... | 1 | ... | 3 | ... | 10 | 1 | 2 | 2 | 1·35 | 4·835 |
| November | ... | ... | 2 | ... | 3 | ... | 2 | 1 | ... | 0·70 | 1·835 |
| December | ... | 1 | ... | ... | 1 | 1 | 4 | 1 | 1 | 0·60 | 5·625 |
| Results for 1827. | ... | 18 | 3 | 30 | 12 | 17 | 66 | 13 | 10 | 25·00 | 29·965 |

## ANNUAL RESULTS FOR 1827.

| *Barometer.* | Inches. |
|---|---|
| Greatest pressure of the atmosphere, Dec. 28th. Wind N.E. | 30·580 |
| Least ditto - ditto March 4th. Wind S.W. | 28·790 |
| Range of the quicksilver .......................... | 1·790 |
| Annual mean pressure of the atmosphere .............. | 29·900 |
| Mean pressure for 190 days with the moon in North decl. | 29·872 |
| ——— for 164 days with the moon in South decl. | 29·927 |
| Annual mean pressure at 8 o'clock A.M. ............... | 29·899 |
| ——— at 2 o'clock P.M. ............... | 29·900 |
| ——— at 8 o'clock P.M. ............... | 29·903 |
| Greatest range of the quicksilver in March and December | 1·510 |
| Least range of ditto in June ............... | 0·690 |
| Greatest annual variation in 24 hours in March ......... | 0·940 |
| Least of the greatest variations in 24 hours in June ...... | 0·320 |
| Aggregate of the spaces described by the rising and falling of the quicksilver ............................ | 73·000 |
| Number of changes............................ | 266· |

| *Self-registering Day and Night Thermometer.* | Degrees. |
|---|---|
| Greatest thermometrical heat, July 7th and 8th, Wind N. | 80 |
| ——— cold, February 16th, Wind E..... | 14 |
| Range of the thermometer between the extremes ........ | 66 |
| Annual mean temperature of the external air ........... | 52·57 |
| ——— of do. at 8 A.M. ............ | 51·43 |
| ——— of do. at 8 P.M. ............ | 51·19 |
| ——— of do. at 2 P.M. ............ | 57·54 |
| Greatest range in February ......................... | 42·00 |
| Least of the monthly ranges in September .............. | 24·00 |
| Annual mean range ............................... | 31·25 |
| Greatest monthly variation in 24 hours in July........... | 25·00 |
| Least of the greatest variations in 24 hours in December .. | 17·00 |
| Annual mean temperature of spring water at 8 o'clock A.M. | 52·46 |

| De Luc's *Whalebone Hygrometer.* | Degrees. |
|---|---|
| Greatest humidity of the atmosphere, several times in November and December ...................... | 100 |
| Greatest dryness of ditto, May 13th .................. | 33 |
| Range of the index between the extremes.............. | 67 |
| Annual mean state of the hygrometer at 8 o'clock A.M... | 67·5 |
| ——— at 8 o'clock P.M... | 71·2 |
| ——— at 2 o'clock P.M. .. | 59·8 |
| ——— at 8, 2, and 8 o'clock | 66·2 |
| Greatest mean monthly humidity of the atmosphere in Dec. | 82·2 |
| ——— dryness of ditto in August ...... | 56·2 |

*Position*

| *Position of the Winds.* | Days. |
|---|---|
| From North to North-east | 38 |
| —— North-east to East | 61½ |
| —— East to South-east | 24½ |
| —— South-east to South | 26 |
| —— South to South-west | 34½ |
| —— South-west to West | 80½ |
| —— West to North-west | 46 |
| North-west to North | 54 |
| | ——365 |

*Clouds, agreeably to the Nomenclature, or the Number of Days on which each Modification has appeared.*

| | Days. | | Days. |
|---|---|---|---|
| Cirrus | 208 | Cumulus | 240 |
| Cirrocumulus | 122 | Cumulostratus | 230 |
| Cirrostratus | 332 | Nimbus | 190 |
| Stratus | 10 | | |

| *General State of the Weather.* | Days. |
|---|---|
| A transparent atmosphere without clouds | 45 |
| Fair, with various modifications of clouds | 149½ |
| An overcast sky without rain | 103 |
| Foggy | 8 |
| Rain, hail, sleet and snow | 59½ |
| | ——365 |

| *Atmospheric Phænomena.* | No. |
|---|---|
| Parhelia, or mock-suns on the sides of the true sun | 18 |
| Paraselenæ, or mock-moons | 3 |
| Solar halos | 30 |
| Lunar halos | 12 |
| Rainbows | 17 |
| Meteors of various sizes | 66 |
| Lightning, days on which it happened | 13 |
| Thunder, ditto ditto | 10 |

| *Evaporation.* | Inches. |
|---|---|
| Greatest monthly quantity in August | 3·85 |
| Least monthly quantity in December | 0·60 |
| Total amount for the year | 25·00 |

| *Rain.* | |
|---|---|
| Greatest monthly depth in December | 5·625 |
| Least monthly depth in February | 0·820 |
| Total amount near the ground for the year | 29·965 |
| Total amount near 23 feet high for ditto | 27·635 |

The instruments are the same, and were placed in the same situations as described in the February Number of this work, in the Results of the Meteorological Journal for Hampshire, for 1826.

Barometrical Pressure.—The mean pressure of the atmosphere

sphere on the mercurial column this year, is $\frac{81}{1000}$ of an inch lower than that of last year, and $\frac{2}{250}$ of an inch higher than the mean of the last twelve years. The yearly maximum pressure is not high, nor the minimum comparatively low.

The aggregate of the spaces described by the alternate rising and falling of the quicksilver, is 14·59 inches greater than in 1826.

The number of changes in the Barometer this year, viz. 266, coincides with that of 1826 and 1816, and is only four short of the annual average for the last twelve years. Although the aggregate of the spaces differs very much in different years, yet the yearly number of changes is pretty uniform; and the average time of each change during the last twelve years, is $32\frac{1}{2}$ hours nearly. The annual uniformity of these changes, as influenced by the varying weight of the atmosphere on the column of quicksilver, might have first suggested to meteorologists the idea of the existence of atmospherical tides, as chiefly effected by the moon's attractive force on the spheroidal mass of the earth's atmosphere. But these changes not happening regularly, compared with the regularity of the tides of the ocean in connection with the moon's motion, as sometimes they are quick and at other times very slow, at least near the earth's surface, a train of imperceptible effects, besides the moon's supposed attraction, should therefore be taken into consideration to account fairly for the atmospherical tides, as the diurnal action of the sun's calorific rays upon the atmosphere; the ascending heat from the earth in the spring and summer months; the rarefaction of the atmosphere, particularly under the sun's vertical rays and about the equator; the consequent perturbations of the air, and dispersion of dry and moist winds, according to their position, to every known place on the earth; and the influence of the seasons upon the constitution of the atmosphere in every climate without the tropics of Cancer and Capricorn.

Temperature.—In the temperature of the atmosphere we generally feel more interest than in the pressure, from the effects it often produces in us when too much exposed to the extremes of either cold or heat. The heat we sustain beyond the temperature of the ground, is the effect of the solar rays on the earth and surrounding atmosphere, while the earth moves in that part of its orbit which coincides with the northern signs of the ecliptic; for while it moves through the space occupied by the southern signs, the mean temperature of the atmosphere in this latitude is almost invariably below the mean temperature of the ground, and especially during the time it moves through *Capricornus*, *Aquarius*, and *Pisces*. However, in respect to the temperature connected with the seasons, it is sometimes reversed for three or four weeks in the spring or autumn. A deviation of this sort occurred last February, the mean temperature of that month being $2\frac{1}{4}$ degrees below the mean of January. Two other deviations also occurred in 1826, as explained in our Meteorological Summary for Hampshire that year.

The mean temperature of the external air this year is about a degree below that of last; but it is nearly three quarters of a degree higher than the mean of the preceding twelve years.

The

The mean temperature of spring water at 8 A.M. is only $\frac{1}{100}$ of a degree less than the mean temperature of the external air, which seems to prove that the additional thermometrical heat near the ground in the summer, is balanced by the decrease of temperature in the opposite season, when the sun's diurnal arc with us is much shorter, and his rays more oblique.

The difference between the annual mean temperature of the air as taken daily at 8 A.M. and 8 P.M. is only ·24, or a quarter of a degree.

WIND.—The scale of the prevailing winds this year deviates very little from that of last. High winds prevailed in January, February, March, April, October, and December; but in the other months the winds were comparatively light. The number of strong gales, or the days on which they have prevailed this year, is as in the following scale:[1]

| N. | N.E. | E. | S.E. | S. | S.W. | W. | N.W. | Gales. |
|---|---|---|---|---|---|---|---|---|
| 10 | 11 | 3 | 3 | 8 | 39 | 8 | 7 | 89 |

Here the number from the S.W. is remarkable, as usual. The weather in January and February was cold; and in March, September, October, and December it was wet, but rather dry in the other eight months.

The amount of evaporation this year is considerably under the depth of rain, from the atmosphere near the ground having been more than usually humid in the summer months.

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## METEOROLOGICAL OBSERVATIONS FOR FEBRUARY 1828.

### *Gosport.—Numerical Results for the Month.*

Barom. Max. 30·44 Feb. 3. Wind N.W.—Min. 28·90 Feb. 21. Wind S.E.
Range of the index 1·54.
Mean barometrical pressure for the month .......................... 29·763
Spaces described by the rising and falling of the mercury........... 6·250
Greatest variation in 24 hours 0·570.—Number of changes 17.
Therm. Max. 61° Feb. 26. Wind SW.—Min. 28° Feb. 12. Wind N.
Range 33°.—Mean temp. of exter. air 45°·74. For 30 days with ☉ in ♒ 45·32
Max. var. in 24 hours 17°·00—Mean temp. of spring water at 8 A.M. 51°·56

### De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the afternoon of the 25th........... 100°
Greatest dryness of the air in the afternoons of the 3rd & 15th ... 59
Range of the index........................................................ 41
Mean at 2 P.M. 71°·6—Mean at 8 A.M. 82°·1—Mean at 8 P.M. 81·8
—— of three observations each day at 8, 2, and 8 o'clock........ 78·5
Evaporation for the month 0·75 inch.
Rain near ground 1·515 inch.—Rain 23 feet high 1·395 inch.
Prevailing Wind S.E.

Sum-

## *Summary of the Weather.*

A clear sky, 2; fine, with various modifications of clouds, 9½; an overcast sky without rain, 12½; foggy ½; rain, 4½.—Total 29 days.

## *Clouds.*

| Cirrus. | Cirrocumulus. | Cirrostratus. | Stratus. | Cumulus. | Cumulostr. | Nimbus |
|---|---|---|---|---|---|---|
| 14 | 8 | 29 | 1 | 10 | 17 | 20 |

## *Scale of the prevailing Winds.*

| N. | N.E. | E. | S.E. | S. | S.W. | W. | N.W. | Days. |
|---|---|---|---|---|---|---|---|---|
| 5 | 2 | 1 | 7 | 2 | 6 | 4 | 2 | 29 |

*General Observations.*—This month has been calm and generally mild, and although it has rained more or less on seventeen different days, yet the amount at the ground is very little more than one and a half inch in depth; therefore it has generally been light.

From the 10th to the 18th instant, the weather was rather cold; and on the 11th and 12th, sleet and granulous snow fell. In the morning of the 14th it snowed three hours, and the depth was about 2½ inches, which dissolved in this neighbourhood in two hours with a temperature from 37 to 41 degrees in the shade. In the evening of that day about 20 minutes before 7 o'clock, a very large meteor, in the form of a cone on its side, passed over towards the S.E. It appeared very low, perhaps not above 200 feet from the ground: its light was glaring for five or six seconds of time, and nearly the colour of the moon's reflected light, and its train sparkling. It passed through an apparent space of about 60 degrees before it disappeared behind some houses, and was seen by many persons in the town and vicinity.

In the evening of the 15th, a short time before Venus set, a mild light reflected in the atmosphere from that planet eastward, was distinctly traced to the first star of *Aries*, marked $\gamma$, a distance at that time of 34 degrees.

The *maximum* temperature of the external air occurred in the nights of the 3rd, 13th, and 24th. The *maximum* temperature of the present month is remarkable, it being four degrees higher than in any February for the last twelve years. It occurred on the 26th, and appeared to have been influenced after light rain, by two warm winds crossing at right angles from the S.E. and S.W., and a dense body of *cumulostratus* clouds with some attenuated parts, sufficient to admit the calorific rays to the earth's surface, which acted powerfully on the exterior thermometer. The mean temperature of the month is very high for the season. Much has been said in conversation on the mildness of the last winter, viz. December, January, and February, which in this latitude are understood and very generally taken as the three winter months; and it certainly has been the mildest during the last thirteen years. The mean temperature of these months for the last thirteen years, is 41·38 degrees; and for the last three months 46·16 degrees; therefore, the mean of the last winter is 4¾ degrees higher than the mean of that period. The next mildest winter occurred in the years 1821–2, the mean temperature of which was 45·56 degrees, that is *three-fifths* of a degree lower than the mean of the last winter. The spring has commenced, and the trees are progressively breaking into buds.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one lunar and two solar halos, two meteors, one rainbow, and two gales of wind, one from the South-east, the other from South-west.

REMARKS.

REMARKS.

*London.*—Feb. 1. Fine. 2. Very fine. 3. Fine. 4. Slight fog: cloudy. 5. Cloudy. 6. Slight rain: cloudy. 7. Cloudy: rain at night. 8. Fine. 9. Hazy, with showers. 10. Clear and cold. 11. Snowy: cloudy. 12. Cloudy. 13. Fine. 14. Cloudy, with sleet. 15. Fine. 16, 17. Very fine. 18. Cloudy, with showers. 19—21. Fine. 22. Cloudy. 23. Showery. 24, 25. Cloudy. 26. Rain in morning: cloudy. 27. Fine. 28. Slight fog: fine. 29. Slight fog: cloudy.

*Boston.*—Feb. 1, 2. Fine. 3. Fine: rain A.M. 4. Cloudy: rain A.M. and P.M. 5. Cloudy. 6. Cloudy: rain early A.M. 7. Dense fog: rain early A.M. 8. Fine: rain at night. 9. Cloudy: rain at night. 10, 11. Cloudy. 12. Snow. 13. Cloudy. 14. Stormy: heavy fall of snow. 15—17. Cloudy. 18. Fine: rain P.M. 19. Cloudy. 20. Fine: rain P.M. 21, 22. Fine. 23. Cloudy: rain A.M. 24. Cloudy. 25. Cloudy: rain A.M. 26, 27. Cloudy. 28. Foggy. 29. Cloudy.

*Penzance.*—Feb. 1. Clear: rain at night. 2. Misty: clear. 3. Clear. 4. Rain. 5. Misty: rain. 6. Fair: misty. 7. Rain: clear. 8. Misty: clear. 9. Clear. 10. Fair: rain. 11. Hail-showers. 12. Fair: rain. 13. Clear: rain at night. 14. Fair: hail and rain. 15. Clear. 16. Clear: fair. 17. Fair: rain. 18. Showers. 19. Fair: showers. 20, 21, Fair: rain. 22. Fair: showers. 23. Clear: hail-showers. 24, 25. Rain. 26. Misty: fair. 27. Fair. 28. Clear. 29. Clear: fair.—Rain-gauge ground level.

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We give the following to supply the deficiencies in our last Number in the Meteorological Register for January.

| London. | | | | |
|---|---|---|---|---|
| | Barometer. | | Thermometer. | |
| | 9 A.M. | 10 P.M. | Max. | Min. |
| Jan. 20 | 30·13 | 30·18 | 53 | 43 |
| 21 | 30·13 | 30·11 | 50 | 45 |
| 22 | 30·00 | 30·13 | 46 | 40 |
| 23 | 30·18 | 30·24 | 52 | 44 |
| 24 | 30·23 | 30.23 | 52 | 48 |
| 25 | 30·20 | 30·01 | 54 | 42 |
| 26 | 30·15 | 30·23 | 51 | 47 |
| 27 | 30·24 | 30·31 | 53 | 37 |
| 28 | 30·31 | 30·25 | 47 | 42 |
| 29 | 30·08 | 30·06 | 50 | 40 |
| 30 | 29·98 | 29·90 | 47 | 40 |
| 31 | 29·90 | 29·86 | 49 | 43 |

We are enabled in the present Number to supply the place of Mr. HOWARD's observations, by extracts from the accurate Register kept by Mr. W. B. BOOTH, at the Garden of the Horticultural Society at Chiswick, near London; with which we trust we shall be favoured monthly.

*Meteoro-*

*Meteorological Observations made at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.*

| Days of Month, 1828. | Barometer. | | | | | | | Thermometer. | | | | | | | Wind. | | | | Evap. | Rain. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | London. | | Penzance. | | Gosport. | | Boston | London. | | Penzance. | | Gosport. | | Bost. | Lond. | Penz. | Gosp. | Bost. | Gosp. | Lond. | Penz. | Gosp. | Bost. |
| | Max. | Min. | Max. | Min. | Max. | Min. | 8½ A.M. | Max. | Min. | Max. | Min. | Max. | Min. | 8½ A.M. | | | | | | | | | |
| ○ Feb. 1 | 30·055 | 29·980 | 29·96 | 29·94 | 30·06 | 29·92 | 29·43 | 51 | 32 | 55 | 47 | 54 | 48 | 46 | SW. | W. | W. | calm | ... | ... | ... | ... | ... |
| 2 | 30·188 | 30·087 | 30·20 | 30·10 | 30·12 | 30·06 | 29·65 | 49 | 35 | 50 | 47 | 52 | 37 | 40 | W. | W. | SW. | calm | ... | ... | 0·200 | 0·040 | ... |
| 3 | 30·521 | 30·232 | 30·40 | 30·30 | 30·44 | 30·38 | 29·97 | 48 | 37 | 54 | 40 | 47 | 39 | 36·5 | W. | SW. | NW. | calm | 0·10 | ... | 0·030 | ·055 | 0·03 |
| 4 | 30·297 | 30·268 | 30·26 | 30·24 | 30·25 | 30·24 | 29·77 | 53 | 46 | 54 | 44 | 56 | 48 | 41 | W. | SW. | W. | calm | ... | ... | 0·300 | ... | ·07 |
| 5 | 30·214 | 30·190 | 30·16 | 30·10 | 30·18 | 30·13 | 29·63 | 52 | 45 | 53 | 48 | 55 | 48 | 48·5 | SW. | SW. | W. | W. | ... | ... | 0·180 | ·010 | ·03 |
| 6 | 30·143 | 30·017 | 30·00 | 29·96 | 30·03 | 29·98 | 29·50 | 54 | 40 | 53 | 47 | 58 | 47 | 47 | SW. | SW. | SW. | W. | ·05 | ·04 | ... | ·010 | ·03 |
| 7 | 29·979 | 29·898 | 29·88 | 29·88 | 29·90 | 29·84 | 29·47 | 51 | 40 | 50 | 48 | 55 | 42 | 48 | W. | W. | SE. | calm | ... | ·28 | 0·180 | ·100 | ·04 |
| ☾ 8 | 29·935 | 29·721 | 29·90 | 29·80 | 29·84 | 29·72 | 29·50 | 50 | 33 | 50 | 45 | 49 | 38 | 37 | NW. | W. | N. | NW. | ... | ... | ... | ... | ... |
| 9 | 29·738 | 29·726 | 29·78 | 29·70 | 29·69 | 29·58 | 29·36 | 41 | 34 | 47 | 36 | 45 | 39 | 39·5 | NW. | NE. | N. | calm | ·05 | ·06 | ... | ... | ·11 |
| 10 | 29·857 | 29·794 | 29·80 | 29·68 | 29·68 | 29·61 | 29·55 | 38 | 30 | 47 | 36 | 44 | 35 | 33 | N. | SE. | NE. | calm | ... | ... | 0·400 | ·480 | ·05 |
| 11 | 29·841 | 29·542 | 29·72 | 29·62 | 29·69 | 29·36 | 29·27 | 34 | 28 | 46 | 38 | 40 | 31 | 32·5 | NE. | NW. | N. | E. | ... | ·14 | 0·200 | ... | ... |
| 12 | 30·108 | 29·933 | 30·00 | 29·92 | 29·96 | 29·90 | 29·73 | 33 | 27 | 41 | 36 | 36 | 28 | 30·5 | NE. | E. | N. | N. | ... | ... | 0·060 | ... | ... |
| 13 | 30·185 | 30·125 | 30·00 | 29·96 | 30·02 | 29·96 | 29·86 | 34 | 29 | 48 | 29 | 40 | 37 | 32 | NE. | SE. | NE. | calm | ·15 | ... | ... | ... | ... |
| 14 | 29·800 | 29·538 | 29·66 | 29·44 | 29·60 | 29·45 | 29·53 | 34 | 29 | 46 | 40 | 44 | 34 | 32·5 | E. | N W. | S. | W. | ... | ·08 | 0·645 | ·360 | ... |
| ● 15 | 29·861 | 29·657 | 29·76 | 29·76 | 29·80 | 29·65 | 29·35 | 42 | 23 | 48 | 36 | 45 | 32 | 36 | NW. | W. | NW. | calm | ... | ... | 0·340 | ... | ... |
| 16 | 29·932 | 29·875 | 29·76 | 29·66 | 29·81 | 29·78 | 29·56 | 44 | 25 | 48 | 35 | 48 | 40 | 32 | W. | SE. | SE. | NW. | ... | ... | ... | ... | ... |
| 17 | 29·782 | 29·618 | 29·50 | 29·26 | 29·64 | 29·50 | 29·45 | 42 | 27 | 46 | 42 | 46 | 39 | 34 | SE. | SE. | SE. | calm | ·10 | ... | 0·080 | ·015 | ... |
| 18 | 29·433 | 29·400 | 29·26 | 29·26 | 29·36 | 29·26 | 29·20 | 46 | 31 | 48 | 36 | 50 | 38 | 32 | S. | W. | S. | calm | ... | ·18 | 0·025 | ·020 | ... |
| 19 | 29·414 | 29·362 | 29·26 | 29·20 | 29·34 | 29·29 | 28·96 | 52 | 32 | 49 | 37 | 52 | 41 | 35·5 | SE. | SW. | SW. | NW. | ... | ... | 0·025 | ·020 | ·46 |
| 20 | 29·372 | 29·228 | 29·14 | 28·80 | 29·26 | 29·09 | 28·82 | 55 | 40 | 50 | 38 | 51 | 44 | 41·5 | SE. | S. | SE. | calm | ·10 | ·02 | 0·265 | ·090 | ... |
| 21 | 29·115 | 29·048 | 28·76 | 28·74 | 28·96 | 28·90 | 28·82 | 52 | 38 | 50 | 45 | 52 | 43 | 41 | E. | S. | SE. | calm | ... | ·03 | 0·455 | ·150 | ·14 |
| ☽ 22 | 29·175 | 29·070 | 28·90 | 28·86 | 29·03 | 28·90 | 28·75 | 51 | 40 | 48 | 41 | 53 | 43 | 42·5 | E. | NW. | SE. | E. | ... | ... | 0·260 | ·015 | ... |
| 23 | 29·561 | 29·371 | 29·40 | 29·21 | 29·50 | 29·24 | 29·05 | 42 | 38 | 48 | 42 | 49 | 38 | 39·5 | N. | NE. | N. | calm | ·05 | ·04 | 0·050 | ·020 | ·22 |
| 24 | 29·858 | 29·838 | 29·70 | 29·70 | 29·81 | 29·73 | 29·40 | 49 | 45 | 52 | 36 | 53 | 48 | 38·5 | NW. | SW. | W. | calm | ... | ·02 | 0·070 | ·030 | ... |
| 25 | 29·982 | 29·883 | 29·76 | 29·70 | 29·89 | 29·83 | 29·33 | 53 | 48 | 54 | 45 | 54 | 51 | 50 | S. | SW. | SW. | NW. | ... | ... | 0·120 | ·085 | ·04 |
| 26 | 30·073 | 30·009 | 29·88 | 29·86 | 29·98 | 29·98 | 29·54 | 58 | 46 | 54 | 49 | 61 | 49 | 51 | SE. | SW. | SW. | calm | ·05 | ·05 | 0·125 | ·015 | ·16 |
| 27 | 30·260 | 30·161 | 30·00 | 29·90 | 30·14 | 30·02 | 29·65 | 60 | 40 | 55 | 48 | 59 | 49 | 46 | S. | NW. | SE. | calm | ... | ... | ... | ... | ... |
| 28 | 30·352 | 30·330 | 30·20 | 30·20 | 30·30 | 30·24 | 29·82 | 56 | 38 | 52 | 44 | 57 | 41 | 43·5 | NW. | N. | N. | calm | ... | ... | ... | ... | ... |
| 29 | 30·343 | 30·309 | 30·30 | 30·30 | 30·27 | 30·20 | 29·80 | 56 | 41 | 51 | 43 | 57 | 44 | 50 | NW. | NE. | N. | NW. | ·10 | ... | ... | ... | ... |
| Aver.: | 29·909 | 29·800 | 30·40 | 28·74 | 30·44 | 28·90 | 29·43 | 47·5 | 35·7 | 55 | 29 | 61 | 28 | 39·9 | | | | | 0·75 | 0·94 | 4·010 | 1·515 | 1·38 |

THE

PHILOSOPHICAL MAGAZINE

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[NEW SERIES.]

*MAY* 1828.

## LI. *On the Ellipticity of the Earth, as deduced from Experiments with the Pendulum; and on the Formulæ employed for obtaining it.* By WILLIAM GALBRAITH, *Esq. A.M.**

THE number of accurate experiments made with the pendulum to determine the figure of the earth, is now considerable, and the discussion of them has been somewhat extensive. From these it appears that discrepancies in the length of the pendulum by experiment in a given latitude may be considerable, amounting to above 0·005 of an inch, arising from the unequal action of the strata immediately under the station, for which no allowance can be easily made. From these considerations, it is evident combinations of pendulum experiments may be employed to bring out any required ellipticity, at least within certain limits, unless a judicious selection be made of those placed under analogous circumstances. To obviate these, the mean of a number of experiments in the same latitude, but in very different longitudes, distributed as regularly as conveniently can be, over the same parallel, should be chosen, comprehending, if possible, every variety of geological basis. From this method of proceeding, local irregularities would be so far counteracted as to bring out results somewhat satisfactory; and until this is accomplished, perhaps other artifices may be advantageously employed to obviate as far as possible these unavoidable and troublesome incongruities. Among other considerations, perhaps a basis of a certain specific gravity might be chosen as a standard; and by comparing the lengths of the pendulum on various bases of different specific gravities, an approximate rule might perhaps be obtained by which, with considerable exactness, the lengths of the pendu-

* Communicated by the Author.

lums on different bases might be reduced to the same. Taking ordinary alluvial formations for the standard density, of which the specific gravity may be about 2·5,—and by experiments made on subsoils of light sand, gravel, or clay, combined with those made on sandstone, basalt, schistus and granite, in the same latitude, and thence finding what effect these have in varying the length of the pendulum,—an approximate value of that effect may be pretty well estimated; whence a rule may be obtained, to reduce them all to the standard density, so far as relates to the exterior crust of the earth. These, combined with the form of the substratum and other peculiarities, of which several instances may be seen in Captain Sabine's work on the Figure of the Earth, would give the means of obtaining pretty accurately the medium length of the pendulum in any given latitude. Thus it may be inferred, that a variation of the specific gravity from 2 to 3 at the surface, and continued for some distance towards the centre, would give a variation of the length of the pendulum amounting to about 0·01 of an inch. Consequently, if 2·5 were taken for the standard specific gravity at the surface, and the lengths reduced to this, they may be increased or diminished about 0·005 of an inch, —a quantity sufficient to reconcile the most discordant of the pendulum experiments. These reductions, no doubt, should be applied to those experiments only which are likely to be employed to determine the figure of the earth, in order to obviate the discordancies from local irregularities; while the actual lengths from observation applied to other purposes,—such as our standards of weights and measures obtained at a particular spot,—ought to be allowed to remain as they are. Indeed, in an extensive series of experiments, the several corrections for the height, shape, specific gravity, &c. of the station, should be all placed in their respective columns beside the original lengths, to be employed or omitted, as might appear advisable, according to the purposes for which they are wanted. No doubt, with regard to these corrections, a good deal is left to the accuracy and judgement of the observer; and in some of them great precision cannot be attained; but in most cases a probable approximation only, which, although not perfectly accurate, will at least tend to give the mean ellipticity more truly than when they are neglected.—Having stated these preliminaries, our attention may now be directed to the formulæ for obtaining the ellipticity.

It appears from our ordinary treatises on the Figure of the Earth, supposing it to differ little from a sphere, that the centrifugal force which acts in opposition to gravity, decreases from the equator to the pole as the square of the cosine of the latitude;

titude; and consequently the diminution of gravity from this cause, in proceeding from the pole to the equator, will also be as the square of the cosine of the latitude. But the square of the sine increases as the square of the cosine diminishes; therefore the increase of gravity in proceeding from the equator to the pole, will be as the square of the sine of the observed latitude nearly. A little consideration will readily show, however, that this is correct, on the supposition of the earth being a perfect sphere, and is only an approximation not far from the truth in the case where the compression is small. The centrifugal force, therefore, is strictly proportional to the ordinate to the polar axis which involves the compression,—the very thing we are in quest of. Now as this is supposed to be the unknown quantity, it must be assumed equal to about $\frac{1}{300}$, what it is already known to be nearly; we may infer that the centrifugal force decreases as the square of the sine of the *reduced latitude* exactly. The difference arising from these two suppositions is undoubtedly small; but in very nice disquisitions, minute quantities ought not to be entirely disregarded.

The length of the pendulum is as the force of gravity; and it has generally been inferred, from suppositions not far from the truth, that the length of the pendulum from the equator to the pole, increases as the square of the sine of the latitude. The latitude hitherto used is that derived directly from observation, supposing the plumb-line to hang perpendicularly to the surface, or to the tangent plane to that surface, at the place of observation. Now there is no doubt that, from the equilibrium of the fluid part of the earth, the *direction* of gravity at the surface is exactly, or very nearly, in this vertical line. But the *force* of gravity at any point on the surface of a spheroid is (Schubert, *Astronomie Physique*, vol. iii. § 125.) inversely proportional to the radius of the spheroid, or the line drawn from the place of observation on the surface to the *centre*. This is conformable to experience; for it is known that the length of the pendulum, independently of centrifugal force, increases as we proceed from the equator to the pole where it is nearest the centre; and consequently, on that very account, its length there is greatest. I am well aware that the length of the pendulum has, so far as I know, always been affirmed to increase from the equator to the pole, nearly as the square of the sine of the observed latitude; though from what I have already advanced, it appears that it increases as the square of the sine of the *reduced latitude*, the compression being assumed by estimation, as has already been observed.

"For in an oblate spheroid (Playfair's Outlines of Natural Philosophy, vol. ii. § 296.) differing little from a sphere, if

 *b* be

$b$ be the polar semiaxis, $b+c$ the radius of the equator, $\phi$ *the angle which a line drawn from the centre to a given point on the surface makes with the axis of the spheroid*, and $f$ the force with which the given point is attracted by the spheroid; then it has been shown by M. Chastellot, (in the French translation of Newton's *Principia*, tom. ii. Paris edition of 1756, § xxxvii. coroll. I. page 236, 237.) that

$$f = \frac{4\pi b}{3}\left(1 + \frac{4c}{5b} - \frac{c}{5b}\sin^2\varphi\right) \text{ ........... } (1)$$

But if $\lambda$ be the latitude, and $\theta$ the angle of the vertical with the radius, commonly called the reduction of the latitude, and therefore $\phi = \lambda - \theta$, or the reduced latitude, then

$$f = \frac{4\pi a}{3}\left(1 + \frac{4c}{5a} + \frac{c}{5a}\sin^2(\lambda - \theta)\right), \text{ or if } a = 1,\ \frac{c}{a} = \varepsilon,$$

there will result

$$f = \frac{4\pi}{3}\left(1 + \frac{4}{5}\varepsilon + \frac{1}{5}\varepsilon\sin^2(\lambda - \theta)\right) \text{ ......... } (2)\text{"}$$

This expression for the attraction, shows that the gravitating force in proceeding from the equator to the pole, increases as the square of the sine of the *reduced latitude.* From similar principles, and the considerations formerly advanced, it follows that the increase of the force of gravity depending upon the diminution of the centrifugal force, also increases as the square of the sine of the *reduced latitude.* On combining these, the absolute increase of the length of the pendulum from the equator to the pole, is proportional to the square of the sine of the reduced latitude, not of the observed latitude. I advance this conclusion with diffidence, and with all due deference to those who have preceded me, and am fully aware of the authority of such names as those of Clairaut and Laplace: but I think my conclusion is founded in truth; and if it be so, it cannot be shaken by the authority of any name, however great. No doubt the difference between the square of the sine of the observed and reduced latitude is not great; since at its maximum at 45°, supposing the ellipticity to be about $\frac{1}{300}$, making the reduction 11′ 29″ only, the square of the sine of the observed latitude, or 45°, is 0·5, and the square of the sine of the reduced latitude, or 44° 48′ 31″, is 0·4966596. Now if the excess of the polar above the equatorial pendulum amount to 0·208 of an inch, then 0·208 (0·5−0·4966596) = 0·000695 of an inch, the quantity that the computed pendulum at the observed latitude of 45° would be too great: and hence, when the lengths of the experimental and computed pendulums are compared in the usual hypothesis, a disagreement at this parallel will always occur, which may very easily be imputed to a wrong cause;

such

such as an error in the experimental results, a protuberance of the earth at that latitude, a diminution of density, &c. This has occurred to me from some of my own computations, as well as from those of others which I have seen, but which seem to disappear when the proper (the reduced) latitude is employed. In order to obtain correct results, the formulæ should possess all the accuracy which can be given them. For this purpose I have elsewhere reconsidered the usual method of obtaining the compression, and made some slight correction.

If $\phi$ denote the centrifugal force at the equator, $r$ the radius of the equator, $t$ the time of rotation of the earth about its axis, and $l$ the length of the pendulum, then $\varphi = \dfrac{r}{\left(\frac{t}{2}\right)^2 l}$ ...... (1)

and consequently the ratio of the centrifugal force to gravity will be expressed by $\dfrac{\varphi}{1+\varphi} = \dfrac{r}{r+\left(\frac{t}{2}\right)^2 l} = q$ ........... (2)

If the most accurate values of these quantities be introduced, $q$ will be found to be $0{\cdot}003455 = \frac{1}{289{\cdot}436}$, somewhat less than $\frac{1}{289}$, that usually employed. Now if $\varepsilon$ denote the ellipticity, $p$ a certain coefficient to be found from an investigation of the conditions of equilibrium of a spheroid, $y$ the excess of the polar above the equatorial pendulum, and $z$ the length of the equatorial pendulum, then

$$\varepsilon = \frac{pr}{r+\left(\frac{t}{2}\right)^2 z} - \frac{y}{z} \quad \text{.............................} \quad (3)$$

Since by considerations derived from the case of a homogeneous spheroid, which may be supposed very nearly applicable to the case of the earth, the value of $p$ is 2·491516, instead of 2·5, that commonly adopted; whence it follows that

$$\varepsilon = 0{\cdot}0086082 - \frac{y}{z} \quad \text{............................} \quad (4)$$

This formula will, I believe, give the ellipticity as nearly correct as the necessary data required in the substitutions can be depended on.

As we have not as yet a sufficient variety of experiments in different longitudes under the same parallel, affected by the various irregularities tending to destroy each other, neither have we sufficient data to reduce them to a substratum of a given density, as formerly proposed; it remains to employ the only method left, which may perhaps effect nearly the same thing. This consists in reducing as many lengths of the pendulum as possible to the same point which are very near it, by the

the usual formula, which, from their proximity, cannot introduce any considerable error, as none are selected for this purpose whose latitude differs more than about 5° on each side of it, from that to which they are reduced. As an equal number was generally chosen on each side of it too, any small error in the formula of reduction which was used, or $\Delta l = \pm 0{\cdot}208 (\sin^2 \lambda'' - \sin^2 \lambda')$, will be by that means counteracted.

No doubt objections may be urged against this mode of procedure; but it is perhaps the best under the present circumstances of the problem which can be employed, as the most accurate method of distributing the observations regularly over the quadrantal arc of the meridian. Now a series of observations distributed regularly over the meridian is of considerable importance to determine the exact ellipticity, even when the method of the least squares is employed; for in this case also a preponderance of observations at any latitude has its effect in obtaining the compression.—Having now assigned the reasons for adopting the methods we have chosen, it remains to apply them to a collection of all the experiments which could with confidence be employed. These are principally the series of Captain Kater, M. Biot, Captain Sabine, M. Freycinet, M. Duperrey, together with a few more from Sir Thomas Brisbane, Captain Hall, and perhaps one or two others.

| Places. | Latitude. | Experimental Pendulum. | Reduced Pendulum to Latitude. | Length. |
|---|---|---|---|---|
| Rawak .......... | 0° 1′ 34″ S. | 39·01482 | | |
| Sumatra......... | 0 1 49 N. | 39·01812 | | |
| St. Thomas...... | 0 24 41 N. | 39·02074 | 0° 0′ 0″ | 39·01650 |
| Galapagos ...... | 0 32 19 N. | 39·01717 | | |
| Maranham...... | 2 31 43 S. | 39·01214 | | |
| Ascension (D.) | 7 55 9 S. | 39·02364 | | |
| Ascension (S.) | 7 55 48 S. | 39·02410 | 5 0 0 | 39·01877 |
| Sierra Leone ... | 8 29 28 N. | 39·01624 | | |
| Trinidad........ | 10 38 56 N. | 39·01884 | | |
| Bahia............ | 12 59 21 S. | 39·02223 | 10 0 0 | 39·02090 |
| Madras ......... | 13 4 9 N. | 39·02338 | | |
| Guam........... | 13 27 51 N. | 39·03023 | 15 0 0 | 39·02913 |
| Jamaica ......... | 17 56 7 N. | 39·03144 | | |
| Isle of France.. | 20 9 56 S. | 39·04769 | 20 0 0 | 39·03856 |
| Isle of Mowi ... | 20 52 7 N. | 39·04737 | | |
| San Blas ....... | 21 32 24 N. | 39·03828 | 25 0 0 | 39·05167 |
| RioJaneiro(H.) | 22 55 22 S. | 39·04374 | | |

Rio

| Places. | Latitude. | Experimental Pendulum. | Reduced Pendulum to Latitude. | Length. |
|---|---|---|---|---|
| Rio Janeiro (F.) | 22° 55′ 13″ S. | 39·04371 | 30° 0′ 0″ | 39·06492 |
| P. Jackson (B.) | 33 51 34 S. | 39·07724 | | |
| P. Jackson (D.) | 33 51 39 S. | 39·07897 | 35 0 0 | 39·08166 |
| Formentera .... | 38 39 56 N. | 39·09424 | | |
| New York ...... | 40 42 43 N. | 39·10168 | 40 0 0 | 39·09908 |
| Toulon ......... | 43 7 9 N. | 39·11038 | | |
| Figeac........... | 44 36 45 N. | 39·11322 | | |
| Bordeaux....... | 44 50 26 N. | 39·11303 | 45 0 0 | 39·11585 |
| Clermont ....... | 45 46 48 N. | 39·11812 | | |
| Paris ............ | 48 50 14 N. | 39·12929 | | |
| Shanklin........ | 50 37 24 N. | 39·13614 | 50 0 0 | 39·13329 |
| Dunkirk......... | 51 2 10 N. | 39·13773 | | |
| London ......... | 51 31 8 N. | 39·13929 | | |
| Falkland I. (D.) | 51 31 44 S. | 39·13966 | | |
| Falkland I. (F.) | 51 35 18 S. | 39·13720 | | |
| Arbury Hill ... | 52 12 55 N. | 39·14250 | 55 0 0 | 39·15163 |
| Clifton ......... | 53 27 43 N. | 39·14600 | | |
| Leith (Biot.) | 55 58 37 N. | 39·15547 | | |
| Leith (Kater.) | 55 58 41 N. | 39·15554 | | |
| Portsoy (Kater.) | 57 40 59 N. | 39·16159 | 60 0 0 | 39·16772 |
| Stockholm ...... | 59 20 34 N. | 39·16541 | | |
| Unst (Biot.) | 60 45 25 N. | 39·17181 | | |
| Unst (Kater.) | 60 45 28 N. | 39·17146 | 65 0 0 | 39·18213 |
| Drontheim (S.) | 63 25 54 N. | 39·17456 | | |
| Hare Isle....... | 70 26 17 N. | 39·19840 | | |
| Hammerfest ... | 70 40 5 N. | 39·19519 | 70 0 0 | 39·19553 |
| Greenland ...... | 74 32 19 N. | 39·20335 | | |
| Melville Isle ... | 74 47 12 N. | 39·20700 | 75 0 0 | 39·20600 |
| Spitzbergen .... | 79 49 58 N. | 39·21469 | | |

| $\lambda$ | $\lambda-\theta$. | Pendulum. |
|---|---|---|
| 0° | 0° 0′ 0″ | $39·01650-z-0·0000000\,y=E$ |
| 5 | 4 58 1 | $39·01877-z-0·0074963\,y=E_1$ |
| 10 | 9 56 5 | $39·02090-z-0·0297652\,y=E_2$ |
| 15 | 14 54 17 | $39·02913-z-0·0661582\,y=E_3$ |
| 20 | 19 52 38 | $39·03856-z-0·1156039\,y=E_4$ |
| 25 | 24 51 14 | $39·05167-z-0·1766568\,y=E_5$ |
| 30 | 29 50 5 | $39·06492-z-0·2475060\,y=E_6$ |
| 35 | 34 49 14 | $39·08166-z-0·3260503\,y=E_7$ |
| 40 | 39 48 42 | $39·09908-z-0·4099407\,y=E_8$ |
| 45 | 44 48 31 | $39·11585-z-0·4966596\,y=E_9$ |

| λ | λ—θ. | Pendulum. |
|---|---|---|
| 50° | 49° 48′ 41″ | 39·13329—z—0·5835803 y=$E_{10}$ |
| 55 | 54 49 12 | 39·15163—z—0·6680546 y=$E_{11}$ |
| 60 | 59 50 3 | 39·16772—z—0·7474900 y=$E_{12}$ |
| 65 | 64 51 11 | 39·18213—z—0·8194250 y=$E_{13}$ |
| 70 | 69 52 36 | 39·19553—z—0·8816350 y=$E_{14}$ |
| 75 | 74 54 15 | 39·20600—z—0·9321740 y=$E_{15}$ |

Whence $z = 39{\cdot}098334 - 0{\cdot}4067622y$ .............. (A)

Again,

$$0{\cdot}0000000 - 0{\cdot}0000000\,z - 0{\cdot}0000000\,y = E$$
$$0{\cdot}2924964 - 0{\cdot}0074963\,z - 0{\cdot}0000562\,y = E_1$$
$$1{\cdot}1614649 - 0{\cdot}0297652\,z - 0{\cdot}0008860\,y = E_2$$
$$2{\cdot}5820970 - 0{\cdot}0661582\,z - 0{\cdot}0043769\,y = E_3$$
$$4{\cdot}5130098 - 0{\cdot}1156039\,z - 0{\cdot}0133643\,y = E_4$$
$$6{\cdot}8987431 - 0{\cdot}1766568\,z - 0{\cdot}0302076\,y = E_5$$
$$9{\cdot}6688209 - 0{\cdot}2475060\,z - 0{\cdot}0612592\,y = E_6$$
$$12{\cdot}7425870 - 0{\cdot}3260503\,z - 0{\cdot}1063088\,y = E_7$$
$$16{\cdot}0283042 - 0{\cdot}4099407\,z - 0{\cdot}1680514\,y = E_8$$
$$19{\cdot}4272624 - 0{\cdot}4966596\,z - 0{\cdot}2466707\,y = E_9$$
$$22{\cdot}8374171 - 0{\cdot}5835803\,z - 0{\cdot}3405660\,y = E_{10}$$
$$26{\cdot}1554265 - 0{\cdot}6680546\,z - 0{\cdot}4462970\,y = E_{11}$$
$$29{\cdot}2774790 - 0{\cdot}7474900\,z - 0{\cdot}5587402\,y = E_{12}$$
$$32{\cdot}1068169 - 0{\cdot}8194250\,z - 0{\cdot}6714573\,y = E_{13}$$
$$34{\cdot}5561511 - 0{\cdot}8816350\,z - 0{\cdot}7772800\,y = E_{14}$$
$$36{\cdot}5468138 - 0{\cdot}9321740\,z - 0{\cdot}8689486\,y = E_{15}$$

Whence $z = 39{\cdot}1498536 - 0{\cdot}6598551\,y$ ................ (B)

Equating (A) and (B) there will result

$$y = 0{\cdot}20348 \quad \text{and} \quad z = 39{\cdot}015576$$

therefore $\frac{y}{z} = 0{\cdot}0052153$, and

$$E = 0{\cdot}0086082 - 0{\cdot}0052153 = 0{\cdot}003393 = \tfrac{1}{293}\text{*}.$$

Adopting the formula for the length of the pendulum, or $l = z + y \sin^2 (\lambda - \theta)$, the lengths of the pendulum by computation may be compared with the foregoing means derived from experiment.

* As there is some reason to believe that several of the pendulums near the equator are too great; if the first three of those above be rejected, then the ellipticity will be $\frac{1}{297}$. Indeed, I have for some time been induced to consider $\frac{1}{300}$ to be that which must ultimately be generally adopted.

| Lat. $\lambda$ | Reduced Latitude. $\lambda-\theta$. | Experimental Pendulum. | Pendulum computed by $y \times \sin^2(\lambda-\theta)$. | Deviations of Formula. |
|---|---|---|---|---|
| 0° | 0° 0′ 0″ | 39·01650 | 39·01558 | $-0·00092=E$ |
| 5 | 4 58 1 | 39·01877 | 39·01710 | $-0·00167=E_1$ |
| 10 | 9 56 5 | 39·02090 | 39·02163 | $+0·00073=E_2$ |
| 15 | 14 54 17 | 39·02913 | 39·02904 | $-0·00009=E_3$ |
| 20 | 19 52 38 | 39·03856 | 39·03910 | $+0·00054=E_4$ |
| 25 | 24 51 14 | 39·05167 | 39·05152 | $-0·00015=E_5$ |
| 30 | 29 50 5 | 39·06492 | 39·06594 | $+0·00102=E_6$ |
| 35 | 34 49 14 | 39·08166 | 39·08192 | $+0·00036=E_7$ |
| 40 | 39 48 42 | 39·09908 | 39·09899 | $-0·00009=E_8$ |
| 45 | 44 48 31 | 39·11585 | 39·11664 | $+0·00079=E_9$ |
| 50 | 49 48 41 | 39·13329 | 39·13432 | $+0·00103=E_{10}$ |
| 55 | 54 49 12 | 39·15163 | 39·15151 | $-0·00012=E_{11}$ |
| 60 | 59 50 3 | 39·16772 | 39·16767 | $-0·00005=E_{12}$ |
| 65 | 64 51 11 | 39·18213 | 39·18231 | $+0·00018=E_{13}$ |
| 70 | 69 52 36 | 39·19553 | 39·19497 | $-0·00056=E_{14}$ |
| 75 | 74 54 15 | 39·20600 | 39·20525 | $-0·00075=E_{15}$ |
| 80 | 79 56 4 | | 39·21284 | |
| 85 | 84 58 0 | | 39·21749 | |
| 90 | 90 0 0 | | 39·21906 | |

From a review of the whole, it appears that the ellipticity is somewhat greater than it has generally been supposed, though the difference is not very great. It seems also probable that the discordancies of the results derived from the formula for computing the length of the pendulum are not very considerable, and are so irregular that no general protuberance, or uncommon diminution of the force of gravity, is any where likely to follow from the observations by the pendulum.

The only doubt that remains, is perhaps the effect of combining the lengths of the pendulum on stations of very different specific gravities, where a preponderance of dense or light materials is likely to prevail.

It is to be feared that the lengths of the pendulum near the equator are rather in excess, as the stations have been, generally speaking, on bases of considerable density; and by this means rendering the equatorial pendulum too great, the excess of the polar above the equatorial pendulum too small, and consequently the compression too great; though more numerous experiments near that circle are still wanting to decide this point in an unexceptionable manner.

It is to be expected, that some light may be thrown on the exact

exact quantity of compression by the various arcs on the meridian, and the parallels now executing throughout Europe; though I am inclined to believe, that measurements of arcs of the meridian near the equator, and as close to the poles as convenient, will be more decisive. In the execution of these, however, care should be taken to select proper portions of the meridian under uniformity of geological character. Some of the extensive plains in the North of Russia and in South America, appear to me to be the most eligible; though I have seen Spitzbergen recommended, from the facility it affords of executing the necessary operations. Yet for these very reasons I am disposed to think that from its irregular and broken character, by extensive *fiords*, as they are called, running amongst the islands, it is to be feared that very irregular local attractions will be found to prevail; so that each series of zenith distances would be something like the Schihallien experiment; and the irregularities thence produced, would rather tend to throw a doubt over the true compression, than to point out the proper quantity.

Edinburgh, April 1827*. WILLIAM GALBRAITH.

P.S. In the foregoing remarks, it will be seen that I have endeavoured to introduce a few small corrections into the usual formulæ for obtaining the ellipticity, which may perhaps be thought by some, to be too minute to deserve much attention in the present state of the problem, as being much within the probable errors of observation. For instance, instead of taking the ratio of the centrifugal force to gravity at the equator at $\frac{1}{289}$, I have adopted $\frac{1}{289\frac{1}{4}}$ nearly, as it ought to be; and for the coefficient $\frac{5}{2} = 2{\cdot}5$, it is assumed at about 2·49. These, no doubt, appear trifling; but nevertheless their adoption reduces the constant 0·00865 to 0·00861, and this small modification diminishes the ellipticity in an equal degree. I have also preferred the reduced latitudes to the apparent, or those got directly from observation; and this removes an irregularity which at one time I was persuaded occurred in the elliptical figure of the earth; namely, a protuberance at about the parallel of 45° N., which I am now disposed to attribute to a slight error in the approximate formula usually employed. It has also been thought advisable to apply the method of the least squares with caution. I am perfectly aware that this method is the best that can possibly be applied in many cases; but that there may be particular instances where, by employing it without discrimination, it may, from the manner in which it must be employed, be the means of leading to more erroneous conclu-

* So in the MS.—EDIT.

sions

sions than by a selection of the most decisive observations. As this subject has, since the first part of these remarks was written, been treated of by Mr. Ivory, in a late Number of the Phil. Mag., much more ably than I could pretend to, it will be unnecessary for me to say any thing more at present upon the subject.

Edinburgh, March 12, 1828. W. G.

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## LII. *Account of a Paper by Prof.* Gauss, *intitled* "Disquisitiones generales circa Superficies Curvas:" *communicated to the Royal Society of Göttingen on the 8th of October* 1827*.

