THE LONDON AND EDINBURGH PHILOSOPHICAL MAGAZINE AND JOURNAL OF SCIENCE.

CONDUCTED BY

SIR DAVID BREWSTER, K.H. LL.D. F.R.S. L. & E. &c.

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

AND

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

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

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ERRATA IN VOL. I. AND VOL. II.

Vol. I. Page 174, line 7 from the bottom, *dele* the word "not".
—— 294, —— 3 from bottom, *for* and no part thereof on rain *read* and no part thereof on depth.
—— 470, —— 23 from bottom, *for* changing *read* charging.
—— 431, —— 6 from bottom, after 100°, *add* we require to have the positions of these two places.
—— 439, —— 11 from bottom, *for* spectrum *read* picture.
—— 440, —— 21, *for* spectrum *read* picture.
—— 440, —— 3 from bottom, *for* crowded *read* crossed.
—— 440, —— 7 from bottom, } *for* covered *read* partially removed.
—— 441, —— 1, }

Vol. II. Page 79, line 10 from bottom, *after* distilled *dele* the comma.
—— 221, for p read q } throughout.
for q read p }

Equations (1) and (2), for h^h read h^n

lines 15 and 16, for $q+h\sin\frac{\pi}{n}$ read $p-h\sin\frac{\pi}{n}$.

—— 238, line 9, *for* nitrate flame *read* nitrate of lime.
—— 317 —— 23, *for* March 26th *read* March 25th.

THE

LONDON AND EDINBURGH

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

JANUARY 1833.

I. *Note on the Mean Temperature of Irkoutsk, in Siberia. By Professor* M. A. Kupffer, *of the Imperial Academy of Sciences of St. Petersburgh**.

M. TCHOUKINE, at Irkoutsk in Siberia, sent me some time ago an abstract of his meteorological observations, which have been continued during *ten* consecutive years; with a perseverance which is the more laudable, as the example of it is rare in the interesting country where he lives.

The months are reckoned after the Old Style, which is still generally used in Russia.

Table *showing the Mean State of the Octogesimal, or Reaumur's Thermometer in Irkoutsk, during Ten successive Years, for* 1820—1830 *inclusive.*

Months.	1820.				1821.			
	7^h A.M.	2^h P.M.	9^h P.M.	Mean.	7^h A.M.	2^h P.M.	9^h P.M.	Mean.
Jan.	...	...	...	...	−15·64	− 4·37	−12·63	−10·88
Feb.	...	...	...	...	11·66	− 0·53	11·00	7·73
Mar.	...	...	...	...	− 7·12	+ 3·55	− 4·14	− 2·57
April	...	...	...	...	+ 4·88	11·18	+ 3·38	+ 5·48
May	...	...	...	...	6·72	13·53	5·56	8·60
June	...	...	...	...	11·02	14·21	8·83	11·35
July	+ 9·56	+16·90	+11·20	+12·55	12·90	20·71	11·40	15·00
Aug.	7·00	14·60	9·45	10·35	10·68	13·80	9·55	12·01
Sept.	+ 1·53	11·63	+ 4·33	+ 5·83	+ 0·78	8·40	+ 0·50	+ 3·23
Oct.	− 5·71	+ 2·06	− 5·45	− 3·03	− 3·06	+ 2·16	− 2·76	− 1·22
Nov.	14·08	− 6·33	13·41	11·27	12·43	− 5·47	12·20	10·03
Dec.	−20·42	−13·58	−18·63	−17·54	−18·42	−12·42	−17·23	−16·02

* Communicated by the Author.

Months.	1822.				1823.			
	7^h A.M.	2^h P.M.	9^h P.M.	Mean.	7^h A.M.	2^h P.M.	9^h P.M.	Mean.
Jan.	−21·93	−12·45	−20·85	−18·41	−18·68	−11·04	−18·00	−15·91
Feb.	11·43	− 4·18	9·71	− 8·44	14·50	− 7·10	14·32	11·97
Mar.	− 2·06	+ 4·76	− 0·66	+ 0·68	− 7·77	+ 2·09	− 6·30	− 3·99
April	+ 1·45	8·93	+ 2·40	4·26	+ 0·77	8·00	+ 2·00	+ 3·59
May	6·97	15·00	6·84	9·60	6·16	13·80	6·42	8·79
June	11·20	17·33	11·50	13·34	12·40	20·60	11·16	14·72
July	12·79	20·20	13·21	15·40	13·03	19·39	12·32	14·91
Aug.	8·00	15·16	9·03	10·73	8·06	14·71	8·23	10·33
Sept.	+ 2·58	9·96	+ 3·02	+ 5·19	+ 1·52	9·01	+ 2·73	+ 4·42
Oct.	− 3·06	+ 2·16	− 2·76	− 1·22	− 4·70	+ 1·06	− 4·58	− 2·74
Nov.	14·13	− 7·73	13·43	11·76	10·40	− 4·83	9·70	8·31
Dec.	−19·03	−14·26	−18·19	−17·16	−20·18	−15·32	−20·58	−18·69
	1824.				1825.			
Jan.	−16·58	−10·00	−15·24	−13·94	−18·03	−10·03	−15·80	−14·62
Feb.	13·20	− 4·27	12·40	9·96	16·00	− 7·67	13·71	12·46
Mar.	− 6·75	+ 1·60	− 4·47	− 3·21	− 8·04	+ 1·48	− 5·19	− 3·92
April	+ 3·70	10·86	+ 4·56	+ 6·37	+ 0·76	12·10	+ 2·06	+ 4·97
May	8·09	13·73	8·51	10·11	8·90	16·42	8·29	11·20
June	11·70	16·63	12·57	13·63	12·80	20·53	12·36	5·23
July	13·50	17·90	13·77	15·06	10·87	13·22	11·00	11·20
Aug.	9·45	13·77	10·11	11·11	9·16	14·16	9·95	11·09
Sept.	+ 3·13	8·00	+ 3·46	+ 4·86	+ 2·30	9·16	+ 2·96	+ 4·81
Oct.	− 2·22	+ 1·07	− 1·51	− 0·89	− 2·85	+ 1·93	− 2·74	− 1·22
Nov.	10·78	− 6·10	10·06	8·98	10·53	− 3·36	8·16	7·35
Dec.	−13·16	− 8·61	−11·68	−11·15	−21·58	−13·93	−20·09	−18·53
	1826.				1827.			
Jan.	−19·64	−10·39	−17·74	−15·92	−18·16	−11·32	−16·59	−15·36
Feb.	18·17	− 7·60	15·32	13·70	13·14	− 4·52	− 9·42	9·03
Mar.	− 8·53	+ 1·83	− 4·77	− 3·82	− 5·04	+ 5·00	+ 2·93	− 0·99
April	+ 3·43	12·60	+ 3·30	+ 6·44	+ 0·70	8·40	1·36	+ 3·49
May	6·48	15·24	6·95	9·62	8·52	17·17	8·26	11·32
June	14·33	20·23	13·60	16·05	13·93	20·60	13·00	15·84
July	12·23	18·39	12·26	14·30	13·00	20·43	12·87	15·43
Aug.	9·00	14·20	9·50	10·90	8·42	14·68	8·87	10·66
Sept.	+ 1·76	10·26	+ 3·63	+ 5·22	+ 2·41	11·90	+ 3·15	+ 5·82
Oct.	− 2·25	+ 3·58	− 2·15	− 0·27	− 3·25	+ 3·84	− 2·80	− 0·74
Nov.	12·00	− 6·23	10·93	9·72	10·90	− 6·16	10·00	9·02
Dec.	−16·93	− 9·93	−15·16	− 4·01	−19·32	−12·16	−17·87	−16·45
	1828.				1829.			
Jan.	−23·77	−13·55	−22·03	−19·78	−19·88	−11·77	−19·22	−16·96
Feb.	17·08	− 3·24	14·15	11·49	18·12	− 4·22	14·16	12·17
Mar.	7·64	+ 5·38	− 5·45	− 2·57	7·68	+ 3·88	− 5·73	− 3·18
April	− 0·25	10·03	+ 0·35	+ 3·38	− 1·11	8·22	+ 0·07	+ 2·39
May	+ 5·27	13·06	5·64	7·99	+ 5·31	14·76	6·40	8·82
June	11·16	19·63	11·30	14·03	10·47	17·03	11·38	12·96
July	12·72	22·22	13·59	16·18	11·93	21·25	13·47	15·55
Aug.	9·00	14·20	9·50	10·90	5·42	14·68	8·87	10·66
Sept.	+ 1·76	10·26	+ 3·63	+ 5·22	+ 2·41	11·90	+ 3·15	+ 5·82
Oct.	− 2·25	+ 3·58	− 2·25	− 0·27	− 3·25	+ 3·84	− 2·80	− 0·74
Nov.	12·00	− 6·23	10·93	9·72	10·90	− 6·16	10·00	9·02
Dec.	−16·93	− 9·93	−15·16	−14·01	−19·32	−12·16	−17·87	−16·45

Months.	1830.			
	7h A.M.	2h P.M.	9h P.M.	Mean.
January......	−16·40	− 8·07	−14·60	−13·02
February	11·84	− 0·74	8·77	7·12
March	−10·00	+ 3·00	− 5·65	− 4·22
April	+ 0·09	10·14	+ 1·40	+ 3·88
May	5·83	15·46	7·47	9·59
June	+10·78	+21·13	+13·00	+14·97

Table *containing the Means of Ten consecutive Years.*

Months.	7h A.M.	2h P.M.	9h P.M.	Mean.
January.. ...	−18·87	−10·30	−17·27	−15·48
February ...	14·51	− 4·41	12·30	10·41
March........	− 7·06	+ 3·26	− 4·53	− 2·78
April	+ 1·14	10·05	+ 2·09	+ 4·43
May	6·83	14·84	7·03	9·57
June	11·98	18·79	11·87	14·21
July	12·25	19·06	12·51	14·61
August.......	8·63	15·16	9·47	11·09
September...	+ 1·81	9·99	+ 2·98	+ 4·93
October	− 3·79	+ 2·51	− 3·11	− 1·46
November...	11·50	− 5·38	10·55	9·14
December....	−18·30	−12·00	−17·06	−15·79
Mean.........	− 2·61	+ 5·13	− 1·57	+ 0·31

Observations on the preceding Results.

(See this Journal, vol. i. pp. 135, 260, 428.)

It appears from the last of these tables that the mean temperature of Irkoutsk for ten years is +0°·31 of Reaumur, or 32°·7 of Fahrenheit, at the hours of 7h A.M. 2h P.M. and 9h P.M. Now it appears from the Leith hourly observations that the mean temperatures of these hours differ from the mean temperature of the day, in the following manner:

7h A.M.	−1°·983 Fahr.	
2 P.M.		+3°·203
9 P.M.	−0°·438	
	−2 ·421	+3°·203
		−2 ·421
		+0 ·782

Hence the mean temperature obtained from the tables exceeds the true mean temperature of the twenty-four hours by 0°·782. The reason of this is obvious, from the circumstance that the observations made at 2^h P.M. are made near the time of maximum, while no observations are made so near the time of minimum temperature. Hence we have

Observed mean temperature of Irkoutsk . . .	32°·7 Fahr.
Correction	−0 ·78
Corrected mean temperatnre	31·92

The mean temperature of Irkoutsk is therefore almost exactly that of the freezing point.

As I have no means of ascertaining even the approximate height of Irkoutsk above the level of the sea, it is impossible to compare the above result with that of the formula. I have not, therefore, calculated its distance from the Asiatic Pole; but taking it rudely from a globe, it is about 27° 10′, which by the formula $T = (81°·8 \sin D') + 1°$, gives for the temperature at the level of the sea 38°·3, leaving a difference from the observed temperature of 6°·4 as due to elevation.