ALTHOUGH geometricians have much occupied themselves with general investigations on curve surfaces, the results of which form a considerable portion of the higher department of geometry, this subject is so far from being exhausted, that it may be safely asserted that as yet a small part only of a most fertile field of inquiry has been cultivated. The author has already endeavoured some years ago to take a new view of this subject in the solution of the problem: To find all representations of a given surface on another surface, in which the smallest parts shall remain similar. The object of the present paper is to open new views, and to unfold a portion of the new truths which are thereby rendered accessible. We shall explain as much as can be rendered intelligible without entering too deeply into the subject; but we must remark, that the new definitions as well as the theorems, in order to be generalized, will require some restrictions and qualifications which must be omitted in this place.

In investigations which involve a variety of directions of straight lines in space, it is advantageous to designate these directions by those points on the surface of an invariable sphere at which the radii drawn parallel to the same, terminate: the radius and centre of this auxiliary sphere are entirely arbitrary; for the latter, the unity of linear dimension may be chosen. This proceeding agrees, in fact, with the one constantly used in astronomy, where all directions are referred to a fictitious celestial sphere of an infinite radius. Spherical trigonometry, and some other theorems to which the author has added one of frequent application, are then employed for solving the various problems that present themselves by a

* From the *Göttingische gelehrte Anzeigen.*—This abstract is probably from the pen of the distinguished Author.

 comparison

comparison of the different directions which occur. If the directions of lines normal to the curve surface, drawn from every point of the same, be designated by the points of the sphere, corresponding to them according to the proceeding above explained, so that each point of the curve surface has its corresponding point on the auxiliary sphere, then, generally speaking, every line on the curve surface will have a corresponding one on the auxiliary sphere, and every portion of surface of the former will have its corresponding portion on the latter. The smaller the deviation of a part is from a plane, the smaller will be the corresponding part of the sphere; and it is therefore a very natural proceeding to employ, as measure of the total curvature, the area of the corresponding portion of the sphere. The author, accordingly, calls this area the *entire curvature* of the corresponding portion of curve surface. Besides this quantity, the *position* of the part comes into consideration, which, independently of the relation of magnitude, may be a similar or a reversed one: these two cases may be distinguished by the positive or negative sign being put before the expression of the total curvature. This distinction, however, has only a definite signification, so far as the figures are assumed to be on definite sides of the surfaces: the author assumes them in the sphere on the exterior, and in the curve surface on that side on which the normal line is supposed to be erected; and it follows that the positive sign belongs to convex-convex and to concave-concave surfaces (which are not essentially different), and the negative sign to concave-convex ones. If the portion of the curve surface in question consists of parts dissimilar in this respect, further specifications become necessary, which must here be passed over.

The comparison of the areas of the portion of the curve surface and its corresponding portion of the auxiliary sphere, leads to a new notion (in the same manner as the comparison of volume and mass produces the notion of density). The author calls *measure of curvature* in any point of the curve surface, the value of the fraction whose denominator is the area of an infinitely small part of the curve surface at this point; and the numerator the area of the corresponding part of the auxiliary sphere, or the *entire curvature* of the element. It is clear that in the sense of the author, *entire curvature* and measure of curvature are in curve surfaces analogous to what in curve lines is respectively called amplitude and curvature; he doubted of the propriety of transferring to curve surfaces the latter expressions, which, though sanctioned by use, are not very appropriate terms. It is however of less consequence how

how these new notions are denominated, than to justify their introduction by remarkable and useful theorems to which they give rise.

The solution of the problem: To find the measure of curvature in each point of a curve surface,—presents itself under various forms, according to the manner in which the nature of the curve superficies is given. The simplest manner is, the points in space being expressed by three rectangular co-ordinates $x$, $y$, $z$, to represent the one co-ordinate as a function of the two others: in this case the expression which is obtained for the measure of curvature is the simplest. This leads at the same time to a remarkable connection between this measure of curvature and the curvatures of those curves which are produced by the intersection of the curve surface by planes perpendicular to it. It is well known that Euler has first demonstrated, that two of the intersecting planes, which are likewise at right angles to each other, have the property,—that to the one belongs the smallest, to the other the greatest radius of curvature; or rather, that in them the extreme curvatures occur. Now it results from the expression for the measure of curvature just alluded to, that this becomes equal to a fraction whose numerator is unity, and the denominator the product of the two extreme radii of curvature.

The expression for the measure of curvature becomes less simple when the nature of the curve surface is given by an equation between $x$, $y$, $z$; and the former still more complicated, when the surface is represented by $x$, $y$, $z$ being given as functions of two new variable quantities $p$, $q$. In the latter case the expression contains fifteen elements; viz. the partial differential quotients of the first and second order of $x$, $y$, $z$ for $p$ and $q$: but it is less important in itself, than by its being the transition to another expression, which forms one of the most remarkable propositions of this theory. In that manner of representing the nature of the curve surface, the general expression for any linear element of the same, or for $\sqrt{(dx^2 + dy^2 + dz^2)}$ assumes this form: $\sqrt{(E dx^2 + 2F dx . dy + G dy^2)}$; where E, F, G become functions of $p$ and $q$; the above-mentioned new expression for the measure of curvature contains only these quantities, together with their partial differential quotients of the first and second order. It is evident, therefore, that for determining the measure of curvature, the general expression of a linear element only is required, and not the expressions for the co-ordinates $x$, $y$, $z$ themselves. An immediate consequence of this is the following remarkable theorem: If a curve surface or a portion of the same can be evolved on another plane, the measure of curvature remains unchanged

unchanged in every point after the evolution. From this follows, as a particular case; that in a curve surface which can be evolved in a plane, the measure of curvature is always $=0$. From this is immediately derived the characteristic equation of the surfaces capable of being evolved in a plane; namely, if $z$ be regarded as a function of $y$ and $x$

$$\frac{ddz}{dx^2} \cdot \frac{ddz}{dy^2} - \left(\frac{ddz}{dx \cdot dy}\right)^2 = 0,$$

an equation which, indeed, has long been known, but in the author's opinion has never yet been rigorously demonstrated.

These propositions lead to a new view of the theory of curve surfaces, and open a wide uncultivated field of investigation. If surfaces be considered not as bounds of bodies, but as bodies one dimension of which is evanescent, and at the same time flexible but not expansible, it will be conceived that two essentially different classes of relations must be distinguished,—those which suppose a definite shape of surface in space, and those which are independent of the various shapes which the surface is capable of assuming. It is of the latter that the author treats; and it appears by what has been observed above, that the measure of curvature is one of them: but it will easily be seen that the consideration of the figures which can be described on the surface, their angles, their areas and *entire curvature*, the connection of the points by shortest lines, &c. belong to this class. All these investigations must proceed from this, That the nature of the curve surface is given by the expression of an indefinite linear element of this form $\surd(E dp^2 + 2F dp \cdot dq + G dq^2)$.

The author has inserted in the present paper a part of the investigations on this subject, which have engaged him during several years, confining himself to such as are not too remote from the point where his labours began, and which may serve as general auxiliaries for numerous further investigations. In this notice we must be still shorter, and be content to give only as a specimen the following theorems.—If on a curve surface, a system of an infinity of shortest lines, all of equal length, proceed from *one* point, the line passing through their extreme points is at right angles to every one of them. If from every point of any line on a curve surface, shortest lines of equal length and at right angles to that line are drawn,—all these lines are likewise at right angles to that line which connects their other extreme points. Both these theorems, the second of which may be considered as a generalization of the first, are demonstrated analytically, as well as by simple geometrical considerations. The excess of the sum of the angles of a tri-

angle

angle formed by shortest lines above two right angles, is equal to the *entire curvature* of the triangle. It is here supposed, that the unity of angles is the one whose corresponding arc equals the radius (57° 17′ 45″); and that for the entire curvature, which is a portion of the surface of the auxiliary sphere, the area of a square whose side is the radius of the auxiliary sphere is considered as unity. This important theorem may evidently be thus expressed: The excess of the angles of a triangle formed by shortest lines above two right angles : eight right angles :: the portion of the auxiliary sphere corresponding to that triangle : the whole surface of the auxiliary sphere. Generally the excess of the angles of a polygon of $n$ sides, all shortest lines, above $2n-4$ right angles, is equal to the *entire curvature* of the polygon.

The general investigations contained in the paper are finally applied to the theory of triangles formed by shortest lines. We shall mention only a few of the principal theorems of this theory. If $a, b, c$ be the sides of such a triangle (which are considered as quantities of the first order), A, B, C the opposite angles, $\alpha, \beta, \gamma$ the measures of curvature in the angular points, $\sigma$ the area of the triangle; then $\frac{1}{3}(\alpha + \beta + \gamma)\,\sigma$ will be the excess of the sum A + B + C above two right angles down to quantities of the fourth order. With the same accuracy the angles of a plane rectilinear triangle, the sides of which are $a$, $b$, $c$, will be

$$A - \tfrac{1}{12}(2\alpha + \beta + \gamma)\,\sigma$$
$$B - \tfrac{1}{12}(\alpha + 2\beta + \gamma)\,\sigma$$
$$C - \tfrac{1}{12}(\alpha + \beta + 2\gamma)\,\sigma$$

It is clear, that the latter theorem is a generalization of a well known proposition first given by Legendre, by which, omitting the quantities of the fourth order, the angles of the rectilinear triangle are obtained by diminishing the angles of the spherical one by one-third of the spherical excess. For surfaces which are *not* spherical, unequal reductions must be applied to the angles, and, generally speaking, the unequal part is a quantity of the third order: if, however, the whole surface deviates but little from the spherical form, it involves besides a factor of the same order with the deviation from the spherical form. It is, undoubtedly, important for the theory of geodetical operations, to be able to calculate the inequalities of these reductions, and to obtain thereby the full conviction that they are to be considered as insensible for all measurable triangles on the surface of the earth. Thus it is found, that in the largest triangle in the measurement conducted by the author,

thor,

thor, the greatest side of which is nearly fifteen (German) geographical miles, and in which the excess of the sum of the three angles above two right ones is nearly fifteen seconds, the three reductions of the angles to angles of a rectilinear triangle are 4″·95113, 4″·95104, 4″·95131. The author has, however, likewise calculated the terms of the fourth order wanting in the above expressions, which assume for the sphere a very simple form; but in measurable triangles on the surface of the earth they are quite insensible; and, in the above example, they would have diminished the first reduction by only two unities in the fifth decimal place, and have increased the third reduction by the same amount.

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## LIII. *On* Savart's *Experiments on the Motions of mediately agitated Membranes.* *By Dr.* Wm. Weber, *Academical Teacher at Halle**.

[With an Engraving.]

"FROM the manner just stated of the division of membranes into vibrating divisions," says Savart, in the *Ann. de Chim. et de Phys.* (1826. tom. xxxii. p. 385.) "it is evident that the acoustic figures called by Chladni distortions, form the transition between a variety of figures of sound which are not distorted (in which may be reckoned several tones of the flageolet). As Chladni has only observed the distortions of figures of sound which are nearest the undistorted figures (which he considers as fundamental figures), and as he fixed the number of vibrations merely by the ear, which does not admit of sufficient accuracy, he might justly assert that the tone in the distortion of the figures of sound is the same as in the fundamental figures. But in the tables accompanying his *Traité d'Acoustique*, we find distortions of figures of sound to which belong semitones, a whole tone, and even one-third higher than when the figure of sound has what is termed the fundamental form." The difference between Chladni and Savart, according to these observations of the latter, is, that Chladni asserts that there is in *sounding* plates no transition from one tone of the flageolet to another; whilst Savart says that by a repetition of experiments, he has discovered this transition.

Savart justly says, that nothing in nature appears isolated. He has actually shown in this treatise, as well as in some former ones, an extraordinary number of transitions of one kind of vibration into the other, in almost all bodies used for musical purposes. But it is also a universal rule of nature,

* From Schweigger's *Jahrbuch der Chemie*, &c. N. R. Band xx. p. 176.

that

that the transitions of the different phænomena do not show themselves when these phænomena appear most regularly and forcibly, but only when they become indistinct and indefinite. Now the *sounding vibrations* (which Chladni always considers by themselves) are constantly the *most uniform* and the *most violent vibrations.* There is, therefore, probably, a transition from a sounding vibration, through a series which do not produce sound (such as are less distinct and precise) standing vibrations, to a very different sounding vibration; but there is no transition from a sounding vibration through nothing but *sounding* vibrations to a quite different sounding vibration (producing a much higher or much deeper tone). However, Savart is perfectly right in affirming that many of the distortions of the figures of sound observed by Chladni are the beginnings of the transition of one kind of vibration to another, and that in these distortions, even the number of vibrations, whilst the tone is becoming weaker and beginning to cease, is a little altered. *But if a vibrating body gives a distinct tone, it is in a very uniform and violent vibration, which is only possible when the number of its vibrating divisions is strictly fixed, and the sum of the motions is in all as equal as possible, in which case the height of the tone (the velocity of the vibrations) is unchangeable, as it depends as much on the elasticity and form of the membrane, as the velocity of the vibrations of a pendulum of a given length on gravitation.* This rule, laid down by Chladni, has thrown an extraordinary light and intelligibility on all acoustic phænomena.

In order, therefore, to render what has been said still more intelligible; and in consequence of there being several similarities in the motions of membranes observed by Savart, with the vibrations of sounding elastic *plates;* and finally, as it is now generally necessary to find out by experiment as closely as possible the extension of small vibrations on visible bodies;—I shall here communicate from the 32nd volume of the *Ann. de Chimie et de Physique*, the phænomena discovered by Savart.

Savart examined the lines upon which the sand remains in its place on mediately agitated bodies.

If an equally-stretched square rectangular or triangular membrane is mediately and regularly agitated, grains of sand scattered over it are thrown off from equally large and regularly-bounded divisions of those membranes, but are collected on the limits of these divisions nearly in the same manner as they are on the sounding plates.

Savart wished to cause the stretched membranes to vibrate mediately and with the utmost uniformity, and thereby to determine at his pleasure, the quicker or slower result of the agitations,

agitations, and to become perfectly acquainted with its velocity. How could these objects be better attained than by the sounding vibration of an organ-pipe, a bell, or disk, before which the membrane was expanded, so that every vibration transmitted by the air of those sounding bodies necessarily agitated the membrane?

In this manner he made the following double series of experiments. Once he held the extended membrane before an organ-pipe, which he was enabled by means of a stopper to lengthen or shorten at will, by which means the same membrane was successively agitated by very various rapid concussions. In the second place, he stretched a membrane of a very hygrometrical substance, viz. of paper, and made it gradually imbibe an increasing quantity of aqueous vapour. In this manner he was enabled to agitate a membrane of very different degrees of elasticity by constantly equally-quick concussions, *ex. gr.* by means of a sounding plate or bell held before it.

The membranes thus mediately agitated show, according to Savart's observations, the following similarity to sounding plates:

1. In mediately agitated membranes vibrating divisions are formed, which are separated by quiescent or slightly moved lines, as in the sounding plates.

2. These quiescent lines may undergo such distortions as Chladni has observed in the sounding plates. On the other hand, there are the following differences between the motions of mediately agitated membranes and sounding plates. First, In mediately agitated membranes the divisions near the edge are as great as those within, whilst in sounding plates the divisions near the border are not half so great. Secondly, In mediately agitated membranes *the different distortions of the quiescent lines are produced by the different width of the agitating undulations*, (*i.e.* by different high tones of the organ-pipe placed before the membrane,) whilst the observation in the sounding plates, as is generally known, is,—that when the quiescent lines are somewhat distorted, the breadth of the waves of the undulation proceeding from the plate is either not altered at all, or very imperceptibly, which may be easily known from the height of the tone.

If we hold a square membrane, the elasticity of which is not changed, before the opening of an organ-pipe provided with a stopper and made to vibrate, we may effect by means of the stopper the circumstance that the sand thrown off from the divisions represented in Plate VI. fig. 1. No. 1. remain only on the boundary line. If the tone of the organ-pipe becomes a trifle higher, the form of this boundary line upon which

which the sand remains, varies as in No. 2; and becomes, as the tone of the organ-pipe gets still higher, as in No. 3, 4, 5, 6, where the boundary lines merely consist of four parallel lines. In this manner the boundary lines which at first intersected each other at right angles, are changed into parallel lines. This transmutation may also be effected as represented at fig. 2, 3, 4, 5, 6, 7.

In the same manner four parallel nodes may pass into two parallels which have towards the former a perpendicular position, as shown at fig. 8; or four parallel nodes may change into four others intersected by two more perpendicularly, as shown in fig. 9. Other remarkable changes of the position and number of these boundary lines with increasing velocity of the agitating concussions, are seen in fig. 10, 11, 12, 13.

From these experiments it may be seen that there is a possibility of several transitions of one and the same sand-figure to another; as for instance, fig. 10. and fig. 11. The question is, what is it that produces any particular transition? Savart mentions a criterion by which, from the first alteration of the figure, it may be foretold what transition will ensue. He says:

First. On proceeding from a figure of nodes intersecting each other at right angles, the character of the following changes depends on the magnitude of the vertical angle on the points of intersection separate from one another. This is very apparent in the comparison of fig. 10. with fig. 11. which both form transitions of four parallels, from two other lines normally crossed, into six parallel lines.

Secondly. If, on the contrary, we have at first only parallel nodes, we may say that the character of the following changes depends on the difference of the curves which these lines may receive, which is distinctly seen from the same figures (10. and 11.), if one begins backwards (from No. 5, 4.); for in fig. 10. the nodes curve inward, whilst in fig. 11. they curve outward. The transitions are particularly remarkable when the lines form two curves outward, and one inward, or *vice versâ;* or when they form three curves outward, and two inward, &c. of which fig. 12. and fig. 13. furnish remarkable instances.

Circular and triangular membranes produce analogous phænomena. On a circular membrane, three diametrical lines may in this manner be produced, and these may be gradually again changed into one diameter and one circular line (fig. 14.). Moreover, on a circular membrane five diametrical lines may be produced and gradually changed into five parallels, as shown in fig. 15; and these five parallels may again be transformed into one diametral and two circular lines.

Very narrow long rectangular strips show similar phæno-

 mena.

mena. Thus the fundamental lines, where the sand keeps its position, may have the form fig. 16. No. 1. As the tone becomes deeper, all these lines approach the end B, so that the interval A$n$, and $n\,n'$, and $n'\,n''$, become larger; but $n''$ finally advances to B, so that the membrane has only two lines left. Another change is shown in fig. 17.

Savart has formed the hypothesis, that the membrane agitated by the undulations of the air is also in a standing and sounding vibration; that from this sounding membrane waves of sound are proceeding exactly of the breadth of those which arrive at it: that he therefore knew the breadth of the waves of sound proceeding from the membrane, from knowing the breadth of those which arrive at it. From these hypotheses it is inferred, that the modes of vibration produced by various flageolet sounds are gradually transfused into one another by an uninterrupted series of intermediate tones. What now may be said of sounding membranes, says Savart, is probably also the case with sounding elastic plates. But according to Chladni's account, it is not so with sounding plates. Savart thinks that Chladni has been mistaken in his observations. (See the beginning of this paper.) But in all the experiments I have hitherto made, I have always found Chladni's observations confirmed. But should Savart find a different result, it would be desirable that he should exactly point out the material, form, and thickness of the plate with which he made his experiments, as well as the manner of his having fastened the plate and made it vibrate, in order that the experiment may be repeated*.

These discoveries of Savart *merely concern the extension of small oscillations in firm bodies, in membranes;* as there is no question here about sounding bodies, nay not even of reverberating bodies, as one may be convinced by the ear, but only as to vibrations and soundless motions, which however show much similarity to the motions of reverberating and even sounding bodies.

I had a small wooden square frame made, which measured in the hollow six Paris-inches in length and breadth. Upon this I glued a sheet of wet English letter-paper (wove paper, without any wire-marks), which was perfectly free from thin places or other defects, and glued upon this paper small laths, so that it was equally stretched, and its edge everywhere immoveable.

* Chladni has investigated, in his *Acoustics*, Leipzig, 1802. § 114. p. 131. a particular case, in which with a similar fundamental figure, but which once has received a curve outward, and at another time inward, several tones are produced, and has proved that here too no transition from one flageolet-tone to another, through an uninterrupted series of tones, is manifested.

Such

Such a paper will sound even on any one pushing against the frame or gently blowing upon it. If one strewed upon this paper held horizontally some grains of coarse sand, and held a bell of a watch or a small disk of glass over the paper, *ex. gr.* near a corner, making it sound by means of a violin-bow, the sand began to move, and collected in those lines described by Savart.

In these experiments I found:—

1. That here also in the mediately agitated membranes an actual intersection of the reposing lines nowhere takes place, as Oerstedt and Strehlke have observed in sounding plates.

2. That the deepening or heightening of the tone of the organ-pipe, and the change of elasticity by the wetting of the membrane, are not the only means whereby the nodes in mediately agitated membranes became curved; but that this curving of the nodes might also be produced by trifling circumstances; by bringing, for instance, a sounding bell sometimes near a corner and sometimes near the centre of the square membrane.

3. That, as I have stated before, no trace of self-sounding or reverberation of the paper is to be observed. At the meeting of the Society for the Investigation of Nature, at Halle, on the 14th of July in the present year (1827), I repeated these experiments, and convinced the assembled members of it, especially the editors of this Journal.

I consider therefore the careful separation of the sounding reverberating vibrations from those slight motions of the bodies that do not sound or reverberate (which may certainly be very similar to the former), and which I have indicated in this article, as necessary; since great confusion is introduced by the interference of the latter with the laws of motion of sounding disks and reverberating bodies.

It has been shown in the Doctrine of Undulation, published by my brother and myself, how sounding and reverberating vibrations, figures of sound and figures of vibration, should be distinguished; and how it results from this, that Savart's and Chladni's experiments do not contradict each other*: but we must also distinguish between these vibrations and *vibrations without any acoustic effect*, which latter may also *unite the scattered sand into nodes.* We have further shown, in the work just quoted, that the sounding vibration is always of the class of the standing vibrations, with which all small motions of all bodies seem to terminate. From the circumstance that in all these small motions of bodies left to themselves, *at last* a certain balance and uniformity manifest themselves; and that this

* Vide *Jahrbuch,* 1825.

uniform

uniform *final vibration* is, in fact, the standing vibration, it may be explained why standing vibrations, and especially the sounding, are most independent of the *first* vibration—of the excitement of the tone; and also why Chladni's figures of sound are likewise very independent of it. The reverberating vibration consists of the *first* intersection of the waves just excited, and may in many instances coincide with the sounding vibration; for which reason also the reverberating figures are often represented like the figures of sound, (see Savart's former treatises). One recognizes, however, the figures of reverberation by a great dependence on the original agitation, *ex. gr.* on the direction in which the sounding body is moved. Finally, it is evident, that vibrations without any acoustic effect may at times be of the same nature as the sounding and reverberating vibrations; for all standing vibrations, for instance such as are so weak as not to act upon the organs of hearing, belong to this class.

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Supplement to the foregoing Paper.——*On the Employment of a resounding Membrane for the Observation of the Interference of the Undulations of Sound*.*

I HAVE stated in an Essay on Savart's Experiment with mediately agitated Membranes, (See p. 340, 341.) that in these membranes we observe neither an original sound nor a resonance, when an organ-pipe, a bell, a swinging disk, or any other longitudinally or transversely swinging body of a flat surface is held before the membrane. I have however succeeded in making such a membrane resound by simply holding a tuning-fork before it, so that one prong was held close to it, parallel with the diagonal line of the square frame. The sound of a tuning-fork by itself is but slightly heard, or not at all, at a little distance. But on bringing it in the manner described towards the membrane, without touching it, the sound is heard distinctly; while it is perceived that it proceeds, not from the fork, but from the membrane.

(With the help of such a resounding membrane I have been enabled to observe the phænomena of the action of the undulations of sound, the description and investigation of which has appeared in the *Jahrbuch*, 1826. iii. p. 385 to 430.)

The sounding of the membrane is in fact very distinct, when the outside of a prong is turned towards it. It is equally distinct when the two prongs are brought near at an equal distance; but on turning the tuning-fork gradually from the first position into the second, we come to a point where the sounding

* From Schweigger's *Jahrbuch*, Band xx. p. 247.

of

of the membrane suddenly ceases almost entirely; but on the fork being turned further, it immediately reappears. If we distinguish in the prongs of the tuning-fork three different surfaces; viz. 1. front and back surfaces; 2. side surfaces; 3. end surfaces; we observe this disappearance of sound at every transition from a front surface to a side one, and *vice versâ:* again, in every transition from a front surface to an end one, and *vice versâ;* but no cessation of sound is perceived in the transition from a side surface to an end one, and *vice versâ.* These phænomena are not distinctly audible when there is any noise, for which reason the experiments will be best made at night.

The following differences in these phænomena from those mentioned in the treatise alluded to (in the *Jahrbuch* 1826, iii. p. 385) are remarkable: 1. That the membrane is a plane extending far beyond the tuning-fork, and that every point of it can be agitated with equal facility, whilst the reciprocating inclosed column of air had only one narrow opening, and could only resound when the undulations of sound penetrated through this aperture; 2. That the membrane did not reciprocate, and nevertheless showed these phænomena, whilst with a column of air not reciprocating, at least if it be narrow, nothing can be observed.

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## LIV. *On the Figure of the Earth, as deduced from Measurements of different Portions of the Meridian.* *By* J. Ivory, *M.A. F.R.S.**

HAVING examined the figure of the earth as deduced from experiments with the pendulum, we are naturally led to consider the same question as it depends upon the measurements that have been made, of different portions of the meridian. If this interesting inquiry be placed within the reach of the human understanding, and be accessible to our industry, the two views of it must be consistent; and, by instituting a comparison between them, we may both learn what is already sufficiently well ascertained, and be directed in our future attempts to the most important points that still remain uncertain.

Let $a$ denote the equatorial radius of the earth; $e$ the eccentricity of the elliptical meridian; $\phi$ an arc of the meridian reckoned from the equator; and $\lambda$ the latitude of the extremity of $\phi$: then

$$d\phi = \frac{a(1-e^2)\,d\lambda}{(1-e^2\sin^2\lambda)^{\frac{3}{2}}}.$$

* Communicated by the Author.

If

If we now put $\sqrt{1-e^2} = 1-\varepsilon$, then $\varepsilon$ will be the compression or the ellipticity; and we shall have,

$$d\varphi = \frac{a(1-\varepsilon)^2 . d\lambda}{(1-(2\varepsilon-\varepsilon^2)\sin^2\lambda)^{\frac{3}{2}}}.$$

By expanding this formula and integrating as usual, rejecting the cube and higher powers of $\varepsilon$, we shall get,

$$\varphi = a.\left\{\lambda - \varepsilon\left(\frac{\lambda}{2} + \frac{3}{4}\sin 2\lambda\right) + \varepsilon^2\left(\frac{\lambda}{16} + \frac{15}{64}\sin 4\lambda\right)\right\}.$$

And, if $\varphi$ denote an arc of the meridian between the latitudes $\lambda'$ and $\lambda$; then

$$\varphi = a.\left\{\lambda - \lambda' - \varepsilon\left(\frac{\lambda-\lambda'}{2} + \frac{3}{4}(\sin 2\lambda - \sin 2\lambda')\right)\right.$$
$$\left. + \varepsilon^2\left(\frac{\lambda-\lambda'}{16} + \frac{15}{64}(\sin 4\lambda - \sin 4\lambda')\right)\right\}.$$

Further, let $n = \lambda - \lambda'$

$m = \lambda + \lambda'$

$p = 57^\circ{\cdot}2957795$

$\Delta = \frac{a}{p}$;

then, $$\phi = \Delta.\left\{n - \varepsilon\left(\frac{n}{2} + \frac{3}{2}p\sin n\cos m\right)\right. \qquad \text{(A)}$$
$$\left. + \varepsilon^2\left(\frac{n}{16} + \frac{15}{16}p\sin n\cos n\cos 2m\right)\right\}.$$

In this formula $\Delta$ is the length of 1° on the earth's equator; and $n$ and the coefficients of $\varepsilon$ and $\varepsilon^2$, are reckoned in degrees.

There has been so much discussion about the merits of the different portions of the meridian that have been determined trigonometrically, and their character is so well known, that it would be superfluous to say any thing on that subject here. The following table, which is taken from a paper by Professor Airy, in the Phil. Trans. for 1826, contains the five arcs which unquestionably deserve the preference, both as they are the longest that have been measured, and likewise on account of the excellent instruments employed, and the great care taken in executing all the operations. But even of these there is one, namely the Swedish arc, measured by Svuanberg, to which some objections are made; because it makes the length of a degree in latitude 66° 20′, more than 200 toises less than it had been found to be by Maupertuis and the French Academicians who accompanied him. I shall therefore leave out this arc in determining the elements of the elliptical figure of the earth; but, these elements being found by means of the other four arcs, we may then apply them to the case omitted, both

both as some test of their own exactness, and likewise in order to judge what real ground there is for objecting to the Swedish measurement.

| Country. | $\lambda$ | $\lambda'$ | $\varphi$ fathoms. |
|---|---|---|---|
| Peru.......... | −0° 2′ 31″ | 3° 4′ 32″ | 188510 |
| India.......... | 8 9 38·39 | 18 3 23·6 | 598630 |
| France........ | 38 39 56·11 | 51 2 9·2 | 751567 |
| England...... | 50 37 5·27 | 53 27 29·89 | 172751 |
| Sweden....... | 65 31 30·27 | 67 8 49·55 | 98870 |

Applying now the data of the four first measurements in the table to the formula (A), the four equations following will be obtained,

$$\left.\begin{aligned}188510 &= \Delta\ (3{\cdot}11750 - 6{\cdot}2261\,.\,\varepsilon + 3{\cdot}095\,.\,\varepsilon^2)\\ 598630 &= \Delta\ (9{\cdot}89589 - 18{\cdot}1985\,.\,\varepsilon + 6{\cdot}163\,.\,\varepsilon^2)\\ 751567 &= \Delta(12{\cdot}37030 - 6{\cdot}2811\,.\,\varepsilon - 10{\cdot}266\,.\,\varepsilon^2)\\ 172751 &= \Delta\ (2{\cdot}84017 - 0{\cdot}3844\,.\,\varepsilon - 2{\cdot}166\,.\,\varepsilon^2).\end{aligned}\right\}\ \text{(B)}$$

It is known that $\varepsilon$ is not much different from $\frac{1}{300}$, and certainly does not surpass $\frac{1}{150}$. We may therefore assume,

$$\varepsilon = \frac{1}{300} + t; \quad \varepsilon^2 = \frac{1}{90000} + \frac{2t}{300};$$

the correction $t$ being a small fraction which we are sure is less than $\frac{1}{300}$. The foregoing equations will now become, by substitution,

$$\begin{aligned}188510 &= \Delta\ (3{\cdot}09678 - 6{\cdot}206\,t)\\ 598630 &= \Delta\ (9{\cdot}83530 - 18{\cdot}157\,t)\\ 751567 &= \Delta(12{\cdot}34925 - 6{\cdot}350\,t)\\ 172751 &= \Delta\ (2{\cdot}83887 - 0{\cdot}399\,t).\end{aligned}$$

In order to find a first approximate value of $\Delta$, I add the four equations, neglecting the terms containing $t$: thus

$$1711458 = 28{\cdot}1202\,.\,\Delta\,; \quad \Delta = 60862.$$

Next, put $D = 60862$, $\Delta = D(1 + s)$; then, by substituting in the four last equations, we shall get,

$$\begin{aligned}\frac{35}{D} &= 3{\cdot}097\,s - 6{\cdot}206\,t\\ \frac{35}{D} &= 9{\cdot}835\,s - 18{\cdot}157\,t\\ -\frac{33}{D} &= 12{\cdot}349\,s - 6{\cdot}350\,t\\ -\frac{27}{D} &= 2{\cdot}839\,s - 0{\cdot}399\,t.\end{aligned}$$

These equations are next to be reduced to two, the number of the unknown quantities, by employing the method of the least squares: thus

$$-\frac{31\cdot550}{D} = 267\cdot234\,s - 277\cdot342\,t$$

$$+\frac{632\cdot332}{D} = 277\cdot342\,s - 408\cdot673\,t.$$

And, by solving these equations,

$$Ds = -6; \qquad \Delta = D + Ds = 60856$$

$$t = -\cdot00009, \quad \varepsilon = \frac{1}{300} + t = \cdot00324 = \frac{1}{309}.$$

Returning now to the original equations (B), and substituting the values of $\Delta$ and $\varepsilon$, we shall obtain the errors, $E^{(1)}$, $E^{(2)}$, $E^{(3)}$, $E^{(4)}$, or the quantities that must be added to the given measurements on the left sides of the equations, in order to make both sides exactly equal; as follows,

$$E^{(1)} = -20, \quad E^{(2)} = +10, \quad E^{(3)} = -7, \quad E^{(4)} = +13.$$

The greatest error is in the Peruvian arc, which appears to be too great, as has generally been surmised. But the smallness of the errors proves the consistency of the different measurements; and shows that the four portions of the meridian, although very remote from one another, belong to one and the same elliptical spheroid.

If we now substitute the data of the Swedish measurement in the formula (A), we shall get,

$$98870 + E^{(5)} = \Delta\,(1\cdot622022 + 0\cdot8378\,.\,\varepsilon - 0\cdot022\,.\,\varepsilon^2):$$

and, by substituting the values of $\Delta$ and $\varepsilon$, it will appear that the error $E^{(5)} = +5$. This measurement is therefore very consistent with the other four; and, as it seems liable to no just objection, it adds to the evidence in favour of the elliptical elements that have been found.

The degree of the meridian bisected by the parallel of 45°, is 111115 metres at zero of the thermometer. This result is deduced from the actual measurement of the meridional arc between the parallels of Greenwich and Formentera, and is independent of any assumption about the figure of the earth. To reduce this length to fathoms at the temperature of 62° of Fahr., it must be multiplied by $\frac{39\cdot37079}{72}$, which gives 60758 fathoms. Now, if we put $\lambda = 45° + \frac{1}{2}$, $\lambda' = 45 - \frac{1}{2}$, we shall have, in he formula (A), $n = 1°$, $m = 90°$, and the length of the degree having its middle point in latitude 45°, will be,

$$\Delta\left(1 - \frac{\varepsilon}{2} - \frac{7}{8}\varepsilon^2\right);$$

or,

or, in numbers 60856 (1 — ·001629) = 60757 fathoms, which is only one fathom less than the observed quantity.

There is also another measurement with which we may compare the elliptical elements that have been found. A portion of the parallel to the equator in latitude 45° 43′ 12″, little short of 13° in amplitude, has been measured, and the most probable length of 1° of longitude on that parallel has been fixed at 77865m·75*. The large arc was measured in six portions, the difference of longitude between the extremities of every portion being ascertained by means of signals made by exploding gunpowder. If we compare the six degrees, deduced from the several partial arcs, it must be allowed that their differences are great and irregular†. There must therefore be considerable uncertainty in the arithmetical mean of such irregular quantities; which mean is only three metres less than the most probable value of a degree. The length of a degree deduced from the four partial arcs contained within the territory of France, which together exceed 7° of longitude, is 77885m·75, or 20 metres more than the length deduced from the whole arc of about 13°‡. It is to be observed that the errors of longitude at the intermediate stations accumulate as the arc is extended; so that there is not the same advantage in measuring a large arc of the parallel, as in the case of the meridian; for, in one instance, the total amplitude is affected by the sum of the errors at all the intermediate stations; whereas, in the other, it is affected only by the errors of the two extreme latitudes. If we further add that a great length of the parallel answers to a minute portion of time; in so much that an error of a single second in the amplitude of the large arc, would produce a variation of no small magnitude in the mean degree of longitude; it must be evident that in point of accuracy, we cannot repose the same confidence in a degree of the parallel measured in the manner described, as we can in a degree of the meridian deduced from an arc of nearly the same amplitude.

The radius of the parallel to the equator at the latitude $\lambda$, or the perpendicular drawn to the polar axis, will be found, by the properties of the ellipse, equal to

$$\frac{a \cos \lambda}{\sqrt{1-e^2 \sin^2 \lambda}} = \frac{a \cos \lambda}{\sqrt{1-(2\epsilon-\epsilon^2)\sin^2 \lambda}}:$$

And if this expression be expanded and multiplied by $\frac{1}{p}$, we shall have the length of a degree of the parallel equal to

$$\Delta \cos \lambda \left\{1 + \epsilon \sin^2 \lambda + \epsilon^2 \left(\frac{3 \sin^4 \lambda}{2} - \frac{\sin^2 \lambda}{2}\right)\right\}.$$

* *Conn. des Tems* 1829, p. 291. † Ibid. p. 290. ‡ Ibid. p. 293.



Now,

Now, put $\lambda = 45° 43' 12''$, and a degree of longitude at that latitude, will be equal

$$42487{\cdot}8 \times (1 + {\cdot}001661) = 42557{\cdot}4 \text{ fathoms.}$$

But $77865^{m}{\cdot}75$ reduced to fathoms at 62° of Fahr. is equal to 42578·2 fathoms, which is 20 fathoms more than the calculated quantity. If this discrepancy appear very great, we shall only remark, that it is very small in comparison of the differences of the degrees deduced from the partial arcs, and that it supposes not quite so much as $1''\frac{1}{2}$ of time, for the sum of all the accumulated errors in the amplitude of the large arc. Unless we possessed some means of estimating with tolerable exactness the error really existing in the degree of the parallel, the discrepancy we have found can hardly be deemed sufficient to throw any doubt upon the elliptical elements which agree so well with all the most exact measurements that have been made on the earth's surface.

If the earth be really an elliptical spheroid, there can be little doubt that the ellipticity must be very nearly what we have found it to be. But the solution of the problem would be greatly improved, and the results obtained would be more exact and more certain, if the measurement of the meridian in England were extended as far north as possible. An arc of 8° or 10° north of Dunnose, would be the most valuable addition that in the present state of this research can be made to the data for determining the figure of the earth. Another very profitable addition would be the extension of the Indian arc, already the largest we have except the French measurement; and more especially if it could be extended southward nearer the equator. Supposing the difference between $\lambda$ and $\lambda'$ to be small, it will readily appear that the coefficient of $\varepsilon$ in the formula (A) will be equal to zero, or so small as to render the term insensible, when

$$\text{Cos}\,(\lambda + \lambda') = -\tfrac{1}{3}, \text{ and } \lambda + \lambda' = 109° 28'.$$

Thus if, by means of the formula mentioned, we compute the expression of the length of an arc extending northward from Dunnose about a degree beyond Portsoy, to latitude 58° 55′ 30″, it will be found that the length of this large arc is almost independent of the ellipticity $\varepsilon$, and equal to an arc of the equator. The reason is that, in latitude 54° 44′, the radius of curvature of the meridian is equal to the radius of the equator, and the degree of the meridian having its middle point in that latitude is equal to $\Delta$, that is, to a degree of the equator. Thus the length of the portion of the meridian contained between the extreme north and south points of Britain depends chiefly on $\Delta$, and very little on the other element $\varepsilon$; at the equator, the compression $\varepsilon$ has greater influence on the length of a meridional arc, than at any other position on the surface of the globe; and it follows that two arcs of considerable extent, in the situations mentioned, combined with other good measurements in our possession, must lead to a very exact determination of the elliptical elements of the figure of the earth. One remark it seems important to add; namely, that the ellipticity cannot be determined with any precision by combining different degrees of the meridian measured in England; because the element sought has little influence on the lengths of such degrees. And this observation may be extended to all latitudes within 15° or 20° of 54° 44′ on either side; within which limits the ellipticity contributes only an inconsiderable share to the length of a meridional arc. And what has now been said will serve to explain the discrepancy of the ellipticities deduced by combining different degrees of the meridian within the boundaries of England and France; and likewise the great variation between the quantities so obtained, and what is found by the comparison of remote degrees. These observations may be illustrated by examining the equations (B). In the English and Swedish measurements, the coefficients of $\varepsilon$ are small, and have opposite signs. The coefficients of the same quantity are nearly equal in the French and Peruvian arcs, although the former has more than four times the length of the latter. In the Indian and French measurements, the coefficients are as 3 to 1, while the arcs themselves are as 3 to 4; and this shows how important it is, for the exact determination of the ellipticity, to have a large arc near the equator.

April 12, 1828. J. Ivory.

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## LV. *Mineralogical and Chemical Examination of the Diopside of Fassa in the Tyrol. By Dr.* H. Wackenroder, *Professor of Chemistry at Göttingen*.*

AMONG the various new minerals discovered by Bonvoisin† during a journey through Piedmont, there were two which this mineralogist considered as distinct species of minerals; and he called them, after the places in which they were found, *Mussite* and *Alalite*. According to an account given by Tonnellier ‡, Haüy found in the crystalline form of these two minerals such a degree of conformity as induced him to

* Communicated by the Author.
† *Journ. de Phys.* par Delametherie, tom. lxii. p. 418, 423.
‡ *Journ. des Mines,* tom. xx. p. 65.

consider

consider them to be one and the same mineral, and which, from its peculiar crystallographic characters, he presented as a particular species, of the name of *Diopside**. Soon afterwards, however, Haüy himself published a memoir, in which, after a further investigation of this mineral, he proved, from crystallographical reasons, that diopside coincides with pyroxene†. He also found diopside to agree with pyroxene in its physical characters‡.

Having found the specific gravity of the mussite to be 3·2374, that of the alalite 3·31, and that of pyroxene from Vesuvius 3·3578, he says that the specific gravity of diopside lies within the limits of that of pyroxene, as indeed the remaining external characters of the mineral substances hitherto considered to belong to pyroxene, sometimes differed more from each other than from diopside.

Other mineralogists have grouped diopside, according to their different views of the systematic arrangement of minerals, with different species, comprehended under augite or pyroxene. Thus Hausmann § enumerates diopside among the formation of the malacolites in the pentaklasite substance; Leonhard ‖ quotes it as a variety of augite; Mohs ¶ refers it, together with the coccolite, fassaite, augite, sahlite, baikalite, malacolite, &c. to the paratomous augite-spar, &c. Independent of these well-known classifications of diopside in the different mineralogical systems, it must form, in a system founded on the relation of the external characters to the chemical composition of the minerals, one of the divisions which, in consequence of the experiments made on the substituted constituents, must now be made in the species of pyroxene and augite. It is in consequence of this that, according to the latest system of Counsellor Hausmann, the diopside is considered as an augitic substance, and as represented by the formula

$$CS^2 + MS^2.$$

To this formation also belongs the diopside which I lately bought of a dealer under the name of *Diopside of Fassa*, and which I subjected to a strict examination, as we do not possess, to my knowledge, either a chemical analysis or a detailed description of the diopside of the valley of Fassa.

* *Journ. des Mines*, tom. xx.

† "Sur l'Analogie du Diopside avec le Pyroxène;" in the *Ann. du Mus. d'Hist. Nat.* tom. xi. p. 77. *Journ. des Mines*, tom. xxiii. p. 145.

‡ *Journ. des Mines*, tom. xxiii. p. 154, 156.

§ *Hanbuch der Mineralogie.* Band ii. p. 694.

‖ In his *Hanbuch der Oryktognosie.* New edit. p. 503.

¶ In his *Grundriss der Mineralogie.* 2d vol. p. 306.

I. *Mineralo-*

## I. *Mineralogical and Chemical Examination of the Diopside of the Valley of Fassa.*

This diopside, in the state I received it, consisted of single loose crystals, which appeared to have been attached. They usually are a couple of inches long, and are mostly compressed, so as to appear flat. They are generally free from extraneous substances, and I saw only a few crystals covered on their surface with laminæ of chlorite. But they are found more frequently covered with a gray tarnish or bloom resembling iron, which is especially perceptible near the ends, and seems to have at times come out from the mineral itself, in which case it also seems more intimately connected with it.

I am indebted to the obliging kindness of my respected teacher Counsellor Hausmann, for the following crystallographical communication on the diopside of the valley of Fassa.

" The crystals of diopside, usually broken at the ends, present themselves most frequently as rectangular prisms, the greater sides of which are more or less striated longitudinally, whilst the other sides are either smooth or slightly striated.

" The appearance of planes replacing the lateral edges explains the nature of the surface of these rectangular prisms. For the planes agreeing with the planes E of the prism of augite, which, according to Haüy, are respectively inclined at 92° 18′ and 87° 42′, the narrower planes of the sides which form with the replacing planes an angle of 136° 9′, present themselves as the planes B′, and the broad ones as the planes B.

" Sometimes the planes BB′ 3 ($f$) appear perfectly formed, making with each other angles of 141° 44′ and 38° 16′. A disposition to the formation of these planes is distinctly discovered in the deep striation of the planes B.

" The observed combinations are:

2 B′. 2 B
2 B′. 2 B. 4 E.
2 B′. 4 BB′3.
2 B′. 2 B. 4 BB′3
2 B′. 4 E. 4 BB′3.

" The respective inclinations of these planes are, the angles given by Haüy being taken as the basis,

| | | |
|---|---|---|
| B′ — B = | 90° | 0′ |
| B′ — E = | 136 | 9 |
| B — E = | 133 | 51 |
| B′ — $f$ = | 109 | 8 |
| B — $f$ = | 160 | 52 |
| E — $f$ = | 152 | 59 |
| $f$ — $f$ = | 141 | 44 |

Sometimes

"Sometimes a combination of several individual crystals is seen. One end of the crystals is always fractured, while the other often shows a tendency to a summit.

"Besides the marked cleavages of E′, those of B′ and B are also perceptible. Besides the cleavages, we perceive fissures; also transverse fissures, which are sometimes irregular and sometimes corresponding to the planes D′."

The fracture of diopside is imperfectly conchoidal and splintery. The angular pieces which are detached from it are frequently tabular and prismatic.

Its colour is a pistachio-green or olive, but sometimes runs in the same prism from greenish white to colourless.

The crystals are usually translucent only, sometimes transparent. Small detached laminæ can always be seen through, and are slightly coloured. The fissures in the interior of the crystals sometimes produce a play of colours.

I have not been able to perceive a double refraction in these crystals. The external planes as well as the cleavage-planes show a glassy and pearly lustre. It scratches fluor spar, and is scratched by felspar.

According to Brewster, diopside will show signs of electricity; but neither by heating nor by rubbing it, have I been able to discover any electricity in this variety. As little does this diopside act on the magnet. Its specific gravity, at 59° Fahr. is = 3·299. A piece strongly striated not belonging to the purest portion, gave at the same temperature a specific gravity of 3·296; and one almost white, covered at one end with the above-mentioned bloom, a specific gravity of 3·277. Heated in a platinum spoon, the diopside of Fassa loses its peculiar green colour, but suffers no further change.

In the flame of the blowpipe, sharp splinters of it are rounded at the edges, but by means only of a very strong heat; while thick pieces only become of a dirty green colour. With borax it is dissolved very slowly and without colouring the bead.

Carbonate of soda at a red heat dissolves it with ebullition, and gives a bead of a reddish colour. Phosphate of soda mixed with the mineral in powder, and melted before the blowpipe, yields a dark yellowish bead. On charcoal with nitre it yields a greenish mass, which by further heating assumes a dirty reddish colour. Concentrated sulphuric acid, muriatic acid, or nitric acid, does not dissolve the diopside.

## II. *Preliminary analytical Experiments.*

A certain quantity of pulverized diopside ignited with thrice the quantity of carbonate of soda freed from water, gave a half-melted mass, which was dissolved in water and muriatic acid.

acid. The solution having been evaporated to dryness, and the residuum digested with water and muriatic acid, a large quantity of silica remained undissolved.

Carbonate of soda precipitated from the solution when cold some oxide of iron with a few traces of alumina; but oxalate of potash precipitated much oxalate of lime, which was partly ignited and partly mixed with caustic ammonia. A very small quantity of deutoxide of manganese was found mixed with the lime. After this, carbonate of magnesia together with a little manganese was precipitated, by means of carbonate of soda, from the hot solution; and subsequently, by means of triple phosphate of soda and ammonia, a considerable quantity of magnesia.

Again, about 2·0 grammes of pulverized diopside were fused with carbonate of soda and 1-6th of its weight of nitre. Water gave with the melted mass a greenish solution, which soon completely lost its colour in the air; and on being tested for the chromic, fluoric, phosphoric and boracic acids, no trace of either was perceptible.

III. *Analysis for the Proportions of the Constituents.*

*a.* 2·502 grammes of select and very pure pieces of diopside dried on a hot iron plate, were exposed for three quarters of an hour to a red heat. But the mineral was unaltered in appearance and weight, except that its colour turned nearer bottle green.

*b.* 2·503 grammes of the diopside finely powdered, were intimately mixed with thrice their weight of carbonate of soda freed from water and exposed for half an hour to a moderate red heat. The fused mass was dissolved in a moderate quantity of water, placed in an evaporating-dish, and diluted with as much muriatic acid as was necessary to effect a perfect solution. By the evaporation to dryness of the yellowish solution, and by digesting the residuum (which was of the same colour) in water and some muriatic acid, pure white silica was obtained, weighing, after having been dried and heated, 1·3581 gramme. It was completely dissolved in a solution of caustic potash, as well as in a concentrated solution of carbonate of soda assisted by heat; and after having stood for some time, only a few unimportant flakes were deposited from this solution. A part of it only was dissolved in caustic ammonia, but the remainder was entirely taken up by carbonate of soda. It will be worthy of a distinct investigation to ascertain the cause of this phænomenon *.

* Dr. C.F.B. Karsten has also mentioned the facility with which silica is dissolved in caustic ammonia. See Poggendorff's *Annalen*, Band vi. p. 357.

 *c.* The

*c.* The solution having been copiously diluted, was mixed first, in the cold, with carbonate, and then with bicarbonate of soda, in order to separate the oxide of iron and alumina. The precipitate was then slightly heated, and again dissolved in concentrated muriatic acid. There remained a small quantity of silica, which after having been heated, weighed 0·002 gramme, and remained unaffected even after a continued digestion in sulphuric acid.

*d.* The solution of iron in (*c*) was abundantly mixed with caustic potash, and the precipitated hydrate of iron separated and slightly heated. The 0·0701 gramme of pure oxide of iron calculated for protoxide, in which state the iron evidently exists in the mineral, give 0·6029 gramme of protoxide, if the equivalents of the two oxides bear the proportion to each other of 39 to 35.

*e.* Carbonate of ammonia, with heat, precipitated from the solution (*d*), which had been saturated with muriatic acid, the alumina contained in the mineral; which, after the burning of the filtre that had been used to separate it, and after deducting the ashes of the latter, consisted of no more than 0·005 gramme.

*f.* In order to separate the manganese from the liquid (*e*) which had been treated with carbonate of soda, I made use of the method lately pointed out by Counsellor Stromeyer*. Accordingly the solution, in consequence of the small quantity of manganese it contained, was first reduced by boiling to a smaller bulk, saturated with chlorine gas, and then mixed with as much neutral carbonate of soda as was necessary to precipitate the deutoxide of manganese, and render the solution colourless. The precipitate having been dried off for forty-eight hours, and then collected on a filtre and heated, amounted to 0·0027 gramme of deutoxide of manganese, which, according to the equivalents, 39·17 for the deutoxide, and 36·5 for the oxide, indicate 0·0025 gramme of the latter, it being requisite to assume that the manganese is in this state of oxidation in the mineral.