Irkoutsk is situated in latitude 52° 16′·7 N., and longitude 104° 11′ E., of Greenwich, according to the observations of Dr. Erman, who at the same time determined the following particulars relative to the magnetic action of the globe.

Dip of the North Pole of the needle	68° 6′·50
East declination	2 4·40
Magnetic intensity	1·6324

The following are the results of M. Hansteen's observations made at the same time:

Dip of the needle	68°12′·9
Declination	1 37·2
Magnetic intensity	1·6466

II. *Observations on the Magnetic Intensity at Paris, Brussels, Göttingen, Berlin, and Stockholm. Extracted from a Letter from* Frederick Rudberg, *Professor of Physics in the University of Upsal, to* Sir D. Brewster.

THE values of the relative magnetic intensity contained in the following table were obtained, during my journey at the beginning of the present year, from experiments made at Paris, Brussels, Göttingen, Berlin, and Stockholm. The observations were made with an intensity compass, constructed by M. Gambey. To this compass belonged two needles, No. 1. and No. 2, with which I determined, by a particular investigation after my return to Stockholm, the variation which the intensity underwent by a change of temperature. The corrections which I found were,

For No. 1. $i' = i\,(1-0·0004660\,t)$, and

For No. 2. $i' = i\,(1-0·0005006\,t)$

in which the temperature *t* is reckoned for the centigrade thermometer.

The following were the times of oscillation observed with a chronometer.

TABLE *of the Times of Oscillation of two Magnetic Needles at Paris, Brussels, Göttingen, Berlin, and Stockholm.*

	Temperature Centigrade.	Time observed in Seconds.	Time reduced to +15° Centigrade.
Paris ....No. 1.	9°·5	1175″·1	1176″·6
——	19·0	1177·8	1176·7
No. 2.	9·5	1197·7	1199·3
——	19·7	1199·9	1198·5
Brussels.....No. 1.	17·0	1194·9	1194·4
No. 2.	16·75	1216·9	1216·4
Göttingen ..No. 1.	19·0	1191·5	1190·3
——	15·3	1190·5	1190·4
No. 2.	12·0	1210·8	1211·7
Berlin ......No. 1.	13·75	1191·8	1192·1
No. 2.	12·0	1214·5	1215·4
Stockholm ..No. 1.	23·0	1273·3	1270·9
——	22·5	1273·3	1271·1
——	29·25	1276·1	1271·9
——	1·75	1267·6	1271·5
No. 2.	17·25	1296·2	1295·5
——	22·10	1298·5	1296·2
——	29·0	1301·2	1296·6
——	1·0	1291·7	1296·2

From these observations the *horizontal intensities* become as follow:

	Paris.	Brussels.	Göttingen.	Berlin.	Stockholm.
	1·0000	0·97042	0·97695	0·97416	0·85697
		0·97208	0·97964	0·97368	0·85858
		0·97078	0·97833	0·97238	0·85607
		———	———	———	0·85586
Mean		0·97109	0·97831	0·97341	0·85493
					———
					0·85648

By means of a dipping-needle of M. Gambey's, I find the dip to be as follows:

At Paris.	Berlin.	Stockholm.
67° 41′	68° 16′	71° 40′

At Brussels, M. Quetelet had found the dip in the month of May of 1831, to be 68° 49′.

At Göttingen the dip has not been determined since 1826; but applying its annual decrease, it would be in 1832, = 68° 13′.

By means of these data we obtain for the *total magnetic intensity* at the beginning of the present year 1832,—

Paris.	Brussels.	Göttingen.	Berlin.	Stockholm.
1·0000	1·0205	1·0010	0·9982	1·0340

The result for Berlin differs greatly from that which might have been expected from the general decrease of the intensity with the latitude. I have no reason, however, to suppose that there is any error in the observations. The dip was determined by four trials, as well by myself alone, as by M. Riess and me; and the differences between the results were extremely small.

Stockholm, Sept. 20th, 1832.

III. *On a New and Simple Heliostat. By* R. Potter, *Esq., Jun.**

[With Figures: Plate I.]

HAVING a short time ago determined upon commencing a series of optical experiments, which will require the use of an instrument having the effect of a heliostat, my first step was to seek to make myself acquainted with the principle of that of Dr. S'Gravesande. This is the only instrument of the kind of which I have yet met with any account; and, by accident, the work which I consulted for a description of it having the plates bound in a separate volume, I could only at that time have access to the description without the plate.

Those who know Dr. S'Gravesande's instrument will not be surprised that I should soon be embarrassed in consulting a description intended only to be intelligible with the assistance of the figures. I had not, however, got through the account, when a thought struck me that the simplest plan of a heliostat must be on the equatorial principle: another moment's reflection convinced me that a very simple, yet effective construction might be adopted upon that method.

I have since made a heliostat upon this plan; and having proved its efficacy, I now proceed to give a description of it. The instrument I have executed is similar to figures 2. and 3, of which fig. 2. represents the side view, and fig. 3. the horizontal one. Before, however, we go to the description of the instrument itself, we will examine the principle of its construction.

Let *e o*, fig. 1. be a spindle which can be made to revolve, and which is set parallel to the earth's axis. Let *a b* be a mirror attached to the end of the spindle. Then the appa-

* Communicated by the Author.

rent daily motion of the sun being nearly in a circle round the earth's axis, and sensibly also in a circle round the spindle *e o*, if the mirror be so set as to reflect the sun's light in the direction *o p*, still parallel to the earth's axis, and if the spindle revolve, with the mirror, once round in twenty-four hours, then whilst the sun continues to shine on the mirror, its light will be reflected in the same direction *o p*. The truth of this will be easily seen on considering that the positions of the sun and the mirror must be the same at all times of the day with respect to the line *o p*; and to an eye placed at *p*, the reflected image of the sun would appear to stand still, and hence the propriety of the appellation Heliostat.

For the source of motion to the spindle, I have used a common clock, shown at *f g*, fig. 3, and *f g h*, fig. 2; the whole apparatus being attached to a rectangular board, as seen in the figures. The hands of the clock being removed, a grooved pulley is fixed on the arbor, which had carried the hour-hand, as at *i*, fig. 3. This pulley revolving once round in twelve hours when the clock is going, communicates motion by means of a band to the pulley *k*, fixed on the spindle *e o*, which being twice the diameter of the pulley *i*, causes the spindle to revolve, as required, once in twenty-four hours.

The band which I have found to answer well for the pulleys is a strong cotton thread passed several times round them, and then fastened with a loop, which leaves the means of adjusting the band to a proper tightness. The pendulum should be adjusted by the revolution of the spindle after the instrument is complete, by which means any slipping of the band, or want of accuracy in the dimensions of the pulleys, may be compensated.

Considerable care is requisite to keep this, as well as every other instrument of a similar kind, correctly in position; for it will readily be perceived that the correct action of it depends as much upon the spindle *e o* being accurately placed, as upon the proper rate of the clock. I have had mine so nearly adjusted, as to reflect the sun's light upon the same spot on the ceiling of a lofty room so exactly, that no perceptible motion could be detected during an hour and a half.

It will be seen that neither this nor Dr. S'Gravesande's heliostat has any provision for counteracting the variation of the sun's declination during the time of use, and of course the instruments may be brought to act more correctly when the sun is near the summer or winter solstice than at other times of the year.

If an instrument-maker were employed to construct this heliostat, he would easily contrive the plan so as to connect the spindle and clock into one whole, and so as to dispense with the band and pulleys: he would also see where to introduce the proper contrivances for adjusting the various parts.

This instrument may certainly be executed at much less expense than that of Dr. S'Gravesande, and I think it will be much more easily and correctly adjusted. It should be always provided with a second mirror, as at *l m*, fig. 2, by which the pencil of light may be thrown in any direction which may be desirable: in many optical experiments, however, the second mirror will be unnecessary.

IV. *Of the Structure of Living Fabrics.* By the *Rev.* Patrick Keith, *F.L.S.**

THE structure of every living fabric is composed partly of solids, and partly of fluids. The solids are the substances which constitute the several parts or organs that give form to the fabric, as the head, feet, limbs and trunk of animals; or the root, trunk, leaves, and flowers of vegetables. The fluids are substances absorbed or imbibed from without, or formed, secreted, or exhaled from within,—as chyle, blood, lymph, urine,—sap, nectar, cambium, expressed juice. While life remains the fluids are in motion, except in the very singular case of the hybernation of some animals, and perhaps of some plants; or in the equally singular case of some very vivacious animalcula, which, though left on the stage of the microscope till they have shrunk, by the evaporation of the fluid in which they were placed, to a mere dry and shrivelled-up membrane, will again revive and move as at first, upon the application of a little fresh water. The same thing happens to many of the mosses, which will revive and recover their verdure when moistened with water, even after having been completely dried, and kept in a dried state, for many years.

The perfection of the individual is in the ratio of the complexity of its organization. The fewer the organs, the fewer the faculties with which the individual is endowed. This is very evident even on the most superficial survey of the grand divisions of the empire of animated nature. Look at any individual, or at any group of individuals, belonging to the vegetable kingdom. Where are the organs of locomotion; where are the organs of sense? They are organs of which the vegetable is altogether destitute. Look at any individual, or at any group of individuals, belonging to the animal kingdom, and the organs of sense and of locomotion are the first

* Communicated by the Author.

that attract your notice. Thus the animal is elevated in the scale of existence to a rank surpassing that of the vegetable; first, by means of the organs of sense and of intellection, by which it holds communication with the external world, and is rendered conscious of its own individuality; and secondly, by means of the organs of locomotion, by which it ranges in pursuit of new gratifications, and transports itself even to distant regions.

But each kingdom has a gradation within itself, from the highest or most organized orders, to the lowest or least organized orders,—from the *Vertebrata* to the *Infusoria*, on the one hand; and from the trees of the forest, to the *fungus* that gives colour to the Polar snows, on the other. This will be rendered evident from a survey of the structure, whether of plants or of animals.—We will begin by taking a survey *Of the Structure of Plants.*

The simplest view of the structure of plants is, perhaps, that by which they are regarded as consisting of two essentially distinct parts; namely, an axis, and its appendages;— the axis including both the *caudex ascendens* and *descendens* of Linnæus; and the appendages, by whatever name designated, being presumed to be merely modifications of leaf. Whether this presumption is well founded or not, we do not at present stop to inquire; though it appears to us that flower and fruit are something very different from mere modifications of leaf: but whether they are so or not, they are, at all events, appendages to the axis. If the axis is itself complete, that is, furnished with the full complement of appendages common to vegetables in general, the plant is said to be Perfect. It is also said to be *Phænogamous*, as being furnished with conspicuous flowers; because conspicuous flowers are the glory of the plant, and in many plants they are wanting. If the axis is itself incomplete, that is, destitute of one or more of the organs common to vegetables in general, the plant is said to be Imperfect. It is also said to be *Cryptogamous*, that is, destitute of conspicuous flowers, because in plants called Imperfect, conspicuous flowers are wanting.