*g.* The solution having been again concentrated by boiling, it was mixed with as much oxalate of potash as was necessary to precipitate the lime; and the precipitate separated as soon as possible by filtering, its separation having been promoted by a small addition of oxalic acid. The oxalate of lime obtained was made red hot, in order to destroy its acid, again dissolved in nitric acid; and the solution mixed with caustic ammonia freed from carbonic acid, gave a slight pre-

* *Götting. Gelehrte Anzeigen.* St. 158. October 1827.

cipitate. This when made red-hot amounted to 0·0024 gramme, and consisted of deutoxide of manganese, which reduced to oxide, according to the proportion of these two oxides assumed above, is equal to 0·0022 gramme of oxide of manganese.

The reason of this small quantity of manganese yet being found, is the facility with which, on its being separated from solutions by means of chlorine, a small portion of it is redissolved, when the precipitate is left in contact with the fluid beyond the time necessary for its formation, during which a little muriatic acid may be produced.

*h.* The nitric solution of lime was now, while boiling, mixed with caustic ammonia and carbonate of ammonia. The precipitated carbonate of lime gave, after heating, 0·6064 gramme of pure caustic lime, which was completely soluble in greatly diluted nitric acid, and with little effervescence.

As in precipitating the lime with carbonate of ammonia, especially without a sufficient addition of caustic ammonia, small quantities may easily be left suspended, and the carbonate of lime not being quite insoluble in the water used for washing it;—the solution having been much boiled down, was again tested with oxalate of potash, and by means of it a little more oxalate of lime was precipitated, but which, when made red-hot, yielded only 0·0150 gramme of caustic lime. Consequently the entire quantity of pure lime separated amounts to 0·6064 + 0·0150 = 0·6214 gramme.

*i.* In order to separate the magnesia from the solution (*g*), the former was acidulated with muriatic acid, concentrated by boiling, and mixed with phosphate of soda and caustic ammonia. At the expiration of about twenty-four hours the crystalline deposit was collected and heated: it weighed 1·1790 gramme.

After a second similar treatment, rendered necessary by the easy solubility of the substance in a large quantity of water, even if the latter should be mixed with some caustic ammonia, which may perhaps itself promote the solubility by the access of carbonic acid from the atmosphere,—another precipitate was obtained weighing, after being heated, 0·0582 gramme. Consequently the whole of the phosphate of magnesia obtained amounts to 1·2372 gramme; in which, if 100 parts of this salt, heated, indicate 37 parts of magnesia*, 0·4577 gramme of the earth are contained.

* See the above-mentioned Essay of M. Stromeyer.

 Conse-

Consequently, 2·503 grammes of the diopside of Fassa are composed of

| | | | |
|---|---|---|---|
| Silica . . . . . . . . . . . . . . | (*b*) | 1·3581 | 1·3601 gramme. |
| | (*c*) | 0·0020 | |
| Lime . . . . . . . . . . . . . . | (*h*) | . . . . . | 0·6214 |
| Magnesia . . . . . . . . . . . | (*i*) | . . . . . | 0·4577 |
| Protoxide of iron . . . . | (*d*) | . . . . . | 0·0629 |
| Oxide of manganese . . | (*f*) | 0·0025 | 0·0047 |
| | (*g*) | 0·0022 | |
| Alumina . . . . . . . . . . . | (*e*) | . . . . . | 0·0050 |
| | | | 2·5118 grammes. |

Accordingly, 100 parts of this diopside contain,

| | | Oxygen. | |
|---|---|---|---|
| Silica . . . . . . . . . . . . . . | 54·154 | 27·07 | |
| Lime . . . . . . . . . . . . . . | 24·740 | 6·94 | 14·22 |
| Magnesia . . . . . . . . . . . | 18·222 | 7·28 | |
| Protoxide of iron . . . . | 2·504 | 0·54 | |
| Oxide of manganese . . | 0·183 | 0·05 | 0·68 |
| Alumina . . . . . . . . . . . | 0·195 | 0·09 | |
| | 100·001 | | |

If as the essential constituents of the diopside we take only silica, lime and magnesia, the composition of this mineral is to be expressed by two single bisilicates of lime and magnesia. But this expression presupposes 2·74 per cent more silica than the analysis has furnished; which difference, however, will be reduced by one per cent, if we assume in the magnesia 38·71 per cent of oxygen; consequently assume in the quantity of magnesia found 7·05 parts of oxygen, instead of putting 20 as its equivalent, as we have done in conformity with Gmelin's latest *Handbuch der Chemie*, whose statement of equivalents has been followed throughout in this calculation.

If we wish yet to take into consideration, that, if the proportion of magnesia should be diminished by so much as was precipitated in the analysis (*i*), but the omission of which may have been assumed by chemists for the calculation of the calcined magnesia, the magnesia first obtained would then contain very nearly as much oxygen as the lime: we have exactly such quantities of the principal constituents of the diopside, that the latter may be exactly expressed, in a chemical point of view, by the symbols,

$$(CaO + 2SiO) + (MgO + 2SiO);$$

and in a mineralogical one by

$$CS^2 + MS^2.$$

The

The composition of the mussite indeed, according to Laugier*, differs considerably from that of our diopside: we might, however, presume that this difference partly arises from the less perfect state of chemistry with respect to those substances in Laugier's time. It is possible, too, that the same circumstance has produced the smaller difference from the composition of our mineral which appears in the analysis of the malacolite or sahlite examined by Vauquelin†.

Among the other existing analyses of minerals referred to pyroxene or augite, according to which other formations of augite-substance are proved, we must consider, with respect to the diopside of Fassa, several examinations made by Von Trolle Wachtmeister Hisinger, Von Bonsdorf, and H. Rose. From the comparison of the results of these analyses enumerated in the well-known treatise of Professor Rose, "On the Pyroxene‡," with those of the analyses just-mentioned of Laugier and Vauquelin, and with those obtained by me on the composition of the diopside of Fassa, for which purpose I have prepared the following table; it seems that all the minerals thus examined, may be referred to the diopside, and their composition expressed by the formula $CS^3+MS^2$.

| Analyses. | Silica. | Lime. | Magnesia. | Oxide of Iron. | Ox. of Manganese. | Alumina. | Loss by Heat. | Total. |
|---|---|---|---|---|---|---|---|---|
| *Laugier.*—Mussite of Piedmont | 57·5 | 16·5 | 18·25 | 6 with ox. ma. | ...... | ... | ... | 98·25 |
| *Von Trolle Wachtmeister.* — White malacolite from the TafelTjötten, in Norway... | 57·40 | 23·10 | 16·74 | 0·20 protox. | ...... | 0·43 | ... | 97·87 |
| *H. Rose.*—Yellowish malacolite from Längbanshyttan in Wärmland .............. | 55·32 | 23·01 | 16·99 | 2·16 | 1·59 | ... | ... | 99·07 |
| *Hisinger.* — Malacolite from Längbanshyttan in Wärmland .......................... | 54·18 | 22·72 | 17·81 | 2·18 | 1·45 | ... | 1·20 | 99·54 |
| *H. Rose.* — Greenish sahlite from Sahla ................. | 54·86 | 23·57 | 16·49 | 4·44 protox. | 0·42 | 0·21 | ... | 99·99 |
| *Von Bonsdorf.* — Perfectly white malacolite from Tammare in Finland ........... | 54·83 | 24·76 | 18·55 | 0·99 | ...... | 0·28 | 0·32 | 99·73 |
| *H. Rose.*—White malacolite from Orrajerwi in Finland | 54·64 | 24·94 | 18·00 | 1·08 | 2·00 mang· | ... | ... | 100·66 |
| *H. Wackenroder.* — Diopside from Fassa in the Tyrol ... | 54·154 | 24·740 | 18·222 | 2·505 protox. | 0·183 | 0·198 | ... | 100·001 |
| *Vauquelin.* — Malacolite or sahlite ...................... | 53·00 | 20·00 | 19·00 | 4·00 with ox. mangan. | ...... | 3·00 | ... | 99·00 |

* *Ann. du Mus. d'Hist. Nat.* tom. xi. p. 153.

† Haüy, *Traité de Mineralogie,* 1st edit. vol. iv. p. 382.

‡ Schweigger's *Journ. f. Chem. u. Phys.* Band xxxv. p. 86. &c.

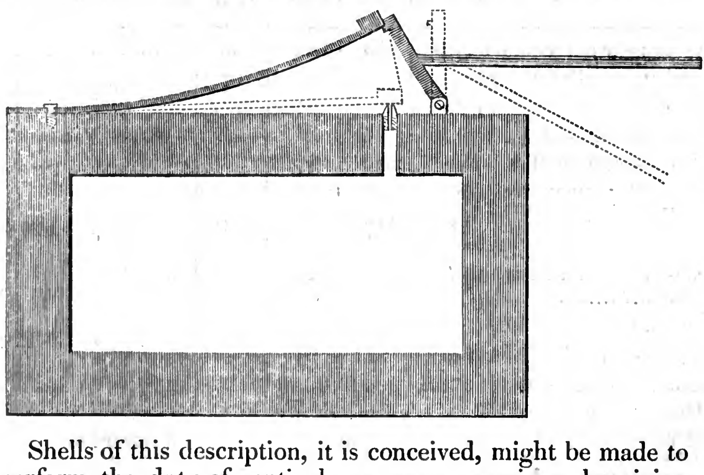
LVI. *De-*

## LVI. *Description of a Shell, exploding by Percussion when trod upon. By Lieut.-Col.* Miller, *F.R.S.**

THIS is a very simple contrivance, and will easily be understood from the figure. A spring is attached to the upper part of the shell, which produces ignition by falling on a copper cap; and a support is placed under the spring, moving on a hinge, with a handle attached to the support; so that by treading on the handle, the support is withdrawn, the spring falls, and explosion follows. The sides of the shell must be of equal thickness, for the better splintering; and as an oval form will be the most convenient for fixing the spring. The following construction is given:

| | |
|---|---|
| Length of shell............... | 8 inches. |
| Breadth of ditto ............. | 5·5 |
| Depth of ditto ............... | 5 |
| Length of chamber ......... | 6 |
| Width of ditto ............... | 3·5 |
| Height of ditto ............... | 3 |
| Length of spring ........... | 6 |
| Fall of ditto .................. | 1 |
| Length of handle from 10 to 30. | |

*Vertical section of shell when cocked, taken lengthwise.*



Shells of this description, it is conceived, might be made to perform the duty of sentinels on many occasions, by giving notice of the approach of an enemy, and presenting a considerable obstacle to his advance by their explosion. They might accordingly be used with advantage in the ditches of fortresses,

* Communicated by the Author.

fortresses, before breaches, and to defend bridges and passes, wherever an enemy is likely to attempt a surprise. They might also be placed around field works likely to be attempted by assault. They would require to be sunk a little in the ground, so that the splinters of one might not derange those near to it, and covered lightly over to protect them from wet, and also to conceal their position from an enemy.

This principle might also be applied to the firing of artillery by percussion; more particularly at sea, where the roll of the vessel presents so great an obstacle to accuracy of fire. In that case, the vent of the gun would require to be placed a little on one side, to be clear of the line of sight, and a hole drilled through the spring opposite the vent, to allow the flash from it to escape. The support of the spring would, of course, be pulled away by a string, so that the man who laid the gun might also fire it.

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## LVII. *Account of the Iron Mine at Haytor, in Devonshire. By* J. T. Kingston, *Esq.*

*To the Editors of the Philosophical Magazine and Annals.*

Gentlemen,

A MINERAL production discovered at the Haytor Iron Mine having formed the subject of two or three papers in your valuable Journal*, you may perhaps consider a description of the mine itself not unworthy of occupying a page or two of your next Number; especially as it is, at least to the best of my knowledge, the only one of the kind hitherto discovered in this island, and as such, of some importance in a geological as well as in an œconomical point of view†.

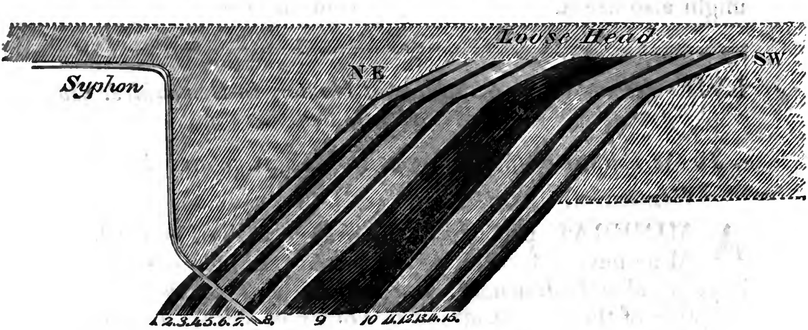
The lode, to the depth at present explored, is a very regularly stratified one, of oxidulated iron ore and argillaceous schist, in alternate beds; and is situated on the edge of the granite district, near the base of the Haytor rocks. The hill, on the brow of which, near the centre, it crops out, is immediately incumbent on the granite; its principal slope is gradual and towards the East, the sides having a more precipitous descent to the North and South. It consists chiefly of micaceous passing into clay schist, and of trap‡ (provincially termed

* See Phil. Mag. and Annals, N.S. vol. i. p. 38, 40, 43.

† Some particulars of the vein of ore worked in this mine, will be found in Mr. W. Phillips's paper on Haytorite, which is the first of the communications just referred-to.—Edit.

‡ Mr. W. Phillips, we observe, has stated that this substance appears to be siliceous schist.—Edit.

termed Ironstone) of a compact basaltic texture, and great specific gravity; and mostly containing a proportion of iron-ore, which occasionally runs in distinct threads and patches through small portions of it. The lode occurs in the clay schist, and the direction of its strata is nearly North-west and South-east, underlying to the North-east at an angle of 22° or 23° only for the first few feet from the surface; but below this the dip is very regular at an angle of 45°. It happens to be situated in two distinct estates, which a road, intersecting the lode near the centre, divides; and having hitherto been worked separately, a complete wall from the surface to the depth explored, has been left, of which the following is an accurate section and measurement; the measurement is taken at the depth indicated by the dotted line, which is also the direction of the level driven from the back of the lode.




| Thickness of Beds. | | |
|---|---|---|
| No. | Ore. | Schist. |
| 1 | 1ft. 0in. | |
| 2 | | 0ft. 6in. |
| 3 | 1 3 | |
| 4 | | 1 3 |
| 5 | 1 3 | |
| 6 | | 2 0 |
| 7 | 0 6 | |
| 8 | | 2 6 |
| 9 | 8 0 | |
| 10 | | 3 6 |
| 11 | 2 0 | |
| 12 | | 1 3 |
| 13 | 0 6 | |
| 14 | | 1 0 |
| 15 | 1 6 | |

16 feet ore.
12 feet schist.

28 feet whole width of lode.

By

By this it will be seen, that the central bed is of iron-ore, and considerably the largest; whilst the other alternating beds of schist and ore are disposed above and below it in a tolerably regular relative proportion. The schist having a tendency to contract in width, as the depth increases, and the ore to approximate each way towards the central bed, into which, the obvious probability is, that the other beds, at a greater depth, run; but whether this is the case, or whether, if they do so, the central bed is proportionally increased, must of course remain uncertain, until a level is driven, which I understand it is proposed to drive from the northern ravine, and which would cut the lode at about 150 feet perpendicularly below the surface. Permanent springs rise at the depth of a few feet, the water from which is carried off by a syphon bent over the northern slope of the hill. A level driven from the back of the lode, at the depth of about 20 feet from the surface, through schist, in a south-west direction, intersected, about 30 feet from its commencement, another small bed of ore, about 3½ feet in width, in which a large proportion of iron-pyrites is disseminated, the dip being the same as that of the principal lode, with which, however, it does not seem to be connected; the latter being included in a well-defined manner, within the limits pointed out by the above section, from which the width will be seen to be about 28 feet. The length, of course, is not capable of being so accurately defined: from the part where the section was taken on the western side, the traces are observable on to the granite, a distance of 230 yards, which they do not enter, but are conformable along its edge for some distance in a northerly direction; and on the eastern or lower side, it has been traced for the distance of 250 yards, down to a compact stratum of trap*, blended with garnet and actinolite, against which the traces appear to have been hove in a zigzag curved manner, and to dip under it. The length however of the beds containing ore of a quality available for smelting, does not (at least to the depth at present excavated) exceed 150 yards.

The surface along the whole line of the lode, as well as on each side of it, is apparently regular and undisturbed, and consists of a loose head of flat schistose stones and fragments, and the earthy mould arising from their decomposition, to the depth of five or six feet. In this head, at either end, along the line of the lode, bunches of manganese contaminated by iron occur. Near the central part of the lode an old sinking was discovered to the depth of 42 feet on one single bed of the ore.

* See former note on this substance.—Edit.

ore. There remain no records in the neighbourhood, as to when or for what purpose this excavation was made; but the probability is, that it was mistaken for tin, old stream-works for obtaining which abound in the neighbourhood. Only a small portion (if any) appears to have been carried off; as the chief part was left in heaps, and strewed on the surface to the extent of two or three acres immediately round, and which in fact led to the discovery of the mine, and remained unaccounted for some time after.

The chief part of the ore is of a compact texture, but portions of it, especially on approaching the surface, are coarsely granular; more or less perfectly formed crystals, loosely aggregated, are also frequent. The per-centage of iron it contains varies from 40 to 70 (the average probably of what has hitherto been worked yielding in the large way about 50); some of the richer specimens are actively magnetic; when pulverized, the ore is brownish black, and passively magnetic. It occurs also mixed with sulphur and with arsenic, in coarsely granular masses. Spathose carbonate of iron and also iron-pyrites are met with; the latter, either in decomposing granular concretions, radiated, in more or less perfect cubic crystals, and in small spangles disseminated through the coarser granular ore. Copper-pyrites and arsenical pyrites also occur, the former very sparingly.

The other mineral specimens discovered in the faults and cavities of the lode, and in the loose head immediately above it, having excited considerable local attention, I shall (with your permission) give as complete an enumeration of them as I am able, though a minute and lengthy description would be equally tedious and unnecessary; as they are chiefly aqueous depositions of siliceous and aluminous matter, coloured by the oxides of iron and manganese, and do not essentially differ from other formations of the same kind of frequent occurrence in other mining districts; for as to the statement, that precious gems, &c. had been discovered there, that being now admitted to have originated in a mistake, no further notice need be taken of it.

*Quartz*—Occurs in a granular form, in corroded amorphous masses, in thin plates crossing each other at right angles (cellular quartz), stalactitic and crystallized. The crystals are all secondary, and mostly hexagonal prisms, terminated by an hexagonal pyramid; the faces generally unequal. In some the pyramid is nearly triangular, the alternate angles being deeply replaced by planes. In some the edges, either of the prism, or pyramid, or both, are bevelled; occasionally very regular prisms, terminated by a pyramid at each end, are met with,

with, and in some specimens the prism is so shortened that the pyramids appear to be nearly joined base to base. Columnar hexagonal prisms of opaque quartz are of frequent occurrence, forming the nucleus, round which the other crystals are often clustered, or stalactitic chalcedony deposited. Of the transparent crystals, some are colourless; of the rest, the colours are ferruginous (this most common), clear topaz yellow, dark red passing into black; smoky brown (cairngorm); various shades of purple, violet, or rose-pink (amethystine quartz), and bright ruby red: these last are minute and very perfectly formed crystals, lining cellular quartz. The finest specimens of the amethystine quartz occur (very sparingly) in geodes, in the loose head; the rest are found clustered in rounded, reniform, or radiated masses, lining cavities, and investing and invested by chalcedony.

*Flint*—In amorphous masses, and passing into chalcedony, &c.; the colours mostly ferruginous, or brownish black.

*Chalcedony*—Passing into and also investing crystallized quartz; stalactitic (this occasionally incloses distinct globules of water), mamillated, botryoidal, and lenticular. The prevailing colours, various shades of ferruginous brown passing almost into black; but occasionally, specimens are met with of clear yellow; pure milk-white, passing into pearly and translucent white; cærulean, passing into peach-colour, and light gray; and fine plum-colour (the last from a bunch of manganese). Frequently two or three successive coatings, sometimes of the same, sometimes of distinct colours, invest each other, between each of which a thin lining of clay is often interposed; some of the white and cærulean varieties are hydrophanous; others have a very delicate coating of aluminous matter, which causes their colour to deepen considerably on the application of moisture. Pseudo-morphous crystals of this substance also occur, formed within cavities from which other crystallized substances have been by some natural cause, (probably solution,) removed. Irregular masses of siliceous matter that have obviously invested,either wholly or partially, crystals of iron-ore. Iron-pyrites, quartz or garnet, are of frequent occurrence. I have specimens of the substance in moulds of this kind, and conceive that much of (if not all) the Haytorite (as those crystals have been named) is referable to some of the different forms of the above substances, and is chalcedonic matter filling the said cavities by stalagmitic deposition; some of the faces of the crystals are very splendent, others rough; in some of the crystals all are rough: the fracture is different from that of a true crystal, nor have I ever succeeded in obtaining a regular cleavage; the crystals are of various sizes;

 the

the sides of the planes in some exceeding an inch, in others scarcely 1-8th of an inch. They are mostly found in a matrix of ferruginous clay, in juxta-position with massive chalcedony, quartz, and specular iron.

*Semi-opal*—Mostly in irregularly rounded nodules, invested by indurated clay (lithomarga). The colours, various shades of blue, green, or yellow, occasionally all passing into each other in the same specimen, or forming distinct stripes. It also frequently alternates in stripes with the indurated clay.

*Garnet*—Massive, blended with actinolite, &c. in the lode, and especially at the eastern end; and crystallized, mostly in rhomboidal dodecahedrons, and their modifications. The colours, various shades of yellowish or reddish brown, passing into black minute crystals, occur (rarely) on the surface of the ore under glassy actinolite. Most of the crystals are coarse, and of inferior size.

*Actinolite*—Intimately blended with the ore, in amorphous masses, and in distinctly radiated clusters, the radii from 1-8th of an inch to an inch in length: the colours varying from a light to a very dark green. A thin variety, with a vitreous lustre, occurs sparingly on the surface of the ore. Actinolite is very abundant, and gives a greenish hue to a large portion of the ore. It is most prevalent in the upper and under beds, and seems to increase with the depth.

Some fine cabinet-specimens of all the above species have been found; an interesting series of which (the most complete I believe that has been formed) is in the possession of Shirley Woolmer, Esq. of Exeter.

The ore, when melted, produces iron of a very tough and superior description, as I have myself proved by some experiments. It is also stated, on good authority, to be capable of conversion into excellent steel; but the purpose to which it has hitherto been applied is that of mixing with the argillaceous ironstone of the Welch coal-measures, to improve its quality; for which, I understand, it is preferable to the hematitic ores of Lancashire; and this, I should suppose, will be the purpose that it will ultimately be found most practically available for; as its distance from proper fuel (the Bovey coal, situated within three or four miles of it, from some late experiments that have been made, giving very little hope of being efficient for the purpose of smelting, and the Dartmoor peat being at too great a distance) would probably be an effectual obstacle to the erection of any works in the neighbourbood, even supposing a less limited quantity of ore to exist, than at present appears to be the case. But this is a part of the subject on which I am incompetent to treat, and it is besides foreign to the object

object of this communication, which is to illustrate a small portion of a district, that in the circle of a few miles (taking the large clay-deposit of Bovey-heath as a centre) is perhaps, in a geological point of view, one of the most interesting districts in the kingdom. And I propose (in conjunction with J. G. Croker, Esq. of Bovey Tracey, who has for many years paid great local attention to the subject), if consistent with the plan of your Journal, to make it the medium of two or three communications (accompanied by sections, &c.), pointing out carefully the different strata, their extent and junction, and the lodes, as far as explored, that occur in them. To this gentleman, as well as to Mr. Petherick the superintendent of the iron-mine, I am indebted for much valuable assistance in drawing up the present paper. I remain, Gentlemen,

Your most obedient servant,

Ilsington, Devon, April 9, 1828. J. T. KINGSTON.

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## LVIII. *Account of a new Method of mounting Thermometers. By Mr.* W. MAGEOUGH.

*To the Editors of the Philosophical Magazine and Annals.*

Gentlemen,

THE following is an account of a method of mounting the thermometer-tube, in order to make it applicable to purposes for which, as fitted-up at present, it could not be used.

This method has not, that I know of, been hitherto practised in making thermometers; but about two years since, the same principle was successfully applied to barometers.

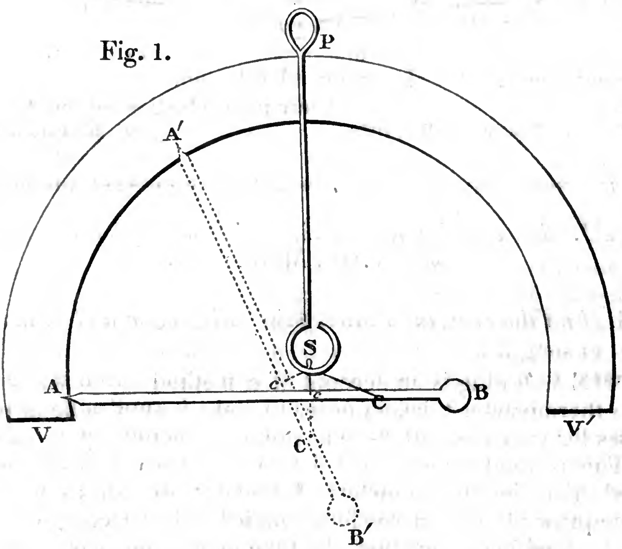
Mounted in this manner, the tube may be made of earthenware or metal, as well as of glass; it may contain metals or other substances which will expand by heat; and by a little management be introduced into, and made to indicate degrees of temperature, that would speedily destroy thermometers of the ordinary construction. The present account relates only to tubes of glass, such as are generally used for making thermometers, without any other condition, than that the size be adapted to the purpose proposed to be effected by the instrument. If large enough, it will move with sufficient force to give notice when the apartment where it is placed attains a given temperature, by touching a light lever connected with the sort of alarum-machinery often put into the cheapest clocks. Or, a prepared paper may be made to pass over a hair-pencil, charged with a liquid not liable to dry or freeze, which pencil, by means of a simple addition to the hour-movement of a common clock, will

will trace on the paper all the changes in temperature occurring through twelve or twenty-four hours.

But, where nothing more is required, the highest and lowest degrees of temperature happening in a given time may be registered by the assistance of a couple of fine slips of light wood, or two bristles, poised on axles, the ends being brought over the circular scale so as to come in contact with a catch fixed on the tube.

Let AB, fig. 1. represent a thermometer tube, containing the mercury, and being marked at the boiling and freezing

Fig. 1.




points. Such tubes are easily obtained at the opticians. Let C be the centre of gravity when the mercury is at the freezing, and *c* the centre of gravity when it is raised to the boiling-point; S is the place of an axle fixed across the tube, and resting in two hooks or rings, one of which is represented in the figure at the termination of the wire PS. Suppose the point S to be in a perpendicular to the axis of the tube, erected at the point *c*; then it is clear that when the mercury rises to the boiling-mark, the centre of gravity moves into *c*, this point will therefore get into the perpendicular to the horizon under S, and put the tube AB into the horizontal position, as shown in the figure. Again, when the mercury sinks to the freezing-mark, the centre of gravity coming into C will cause that point to get into the perpendicular under S, and put the tube into the position A′B′: the arch AA′ through which the end of the tube

tube has moved, measuring an angle equal to 90° less by the angle *c*CS.

If the arch AS and the tube be graduated, the corresponding divisions are easily noted as the mercury rises or sinks, and the divisions can be marked on the former at leisure.

Because in the tubes commonly to be obtained, the distance *c*S, of the point of suspension above the axis of the tube, must be very small, the adjustment is rendered comparatively easy, by making the axle of a piece of thin steel wire bent in the form of fig. 2, the inner part of the curve being as nearly as

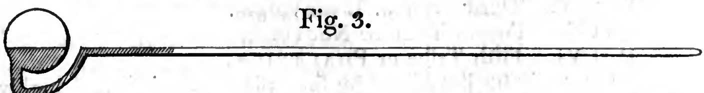
Fig. 2.


possible half the circumference of the tube; the curve being underneath, the axle is fastened to the tube by a thread wound four or five times round both, and they may be secured from slipping by a drop or two of varnish. Now it is evident that by increasing or diminishing the distance between the points of support or bearing hooks, the centre of gravity *c* will be more or less sunk below the points of suspension S, and the distance *c*S will be obtained without the trouble of filing. A knife-edge is however undoubtedly preferable to the wire.

To find the centres of gravity on the tube, it is only necessary to sling it in a horizontal position, by means of a thread fastened to it at each end, and put over a pin: then a plumb-line, hung from the same pin, will cross the tube at the centre of gravity, the place of which should be marked on the glass by a file or diamond. The arch VPV′ may be of pasteboard, wood, or metal, and should be attached to the wires SP. At P is a loop or ring by which the instrument is suspended.

If the figure of the tube be altered, and the bulb contain spirits of wine in the upper part and mercury in the lower, it is evident the distance between the centres of gravity at any two temperatures, and consequently the power of the instrument, will be much increased. The shading in the subjoined figure shows the portion of the bulb and tube to be occupied by the quicksilver when the temperature is low.

Fig. 3.



It may be added that thermometers of this construction might perhaps advantageously accompany wheel-barometers in large apartments or halls. I am, Gentlemen, &c.

Feb. 23, 1828. W. Mageough.

LIX. *No-*

## LIX. *Notices respecting New Books.*

*Part I. of a Descriptive Catalogue of the Lepidopterous Insects contained in the Museum of the Honourable East-India Company, Illustrated by Coloured Figures of New Species and of the Metamorphosis of Indian Lepidoptera, with Introductory Observations on a General Arrangement of this Order of Insects.—By* THOMAS HORSFIELD, M.D. F.R.S. L.S. & G.S. *Member of the Royal-Asiatic, and Zoological Societies of London, and of the Imperial Academy Naturæ Curiosorum; Corresponding Member of the Academy of Natural Sciences of Philadelphia, and of the Historical Society of Pennsylvania, &c. To be completed in Six Parts.*

THE subjects which this work will bring before the public are arranged in the Museum of the Honourable East-India Company. They consist principally of a general series of Lepidopterous insects from the island of Java, accompanied by an extensive set of drawings representing their metamorphosis, the history of which is detailed in the Introduction. To this will be added various subjects contained chiefly in a collection of insects from Ceylon, presented by M. Jionville, and in a smaller miscellaneous collection from continental India, presented by Claude Russell, Esq., the brother of Dr. Patrick Russell. It is likewise the author's intention to include such additions as may be made from time to time to the Museum during the progress of his work, from the territories of the Honourable Company in the Eastern World.

The work will be published in royal quarto, and will consist of six parts, each containing about eighty pages of letter-press; the distribution of the subjects being the following:

Part I. INTRODUCTION: detailing the outline of a general arrangement of Lepidopterous insects according to their metamorphosis. Description of the First Tribe, or of the LEPIDOPTERA DIURNA. Stirps the *first*, with VERMIFORM LARVÆ. Genera *Polyommatus* and *Lycæna*.

Part II. Conclusion of the Vermiform Stirps. *Second* Stirps of Lepidoptera Diurna with CHILOGNATHIFORM or IULIFORM LARVÆ. *Third* Stirps with CHILOPODIFORM or SCOLOPENDRIFORM LARVÆ. *Fourth* Stirps with THYSANURIFORM LARVÆ.

Part III. *Fifth* Stirps of Lepidoptera Diurna, with ANOPLURIFORM LARVÆ.
Second Tribe of LEPIDOPTERA or SPHINGIDÆ.

Part IV. Third Tribe or BOMBYCIDÆ.

Part V. Fourth Tribe or NOCTUIDÆ.

Part VI. Fifth Tribe or PHALÆNIDÆ.

Each part will be illustrated by four plates consisting of highly finished engravings by artists of eminence. Three of these will be coloured with accuracy and elegance; the fourth, more elaborate as an engraving, will be given plain.

The first plate is devoted to the illustration of new *species*, and such subjects will be preferred as are typical of the groups defined in in the progress of the work: they will be arranged, as far as possible, according to their affinities. For the second plate, those subjects will be chiefly selected which form types of *genera;* and they will, in most cases, be accompanied by dissections. On the third and fourth plates the history of the *metamorphosis* will be elucidated, and they will likewise contain additional generic illustrations and dissections.

As to the plan of the descriptive part, a very concise outline can only be given in this place. The arrangement proposed to be followed, and the constitution of the higher divisions, namely of *tribes* and *stirpes*, are explained in the Introduction. These are defined from a review of the whole order: but the sections indicated either in the stirpes or in the genera are provisional only, as they are regulated by the extent of the collection. A detailed generic character is given in the Latin language; this is followed immediately by a somewhat amplified description in English. Every species is distinguished by the generic and specific name at length. This is followed by a Latin description, in technical language, intended to exhibit a concise but accurate delineation, sufficiently minute to afford the means of precise discrimination from all other species. In the English description of new species the object is to give a full history of the external character in all its details. It is not consistent with Dr. Horsfield's present plan to give specific characters according to the Linnæan models: these belong, in his opinion, to works in which a general comparison of species contained in extensive collections, enables the writer to define the characters with a precision and confidence which cannot be obtained in the examination of a merely local collection.

The detailed specific descriptions will be followed, in most cases, by a series of miscellaneous observations. In these it is the author's chief object to illustrate the history of those individuals which he has traced through their various stages of existence, and of which the collection contains representations in their larva and pupa states. The arrangement projected for this work being founded primarily on the metamorphosis of the insects of this order, this part of the subject will be found to have an important bearing on the whole. These observations will also afford the necessary explanations of the figures contained in the third and fourth plates, and they will lead to the detail of the remarks made on the food of the larvæ, the season of the year when found, their abundance or scarcity, and to such other peculiarities as may have been noticed in Java. Under this head, he will also give an account of the state of the collection regarding the materials from which the descriptions have been made, with the view to illustrate many doubtful or imperfectly known species. The public or private collections in the British metropolis, in which the species described may have been observed, will also be indicated; and finally their range through other parts of India: and in the whole of these miscellaneous observations, as well as in the generic and specific descriptions,

scriptions, a principal object will be to render the work generally useful and interesting to the British naturalist.

The parts will follow each other with every degree of expedition consistent with the preservation of the style of publication in which the work has been commenced. Those preparatory arrangements which are inseparable from every undertaking of this nature have in some measure retarded the first part, but the publishers are enabled to engage, with every prospect of success, a regular continuation of the work: accordingly they announce the appearance of the *second* part early in July next, of the *third*, at the commencement of the ensuing year, and of the remaining parts at intervals of six months. According to this plan, the whole will be completed within three years from the commencement.

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*Lepidoptera Britannica; sistens Digestionem novam Insectorum Lepidopterorum quæ in Magnâ Britanniâ reperiuntur, larvarum pabulo, temporeque pascendi; expansione alarum; mensibusque volandi; synonymis atque locis observationibusque variis. Autore* A. H. Haworth, *Linn. Soc. Lond. Soc., &c. &c.*

We congratulate the admirers of British Entomology on the long expected appearance of Part IV. of Mr. Haworth's *Lepidoptera Britannica*, which is at last completed.

This comprehensive and valuable work consists of 609 closely printed octavo pages, and 36 of Preface, and has been divided into four parts; to the first of which was appended above 200 similar pages, called *Miscellanea Naturalia*.

Complete new-wrought descriptions and synonymies of all the known British *Lepidoptera* are given throughout in Latin; together with occasional observations in English, respecting their peculiarities, size, food, times and places of appearance, &c.

The First Part, which was published by the author in 1803, contains the *Papilionidæ*, *Sphingidæ*, and *Bombycidæ*. After this, the work languished for want of encouragement, till the beginning of 1809, when the Second Part was published. It contains the *Hepiali*, *Lithosiæ*, *Falcariæ*, *Noctuadæ*, *Phalænadæ*, &c.

The Third Part appeared in 1811. It contains the *Pyralidæ*, *Tortricidæ*, &c. And the Fourth Part, which has just been published, completes the arduous and useful undertaking of its indefatigable author. It contains the *Tineadæ*, &c.; and concludes the work with a complete Index of all the genera and species of *Lepidoptera* described; about 1450 in number.

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## LX. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

Feb. 21 & 28.—READ An account of the accident to the packet-ship the New York, from lightning. By T. Stewart Traill, M.D. of Liverpool. Communicated by Henry Brougham, Esq. M.P. F.R.S.

The

The ship which met with the accident of which the effects are the subject of this communication, was the American packet the New York, of 526 tons, commanded by Captain Bennet. She sailed from New York for Liverpool, on the 16th of last April; and on the morning of the 19th was struck by lightning, which shattered the main royal mast, and gliding down the iron chain main-top-sail tie, burst the iron bands on the main-mast head. It was thence conducted by the iron main-top-sail sheets, to the iron work of the pumps. It then entered between decks, demolishing the bulk-heads that formed the store-room, in its way to a small leaden cistern: whence it was conducted, by a leaden pipe, through the starboard side of the ship, where it started three five-inch planks, ten feet in length, at the lower part of the bends. Many other parts of the ship, not in the direct line of its passage, were also shattered, apparently from the effects of a lateral explosion; several doors and partitions were thrown down, a large mirror in the cabin was shivered into small fragments, and a piano-forte was thrown down, its top blown off and broken in pieces. The loudness of the explosion was appalling and spread universal consternation. A sulphureous smoke, which had issued with a bluish flame from the hatches, filled the cabins; and at first inspired alarm, lest the cargo in the hold, consisting chiefly of cotton and turpentine, had taken fire; but on clearing the main hatch, it was soon ascertained that no danger from fire existed. The ship however had sprung a leak which made four inches of water every hour, but which on washing the pumps was found to be under command, and would not prevent her proceeding on her voyage to England.

When the first terror created by the accident had somewhat subsided, it was found that none of the passengers or crew had sustained any injury. The chief mate was sleeping in the birth opposite to the main hatch, near the spot where the lightning entered the store-room, the lock of which was forcibly driven into his cabin: but he was not himself affected by the shock, and a quantity of gunpowder which was kept under his bed was fortunately not ignited by the lightning. An ewer and a basin placed on a stand over a child's bed were thrown down by the explosion, but the child had escaped unhurt. A remarkable effect was however produced on an elderly gentleman, who for the last five years had not been able to walk half-a-mile at a time: terrified by the crash, he forgot his debility, and springing from his bed, rushed on deck with singular quickness and agility. He has retained, ever since the event, the power over the muscles of his limbs, derived from this sudden emotion.

The threatening aspect of the heavens, the appearance of numerous water-spouts on the surface of the sea, and other electrical indications, gave rise to apprehensions of further danger, and induced the captain to put up the conductor with which he was provided, but which had not been previously applied. It was made of iron links eighteen inches long, connected by iron rings one inch in diameter; and was furnished at the top with an iron rod four feet long and half an inch in diameter, tapering to a fine point. This rod was fixed so as to rise three feet above the main royal mast head; and the

the chain was made to descend along the back-stay, and below was kept at a distance of ten feet from the starboard bulwarks by a light wooden outrigger, or spar. Its whole length was 145 feet, of which about nine feet of its lower part descended into the sea. The wisdom of adopting this precaution was soon apparent, for in the course of the same morning the ship was struck by a second explosion, which is stated by the unanimous testimony of all on board to have far exceeded in violence the first. It melted a great part of the conductor, producing a vivid combustion of many of the links, which burned like so many tapers; and descending into the sea, darted off to a considerable distance along the surface of the waves. The resistance to its passage was so great as to cause the ship to recoil with a sudden and violent shock, so as to throw down several of the crew. The melted iron of the conductor fell in large drops on the deck, which although already strewed with hailstones that had previously fallen, intermixed with rain, was set fire to in many places by the ignited metal. No damage, however, was done to the masts or rigging, not the least injury to any of the crew, with the exception of a carpenter, who being at work with an iron auger in his hand, received a smart shock through the wrists, which occasioned a livid tumour which was still visible six weeks after the accident.

Soon after the arrival of the vessel in Liverpool, she was docked in order to ascertain what damage she had sustained. Some of her planks were found to have started, but her timbers were uninjured. Every instrument made of steel,—such as the carpenter's tools, and the knives and forks; and also those made of soft iron, even to the very nails in every part of the ship,—had been rendered permanently magnetic. All the watches and chronometers were either stopped or rendered useless, by the magnetism imparted to the balance-wheels and other parts of their works that were made of steel. Contrary to what usually happens from shocks of artificial electricity, the lightning had given a strong northern polarity to the upper part of the conductor. Many parts of the iron-work indeed had acquired the magnetism corresponding to their position with respect to the magnetic direction; but in others no relation of this kind could be traced. Great changes were produced on the magnetism of the compass needles, in many of which were formed several sets of poles, and their indications could therefore no longer be relied on.

The circumstances attending the accident which is the subject of this paper, are considered by the author as strongly confirming the value of conductors to ships in obviating the destructive effects of lightning. From the inquiries he has made, he is led to the belief that injuries from lightning at sea are much more frequent than is generally imagined. One source of increased danger of late years is to be found in the greater proportion of metal, and particularly of iron, which is employed in the rigging; more especially as the metallic masses are there nearly insulated, or connected only by very imperfect conductors. In the instance before us, it is in the highest degree probable, that if the New York had been without the

the protection of the conductor, she must inevitably have been destroyed by the second tremendous explosion, which, thus guarded, she sustained without the slightest injury. The author remarks that copper is a better material for such a conductor than iron, from its being less liable either to fusion or corrosion: and also that a rod is, from its continuity, a better form of conductor than a chain. In the case of ships, however, the greater convenience of a chain, arising from its flexibility, will generally insure it the preference. The author recommends, that, instead of carrying the conductor through the decks to the keel, as suggested by Mr. Harris, the lower end of the chain should be kept at a distance from the sides of the ship, by means of a light outrigger, or spar, as was done in the New York.

March 20.—Read a paper On the Phænomena of Volcanoes; by Sir H. Davy, Bart. F.R.S.

In a paper on the decomposition of the earths, published in the Phil. Trans. for 1812, the author offered it as a conjecture, that the metals of the alkalies and earths might exist in the interior of the globe; and on being exposed to the action of air and water, give rise to volcanic fires, and to the production of lavas, by the slow cooling of which basaltic and other crystalline rocks might subsequently be formed. Vesuvius, from local circumstances, presents peculiar advantages for investigating the truth of this hypothesis; and of these the author availed himself during his residence at Naples, in the months of December 1819, and of January and February 1820. A small eruption had taken place a few days before he visited the mountain, and a stream of lava was then flowing with considerable activity from an aperture in the mountain a little below the crater, which was throwing up showers of red-hot stones every two or three minutes. On its issuing from the mountain, it was perfectly fluid, and nearly white hot: its surface appeared to be in violent agitation from the bursting of numerous bubbles, which emitted clouds of white smoke. There was no appearance of vivid ignition in the lava when it was raised and poured out by an iron ladle. A portion was thrown into a glass bottle, which was then closed with a ground stopper, and, on examining the air in the bottle some time afterwards, it was found not to have lost any of its oxygen. Nitre thrown upon the surface of the lava did not produce such an increase of ignition as would have attended the presence of combustible matter. The gas disengaged from the lava proved on examination, to be common air.

When the white vapours were condensed on a cold tin plate, the deposit was found to consist of very pure common salt; and the vapours themselves contained nine per cent of oxygen, the rest being azote, without any notable proportion of carbonic acid or sulphureous acid gases; although the fumes of the latter of these gases were exceedingly pungent in the smoke from the crater of the volcano. On another occasion, the author examined the saline incrustations in the rocks near the ancient bocca of Vesuvius; and found them to consist principally of common salt, with some chloride of iron, a little sulphate of soda, and a still smaller quantity of sulphate

sulphate or muriate of potassa, with a minute portion of oxide of copper. In one instance, in which the crystals had a purplish tint, a trace of muriate of cobalt was detected. From the observations made by the author at different periods, he concludes that the dense white smoke which rose in immense columns from the stream of lava, and which reflected the morning and evening light of the purest tints of red and orange, was produced by the salts which were sublimed with the steam. It presented a striking contrast to the black smoke arising from the crater, which was loaded with earthy particles, and which in the night was highly luminous at the moment of the explosion. The phænomena observed by the author afford a sufficient refutation of all the ancient hypotheses, in which volcanic fires were ascribed to such chemical causes as the combustion of mineral coal, or the action of sulphur upon iron; and are perfectly consistent with the supposition of their depending upon the oxidation of the metals of the earths upon an extensive scale, in immense subterranean cavities, to which water or atmospheric air may occasionally have access. The subterranean thunder heard at great distances under Vesuvius, prior to an eruption, indicates the vast extent of these cavities; and the existence of a subterranean communication between the Solfatarra and Vesuvius, is established by the fact that whenever the latter is in an active state, the former is comparatively tranquil. In confirmation of these views, the author remarks, that almost all the volcanoes of considerable magnitude in the old world, are in the vicinity of the sea: and in those where the sea is more distant, as in the volcanoes of South America, the water may be supplied from great subterranean lakes; for Humboldt states that some of them throw up quantities of fish. The author acknowledges, however, that the hypothesis of the nucleus of the globe being composed of matter liquefied by heat, offers a still more simple solution of the phænomena of volcanic fires.

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## LINNÆAN SOCIETY.

April 1.—Lord Stanley in the chair.

His Lordship opened the meeting of the Society by adverting, with much feeling, to the great loss which had been sustained by the country and by the world, and more especially by the Society, in the death of its illustrious and beloved President Sir James Edward Smith, who from its first establishment, in which he had taken an active part, had been called upon to preside over it by the annual and unanimous votes of its members, and had greatly contributed to place the Society in the distinguished rank which it had attained, by his great talents, indefatigable industry, sound judgement, and enlarged views as a naturalist;—by the high estimation in which he had long been held by men of science all over the world; by the excellence of those valuable and accurate works in which he had done so much to promote and improve the study of natural history; and especially by the qualities of his heart, mind, and temper, for which his memory would long be revered by those who had enjoyed the happiness of his friendship.—He could not forbear expressing what he felt

felt on the present occasion, especially with reference to the particular moment of his loss,—at a time when those considerations of religious distinction were about to be removed, which had seemed to have a tendency to deprive those who, like this excellent and distinguished man, differed from the established religion, of the rank in society due to their talents or their worth*.

His Lordship expressed his anxiety that whatever choice might be made by the Society to fill the vacancy in its Chair, should be such as would contribute to its prosperity, however impossible it might be adequately to supply the loss which it had now so much to regret.

Lord Stanley then adverting to the last volume of the English Flora, which had been received from Sir James Smith but a few days before his death, and was among the presents on the table, related that, showing it to a friend, he had exclaimed, "This is the close of my labours."—As its distinguished author was now removed from the possibility of receiving the customary vote of thanks, His Lordship concluded, by proposing that the grateful feelings of the Society might be expressed to Lady Smith for this last gift of their revered President.

A portion of Dr. F. Hamilton's Commentary on the *Hortus Malabaricus*, Part V., was then read.

April 15.—Read a letter addressed to the Secretary, from Charles Lucien Buonaparte, Prince of Musignano, F.M.L.S., dated on board the Delaware, near Gibraltar, March 20th, 1828, containing the following notice relative to the migration of certain birds.—"A few days ago, being 500 miles from the coasts of Portugal, 400 from those of Africa &c., we were agreeably surprised by the appearance of a few swallows, *Hirundo urbica* and *rustica*. This, however extraordinary, might have been explained by an easterly gale, which had cut off the swallows migrating from the Main to Madeira, only 200 miles distant from us;—but what was my surprise on observing several small warblers hopping about the deck and rigging! These poor little strangers, exhausted as they were, were soon caught and brought to me. The following short list is that of the species.

"1. *Sylvia trochilus*.—2. *Sylvia erithacus* (*tethys*, Temminck).—3. *Sylvia suecica*, or rather a similar species, which I have already received from Egypt and Barbary.—4. A species new for Europe and perhaps even a nondescript, having the plumage of an *Anthus*, and which I think belongs (as *Sylvia cisticola* and others) to the *hitherto African* genus *Malurus*. This, however, must rest undecided, my specimen missing its tail, which was pulled off by the sailor who caught the bird."

A communication was likewise read to the Society from J. Morgan, Esq. F.L.S., relative to the structure of the mammary organs of the Kangaroo, in which a detailed account was given of a recent dissection of

* Alluding to the proceedings for the abolition of the sacramental test,—Sir James Smith having been a member of the congregation of Unitarian dissenters at Norwich.

of these parts, both in the virgin and in the impregnated animal; together with the author's opinions respecting the physiology of certain structures which have been hitherto unnoticed, and of others which have been incorrectly or imperfectly described by former investigators of this interesting branch of natural science.

The author first pointed out the anatomical peculiarities which he had discovered in a dissection of the pouch and mammæ of a young and unimpregnated kangaroo; by which it appears, that, in the virgin state, the two upper nipples only are found to be developed, and that beneath each of these a minute circular aperture, resembling in appearance the mouth of a follicle, occupies the exact situation in which the lower teat is known to exist in the adult impregnated animal.—The mammæ are described as consisting of double glandular structures on each side; they are situated directly behind the follicular openings already mentioned, and are closely confined to the posterior surface of the integuments. Each double mamma is formed by an upper and smaller gland, which is attached by its excretory ducts to the already developed nipple, and by a second and larger glandular substance from which no excretory duct could be traced. The follicular apertures which occupy in the pouch the situation of the lower teats, form the external openings of cylindrical membranous canals which lie imbedded in the substance of the larger and lower mammary glands. Each of these membranous canals or tubes is about three-fourths of an inch in length, and extends through nearly the whole diameter of the larger gland which encloses it; the interior of the tube is lined by cuticle, and the internal extremity is terminated by a rounded papilla which projects into its cavity.

In these papillæ the author found a perfect miniature resemblance to the extremities of the lower teats in the adult animal, which teats he considers to be formed, *during the first gestation*, by the complete eversion of the membranous canals, and the consequent projection of their papillary terminations. He further states, that by artificially everting the parts, two perfect teats are produced in the precise situation of those which are found in after life. It has been however ascertained that this extraordinary change occurs only during the first gestation, since after being once developed the teat remains permanently formed and projected.

Having thus described the condition of the mammary glands and the development of the lower teats in the virgin animal, the author gave a particular account of his dissection of an adult female, which at the time of death was suckling a young one nearly half-grown. In this we are informed that the panniculus carnosus which covers the anterior surface of the belly is of extraordinary thickness, and composed principally of perpendicular muscular fibres, which in their course from the thorax downwards surround the mouth of the pouch to which they form a sphincter muscle, and that a fasciculus of its fibres descending over the symphysis pubis is inserted into the sphincter muscle of the cloaca; so that the contraction of this part of the panniculus carnosus would powerfully operate in approximating the external aperture of the vagina with the mouth of the pouch.

On

On removing the panniculus carnosus a pair of muscles (of which the attachments and uses have been hitherto incorrectly described) are brought into view. Each of these muscles is of a triangular shape, being attached by a narrow origin to the posterior part of the pelvis; and expanding in its course, is continued transversely round the lower part of the belly, before the abdominal muscles, and immediately above the brim of the pelvis. Each of these triangular muscles encloses, between an anterior and posterior layer of its fibres, the mammary gland, and the two muscles afterwards cross the fore part of the abdomen to unite in front of the linea alba. By this union a perfect muscular girdle is formed, by the contraction of which the mammæ are compressed against that part of the abdomen in which the marsupial bones lie imbedded.

The conclusion of this paper, containing further particulars of the dissection of the mammary organs as well as of the muscles attached to the marsupial bones of the adult and impregnated animal, together with the author's opinion respecting their physiology, remains to be read at a future meeting.