We are aware that the propriety of the division of plants into Perfect and Imperfect has been lately called in question by a Professor of Botany of the present day. Mr. G. Burnett of King's College, London, says that plants have been regarded as Imperfect, merely because they were imperfectly considered;—since a plant that has no visible root, has still a potential root*. We do not regard the objection as being

* Journ. of Royal Instit. 1831, p. 84.

of any great weight. We are not, indeed, entitled to call any work of God imperfect in its kind: but surely we may call it imperfect as compared with others, or as located in a scale of degrees. Men have, in short, always done so. Finding a standard in the highest order of a class, they have compared other orders with it, and have regarded them as being more or less perfect according to the degree of their proximity to that order, determined by the anatomy of their fabric, or complexity of their organization. What are the arrangements of Cuvier in the animal kingdom, but arrangements founded upon the comparative perfection of the organization of his different divisions? Why are the *Vertebrata* put in the first rank, but because they are more perfect in their organization than the *Mollusca*, which are put in the second rank, and these more perfect than the *Articulata*, which are put in the third rank; and so on? Because an oyster can move itself, through means of great labour, a little way on its native bed, are we to say that its organization is as perfect as that of the "Leviathan that playeth, or taketh his pastime, in the great and wide sea?" We do not insist upon the introducing of such a division into the arrangements of a Flora; but in any comparative view, whether of plants or of animals, its utility is obvious. Yet Mr. Burnett seems scarcely inclined to admit even this. For phytologists, he affirms, have through "ignorance or prejudice" set up a type in the selected seed, or root, of some peculiar plant, and then they have required that all other plants should conform to it; and failing in that conformity, they have pronounced them to have no seed, or no root, at all. Surely this is not sufficiently liberal. Phytologists were doing their best according to the existing state of the science, and in return for their labours they are told that they were ignorant or prejudiced. If Mr. Burnett has acquired new light, let him enlighten us; but let him not censure us for faults of which we are not guilty. If we have been groping our way in the dark, we are now willing to open our eyes to the light of day. Phytologists have in fact described many varieties and modifications both of seeds and of roots;—only where they have found no visible organs to which they could apply the name, they have said that such plants were without seeds, or without roots. Yet, says Mr. Burnett, they have potential roots. Be it so: and how is the phytologist to describe, or to represent a potential root? There is indeed a considerable advantage in the supposition of a potential root or seed. For upon this principle you may prove the existence of almost any organ whatever, in almost any plant or animal whatever. Thus you may prove that frogs have tails. The syllogism will run thus: All animals have tails. Cows have tails, horses have tails, asses have tails; *ergo*, frogs have tails also. Your antagonist may indeed say, Oh, but I have examined a frog, and I cannot see its tail. But your reply will be,—It is of no consequence that it has not a visible tail; it has a potential tail, and that is enough.—Such is the advantage of the argument from potentiality.

Yet Mr. Burnett admits in his own creed, what he condemns in the creed of others. Speaking of animals, he says, "Nutrition may be performed without a mouth to receive, teeth to chew, or even a stomach to digest, the food; respiration without either lungs or gills; prehension without either hands or claws; and progression without either wings or feet." What is this, but to admit that certain animals are destitute of these organs? and what is the describer to say? or the arranger to do?—Oh, says Mr. Burnett, found your divisions upon special functions, and not upon special organs: divide the several parts of the plant into nutrients and generants, and then you are sure to be right; for whatever is not a generant is a nutrient, and whatever is not a nutrient is a generant. To be sure there is in vegetables a certain *tertium quid*, a thing called a stock or caudex, "an accessory or intermediate,—the organ of extension, formed more or less of both extremes, and serving equally for their varied segregation and extension," which there is some little difficulty in disposing of; and yet after all it may belong either to the one or to the other, and be disposed of accordingly. But with all due deference to Mr. Burnett and his opinions, we may safely affirm that the division into nutrients and generants will leave him just where he was before. If he is describing any particular plant, he must tell us of what organs its nutrient system consists. Has it a root? has it a branch? has it a leaf? He must do the same thing by the generant system also. Has it a flower? has it a seed? has it a seed-vessel?—And after all, there is nothing novel in the matter. The division here recommended has been long recognized by physiologists, and even introduced into their arrangements. If Mr. Burnett will take the trouble to look into Keith's System of Physiological Botany, published in 1816, he will find that the structure of the plant is exhibited upon the express ground of such a division; namely, upon that of conservative organs and reproductive organs,—the former corresponding to Mr. Burnett's nutrients, and the latter to his generants; and the method followed up throughout the whole extent of the vegetable kingdom, as distributed into Perfect and Imperfect plants, the Perfect plants being regarded as comprising the *Phænogamia* of modern botanists, and the Imperfect plants the *Cryptogamia* of Linnæus.

After all this, the reader will perhaps be of opinion that the division of plants into Perfect and Imperfect, is sufficiently well founded to justify our adoption of it, at least in a general and popular survey of the vegetable structure, which it is our object briefly to exhibit, and to which we now proceed,—taking, first, the external structure, and, secondly, the internal structure.

I. *The External Structure.*

If a plant of the perfect class is detached from the soil, and surveyed externally in the season of flowering, it may be perceived, even by the most inattentive observer, to be composed of the following distinct parts: the root, the trunk, the branch, the bud, the bulb, the leaf or frond, the flower, the fruit, and perhaps the seed. Of these parts some are temporary, and some permanent; some conservative, and some reproductive; or, as Mr. Burnett would say, some nutrients, and some generants.

The Root.—The root, or *caudex descendens* of Linnæus, is that part of the plant by which it attaches itself to the soil in which it grows, or the substance on which it feeds, and is the principal organ of nutrition. To this definition there are no doubt a good many exceptions. The several species of *Lemna* or Duck-meat float on the surface of the water, and are not fixed by their roots to any particular spot. Many of the *Confervæ* have no root at all, or, at least, no distinct organ that can be called by that name; while the Truffle (*Tuber cibarium*) is apparently altogether root. But almost all plants of the higher orders are fixed in the earth by a root, descending in species of large growth, and even in many species of small growth, to a considerable depth below the surface,—

> ———— ———— quæ quantum vertice ad auras
> Æthereas, tantum radice in Tartara tendit.—*Virg. Georg.* ii. 291.

and spreading by means of lateral divisions to a considerable extent around the centre. The divisions of the root of the Baobab, or African Calabash-tree (*Adansonia digitata*), have been known to measure upwards of a hundred feet in length.