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## ASTRONOMICAL SOCIETY.

Feb. 8.—This day being the Anniversary, Dr. O. G. Gregory, one of the Secretaries, read the following Report of the Council to the Eighth Annual General Meeting.

Your Council are happy in being able to commence this, their Eighth Report, with a general expression of congratulation on the progress of the Society, and on the prospects of increased and increasing utility, which a view of its actual state and resources, and a consideration of the events of the year now closed, in their minds appear to justify. They trust, too, that a review of these events will at the same time justify the confidence reposed in them, by showing that they have not been neglectful of the opportunities which have offered of advancing in various ways the objects and interests of the, Society.

One of the first acts of the Council of the year elapsed, was to enter into an arrangement with Mr. Taylor, the printer to the Society; and who is also one of the editors of the Philosophical Magazine, for the publication of a series of monthly notices of its Proceedings, and for the supply of a sufficient number of copies of them, in succession, for distribution among the members. The convenience and advantages of this plan have been sufficiently proved by the trial which has been given it, and it will of course be continued. The public is hereby brought more immediately into contact with the Society—the labours of its contributors are canvassed and discussed, while the interest of the author in his subject is yet warm, and when the interchange of ideas respecting it is most beneficial, not only to the public, but to the author himself, whose views may, and probably in many instances will, be enlarged or corrected by such intercourse. An authentic and at the same time public record is, as it were, opened, of the papers read, and the outlines of their contents rendered matter of history;—thus affording ready means of establishing the claims of authors

authors to priority of discovery, so far at least as priority of public communication can be regarded as evidence in questions of that nature. The same arrangement offers the further advantage of relieving the Council from all difficulty in the disposal of matter of merely temporary interest; such as notices of phænomena, ephemerides of the smaller planets, comets, &c. which require speedy circulation, and do not need to be formally enrolled in the *Memoirs;* and it may occasionally happen, that the matter of more regular communications may so far be condensed into the monthly abstract, as to dispense with a second publication in the Memoirs—to the material relief of the funds of the Society.

The same consideration of the advantages derived from the speedy publication of communications read to the Society, has induced the Council to adopt the principle of sending immediately to press all papers which, on passing through the prescribed formalities, shall be deemed of a nature for publication in their Memoirs; and although objections have been considered to exist against the separate publication of each particular memoir, the division of their volumes into smaller parts can be attended with no inconvenience. Acting on this principle, the Council have directed the publication of Part I. Vol. 3. of the Memoirs, including all papers (regarded by them as suitable for printing in the body of the Memoirs) read during the interval elapsed from the last publication. This Part is now, accordingly, before the public, and furnishes satisfactory proofs of the zeal, diligence, and talents, both of our Home Members and our Associates.

It is not merely, however, in communicating to the world the observations of their members, but also in causing observations to be made, and in lending every assistance in their power to those meritorious and public-spirited individuals, who, actuated only by an earnest desire to render available their leisure and talents in the cause of science, are willing to undertake the task of regular observation,—that a Society like this can render service to science. The munificent act of a private individual, has happily placed this particular line of utility more directly than heretofore in the power of the Society; and its members have not been slow in availing themselves of its advantages, and applying them to effective use.—Among the great and lamented losses which the Society has sustained in the course of the last year, is that of the late Colonel Mark Beaufoy; the latter days of whose existence we recollect with a melancholy pleasure to have been cheered and gratified by the highest mark of this Society's approbation, in the award of their medal for his Astronomical Observations. His son, Lieut. George Beaufoy, has, with the utmost liberality, placed his deceased father's astronomical instruments in the possession of this Society. They consist of

One 4-feet Transit, by the late Mr. Cary, Strand.
One Altitude and Azimuth Circle, by the same.
One Clock, adjusted to mean Solar time.
One Clock, adjusted to Sidereal time.

Your Council conceive, that the opportunity fortunately placed before

fore them, by Lieut. Beaufoy's recent election as a member of this Society, of marking their sense of this liberal and public-spirited conduct on his part, by a high and complimentary distinction, ought not to be neglected. They have therefore thought it adviseable to recommend to the Society to elect him a member for life.

The Council wish to observe, with respect to the measure so recommended, that though it may seem equivalent to the acknowledgement of a class of members similar to that, which in the constitutions of some scientific bodies are deemed honorary members,—yet they are rather desirous to avoid the establishment of purely complimentary distinctions, and to mark by it, in this and any future case which may occur, the Society's sense of some distinct benefit accruing to itself or to astronomical science, through the individual so distinguished, which could not be properly acknowledged otherwise:—thus leaving every future case to rest on its own individual merits, as if it were the first of its kind.

The surest criterion of the utility of a donation is its immediate and effective practical application. That of Lieut. Beaufoy was scarcely announced to the Council, when an application was made to them by one of our members, Captain Smyth, R.N. (justly distinguished for his knowledge of the resources of practical astronomy), for their loan, which was immediately accorded; and the Council have the high satisfaction of being able to announce to you, that the instruments in question are at this moment (with the exception of one of the clocks) mounted in the best manner, in a regular observatory established by Captain Smyth, at his residence at Bedford, for their express reception, and already in actual use in celestial observation. The Council, though not unaware of the general nature of Captain Smyth's astronomical views, purposely forbear from publicly stating at present the course of observations in which he purposes to engage; being desirous to leave his meritorious exertions as far unfettered as possible by any public pledge—and trusting rather to his high character and well-known zeal, talent, and activity, than to any express stipulation, that the means thus placed in his hands will be exerted for the advancement of astronomical science.

Actuated by a similar desire to promote, as far as our means will admit, the great objects of astronomical inquiry in all its branches, the Council have ordered two invariable pendulums of iron and of copper, on a construction somewhat different from those hitherto employed, to be consigned to Captain Foster, for the purpose of investigating the possible effects of the earth's magnetism in various geographical positions, in these delicate researches, in the course of his approaching scientific voyage; an enterprise which honourably characterizes the enlightened views of Government, and from which a rich harvest of important results may be anticipated.

From the report of the Finance Committee it appears that the funds of the Society continue in a flourishing state, notwithstanding the heavy demands for computation and printing during the last year. This extraordinary expenditure has principally arisen from the great expense incurred in publishing the Catalogue of the principal fixed stars,

stars, alluded to in the last Report, and which appears to have met the approbation of every astronomer. But the Council are happy to state, that this disbursement has been followed by a corresponding spirit of zeal and liberality on the part of several of the members, who, by entering into a subscription, have enabled the Council to defray nearly the whole expense of that useful and valuable work, without intrenching on the ordinary funds of the Society. And it may be mentioned, as a proof of the liberal spirit which animated those members, that the sum of 320*l.* was subscribed, within a few hours, towards the expense of publishing the Catalogue above alluded to.

The Council cannot too earnestly impress on the attention of the members, the necessity of encouraging the sale of the Memoirs as much as possible: since it is only by a quick return, arising from such sale, that they can expect to continue the publication, from year to year, without a serious diminution of the funds of the Society.

Among the accessions to our home list of members, in the year elapsed, the Council have to record, with all those sentiments of respect which his illustrious rank so peculiarly inspires, the name of His Royal Highness the Lord High Admiral: and they trust that this accession, so honourable in itself, and so gratifying to the Society, will be attended with the most beneficial results to the science which this Society was formed to promote, and which is so intimately connected in its practical application with the department over which His Royal Highness presides. Our home list has been further increased by the accession of fourteen new members, and our foreign, by that of one associate.

Among the losses sustained by death, the Society has to lament, in addition to Colonel Beaufoy, that of its most illustrious associate Laplace: and on our home list, of the Rev. Lewis Evans; His Grace the Duke of Gordon; our amiable and excellent trustee, Mr. Daniel Moore (whose loss will be felt far beyond the limits of this body by many, as the privation of a benefactor, in whose ears the calls of distress never sounded in vain); Mr. W. M. Mosely; and Mr. J. Sanders.

Mr. Mosely, in addition to a competent knowledge of various sciences, had turned much of his attention, in the latter part of his life, to astronomy. He possessed several valuable instruments, and is said to have left behind him a series of observations of transits and north polar distances, and some measures of double stars confirmatory of their changes.

Mr. Evans also possessed instruments of considerable merit, and for several years employed himself as a skilful and successful observer.

It will not be expected that any sketch should here be attempted of the labours of M. de Laplace. No history of his scientific life, of the origin and development of his views, adequate to the occasion, could be comprised in such limits as those of this Report: and the general nature of the more important results of his researches is already so well known, and so incorporated with the intellectual history of the last half century, that it constitutes a portion of the knowledge of every well informed man. During the long period of fifty-five years,

years, from the date of his first considerable mathematical production on the integration of equations of differences, in the *Turin Memoirs* in 1772, to almost the moment of his death, his march was in the van of intellect; and the highest point of scientific attainment to which the age had reached, was uniformly marked by his progress. His whole career was a succession of brilliant and profound discoveries, where every great step in the theoretical departments of analytical science was sure to be attended with a corresponding advance in its practical application; and every difficulty which occurred in applying his principles to the sublime problems of physical astronomy, served only to give rise to new methods, and create more powerful engines of mathematical inquiry; a rare combination,—of which Archimedes and Newton had afforded the only previous examples,—of a philosophical spirit of the first rank wielding all those unbounded resources of abstract science, which a few extraordinary men of a different turn may have perhaps possessed in an equal or superior degree, but which only attain their highest value when exerted from the vantage-ground of a mind at home in every department of experimental philosophy, impressed with the fullest sense of the importance of practical application, and familiar with all the means of disentangling principles from natural phænomena. Laplace also afforded a conspicuous instance of that union of gentle and amiable social qualities, which is one of the best characteristics of the highest order of genius—as indicating a mind secure of its rank. No pretension—no assumption—nothing dictatorial in science or offensive in taste, marked his mild and modest deportment. They, who have heard his sublimest views propounded with the diffidence of a youth seeking information; they, whose early scientific attempts have been encouraged, and whose maturer efforts were assisted and directed by his talents,—will long retain a penetrating sense of this estimable feature of his character.

The only one of the prize questions proposed at the instance of the Council in February 1824, the period of which remained unexpired during the year elapsed, is that relating to the development of the differential equations of the lunar theory, and the improvement of the lunar tables. No answer has been received to this question; and the proposed period has now expired. The Council have not thought it adviseable either to prolong this period, or to propose any new questions for the present or future years: but, should subsequent researches either afford complete solutions of any of those questions, or material elucidations of their peculiar difficulties, future Councils will not fail to bear in mind the importance that attached to their subjects in the minds of former ones, and recommend to the Society the accordance of such marks of their approbation as the degree of progress made in them may appear to demand.

Two medals have been awarded this year by the Council. One to Sir Thomas Macdougal Brisbane for the inestimable benefit conferred by him on astronomical science, in the establishment of his observatory at Paramatta in New South Wales, and for the valuable and important series of observations made there by himself, and under his

his directions, during his residence as governor of that colony. The other to Mr. James Dunlop, for his disinterested and indefatigable pursuit of astronomical researches, subsequent to the departure of Sir T. M. Brisbane from the colony of New South Wales, whereby he has added, in a most material degree, to our knowledge of the nebulæ of the southern hemisphere.

These medals will be delivered to these gentlemen respectively, or to their proxies, by the President, before the Society proceed to the business of election of the officers for the ensuing year.

The Council cannot conclude this Report, without an earnest exhortation to the members of the Society, collectively and individually, to cooperate by their active exertions in the great cause for which the Institution exists. They entreat them to remember that every one who possesses an instrument, whose claims rise even not above a humble mediocrity, has it in his power to chalk out for himself a useful and honourable line of occupation for leisure hours, in which his labour shall be really valuable, if duly registered: that those who possess *good* instruments, have a field absolutely boundless for their exertions. To such they would hold out the brilliant examples of many other of the members and associates of this Society,—of a Bessel, a Struve,—as showing what may be accomplished by persevering industry, and how little reason there is to apprehend the failure of matter for their researches. They would strongly impress on the minds of all observers, however, whether private or public, the daily increasing necessity which exists for the *reduction* of their observations when made, and for the employment for that purpose of a uniform system of corrections adopted by common consent: and that, not when the work of many years shall have accumulated on hand, till it shall have become overwhelming; but regularly, year by year—nay, month by month, while the immediate interest of the observations retains its freshness in the memory, and while their actual value can be checked, and the means of further refinement suggested and attained. The extreme facility afforded for these reductions by the Catalogue published by this Society, leaves no excuse for their neglect, when stars contained in it are observed; and it is most earnestly to be hoped, that no departure in other cases, from the coefficients there used, will take place.

The Council would also strongly draw the attention of the members of the Society to the high importance, both for nautical and astronomical science, of observations of occultations, eclipses of the sun, and satellites of Jupiter, moon-culminating stars, and even ordinary lunar distances well and carefully observed on land, at fixed stations, such as observatories, and at all remarkable points in distant countries, where opportunities may offer. The Society will feel its utility most materially increased by the communication of such observations, if carefully made. No class of individuals have so frequent and available opportunities for such observations, as naval officers, nor to any is the object of such direct utility.—It is gratifying to know, that the requisite scientific attainments for such researches are already widely diffused among them, and daily becoming more

more so; and it is cheering to indulge the pleasing reflection, that the calls thus made upon them will not be unavailing.

(*The President then addressed the meeting on the subject of the award of the Medals, as follows:—*)

Gentlemen,—In pursuance of the award of your Council, which you have just heard, I have now to call your attention to the subject of the honorary marks of this Society's approbation, which it is part of our business at this meeting to bestow. The selection of objects on which such distinction may most deservingly and most usefully be conferred, has been, in this instance, of much interest and some difficulty,—not from a paucity of claims, but from their variety and magnitude. On all sides, both abroad and at home, the spirit of Astronomical research and discovery has been diligently alive. The great work which has been commenced on the Continent, for the determination of the places of all the stars of our hemisphere in zones, has been continued with a patient ardour to which no words can do justice.—The heavens have been ransacked for double stars; and the results of the search, developing a most rich and unlooked-for harvest of striking discoveries, being the first fruits of the great telescope of Fraunhofer, have been consigned to immortality, in a work which does honour to its age and nation, and which has already been brilliantly rewarded in another quarter. The ingenuity of one of our own countrymen, has placed new, simple and powerful means in the hands of observers, for verifying the stability of their instruments, and determining their fluctuations. And in every quarter, to go no further in this detail, an activity worthy of the high ends and dignity of our science, has been remarkably displayed. Among so many important labours, however, some of which are yet awaiting their final completion, or receiving the last touches of their authors, the attention of your Council has been fixed, by the imposing mass of valuable observations which has emanated during a series of years, from the Observatory at Paramatta, established by the late governor of the colony of New South Wales, Sir Thomas Macdougal Brisbane, one of our Vice-Presidents, long distinguished among us by his ardent love of Astronomy, and an intimate familiarity both with its theory and practice.

Nothing can be more interesting in the eyes of an European astronomer, especially to those whose field of research, like our own, is limited by a considerable northern latitude,—than the southern hemisphere, where a new heaven, as well as a new earth, is offered to his speculations; and where the distance, the novelty, and the grandeur of the scenes thus laid open to human inquiry, lend a character almost romantic to their pursuit.

A celestial surface equal to a fourth part of the whole area of the heavens, which is here for ever concealed from our sight, or whose extreme borders at least, if visible, are only feebly seen through the smoky vapours of our horizon,—affords to our antipodes the splendid prospect of constellations different from ours, and excelling them in brilliancy and richness. The vivid beauty of the Southern Cross has

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been sung by poets, and celebrated by the pen of the most accomplished of civilized travellers; and the shadowy lustre of the Magellanic clouds, has supplied imagery for the dim and doubtful mythology of the most barbarous nations upon earth. But it is the task of the Astronomer to open up these treasures of the southern sky, and display to mankind their secret and intimate relations. Apart, however, from speculative considerations, a perfect knowledge of the astronomy of the southern hemisphere is becoming daily an object of greater practical interest, now that civilization and intercourse are rapidly spreading through those distant regions,—that our own colonies are rising into importance,—and that the vast countries of South America are gradually assuming a station in the list of nations, corresponding with their extent and natural advantages. It is no longer possible to remain content with the limited and inaccurate knowledge we have hitherto possessed of southern stars, now that we have a new geography to create, and latitudes and longitudes without end, to determine by their aid. The advantages too, to be obtained, even for the perfect and refined astronomy of the north, by placing nearly a diameter of the globe, between the stations of observatories, and taking up the objects common to both hemispheres, in a point of view, and under circumstances so every way opposite to those which exist here, have been strongly pointed out by a venerable and illustrious member of this Society, in an elaborate paper published in its Memoirs, and would alone suffice to justify a high degree of interest, as due to every well conducted series of observations from that quarter. The observations of Halley at St. Helena, had made known the places of a moderate number of the brighter southern stars; but the only catalogue of any extent and accuracy, which existed previous to the establishment of the observatories of the Cape and Paramatta, was that of Lacaille, who spent three years at the Cape of Good Hope, and the Isles of France and Bourbon; and, though with very inadequate instrumental means, yet, by dint of the most indefatigable industry, succeeded in observing and registering upwards of 10,000 stars. But by far the greater part of these observations have never been reduced; a selection only from them of 1942 of the principal ones, not amounting to a fifth of their whole number, having been formed into a catalogue, and published by this meritorious astronomer. It must be admitted, however, that the degree of accuracy stated by Lacaille himself to have been probably attained by him, is hardly such as to make us now very deeply regret their want of reduction, especially as the observations themselves are printed with every requisite for that purpose, when required. Still, however, from his method of observing, which was with a fixed telescope and rhomboidal network, his observations have what may be termed a dormant value, as they most probably give correct differences for each night's work; and when a catalogue of standard southern stars shall be completed, Lacaille's observations will become available, by regarding these as zero points, and referring all the rest to them.

Such was nearly, with little improvement, the state of the astronomy

nomy of the southern hemisphere, when Sir Thomas Brisbane was appointed governor of the Colony of New South Wales. The intention of our Government to found an observatory on the largest scale, at the Cape of Good Hope, was, indeed, already fixed; and the observer, a member of this Society, supplied with instruments sufficient for the purpose of constructing a preliminary catalogue, occupied himself with the necessary observations, while awaiting the arrival of those ultimately destined to adorn that establishment, and the building of his observatory. The approximate catalogue so constructed and reduced, containing all the southern stars observed by Lacaille, down to the 5th magnitude, is already printed by the Royal Society in their Transactions.

Sir Thomas Brisbane's attachment to Astronomy had ever been a prevailing principle of his mind, and one which even amidst the distractions of a military life of no ordinary degree of activity and adventure, he found means to indulge; and which never deserted him, however the calls of his country might demand his services in a different and more splendid career.

His appointment to the important office of governor of New South Wales, however, put it in his power to execute to their fullest extent and under the most favourable circumstances, plans of astronomical investigation, which to a private individual would have been utterly impracticable. The opportunity was embraced with eagerness. The best instruments,—consisting of an excellent transit of 5½ feet focal length, by Troughton; a mural circle of two feet in diameter, the workmanship also of Troughton, and said to have been the model on which that of Greenwich was constructed, and which had long been in his possession; and a fine 16-inch repeating circle of Reichenbach,—were destined for this service: and two gentlemen engaged as assistants at considerable salaries; the one a foreigner of high estimation as a mathematician and calculator, the other Mr. Dunlop, of whom I shall presently have occasion to say much more. It ought to be mentioned, that this noble equipage was furnished entirely from Sir Thomas's private fortune, and maintained wholly at his own expense. Immediately on his arrival in the colony in 1821, and so soon as an observatory could be erected, and the instruments established, the work of observation commenced, and continued with little interruption under the immediate superintendence and direction of Sir Thomas Brisbane himself, who, though the pressing and important duties of his high office would of necessity seldom admit of his devoting any material proportion of his time to actual observation, yet frequently took a personal share in the labours of the observatory, as a relaxation from higher duties, and in particular, a great portion of the transits were observed by himself.

The first fruits of this enterprise, were the observations of the December solstice of 1821, which were published in the Astronomical Notices of Schumacher; in which work also appear those of both the solstices of 1822, and a number of detached and occasional observations, which reached Europe at different times by a variety of channels, and found their way into that valuable collection. The solstices of 1823, were communicated by Sir Thomas Brisbane to this Society, in a letter to our late worthy president, together with a considerably extensive series of observations of principal stars, chiefly those visible in both hemispheres, and which have undergone a careful reduction and close scrutiny in the hands of Dr. Brinkley, the details of which, as well as the original observations, are printed in the first part of the second volume of the Transactions of this Society, and which justify, in the eyes of that experienced observer, as they must in those of every practical astronomer, a decided opinion of the great care and skill with which they have been made.

A great number of occasional observations,—such as eclipses, occultations, and observations of the planets Venus and Uranus, near their conjunctions and oppositions, and of comets from the same source,—are also printed in the same volume. One of the most remarkable single results we owe to the establishment of Sir Thomas Brisbane's observatory, consists in the re-discovery of the comet of Encke in its predicted place, on the 2d June 1822. The history of this extraordinary body is well known to all who hear me; and as its re-discovery at Paramatta by Mr. Rümker, has already been, on a former occasion, distinctly noticed and rewarded by this Society, there is no occasion that I should here enlarge on it; and yet I cannot help pausing a moment to figure the delight its celebrated discoverer must have experienced, to find the calculations, on whose exactness he had pledged himself, thus verified beyond the gaze of European eyes; and this strange visitant gliding, as if anxious to elude pursuit, into its primitive obscurity, thus arrested on the very eve of its escape, and held up to mankind,—a trophy at once of the certainty of our theories, and the progress of our civilization.

Observations of the length of the pendulum were not neglected by Sir Thomas Brisbane; and the determination of this important element at Paramatta, forms the subject of a highly interesting and valuable communication made by him to the Royal Society, and printed by them in their Transactions for 1823, and discussed by Captain Kater with his usual care and exactness.

The remainder, and indeed the great mass of the observations made with the mural circle and the transit instrument, have at different periods been communicated to the Royal Society, and are for the present deposited in its archives. Forming our judgement only upon those of which an account has been publicly read at the meetings of that illustrious body, but which are understood to constitute only a comparatively small part of the whole,—they form one of the most interesting and important series which has ever been made, and must ever be regarded as marking a decided æra in the history of Southern Astronomy.

It is for this long catalogue of observations—whether scattered through the journals of Europe, printed in our own Transactions, or deposited as a precious charge in the care of a body so capable of appreciating their merits; but still more for the noble and disinterested example set by him in the establishment of an observatory on such a scale, in so distant a station, and which would have equally

merited

merited the present notice, had every observation perished in its conveyance home—that your Council have thought Sir Thomas Macdougal Brisbane deserving the distinction of a medal of this Society, which, as he is unable personally to attend this meeting, I will now deliver to his proxy, Mr. South.

Mr. South,—We request you to transmit to Sir Thomas Brisbane this medal, accompanied with the strongest expressions of our admiration of the patriotic and princely support he has given to Astronomy, in regions so remote. It will be a source of honest pride to him while he lives, to reflect that the first brilliant trait of Australian history marks the æra of his government, and that his name will be identified with the future glories of that colony in ages yet to come, as the founder of her science. It is a distinction truly worthy of a British governor. The colonial acquisitions of other countries have been but too frequently wrested from unoffending inhabitants, and the first pages of their history blackened by ferocious conquests and tyrannical violence. The treasures of gold and silver they have yielded—the fruits of rapine—have proved the bane of those who gathered them; and in return, ignorance and bigotry have been the boons bestowed on them by their parent nations. Here, however, is a brighter prospect. Our first triumphs in those fair climes have been the peaceful ones of science; and the treasures they have transmitted to us, are imperishable records of useful knowledge, speedily to be returned with interest, to the improvement of their condition and their elevation in the scale of nations.

(*The President then resumed his address to the Members, as follows* :—)

I have now to call your attention, Gentlemen, to the award of another Medal, to Mr. Dunlop, who accompanied Sir Thomas Brisbane in capacity of his assistant, and who, since the middle of the year 1823, when his companion Mr. Rümker left the observatory, remained in the sole charge of the instruments; and up to the period of the departure of his principal from the colony, continued an uninterrupted series of observations with a care and diligence seldom equalled, and never surpassed. In such cases it is not only the head which plans, but the hand which faithfully and promptly executes, that claims our applause. The most liberal provision of instrumental means would have been comparatively unavailing, had the spirit of him who supplied them, been seconded by any ordinary zeal on the part of his assistants. The records of this Society already alluded to, bear sufficient testimony to the merits of Mr. Rümker, and to our sense of them. In Mr. Dunlop were combined qualities rendering him of all others, the very individual fitted for the duties imposed on him—zealous, active, ready—but above all (and the combination is not an ordinary one), industrious and methodical. In the vast mass of observations made and registered by him, all is equable and smooth, as if the observations had all been made at a sitting:

"..... Servatur ad imum
Qualis ab incepto processerit"—

no lacunæ—no long intervals of inactivity—nothing hurried or sketchy; but the same pains-taking, laborious filling in, pervading the whole,—marking that the observer's whole heart and soul were in his work, and that each individual observation possessed its own peculiar, though momentary, interest. Nor is this wonderful. The heavens visible to Europeans, have been so thoroughly examined, and their contents so carefully registered, that there is not the slightest rational probability of any thing new or uncommon offering itself to instruments of moderate power in the ordinary course of observation. Here, however, all was new;—for the optical power of Lacaille's telescope was far too feeble to afford much insight into the physical constitution of the objects determined with it: and thus all the excitement of discovery was maintained during every step in the progress of the work.

But to be susceptible of this excitement, so maintained, the observer must be animated by the true spirit of the Astronomer; and few have possessed this spirit in a greater degree than Mr. Dunlop. In a scientific point of view, therefore, he must be regarded as the associate, rather than the assistant of his employer; and their difference of situation becomes merged in their unity of sentiment and object.—These considerations alone would have rendered it impossible to your Council to disunite in any expression or mark of their approbation, individuals who have thus, each in his sphere, gone hand in hand together, towards the perfection of Southern Astronomy, even had the labours of Mr. Dunlop been confined to the ordinary business of an observatory, or to observations with fixed instruments. But this is very far from having been the case. The nebulous, as well as the sidereal heavens, have occupied his attention; and in the prosecution of this most delicate and difficult branch of Astronomy, he has availed himself entirely of his own resources, in the most literal sense,—the instrument which he used being not simply his *own*, but the work of his own hands; and the observations being performed by him after the departure of Sir Thomas Brisbane from the colony, at a personal sacrifice of his private interests, and in the face of difficulties which would have deterred any one not animated with a real and disinterested love of science from their prosecution. The results of these observations have been the description and determination of the places of upwards of 600 nebulæ and clusters of stars. And when it is recollected that Lacaille was able to observe not more than about 40 or 50 of these curious objects, we may form some idea of the extent of this labour. In addition to these interesting results, Mr. Dunlop has amassed a copious and valuable collection of Southern double stars, which he is at present occupied in reducing and arranging; and a variety of interesting and curious particulars relative to the magnitudes, colours, and other peculiarities of all the more conspicuous single ones.

Shut out as we are by our geographical situation from the actual contemplation of these wonders, the astronomers of Europe may view, with something approaching to envy, the lot of these their more fortunate brethren. The feeling, if an unworthy, is, however, but a passing

passing one, and merges in that of admiration of their zeal and enterprise, and of gratitude for the information they have afforded us. In testimony of that admiration and that gratitude, on the part of this Society, towards Mr. Dunlop, I beg of you, Mr. South, to transmit to *him* also, this our medal, and to accompany it with the assurance that wheresoever his future fortunes may lead him—whether in the land which has already witnessed his meritorious labours—to complete and extend them, or in his native country, which is both able and willing to appreciate his value, to put the finishing stroke to the noble fabric he has been mainly instrumental in raising, by taking a leading part in the less exciting, but not less useful or indispensable work, of reducing the observations already made:—in either case he will be attended by our best wishes for his prosperity and happiness, and our confidence that science will continue to benefit by his exertions.

(*The President having quitted the chair, was succeeded by* Mr. South, *one of the Vice Presidents, who addressed the Meeting as follows:*—)

Gentlemen,—Our excellent President in his Address has informed you of the appropriation of two of your Gold Medals, since our last Anniversary:—a third, however, has been decreed by your Council; and when it is known that Miss Caroline Herschel is the individual to whom it stands adjudged, it is not difficult to determine why the President has avoided the slightest allusion to it.

But that your Council has not selected one from the many of its Members, infinitely more competent to do justice to the transcendent merits of that illustrious lady, is most assuredly matter of regret. I must, therefore, throw myself upon your indulgence, hoping that the goodness of the cause may in some measure compensate for the inability of its advocate.

The labours of Miss Herschel are so intimately connected with, and are generally so dependent upon, those of her illustrious brother; that an investigation of the latter is absolutely necessary ere we can form the most remote idea of the extent of the former: but when it is considered that Sir W. Herschel's contributions to Astronomical Science occupy sixty-seven Memoirs, communicated from time to time to the Royal Society, and embrace a period of forty years, it will not be expected that I should enter into their discussion. To the Philosophical Transactions, I must refer you, and shall content myself with the hasty mention of some of her more immediate claims to the distinction now conferred. To deliver an eulogy (however deserved) upon *his* memory is not the purpose for which I am placed here.

His first catalogue of new nebulæ and clusters of stars, amounting in number to one thousand, was made from observations with the 20-feet reflector, in the years 1783, -4, and -5. A second thousand were furnished by means of the same instrument in 1785,-6,-7, and -8; whilst the places of five hundred others were discovered between 1788 and 1802. But when we have thus enumerated the results obtained in the course of *sweeps* with this instrument, and taken into consideration the extent and variety of the other observations, which were

were at the same time in progress, a most important part yet remains untold. Who participated in his toils? Who braved with him the inclemency of the weather? Who shared his privations? A female.—Who was she? His sister.—Miss Herschel it was, who by *night* acted as his amanuensis. She it was whose pen conveyed to paper his observations as they issued from his lips; she it was who noted the right ascensions and polar distances of the objects observed; she it was who having passed the night near the instruments, took the rough manuscripts to her cottage at the dawn of day, and produced a fair copy of the night's work on the subsequent morning; she it was who planned the labour of each succeeding night; she it was who reduced every observation, and made every calculation; she it was who arranged every thing in systematic order; and she it was who helped him to obtain an imperishable name.

But her claims to our gratitude end not here; as an original observer she demands, and I am sure she has, our most unfeigned thanks. Occasionally her immediate attendance during the observations could be dispensed with. Did she pass the night in repose? No such thing; wherever her illustrious brother was, there you were sure to find her also. A sweeper planted on the lawn became her object of amusement, but her amusements were of the higher order, and to them we stand indebted for the discovery of the comet of 1786; of the comet of 1788; of the comet of 1791; of the comet of 1793; and of the comet of 1795, since rendered familiar to us by the remarkable discovery of Encke. Many also of the nebulæ contained in Sir Wm. Herschel's catalogues were detected by her during these hours of enjoyment. Indeed, in looking at the joint labours of these extraordinary personages, we scarcely know, whether most to admire the intellectual power of the brother, or the unconquerable industry of the sister.

In the year 1797, she presented to the Royal Society a catalogue of 560 stars taken from Flamsteed's observations, and not inserted in the British catalogue; together with a collection of errata that should be noticed in the same volume.

Shortly after the death of her brother, Miss Herschel returned to Hanover. Unwilling, however, to relinquish her astronomical labours whilst any thing useful presented itself, she undertook, and completed the laborious reduction of the places of 2500 nebulæ, to the 1st Jan. 1800, presenting in one view the results of all Sir Wm. Herschel's observations on those bodies; thus bringing to a close half a century spent in astronomical labour.

For this more immediately, and to mark their estimation of services rendered during a whole life to Astronomy, your Council resolved to confer on her the distinction of a medal of this Society. The peculiarity of our President's situation, however, and the earnest manner in which the feelings, naturally arising from it, were urged when the subject was first brought forward, caused your Council to pause,—and waive on that occasion the actual passing their proposed vote. The discussion was however renewed on Monday last; and although there was every disposition to meet the President's wishes, still under a conviction

conviction that the doing so would have been a dereliction of public duty, it was

Resolved unanimously, "That a Gold Medal of this Society be given to Miss Caroline Herschel, for her recent reduction, to January 1800, of the Nebulæ discovered by her illustrious brother, which may be considered as the completion of a series of exertions probably unparalleled either in magnitude or importance, in the annals of astronomical labour." A vote which I am sure every one whom I have the honour to address, will most heartily confirm.

Mr. Herschel.—In the name of the Astronomical Society of London, I present this Medal to your illustrious Aunt. In transmitting it to her, assure her that since the foundation of this Society no one has been adjudged, which has been earned by services such as hers. Convey to her our unfeigned regret that she is not resident amongst us; and join to it our wishes, nay, our prayers, that as her former days have been glorious, so her future may be happy.

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## PROCEEDINGS AT THE FRIDAY-EVENING MEETINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN.

March 21.—Mr. Millington entered into an experimental account of the manufacture of paper, principally with a view of introducing and explaining by working models the making of paper by machinery, and especially some recent and important improvements. Numerous specimens of paper, varying both in quality and size, were exhibited.

Some experiments on the deceptive appearances of coloured shadows were shown in the Library by Mr. Marshall; and a large meteoric stone having a very peculiar polished metallic fracture was laid with other things upon the table.

March 28.—Dr. Harwood gave an account of some parts of the structure and œconomy of the Greenland whale, during which he entered into the illustration of certain views relative to the blubber and the skin which he had been induced to entertain, from a close examination of these and other parts of the animal. The specimens obtained for illustration from the museums of the Zoological Society, by Mr. Brookes and Dr. Harwood, were very fine and numerous.

The library-tables were covered with objects of interest from the East, from the museum of Lady Raffles and the collection of Mr. Bennett.

April 18.—Mr. Ainger delivered an illustrated explanation of some recent improvements in clock and watch escapements, especially of the one invented by Mr. Hardy. The principles of the four great divisions of escapements were exhibited and distinguished.

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## OBITUARY:—SIR J. E. SMITH.

On Monday the 17th of March, died at his residence in Norwich, Sir James Edward Smith, President of the Linnæan Society. This distinguished botanist was born at Norwich, December 2nd, 1759; and to the locality of his birth we are probably to attribute his early predilection for Natural History, for here he fell in with some of the earliest

earliest and most devoted disciples of the great Linnæus. This city has for more than two hundred years been famous for its florists and botanists. Here lived and flourished Sir Thomas Browne, the author of "Vulgar Errors," and "The Garden of Cyrus, or the quincuncial, lozenge, or network Plantations of the Ancients, artificially, naturally, and mystically considered." A weaver of this commercial place claims the honour of having been the first person who raised from seed a *Lycopodium*; as a Manchester weaver was the first to flower one of our rarest *Jungermanniæ*. During the middle of the last century Mr. Rose, the author of the Elements of Botany, Mr. Pitchford, and Mr. Crowe, names familiar to every botanist, took the lead in botanical science in their native city; and instilled into the youthful mind of the future President an ardent attachment to their favourite pursuit, and a skill in discriminating species for which these gentlemen were so eminent. Having remained the usual time at a school in the city, he went in the year 1780 to the University of Edinburgh, where he distinguished himself by obtaining the gold medal given to the best proficient in botany.

Upon leaving Edinburgh he came up to London to finish his studies, and soon became acquainted with the late Sir Joseph Banks. This acquaintance, and the access it obtained for him to men of science, only riveted more firmly his ardent attachment to botany; and accordingly, we find Sir Joseph recommending him as early as 1783 to become the purchaser of the Linnæan collection. As this circumstance laid the foundation of the President's future fame, and is one of peculiar interest at the present moment, we shall detail the history of the transaction.

The younger Linnæus had died suddenly, November 1, 1783; and his mother and sisters, desirous of making as large a profit as they could by his Museum, within a few weeks after his death offered through a mutual friend the whole collection of books, manuscripts, and Natural History, including what belonged to the father as well as the son, to Sir Joseph Banks, for the sum of one thousand guineas. Sir Joseph declined the purchase, but strongly advised Sir James Smith to make it, as a thing suitable to his taste, and which would do him honour.

Sir James in consequence communicated his desire to become the purchaser to Professor Acrel, the friend of the family of Linnæus, and who seems to have conducted the negotiation with scrupulous honour. The owners now began to suspect they had been too precipitate; having received an unlimited offer from Russia, while also Dr. Sibthorpe was prepared to purchase it, to add to the treasures, already famous, of Oxford. They wished to break off their treaty with Sir James Smith; but the worthy Swedish Professor would not consent to it, and insisted on their waiting for his refusal.

In consequence of the subtraction of a small herbarium made by the younger Linnæus, and given to a Swedish baron to satisfy a debt he claimed, a deduction of one hundred guineas was made in the purchase-money; and in October 1784 the collection was received, in twenty-six great boxes, perfectly safe. The whole cost, including the

freight

freight, was 1029*l*. The duty was remitted on application to the Treasury. The ship which was conveying this precious treasure had just sailed, when the King of Sweden (Gustavus III.), who had been absent in France, returned, and hearing the story, sent a vessel in pursuit, but happily it was too late.

The collection consists of every thing possessed by the great Linnæus and his son relating to natural history and medicine. The Library contains about 2500 volumes. The old herbarium of the father comprehends all the plants described in the *Species Plantarum*, except perhaps about 500 species (*Fungi* and *Palmæ* excepted), and it had then perhaps more than 500 undescribed.

The herbarium of young Linnæus appears to have had more attention bestowed upon it, and is on better paper. It consists of most of the plants of his *Supplementum*, except what are in his father's herbarium, and has besides about 1500 very fine specimens from Commerson's collection, from Dombey, La Marc, Pourrett, Gouan, Smeathman, Masson, &c., and a prodigious quantity from Sir Joseph Banks, who gave him duplicates of almost every one of Aublett's specimens, as well as of his own West Indian plants, with a few of those collected in his own voyages round the world.

The insects are not so numerous; but they consist of most of those that are described by Linnæus, and many new ones. The shells are about thrice as many as are mentioned in the *Systema Naturæ*, and many of them very valuable. The fossils are also numerous, but mostly bad specimens and in bad condition.

The number of the MSS. is very great. All his own works are interleaved with abundance of notes, especially the *Systema Naturæ*, *Species Plantarum, Materia Medica, Philosophia Botanica, Clavis Medicinæ*, &c. There are also the *Iter Lapponicum* (which was afterwards published), *Iter Dalecarlicum*, and a Diary of the Life of Linnæus for about thirty years of his life. The letters to Linnæus, (from which a selection was also published by the President,) are about three thousand.

This splendid acquisition at once determined the bent of the proprietor's studies. He considered himself, as he has declared, a trustee only for the public, and for the purpose of making the collection useful to the world and to natural history in general. How well he has fulfilled this trust will appear from the sequel. He had no sooner obtained quiet possession than he began to fulfill his engagement, for we find him in the year 1785 making his first appearance as an author, by translating the Preface to the *Museum Regis Adolphi Friderici* of Linnæus, being succinct and admirable reflections on the study of nature.

In the year 1786 he prepared himself for an extensive tour on the Continent; in which his chief object was to examine into the state of natural history in the different cities and towns he might pass through, not neglecting the incidents, especially the fine arts, which usually engage the attention of travellers. At Leyden he graduated in medicine; but it does not appear that he tarried there a longer time than was necessary for this purpose. On this occasion he published

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his Thesis *De Generatione.* The "Sketch of a Tour on the Continent," though long superseded as a companion to the tourist, is still curious to the naturalist, as showing the state of science at that time. It contains, too, a fund of good sense expressed with facility; and to those who enjoyed the acquaintance and friendship of the author, will always remain valuable, as furnishing the truest image of his mind, reviving his liberal opinions in their recollection, and his easy and elegant manner of communicating them.

In the year 1788, when he had returned and was settled in London, he with some other naturalists projected the establishment of the Linnæan Society, which had for its object the cultivation of Natural History in all its branches, and especially that of Great Britain. This Society, which has grown now into considerable importance, was a scion of the Royal Society, and had its origin in the jealousy which some of the members of the parent Society entertained of the preference which they alleged was given to Natural History in their Transactions; while its then President was thought to favour the subject, to the exclusion of others of equal, if not of greater importance. There are still some who recollect the argumentative and vehement eloquence by which this side of the question was supported by a reverend Prelate.

It was during this stormy period that Sir James Smith, in conjunction with the late Bishop of Carlisle, Sir Joseph Banks, and others, laid the foundation-stone of the Linnæan Society. Its first meeting was held April 8, 1788. The Society then consisted of fifty Fellows, and about twice as many more Foreign Members, Dr. Smith being the first President, Dr. Goodenough the first Treasurer, and Mr. Marsham the first Secretary. Of these original Fellows, how few are left! and of those who are, their hoary locks, still seen occasionally at the meetings of the Society, remind us of the respect and gratitude we owe to them as fathers. May their declining years derive consolation from the success of this their early project!

At the first Meeting, the President delivered a Discourse, judicious and appropriate, On the Rise and Progress of Natural History. We find him also about this time producing a paper which was read before the Royal Society, entitled Observations on the Irritability of Vegetables. It chiefly regards the mode of impregnation in the Barberry; and attracted considerable attention at the time, being translated into other languages, and appearing in different publications.

The next considerable work which we find him undertaking, is the re-publication of the wooden blocks of Rudbeck, which had fallen into his hands with the Linnæan collections. Linnæus was possessed of about 120 of these blocks, which had escaped the fire at Upsal, where almost the whole impression of the second volume, and all but three copies of the first, were burnt. As Rudbeck was the founder of a school at Upsal destined afterwards to give laws to the rest of the world, the re-publication of this fragment of his great work was a tribute of gratitude to his profound and varied learning.

From 1789 to 1793, our author was engaged in various publications relating to his favourite science. Most of them terminated in being

being only fragments, for want of patronage by the public. Such were his *Plantarum Icones hactenus ineditæ; Icones pictæ Plantarum rariorum; Specilegium Botanicum;* and "Specimens of the Botany of New Holland." One of these literary projects, "English Botany," however, did not suffer the shipwreck experienced by the others, but has received the encouragement it deserved. This is not attributable to its execution being superior to the other works which have failed, but because it treats of the plants of our own country, in which all are interested. It has the singular merit of being the only national Flora which has given a figure and description of every species native to the country whose productions it professes to investigate; and while other works of a similar kind have enjoyed the patronage of foreign Crowns, and have even been supplied with funds to carry them forward in their tardy progress, this work has been rendered complete by the patronage of the public alone, and was brought to a successful termination in 1814, by the united efforts of the President of the Society, and of Mr. Sowerby, the draughtsman and engraver.

In the year 1793 appeared in the Memoirs of the Academy of Turin, of which he was a Member, his essay *De Filicum Generibus dorsiferarum,* and which was republished in English in his "Tracts on Natural History."

Soon after our author's marriage in 1796 he removed to Norwich, his native place, where he continued to reside, paying occasional visits to London, for the remainder of his life.

The next considerable work upon which the reputation of our author is built, is the *Flora Britannica,* which appeared in the years 1800—1804. It is remarkable, like all his other labours, for accuracy in observing, accuracy in recording, and unusual accuracy in printing. It comprises descriptions of all the phænogamous plants, of the *Filices,* and the *Musci;* and every species has been carefully collated with those which Linnæus described. Being written in the Latin language, the information is condensed into a small compass; while it has the rare advantage of having had every synonym compared with the original author.

The *Compendium Floræ Britannicæ* has gone through four editions, and is become the general text-book of English botanists. It is perhaps the most complete example of a manual furnished on any subject.

While he was engaged in the *Flora Britannica,* the executors of the late Professor Sibthorpe selected him as the fittest person to engage in editing the splendid posthumous work of that liberal patron of science; a task for which the unrivalled attainments of the President, and his personal friendship with the Professor, peculiarly qualified him. The drawings which were made by Ferdinand Bauer, and the letter-press which was written by Sir James Smith from scanty materials furnished by Dr. Sibthorpe, are both worthy of so munificent an undertaking. To complete the work, which is to consist of ten folio volumes, and 100 coloured plates in each, Dr. Sibthorpe bequeathed a freehold estate at South Leigh, in Oxfordshire; which, after the completion, is to be charged with the support of a Professor of Rural Economy in the University of Oxford.

The Introduction to Physiological and Systematic Botany has also been a most successful publication, having passed through five editions. It is indebted for its popularity to a happy method which the author has of communicating knowledge, to the good taste he every where displays, and to that just mixture of the *utile* with the *dulce*, which he knew so well how to apportion.

In 1810 appeared his Tour to Hafod, the seat of his old and accomplished friend Thomas Johnes, Esq.

In 1814 he received the honour of knighthood from the hands of his present Majesty, on the occasion of his Majesty consenting to become the patron of the Linnæan Society, and granting them a charter.

About 1818 the Professor of Botany at Cambridge encouraged the President to offer himself for the Professorship of that University. He obtained the countenance of many of the heads of houses, but unfortunately it terminated in a controversy on his religious opinions, which he could not, and never would, compromise. It produced two small tracts from his pen, which at least show that he was not disqualified by the absence of the most charitable spirit.

In 1812 his Grammar of Botany appeared; and in the same year, a "Selection of the Correspondence of Linnæus and other Naturalists."

During a large portion of his literary life, he was in the habit of writing articles for Dr. Rees's *Cyclopædia* on different subjects in botany and biography connected with it. Many of these biographical memoirs are choice morsels of original information; and we need only refer to the words Curtis, Linnæus, Hudson, and Sibthorpe, in justification of our assertion. Most of his articles will be found marked with the letter S, it being his undeviating rule never to publish any thing on anonymous authority in science. Even some Reviews which he had written early in life, he afterwards avowed by republishing them in his "Tracts."

The second volume of the Supplement to the *Encyclopædia Britannica* is indebted to our author's pen for a Review of the Modern State of Botany, an article which supplies some deficiencies in his Introduction, though chiefly an abridgement of the *Prælectiones* of Linnæus, as published by Giseke.

During the whole of his literary career he occasionally contributed papers to the Linnæan Transactions.—But the last and best work of the distinguished President is the "English Flora," consisting of four volumes octavo, and describing the phænogamous plants and Ferns of Great Britain, though its title might imply a more limited range. *Finis coronat opus.* There is no Flora of any nation so complete in flowering species, and none of any country in which more accuracy and judgement are displayed. If any person should in future contemplate a work of this kind,—whatever the originality of his information, whatever the novelty of his subject,—let him imitate this illustrious author in careful remark, in taking nothing upon trust, in tracing every synonym to its source, and lastly in arranging his matter in such a manner, by the aid of different types, as shall render it easy of reference,

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ence, and point out at a glance the nature of it. However mechanical some of this may appear, it is absolutely essential to be attended to in Natural History, where the subjects are infinite in number, and where aid must be derived from every mode of generalizing particulars.

In summing up the scientific character of the deceased, it may be comprised in a few words. As a naturalist, he contributed greatly to the advancement of science; and stood pre-eminent for judgement, accuracy, candour, and industry. He was disposed to pay due respect to the great authorities that had preceded him, but without suffering his deference for them to impede the exercise of his own judgement. He was equally open to real improvement, and opposed to the affectation of needless innovation. He found the science of Botany when he approached it, locked up in a dead language,—he set it free by transfusing into it his own: he found it a severe study fitted only for the recluse,—he left it of easy acquisition to all. In the hands of his predecessors, with the exception of his immortal master, it was dry, technical, and scholastic; in his, it was adorned with grace and elegance, and might attract the poet as well as the philosopher.

## LXI. *Intelligence and Miscellaneous Articles.*

### NEW MINERALS.

PROF. BREITHAUPT (Schweigger's *Journal der Chemie und Physik*, N. S. vol. xx. p. 314, &c.) gives a description of the following new mineral species:

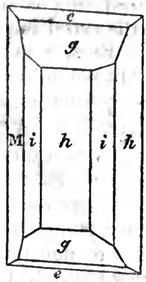
I. *Karphosiderite.* Name derived from the straw-yellow colour. Reniform masses, rarely from granular composition, uneven; shining and glimmering in the streak, with resinous lustre. Colour and streak pale and high straw-yellow. Hardness = 4·0...4·5. Sp. gr. = 2·5. Feels greasy. Before the blowpipe upon the coal it becomes black, and melts in a strong fire into a globule which is attractible by the magnet. In glass of borax it is easily soluble, and with salt of phosphorus it melts into a black scoria. It contains oxide of iron, phosphoric acid, water, with small quantities of oxide of manganese and zinc. It has a great similarity to oxalite, yellow iron-ore, or iron-sinter. It occurs in Greenland.

II. *Mesitine-spar.* Name derived from μεσιτης, that is, what stands in the middle (of brachytypous lime-haloide, and brachytypous parachrose baryte). Rhombohedral, R — ∞. R = 107° 14′ R + ∞. Cleavage distinct, parallel to R. Lustre vitreous. Colour dark-grayish, and yellowish-white... yellowish-gray. Streak white. Transparent...translucent. Hardness = 4. Sp. gr. = 3·34...3·37. Before the blowpipe the mesitine-spar decrepitates. In muriatic and nitric acid a feeble effervescence takes place, but it is entirely soluble. It contains probably magnesia, lime, protoxide of iron, and oxide of manganese. It is found in little crystals, in rhombohedral quartz at Traversella in Piemont.

III. *Tauto-*

III. *Tautolite.* Prismatic. Fundamental form, scalene four-sided pyramid.

$$a:b:c = 1:1\cdot9451:1\cdot3648.$$




Observed combinations:

1, $M = \infty a : b : c = 109° 46'$.
$g = 4a : b : \infty c = 51° 52'$.
$h = \infty a : \infty b : c$ fig. 9 of Mohs's treatise,
2, M; $g$; $h$;
$i = \infty a : \frac{3}{4} b : c = 86° 22'$.
$e = 2a : b \infty c = 88° 25'$.

Cleavage, only in traces and interrupted, parallel to M and $h$. Fracture conchoidal...uneven. Lustre vitreous. Colour velvet-black. Streak gray. Opaque. Hardness = 6·5...7·0. Sp. gr. = 3·865. Before the blowpipe, upon charcoal, the tautolite melts to a blackish scoria, which is attracted by the magnet; with borax it melts to a clear green glass. These and other reactions show that the mineral consists of silica, black protoxide of iron, magnesia, and alumina. It is found in the volcanic feldspath-rocks, in the neighbourhood of the Lake Laach Sea in Rhein-Prussia. The tautolite seems to be related to the chrysolite, as the Ceylanite to the Spinelle.—Brewster's *Journal of Science.*

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## METEOROLOGICAL OBSERVATIONS FOR MARCH 1828.

### *Gosport.—Numerical Results for the Month.*

Barom. Max. 30·30 Mar. 16. Wind W.—Min. 29·02 Mar. 21. Wind S.W.
Range of the index 1·28.
Mean barometrical pressure for the month .......................... 29·879
Spaces described by the rising and falling of the mercury........... 5·640
Greatest variation in 24 hours 0·580.—Number of changes 17.
Therm. Max. 63° on several days.—Min. 30° March 6. Wind NW.
Range 33°.—Mean temp. of exter. air 47°·92. For 30 days with ☉ in ♓ 49·72
Max. var. in 24 hours 23°·00—Mean temp. of spring water at 8 A.M. 50°·86

### De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the morning and evening of the 16th 100°
Greatest dryness of the air in the afternoons of the 19th & 24th... 44
Range of the index........................................................ 56
Mean at 2 P.M. 58°·4—Mean at 8 A.M. 73°·0—Mean at 8 P.M. 71·4
—— of three observations each day at 8, 2, and 8 o'clock......... 67·6
Evaporation for the month 1·90 inch.
Rain near ground 1·755 inch.—Rain 23 feet high 1·620 inch.