Yet there are many roots which descend into the soil, merely in one single and undivided mass, large at the base, and tapering in a spindle-shaped form to the apex, without either branch or fork, beyond that of a few scattered and thread-like fibres. The Carrot, the Parsnip, and the Radish are well-known examples. Roots of this species are sometimes found to terminate abruptly, as if cut or bitten off at the point. The root of *Scabiosa succisa*, better known, perhaps, by the vulgar appellation of Devil's-bit Scabious, affords an example of the case in question, as well as of the whimsical and superstitious notions of the simplists of ancient times with regard to the virtues of plants. Almost all plants were believed to be possessed of some peculiar and medicinal properties; and the Devil was believed to be,—what it would certainly not have been very orthodox to doubt,—the grand and leading agent in the production of all evil whatsoever affecting the interests of man. Now here was a plant with part of the root bitten off; and what was the inference that seemed the most probable? Why, that the part wanting, was wanting through the fraud and malice of the Devil, bitten off out of sheer hatred to mankind, and secreted or destroyed on account of the peculiar potency of its medicinal virtues. But unhappily for the patients of modern times, the medicinal virtues of this plant do not upon inquiry turn out to be anything remarkable, and the deficiency of the part bitten off has been accounted for in another way.

Many roots are fibrous or capillary, that is, consisting of several small and thread-like fibres, supporting the plant, not by their individual strength, but by their numbers and distribution, elongating in a divergent direction, and riveting down the plant on all sides. Such roots are exemplified in the greater part of the Grasses, as in Wheat, Oats, and Barley.

Some roots are bulbous, that is consisting of a circular assemblage of small fibres originating in the under surface of a bulb or knob, solid, or composed of a number of succulent coats, or scales, and containing the rudiments of a future plant. They are exemplified in the bulbs of the Crocus, Tulip, and Lily.

Some roots are tuberous, that is, consisting of a knob or tubercle, furnished with a number of small and scattered fibres, or of a number of such knobs or tubercles, united by means of such fibres, and forming a cluster. If the knob is single, it is generally solid, and of a spherical form, as in *Bunium bulbocastanum.* If the knobs are not single they are very often in pairs, as in *Ophrys spiralis* or Ladies' Traces, or in *Orchis mascula* or Early Orchis. If the knobs of this last species are taken and separated, and then immersed in water, the one will be found to sink, and the other to swim. This is a phænomenon that seems also to have puzzled the simplists of antiquity not a little, and to have given rise to a great deal of idle and superstitious conjecture. It was believed that the knob which sinks must necessarily have possessed some peculiar and potent properties, and accordingly some potent properties were very liberally ascribed to it, of which the reader will find a full and particular account in Gerard's great work, who seems to have believed all that he relates, and treats the subject as if he loved it*. One thing he has omitted: If prepared in a particular manner, and secretly attached to, or concealed in, the dress of any one, it was believed to have the singular property of exciting, by means of due management, a violent attachment, in the breast of the wearer, to the person who had thus concealed it. This belief is still a vulgar error among the ignorant and superstitious, though the sinking of the one knob, and the swimming of the other, have been accounted for from the regular operation of natural causes, and the mystery and magic charm of the phænomenon thus altogether dissolved.

Such are the principal sorts of roots distinguished by botanists, at least as regarding the general outline of their figure; all of which when inspected more closely will be found to be furnished with a number of minute and lateral fibres, which are themselves furnished with a number of minute and secondary fibrils, forming the *chevelure* of the root, and terminating ultimately in soft, bibulous, and club-shaped appendages, which, from their ready capacity of absorbing fluids, have obtained the name of *spongiolæ*, or little sponges.

The Trunk.—The trunk, or *caudex ascendens* of Linnæus, is that part of the plant which springs immediately from the root, and ascends in a vertical position above the surface of the soil, supporting the branches, and constituting for the most part the principal bulk of the individual. It is a term taken from the Latin *truncus*, and has the same signification among botanists which it had among the ancient classics.

Olim truncus eram ficulnus.—*Hor.* lib. I. Sat. viii. 1.

As applicable to the higher orders of plants it is distinguished into three species, the Stem, the Culm, and the Stipe.

The stem is the trunk of trees, shrubs, under-shrubs, and the greater part of herbs. It is cylindrical and tapering, as in the oak and elm; or compressed, as in flat-stalked Pondweed; or triangular, as in some species of Carex; or jointed, as in the Pink, and the Grasses. It is also further distinguished as being simple or compound, solid or tubular, upright or nodding, creeping, climbing, and twining. Of these varieties the last three are the most remarkable. First, the creeping stem, which being too feeble to support itself in an upright position, extends or creeps horizontally along the surface of the earth, and sends down roots at regular intervals, to extract from the soil new supplies of nourishment. It is exemplified in *Poten-*

* Historic of Plants, p. 207.

tilla reptans, or Common Creeping Cinquefoil. Secondly, the climbing stem, which being also too feeble to support itself in an upright position, attaches itself, by means of lateral roots, or of other appropriate organs, to other plants, or to other bodies for support, and thus attains to the elevation proper to the species. It is exemplified in the case of the Vine and Ivy. Thirdly, the twining stem, the most elegant and most singular of them all, which being also too feeble to support itself in an upright position, ascends, not merely by clinging to a prop, but by winding spirally around the first plant or prop that it meets with; the winding never being effected at random, but always in a specific and determinate manner, which is also always the same in the same species of plant. Thus in the Hop plant (*Humulus lupulus*) the winding proceeds in a direction from left to right, or according to the apparent motion of the sun, and never otherwise; while in *Convolvulus sepium*, or Great Bindweed, it proceeds in a direction from right to left, or contrary to the apparent motion of the sun, and never otherwise. If you attempt to compel the stem to reverse its mode of winding, you kill the plant.