### *Summary of the Weather.*

A clear sky, 4½; fine, with various modifications of clouds, 15; an overcast sky without rain, 7½; foggy 1; rain, 3.—Total 31 days.

### *Clouds.*

| Cirrus. | Cirrocumulus. | Cirrostratus. | Stratus. | Cumulus. | Cumulostr. | Nimbus. |
|---|---|---|---|---|---|---|
| 12 | 6 | 24 | 1 | 19 | 25 | 15 |

### *Scale of the prevailing Winds.*

| N. | N.E. | E. | S.E. | S. | S.W. | W. | N.W. | Days. |
|---|---|---|---|---|---|---|---|---|
| 3½ | 2½ | 1 | 3 | ½ | 6 | 5½ | 9 | 31 |

General

*General Observations.*—This month commenced with cold weather, which kept up till the 7th; and it was also cold and humid from the 23rd to the end: the other part was mild for the season, the maximum temperature of the external air in the day often being at 63°.

Rain, frequently accompanied with hail, fell on twelve different days, but the quantity does not amount to two inches.

On the 14th, 15th, and 16th, thick fogs prevailed here.

In the afternoon of the 22nd there were two winds, the lower one from N.W., and the upper one from the South, which, uniting the clouds, gradually brought on several vivid flashes of lightning, and loud claps of thunder, which were immediately followed by a heavy shower of hailstones coated with a snowy substance. Soon after 5 o'clock in the morning of the 29th, a smart shower of snow fell, which covered the ground and the surrounding hills for a short time only, as it was soon afterwards succeeded by rain. In the evening of the 30th it again snowed a little, but it disappeared before the morning.

After the vernal equinox winter revisited us in keen northerly and easterly winds in the days, and thick hoar frosts in the night; so that winterly weather was felt here some weeks after the beginning of spring.

The mean temperature of the air this month is about three degrees higher than the mean of March for the last twelve years. The temperature of spring water is nearly at a stand; and the ground is now warmer than it has been at the close of March since 1822. But this additional heat does not appear to have modified the prevailing chill in the atmosphere, it being either too small, or dissipated by evaporation, before it could arrive at a sufficient altitude to have any effect.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one solar and one lunar halo, one meteor; lightning and thunder in the afternoon of the 22nd, and nine gales of wind, or days on which they have prevailed; namely, two from the North, two from North-east, one from South-east, one from West, and three from North-west.

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REMARKS.

*London.*—March 1, 2. Cold and cloudy. 3—5. Fine. 6. Clear and cold. 7. Cloudy: showers at night. 8. Hazy: fine. 9, 10. Very fine. 11. Slight fog: very fine. 12. Cloudy. 13—15. Very fine. 16, 17. Cloudy. 18. Fine: stormy at night. 19. Very fine. 20, 21. Cloudy: with showers of hail. 22. Cold with slight showers. 23—25. Clear and cold. 26. Cloudy: rain at night. 27. Rain in morning: fine. 28. Drizzly: cloudy. 29. Cloudy and cold. 30. Cloudy. 31. Foggy: very fine.

*Boston.*—March 1. Cloudy: rain P.M. 2, 3. Cloudy. 4. Cloudy: stormy night. 5. Fine. 6. Fine: snow A.M. 7. Cloudy: rain P.M. 8. Cloudy. 9—11. Fine. 12, 13. Cloudy. 14, 15. Fine. 16—18. Cloudy. 19. Stormy. 20. Cloudy: rain at night. 21. Cloudy: rain P.M. 22. Fine: rain at night. 23. Fine. 24. Fine: rain P.M. 25, 26. Cloudy. 27. Stormy and rain: rain A.M. 28. Cloudy. 29. Fine: rain A.M. 30. 31. Fine.

*Penzance.*—March 1. Clear: fair. 2. Fair: clear. 3—6. Fair. 7. Fair: showers. 8. Clear. 9. Fair. 10. Fair: misty. 11. Misty: rain. 12—14. Fair. 15. Foggy. 16, 17. Fair. 18. Clear: rain. 19. Rain: fair. 20. Rain: showers. 21, 22. Hail-showers. 23. Hail and snow. 24, 25. Clear: hail-showers. 26. Cloudy: rain. 27. Cloudy: clear. 28. Cloudy: showers. 29, 30. Clear: showers. 31. Clear.—Rain-gauge ground level.

*Meteoro-*

*Meteorological Observations made at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.*

| Days of Month, 1828. | Barometer. | | | | | | | Thermometer. | | | | | | | Wind. | | | | Evap. | Rain. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | London. | | Penzance. | | Gosport. | | Boston | London. | | Penzance. | | Gosport. | | Bost. | Lond. | Penz. | Gosp. | Bost. | Gosp. | Lond. | Penz. | Gosp. | Bost. |
| | Max. | Min. | Max. | Min. | Max. | Min. | 8½ A.M. | Max. | Min. | Max. | Min. | Max. | Min. | 8½ A.M. | | | | | | | | | |
| ○ Mar. 1 | 30·315 | 30·091 | 30·28 | 30·26 | 30·24 | 30·08 | 29·75 | 49 | 42 | 50 | 46 | 54 | 44 | 44 | NW. | N. | NW. | calm | ... | ... | ... | 0·015 | ... |
| 2 | 30·121 | 30·083 | 30·08 | 30·06 | 30·04 | 30·02 | 29·62 | 49 | 33 | 50 | 44 | 50 | 36 | 46 | NW. | NE. | N. | N. | ... | ... | ... | ... | 0·03 |
| 3 | 30·028 | 29·981 | 30·06 | 30·00 | 30·01 | 29·83 | 29·50 | 50 | 40 | 50 | 44 | 54 | 40 | 42 | NW. | N. | NW. | calm | 0·15 | ... | ... | ·030 | ... |
| 4 | 29·967 | 29·862 | 30·02 | 30·00 | 29·94 | 29·80 | 29·40 | 50 | 36 | 52 | 46 | 53 | 42 | 41·5 | NW. | N. | NW. | calm | ... | ... | ... | ... | ... |
| 5 | 29·891 | 29·731 | 29·90 | 29·88 | 29·75 | 29·69 | 29·30 | 46 | 28 | 48 | 44 | 54 | 31 | 40 | N. | NW. | N. | W. | ... | ... | ... | ... | ... |
| 6 | 30·181 | 29·964 | 30·03 | 29·96 | 30·07 | 29·91 | 29·55 | 39 | 27 | 44 | 39 | 42 | 30 | 31 | N. | NE. | N. | NW. | ... | ... | ... | ... | ... |
| 7 | 30·281 | 30·164 | 30·12 | 30·10 | 30·18 | 30·13 | 29·84 | 42 | 40 | 49 | 38 | 48 | 45 | 33 | NW. | NW. | NW. | calm | ·20 | ·10 | ... | ·100 | ... |
| 8 | 30·271 | 30·110 | 30·14 | 30·12 | 30·22 | 30·06 | 29·70 | 59 | 47 | 53 | 44 | 59 | 45 | 50 | W. | NW. | NW. | calm | ... | ... | ... | ... | ·22 |
| ☾ 9 | 30·301 | 30·260 | 30·20 | 30·19 | 30·22 | 30·22 | 29·72 | 61 | 42 | 54 | 45 | 58 | 45 | 50 | SW. | W. | SW. | calm | ... | ... | ... | ... | ... |
| 10 | 30·278 | 30·144 | 30·19 | 30·18 | 30·23 | 30·20 | 29·63 | 61 | 38 | 55 | 45 | 57 | 44 | 52 | W. | W. | SW. | W. | ·20 | ... | ... | ... | ... |
| 11 | 30·284 | 30·252 | 30·18 | 30·08 | 30·22 | 30·17 | 29·75 | 62 | 48 | 53 | 45 | 60 | 44 | 50·5 | SW. | S. | NW. | W. | ... | ... | ... | ... | ... |
| 12 | 30·105 | 30·073 | 29·99 | 29·98 | 30·06 | 30·02 | 29·65 | 58 | 41 | 54 | 46 | 59 | 46 | 44·5 | SW. | W. | SW. | NW. | ... | ... | 0·090 | ... | ... |
| 13 | 30·220 | 30·163 | 30·04 | 30·00 | 30·17 | 30 14 | 29·58 | 61 | 47 | 59 | 47 | 60 | 48 | 49·5 | W. | W. | SW. | calm | ·15 | ... | ... | ... | ... |
| 14 | 30·339 | 30·298 | 30·18 | 30·14 | 30·21 | 30·20 | 29·71 | 63 | 40 | 54 | 48 | 59 | 44 | 53·5 | W. | S. | W. | W. | ... | ... | ... | ... | ... |
| ● 15 | 30·395 | 30·385 | 30·28 | 30·18 | 30·29 | 30·24 | 29·83 | 67 | 42 | 54 | 47 | 63 | 41 | 51 | S. | W. | SE. | W. | ... | ... | ... | ... | ... |
| 16 | 30·379 | 30·224 | 30·28 | 30·26 | 30·30 | 30·19 | 29·73 | 62 | 48 | 54 | 47 | 59 | 43 | 53 | SW. | W. | W. | calm | ·18 | ... | ... | ·010 | ·03 |
| 17 | 30·195 | 30·167 | 30·18 | 30·16 | 30·12 | 30·12 | 29·62 | 61 | 45 | 54 | 48 | 60 | 49 | 53 | W. | NW. | W. | calm | ... | ... | ... | ... | ... |
| 18 | 30·213 | 29·760 | 30·20 | 30·00 | 30·12 | 29·89 | 29 68 | 58 | 48 | 54 | 44 | 63 | 48 | 47 | W. | W. | NW. | W. | ... | ... | ... | ·150 | ... |
| 19 | 29·607 | 29·498 | 29·80 | 29·74 | 29·54 | 29·53 | 28·87 | 55 | 38 | 52 | 46 | 63 | 40 | 49·5 | SW. | NW. | NW. | W. | ·25 | ... | 0·280 | ... | ... |
| 20 | 29·410 | 29·180 | 29·50 | 29·30 | 29·46 | 29·15 | 28·91 | 53 | 40 | 53 | 45 | 54 | 48 | 47 | SW. | NW. | W. | calm | ... | ·06 | 0·080 | ·150 | ... |
| 21 | 29·151 | 28·997 | 29·20 | 29·20 | 29·16 | 29·02 | 28·65 | 55 | 35 | 48 | 44 | 56 | 44 | 41 | NW. | NW. | SW. | calm | ... | ·09 | 0·390 | ·140 | ·04 |
| 22 | 29·326 | 29·287 | 29·34 | 29·30 | 29·30 | 29·23 | 28·84 | 48 | 30 | 48 | 37 | 54 | 39 | 42 | W. | NW. | NW. | W. | ·16 | ·03 | 0·230 | ·020 | ·06 |
| ☽ 23 | 29·569 | 29·387 | 29·50 | 29·42 | 29·48 | 29·37 | 28·95 | 48 | 32 | 48 | 34 | 53 | 36 | 41 | N. | NE. | NW. | NW. | ... | ... | 1·010 | ... | ·05 |
| 24 | 29·810 | 29·679 | 29·87 | 29·70 | 29·66 | 29·58 | 29·21 | 47 | 27 | 47 | 40 | 54 | 33 | 39·5 | NE. | NE. | N. | NW. | ... | ... | 0·105 | ... | ... |
| 25 | 29·906 | 29·861 | 29·88 | 29·88 | 29·83 | 29·75 | 29·45 | 42 | 24 | 47 | 38 | 52 | 33 | 41 | E. | NE. | NE. | NE. | ·18 | ... | ... | ... | ·05 |
| 26 | 29·900 | 29·763 | 29·73 | 29·42 | 29·79 | 29·64 | 29·52 | 47 | 38 | 46 | 38 | 49 | 31 | 42·5 | SE. | SE. | SE. | E. | ... | ·12 | 0·405 | ·380 | ... |
| 27 | 29·509 | 29·470 | 29·35 | 29·30 | 29·41 | 29·36 | 29·20 | 55 | 36 | 50 | 41 | 52 | 43 | 41·5 | SE. | E. | SE. | E. | ... | ·14 | 0·360 | ·500 | ... |
| 28 | 29·778 | 29·553 | 29·75 | 29·50 | 29·66 | 29·38 | 29·25 | 50 | 28 | 49 | 43 | 59 | 42 | 43 | SE. | NE. | NE. | E. | ·20 | ·02 | ... | ·150 | ·09 |
| 29 | 29·944 | 29·865 | 29·80 | 29·90 | 29·78 | 29·74 | 29·46 | 45 | 31 | 48 | 40 | 50 | 36 | 38 | SE. | NW. | NE. | NW. | ... | ·03 | 0·200 | ·110 | ... |
| 30 | 30·223 | 29·908 | 30·06 | 30·00 | 30·05 | 29·90 | 29·65 | 49 | 26 | 50 | 36 | 48 | 35 | 42 | N. | E. | E. | calm | ... | ... | 0·170 | ... | ·10 |
| ○ 31 | 30·388 | 30·303 | 30·20 | 30·20 | 30·27 | 30·17 | 29·90 | 54 | 27 | 52 | 38 | 52 | 38 | 43·5 | NE. | S. | SE. | calm | ·23 | ... | 0·125 | ... | ... |
| Aver.: | 30·019 | 29·886 | 30·28 | 29·20 | 30·30 | 29·02 | 29·46 | 53 | 37 | 59 | 34 | 63 | 30 | 44·6 | | | | | 1·90 | 0·59 | 3·445 | 1·755 | 0·67 |

THE

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[NEW SERIES.]

*JUNE* 1828.

## LXII. *On the Artificial Production of Cold.* *By* RICHARD WALKER, *Esq. of Oxford.*

*To the Editors of the Philosophical Magazine and Annals.*

Gentlemen,

IT is now forty-one years since my discoveries on the "artificial production of cold" were first made public by their appearance in the Philosophical Transactions for the year 1787, and several succeeding volumes. Passing over what has already been published respecting them, I shall proceed to a detail of a few other circumstances as a kind of appendix, which I have for several seasons intended to offer for publication, had not other matters, as professional avocations and professional communications, too much engaged my attention to allow of it.

Immediately on the announcement of the discoveries as above stated, I received various proposals from respectable persons respecting their practical utility in this country. I answered these by a declaration that wherever natural ice could be obtained and preserved, this must ever supersede the use of the artificial means alluded-to. It is true that I had an eye to their application in hot climates, as between the tropics; and so soon as my experiments became public, a treatise on the diseases of tropical climates appeared from the pen of Dr. Moseley, who fixed upon one, which he considered the most appropriate, and strongly recommended its adoption as a very valuable acquisition as well in a medicinal point of view, as a luxury.

Relinquishing, from various causes, the design of applying them myself to any such purpose, I took care however to point out, in my original communications, the complete efficiency of them for such intention to their utmost extent, and the best mode, as it appeared to me, of applying them in hot climates.

Understanding, a few summers ago, that a manufactory had been established for preparing ice-creams, as well without the use of ice, as with it; and likewise for making for sale an apparatus for the purpose,—I was induced to visit it. I examined the apparatus,—a very appropriate one for the purpose, and likewise the freezing powder, which I instantly recognized to be the weakest in power of my various compositions for the purpose, but possessing the advantage of being readily recovered repeatedly for the same purpose with undiminished effect. This powder, by its taste and appearance, I found to be a mixture of sal ammoniac and nitre, which I was informed was repeatedly recoverable in a fit state for refrigeration. I originally exerted every effort, in vain, to increase its power by the addition of a third ingredient, possessing likewise the advantage merely by evaporation to dryness, of being repeatedly recovered for the same use. This powder, as related in my original communications, consists of equal parts by weight of sal ammoniac and nitre. By way of test, I recovered it by evaporation twelve times, without any abatement of its efficacy, as originally stated*.

It is unnecessary to enter into a description of the apparatus just mentioned, or the principle and mode of its application, especially as the whole is embraced in the following statement.

A circumstance occurred here (at Oxford) which occasioned the method to be put to the test of useful application. A confectioner, happening in a scarce season to be unprovided with natural ice, applied to me for assistance. I assured him that in the large way (as I have stated in my original communications) the best method was to freeze water first, and then to use the ice in the usual way for freezing creams. Accordingly an apparatus of large dimensions, of rather an oblong form, was made of tin (fitter for the purpose if cased with wood) consisting of channels so constructed that the water to be frozen should be subjected to the freezing mixture on both sides. This, properly prepared, was placed in a cool cellar during the night, and early in the morning (the temperature in the open air in the shade in the day-time being above 80°) the ice was collected, which amounted to several pounds in weight. This ice, which was as limpid as the finest flint-glass, was applied in the usual way, and with the apparatus ordinarily used by confectioners for the purpose of freezing creams, and the mixed

* The manufactory alluded-to is at "No. 41, New Bridge-street, Blackfriars, London (late Patterson's), now Armstrong's."

powder,

powder, of which he had procured an adequate quantity, repeatedly recovered by evaporation over his hot iron plates, for fresh use.

I shall now present the immediate object of my present communication; viz. what I consider to be the best mode and fittest apparatus for cooling wine in summer, for freezing creams in the small way for private use, and likewise for freezing a small portion of water, merely as an experiment for public or private exhibition.

The drawing annexed (Plate VII.) is designed to represent on a small scale the construction and exact proportions of each freezing apparatus, and likewise the construction and form of the apparatus for cooling wine.

Fig. 1. is an apparatus for freezing water on the smallest scale, as above mentioned, in the hottest weather. The vessel for containing the freezing mixture is three inches and a half in width, and its height equal in measure to its width; and the tube for containing the water to be frozen five-eighths of an inch in width, and reaching, as represented, very near to the bottom of the vessel: there is likewise a rim or continuation of the vessel, without a bottom, to insulate it from the table or stand it rests upon. The apparatus itself consists of two parts; viz. the vessel for containing the freezing mixture, and its cover, in one piece with the tube, fitting close over it (represented together in the drawing). When the water is frozen, upon taking off the cover and wiping the tube, the solid ice will have become detached by the heat, and on inverting it drop out.

The process may be known to be completed by the going off or melting of the hoar-frost which exhibits a curious appearance outside the apparatus.

Fig. 2. consists of an apparatus in one piece; viz. the vessel for containing the cooling mixture, and the cup or can (if I may so call it) for receiving the decanter, its top rising somewhat above the height of the vessel for an obvious reason, with a cover that will admit of easy removal (in the drawing represented together). This apparatus likewise has an appendage or rim like the former, to insulate it from the table:—it may be convenient to be possessed of a couple of these.

Fig. 3. The apparatus for freezing creams, in which the freezing mixture is to act on both surfaces of the part containing it, as being more œconomical and expeditious, is not so simple. This however consists only of two parts; viz. the vessel for containing the freezing mixture; and a cover, to which is attached, in the same piece (instead of a tube or cup as in fig. 1.), a concentric annular cavity or chamber, in which the

 prepared

prepared cream is to be frozen: this cavity, forming a circle within the vessel itself, is open at the top, as represented, and of course closed at the bottom, and reaching very nearly (as the tube in fig. 1.) to the bottom of the vessel: this secondary part, as likewise represented, fits close as in fig. 1. over the vessel containing the freezing mixture. The proportions of the apparatus when together are thus: The outer space in width, two parts all round; the middle space, or that which contains the cream, one part all round; and the inner space three parts in width,—this serving as a general scale of proportions for an apparatus of any size. The proportions for an efficient apparatus, as my own, may be: for the first space ten-eighths of an inch (one inch and one-fourth); for the second, five-eighths of an inch; and for the third space, fifteen-eighths, or rather two inches, making the width of the apparatus itself somewhat above five inches and a half; its height being equal to its width, a projecting rim at the bottom likewise to insulate it from the table. It will be perceived that in the figure there are seven very small holes or apertures in the central part of this cover (one in the centre and six round at due distances), just sufficient for the escape of the air, to admit of the ascent of the freezing mixture in the middle part of the vessel. This apparatus is somewhat elevated at the top, or slightly convex, and the part in which the apertures are placed guarded by a shallow rim to prevent an accidental running-over of the mixture into the part containing the cream. This apparatus should be furnished (as expressed in the figure) with an outer cover similar, but less elevated, to the one at fig. 2. Previously to use it will be proper to ascertain the quantity of liquid the apparatus will contain when together, and mark its height; likewise the proportion of the ingredients for furnishing a given quantity in measure should be known. Thus, if the three salts are used (which I would recommend to a private individual, always doing so myself, although these cannot be recovered for future use, but being more efficacious than the two only) for each pint, small or old measure, will be required of sal ammoniac and nitre (each equal parts by weight reduced together into fine powder) six ounces, and of Glauber's salt, in clear crystals and dry, four ounces and a half, freely reduced to fine powder, or kept from the access of air, and in a separate parcel from the former; and water ten ounces, or enough to make up one pint in measure when added to the former ingredients:—of course, the whole must be well stirred together, and expeditiously, before introducing that part of the apparatus which contains the article to be frozen, and occasionally afterwards, till the object is completed, avoiding as much as possible

ble any accidental accession of heat. A freezing mixture composed of sal ammoniac and nitre with water, all at the temperature of 50°, to which temperature, or nearly so, they may all be reduced by water from a pump by drawing off a sufficient quantity first, will from 50° produce a cold of 22° below the freezing point, and with the addition of Glauber's salt to 28°. The confectioners find a degree of cold at 12° or 15° below the freezing point sufficient for their purpose; but it must be recollected that the cold produced by salts dissolved in water is not so durable as with ice and salt; the duration of the refrigerating power in the above mixtures will of course be in proportion to the quantity and thickness of the apparatus. In the way the confectioner managed, the mixture in the apparatus retained its freezing property till the morning: my usual way is, in extreme hot weather, to place the vessel containing the powdered salts in the coldest water drawn from the pump previously; but in the ordinary way it will suffice to add the cold water without the above precaution: it may be advisable to be provided with a second quantity of the ingredients to preserve the cold by a renewal of the mixture. The drawings are taken from an apparatus of each kind of my own,—they are made of tin, for want here of a fitter material, and are painted outside of a grass-green colour. The confectioner abovementioned laid in a stock of a hundred weight of each of the articles; viz. sal ammoniac and nitre; the former at the rate of one shilling per pound, and the nitre at fourpence—which of course when mixed, was at the moderate price of only eightpence per pound. Glauber's salts may be procured in the large way at the rate of about twopence per pound, and by the single pound at fourpence. The apparatus abovementioned may be only half or three parts filled for use; care must be taken in every instance that the surface of the subject to be acted upon be rather below the surface of the freezing mixture.

For cooling wine, the coldest water drawn from a pump will be quite sufficient; however, if required, a small portion of the cooling powder may be added to the water.

The addition of Glauber's salt, it may be observed, increases the density of the mixture, which then becomes a better conductor of the cold, if I may so express myself, and moreover retains the same temperature longer: of course it will be better of the two to overcharge than undercharge the proportion of the salts to the water. It will be apparent, for obvious reasons, that the part containing the subject to be cooled should be as thin as may be, and the whole of the external part in every apparatus thick.

This detail may probably appear prolix to any person induced

duced by curiosity only to look it over; but to any one who means to put it in practice, the whole will be found essential, and with a little attention and experience become familiar and easy, and in which I have endeavoured to combine every advantage the subject will admit of; and as coming from the "fountain head," it may not prove uninteresting to some at least of your numerous readers. I am, Gentlemen,

Your most obedient servant,

Oxford, April 28, 1828. RICHARD WALKER.

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## LXIII. *On the Causes of Single and Erect Vision*. By* L*** M*** S*******.†

IN order to understand aright the reason of single and erect vision, it is necessary first of all to perceive the truth of certain metaphysical positions in relation to vision, without the establishment of which, confused ideas, hypothetical assumptions, and inconclusive reasonings on optical experiments and facts are presented to the mind, and tend to embarrass the simplicity of that truth which might otherwise be immediately revealed.

First,—Vision is a consciousness in the mind, and its next proximate cause must be a power equal to its production, and which unites it to the material world.

Secondly,—Vision of *one colour only* can never yield the vision of figure, because the proximate cause of the vision of figure is a line of demarcation formed by the sensation of the junction of *two colours*.

Thirdly,—The physical impulse producing such consciousness of colouring, is an equal proportional variety upon the retina of an eye; one eye alone being first supposed, as it is truly efficient to yield the idea of figure.

Fourthly,—An object cannot be in two places at the same time.

Fifthly,—An object cannot exist and put forth its action *where it is not*.

These premises being supposed to be granted, let the question be asked, Why with two eyes given, two objects are not seen, although there be but one object given externally?

The answer (when supported by the foregoing premises, and conjoined with certain optical facts with which all who are conversant with the subject, are acquainted) will be, *because there is not presented to the mind that variety of colouring which is necessary and alone efficient as the next proximate cause of vision*; that is, *there are not two lines of separate demarcation between two objects, but one line of demar-*

* Communicated by a friend of the Author.

† Author of "An Essay on the Relation of Cause and Effect;" and of "Essays on the Perception of an External Universe," &c.

*cation*

*cation only is presented, as in* ONE *eye supposed.* Should it be asked, whence is it that such a proportional variety is not presented to the mind? The answer which the premises and optical experiments equally support, is, *because the impulse upon each eye (when the axes of both are directed to the same point or object,) being precisely alike, there is no variety of colouring painted upon either eye, equal to the production of that variety of perception, necessary to yield the ideas of two objects separated from each other, between their interior and horizontal edges.*

Let the letter A, for instance, be painted upon *one* eye, and the perception of its figure arises in the mind, from the points of distinction between the black letter and the white around it: there is a sense of difference created. Place it on similar points of two retinæ, and each point of the figure painted on each retina will yield to the mind but *one* point of conscious black against *one* point of conscious white; and not *two* points of black against *two* points of white, because there is no intervening white painted on either retina, which can yield a consciousness of the separation of the two A's to a distance from each other, thus, A——A.

The white space between the two A's is not painted on either retina. How then can any idea of it arise in the mind?

If, in order to render these ideas more intelligible, we analyse with still greater nicety the question, why we see duplicates of similar figures with *one* eye only supposed, it will at once appear obvious why we can perceive but *duplicates* of such figures, instead of *quadruplicates*, when *two* eyes are used.

Now if *one* eye should see but one colour only, it is supposed to be granted that there could be no sense of any defined figure whatever: one impulse therefore yields not figure.

If one and the same colour should be seen by two eyes, it must still be acknowledged there would be no figure: two *similar* impulses therefore cannot give the sense of figure to the mind. Now upon *one colour* (say a purple ground) painted upon one retina, mark a scarlet circle ○; a sense of one figure will immediately arise from two varieties of colour being carried to the mind, viz. a line of demarcation to the purple ground by the scarlet circle: *two* impulsions, or *two* varieties of colouring, are therefore necessary to the perception of *one* figure. Again, if with *one* eye given, I wish to see *two* scarlet circles upon the purple ground, what must I do? Will *four* impulsions yield *two* similar figures? I answer, No. There must be *five* impulsions in order to convey to the mind the sense of *two* figures: there must be $\overset{2}{\textcircled{1}}\overset{5}{\underset{5}{\text{———}}}\overset{4}{\textcircled{3}}$; that is, the im-

pression

pression of the purple ground must be repeated in *two* different parts of the sentient retina; *two* scarlet lines must be thence impelled, and these made obvious by *the intervening horizontal impulse between the circles;* for could the intervening space be absorbed, and each point of scarlet coalesce with each point of scarlet, and purple with purple, there would then be but *four* impulsions of colouring, but *four* varieties, which would be inefficient to the observation of *two* figures. A coalescence of similar points of colour may perhaps produce a superposition or increment of colour, so as to create a superior brilliancy in the appearance of the object; but a *coalescence of points* cannot give a sense of *the separation of points.* Therefore for the mind to have a sense of *one* figure, there must be two consciousnesses of colour; and to have a sense of *two* figures, there must be *five* distinguishable consciousnesses of colour. However often *one* colour only be repeated, there will be no figure; however often *two* similar colours be repeated, (no intervening one being supposed,) but *one* figure will arise; whilst to entertain a sense of *two* figures, *five* impulsions are necessary. I hold it therefore as an axiom in the laws of vision, *that the repetition of similar impulsions of colour will not yield a number of figures equal to the number of such impulsions; but that the number of figures perceived, arises from those proportional intervals of the impulsions of colour, which must vary in a certain ratio to the number of figures impelled.*

However, therefore, the number of eyes may be multiplied, the mind can have no consciousness of any additional number of figures, whilst only similar impulsions of colour are yielded to it.

Let us enter into some further detail. If *one* scarlet circle on the purple ground be painted on the corresponding points of *two* retinæ, and thence impelled to the mind, there can still only arise the sense of *one* scarlet circle; for there have existed but *four* physical varieties on the retinæ, and but *four* varieties have been impelled to the mind: and it has been proved that *five* physical varieties are necessary to exist upon a retina, or upon retinæ however numerous, in order to impel corresponding consciousnesses to the mind, and which would be necessary for the mental apprehension of *two* figures. Let the figures without an intervening horizontal colour be supposed to be marked thus, $\overset{2}{\textcircled{1}}\frac{0}{0}\overset{4}{\textcircled{3}}$. The intervening horizontal colouring necessary to separate the painting of *two* figures on the retinæ, is not painted on either retina; *it does not exist;* and therefore no conscious separation of *two* figures can possibly arise to the

the mind. There exists indeed a certain space between the two eyes outwardly on the face, but the colouring of this intervening space is not painted on either retina, and therefore cannot be noticed by the mind; nor is there any intervening horizontal purple presented to the mind: Each point of scarlet does but coalesce with each point of scarlet, and each point of purple on *one* retina, with each point of the similar purple on the *other* retina; there is no surplus intervening purple on either retina, and it has been shown that "repetitions of similar impressions of colour do not yield to the mind the sense of an equal number of figures."

But should the scarlet circle be painted on points of the retinæ which do *not* correspond, then there will necessarily arise the sense of *two* figures; because in that case dissimilar impulsions of colour are carried forward to the mind, and yield that proportional variety which determines the observation of conscious duplicates: for the pictures are then immediately painted upon different parts of their respective retinæ, and the distance between two objects "will be proportional to the arch of either retina, which lies between the picture on that retina, and the point corresponding to that of the picture on the other retina*." On this intervening arch a surplus quantity of purple would be interiorly and horizontally painted, and would thence separate the two scarlet figures: *five* impulsions would carry *five* conscious colours to the mind, and *two* separated figures would immediately be observed.

An illustration of these ideas, and especially of this last statement of Dr. Reid's, might very easily be imagined, by conceiving two small terrestrial globes to be painted with precisely similar colours: Let them be rectified to the same degree of latitude and longitude, and similar colours only will appear on the visible surfaces of each; turn them both so many degrees to the east or west, still only similar colours will present themselves; but let one of the globes remain at rest, and turn the other any number of degrees of longitude to the west, a new country will arise to the east on the globe so moved, whose *variety* of colouring must necessarily prevent the notice of a *mere uniformity of colouring* on the two globes, and which variety will separate the appearance of any given country on them, "*proportionally to the number of degrees marked on the arch of the horizon,*" through which the globe so moved had passed. Inspire the colouring on their surfaces with a simultaneous single sensibility, and it will immediately be perceived, that no consciousness of the existence of any two similar

* See Dr. Reid's Inquiry, ch. vi. sec. 13.

countries could arise, so long as the surfaces were regulated to the same degree of latitude and longitude; but the moment they were separated by an alteration of the longitude of either, or both, the sense of that newly arisen continent, or sea, would divide the sense of the remainder.

The following passage is extracted from the Encyclopedia Britannica, and is a quotation from Dr. Wells's Essay on Single Vision. "If the question be concerning an object at the concourse of the optic axes, it is seen single; because its two similar appearances in regard to shape, size, and colour, coincide with each other through the whole of their extent."

This opinion thus expressed, comes nearer to my meaning than any other with which I am acquainted; nevertheless Dr. Wells's argument on the subject (and which is too long to insert here,) is as fallacious as that of any of his predecessors; inasmuch as it assumes *an hypothetical law of vision*, in order to establish that *coincidence of shape, size, and colour* upon which he perceived single vision did necessarily depend.

This laborious argument of his, is as entirely needless as it is futile; because it proceeds upon the supposition that objects are *seen by the mind, beyond the mind*, at the angle formed by the axes of the eyes, in their direction to the same point of distance.

Now when, on the one hand, colour (conscious visible colour) is admitted to be *in* the mind, and never to proceed again *out* of it, in any line, or at any angle to form an object; and on the other hand, by those demonstrable laws of optics established by Sir I. Newton and others, "that when the axes of the eyes are directed to a given point or figure, the said figure is painted on corresponding points of the retinæ," — then the *coincidence* which Dr. Wells speaks of, must of necessity take place; and such coincidence can only determine a consciousness of single vision to the mind; or in other words, can only determine those similar appearances of colour, on which visible size and figure ultimately and alone depend: for the centres coincide with the centres, and the edges with the edges, of the figures; without any variety, or interval of colouring between the interior and horizontal edges, —the conscious sense of which is absolutely necessary in order to induce a *sense* of *variety* or plurality of figure. When the figure is painted upon points of the retinæ which do not correspond, then there must necessarily arise a *sense of two figures;* because the centres *not coinciding* with the centres, nor the edges with the edges, *there exists a surplus colour* in one eye which divides the interior and horizontal edges of the two figures. This surplus colouring is determined to the retina by by some exterior object, which by the shifting of the axis finds a place on which to paint its rays, "and is equal to the arch between the picture of the given figure on that retina, and the point corresponding to that of the picture on the other retina;" and which surplus colouring must determine a proportional consciousness to the mind, observing thereby the same rule which determines the "notice of two similar figures, when one eye only is used;...*when the apparent distance of two objects seen with one eye is proportional to the arch of the retina which lies between their pictures*," and on which an interval of colouring is necessarily painted, *but which circumstance Dr. Reid did not consider it material to notice* *.

The optical facts to which I have alluded are very shortly and very well expressed in Dr. Reid's "Inquiry †:" The passages in the chapter from which I have partially quoted, and which it may be as well to give entire, are the following; and I repose on them as stated facts, not containing either hypothesis, opinion, or reasoning.

"First,—When the axes of both eyes are directed to one point, an object is seen single; and in this case the two pictures which show the objects single, are in the centres of the retinæ. Now in this phænomenon it is evident that the two centres of the retinæ are on corresponding points.

"Secondly,—Pictures of objects seen double, do not fall upon points of the retinæ similarly situate with respect to the centres of the retinæ.

"Thirdly,—The apparent distance of two objects seen with one eye, is proportioned to the arch of the retina which lies between their pictures: in like manner the apparent distance of two appearances seen with two eyes, is proportioned to the arch of either retina, which lies between the picture on that retina and the point corresponding to that of the picture on the other retina."

These facts are valuable for many reasons; but on no account more so, than because they serve to explain the manner by which nature yields the knowledge of external tangible figure, and the proportional motion which is in relation to it, by means of corresponding varieties of colour.

Dr. Reid's *arguments* (although he was in possession of these facts, which might have afforded premises for better reasoning) are altogether inconclusive, not to say puerile; and that on account of his steady adherence to the main object with which he set out upon his "Inquiry," namely, to show upon the principles of *common sense* whence comes the knowledge we have of the existence of an external universe: Following up these

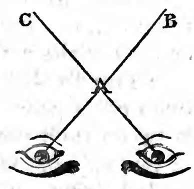
* See Dr. Reid's Inquiry, ch. vi. sec. 13. † Ibid.

 principles,

principles, he placed *visible* figure beyond the body, at a distance from the perceiving mind, denying it to consist either in a sensation, impression, or idea,—and as possible to be *seen* without the intervention of colour*.

It appears to me strange, when contradiction is stamped upon the very expressions which convey these ideas, that Dr. Reid's notions should seem to be the data for the reasonings of the author of the "*Explanation of Erect and Single Vision,*" published in the "Library of Useful Knowledge."

I must however, in common honesty, here take notice of an objection which I have known to be made to the views I entertain on this subject: it is, "that we see objects in different directions by either eye, when the other is alternately opened or closed." This objection appears to me perfectly nugatory, when it is considered, that both eyes being opened together, they are allowed by the condition of the question to be *directed to one point;* in which case *neither* of them can be directed *to any point beyond that point;* it would be a contradiction in terms to admit it. The axes cannot *cross* each other, and look *at points beyond the given point,* and that with a separate consciousness in the mind of so doing; for then these would not be merely *one* given point, but *three* given points; and the figure, the cause of whose single vision is in question, would be supposed to be placed, and at the same time supposed *not* to be placed, at the junction of the axes. For instance, when two eyes are directed to A, the left eye cannot be turned to B, look by itself at B, and the right eye at the same time be made to look by itself at C. Experience shows this to be an impossibility; but when either eye is shut, the other may be moved in any direction we please. However, were I in error in this statement, the argument of my objector would by no means be conclusive against my doctrine of single vision, provided only that A be placed at the junction of the axes; for the utmost which could happen would be, that A plus B, plus C would appear to the mind; but *not* two A's (two B's and two C's), because there would still be only a superposition, or increment of the colouring of A.




The central point of the colour of A would coincide on each retina; the whole of the rest of the colouring in relation to it would be painted on corresponding points, and coincide on their respective retinæ; and there could in no wise arise that proportional variety of colour, painted between the in-

* See Dr. Reid's "Inquiry," ch. vi. sect. 12. p. 135. 12mo.

terior

terior horizontal edges of the two A's, which is necessary to yield the ideas of their separate figures.

But to return "to the explanation of the Cause of Erect and Single Vision, published by the Society for the Diffusion of Useful Knowledge*;"—it appears to me to be as much at variance with a sound, metaphysical, demonstrable conclusion concerning the nature of the perception of visible figure by the mind, as are the authors to whom I have alluded; and as much so with an acknowledged law,—with a proved physical fact, in respect to the *time* required for the motion of light.

The author of the "Explanation of the Cause of Erect and Single Vision," says†: "As the lines of *visible direction* cross each other at the *centre* of visible direction, an erect object is the necessary result of an inverted image;" but this is not the same thing with THE PERCEPTION *of an erect object.* If it be said the word *perception* is understood though not expressed, then the mind is supposed to see the very erect object, out of itself, at a distance from itself; that is, the mind feels colour, perceives visible figure (its result), *there, where it is not*, which is impossible.

Again, it is a known fact, that the light emitted from the sun, employs about eight minutes in its journey to the earth. Now let an object be seen at that distance in an erect position, but the moment after its light is effused, let it be obliterated: the mind will still see it erect, eight minutes after its annihilation, how then shall it signify the drawing of any rays back through a centre, towards the place of an obliterated object, which *once* stood there erect? The author's explanation is little more than the very circumstance in question, re-stated by an inversion of words. "An erect object (at a distance) is the result of an inverted image on the retina by the crossing of rays at a centre;" is merely saying over again, *that an inverted image on the retina is the result of an erect object at a distance*, when rays cross at a centre.

The question still remains untouched and unexplained; namely, why does the mind perceive an erect image, the result of an inverted image, which inverted image is the proximate cause of vision, and not the erect object which might be obliterated without affecting the mental consciousness of it? The only answer appears to me to be that, which I have formerly stated in my "Essay on Single and Erect Vision;" viz. "*Inversion of figure is merely a* RELATIVE *quality: when all rays from every object within the sphere of vision become inverted on the retina, there truly can be no mental consciousness of any in-*

* Optics, part ii. "Library of Useful Knowledge." † Ibid.

*version*

*version whatever: for there is no relative variety by comparison with any other set of similar images; and they will necessarily bear a given relative proportion to the ideas of motion and tangibility; and which ideas, taken collectively, include all the* ELEMENTS *we have of the knowledge of the position, figure, and colour of objects*.*"

No doubt the relations of these in indefinite modifications are perceived by the judgement, as well as innumerable associations of them by the imagination; and thence the large use of vision in the world; thence the warm affections which are approved of by the understanding, or delighted in by the fancy.

But instead of taking this simple and easy mode of viewing the subject, philosophers, when they discuss the reason of erect vision, really suppose (although they may not be willing to allow it in so many words), that mental vision arises from, and is occupied about, *two* sets of objects at the same time; viz. the *external objects in nature,* and the *inverted images of them on the retina:* whereas the external object becomes virtually null and void immediately upon the rays of light being emitted from it.

*The idea of inversion is the result of the comparison of the line of demarcation of one object with that of another of a similar kind placed in a contrary direction to it.* But as in the picture on the retina, the line of demarcation of each particular image touches the line of demarcation of the rest, in the same manner and after the same proportion as their corresponding objects do in external nature; so no such comparison can take place: for *one* set of images only is painted, and these in precisely the same relative positions to each other as are their counterparts. The mind therefore necessarily perceives the same positions with respect to each other; for no *two* objects of a kind present themselves, by which a comparison can take place.

Philosophers, therefore, when they compare the image on the eye of an ox, for instance, with the object in external nature of which such image is the reflection, forget that both together make but one picture on their own eyes: For any given object forms on the human eye an inverted image, and the mind sees it erect; but the image on the eye of the ox (which is already inverted) makes on the eye of the person who observes it, an image again inverted that is erect, and the mind perceives it *inverted.*

*In this latter case there is a comparison of the line of demarcation*

* See "Essays on the Perception of an External Universe," &c. by Lady Mary Shepherd. 1827. Essay, xiv. p. 408.

*cation of one object with that of another of a similar kind, placed in a contrary direction to it.* In the former case, *two* objects do not present themselves, but only *one* of a kind, and that surrounded by each and every line of demarcation, precisely in the same relations to each other as are those of external nature. The same observation holds good when drawings are used with two images on them, placed contrariwise to each other; as an arrow without the retina, and an arrow within the retina. Did the arrow within the retina *feel* along with the surrounding lines of colouring, there could be no *sense* of an inverted arrow; for there would exist no reference to another arrow, which reference is only made by the observer, who is looking on *two* arrows.

Observations analogous to these must be made on the attempted explanation "of the cause of single vision," by the same author, who says*, "Because the lines of visible direction from similar points of the image (on *one retina*) meet the lines of visible direction from similar points of the other *image* upon the *other retina*, each pair of similar points must be seen as *one* point." How so, when the mind sees not *out* of itself *at the junction of the points*, and when if the object which sent forth the rays were annihilated, there would still result a *single vision from separated points of colour painted on* SEPARATED RETINÆ *at a distance from each other;* such duplicate separate figures on the retinæ being the proximate cause of the single vision of the object, and *not the junction of similar points*, when rays are drawn *back* again from the retinæ to such points of junction.

The question still recurs, and is still untouched and unexplained; *Why are pairs of points perceived by the mind as single points?* No doubt the determination of rays upon the retinæ in such a manner that when drawn back again they will meet at a central point, is a property closely connected with the method of vision; but it is rather *a corollary or consequence of the manner of the entry of the rays at the pupil of the eye* by which equal arches are subtended upon the retina, than the efficient cause of either *single* or *erect* vision. I again ask, Why are *two* objects on the retinæ perceived as only *one* object by the mind? For it is *not* a junction of *external* points which is perceived, but *two* sentient retinæ determine two separate images (equally perfect in their form, equally brilliant in their colouring), as but *one* image to the mental capacity of perception. Is not the answer, *Because there are no points of*

* Optics, part ii. "Library of Useful Knowledge."

*colouring*

*colouring painted on either retina, by which the separation of their forms can be distinguished?*

Press the axis of either eye sufficiently to the right or left, a larger quantity of colouring will immediately be painted upon one retina than upon the other, which will separate their interior and horizontal edges, and *two* images will thence immediately and necessarily arise upon the perception of the mind.

I feel convinced that the more these ideas are contemplated, and the more clearly they are apprehended, the better will they serve to elicit the reason of several other phænomena concerning vision, which it has hitherto been considered difficult to explain; and what is of still greater importance, they may throw some light upon those which belong to every analogous operation of the human senses and intellect.

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## LXIV. *On the Reaction of effluent Water, and on the Maximum Effect of Machines. By* Mr. P. Ewart*. *With Notes relating to the Theory of Barker's Mill. By* J. Ivory, *Esq., M.A. F.R.S.*†

THE following important proposition relating to this subject, is laid down by Daniel Bernoulli in his "Hydrodynamics," page 278. If a jet of water I (fig. 1.) issue from the side of a vessel A, with the velocity which a body would acquire in falling freely from the surface B to C, he says the *repulsion* of the water in the opposite direction to the jet will be equal to the weight of a column of water, of which the base is equal to the section of the contracted vein, and the height equal to 2 BC.

This question respecting the amount of what has been termed the "reaction of the effluent water," derives additional interest from the circumstance of its having particularly engaged the attention of Sir Isaac Newton, and from his having given a solution

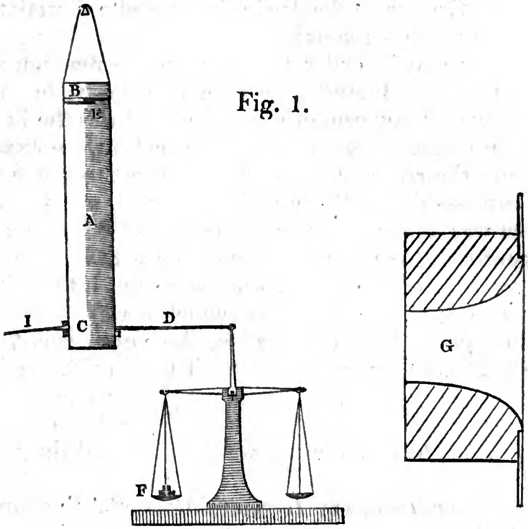
* From a paper "On the measure of moving force," in the Memoirs of the Lit. and Phil. Society of Manchester, second series, vol. ii. 1813.

We insert this extract because it treats, correctly we believe, of subjects which have engaged the attention of many eminent mathematicians (in former times as well as recently), whose reasonings and conclusions on the points in question are at variance with each other. These discrepancies are to be regretted, inasmuch as some of the essential points in the application of the principles of mechanics to practical purposes are involved in them.—Edit.

† Communicated by the Author.

We are glad to lay before those of our readers who have attended to the various intricate theories that have been offered of the action of Barker's Mill, Mr. Ivory's Notes on that subject.—Edit.

Fig. 1.




solution of the problem in the first edition of the "*Principia*," which he materially altered in the succeeding editions. In the first edition (book 2nd, prop. 37.) he infers, that the reaction is equal to the weight of a column of water of which the base is equal to the area of the orifice, and the height equal to that of the surface of the water above the orifice. In the succeeding edition, the subject is more fully discussed in the 36th prop. of the second book, where he infers (cor. 4.) that, when the area of the surface B is indefinitely large compared with that of the orifice, the reaction is, what it was afterwards in a different manner demonstrated to be by D. Bernoulli. Sir Isaac Newton further observes, that he found, by admeasurement, the area of the orifice in a thin plate to be to that of the section of the contracted vein, at the point of its greatest contraction, in the ratio of $\sqrt{2} : 1$ nearly. He takes the reaction, therefore, to be greater than what he understood it to be when he published the first edition, in the ratio of $\sqrt{2} : 1$ nearly. He refers, however, more to experiment than to theory for a solution of this question; and many valuable experiments have since been made on effluent water; yet I cannot find that the results of any direct experiments have been published which go to determine the precise amount of this reaction.

Sir Isaac Newton suggested (*Principia*, first edit. p. 332.) a method by which the reaction may be easily measured. If the vessel be suspended like a pendulum, he observes, it will recede from the perpendicular in the opposite direction to the jet.—I have made some experiments on a vessel suspended in that manner; and in order to ascertain the reaction as accurately as possible, I made use of a balance-beam furnished with a perpendicular arm of the same length as the horizontal arms, as represented at fig. 1. The scales were exactly balanced, and the end of the rod D made just to touch the side of the vessel.—The orifice was then opened, and the water in the vessel was kept uniformly at the same height by a stream falling gently on the plate E. The scale F having been raised by the reaction of the jet, weights were put into it till it was brought exactly to the position in which it was before the orifice was opened. The diameter of the vessel was 7 inches, and the height BC exactly 3 feet. I tried orifices of various diameters from ·35 to ·7 of an inch. Their exact diameters were ascertained by a micrometer, and the time carefully observed in which 30 lbs. of water were discharged through each orifice.

When the orifice was made in a thin plate ($\frac{1}{30}$th of an inch in thickness), I found the reaction to be greater than that stated in Sir Isaac Newton's first conclusion, in the ratio of 1·14 to 1. There was some variation in the results of the experiments. The greatest reaction, however, was as 1·16 to 1, and the least as 1·09 to 1, which fall far short of Sir Isaac Newton's last inference. The velocity of the water at the orifice (ascertained by observing the time in which 30 lbs. were discharged) was less than that which a body would acquire in falling freely from B to C, in the ratio of ·6 to 1.

I found no constant ratio to subsist between the diameter of the contracted vein and that of the orifice; and observing considerable opacity in the jet at the contracted vein, I concluded it to be divided into a number of different filaments, and I gave up all hopes of ascertaining the actual area of the section of the stream at that place by measuring its diameter. After repeated trials I found that when the water issued through a contracted hole, of the shape represented at G, the jet was quite transparent, and the reaction (taking the mean of 12 experiments with 4 different orifices) was less than the weight of a column of water of twice the height of the head and diameter of the smallest part of the hole, in the ratio of ·865 to 1. The least reaction was as ·85 to 1, and the greatest as ·88 to 1. By measuring the quantity of water delivered in a given time, I found the velocity of the jet, at the smallest part of the orifice, to be less than that which a body would acquire in falling freely from B to C, in the ratio of ·94 to 1. The highest ratio was as ·95 to 1, and the lowest ·89 to 1.

From

From these results it appears, that when the contracted vein is not opaque, and when its velocity is nearly equal to that which is due to the head, the reaction is nearly equal to what it was concluded to be by Sir Isaac Newton and M. Dan. Bernoulli; and the great apparent difference between Sir Isaac Newton's first and second conclusions arises from his having been misled by some experiments to which he alludes. He says—"Per experimenta vero constat, quod quantitas aquæ, quæ, per foramen circulare in fundo vasis factum, dato tempore effluit, ea sit, quæ cum velocitate prædicta," [viz. the velocity due to the head] "non per foramen illud, sed per foramen circulare, cujus diametrum est ad diametrum foraminis illius ut 21 ad 25, eodem tempore effluere debet *." We must presume, however, that he refers to experiments made by others; for if he had made them himself, he would, no doubt, have arrived at the same results which have since been so well established by various authors, and he would have stated the above ratio to be as 19·5 to 25 nearly.

But his demonstration of the reaction requires that the velocity at the contracted vein shall be equal to that which is due to the head. Now that velocity cannot be determined by measuring the imperfectly contracted vein in cases of water spouting through a hole in a thin plate.