The culm, or straw, is the trunk of the Grasses, Rushes, and several other plants nearly allied to them, preserving still the original signification of the Latin term *culmus*, from which it is derived,

———— ne gravidis procumbat *culmus* aristis.—*Virg. Georg.* i. 111.

In its figure it is generally cylindrical, as in Wheat and Barley; but in some few plants it is triangular, as in *Schœnus* and *Cyperus*. In its structure it is hollow and jointed, as in the Grasses; or solid, that is, filled with a soft and spongy pith, as in the Bulrush.

The stipe, which is an anglicized spelling and pronunciation of the Latin term *stipes*, a club, or stake,—

Stipitibus duris agitur.—*Æneid* vii. 524.

is, in the language of botany, a sort of secondary trunk that supports the foliage, at least with regard to the higher orders of plants, and is peculiar to Palms; issuing annually from the root for the first four or five years of the plant's growth, and, for the future, from the summit of the main stem, which begins now to appear.

In their size trunks are to be found of all dimensions, from that of the diminutive *Draba* that surmounts the parched wall, to that of the lofty Mountain Palm that rears its head to the clouds. This immense and gigantic tree, the *Palma altissima* of Sloane, and the *Areca oleracea* of modern botanists, is a native of the West Indies, growing to the height of one hundred and twenty feet*, sometimes to the height of one hundred and fifty feet, and even, as it is said, to the very extraordinary height of upwards of two hundred feet, being about seven feet in circumference at the base, but gradually tapering towards the summit, and thus forming with its lofty crown of fronds the noblest object of vegetable creation.

——— Where casts the mountain palm, on high,
Its lengthen'd shadow from the evening sky.—*Montgomery's West Indies.*

The trunks of oak-trees attain, oftentimes, to a very great size. We may take the testimony of Ovid with regard to the oaks of ancient Italy;

Sæpe etiam, manibus nexis ex ordine, trunci
Circuière modum; mensuraque roboris ulnas
Quinque ter implebat.—*Metamorph.* viii. 747.

and we have only to make use of our own optics with regard to the existing oaks of old England. At Cowthorpe near Wetherby, in Yorkshire, there is now growing an oak that measures seventy-eight feet in circumference close to the ground, and forty-eight at the height of a yard. It is said to have begun to decline in the reign of Queen Elizabeth, and though now much in decay, is still likely to stand for many years. But the trunk of *Adansonia digitata* is beyond all comparison the largest that is yet known. Adanson in his voyage to Senegal, saw a tree of this species having a trunk that measured twelve feet in height, by twenty-seven feet in diameter†. Such trunks are sometimes hollowed out, and converted into a sort of house or cabin, serving for the abode of several families of negroes. Nor is this all! From the leaves they obtain a pleasant seasoning for their food; from the root a purgative; from the bark a pectoral anodyne; from the parenchyma of the trunk, a cataplasm that cures cutaneous eruptions; from the fruit they compose an agreeably astringent draught; they eat the kernel; they smoke the calyx; and they use the capsule as a spoon‡.

[To be continued.]

V. *Remarks on* Lagrange's *Proof of the Principle of Virtual Velocities. By the Rev.* J. CHALLIS, *Fellow of the Cambridge Philosophical Society.*§

THERE is one part of the celebrated Proof of the Principle of Virtual Velocities at the beginning of the *Mécanique Analytique*, which has been thought to be obscure, or

* Sloanes's Natural History of Jamaica.
† *Fam. des Plantes, Pref.* ccxii.
‡ *Naufrage de la Fregate la Medusa*, 1816.
§ Communicated by the Author.

not sufficiently explained. The following attempt to meet the difficulty complained of, is offered to the consideration of those who may wish to see a proof, in other respects remarkable for elegance and brevity, free from every objection.

Lagrange's reasoning is of the following nature. Instead of the given forces he substitutes other equivalent forces, in a manner equally applicable to all cases of equilibrium. The way in which he does this, though not the only one that might be adopted, is perhaps the best. A system consisting of two blocks of pulleys is placed so that one block is attached to the point of application of one of the forces, as P, and the other to an arbitrary point taken in the line in which P acts. A cord, having a weight w attached to one extremity, passes over a fixed pulley, that the weight may hang vertically, and is then carried over the pulleys of the blocks, forming m strings between them. The continuation of the *same* cord is then made to pass over the pulleys of another system, situated with respect to another force Q as the first was with respect to P; and the strings between the blocks of this system are m' in number. The same thing is done with respect to all the forces, and the other extremity of the cord is attached to a fixed point. That the strings between the blocks may be parallel, we may conceive the blocks and pulleys to be indefinitely small. The tension of the cord will be the same throughout, and equal to w. Hence if $m\,w = \mathrm{P}$, $m'\,w = \mathrm{Q}$, $m''\,w = \mathrm{R}$, &c., the effect of the systems of pulleys will be exactly the same as that of the given forces. This supposes the forces to be commensurable one with another: if they are not so, we may take w as small as we please, and so make the substituted forces as nearly equal to the given forces as we please.

The substitution being thus made, Lagrange goes on to say:—" It is evident that in order that the system drawn by these different (substituted) forces may remain in equilibrium, it is necessary that the weight (w) should not be able to descend by any infinitely small displacement whatever of the points of the system; for the weight tending always to descend, *if there be a displacement of the system which permits it to descend, it will descend necessarily and produce this displacement.*" This is the part of the proof to which I have alluded above; and certainly the reason here given for the immobility of w is not easy of comprehension.

The reason that w neither ascends nor descends may I think be seen, if the following principle, which may be considered a definition of equilibrium, be admitted: When a rigid mass is held in equilibrium by any forces, it may receive any indefinitely small displacement whatever, when it is not retained by a fixed point, or axis, or against a surface; and any, consistent with its state of retention, when it is retained, just as if it were acted upon by no forces at all. From this principle it follows that the force or forces which produce the displacement will at the first moment be solely employed in moving an inert mass, and will not alter P, Q, R, &c. Hence the tension of the cord will remain the same, and w will neither ascend nor descend; for any motion of w must be accompanied by a change of tension.