We may safely indeed infer, that, in such cases, the velocity is considerably less than what is due to the head. For, the jet being opaque, some moving force must be expended in separating the particles from each other, and the distance to which the jet from such an orifice is projected on a horizontal plane, confirms that inference. The demonstration, therefore, of the reaction, can be properly applied to such cases only as those where the water, issuing through a tube properly contracted, acquires the velocity nearly which is due to the head, and in those cases the experimental results agree, as I have stated, remarkably well with the demonstration.

These results agree also with the explanations which have been given of *moving force* †. If we suppose the velocity of the jet to be equal to that which is due to the head, and the vessel to move uniformly in the opposite direction CD with the same velocity; the water will be at rest as it issues.

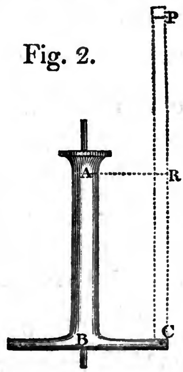
Let $a$ represent the area of the smallest section of the orifice. Then while the vessel has moved through a space = 2 BC, a quantity of water represented by $a \times 2\text{BC}$ has descended

* *Principia*, edit. ii. lib. 2. prop. 36.

† By *moving force* is meant the product of the pressure into the space through which it acts, or of the quantity of water into the height through which it falls. The same sense in which the term is used by Euler.

scended from B to C, and has been brought to rest. But the reaction is $= a \times 2\,BC$, and this multiplied by $2\,BC$, the space through which it has acted, gives $a \times \overline{2\,BC}|^2$ for the amount of the moving force produced, which is exactly the quantity of moving force necessary to raise the column $a \times 2\,BC$ to the height BC, and to project it with the velocity 2 BC. For, a moving force $= a \times 2\,BC \times BC$ will raise that column from C to B, and an equal moving force will generate the velocity 2BC in the same column, therefore $2a \times 2BC \times BC = a \times \overline{2BC}|^2$ is the whole moving force necessary to restore that column to the place and condition in which it was before it began to descend; and as no moving force has been expended in producing change of figure, that quantity of moving force must be found in the reaction of the water through the space which the vessel has moved while the water descended and was brought to rest.

Fig. 2.




Upon the same principle an easy and simple explanation may be given, I apprehend, of the action of the hydraulic machine called Barker's Mill. Let AB (fig. 2.) be the perpendicular tube, and BC the horizontal arm; let $v$ express, in feet per second, the rotatory velocity of the arm at the orifice C, and let the water be supposed to issue with the velocity due to the pressure*. Put $g = 16\frac{1}{12}$ feet.

If BC be a cylindrical tube, and if $q$ represent the quantity of water it contains from B to C, the centrifugal pressure upon a section of the arm at C, will be $\frac{q v^2}{4g\,BC}$; and whatever the length BC may be, the diameter remaining the same, $q$ being as BC, the centrifugal pressure at C will always be as $v^2$; and it will be equal to the pressure of a perpendicular column of water whose height in feet is $\frac{v^2}{4g}$. Then if $h$ express in feet the height AB of the water in the vertical tube, $h + \frac{v^2}{4g}$ will be the whole pressure at C; and if $a$ express in feet the area of the most contracted section of the orifice, $2a\left(h + \frac{v^2}{4g}\right)$ will

* It is here understood that the areas of the sections of the perpendicular tube and of the horizontal arm shall be indefinitely large when compared with the area of the orifice.

express

express the reaction, which being multiplied by $v$, the space through which it acts in a second, gives $2av\left(h+\frac{v^2}{4g}\right)$ for the *total* moving force of the arm in a second. But a part of this moving force is expended in producing the rotatory motion of the water, and in raising it to the height $\frac{v^2}{4g}$. For, if we suppose a perpendicular tube CP to rise from the arm at C, the surface of the water in that tube would stand at P, PR being $=\frac{v^2}{4g}$. Now if instead of letting the water escape at C, it be allowed to flow over the perpendicular tube at P, and fill another similar perpendicular tube adjoining it, and issue from an orifice at the bottom of that tube, the effect must be the same as if it issued at C, and a moving force must be expended at C, sufficient to generate the velocity $v$, in the water which passes, and also to raise it from R to P.

The pressure at C being equal to the weight of a column of water whose height is $h+\frac{v^2}{4g}$, (that is $=$ AB$+$PR), the velocity with which the water issues will be $\sqrt{4g\left(h+\frac{v^2}{4g}\right)}$ or $\sqrt{4gh+v^2}$. Let V express that velocity, then $a$V will express the quantity of water which passes in a second; and $2aV\frac{v^2}{4g}$ will express the moving force necessary to generate the velocity $v$, in that quantity of water, and to raise it from R to P. That quantity of moving force being deducted from the total moving force of the arm, leaves $2av\left(h+\frac{v^2}{4g}\right)-2aV\frac{v^2}{4g}$ for the *effective* moving force of the arm in a second.

That this is the effective moving force, may be shown also in another manner, as follows:

The *absolute* velocity of the water after it has left the machine will be $V-v$, and $\frac{(V-v)^2}{4g}$ will be the head which would produce that velocity; which being multiplied by $a$V, the quantity of water delivered in a second, gives $aV\frac{(V-v)^2}{4g}$ for the moving force which remains with the water after it has left the machine.

If that be deducted from $aVh$, the whole moving force of the water, there will remain $aVh-aV\frac{(V-v)^2}{4g}$ for the *effective* moving force, which will be found to be equal to $2av\left(h+\frac{v^2}{4g}\right)-2aV\frac{v^2}{4g}$, the *effective* moving force stated above.

The

The theory of this machine has occasionally occupied the attention of many distinguished mathematicians, and M. Euler has given two elaborate treatises on its principles in the Memoirs of the Berlin Academy for 1750, p. 311; and for 1751, p. 271. His demonstrations relating to this subject are very complicated, and they do not appear to have been adopted by succeeding authors*.

Mr. Waring, of America, has given quite a different theory, which has been approved of by several good writers on hydraulics. He concludes that the greatest effect will be produced when the velocity of the orifice is half that of the issuing water; and that this effect will be nearly the same as that of a well-constructed undershot water-wheel†.

The explanation which I have offered of the action of the water on this machine is different from any other that I have had an opportunity of consulting. I offer it, therefore, merely as an attempt to solve an intricate problem.

If it were possible for the water to issue with the velocity due to the pressure, it is obvious, if my explanation be right, that although a very large proportion of the moving force of the water may be communicated to the machine, moving with a moderate velocity, the maximum of effect can only be obtained by an infinite velocity. But when the water issues with a velocity which is less than what is due to the pressure, as must always be the case in practice, the velocity at which the maximum of effect is produced, may be found as follows. It should first be ascertained by experiment how near the issuing velocity can be brought to that which is due to the pressure. From the experiments which I have made, I have been led to conclude that no greater issuing velocity can possibly be obtained from a machine of this kind than what is due to ·8 of the pressure. If this conclusion be correct, it follows, that, whatever may be the issuing velocity of the water, a moving force, equal to $\frac{1}{4}$ of the moving force which is necessary to generate that velocity in the water, when falling freely, is expended in producing change of figure; that is, in forcing the water through the tubes and through the orifice C; and if the velocity of the machine be such that PC = 5 AB, the issuing velocity will be equal to the velocity of the orifice, and the whole moving force of the water in descending from A to B will be expended in producing change of figure.

For, the head due to V, the issuing velocity, will in this case

* M. Euler says, "In employing the same quantity of water, and the same fall, this machine will produce an effect nearly four times greater than the ordinary machines."

† American Philos. Trans. vol. iii. p. 191 and 192.

be

be PR, which is also the head due to $v$, the velocity of the orifice. We shall therefore have $V = v$; and if CP represent the total moving force necessary to raise the water from C to P, CR = AB will represent that part of it which is expended in producing change of figure. The greatest velocity, therefore, that the orifice, when the machine meets with no resistance, can acquire, will be $\sqrt{4g \times 4h}$.

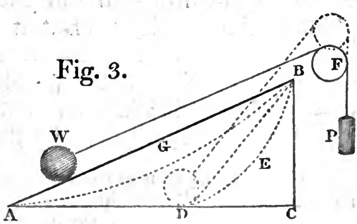
When the velocity of the orifice is less than that, V will be greater than $v$; and $V - v$, the absolute velocity of the water after it has left the machine, will be $\sqrt{\cdot 8\,(4gh + v^2)} - v$. The head or the moving force expended in producing that velocity will be as $\frac{\overline{\sqrt{\cdot 8\,(4gh + v^2)} - v}\,^2}{4g}$.

The moving force expended in producing change of figure will be as $\cdot 2\left(h + \frac{v^2}{4g}\right)$. Now when the sum of these two quantities, or $\frac{\overline{\sqrt{\cdot 8\,(4gh + v^2)} - v}\,^2}{4g} + \cdot 2\left(h + \frac{v^2}{4g}\right)$, is a minimum, we shall find $v = \sqrt{2gh\,(\sqrt{5} - 1)} = 6\cdot 3056\,\sqrt{h}$ for the velocity of the orifice when the machine produces a maximum of effect; and in that case the above sum becomes $= \cdot 4472 h$.

We shall therefore have $h - \cdot 4472\,h = \cdot 5528\,h$ for the maximum of effect, supposing $h$ to represent the whole moving force of a given quantity of water descending from A to B. This effect is considerably greater than that which the same quantity of water would produce if applied to an undershot water-wheel, but less than that which it would produce if properly applied to an overshot water-wheel.

Respecting the maximum of effect produced by machines, I wish to observe, that in the actual construction of machines it is necessary to aim at a maximum quite different from that which is usually proposed in books on the theory of mechanics. This will perhaps be best explained by examining the simple case where a given weight P, (fig. 3) connected with another W, by a string passing over the pulley F, descends vertically and raises W, without friction, from the horizontal line AC along the inclined plane AB. If we make AB : BC : : 2 W : P, W will be raised to B in the least time;

Fig. 3.



time*; and upon this principle, the maximum of effect in machines is usually demonstrated in theory. In practice, however, the object is not merely to raise W to B in the *least time*, but to raise it with the least expenditure of *moving force*. When it is raised in the least time, P must descend through a space = AB, but when it is raised with the least moving force, P descends through a space = $\frac{1}{2}$ AB only. For, if we make BD = $\frac{1}{2}$ AB, and let W ascend along any concave surface DEB, of which BD is the chord, it will be raised to B by the descent of P through a space = BD, and it will be at rest when it arrives at B. This is so obvious, that it would be superfluous to give a demonstration of it. It appears then, that twice the quantity of moving force which is absolutely necessary to raise W to B, must be expended if it is to be raised by P in the least time. To determine the curve by which W will ascend from D to B in the least time, is an intricate problem, and I do not know that it has ever been solved†; but a practical approximation to it in any particular case may be easily found. A well constructed steam-engine for raising water exhibits in every stroke a practical example of the same problem. At the commencement of the stroke, a very great pressure of steam is thrown upon the piston, and this pressure is gradually diminished, so that at the end of the stroke there is a considerable preponderance in the opposite direction. In consequence of this regulated pressure of the steam, the motion of the machine resembles the uniform vibrations of a pendulum, and the moving force of the steam is applied to the greatest advantage.

By proceeding on the principle that when W is raised to B in the least time, the maximum of effect is produced, many erroneous conclusions have been drawn respecting the proper construction of machines. It is laid down for example, on this principle, that "In an overshot water-wheel, the machine will be in its greatest perfection, when the diameter of the wheel is two-thirds of the height of the water above the lowest point of the wheel‡." But it is very well known that there would be lost, by that construction, nearly one-third of the moving force of the water, which is saved by making the wheel one-half larger in diameter, and by making its velocity much less than what is required by the above rule.

* If the ascent be made in the least *possible* time, W must ascend not along the plane AB, but along a concave surface AGB.

† This difficult problem, we understand, has lately been solved by the Rev. E. Sibson of Ashton, in Makerfield, Lancashire, and the solution will appear in the next volume of the Memoirs of the Lit. and Phil. Society of Manchester.—Edit.

‡ Gregory's Mechanics, vol. i. p. 447.

## NOTES relating to the Theory of *Barker's Mill.* *By* J. IVORY, *Esq. M.A. F.R.S.*

### I. *On the Pressure caused by the Centrifugal Force.*

The centrifugal pressure may be investigated in the manner following.—The whole pressure at the orifice is the sum of the variable pressures of all the molecules, or infinitely small portions of the fluid, in the length of the arm. Let $r$ be the length of the arm between the orifice and the axis; $g = 16\frac{1}{12}$ feet; $v$, the absolute velocity of the orifice in feet: then if $d\,x$ (or $\dot{x}$) be a small portion of the fluid in the arm at the distance $x$ from the axis, the centrifugal pressure of $d\,x$ will be

$$\frac{x^2\,v^2}{r^2} \times \frac{d\,x}{2\,x\,g} = \frac{v^2.\,x\,d\,x}{2\,g\,r^2};$$

and the pressure of the prism of the fluid, of which the length is $x$, will be $= \frac{v^2\,x^2}{4g\,r^2}$; and the whole pressure of all the fluid in the length $r$, will be $= \frac{v^2\,r^2}{4g\,r^2} = \frac{v^2}{4g}$.

The same thing may be more shortly stated, thus: The centrifugal force of every small portion of the fluid in the arm being as the distance from the axis, we may assume that every portion is acted upon by the force which takes place at the middle of the arm, or at the distance $\frac{1}{2}r$ from the axis. Now this force is $= \frac{v^2}{4gr}$; which multiplied by $r$, the sum of the portions of the fluid, gives $\frac{v^2}{4gr} \times r = \frac{v^2}{4g}$, the same as before.

And since we have,

$$\frac{v^2}{4g} = \frac{r^2}{4g} \times \left(\frac{v}{r}\right)^2;$$

$\frac{v}{r}$ being the *angular velocity* of the machine, or the angle through which it turns in a second; it follows that the angular velocity being the same, the centrifugal pressure varies as the square of the length of the arm.

Some authors, and in particular Bossut (*Hydrod.* tom. i. § 429), reckon the centrifugal pressure, not in the whole length $r$, but in the distance between the upright tube and the orifice in the horizontal arm. Let $r'$ be the radius of the upright tube; then, according to the authors mentioned, the centrifugal pressure at the orifice will be equal to

$$\frac{v^2}{4g} \times \frac{r^2 - r'^2}{r^2},$$

being the difference between the pressures at the distances $r$ and $r'$, from the axis.

II. *Velocity with which the Fluid issues from the horizontal Arm, supposing that v is the* absolute Velocity *of the Orifice.*

The velocity required is produced by the pressure of the fluid in the upright tube increased by the centrifugal pressure. Wherefore if $h$ denote the length of the upright tube, supposed to be kept constantly full, the sum of the two pressures mentioned will be $= h + \frac{v^2}{4g}$; and, if V denote the velocity of the effluent water, we shall have,

$$V^2 = 4g\left(h + \frac{v^2}{4g}\right) = 4gh + v^2.$$

And, if we put $v^2 = 4gf$, then

$$V^2 = 4g(h + f).$$

III. *Demonstration of Daniel Bernoulli's Proposition respecting the Reaction of effluent Water* (or, *Cor.* 2. *prop.* 36. *lib.* 2. Principia).

Suppose that water issues from a small orifice in the side, or bottom, of a vessel which is kept full; let $k$ be the height of the surface above the level of the orifice: then, $2\sqrt{gk}$ is the velocity in a second with which the water issues; and, if $a$ be the area of the orifice, the quantity of water discharged in a second is equal to the prism $a \times 2\sqrt{gk}$; and, as the water issues with the velocity $2\sqrt{gk}$, the quantity of motion in the water discharged in a second is equal to,

$$a \times 2\sqrt{gk} \times 2\sqrt{gk} = 2ak \times 2g;$$

and the same quantity of motion; viz. $2ak \times 2g$, is evidently the reaction of the water projected in a second. But the prism or weight, $2ak$, by falling, produces a quantity of motion equal to $2ak \times 2g$ in a second: wherefore the reaction of the projected fluid is equal to the weight $2ak$; that is, to the weight of a prism of the fluid having its base equal to the orifice $a$, and its altitude equal to $2k$.

This proposition supposes that the water has acquired its complete velocity of projection, due to the head $k$. Before the efflux

efflux commences, and while the fluid is at rest, the weight $a\,k$ is sufficient to counteract the tendency to begin motion.

IV. *Computation of the Moving Force of Barker's Mill, or of the impulse produced by the Reaction of the projected Water; supposing that h is the length of the upright Tube kept constantly full, and v the absolute Velocity of the Orifices in the horizontal Arms.*

This machine has generally two horizontal arms diametrically opposite; but it may have any number of such arms with an orifice in each. Whatever be the number of the arms, I shall suppose that all the orifices are at the same distance, $r$, from the axis; and I shall use the symbol $a$ to denote the sum of the areas of all the orifices.

The velocity V with which the water issues from the orifices is known by the formula,

$$V^2 = 4\,g\,h + v^2 = 4\,g\,(h + f).$$

And, since the water is propelled from the orifices with the velocity V, and has acquired from the machine the velocity $v$ in the opposite direction, the velocity with which it is projected from the arms, is equal to $V - v$: but $a\,V$ is the quantity of water so projected in a second; wherefore the momentum of the projected water is equal to $a\,V \times (V - v)$; and the same expression is equal to the impulse communicated to the machine by reaction. Wherefore, since the impulse is exerted at the end of the lever $r$, its effect to turn the machine, or the motive force, is equal to

$$r \times a\,V \times (V - v). \qquad (1)$$

We may likewise measure the motive force by the momentary impulse multiplied by the space through which it acts; and the force accelerating the machine will be,

$$v \times a\,V \times (V - v). \qquad (2)$$

These two formulæ seem to be the most elementary expressions of the motive force of this machine.

*Mr. Ewart's Formula.*

Let us put $\dfrac{a\,V \times (V - v)}{2\,g} = P$;

then, $a\,V \times (V - v) = P \times 2\,g$; wherefore the reaction is equal to the momentum which the weight P acquires in a second by falling; in other words, the momentary impulse of reaction is equal to the pressure of the weight P. The mo-



tive

tive force of the machine, according to the formula (2), is therefore equal to $P \times v$. Now

$$P \times v = 2av \times \frac{V^2}{4g} - 2aV \times \frac{v^2}{4g};$$

and, by substituting the value of $V^2$,

$$P \times v = 2av\left(h \times \frac{v^2}{4g}\right) - 2aV\frac{v^2}{4g}.$$

*Euler's Formula.*

In the formula (1), put $2\sqrt{g(h+f)}$ and $2\sqrt{gf}$ for V and $v$; then the impulse to turn the machine will be,

$$2ar \times 2g(h+f-\sqrt{hf+f^2}),$$

which is Euler's formula.

N. B. It must be observed that Euler makes gravity the unit of forces; that is, he denotes it by 1, and expresses the velocities by the square roots of the heights: and, in order to make the foregoing expression agree with Euler's assumption, we must make $2g = 1$; because $2g$ has been taken for the measure of the force of gravity.

*Bossut's Formula.*

This author considers the machine in a form more complicated than we have here contemplated; but, when we make allowance for the peculiar mechanism he supposes, the result of his investigation will be found to agree with the expression (1).

V. *Effect of the Machine.*

We have found that the moving power of the machine is $P \times v$; and

$$P \times v = v \times \frac{aV \times (V-v)}{2g} = aV \times \frac{v^2}{2g} \times \left(\frac{V}{v} - 1\right).$$

Now $a \times V$ is the prism of water that issues from the orifices in a second, which quantity we shall denote by Q; then the equation,

$$Q = a \times V \qquad \text{(A)}$$

will express the relation between the water expended, the velocity of expenditure, and the sum of the areas of the orifices. Further, put

$$x = \frac{v}{V} = \frac{\sqrt{f}}{\sqrt{h+f}},$$

and

and $x$ will be a number less than 1: then

$$f = \frac{x^2 h}{1-x^2},$$

$$\frac{v^2}{2g} = 2f = \frac{2x^2 h}{1-x^2},$$

$$\frac{V}{v} - 1 = \frac{1-x}{x};$$

wherefore, by substitution,

$$P \times v = aV \times \frac{v^2}{2g} \times \left(\frac{V}{v} - 1\right) = Qh \times \frac{2x}{1+x}.$$

If we make $x = 1$, which supposes that $f$ and $v$ are infinitely great, then $P \times v = Qh$; and the effect of the machine would be equal to the whole mechanic power in the quantity of water Q falling from the head $h$. This is an unattainable limit; but the nearer $x$ is to 1, the greater will be the moving force of the machine; which seems to be the only general rule we can have to guide us in the construction of this machine. Having pitched upon the most convenient value of $x$; then,

$$V = \frac{2\sqrt{gh}}{\sqrt{1-x^2}},$$

$$v = 2\sqrt{gh} \times \frac{x}{\sqrt{1-x^2}},$$

$$a = \frac{Q}{2\sqrt{gh}} \times \sqrt{1-x^2},$$

effect of the machine, $P \times v = Qh \times \frac{2x}{1+x}$. If $x = \frac{1}{2}$, that is, if V be double of $v$, then the effect of the machine is $\frac{2}{3} \times Qh$.

In what goes before we have taken the full velocity due to the head, which is always greater than the real velocity in practice. But although the real velocities are less than according to theory, yet they still nearly follow the same proportion; that is, their squares are as the pressures, or as the heights of the head. We may therefore assume

$$V^2 = 4g \cdot m(h+f),$$

$m$ being a quantity less than 1, to be determined experimentally. It is evident that this assumption does not affect the equation (A). We shall now have,

$$v = 2\sqrt{gf},$$

$$V = 2\sqrt{gm(h+f)},$$

$$x = \frac{v}{V} = \frac{\sqrt{f}}{\sqrt{m(h+f)}},$$

$\frac{v^2}{2g}$

$$\frac{v^2}{2g} = 2f = 2h \times \frac{mx^2}{1-mx^2}$$

$$P \times v = aV \times \frac{v^2}{2g} \times \left(\frac{V}{v} - 1\right) = Qh \times \frac{2m(x-x^2)}{1-mx^2}.$$

Now it is obvious that the expression $\frac{x-x^2}{1-mx^2}$ is susceptible of a maximum; and, by applying the usual rule, the value of $x$ answering to the maximum will be found; viz. $x = \frac{1-\sqrt{1-m}}{m}$. This expression shows that, whatever fraction of unit $m$ stands for, $x$ is contained between the limits $\frac{1}{2}$ and 1*; so that, when the machine works to the greatest advantage, $v$ is greater than $\frac{1}{2}$ V and less than V. With a given value of $m$, the rules for constructing the machine so as to work to the greatest effect, are contained in the following formulas, in which $n = \frac{1}{m}$, viz.

$$x = \frac{1-\sqrt{1-m}}{m},$$

$$v = 2\sqrt{gh} \times \frac{x}{\sqrt{n-x^2}},$$

$$V = 2\sqrt{gh} \times \frac{1}{\sqrt{n-x^2}},$$

$$a = \frac{Q}{2\sqrt{gh}} \times \sqrt{n-x^2},$$

effect of the machine, $P \times v = Qh \times (1 - \sqrt{1-m})$†. It now appears that, $m$ falling short of 1, the effect of the machine decreases rapidly. If $m = \cdot 8$, then $P \times v = Qh \times 0{\cdot}5528$, or $P \times v = \frac{5}{9} \times Qh$, nearly.

If $m = \frac{1}{2}$, that is, if half the pressure were lost in forcing the water through the tubes, the effect of the machine would be reduced to $Qh \times 0{\cdot}293$, or nearly $\frac{3}{10} \times Qh$.

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*
$$x = \frac{1-\sqrt{1-m}}{m} = \frac{1}{2} + \frac{m}{8} + \frac{m^2}{16} + \frac{5m^3}{128} + \&c.$$

when $m = 0$, $x = \frac{1}{2}$; when $m = 1$, $x = 1$.

†
$$x = \frac{1-\sqrt{1-m}}{m},$$

$$mx^2 = 2x - 1,$$

$$1 - mx^2 = 2(1-x)$$

$$\frac{2m(x-x^2)}{1-mx^2} = \frac{2m(x-x^2)}{2(1-x)} = mx = 1 - \sqrt{1-m}.$$

LXV. *On*

## LXV. *On the Figure of the Earth, as deduced from Measurements of the Meridian.* By J. IVORY, *Esq. M.A. F.R.S.**

IN my last communication I have shown that the five portions of the meridian which have been most exactly measured, belong to one and the same elliptical spheroid, although they occupy very dissimilar situations on the earth's surface. The almost perfect precision with which the elliptical elements agree with the actual measurements, is indeed not a little remarkable, and seems hitherto to have escaped notice. In returning to this subject, which is of great importance in the question about the figure of the earth, my intention is to discuss it more thoroughly, by examining it in every point of view that may throw light upon it.

The equations deduced from the five measurements are as follows (Phil. Mag. for May, pp. 345, 346):

$$188510 = \Delta\,(3{\cdot}117500 - 6{\cdot}2261\,.\,\varepsilon + 3{\cdot}095\,.\,\varepsilon^2)$$
$$598630 = \Delta\,(9{\cdot}895891 - 18{\cdot}1985\,.\,\varepsilon + 6{\cdot}163\,.\,\varepsilon^2)$$
$$751567 = \Delta\,(12{\cdot}370302 - 6{\cdot}2811\,.\,\varepsilon - 10{\cdot}466\,.\,\varepsilon^2)\dagger$$
$$172751 = \Delta\,(2{\cdot}840172 - 0{\cdot}3844\,.\,\varepsilon - 2{\cdot}166\,.\,\varepsilon^2)$$
$$98870 = \Delta\,(1{\cdot}622022 + 0{\cdot}8378\,.\,\varepsilon - 0{\cdot}022\,.\,\varepsilon^2).$$

In solving these equations I before supposed that the elements sought were approximately known; but I shall here proceed by a more direct method, which is independent of any previous knowledge of the elements. For this purpose, assume,

$$\Delta = R\,(1 + A\varepsilon + B\varepsilon^2),$$

R, A and B being quantities to be determined: substitute the value of $\Delta$ in every one of the five equations, neglecting the powers of $\varepsilon$ above the square; and, having added all the resulting expressions into one sum, equate the coefficients of $\varepsilon$ and $\varepsilon^2$ to zero: then,

$$1810328 = 29{\cdot}845887 \times R; \qquad R = 60655{\cdot}9$$
$$29{\cdot}8459 \times A = 30{\cdot}2523, \qquad A = 1{\cdot}0136$$
$$29{\cdot}846\ B = 30{\cdot}252\ A + 0{\cdot}396, \qquad B = 1{\cdot}141$$

and, consequently,

$$\Delta = R\,(1 + 1{\cdot}0136\,.\,\varepsilon + 1{\cdot}141\,.\,\varepsilon^2).$$

It is obvious that R is the radius of the circle in which the sum of the lengths of all the arcs answers to the sum of their amplitudes. The element $\Delta$ now depends upon $\varepsilon$, which is the only unknown quantity. In order to find $\varepsilon$, substitute the value of $\Delta$ in every one of the five equations, bringing all the terms

* Communicated by the Author.

† In the Phil. Mag. for May, the coefficient of $\varepsilon^2$ is 6·266; but the inadvertence does not affect any of the results.

terms to one side after having divided by R; by which procedure we shall obtain these five equations,

$$x^{(1)} = \cdot 009642 - 3\cdot 0662 \,.\, \varepsilon + 0\cdot 341 \,.\, \varepsilon^2$$
$$x^{(2)} = \cdot 026612 - 8\cdot 1680 \,.\, \varepsilon - 0\cdot 992 \,.\, \varepsilon^2$$
$$x^{(3)} = \cdot 020364 - 6\cdot 2574 \,.\, \varepsilon + 2\cdot 719 \,.\, \varepsilon^2$$
$$x^{(4)} = \cdot 007875 - 2\cdot 4944 \,.\, \varepsilon - 0\cdot 684 \,.\, \varepsilon^2$$
$$x^{(5)} = \cdot 007992 - 2\cdot 4819 \,.\, \varepsilon - 2\cdot 678 \,.\, \varepsilon^2$$

The symbols $x^{(1)}$, $x^{(2)}$, &c. stand for the errors, or rather, for the quantities which the want of perfect exactness in the experimental numbers, makes it necessary to supply in order that the same value of $\varepsilon$ may produce an equality in all the five expressions. The best mode of solution is to employ the method of the least squares, or to determine $\varepsilon$ so as to satisfy this equation, viz.

$$x^{(1)} \frac{dx^{(1)}}{d\varepsilon} + x^{(2)} \frac{dx^{(2)}}{d\varepsilon} + \&c. = 0.$$

Thus we have,

$$0 = (\cdot 009642 - 3\cdot 0662 \,.\, \varepsilon + 0\cdot 341\, \varepsilon^2)(-3\cdot 0662 + 0\cdot 682\, \varepsilon)$$
$$+ (\cdot 026612 - 8\cdot 1680\, \varepsilon + 0\cdot 992\, \varepsilon^2)(-8\cdot 1680 + 1\cdot 984\, \varepsilon)$$
$$+ \&c.$$

All the operations being performed, this final equation will be obtained, viz.

$$0\cdot 413829 = 127\cdot 666 \,.\, \varepsilon + 4\cdot 795 \,.\, \varepsilon^2;$$

from which we get $\varepsilon = \cdot 00324$. And if this value of $\varepsilon$ be substituted in the foregoing expression of $\Delta$, we shall find $\Delta = 60655\cdot 9 \times 1\cdot 003295 = 60855\cdot 7$, or 60856. The direct method of calculation here followed, has therefore brought us to the same elements as before, which, as has already been shown, represent all the five measurements with very small errors*.

It is easy to verify the value that has been found for $\Delta$; namely, by combining the original equations so as to eliminate the terms containing $\varepsilon$, or so as to render the same terms so small that they may be neglected. Thus, if the expression of the French measurement be multiplied by 2·9, we shall have,

$$2179544 = \Delta(35\cdot 8739 - 18\cdot 2152 \,.\, \varepsilon - 30\cdot 351 \,.\, \varepsilon^2);$$

and, if the Indian expression be subtracted from this, the remainder will be,

$$1580914 = \Delta(25\cdot 9780 - 0\cdot 0167 \,.\, \varepsilon - 36\cdot 514 \,.\, \varepsilon^2),$$

from which we get $\Delta = 60856$ as before. We are therefore sure that there is no uncertainty in the values that have been assigned to the two elementary quantities.

The elements of the elliptical meridian being known, we

* Phil. Mag. for May, p. 346.

may

may thence deduce its curvature at certain fixed points; and, in order to judge more accurately of the near agreement of the measurements with the theory, we may compare them separately with the curvature of that portion of the meridian to which they are nearest in situation. For this purpose I shall use the notation D ($\lambda$) to denote the length of a degree of which the middle point is placed in the latitude $\lambda$. The extreme latitudes of the degree will therefore be $\lambda+\frac{1}{2}$ and $\lambda-\frac{1}{2}$; and, in the formula (A)*, we shall have $n = 1°$, $p \sin n = 1°$, $\cos n = 1$, $m = 2\lambda$; consequently,

$$D(\lambda) = \Delta\{1 - \varepsilon(\tfrac{1}{2} + \tfrac{3}{2}\cos 2\lambda) + \varepsilon^2(\tfrac{1}{16} + \tfrac{15}{16}\cos 4\lambda)\}. \quad \text{(C)}$$

In this expression put $\lambda$ successively equal to 0°, 45°, 90°; and we shall have, for the length of 1°,

| | | Fathoms. |
|---|---|---|
| At the equator, | $D(0°) = \Delta(1 - 2\varepsilon + \varepsilon^2)$ | = 60462·4 |
| At latitude 45°, | $D(45°) = \Delta(1 - \frac{1}{2}\varepsilon - \frac{7}{8}\varepsilon^2)$ | = 60757 |
| At the pole, | $D(90°) = \Delta(1 + \varepsilon + \varepsilon^2)$ | = 61054 |

Let us now compare the arcs measured in Peru and India, with the curvature of the meridian at the equator. From the expressions just set down, we get,

$$\Delta = D(0°).(1 + 2\varepsilon + 3\varepsilon^2);$$

and if we substitute this value in the two measurements mentioned, we shall get these three equations, the last being the sum of the other two, viz.

$$188510 = D(0°)\ (3{\cdot}11750 + 0{\cdot}0089.\varepsilon - 0{\cdot}005.\varepsilon^2)$$
$$598630 = D(0°)\ (9{\cdot}89589 + 1{\cdot}5933.\varepsilon - 0{\cdot}546.\varepsilon^2)$$
$$787140 = D(0°)\ (13{\cdot}01339 + 1{\cdot}6022.\varepsilon - 0{\cdot}551.\varepsilon^2)$$

In the first of these expressions the terms containing $\varepsilon$ may be neglected, and D (0°) will be found equal to 60468, or near 6 fathoms too long, which arises from the Peruvian arc being about 20 fathoms too long. If we substitute the value of $\varepsilon$ in the two remaining expressions, we shall get 60461·3 and 60462·9 for the value of D (0°), hardly different from the length previously deduced from the two elements.

The French arc is nearly bisected by the parallel of 45°, and it must be compared with the curvature at that latitude. Now,

$$\Delta = D(45°).(1 + \tfrac{\varepsilon}{2} + \tfrac{9}{8}\varepsilon^2);$$

and, this value being substituted in the expression of the measurement in question, we shall obtain,

$$751567 = D(45°).(12{\cdot}3703 - 0{\cdot}0960.\varepsilon + 0{\cdot}310\,\varepsilon^2).$$

* Phil. Mag. and Annals for May, p. 344.

 Here

Here the factor on the right-hand side is but slightly affected by the terms containing $\varepsilon$, which reduce it to 12·37; and D (45°) is found equal to 60757, the same value derived from the two elements, and very nearly the same that results from the actual measurement without any assumption about the figure of the earth *. It is therefore certain that the elements which have been found represent with great exactness both the whole arc between Dunkirk and Formentera, and also its mean curvature. I have likewise found that the same elements represent, with small errors, the partial arcs between Dunkirk and Montjouy, and Montjouy and Formentera. But it is well known that the four sections of the arc between Dunkirk and Montjouy are very irregular; and the causes of the irregularity have not been very well explained. Whatever these causes may be, they extend their influence to the measurements that have been made in England. Unless I have been misinformed, a re-measurement between Greenwich and Paris has lately been executed, the results of which, when made public, will probably help to clear up the perplexing anomalies that occur in the comparison of the partial arcs.

We may now compare the Swedish, or most northerly, arc with the curvature at the pole. We have,

$$\Delta = D\,(90°) \times (1-\varepsilon);$$

and, by substitution, we get,

$$98870 = D\,(90°)\,.\,(1{\cdot}622022-0{\cdot}7842\,.\,\varepsilon-0{\cdot}860\,.\,\varepsilon^2).$$

From this equation, taking the foregoing value of $\varepsilon$, D (90°) will be found equal to 61051, or only three fathoms less than the value deduced from the two elements; and even this difference will disappear, if we take into account the error of the arc itself.

If in the expression (C) we make

$$\text{Cos}\ 2\,\lambda = -\tfrac{1}{3},\ \lambda = 54°\ 44',$$

we shall get $\cos 4\,\lambda = -\tfrac{7}{9}$, and

$$D\,(\lambda) = \Delta(1-\tfrac{2}{3}\,\varepsilon^2) = \Delta.$$

It appears, therefore, that a degree of the meridian in Britain, very nearly midway between York and Edinburgh, is equal to a degree of the earth's equator. Although the Trigonometrical Survey has been extended between the extreme north and south points of the island, yet, as far I know, the calculations have not been completed except for a small part of the meridian between Dunnose and Clifton. We cannot therefore verify what has just been shown by comparing it with any

* Phil. Mag. and Annals for May, pp. 346, 347.

actual

actual measurement in the proper latitude; but we may in some measure be enabled to judge of its exactness by employing the portion of the meridian that has been calculated. Let $\lambda'$ represent the mean latitude between Dunnose and Clifton, then

$$\lambda = 54^\circ\ 44'\ \ 0''$$
$$\lambda' = 52\ \ \ 2\ 20;$$

and, by means of formula (C), we shall readily obtain,

$$D(\lambda') - D(\lambda) = \tfrac{3}{2}\Delta\,\varepsilon\,(\cos 2\lambda - \cos 2\lambda'),$$

the term containing $\varepsilon^2$ being insensible. Now, by dividing the length of the English arc by its amplitude, we get $D(\lambda) = 60824{\cdot}1$; and hence,

$$D(\lambda') = 60824{\cdot}1 + 26{\cdot}6 = 60850{\cdot}7,$$

which is only five fathoms less than the exact value of $\Delta$, and this deficiency is caused by the small error of the arc, which is too short.

It follows, from the foregoing minute examination, that the five arcs are represented with great precision by the elements that have been found; and further, that each arc, taken separately, agrees accurately with the curvature of that portion of the meridian in which it is situated. And there cannot be a more satisfactory way of proving that the meridian of the earth is an ellipsis, than by showing that it coincides with that figure, at the equator, at the mean latitude of 45°, at the parallel of 54° 44′ where the curvature is identical to the equatorial circle, and at the pole. We have briefly endeavoured to accomplish this task as far as the measurements in our possession put it in our power to do so; but the proof would have been much more complete, if we could have confirmed it by the length of the meridian through the whole extent of Britain.

To the fixt points on the meridian that have already been mentioned, we may add another intermediate between the equator and the parallel of 45°. In the expression (C), put,

$$\cos 2\lambda = +\tfrac{1}{3},\ \lambda = 35^\circ\ 16';$$

then,
$$D(\lambda) = \Delta\left(1 - \varepsilon - \tfrac{2}{3}\varepsilon^2\right) = \Delta\,(1 - \varepsilon).$$

At this latitude, which is the complement of 54° 44′, the radius of curvature of the meridian is equal to half the polar axis. The length of the degree depends upon the elements of the ellipsis; and it may serve as a quantity of reference for judging of the consistency of any measurement made near its parallel of latitude. In the present state of this research there is no measurement that can be compared with it. The nearest to

 it

it is the degree of La Caille at the Cape of Good Hope, in latitude 33° 18′; but this is so disproportionately great, that no use can be made of it.

May 12, 1828. J. IVORY.

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## LXVI. *On the characteristic Equation of Curve-surfaces capable of being evolved in a Plane.* By M. FAYOLLE.

*To the Editors of the Philosophical Magazine and Annals.*

Messieurs,

CE n'est pas sans surprise que j'ai lu, dans votre dernier N°. du *Philosophical Magazine*, le passage suivant, p. 334:

$$\frac{d^2 z}{d x^2} \cdot \frac{d^2 z}{d y^2} - \left(\frac{d^2 z}{d x d y}\right)^2 = 0,$$

" an equation which, indeed, has long been known, but in the opinion of Mr. Gauss *has never yet been rigorously demonstrated.*"

Les géomètres savent que Monge et Meusnier ont donné des démonstrations *rigoureuses* de cette équation différentielle partielle du second ordre, qui appartient aux surfaces développables. Pour s'en assurer, je renvoie vos lecteurs aux volumes 9 et 10 de la collection des *Mémoires des Savans étrangers.*

Ce 15 Mai, 1828. FAYOLLE,
Ancien chef d'étude à l'école Polytechnique.

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## LXVII. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Being enabled to resume our regular reports of the proceedings of this Society, we commence at present with a list of the papers read before the Society during the present session, which have not hitherto been noticed in the Philosophical Magazine.]

Dec. 20, 1827.—RESEARCHES to discover the faculties of pulmonary absorption with respect to charcoal. By G. Pearson, M.D. F.R.S. —A Catalogue of nebulæ and clusters of stars in the southern hemisphere, observed at Paramatta in New South Wales. By James Dunlop, Esq. in a letter addressed to Sir Thomas M. Brisbane, Bart. Communicated by Mr. Herschel.

Jan. 10, 1828.—On the life of plants and animals. By Sir G. Gibbs, M.D. F.R.S.—Observations on the comparative magnetic intensity shown by a horizontal needle at the bottom and on the tops of mountains at Port Bowen and Spitzbergen. By Captain Henry Foster, R.N. F.R.S.

17.—On Capt. Parry's and Lieut. Foster's experiments on the velocity of sounds. By Dr. G. Moll, Professor of Natural Philosophy in the University of Utrecht. Communicated by Capt. Henry Kater, V.P.R.S.—An account of a series of experiments, made with

with a view to the construction of an achromatic telescope with a fluid concave lens, instead of the usual lens of flint glass. In a letter addressed to the President. By P. Barlow, Esq. F.R.S.

24.—On the structure and use of the capsulæ renales. By Sir Everard Home, Bart. V.P.R.S.—Abstract of a meteorological journal kept at Benares in the years 1824, -25, and -26; with remarks. By James Prinsep, Esq. Communicated by Dr. Roget.—Description of a percussion rifle, igniting by a spring instead of a lock. By Lieut.-Col. Miller, F.R.S.*

31. Feb. 7.—An account of trigonometrical operations in the years 1821, -22, and -23, for determining the difference of longitude between the royal observatories of Paris and Greenwich. By Capt. Henry Kater, V.P.R.S.

14.—On the mode in which the nerves belonging to the organs of sense terminate. By Sir E. Home, Bart. V.P.R.S.—Experiments on heated iron, in reference to the magnetic and electric fluids. By William Ritchie, Esq. A.M. Rector of the Royal Academy of Tain. Communicated by Captain Sabine.

April 24.—A paper was read, containing An account of experiments on the elastic curve. By B. Bevan, Esq. Communicated by the President.

In inquiries on the strength of materials, it is often desirable to know the real nature of the curve assumed by a prismatic rod, when acted upon by the weight of its own parts. This curve has generally been stated to be the parabola; but repeated observation has led the author to doubt the accuracy of the theory from which this conclusion has been deduced; and with a view, therefore, to determine, by direct trial, the real form of the curve, he instituted a series of experiments on prismatic rods, of various substances, and of various depths and lengths, some fixed at one end, and others supported at both ends, in a horizontal position. In every instance he found the actual curve to differ from a parabola, and the deviations in the several points examined were such as indicated a regular and determinate species of curve. No modification of the exponent of the order of the parabola was adequate to express the relation of the coordinates with sufficient accuracy in all cases. He found, however, after many trials, that the following formula, which is that of the common hyperbola, gave a very near approximation in all practical cases, namely, $Ax^2 + Bx = y^2$

The accurate determination of the elastic curve is a subject of some importance in practical mechanics; since the rules at present used by mathematicians and engineers for determining the modulus of elasticity of different materials, are founded upon the parabolic theory, and must therefore be liable to error.

May 1.—A paper was read, intitled, "A description of a vertical floating collimator, and an account of its application to astronomical observations, with a circle, and with a zenith telescope. By Capt. Henry Kater, V.P.R.S.

The construction of the instrument which forms the subject of this

* See Phil. Mag. and Annals, vol. iii. p. 277.

this paper is a material improvement on that of the horizontal floating collimator, of which an account was given by the author in the Philosophical Transactions for 1825. Its superiority is derived from its adaptation to the vertical, instead of the horizontal, position; by which the sources of error arising from the necessity of transferring the instrument to different sides of the observatory, and of taking the float out of the mercury and replacing it at each observation, are wholly obviated. The vertical floating collimator has the further advantage of being adapted for use, not only with a circle, but also with a telescope, either of the refracting or reflecting kind. Such a telescope, furnished with a wire micrometer, and directed to the zenith, becomes a zenith telescope, free from all the objections to which the zenith sector, and the zenith telescope, with a plumb-line, are liable.

The instrument itself is supported on a square mahogany stand, which slides on two parallel beams, fixed at the upper part of the observatory, in the direction of the meridian, and which has a circular aperture in the centre, having at its edge a projecting rim of iron, to admit of the passage of the telescope. The telescope having a focal length of eight inches, and an aperture of one inch and a quarter, is supported in the vertical position by a bridge, connecting it with a circular iron ring, ten inches and six-tenths in diameter, which floats in mercury. The mercury is contained in a circular iron trough, the central aperture of which is sufficiently large to allow of its turning freely round the rim which rises from the margin of the aperture of the stand. The object-glass of the telescope is placed at its lowest end, and its focus is occupied by a diaphragm, composed of two brass plates, each cut so as to form an angle of 135 deg. and placed opposite to each other, so that the angular points are brought to an accurate coincidence, thus leaving on each side intervening spaces, which form vertical angles of 45 deg. each. The telescope below, whether belonging to a circle or a zenith telescope, is to be directed so that the image of these angles shall be bisected by the micrometer wire; for which purpose the diaphragm of the collimator is illuminated by a bull's-eye lantern, placed at a convenient distance upon one of the beams crossing the observatory; the light being reflected downwards by a plane mirror placed in a screen, with a suitable aperture immediately above the collimator. The collimator is then to be turned half round in azimuth, the motion being facilitated by rollers, and limited at its extent by two catches, which receive a projecting wire fixed to the outer circle of the trough. When in this situation, the observation of the diaphragm by the telescope, and the bisection of its angles, are to be repeated, and the mean of the two positions will indicate the exact point of the zenith. Minute directions are given by the author for the construction of all the parts of the collimator, and for their proper adjustments; together with an account of the precautions to be taken in the employment of the instrument. The time required for completing the determination of the zenith point by its means need not exceed two minutes; and if to this be added the time necessary for a second set of observations

observations of the same kind, for the purpose of verification, and of a nearer approach to accuracy, the whole time required will not be more than five minutes, during which it is not probable that any sensible disturbance can have taken place in the position of the instrument from changes of temperature.

Tables are given containing registers of numerous series of experiments made, both by the author and by several of his friends, with a view to determine the stability of the instrument, and the degree of reliance that can be placed in the results. In the first series, out of sixty independent determinations of the zenith point, there are twenty-five, the error of each of which does not exceed one-tenth of a second; thirty-seven under two-tenths; forty-seven under three-tenths; fifty-five under four-tenths; three between four and five-tenths; and two a little above half a second. But the author thinks it probable that the greater part of these errors, minute as they are, must be attributed to want of power in the micrometer, which power is directly as the focal length of the object-glass, or mirror, of the telescope to which it is attached, and which necessarily limits the precision of which it is capable.

The author next gives the results of some experiments with a collimator made for Captain Foster, having a float of only five inches in diameter, and with a telescope five inches long: the errors, generally, do not amount to more than two-tenths of a second.

He then enters into details as to the manner of using the vertical floating collimator in astronomical observations, beginning with the portable azimuth and altitude circle described by the Rev. F. Wollaston in his *Fasciculus Astronomicus*, and applicable to other similar instruments. The new collimator affords, also, the most perfect method of adjusting the line of collimation of a mural circle, or of placing it at right angles to the axis.

The author next proceeds to describe the method of applying the instrument to the zenith telescope. In comparing the observations made by the zenith sector belonging to the Board of Ordnance, with the zenith telescope used in conjunction with the vertical floating collimator, the mean of errors in the former case was $+0''\cdot54$ and $-0''\cdot75$; in the latter, $+0''\cdot44$ and $-0''\cdot66$. From observations made on $\gamma$ *Draconis*, the zenith distance of which at Greenwich is $0^\circ\ 2'\ 6''\cdot36$, and at York Gate $0^\circ\ 0'\ 35''\cdot67$, the difference of latitude between the two places was found to be $0^\circ\ 2'\ 42''\cdot03$, that of Greenwich being $51^\circ\ 28'\ 38''\cdot96$, and of York Gate $51^\circ\ 31'\ 20''\cdot99$. The decimals of a second, by the azimuth and altitude circle and horizontal floating collimator were ·94; by the same instrument and the vertical floating collimator ·76, and by the zenith telescope, and the vertical floating collimator ·99: the mean being ·9″.

From the greater degree of precision attainable by the employment of the vertical floating collimator, from the facility of its construction and application, and the time saved by using it, the author deems it not unreasonable to infer, that, ere long, the use of the level and plumb-line in celestial observations will be wholly abandoned.

LIN-

## LINNÆAN SOCIETY.

May 4.—The reading of Mr. Morgan's paper On the mammary organs of the kangaroo was continued, containing further particulars of the dissection of these parts, as well as of the muscles attached to the marsupial bones, in the adult and impregnated animal. These bones, with their ligamentous and muscular connections, were described, and several errors in Sir Everard Home's published account of these parts were pointed out. The author then stated his own opinions respecting the use of these structures. He stated that the marsupial bones are formed, 1st, for the purpose of giving that firm support to the superincumbent abdominal viscera which the narrow pelvis of the animal is incapable of affording while in the erect posture; and, 2ndly, for the purpose of constituting a fixed point of resistance against which the mammæ are squeezed by the muscular girdle already described as inclosing those glands between their fibres. By this arrangement the female is enabled to empty by compression the excretory ducts of its mammæ, and thus to force their secretions into the mouth of the imperfectly organized young, which during the earlier periods of its existence appears incapable of extracting a nutritious fluid from that part by the usual means.

It appears that the secretion of this fluid (or milk) takes place only in the larger and lower gland, and that its ejection through the inferior and longer teat is assisted by a muscular investment which incloses the ducts throughout their whole course from the gland to the extremity of the nipple. The existence of this structure has been noticed by M. Geoffroy St. Hilaire, who has assigned to it the same use. Under this compressing muscle of the lower (or as Mr. Morgan has named it, the true marsupial) teat, a congeries of vessels which principally consisted of veins was described as forming a plexus around the central fasciculus of ducts. These veins, together with those of the gland, were stated to occasion a considerable distention of the mammary organ during the time of suckling, in consequence of the congestion which must necessarily occur in the vessels at that period, from the pressure made upon their main trunks by the action of the compressing muscle of the mamma; for it has been found that the size of the organ on such occasions exceeds that which a loaded state of the ducts only could produce. The mammæ were found, as in the virgin animal, to consist in double glands on each side, the upper and smaller presenting the same anatomical characters as in the former instance; its excretory ducts, however, in their course towards the upper nipple were found to be inclosed in an indistinct muscular sheath, and there was a faint indication of the existence of a plexus of vessels similar to that which was found in the lower or true marsupial teat. This smaller mammary organ is considered by the author as analogous to the supernumerary mammæ and teats of other mammiferous animals, since the lower or true marsupial mammary glands and their teats appear to perform exclusively the office of preparing a nutritious fluid for the support of the young animal.

May

May 24.—This day the Anniversary Meeting of the Society took place, at which Edward, Lord Stanley, was elected President, in the room of the late Sir J. E. Smith; and Edward Forster, J. E. Bicheno, and R. Taylor, Esqs. respectively re-elected to the offices of Treasurer, Secretary, and Under Secretary.

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GEOLOGICAL SOCIETY.

March 21.—Benjamin Silliman, M.D. LL.D. of Yale College, North America, was elected a Foreign Member of this Society. Francis Finch, Esq. and Thomas Winter, Esq., both of West Bromwich, Staffordshire, were elected Fellows of this Society.

A paper was read, intitled "Topographical and Geological Notices, from information collected during the Expedition to the North-west coast of America under the command of Captain Franklin; by John Richardson, M.D. F.R.S., &c."

The expedition under Captain Franklin having arrived at their intended winter-quarters on the shore of Great Bear Lake, examined in 1825 the vicinity of that lake, and the course of the Mackenzie River from thence to the sea. The author subsequently accompanied Captain Franklin down the river, as far as Point Separation, in lat. 67° 38′; from whence the latter proceeded westward to lat. 70° 26′, long. 148° 52′:—the extreme western point seen by the expedition being in long. 149° 37′ west. Dr. Richardson at the same time went eastward to the mouth of the Copper Mine River, and thence returned overland and across the Great Bear Lake, to the head-quarters.

This paper contains an account of the specimens collected, and the geological observations made by both divisions of the party; and gives in considerable detail a description of the vicinity of Great Bear Lake, with a more general one of the banks of the Mackenzie and of the coast to the East of it; to which are subjoined some observations respecting the country previously passed over by the expedition, between Lake Superior and Fort Franklin. The distances traversed being, in latitude, about 23 degrees N. of Lake Superior; and in longitude, altogether about 80 degrees;—60 degrees to the west of Lake Superior, and 20 degrees on the coast, eastward from the mouth of the Mackenzie. The total extent passed over in America by the expedition, in going and returning, was about 14,000 miles; and that surveyed and laid down for the first time on the maps, is about 5000 miles.

The author however mentions, that a very limited portion of his time could be devoted to geological researches; the ground being for eight months in the year covered with snow; and the other objects of the journey demanding his principal attention during the short summer.

The country described consists, in general, of three or four formations, or series of beds, which occupy well marked divisions:—

1. The most western division comprehends the Rocky Mountains, which appear to be composed of primitive rocks; and the course of the ranges is from about S.E. to N.W.; the faces of the hills to

the eastward being abrupt, but the slope towards the W. more gradual. These mountains join the sea on the west of the Mackenzie; and at their termination are divided into four groups or chains, to which Captain Franklin has given the names of *Richardson's, Buckland's,* the *British,* and *Romanzoff* chains. The land again becomes lower to the west of the chain last mentioned, and continues to be so from thence to the remotest point arrived at; no prominent elevations having been observed to the west of long. 146°.

2. Another very extensive tract of primitive rocks in the north of America has nearly the same direction with the range of the Rocky Mountains, but the two ranges converge towards the north; the distance between them being, in lat. 50°, 700 miles;—about 220 miles where it was traversed by Captain Franklin, in going from Hudson's Bay to Lake Winipeg;—and in lat. 66° only 200 miles. This eastern primitive tract consists principally of granite and gneiss; it exhibits great uniformity of character, contains no very elevated ground, and is in fact traversed by several rivers which arise in the Rocky Mountains. It is flanked on both sides by extensive calcareous tracts.

3. The north-eastern extremity of the Rocky Mountain chain, near the mouth of the Mackenzie, consists of grauwacke and other transition-rocks, interposed apparently between the primary and the calcareous districts. In some of the other places described, a rock resembling the old-red-sandstone of England, occupies a similar situation.

4. The tract that intervenes between the Rocky Mountains and the eastern primary band above mentioned, consists principally of calcareous strata, and is remarkable from its including, throughout, a series of great lakes or lake-like rivers, with which a very large proportion of the surface is occupied, and the bottoms of which appear in several instances to be below the level of the sea. This intermediate calcareous band was traced in one place by the author, to the width of about 280 miles from the eastern primary tract; and one of its highest summits, about a mile from Bear Lake, was supposed to be about 950 feet above the sea. The limestone of which this district is composed, as well as that of the calcareous tract on the east of the primary band above mentioned, presents considerable uniformity of character: the ridges of hills are nearly parallel to those of the Rocky Mountains; and a very large proportion of the rocks observed by the author, was found to be magnesian limestone,—apparently belonging either to the magnesian limestone formation of England, or to our mountain-limestone, which it is well known includes in Europe numerous beds of dolomite.

The fossils also of this calcareous formation, are of the same genera with those of our mountain-limestone and of the magnesian beds in the north of England; including corallines, productæ, terebratulites, and a cardium: and in several places the calcareous beds contain a large proportion of chert and flinty slate. The correct determination of the relations of this great calcareous tract, is one of the chief points of interest remaining for future research, in the country described by the author; for while he agrees with other geologists in assigning

assigning a portion of it, (as in the vicinity of Lake Winipeg,) to the mountain-limestone of Europe, he justly remarks that in other places the quantity of gypsum, in connection with copious salt springs, and great abundance of petroleum, together with the occurrence of soft marly-sandstone, and beds of breccia interstratified with those of dolomite, and above all, the fact that dolomitic limestone is by far the most common and extensive rock in the deposit, would lead to its identification with the zechstein of continental geologists,—the magnesian limestone of the North of England.

5. Above the limestones, and in some cases, it would appear, alternating with the dolomite, is a very extensive deposition of sandstone, bituminous-shale, and slaty-clay (which last exhibits in some places the peculiar structure denominated cone-in-cone) containing nodular ranges of clay-iron-stone and beds of lignite. The shales include impressions of ferns, lepidodendrons, and other vegetable remains; and among the fossils of this formation was also found an ammonite, supposed by Mr. Sowerby to belong to a part of the oolitic series of England. It deserves inquiry therefore, whether this may not be the equivalent of the carboniferous strata which form a portion of the oolitic series in Yorkshire, and at Brora in Scotland.

The series of beds above described occurs extensively in the course of the Mackenzie River, and on the shores of the Great Bear Lake; and from its being found also on the northern coast, at a distance of about 300 miles from thence, and in a direction precisely corresponding, it not improbably occupies the intervening country.

About Cape Bathurst (lat. 70° 36′, long. 127° 35′) cliffs of alum shale form the coast for more than 60 geographical miles, and are described as resembling those of Whitby in Yorkshire.

6. On the promontory of Cape Lyon are extensive ridges of columnar trap associated with limestone and slate-clay; and greenstone is of frequent occurrence there and in some other places. Porphyry also, forms low conical hills in the high ground between the Copper Mountains and Bear Lake.

7. Near the western boundary of the limestone, and not far from the base of the Rocky Mountains, there occur at intervals, from lat. 50° to 69° N. extensive (*tertiary?*) deposits, consisting generally of sandstone, gravel, clay more or less bituminous, and brown wood-coal. In some spots the wood-coal was replaced by an excellent pitch-coal, the fractured surface of which is marked with very peculiar concentric semicircular depressions; and it is interesting to know that this coal, which would be excellent fuel for a steam-vessel, occurs on the coast of the Polar sea near the Mackenzie in considerable quantity. This formation contains layers of a variety of pipe-clay which is eaten by the natives, and is said to sustain life for a considerable time. The deposit at the mouth of Bear Lake River includes some beds of impure porcelain earth. The author found occasionally much difficulty in distinguishing the sandstones and shales of this deposit, from those of the formation mentioned above in Section 5.

8. Among the indications of other strata more recent than the magnesian limestone, was a loose fragment of soft limestone found

 at

at the mouth of Babbage River, on the coast west of the Mackenzie, containing the species of Cyclas (C. medius) which occurs extensively in the weald-clay of England.

This memoir, which will be published in full in the Appendix to Captain Franklin's Narrative of the expedition, is illustrated by maps and drawings, and accompanied by a catalogue in detail, of the specimens referred to, which have been presented to the Geological Society.

April 18.—William Hutton, Esq. of Newcastle-upon-Tyne, Beriah Botfield, Esq. of Christchurch Oxford, and William Parker Hamond, Esq. of St. John's College Cambridge, were elected Fellows of this Society.

A Paper was read, "On the fossil remains of two new species of Mastodon, and of other vertebrated animals, found on the left bank of the Irawadi; by William Clift, Esq. F.G.S. F.R.S., conservator of the Museum of the Royal College of Surgeons."

The author having been requested to describe the fossil remains which the zeal and liberality of Mr. Crawfurd have transferred from the deserts of the Irawadi to the Museum of the Geological Society, confines himself strictly to zoological and anatomical details; and following the system of Cuvier, commences with the

*Pachydermata proboscidifera.*—The only genus of this order indicated by the remains is the Mastodon; and of this there are two species, Mastodon latidens and Mastodon elephantoides, not only commanding attention from their novelty, but from the beautiful gradation which they exhibit between the mastodons already described and the elephant. On comparing the teeth of Mastodon latidens with those of the mastodon of the Ohio (M. giganteum) the denticules are found to be more numerous, and less distant, and the interstices less deep than in those of the latter. The teeth, in short, begin to assume the appearance of those of the elephant. On advancing to Mastodon elephantoides, these features of similarity are more strongly developed: the many-pointed denticules are still more numerous and more compressed; and the structure, were it not for the absence of crusta petrosa, becomes almost that of the tooth of the elephant. In both, though the teeth are formed upon the principle by which the tooth of the mastodon is distinguished from that of the elephant, the crown of the tooth wears away more like that of the elephant than that of the other mastodons.

The species are thus characterized:

Mastodon latidens.—Mastodon dentibus molaribus latissimis, denticulis rotundatis, elevatis. Palato valdè angusto.

The dentition very much resembles that of the elephant. The molar tooth is gradually pushed forward, and rises as the fangs are added, according to the demand occasioned by the abrasion of the exposed crown, and the consequent absorption of the anterior fang; the posterior part of the tooth not having yet cut the gum, while the anterior portion is completely worn away. Before it are seen the relics of the preceding tooth, the place of which the tooth in use was progressively supplying.

The lower jaw in this species is less square and deeper than it is in M. giganteum.

The

The tusks, judging from the alveoli, must have been of equal volume with those of the largest living elephant.

The following is the measurement of some of the remains of M. latidens.

| | Ft. | In. |
|---|---|---|
| Extreme breadth of fragment of cranium (upper jaw with the greatest part of both grinders) ........ | 1 | 3 |
| Length of ditto ........................... | 1 | 8 |
| Extreme length of right anterior grinder (6 denticuli and the spur) ........................... | 0 | 8¼ |
| Extreme breadth at third denticulus ............ | 0 | 4 |
| Circumference of lower jaw, measured over the grinding surface of the tooth .................... | 2 | 4 |
| Extreme length of tooth ........................ | 0 | 11¾ |
| Extreme breadth ........................... | 0 | 4½ |
| Circumference of the lower extremity of right femur | 2 | 2 |
| Same, round the condyles .................... | 2 | 4 |

Mastodon elephantoides.—M. dentibus latis; denticulis numerosis, compressis.

This species must have been smaller than the last. There is a fine example of the lower jaw, showing the tooth in the highest degree of perfection. The tooth is 11 inches long and 3½ inches broad, has no less than ten denticules, and each of these denticules is mamillated with small points; five being the smallest, and eight the greatest number on any one denticule. In front of this tooth is seen the remnant of the preceding one, worn down and disappearing; and behind it is the cavity wherein the young tooth, intended as a successor to that in existence, was in the course of formation. The denticules are much more compressed than those in the species last described; they are closer together, and the whole tooth approaches still more nearly to that of the elephant, while the jaw is in unison with the appearance of the tooth.

*Pachydermata ordinaria.*—In this group we have the remains of the genera Sus, Hippopotamus, and Rhinoceros. Of the first there is only a single specimen, consisting of a small portion of the lower jaw, containing one molar tooth and the fragment of another. Of the second there are but few fragments, nor are they sufficiently characteristic to warrant a definition of the species, which must have been comparatively small. Of the third there is a portion of the upper jaw, containing two molar teeth; and portions of the lower jaw with molares, which seem to approach nearer to those of the rhinoceros of Java than to those of any other living species.

*Ruminantia.*—In this group we have fragments of the ox and of the deer.

### Reptilia.

*Chelonia*, Cuv.—(*Testudinata*, Bell).—There are many fragments of a large species of trionyx, and some of an emys. But the remains are not sufficiently defined for specific description.

*Sauria.*—Fam. *Crocodilidæ.*—Of this family we have the remains of two genera; viz. a Leptorhynchus allied to, if not identical with, the

the great gavial; and a crocodile resembling Crocodilus vulgaris. Of the former there are portions of the lower jaw and several vertebræ; of the latter, there is the anterior termination of the lower jaw, which must have belonged to a very large individual.

The specimens, in general, do not appear to have undergone any mineral change, with the exception of being abundantly penetrated with iron, and are very brittle. This last circumstance, arising from the loss of their animal gluten, indicates great antiquity, and that they have not been imbedded in any very compact soil; unlike the teeth of the mastodon of the Ohio, which lie in a strong blue clay, and have almost as much animal matter as is to be found in a recent tooth.

The bones are almost in every instance broken; and from the firmness of texture of most of them, the direction and cleanness of the fracture, and the sharpness of its edges, the injury, which must have been the result of an immense power operating with sudden violence, appears to have taken place at the period, or very soon after the period, of the destruction of the animal.

A paper was next read "On a collection of vegetable and animal remains, and rocks, from the Burmese Country, presented to the Geological Society by J. Crawfurd, Esq," by the Rev. W. Buckland, D.D. V.P.G.S. F.R.S. &c.

Mr. Crawfurd collected these specimens during his voyage up the Irawadi in a steam-boat, on an embassy to Ava, in the latter part of the year 1826. The author considers them to be of high importance, as affording an answer to the curious, and till now undecided question, whether there be, or be not, in the southern regions of Asia, any remains of fossil quadrupeds analogous to those which are found so widely dispersed in the diluvium of northern Asia, and of Europe and America.

The evidence which Mr. Crawfurd has imported, consists of several chests full of fossil wood and fossil bones, and of specimens of the strata that are found along the course of the Irawadi, from Prome up to Ava, being a distance of nearly 500 miles. The greater part of the fossil wood is beautifully silicified; other specimens of it are calcareous; they are mostly portions of large trees, both monocotyledonous and dicotyledonous, and were found along the whole valley of the Irawadi from Ava to Prome. The bones were all collected from a small district near some wells of petroleum, about half way between these towns, and on the left bank of the river. From Mr. Clift's examination, it appears, that although we have among them no remains of fossil elephants, we have the same fossil pachydermata that are found associated with elephants in Europe; namely, rhinoceros, hippopotamus, mastodon, and hog. We have also two or three species of ruminantia resembling the ox, antelope and deer, with the addition of the gavial and alligator, and two freshwater tortoises, namely, trionyx and emys.

The teeth of the mastodon belong to two unknown species of that genus, both of them approaching in size to the largest elephant. Mr. Clift has designated them by the names of *Mastodon latidens* and *M. elephantoides.* The teeth are from animals of all ages; and there are

are many fragments of ivory, derived probably also from the mastodon.

The remains of the mastodon are by far the most abundant in this collection, and amount to about 150 fragments.

Of the rhinoceros there are about 10 fragments.

Of a small species of hippopotamus, 2.

Of the hog, 1; and of the ox, deer and antelope, about 20.

Of the gavial and alligator, about 50.

Of the emys, 20; and trionyx, 10.

One fragment of emys is so large, that the animal of which it formed a part, must have been several feet in width.

The state of preservation of these bones is very perfect, from their being penetrated with hydrate of iron, and thereby rendered strong. Not one of them is silicified, though they have been erroneously stated to be so, in some of the periodical journals.

The district in which they were found is a little North of the town of Wetmasut, and is composed of barren sand-hills and beds of gravel intersected by ravines, and cemented occasionally into a breccia by carbonate of lime, and sometimes by hydrate of iron. Over the surface of these hills were scattered the fragments of bones and wood, some quite naked and loose, others half buried in the sand and gravel. Many fragments of wood lay also at the bottom of the ravines. About one-third of the bones have been slightly rolled; and the rest had all been broken before they were lodged in the places where Mr. Crawfurd found them, and where they appear to have been dispersed and buried, by the action of the same waters that produced the diluvial sand and gravel, whence they have since been washed out, and left bare by the action of rains and torrents.

Concretions of sand and gravel adhere to many of the bones, but they contain no traces of shells, and differ mineralogically from all the rock specimens in this collection, which we recognize as belonging to tertiary and freshwater strata.

Indications of freshwater formation were found in one spot only, not far from the fossil bones, and they consist of a marly blue clay, abounding with shells of a large and thick species of Cyrena.

The tertiary rocks are: 1st, a dark slaty limestone, containing many shells, that have been identified by Mr. Sowerby with those of the London clay. 2nd, a yellow sandy limestone containing shells, and resembling the calcaire grossier, and 3rd, a soft greenish sandstone resembling the sandy beds of our plastic clay formation.

This London clay and *calcaire grossier* afford an additional locality of these strata to those indicated by the specimens described by Mr. Colebrooke, in vol. i. Part 1, 2nd series of the Geological Transactions, —which had already established the existence of this formation in the N.E. border of Bengal.

Mr. Crawfurd states distinctly, that it is impossible to refer the situation of the bones, or the origin of the hills containing them, to any operations of the existing river: these hills are sixty feet above the level of its highest flood; the effect of its actual operations, he observes also, is distinctly visible in the shifting islands of mud and sand that

that abound along the whole course of the river within this high flood level, and in the great alluvial delta that extends from a little below Prome to Rangoon and the gulf of Martaban.

The recent bones and recent wood which he observed to be stranded on some of these islands, were not in a state of progress towards becoming mineralized, but were falling rapidly to decay.

The existence of so many animal remains analogous to those that occur in the diluvium of Europe, in a matrix which so nearly resembles that diluvium, and which so decidedly differs from the alluvium, and freshwater, and tertiary strata of the adjacent country, seems to authorize us to refer this matrix to a similar diluvial deposit in the valley of the Irawadi, reposing irregularly upon the tertiary and other stratified rocks, that form the basis of that district.

Besides the tertiary strata above enumerated, there are specimens of grauwacke and transition-limestone from several distant points in the valley of the Irawadi between Prome and Ava, which render it probable that the fundamental rocks of this valley belong to the transition series.

On the north of Ava there are chains of primitive mountains abounding with statuary marble, associated, as usual, with hornblende and mica slate.

We may therefore consider it as now established, on the authority of Mr. Crawfurd's notes and specimens, that the Burmese country not only contains the remains of fossil animals above enumerated, but also affords examples of the following geological formations, which can be identified with those of Europe; namely—

1. Alluvium.
2. Diluvium.
3. Freshwater Marl.
4. London Clay and Calcaire grossier.
5. Plastic Clay, with its sands and gravel.
6. Transition limestone and grauwacke.
7. Primitive marble and mica slate.

On the same evening, after the ordinary business of the Society had been transacted, a special general meeting was held, when the President having stated, that the Lords Commissioners of his Majesty's Treasury had been pleased to transfer to this Society some of the apartments in Somerset House, formerly used as the Lottery Office, and lately in the possession of the Royal Society; and that a sum not less than 1000*l.* would be required for preparing the said apartments for the reception of the Society, and the removal of their Library and Collections:—

It was resolved unanimously,

I. On the motion of Davies Gilbert, Esq. M.P., Pres. R.S., seconded by Henry Warburton, Esq., M.P.,—That the thanks of this Society be given to the Right Honourable the Lords Commissioners of his Majesty's Treasury, for the grant which they have been pleased to make to this Society, of apartments in Somerset House.

II. On the motion of the Rev. Dr. Buckland, Professor of Geology at Oxford, seconded by the Rev. A. Sedgwick, Woodwardian Professor

fessor at Cambridge,—That the thanks of this Society be given to Davies Gilbert, Esq. the President, and to the Council of the Royal Society, for their aid and cooperation in obtaining from the Lords of his Majesty's Treasury a grant of the apartments in Somerset House.

III. On the motion of Robert Ferguson, Esq., seconded by Leonard Horner, Esq.,—That a Subscription be immediately entered upon, to defray the expense of the necessary repairs in the apartments recently granted to the Society in Somerset House, and of the removal thereto.

May 2.—At a special general meeting holden this day at one o'clock, for the purpose of electing a Member of the Council in the room of Ashhurst Majendie, Esq.; and also for electing a Secretary in the room of R. I. Murchison, Esq., and a Foreign Secretary in the room of Henry Heuland, Esq., who had retired from their respective offices;

It was resolved unanimously,

I. That the thanks of this Society be given to Ashhurst Majendie, Esq., retiring from the Council.

II. That the thanks of this Society be given to Henry Heuland, Esq., for his long services in the office of Foreign Secretary, and for the high regard which he has always manifested for the welfare of the Society.

III. That the thanks of this Society be presented to R. I. Murchison, Esq. on his retiring from the office of Secretary.

A ballot having been held for electing a Member of Council in the room of Ashhurst Majendie, Esq., the scrutineers reported that Dr. Henry Burton was duly elected.

A ballot having been held for electing a Secretary in the room of R. I. Murchison, Esq., and a Foreign Secretary in the room of Henry Heuland, Esq., the scrutineers reported that Dr. Burton was elected Secretary; and that R. I. Murchison, Esq., was elected Foreign Secretary.

At the Ordinary Meeting holden on the same evening, John Claudius Loudon, Esq., of Porchester Terrace, Bayswater; and Thomas Copeland, Esq., of Golden Square, were elected Fellows of this Society.

An extract of a letter was read from Lieutenant William Glennie, R.N., dated Mexico, May 6th, 1827, intitled "The Ascent of Popocatapetl."

Many contradictory reports having long existed respecting the volcanic nature of this mountain, the author felt desirous of ascertaining its actual condition in person.

The ascent commenced during the month of April 1827, from the village of Ameca, situated in the province of Puebla, and near the N.W. foot of the volcano, at an elevation of 8216 feet above the level of the sea, and distant 14 leagues from Mexico.

The author describes the sides of the mountain as thickly wooded with forests of pines, extending to the height of near 12,693 feet, beyond which altitude vegetation ceased entirely. The ground consisted of loose black sand of considerable depth, on which numerous fragments of basalt and pumice-stone were dispersed. At a greater elevation, several projecting ridges, composed of loose fragments of basalt, arranged one above another, and overhanging precipices 600 or 700 feet deep, presented formidable impediments to the author's progress; and, in one direction only, a ravine was observed to pass through these ridges, having its surface covered with loose black sand, down which fragments of rocks ejected from the crater continually descended.

After twelve hours of incessant fatigue the author gained the highest point of the mountain on the western side of the crater, 17,884 feet above the sea; at which station the mercury in the barometer subsided to 15·63 inches, and the temperature indicated by the attached and detached thermometers, was respectively 39° and 33° Fahr. at 5 o'clock P.M., when exposed to the direct rays of the sun. The plain of Mexico was enveloped in a thick haze, and the only distant objects visible at that time, were the volcanoes of Orizaba and Iztaccihuatl. The crater of Popocatapetl appeared to extend one mile in diameter, and its edges of unequal thickness descended towards the east. The interior walls consisted of masses of rock arranged perpendicularly, and marked by numerous vertical channels, in many places filled with black sand. Four horizontal circles of rock differently coloured were also noticed within the crater; and from the edges of the latter, as well as from its perpendicular walls, several small columns of vapour arose smelling strongly of sulphur. The noise was incessant, resembling that heard at a short distance from the sea shore during a storm; and at intervals of two or three minutes the sound increased, followed by an eruption of stones of various dimensions; the smaller were projected into the ravine before mentioned, the larger fell again within the crater.

The sensations experienced by the author were analogous to those usually felt by travellers at considerable elevations; viz. weariness, difficult respiration, and headache, the latter inconvenience having been first perceived at a height of 16,895 feet. Tobacco smoke and spirituous liquors were also found to produce an unusually rapid effect upon the sensorium.

At the same meeting a letter was read from J. B. Pentland, Esq., addressed to W. H. Fitton, M.D. P.G.S., respecting the fossil remains of some animals from the N.E. border of Bengal.

The author has discovered among the mutilated fragments of bones obtained from the tertiary deposits on the Bramahpootra River in the small state of Cooch-Behar,—presented to the Society some years ago, by David Scott, Esq., and referred to in a former volume of the Transactions *,—the remains of four distinct species of mammalia, making an interesting addition to the list already published by Mr. Colebrooke, viz.—

1. A species of the genus Anthracotherium of Cuvier, which the author proposes to distinguish by the name of A. Silistrense,—a specific denomination derived from one of the many names by which the

* Geol. Trans. 2nd Series. vol. i. p. 135.

great

great Bramahpootra river appears to have been designated by ancient geographers.

2. A small species of the order Ruminantia allied to the genus Moschus.

3. A small species of herbivorous animal referable to the Pachydermata, but more diminutive than any of the fossil or living species of that family at present known.

4. A carnivorous animal of the genus Viverra.

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## ASTRONOMICAL SOCIETY.

March 14.—The first paper read this evening was the following: Ephemeris of the place of Encke's Comet, during the time of its reappearance at the end of the present year. Drawn up at the request of the Council of the Society. By F. Baily, Esq.

| 1828. | Æ. | Dec. | 1828. | Æ. | Dec. |
|---|---|---|---|---|---|
| | h m s | ° ′ | | h m s | ° ′ |
| Aug. 23 | 1 47 24 | +22 43 | Nov. 15 | 21 58 4 | +18 25 |
| 27 | 1 47 8 | 23 20 | 17 | 21 49 36 | 17 20 |
| 31 | 1 46 12 | 23 57 | 19 | 21 41 20 | 16 13 |
| Sept. 4 | 1 44 40 | 24 34 | 21 | 21 33 16 | 15 5 |
| 8 | 1 42 16 | 25 11 | 23 | 21 25 20 | 13 57 |
| 12 | 1 39 0 | 25 48 | 25 | 21 17 36 | 12 48 |
| 16 | 1 35 44 | 26 24 | 27 | 21 9 56 | 11 38 |
| 20 | 1 29 20 | 26 58 | 29 | 21 2 20 | 10 27 |
| 24 | 1 22 36 | 27 30 | Dec. 1 | 20 54 44 | 9 14 |
| 28 | 1 14 28 | 27 59 | 3 | 20 47 4 | 8 1 |
| 30 | 1 9 48 | 28 12 | 5 | 20 39 20 | 6 45 |
| Oct. 2 | 1 4 48 | 28 23 | 7 | 20 31 20 | 5 27 |
| 4 | 0 59 16 | 28 32 | 9 | 20 23 12 | 4 7 |
| 6 | 0 53 24 | 28 40 | 11 | 20 14 40 | 2 42 |
| 8 | 0 47 4 | 28 45 | 13 | 20 5 48 | + 1 14 |
| 10 | 0 40 16 | 28 47 | 15 | 19 56 28 | − 0 20 |
| 12 | 0 33 4 | 28 47 | 17 | 19 46 40 | 1 58 |
| 14 | 0 25 24 | 28 43 | 19 | 19 36 20 | 3 42 |
| 16 | 0 17 24 | 28 36 | 21 | 19 25 32 | 5 32 |
| 18 | 0 9 0 | 28 25 | 23 | 19 14 20 | 7 28 |
| 20 | 0 0 16 | 28 9 | 25 | 19 2 48 | 9 28 |
| 22 | 23 51 12 | 27 50 | 27 | 18 51 16 | 11 31 |
| 24 | 23 41 52 | 27 25 | 29 | 18 40 0 | 13 35 |
| 26 | 23 32 24 | 26 56 | 31 | 18 29 28 | 15 38 |
| 28 | 23 22 44 | 26 23 | 1829. | | |
| 30 | 23 13 0 | 25 44 | Jan. 10 | 18 4 24 | 24 14 |
| Nov. 1 | 23 3 12 | 25 1 | 13 | 18 10 16 | 25 50 |
| 3 | 22 53 28 | 24 14 | 16 | 18 20 12 | 26 57 |
| 5 | 22 43 48 | 23 23 | 19 | 18 31 4 | 27 43 |
| 7 | 22 34 16 | 22 29 | 21 | 18 39 20 | 28 4 |
| 9 | 22 24 52 | 21 32 | 26 | 19 0 40 | 28 24 |
| 11 | 22 15 44 | 20 31 | 31 | 19 21 4 | 28 19 |
| 13 | 22 6 48 | +19 29 | Feb. 9 | 19 53 52 | −27 34 |

 The

The first part of the above ephemeris (to the end of the year 1828) is taken from Mr. Encke's own computations for Paris time $0^{d}.3$, as inserted in Professor Schumacher's *Astronomische Nachrichten*, No. 123. That part of it which belongs to the ensuing year 1829, is taken from a letter addressed to the President of this Society by Dr. Olbers, and is computed for the time of midnight at Paris. The positions are computed for every third or fourth day only, at the beginning and end of the ephemeris, and for every second day towards the middle: this being quite sufficient to enable the observer to find the place of the comet in the heavens. The original computations are extended only to minutes of space; but the right ascensions are here converted into the nearest second of time, for the convenience of the observer.

[It being desirable that as many observations should be made of this comet as possible, particularly in the southern hemisphere, where it will probably be seen after its return from the sun, an extra number of copies of the above ephemeris have been struck off for distribution, not only amongst the Members of the Society, but also for circulation in various parts of the world where the comet is likely to be seen. And Capt. Foster has kindly undertaken to distribute them as much as possible in those parts of the southern hemisphere at which he may touch during the scientific voyage in which he is about to embark.]

There was next read a paper, "On finding the rates of time-keepers;" by E. Riddle, Esq. In this communication the author observes, that there are many persons fond of astronomy who are not possessed of a transit instrument, or who have not a convenient situation in which to place it; and many others, such as nautical men in particular, who have not the means of using one, who are desirous on many accounts, of knowing the *rates* of their chronometers, independent of the absolute time which they indicate. The method of equal altitudes on *each side* of the meridian is the course usually pursued on such occasions: but Mr. Riddle proposes another mode, often much more convenient in practice and equally correct in its results; viz. by taking equal altitudes of a fixed star on the *same side* of the meridian, on successive nights. It is well known that a star will, at the same sidereal hour, arrive at the same altitude, for several succeeding nights: the only difference which occurs, arising from the small change in the aberration, and also from the variation in the refraction. The former is insensible, if the interval between the observations be not too long; and for the latter, appropriate tables are given. The best time for the observation of such stars is when they are due east or due west; and any known star may be chosen for the purpose. All we have to do therefore, is to note the difference of the two consecutive times at which the star attains the same altitude (whatever it be) on the same side of the meridian; and if that difference be *less* than $3^{m}\ 55^{s},91$, the chronometer (presuming that it is regulated to mean solar time) will have *gained*; and if *more*, it will have *lost* so much in a sidereal day. And, if the observations are made at an interval of $n$ days, the $n$th part of the difference between the times of observation, compared with $3^{m}\ 55^{s},91$, will in like manner give the mean rate for that interval; and if this quantity be multiplied by 1,0027, it will give the

the rate for a mean solar day. The author concludes his paper with several practical examples of the method; and also with a formula for reducing a series of observations made on any one night, to the same altitude as shown by a series made on any other night: whereby the whole become strictly comparable.

Lastly, there was read the following communication from the Rev. Thomas John Hussey, to Francis Baily, Esq.

"In forming a correct catalogue of the stars situated between 15° N. and 15° S. declination, and extending from $13^h\ 56^m$ to $15^h\ 4^m$ R.A. from the catalogues of Piazzi and Bradley, and the observations of Lalande and Bessel, Mr. Dawson and myself have been much struck with the difference between the places of particular stars, as laid down by Piazzi and Bradley, and the places assigned by reducing the observations of M. Bessel. The results obtained from the *Histoire Céleste* come as nearly to the standard catalogue as can be expected, considering the nature of the instrument employed for determining the zenith distances, and thence the declination in which principally the differences exist: some few of these between observations of the same star are rather unaccountable, others may be attributed to errors of the press; they are as follow.—*Histoire Céleste*,

| | | | h | m | s | | ° | ′ | ″ | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Page | 154 | Æ. | 14 | 42 | 44·5 | ZD. | 57 | 2 | 12 | read | $34^s$ |
| — | 155 | — | 14 | 13 | 43 | — | 50 | 2 | 36 | — | $12'$ |
| — | 155 | — | 14 | 59 | 38·5 | — | 50 | 30 | 16 | — | $13^h$ |
| — | 155 | — | 14 | 32 | 52·4 | — | 59 | 5 | 42 | — | $49^\circ$ |
| — | 156 | — | 14 | 37 | 47 | — | 50 | 45 | 34 | — | $58^\circ$ |
| — | 167 | — | 15 | 2 | 36 | — | 38 | 47 | 6 | — | $27'$ |
| — | 289 | — | 14 | 44 | 33·5 | — | 61 | 10 | 8 | — | $32^s$? |
| — | 291 | — | 14 | 52 | 7·3 | — | 46 | 59 | 0 | — | $58'$ |
| — | 340 | — | 14 | 45 | 56·5 | — | 54 | 20 | 49 | — | $50^s$ |
| — | 471 | — | 14 | 46 | 18 | — | 39 | 5 | 22 | — | $8^s$ |

Between the places shown by the observations of Lalande and those of M. Bessel, even where there are more than one observation by each of these astronomers, there are differences more or less considerable in *almost* every instance; occasionally, however, there is a surprising coincidence. On this subject we may hereafter trouble you with some remarks, my present object being to point out the differences between the standard catalogue of M. Piazzi and a catalogue reduced to 1800 from the observations of M. Bessel, of the only stars comprised within the above limits, which have been observed more than once by M. Bessel, so as that the place of the star may be considered as pretty nearly ascertained by this indefatigable astronomer alone. Their number amounts to 31, and the results are exhibited in the following table. The differences are expressed in space.

| No. | Mag. | Right-Ascension. Piazzi. | Right-Ascension. Bessel. | Diff. | Declination. Piazzi. | Declination. Bessel. | Diff. | No. of Obs. | When observed. | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | h m s | h m s | ″ | ° ′ ″ | ° ′ ″ | ″ | | | |
| 1 | 8 | 13 56 25,87 | 13 56 25,60 | − 4·10 | +11 47 14·60 | +11 47 13·24 | −1·37 | 2 | 1822—3 | |
| 2 | 8 | 57 39,71 | 57 41,29 | +23·69 | −11 36 26·20 | −11 36 29·51 | +3·31 | 2 | 1824 | +0·9872 annual proper motion? |
| 3 | 8 | 59 39,27 | 59 39,68 | + 6·20 | +11 12 22·50 | +11 12 17·59 | −4·91 | 2 | 1822—3 | |
| 4 | 8 | 14 0 25,10 | 14 0 25,47 | + 5·49 | −10 59 58·70 | −11 0 1·45 | +2·75 | 3 | 1824 | |
| 5 | 8 | 36,90 | 37,04 | + 2·09 | + 1 45 9·00 | + 1 45 4·15 | −4·86 | 2 | 1822 | |
| 6 | 8·9 | 54,84 | 54,65 | − 2·91 | + 5 21 15·40 | + 5 21 12·76 | −2·64 | 2 | 1822 | |
| 7 | 7 | 1 54,80 | 1 55,11 | + 4·64 | − 8 57 8·00 | − 8 57 12·90 | +4·90 | 3 | 1824 | |
| 8 | 8 | 2 53,47 | 2 53,60 | + 1·97 | + 8 57 20·00 | + 8 57 19·06 | −0·94 | 2 | 1823 | |
| 9 | 7·8 | 3 57,10 | 3 57,11 | + 0·08 | − 5 0 37·00 | − 5 0 34·58 | −2·43 | 2 | 1822—4 | |
| 10 | 7 | 4 6,50 | 4 6,96 | + 6·83 | +12 56 30·20 | +12 56 26·13 | −4·08 | 2 | 1822 | |
| 11 | 6 | 5 3,40 | 5 3,47 | + 0·99 | +11 2 59·00 | +11 2 50·41 | −8·59 | 3 | 1822—3 | |
| 12 | 6 | 11 40,87 | 11 41,10 | + 3·42 | − 1 3 58·00 | − 1 4 3·47 | +5·47 | 2 | 1822 | |
| 13 | 8 | 12 4,70 | 12 4,85 | + 2·19 | − 6 50 29·00 | − 6 50 34·76 | +5·76 | 3 | 1824 | |
| 14 | 6 | 13 32,70 | 13 33,05 | + 5·20 | + 9 21 50·20 | + 9 21 51·37 | +1·17 | 2 | 1823 | This star is double, and the observations are of the one which is of Mag. 6. |
| 15 | 7 | 14 9,67 | 14 9,66 | − 0·09 | + 9 9 36·40 | + 9 9 32·79 | −3·61 | 3 | 1822—3 | |
| 16 | 7 | 14 28,24 | 14 28,40 | + 2·36 | + 9 0 19·10 | + 9 0 14·07 | −5·03 | 3 | 1822—3 | |
| 17 | 7·8 | 17 51,90 | 17 51,99 | + 1·35 | − 9 5 56·00 | − 9 6 1·53 | +5·53 | 3 | 1824 | |
| 18 | 8 | 22 11,70 | 22 11,89 | + 2·81 | + 5 2 4·70 | + 5 2 0·91 | −3·80 | 2 | 1822 | |
| 19 | 8 | 23 29,87 | 23 30,17 | + 4·54 | + 7 11 4·50 | + 7 10 56·33 | −8·18 | 2 | 1822—3 | |
| 20 | 8 | 25 37,72 | 25 37,54 | − 2·71 | − 8 43 56·00 | − 8 43 50·56 | +4·56 | 2 | 1824 | |
| 21 | 8 | 26 43,96 | 26 44,15 | + 2·78 | − 3 0 43·70 | − 3 0 50·92 | +7·22 | 2 | 1822 | |
| 22 | 7·8 | 27 24,07 | 27 24,39 | + 4·88 | − 4 40 27·50 | − 4 40 19·28 | −8·22 | 2 | 1822—4 | |
| 23 | 8·9 | 28 29,94 | 28 30,42 | + 7·23 | − 4 54 46·50 | − 4 54 52·48 | +5·98 | 2 | 1822—4 | |
| 24 | 9 | 30 27,54 | 30 27,39 | − 2·25 | − 5 35 29·20 | − 5 35 32·11 | +2·91 | 2 | 1823—4 | |
| 25 | 7·8 | 37 18,80 | 37 19,06 | + 3·83 | + 2 53 2·05 | + 2 52 55·77 | −6·74 | 2 | 1822 | Bessel's obs. quite close. |
| 26 | 8 | 41 30,13 | 41 30,11 | − 0·35 | −14 33 23·40 | −14 33 26·45 | +3·05 | 2 | 1824 | |
| 27 | 7·8 | 43 9,30 | 43 9,44 | + 2·05 | − 8 15 31·00 | − 8 15 35·93 | +4·93 | 2 | 1824 | |
| 28 | 4·5 | 50 18,20 | 50 18,34 | + 2·13 | − 7 42 55·80 | − 7 43 2·41 | +6·61 | 2 | 1824 | |
| 29 | 8 | 52 58,50 | 52 59,09 | + 8·84 | − 6 46 37·70 | − 6 46 43·25 | +5·55 | 2 | 1823—4 | |
| 30 | 9 | 53 40,74 | 53 41,22 | + 7·23 | −14 12 20·20 | −14 12 25·61 | +5·41 | 2 | 1824 | Bessel's obs. quite close. |
| 31 | 8 | 58 2,07 | 58 2,32 | + 3·75 | −13 13 29·50 | −13 13 32·33 | +2·83 | 3 | 1824 | |

"In many of the above cases it is probable that the difference between Piazzi's catalogue for 1800, and one reduced to that epoch from Bessel's observations, may arise from error of observation : in others the almost identity of the observations seems to prove at once the skill of the observer, and a proper motion in the star ; while again there is one (No. 2.) which possibly may be regarded as conclusive on the latter point. I would also suggest two instances of what appear errors of the press, or perhaps in reading off the instrument, in M. Bessel's zones.

Z. 154. Æ. $14^h\ 33^m\ 33^s{\cdot}7$. Dec. $12°\ \overline{41}'\ 22''{\cdot}4+$ Unless 26′ be read for 41′ this star of the sixth magnitude, 32 Bootis, will differ from Piazzi's catalogue $+14'\ 56''{\cdot}88$.

Z. 162. Æ. $14^h\ 37^m\ 10^s{\cdot}3$. Dec. $\overline{9}°\ 55'\ 36''{\cdot}8+$ Unless 8° be read for 9° this star of the 7·8 magnitude will differ from Piazzi $1°\ 0'\ 0''{\cdot}39$."

April 11.—A paper was read "On the construction of large Achromatic Telescopes." By A. Rogers, Esq.—In this paper the author describes a new construction of an Achromatic Telescope, the object of which is to render a small disc of flint glass available to perform the office of compensation to a much larger one of crown, and thus to render possible the construction of telescopes of much larger aperture than are now common, without hindrance from the difficulty at present experienced in procuring large discs of flint glass. It is well known that in the ordinary construction of an achromatic object-glass, in which a single crown lens is compensated by a single one of flint, the two lenses admit of being separated only by an interval too small to afford any material advantage in diminishing the diameter of the flint lens, by placing it in a narrower part of the cone of rays, the actual amount of their difference in point of dispersive power being such as to render the correction of the chromatic aberration impossible, when their mutual distance exceeds a certain limit.

This inconvenience Mr. Rogers proposes to obviate, and obtain the advantage in question, by employing as a correcting lens, not a single lens of flint, but a compound one consisting of a convex crown, and concave flint, whose foci are such as to cause their combination to act as a plane glass on the mean refrangible rays. Then it is evident that by reason of the greater dispersive power of flint than of crown glass, this will act as a concave on the violet, and as a convex on the red rays ; and *that,* the more powerfully, according as the lenses separately have greater powers or curvatures. If then, such a compound lens be interposed between the object-glass of a telescope, supposed to be a single lens of plate or crown glass, and its focus, it will cause no alteration in the focus for mean rays, while it will lengthen the focus for violet, and shorten it for red rays. Now this is precisely what is wanted to produce an achromatic union of all the rays in the focus ; and, as nothing in this construction limits the powers of the individuals composing the correcting lenses, they may therefore be applied any where, that convenience may dictate ; and thus, theoretically speaking, a disc of flint glass, however small, may be made to correct the colour of one of crown, however large.

But

But this construction possesses other and very remarkable advantages. For, first, when the correcting lens is approximately constructed on a calculation founded on its intended aperture, and on the refractive and dispersive indices of its materials, the final and complete destruction of colour may be effected not by altering the lenses by grinding them anew, but by sliding the combination nearer to, or further from, the object-glass, as occasion may require, along the tube of the telescope by a screw motion, till the condition of achromaticity is satisfied in the best manner possible. And, secondly, the spherical aberration may in like manner be finally corrected by slightly separating the lenses of the correcting glass, whose surfaces should for this purpose be figured to curvatures previously determined by calculation, to admit of this mode of correction—a condition which the author finds to be always possible.

Mr. Rogers explains his construction by reference to a diagram, and states the rule for the determination of the foci of the lenses of the correcting glass in a formula which may be thus interpreted. "The focal length of either lens of the correcting lens is to that of the object-glass, in a ratio compounded of the ratio of the square of the aperture of the correcting lens to that of the object-glass, and of the ratio of the difference of the dispersive indices of crown and flint glass, to the dispersive index of crown:"—for example, to correct the colour of a lens of crown or plate glass of nine inches aperture, and fourteen feet focal length (the dimensions of the celebrated telescope of Fraunhofer at Dorpat) by a disc of flint glass three inches in diameter, the focus of either lens of the correcting lens will require to be about nine inches. To correct it by a four inch disc will require a focus of about sixteen inches for each.

The author then remarks, that it is not indispensable to make the correcting glass act as a plane lens. It is sufficient if it be so adjusted as to have a shorter focus for red rays than for violet. If, preserving this condition, it be made to act as a concave lens, the advantage procured by Mr. Barlow's construction of reducing the length of the telescope with the same focal power, is secured: and he considers, moreover, that by a proper adaptation of the distances, foci, &c. of the lenses, we might hope to combine with all these advantages, that of the destruction of the secondary spectrum, and thus obtain a perfect telescope.

There was also read a portion of a paper "On the Occultation of δ Piscium observed in Blackman-street, in the month of February 1821. References to recorded observations of occultations in which peculiarities have been apparently seen, either at the moon's limb, or upon her disc; together with an enquiry how far certain hypotheses seem adequate to account for the phenomena. By James South, Esq."

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## PROCEEDINGS AT THE FRIDAY-EVENING MEETINGS OF THE ROYAL INSTITUTION OF GREAT BRITAIN.

April 25.—The lecture-room subject was practical sculpture, and was delivered by Mr. Sievier. It consisted of an account of the various processes adopted by the sculptor from the commencement of of modelling in clay, to the last touch required to complete his subject in marble; and the various operations of modelling, casting, chiseling, and the use and theory of the different instruments used, were explained and illustrated by examples. For these purposes clay-models, plaster-casts, and blocks of marble in different progressive states, were placed upon the table, upon which the workmen carried on their several operations.

May 9.—Mr. Faraday gave a lecture on the production of musical sound, with an explication of the principles of action in some new musical instruments. He stated his information to be derived altogether from Mr. Wheatstone, to whom, of course, the new facts brought forward belonged. The nature of the pitch of sounds was first explained, and stated to be the quality by which musical sounds are distinguished from mere noise. The credit of the first philosophical account of this quality was attributed to Gallileo, who proved that it depended altogether upon the number of impulses in a given time. An experiment by that philosopher, apparently forgotten in modern times, but described in his Dialogues, was repeated:—it consisted in drawing the point of a blade over a metal plate so as to produce sound, and counting or comparing the dots which are always produced upon the plates, and which are the records of the number of vibrations necessary to the pitch of sound produced.

Hook's experiment, in which the teeth of a revolving wheel were made to beat against a card, was repeated, and the pitch of sound shown to correspond with the number of impulses in a given time. An extension of this experiment by Mr. Wheatstone was also described, in which the harmonics of a stretched string were produced by holding it against the wheel; the string producing sound whenever the velocity of the wheel was such that the impulses of the teeth corresponded in frequency with the vibrations of the string or of any of its aliquot parts.

Robison's mode of producing musical sounds by air passing through a stop-cock rapidly revolving, was then explained and illustrated; afterwards, Cagniard de la Tour's syren was set in action; and from these were drawn the explanation of the principles of two new musical instruments from Germany, the Munt-harmonica and the Acol-harmonica.

May 2 and 16.—On these evenings Mr. Knowles, of the Navy Board, gave an account of the rise, progress, and present state of naval architecture. To make his historical observations more clear, he first stated the elements concerned in the structure of ships, as stability, floatation, masts and yard, &c. &c., and then traced the state of the navy from the reign of Alfred to the present day; pointing out the times and progression of its increase; the period when any improvement had taken place; and more minutely entering into the important alterations introduced of late years by Sir W. Rich, Sir Robert Seppings, and others. All the observations relating to construction were fully illustrated by numerous models from the magnificent collection belonging to the Navy Board.

## LXVIII. *Intelligence and Miscellaneous Articles.*

### ORIGIN OF TRAP ROCKS.

[The following is an abstract of a Paper on this subject by Mr. S. Solly, read before the Royal Society on the 6th of March last.]

WHILE Mr. Solly was examining the hills of porphyry near Christiania, he discovered a low, detached, quadrangular rock of trap of the species called *diorite*, which exhibited in a manner that had never before been noticed, except in the experiments of Gregory Watt, the gradual development of crystallization commencing with globules of green pyroxene and white felspar, sometimes enveloping each other, and varying in size from that of a small pea to extreme minuteness; they were thickly strewed on a perpendicular side of the rock near the ground, and at a distance had the appearance of cryptogamia.

On a close examination he found some of the globules stretched out, and in other parts close to the former; the felspar had shot into parallel lines, which here and there had united to form rhombs. He likewise found the pyroxene standing out upon the surface in regular crystals, exhibiting a curious variety of modification; some of them crossing each other like the double crystals of harmotome.

To his communication of this fact, Mr. Solly has prefixed some remarks on the probable history of rocks of trap, derived from a comparison of the different situations in which he has met with them. From observing their partial resemblance to lava, and that they are most frequently met with in coal-fields, Mr. Solly infers that they owe their origin to internal combustion; and as little doubt seems to remain that our beds of coal were formerly bogs, which have been compressed and protected by a covering of clay and sand, he conjectured that other masses of bog and combustible strata lying more exposed have been converted into trap. The position of the Swedish rocks, (from whose quadrangular form this name is derived,) strengthened this conjecture, which was confirmed by his meeting with a stone perfectly similar in all its characters, recently formed by the spontaneous combustion of substances which perfectly correspond with some of the strata* found beneath the Swedish rocks.

Mr. Solly introduced the subject with observations on the assistance geology ought to derive from the discoveries in experimental science. Various observations on currents are interspersed through this paper, to explain the difference of stratification in different situations. From the insulated character of the formations in countries intersected by granite, Mr. S. infers that the latter cannot be a rock of posterior formation as the Scotch Plutonists imagine.

An explanation is given of some instances of high temperature in the lower part of deep mines, from which erroneous conclusions have been drawn. Mr. S. touches on the subject of electrical as

* They are sufficiently impregnated with bitumen and sulphur to be used as fuel or roasted for alum. See *Thomson's Travels*.

well

well as chemical action beneath the surface of the earth, and is of opinion that the gaseous products of beds of coal have assisted in converting their covering of loose sand and clay into coal sandstone and clay ironstone.

As the subject of internal changes is of practical importance to the miner, Mr. S. recommends its investigation. The garnets and zeolites found in shale by Mr. Henslow, are decided instances of internal changes, and coincide with some of the facts previously noticed by Mr. S.

His distinction between lava and trap, which we more generally call Basalt, and in Scotland is chiefly known by the name of Whin, is confirmed by the authority of Sir James Hall: "Calcareous spar frequently occurs in whinstone, either in veins, or in detached nodules, but is never found in lava, and could not exist in a volcanic stream. It is generally supposed that some lavas of Ætna contain calcareous spar and zeolites; but this I conceive to be a mistake. It is true I have seen that many rocks of Ætna contain these substances in abundance, but in my opinion these rocks are not lavas, but have flowed subterraneously like our whins, and are the same with them in every respect. A particular district of Ætna, comprehending the Cyclopian Isles, and the country round La Trizza, and the Castle of Jaci, is decidedly of this description; and vestiges of this kind occur in other parts of the mountain. In one place fossil coal has been found, and in another we saw marine shells. In the neighbourhood of Bronte, we observed a high ridge formed of strata of sandstone and limestone, partly overflowed and concealed by recent lava, but so placed as to render it evident that its construction formed no inconsiderable part of the mountain."

Mr. S. has heard disciples of Werner mention, that from its contiguity to basalt, he supposed Ætna was erected on the site of a coal mine: the additional coincidences in Sir James Hall's description seem to prove that coal is a part of the fuel of Ætna. Breislack, who has spent many years in examining Vesuvius, is of opinion that it is fed by the bituminous strata of the Appenines.

The principal objection to this opinion has been removed by Sir Humphry Davy, whose genius has been scarcely more conspicuous in anticipating the discoveries made by the galvanic battery, than in drawing those inferences from them which have assisted his contemporaries in bringing other discoveries to light. Thus our knowledge of the properties of iodine is a natural consequence of his observations on the nature of chlorine: these and every other agent of combustion ought to be enumerated in discoursing on the infinite variety of chemical and electro-chemical operations which may take place in the vast laboratory of Vesuvius. Since with the assistance of the electrical energies of iron we can make potash yield up its oxygen, is it not possible that the earths may be reduced to a metallic state by a similar process in the bowels of the mountain? These considerations show there is no necessity for any contrivance or reservoir to keep up the supply of elastic oxygen. By placing at the command of the philosopher powers which are always close at hand, and are capable of producing tremendous

 results,

results,—by teaching him to conjure up inflammatory spirits from the surface of the "vasty deep," and to turn water into fire,—Sir H. Davy has shown it is not necessary that Pluto should ascend from the lowest depths of his empire to produce the phænomena of volcanos, and much less so to bring about a chemical union between silex, alumina, lime, and iron, the chief constituents of basalt or trap. *Nec Deus intersit dignus nisi vindice nodus.*

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## ANALYSES OF TOURMALINES.

M. Gmelin has analysed a great many varieties of this mineral; the method adopted is the following:—The mineral reduced to a fine powder is mixed with carbonate of barytes, and strongly heated. The mass is afterwards treated with a sufficient quantity of muriatic acid to dissolve it entirely, and the solution is evaporated upon a sand bath to dryness. M. Gmelin ascertained by direct experiment that at this temperature the quantity of boracic acid volatilized is so minute that it may be neglected without any sensible error. The silica is obtained in the usual manner, by treating the residuum of evaporation with water. Carbonate of ammonia is added to the solution, and after filtration, and evaporation to dryness, the residuum is gradually heated to low redness. In this manner, no boracic acid can be lost, because it is combined with ammonia; and thus during calcination at a red heat, no acid or aqueous vapour is evolved, as in the decomposition of sulphate of ammonia. The residuum after being weighed, is washed with alcohol mixed with a little muriatic acid; the alcohol being separated is burnt; the operation is repeated until the alcohol does not give a green flame. All the boracic which is combined with the ammonia is thus obtained. The residuum again heated and dried, and the loss of weight determines the quantity of boracic acid.

Tourmalines are divided by M. Gmelin into three classes: the first of which contains lithia:—I. Red Tourmaline from Rôsna in Moravia; sp. gr. 2·96 to 3·02. II. Red Tourmaline from Perm in Siberia; sp. gr. 3·059. III. Celadon Green Tourmaline from Brazil; sp. gr. 3·079.

| | I. | II. | III. |
|---|---|---|---|
| Boracic acid | 5·74 | 4·18 | 4·59 |
| Silica | 42·13 | 39·37 | 39·16 |
| Alumina | 36·43 | 44·00 | 40·00 |
| Oxidulous oxide of iron | .. | .. | 5·96 |
| ——— manganese | 6·32 | 5·02 | 2·14 |
| Lime | 1·20 | | |
| Potash | 2·41 | 1·29 | |
| Lithia | 2·04 | 2·52 | 3·59 with potash. |
| Volatile matter | 1·31 | 1·58 | 1·58 |
| | 97·58 | 97·96 | 97·02 |

Tourmalines

*Tourmalines which contain potash or soda, or both together, without lithia, and without a notable quantity of magnesia.* The following are the varieties :—I. Black Tourmaline from Bovey in Devonshire, found with quartz and phosphate of lime; sp. gr. 3·246. II. Black Tourmaline from Eibenstock in Saxony; sp. gr. 3·123. III. Green Tourmaline from Chesterfield, North America, sp. gr. 3·102.

| | I. | II. | III. |
|---|---|---|---|
| Boracic acid ........ | 4·11 | 1·89 | 3·88 |
| Silica .............. | 35·20 | 33·05 | 38·80 |
| Alumina ............ | 35·50 | 38·23 | 39·61 |
| Oxidulous oxide of iron | 17·86 | .. | 7·43 |
| Protoxide of iron .... | .. | 23·86 | |
| ———— manganese | 0·43 with magnesia | .. | 2·88 † |
| Magnesia .......... | 0·70 with manganese | | |
| Lime .............. | 0·55 | 0·86 | |
| Soda .............. | 2·09 | 3·17 * | 4·95 |
| Loss in the fire ... .. | .. | 0·45 | 0·78 |
| | 96·44 | 101·51 | 98·33 |

*Tourmalines which contain a considerable quantity of magnesia.*—Four specimens were analysed :—I. Black Tourmaline from Käringbricka, a province of Westmanland in Sweden ; sp. gr. 3·044. II. Black Tourmaline from Rabenstein in Bavaria; sp. gr. 3·113. III. Black Tourmaline from Greenland; sp. gr. 3·062. IV. Deep Brown Tourmaline, from the mica slate of St.-Gothard.

| | I. | II. | III. | IV. |
|---|---|---|---|---|
| Boracic acid ........ | 3·83 | 4·02 | 3·63 | 4·18 |
| Silica .............. | 37·65 | 35·48 | 38·79 | 37·81 |
| Alumina ............ | 33·46 | 34·75 | 37·19 | 31·61 |
| Magnesia.......... | 10·98 | 4·68 | 5·86 | 5·99 |
| Oxidulous oxide of iron | 9·38 | 17·44 | 5·81 | 7·77 |
| Oxide of manganese .. | .... | 1·89 | trace. | 1·11 |
| Potash .......... } Soda .......... } | 2·53 | 0·48 | 0·22 | 1·20 |
| | | 1·75 | 3·13 | |
| Lime .............. | 0·25 | trace. | .... | 0·98 |
| Loss in the fire ...... | 0·03 | .... | 1·86 | 0·24 |
| | 98·11 | 100·49 | 96·48 | 90·89 |

M. Gmelin is at a loss to what cause to attribute the deficiency in the last analysis. He thinks the tourmaline from St.-Gothard should be again examined; especially as the loss in Bucholz's analysis is still greater.—*Ann. de Chimie et de Phys. Nov.* 1827.

* With potash and a trace of magnesia. † With a trace of magnesia.

A NEW

## A NEW METHOD OF SEPARATING MANGANESE FROM LIME AND MAGNESIA.—BY PROFESSOR STROMEYER.

We are informed in a letter from Professor Stromeyer, that he has found the following method successful in procuring the complete separation of manganese from magnesia and lime. To an acid liquid containing the peroxide of iron together with manganese, lime, and magnesia, the carbonate of soda is added in the usual manner, so as to precipitate the first, while the three latter oxides are held in solution by an excess of carbonic acid; and in order to prevent any manganese from falling, the actual precipitation of the iron is effected by the bicarbonate instead of the carbonate of soda. After acidulating the filtered solution and concentrating it by evaporation, a current of chlorine gas is transmitted through it. On neutralizing the free acid by the gradual addition of bicarbonate of soda, the manganese subsides in the form of the red oxide, being thus completely separated from the magnesia and lime.—*Brewster's Journal, April* 1828.

## SINGULAR ACTION OF PHOSPHORIC ACID ON ALBUMEN.

In his essays on the animal fluids Berzelius stated, that a solution of albumen is not precipitated by phosphoric acid; whereas Engelhart, in his interesting researches on the colouring matter of the blood, found that albumen is coagulated even in a dilute solution by phosphoric acid. As Engelhart was at Stockholm last winter, he and Berzelius inquired into the cause of difference in their statements, and discovered that they were both right. A solution of phosphoric acid, which had been kept some time in the laboratory, did not precipitate a solution of albumen; but phosphoric acid, recently prepared either by the action of nitric acid on phosphorus or by direct combustion, caused an abundant precipitate. On further examination, it was found that phosphoric acid, recently ignited, always throws down albumen; but that after being kept in solution for a few days, it loses that property. The coagulating power is restored by heating the acid to redness, but disappears again after the interval of a day.

The cause of these phænomena is by no means apparent. It does not depend on a higher degree of oxidation, for the change ensues in close vessels equally as by exposure to the air. Perhaps, says Berzelius, there may be some peculiar compound of water and the acid, which is not formed at the moment of solution, and which has not the property of precipitating albumen.—*Annales de Chimie et de Physique,* xxxv. 110.

*Remark on the preceding notice, by Dr. Turner.*—On comparing the facts observed by Berzelius and Dr. Engelhart, with the formation of the pyrophosphate of soda, described by Mr. Clark in the fourteenth Number of this Journal, it appeared probable that phosphoric acid heated to redness may be converted into pyrophosphoric acid, or undergo that change which enables it to form a white salt with silver. To put this supposition to the test of experiment, some fragments of phosphorus were treated in a platinum crucible by by nitric acid, and the product heated to redness. The solution of the resulting pure phosphoric acid precipitated a dilute solution of albumen; but when carefully neutralized by carbonate of soda, and then mixed with a solution of the nitrate of silver, the common yellow phosphate subsided. Consequently, it was not in the state of pyrophosphoric acid.—*Brewster's Journal, April* 1828.

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### LIST OF EARTHQUAKES WHICH OCCURRED IN 1827.

Jan. 2.—At Mortagne (Orne) and the environs. A violent shock of short duration, accompanied with an intense noise. Chimneys and household furniture were thrown down. The commotion reached as far as Alençon. The day was cloudy, the weather thick and stormy, which is not usual at that time of the year.

Feb. 9.—At seven o'clock in the evening; in the north-west part of Wales and the Isle of Anglesea. The shocks continued from forty seconds to a minute; they were sufficiently violent to overturn several pieces of furniture. A noise was heard like that of a heavy laden cart going on the stones.

April 2.—At Bevers, at twenty minutes past one in the morning; two strong consecutive shocks. The inhabitants of Basse-Engadine assert that they counted twenty similar shocks during the winter.

May 29.—At Vajaca, in Mexico; two slight shocks.

June 3.—At Martinique; a slight shock.

June 12.—At Tehenacan, in Mexico, at half-past one o'clock; a violent shock, with a frightful noise. Many buildings damaged.

June 16.—At Aquila, in the kingdom of Naples; a shock at five o'clock in the morning.

June 21.—At Palermo, at eleven o'clock in the morning. Four strong shocks in the space of seven seconds; it was an oscillatory motion from the west to the east.

Aug. 14.—At Palermo, at 2 p.m. Several shocks; they continued about eighteen minutes, with very short intervals; the motion was always oscillatory.

Sept. 18.—At Lisbon. A slight shock.

Oct. 10.—At Zurich, and all the shores of the lake. At twelve minutes before 3 p.m., a strong shock.

Oct. 15.—At Jassy, at eight in the evening. Two violent shocks, directed from north to south, and accompanied by a subterraneous noise; two or three days after, the heat was very great.

Oct 30.—At Corsica, in the cantons of Taravo, Taliano, and Sartène. Two shocks at twenty minutes past 5, a.m.

Nov. 30.—At Pointe-à-Petre, Guadaloupe, at three in the morning. Violent earthquake. At Mariegalante it was preceded by a strong and sudden storm.—*Ann. de Chimie et de Phys. Nov.* 1827.

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### RED RAIN SUPPOSED TO ARISE FROM BUTTERFLIES.

The following narrative seems curious and important in connection with the various accounts of red rain. It is extracted from *Gassendi's Life of Peiresc*, p. 110-113. "Through the whole of this year (1608) nothing

thing gave M. Peiresc greater pleasure than his observations upon the *bloody rain*, said to have fallen about the beginning of July. Large drops were seen both in Paris itself upon the walls of the cemetery of the greater church, which is near the walls of the city, upon the walls of the city, and likewise upon the walls of villas, hamlets, and towns for some miles round the city. In the first place, M. Peiresc went to examine the drops themselves, with which the stones were reddened, and spared no pains to obtain the means of conversing with some husbandmen beyond Lambesc, who were reported to have been so astonished at the shower, as to leave their labour and fly for safety into the neighbouring houses. This story he ascertained to be without foundation. To the explanation offered by the philosophers, who said that the rain might have come from vapours, which had been raised out of *red* earth, he objected that evaporated fluids do not retain their former hues, as is plainly exemplified in the colourless water distilled from red roses. Nor was he better satisfied with the opinion of the vulgar, countenanced by some of the theologians, who maintained that the appearance was produced by demons, or witches, shedding the blood of innocent babes. This he thought was a mere conjecture, scarcely reconcileable with the goodness and providence of God. In the meantime an accident happened, which discovered to him, as he thought, the true cause of the phænomenon. He had found some months before a chrysalis of a remarkable size and form, which he enclosed in a box. He thought no more of it, until, hearing a buzz within the box, he opened it, and perceived that the chrysalis had been changed into a most beautiful butterfly, which immediately flew away, leaving at the bottom of the box a red drop of the size of a shilling. As this happened about the time when the shower was supposed to have fallen, and when a vast multitude of those insects was observed fluttering through the air in every direction, he concluded that the drops in question were some kind of excrementitious matter emitted by them, when they alighted upon the walls. He therefore examined the drops again, and remarked, that they were not upon the upper surfaces of stones and buildings, as they would have been, if a shower of blood had fallen from the sky, but rather in cavities and holes, where insects might nestle. Besides this, he took notice that they were to be seen upon the walls of those houses only, which were near the fields, and not upon the more elevated parts of them, but only up to the same moderate height at which the butterflies were accustomed to flutter. In this way he explained the story, told by Gregory of Tours, of a bloody shower seen at Paris in the time of Childebert, at different places, and upon a house in the vicinity of Senlis; and another said to have fallen in the time of King Robert, about the end of June, the drops of which could not be washed out by means of water, when they had fallen upon flesh, garments, or stones, but might be washed out from wood; for the time here stated was the season for the butterflies; and he showed that no water could wash out these red marks from stones. After discussing these and similar arguments in the presence of much company at the house of his friend Varius, they determined to inspect the appearance together,

and

and, as they wandered through the fields, they saw many drops upon stones and rocks, but only in hollows or upon sloping surfaces, and not upon those which were presented to the sky." The butterfly observed by Peiresc was probably the *Papilio C. album*, or common butterfly. It has been observed to deposit the same red fluid in England.

WEATHER IN PARIS.

The following was the state of the weather during the last year in Paris.

| | |
|---|---|
| Rain............... | 146 days. |
| Snow ............ | 21 |
| Hail or hoar frost.... | 6 |
| Frost ............ | 59 |
| Thunder .......... | 21 |
| Very cloudy........ | 178 |

*Ann. de Chimie et de Phys. Dec.* 1827.

ANALYSIS OF ALCOHOL, ÆTHER, &C. BY MM. DUMAS AND BOULLAY.

In the Philosophical Magazine and Journal for April, I gave some account of the paper by the above-named chemists, and proposed to offer some remarks upon it in the present Number: the Royal Institution Journal for April contains some observations so much to the point which I had intended to notice, that I shall content myself with copying them.—R. P.

"MM. Dumas and Boullay then consider the formation and nature of the sulphuric acid, stating the opinions of MM. Vogel and Gay Lussac, that it is a compound of hyposulphuric acid with a vegetable matter; and also that of Mr. Hennel, that it is an acid in which half the saturating power of the sulphuric acid present is neutralized by the hydrocarbon in combination. With the latter opinion they also class that entertained by Mr. Faraday relative to the nature of sulphonaphthalic acid. MM. Dumas and Boullay consider the question, and decide in favour of the former opinion; after which they say they have observed facts which are better explained by the latter. We think it a pity that these philosophers did not refer to Mr. Hennel's paper in the Philosophical Transactions for 1826, p. 240, where they would have gained the knowledge of a compound produced by the action of sulphuric acid and alcohol, of which they seem at present altogether ignorant, and which would have probably caused serious alterations in their views, at least with regard to the sulphovinic acid. We refer to what is known in London, and sold at Apothecaries' Hall, by the name of *oil of wine;* not the hydrocarbon referred to by MM. Dumas and Boullay, but a *neutral* compound of sulphuric acid with hydrocarbon, containing, with the same proportion, of sulphuric acid, twice as much hydrocarbon as the sulphovinic acid. It is of this compound that Mr. Hennel speaks in the following passage, which we cannot refrain from quoting: 'Mr. Vogel, who has particularly described some of these salts (sulphovinates), and I believe also M. Gay Lussac, have supposed that this loss of saturating power arises from the formation of hyposulphuric acid, and that the hyposulphates and sulphovinates only differ in the latter containing some æthereal oil, which in some way acts the part of water of crystallization. It is evident that the properties of oil of wine *cannot* be thus explained; and it appears to me more probable that the power of combination *which hydrocarbon is shown to be possessed of* in oil of wine is effective in neutralizing half the acid of the salts formed from it (sulphovinates) as before described.'"

## TEST OF THE PRESENCE OF AMMONIA.

M. Plessin having occasion to ascertain whether the action of a salifiable base upon a body containing azote was simply that of evolving ammonia previously existing, or that of forming ammonia by combination, endeavoured to find a base, which would effect the former object but not the latter. Potash, lime, magnesia, and many other bodies, do both; but the hydrated oxide of lead answered the purpose very well. It gives no indication of ammonia when put into contact with azotated substances, not containing that alkali; even urea is not effected by it; but being put in contact with an ammoniacal salt, ammonia was instantly evolved, and rendered evident by the visible fumes which arose upon the approximation of a little acetic acid.—*Annales de Chimie,* xxxvi. p. 377.

## LIST OF NEW PATENTS.

To Jane B. Lowrey, wife of T. S. Lowrey, of Exeter, for her improvements in the manufacture of hats and bonnets.—Dated the 25th of March 1828.—6 months allowed to enrol specification.

To E. Cowper, of Clapham-road Place, Lambeth, for improvements in cutting paper.—26th of March.—6 months.

To F. de Fourville, of Piccadilly, for improvements on filtering apparatus.—26th of March.—6 months.

To T. Lawes, of the Strand, for an improved thread to be used in the manufacture of bobbin net lace.—29th of March.—6 months.

To H. Marriott, of Fleet-street, and Augustus Siebe, of Princes-street, Leicester-square, for certain improvements in hydraulic machines.—29th of March.—6 months.

To Peter Taylor, of Hollinwood, Lancaster, for certain improvements in machinery for hackling, dressing or combing flax, hemp, &c.—29th of March.—6 months.

To John Davis, of Leman-street, Goodman's-fields, for an improvement (communicated from abroad) in boiling or evaporating solutions of sugar and other liquids.—29th of March.—6 months.

To C. Harsleben, of New Ormond-street, Esq., for improvements in machinery to be used in navigation and the propelling of ships.—3rd of April.—6 months.

To S. W. Wright, of Webber-street, Lambeth, engineer, for improvements in the construction of wheel carriages.—15th of April.—6 months.

To J. G. Ulrich, of Cornhill, for his improvements on chronometers. 19th of April.—6 months.

SCIENTIFIC

## SCIENTIFIC BOOKS.

### *Just Published.*

On the Methods of determining Terrestrial Longitudes by the Moon's Right Ascension, as deduced from her Altitudes and Culminations. By John Crisp, Captain in the Madras Army. In quarto, Price 6s.

It is the intention of the Medico-Botanical Society of London, to print their Transactions in the shape of an octavo Quarterly Journal, in conjunction with those of the Academy of Minute Anatomy at the London Ophthalmic Infirmary. The first Number of this Journal will appear shortly illustrated with coloured Engravings in quarto.

The Astronomical Doctrine of a Plurality of Worlds irreconcilable with the Popular Systems of Theology, but in perfect harmony with the True Christian Religion. By the Rev. S. Noble.

A Series of Treatises on the principal Branches of Manufacturing Chemistry, by Mr. Astley of Edinburgh, is about to be published. The manufacture of Common Salt will form the subject of the first, which will shortly be published separately, comprising full details of its history, physical, chemical, commercial, and œconomical; with suggestions for the material improvement of the manufacture, and a full digest of the results of the experiments in the use of salt by agriculturists, since the repeal of the duty.

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## METEOROLOGICAL OBSERVATIONS FOR APRIL 1828.

### *Gosport.—Numerical Results for the Month.*

Barom. Max. 30·28 April 30. Wind NE.—Min. 29·10 April 8. Wind S.E.
Range of the index 1·18.
Mean barometrical pressure for the month .......................... 29·736
Spaces described by the rising and falling of the mercury............ 5·550
Greatest variation in 24 hours 0·380.—Number of changes 18.
Therm. Max. 69° April 29. Wind S.E.—Min. 35° April 3. Wind N.
Range 34°.—Mean temp. of exter. air 51°·33. For 30 days with ☉ in ♈ 48·22
Max. var. in 24 hours 20°·00—Mean temp. of spring water at 8 A.M. 50°·89

### De Luc's *Whalebone Hygrometer.*

Greatest humidity of the air in the evening of the 24th.............. 88°
Greatest dryness of the air in the afternoon of the 28th ............ 42
Range of the index................................................ 46
Mean at 2 P.M. 60°·0—Mean at 8 A.M. 65°·0—Mean at 8 P.M. 74·4
—— of three observations each day at 8, 2, and 8 o'clock......... 66·5
Evaporation for the month 2·20 inches.
Rain near ground 2·725 inch.

### *Summary of the Weather.*

A clear sky, 2; fine, with various modifications of clouds, 11½; an overcast sky without rain, 9½; rain, 7.—Total 30 days.

### *Clouds.*

| Cirrus. | Cirrocumulus. | Cirrostratus. | Stratus. | Cumulus. | Cumulostr. | Nimbus. |
|---|---|---|---|---|---|---|
| 20 | 12 | 26 | 0 | 15 | 25 | 22 |

 *Scale*

*Scale of the prevailing Winds.*

| N. | N.E. | E. | S.E. | S. | S.W. | W. | N.W. | Days. |
|---|---|---|---|---|---|---|---|---|
| 1 | 5 | 0 | 5½ | 1½ | 10 | 3½ | 3½ | 30 |

*General Observations.*—The first five days of this month were dry and cold; afterwards it rained more or less every day till the 26th: but the last four days were dry and warm, in which more progress appeared to have been made in the blooming and growth of fruit and vegetation, than during the preceding fortnight.

The North-east and North-west winds on several days were blighty, the effects of which may be traced in the formation of the young wall-fruit, &c. The 4th and 22nd were very cold days, whose maximum temperature in the shade was only 46 and 47 degrees, which is three or four degrees colder than some of the nights. The first swallow appeared here in the morning of the 21st, and the nightingale was first heard on the 11th instant.

At noon of the 27th three winds prevailed in different directions: the first next the earth, as pointed out by a vane, was from South-east; the next, which was ascertained by keeping the eye in a line with a fixed object whose bearing on the horizon was known, and observing the motion of the passing *cumulus* clouds directly from the same point, was from South-west by South; and the upper one, ascertained in the same way by observing the direction of plumose *cirri*, was from North North-west.

The planet Venus, which has now the appearance of a half-moon, was conspicuous with the naked eye at the same time; and its apparent distance from the Sun's centre, as measured by a sextant, was 44° 8′, which is only a few minutes of a degree less than it will be at its greatest elongation on the 19th of next May.

The mean temperature of the external air this month is 1½ degree higher than the mean of April for the last twelve years.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are four solar and two lunar halos, one rainbow, one meteor, and eight gales of wind, or days on which they have prevailed; namely, one from the South-east, two from the South, four from the South-west, and one from the North-west.

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REMARKS.

*London.*—April 1, 2. Fine. 3, 4. Clear and cold. 5. Cloudy, with rain at night. 6. Cloudy. 7. Rain in morning: fine. 8. Fine. 9. Drizzly: fine. 10. Drizzly: very fine. 11. Fine: rain at night. 12. Showery. 13. Stormy, but fair. 14. Very fine: rain at night. 15. Wet morning: showery. 16. Cloudy: with showers. 17, 18. Wet. 19, 20. Cloudy. 21. Cold and showery. 22. Showery. 23. Fine: stormy and wet at night. 24. Very fine. 25. Fine: stormy and wet at night. 26. Clear and fine. 27—30. Very fine.

*Boston.*—April 1, 2. Fine. 3—5. Cloudy. 6. Rain. 7, 8. Cloudy. 9. Fine. 10. Cloudy. 11. Fine. 12. Cloudy. 13, 14. Fine. 15. Rain. 16. Fine. 17—20. Cloudy. 21. Rain. 22, 23. Cloudy. 24. Fine. 25. Cloudy. 26—30. Fine.

*Penzance.*—April 1. Clear: fair: heavy rain at night. 2, 3. Fair. 4. Clear: fair. 5. Clear: a shower. 6, 7. Clear: showers. 8. Rain. 9. Fair: showers. 10. Hail-showers: fair. 11. Rain. 12. Rain: showers. 13. Showers. 14. Fair: clear. 15. Rain: clear. 16. Clear: showers. 17. Showers. 18. Rain: clear. 19. Clear: showers. 20, 21. Fair: showers. 22. Fair. 23. Clear: rain. 24. Misty: rain. 25. Rain. 26. Clear: fair. 27—30. Fair: clear.—Rain-gauge ground level.

*Meteoro-*

*Meteorological Observations made at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.*

| Days of Month, 1828. | Barometer. | | | | | | | Thermometer. | | | | | | | Wind. | | | | Evap. | Rain. | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | London. | | Penzance. | | Gosport. | | Boston | London. | | Penzance. | | Gosport. | | Bost. | Lond. | Penz. | Gosp. | Bost. | Gosp. | Lond. | Penz. | Gosp. | Bost. |
| | Max. | Min. | Max. | Min. | Max. | Min. | 8½ A.M. | Max. | Min. | Max. | Min. | Max. | Min. | 8½ A.M. | | | | | | | | | |
| April 1 | 30·305 | 30·146 | 30·20 | 30·00 | 30·24 | 29·94 | 29·87 | 54 | 40 | 52 | 45 | 52 | 38 | 44·5 | SE. | SE. | SE. | calm | ... | ... | ... | ... | ... |
| 2 | 30·168 | 30·080 | 30·06 | 29·90 | 29·98 | 29·96 | 29·70 | 50 | 33 | 50 | 43 | 56 | 42 | 45·5 | N. | SE. | NE. | E. | ... | ... | 1·760 | ... | ... |
| 3 | 30·144 | 30·072 | 30·06 | 30·02 | 30·06 | 29·99 | 29·67 | 47 | 30 | 50 | 40 | 55 | 35 | 39 | N. | NE. | NE. | N. | 0·25 | ... | ... | ... | ... |
| 4 | 30·005 | 29·991 | 30·00 | 29·90 | 29·92 | 29·92 | 29·60 | 46 | 29 | 52 | 38 | 46 | 38 | 39 | NW. | NW. | NE. | calm | ... | ... | ... | ... | ... |
| 5 | 29·864 | 29·711 | 29·90 | 29·86 | 29·85 | 29·71 | 29·36 | 47 | 36 | 51 | 44 | 51 | 43 | 44 | W. | NW. | W. | NW. | ... | ·24 | ... | ... | ... |
| 6 | 29·647 | 29·603 | 29·80 | 29·70 | 29·59 | 29·58 | 29·20 | 53 | 38 | 52 | 44 | 55 | 44 | 39·5 | N. | NW. | NW. | E. | ·25 | ·04 | 0·100 | 0·250 | 0·15 |
| ☾ 7 | 29·482 | 29·381 | 29·50 | 29·40 | 29·41 | 29·35 | 29·05 | 55 | 34 | 48 | 43 | 56 | 42 | 45 | E. | NW. | W. | calm | ... | ·15 | 0·300 | ·060 | ·05 |
| 8 | 29·386 | 29·221 | 29·15 | 29·00 | 29·29 | 29·10 | 29·03 | 55 | 41 | 48 | 38 | 56 | 44 | 43·5 | NE. | SW. | SE. | E. | ... | ... | 0·455 | ·020 | ... |
| 9 | 29·501 | 29·268 | 29·50 | 29·30 | 29·48 | 29·29 | 28·94 | 57 | 40 | 53 | 40 | 58 | 48 | 49 | SW. | NW. | SW. | E. | ·22 | ·04 | 0·205 | ·055 | ... |
| 10 | 29·620 | 29·450 | 29·66 | 29·56 | 29·52 | 29·40 | 29·00 | 61 | 38 | 52 | 47 | 56 | 46 | 47 | NW. | NW. | NW. | W. | ... | ·02 | 0·150 | ·055 | ... |
| 11 | 29·793 | 29·731 | 29·76 | 29·74 | 29·76 | 29·73 | 29·26 | 59 | 47 | 53 | 46 | 57 | 48 | 49 | SW. | SW. | W. | W. | ... | ·05 | 0·200 | ·140 | ... |
| 12 | 29·777 | 29·578 | 29·56 | 29·50 | 29·60 | 29·53 | 29·15 | 60 | 49 | 56 | 48 | 56 | 48 | 55 | SW. | SW. | SW. | calm | ·18 | ·09 | 0·225 | ·280 | ·22 |
| 13 | 29·651 | 29·528 | 29·60 | 29·40 | 29·62 | 29·49 | 28·95 | 58 | 41 | 56 | 45 | 58 | 48 | 53 | SW. | W. | SW. | W. | ... | ... | 0·250 | ·005 | ·10 |
| ● 14 | 29·869 | 29·760 | 29·80 | 29·64 | 29·82 | 29·74 | 29·22 | 62 | 42 | 57 | 46 | 59 | 49 | 51 | S. | SW. | SW. | W. | ... | ·14 | ... | ·060 | ·13 |
| 15 | 29·784 | 29·679 | 29·60 | 29·50 | 29·73 | 29·60 | 29·30 | 57 | 45 | 56 | 48 | 55 | 49 | 48·5 | S. | SW. | SW. | calm | ·23 | ·10 | 0·290 | ·200 | ·20 |
| 16 | 29·649 | 29·275 | 29·48 | 29·30 | 29·58 | 29·34 | 29·08 | 58 | 45 | 54 | 48 | 53 | 48 | 52·5 | SW. | SW. | S. | S. | ... | ·13 | 0·160 | ·100 | ·20 |
| 17 | 29·441 | 29·356 | 29·30 | 29·20 | 29·34 | 29·23 | 28·92 | 56 | 45 | 54 | 47 | 57 | 48 | 52 | S. | SW. | SE. | W. | ... | ·28 | 0·270 | ·430 | ·16 |
| 18 | 29·498 | 29·360 | 29·60 | 29·50 | 29·40 | 29·27 | 28·94 | 57 | 42 | 51 | 46 | 57 | 48 | 54 | S. | NE. | S. | calm | ·17 | ·49 | 0·560 | ·430 | ·21 |
| 19 | 29·738 | 29·454 | 29·72 | 29·70 | 29·62 | 29·44 | 29·15 | 58 | 45 | 52 | 45 | 57 | 47 | 51 | SE. | NE. | NW. | calm | ... | ... | 0·340 | ·050 | ·19 |
| 20 | 29·877 | 29·778 | 29·76 | 29·72 | 29·76 | 29·64 | 29·46 | 53 | 44 | 54 | 45 | 55 | 46 | 48·5 | NE. | NE. | NE. | E. | ... | ... | 0·700 | ·030 | ... |
| 21 | 29·752 | 29·677 | 29·80 | 29·80 | 29·68 | 29·62 | 29·30 | 49 | 40 | 48 | 45 | 56 | 43 | 46 | N. | NE. | N. | NE. | ·15 | ·07 | 0·230 | ·070 | ·14 |
| ☽ 22 | 29·845 | 29·700 | 29·86 | 29·82 | 29·67 | 29·60 | 29·20 | 51 | 42 | 50 | 43 | 47 | 45 | 43·5 | N. | NW. | W. | W. | ... | ·18 | ... | ·020 | ·14 |
| 23 | 29·883 | 29·780 | 29·90 | 29·86 | 29·82 | 29·74 | 29·26 | 52 | 45 | 54 | 45 | 57 | 49 | 46·5 | NW. | W. | SW. | W. | ... | ·20 | 0·600 | ·240 | ·07 |
| 24 | 29·898 | 29·777 | 29·80 | 29·80 | 29·89 | 29·78 | 29·15 | 60 | 46 | 55 | 46 | 59 | 50 | 53 | W. | W. | SW. | W. | ·10 | ... | 0·115 | ·080 | ·04 |
| 25 | 29·949 | 29·764 | 29·84 | 29·80 | 29·86 | 29·74 | 29·32 | 59 | 47 | 56 | 48 | 58 | 47 | 53 | W. | SW. | SW. | SW. | ... | ·22 | 0·300 | ·140 | ·01 |
| 26 | 30·162 | 29·957 | 30·00 | 29·90 | 30·10 | 29·97 | 29·43 | 63 | 36 | 56 | 46 | 59 | 45 | 49·5 | SW. | SW. | SW. | W. | ... | ... | 0·290 | ·010 | ·28 |
| 27 | 30·295 | 30·257 | 30·12 | 30·10 | 30·24 | 30·22 | 29·77 | 67 | 41 | 59 | 47 | 63 | 49 | 56 | S | SW. | SE. | calm | ·35 | ... | ... | ... | ... |
| 28 | 30·281 | 30·238 | 30·15 | 30·15 | 30·27 | 30·24 | 29·72 | 72 | 45 | 59 | 49 | 64 | 51 | 57 | S. | SW. | SE. | W. | ... | ... | ... | ... | ... |
| ○ 29 | 30·233 | 30·220 | 30·20 | 30·17 | 30·20 | 30·19 | 29·60 | 75 | 48 | 61 | 49 | 69 | 52 | 63 | S. | SW. | SW. | W. | ... | ... | ... | ... | ... |
| 30 | 30·383 | 30·290 | 30·25 | 30·25 | 30·28 | 30·25 | 29·73 | 68 | 38 | 62 | 52 | 66 | 47 | 57.5 | N. | N. | NE. | E. | ·30 | ... | ... | ... | ... |
| Aver.: | 29·862 | 29·736 | 30·25 | 29·00 | 30·28 | 29·10 | 29·31 | 57 | 41 | 62 | 38 | 69 | 35 | 49·1 | | | | | 2·20 | 2·44 | 7·500 | 2·725 | 2·29 |

# INDEX TO VOL. III.

END OF THE THIRD VOLUME.

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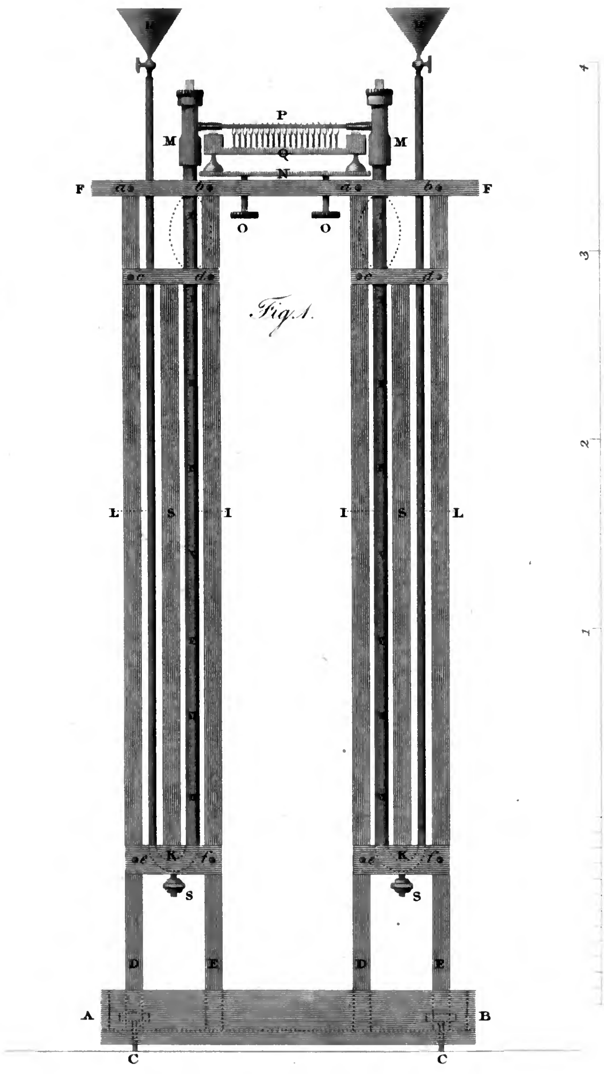
1828.

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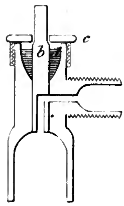


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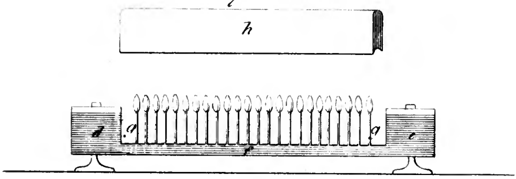


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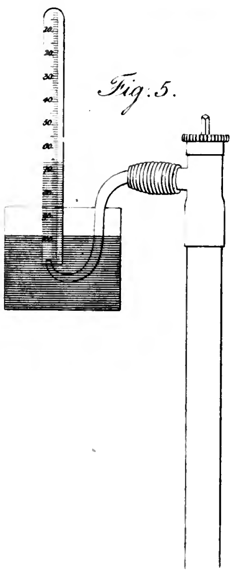

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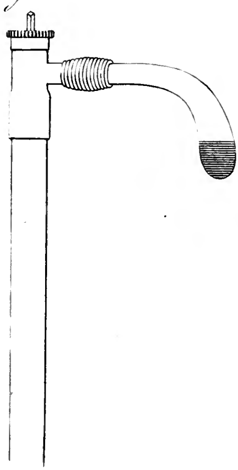

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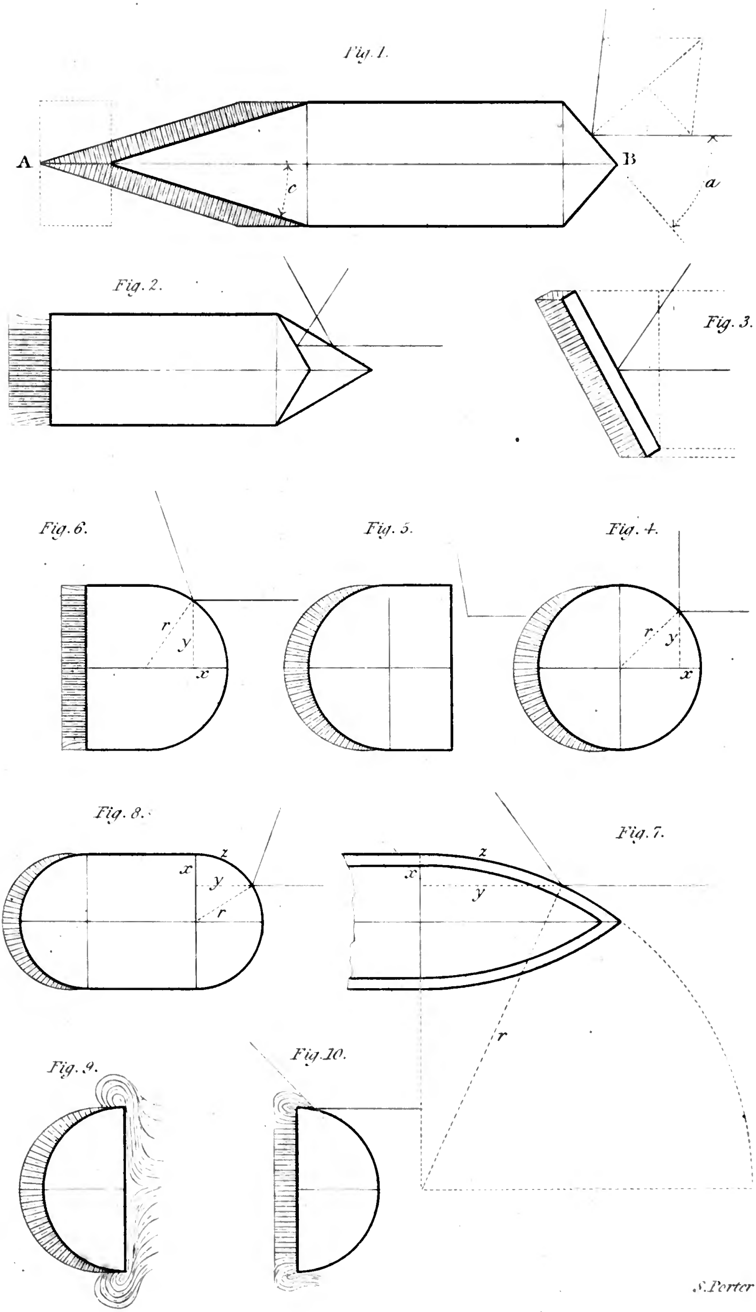
Trigonometrical position of some Hills of the Penine Chain.

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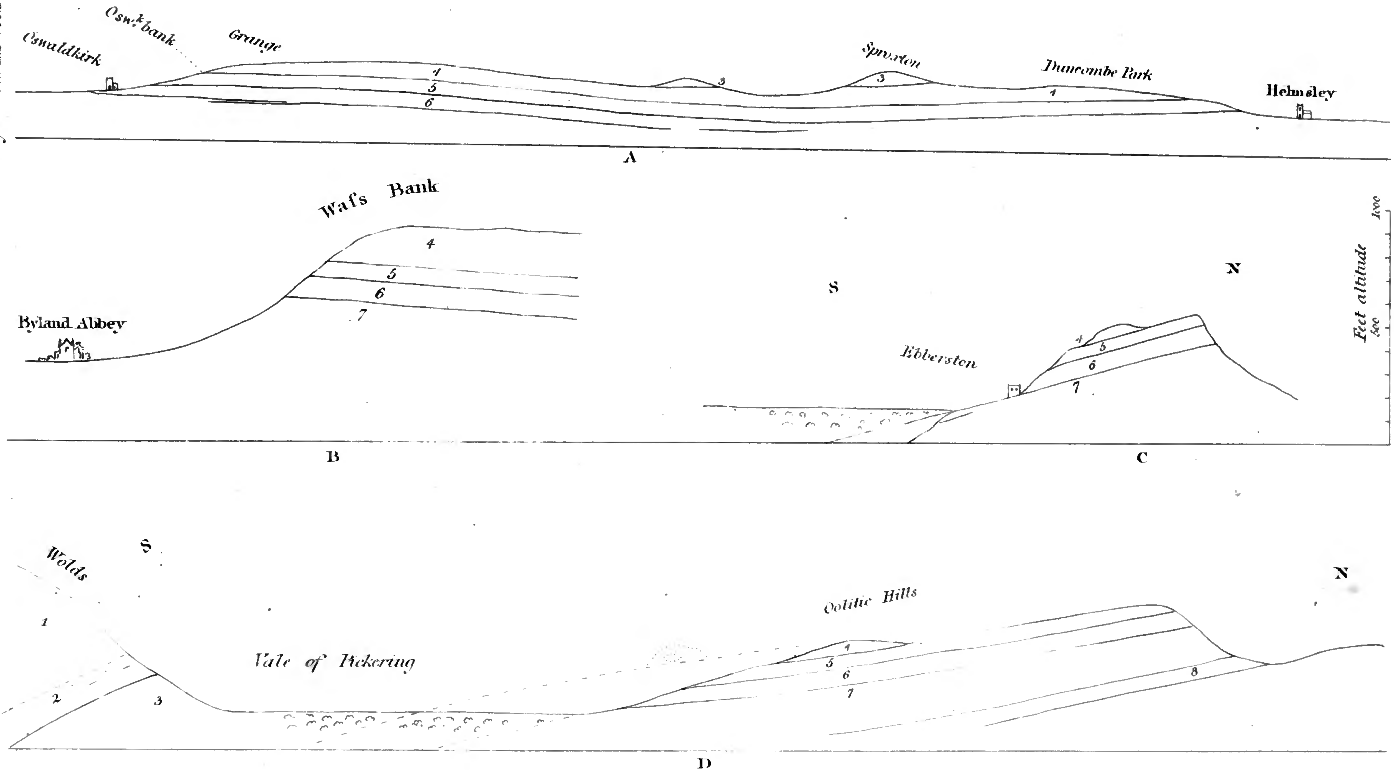
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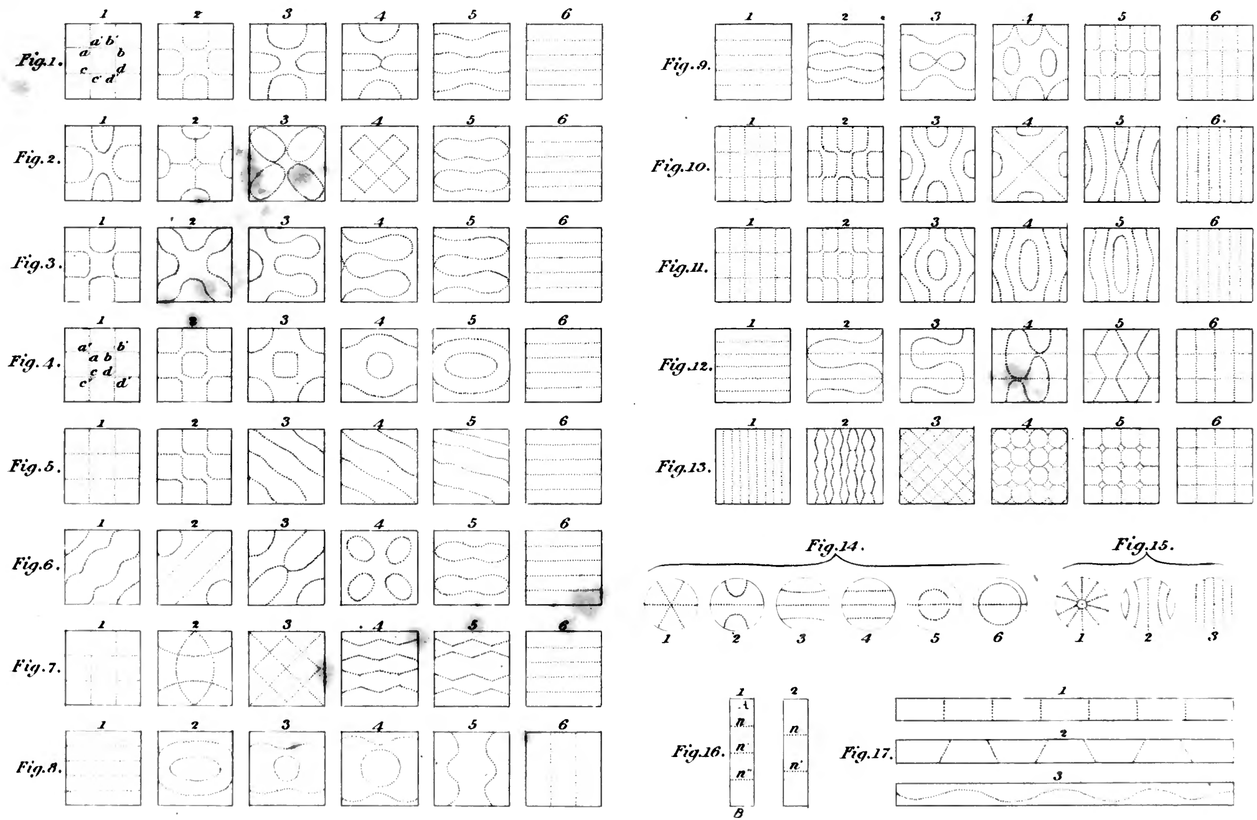
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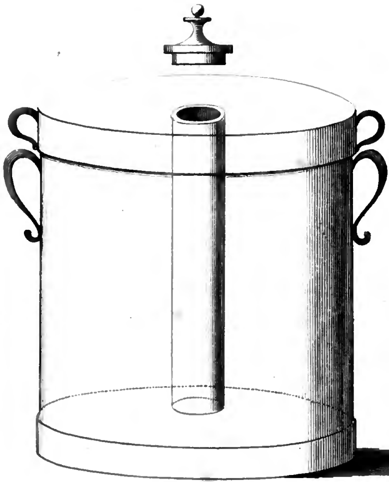
Fig. 17.

ARTIFICIAL COLD.





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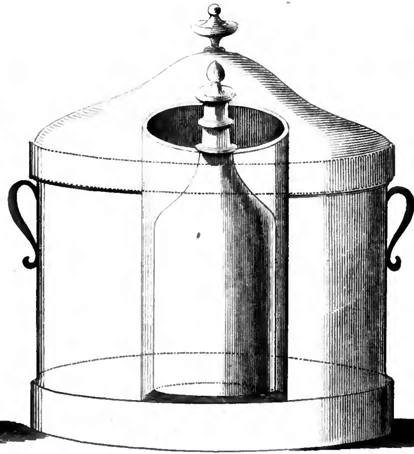

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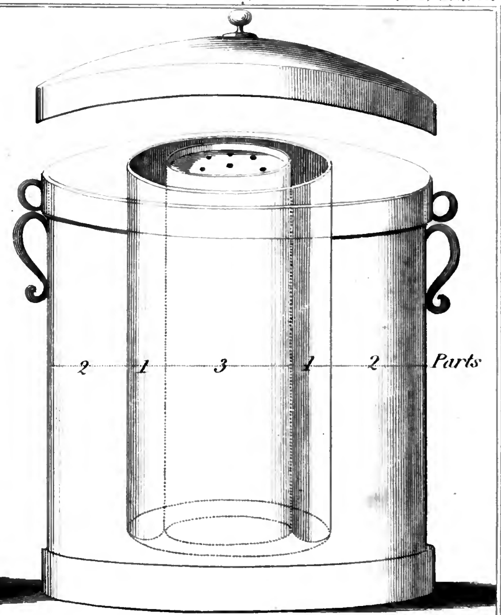


Fig. 3.

Richard Walker, Oxford, delt.

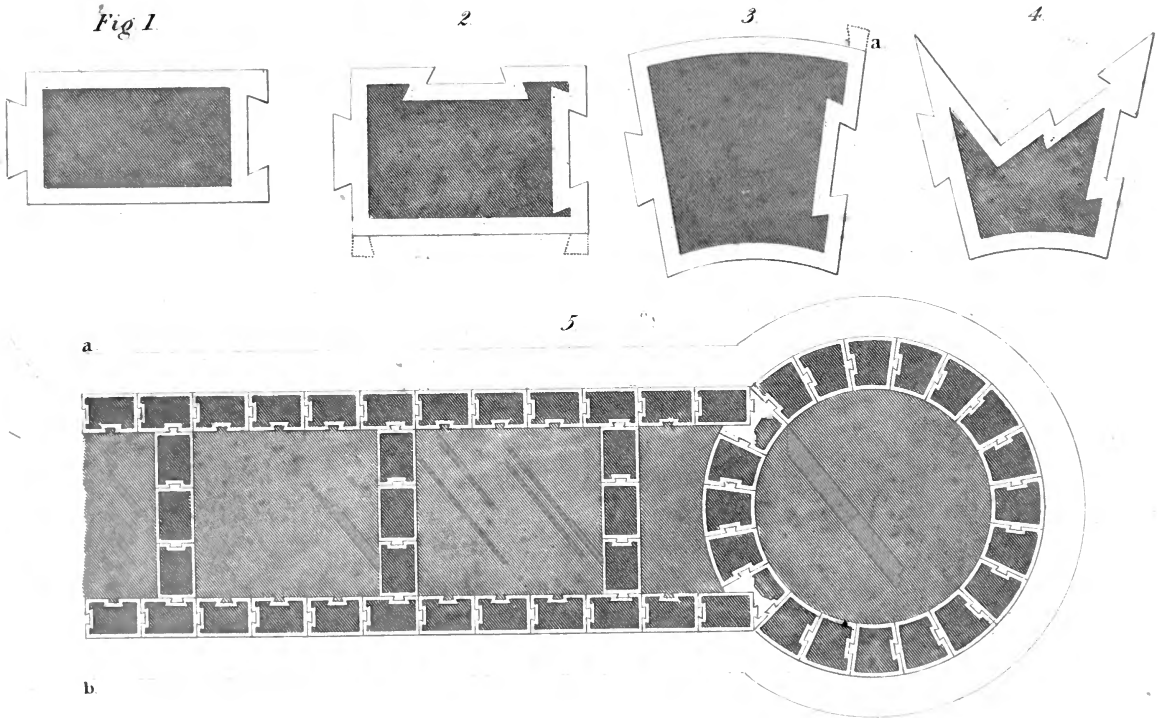

Fig 1.
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a
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## CONTENTS of N° 13.—*New Series.*



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