If then p be the interval between the blocks of the first system of pulleys, q of the second, r of the third, &c. and l be the length of the rest of the cord, its whole length =

$$mp + m'q + m''r + \&c. + l.$$

This must remain the same whatever displacement be made. Therefore

$$m\delta p + m'\delta q + m''\delta r + \&c. + \delta l = 0,$$

or $$P\delta p + Q\delta q + R\delta r + \&c. + w\delta l = 0,$$

whatever be the magnitudes of δp, δq, δr, &c. But if the displacement be indefinitely small, it follows, from what is said above, that $\delta l = 0$. Consequently

$$P\delta p + Q\delta q + R\delta r + \&c. = 0 \qquad \text{(A)}$$

If instead of a single mass, as we have supposed, the forces P, Q, R, &c. acted on several masses connected by inextensible cords or by hinges, for each of these masses an equation like (A) will be obtained if the tensions of the cords and reactions at the hinges be included in the forces. By the addition of these several equations, the tensions and reactions will disappear, because their virtual velocities enter with opposite signs. The resulting equation will therefore still be of the form of (A).

If it be questioned how a method which seems to have no reference to the first principles of statics, as given in the elementary treatises, should lead to a general solution of all statical problems, we may answer, that in the inductive method, (as given for instance in M. Poisson's Treatise), only two principles are admitted: 1°, that the direction of the resultant of *any* two equal forces acting on a point bisects the angle which the directions of the forces make with each other; 2°, a force produces the same effect at whatever point in its direction it be applied. By the first it comes to pass that all equations of equilibrium are homogeneous with respect to the forces; and such the general equation (A) becomes by the disappearance of w; by reason of the other, these equations are independent of the distances of the points of application of the forces from fixed points in their directions; and the same thing happens in the equation (A) on account of the indefinitely small magnitudes of δp, δq, δr, &c.

I will here add an example of applying the equation (A) in a manner which may be in some respects new. Three forces P, Q, R, act in the same plane on a rigid rod; $x\,y$, $x'\,y'$, $x''\,y''$ are the coordinates of their points of application referred to rectangular axes in the plane; and θ, θ', θ'' are the angles which the directions of P, Q, R, make with the axis of x. Now whatever small displacement be given to the rod, it may be considered to be produced by its revolving through a small angle $\delta\lambda$ about some fixed point in the plane. Let the coordinates of this point be X, Y; and let its distances from the points of application of the forces be r, r', r''. Then these points move through $r\,\delta\lambda$, $r'\,\delta\lambda$, $r''\,\delta\lambda$. If α, α', α'' be the angles which r, r', r'' make with the axis of x, then $\frac{\pi}{2}+\alpha-\theta$, $\frac{\pi}{2}+\alpha'-\theta'$, $\frac{\pi}{2}+\alpha''-\theta''$ are the angles which the directions of the forces make with the directions of the motions of their points of application. Hence the virtual velocities are $r\,\delta\lambda\cos\left(\frac{\pi}{2}+\alpha-\theta\right)$, $r'\,\delta\lambda\cos\left(\frac{\pi}{2}+\alpha'-\theta'\right)$, and $r''\,\delta\lambda\cos\left(\frac{\pi}{2}+\alpha''-\theta''\right)$. Therefore by the equation (A),

$$P\,r\sin(\alpha-\theta)+Q\,r'\sin(\alpha'-\theta')+R\,r''\sin(\alpha''-\theta'')=0.$$

But $\quad \sin\alpha=\dfrac{y-Y}{r},\qquad \cos\alpha=\dfrac{x-X}{r}$, &c.

Hence
$$\begin{aligned}0=\;&P\{(y-Y)\cos\theta-(x-X)\sin\theta\}\\&+Q\{(y'-Y)\cos\theta-(x'-X)\sin\theta\}\\&+R\{(y''-Y)\cos\theta-(x''-X)\sin\theta\}\end{aligned}$$

As this equation is to be true whatever be the displacement, that is, whatever be X and Y, we must have,

$$P\cos\theta+Q\cos\theta'+R\cos\theta''=0$$
$$P\sin\theta+Q\sin\theta'+R\sin\theta''=0$$
$$\begin{aligned}&P(y\cos\theta-x\sin\theta)+Q(y'\cos\theta'-x'\sin\theta')\\&\quad+R(y''\cos\theta''-x''\sin\theta'')=0,\end{aligned}$$

the known equations applicable to this instance.

If the point of application of R be fixed, the displacement must consist in a motion of rotation about this point. Making it the origin of coordinates, we have $y''=0$, $x''=0$, $Y=0$, $X=0$, and $P(y\cos\theta-x\sin\theta)+Q(y'\cos\theta-x'\sin\theta)=0$, which is the equation of equilibrium on the lever.

Papworth St. Everard, Nov. 16, 1832.

VI. *On the Phænomena of* Newton's *Rings when formed between two transparent Substances of different refractive Powers. By* G. B. Airy, *M.A. F.R.A.S. F.G.S. Late Fellow of Trinity College, and Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge**.

IN a paper communicated to this Society about four months since, I stated my expectation (founded on Fresnel's theory), that if a lens of a low-refracting substance were placed on a plane surface of a high-refracting substance, and if light polarized in the plane perpendicular to the plane of reflexion were incident upon it, then so long as the angle of incidence was less than the polarizing angle of the low-refracting substance, or greater than that of the high-refracting substance, Newton's rings would be seen with a black centre; but if the angle of incidence was greater than the first of these and less than the second, Newton's rings would be seen with a bright centre. I have now to announce the fulfilment of this anticipation.

Before describing the method by which I have succeeded in the examination of these phænomena, I think it right to give a theoretical calculation of the intensity of light in the rings; as without this, the necessity for some of the precautions will not be sufficiently evident.

Conceive two nearly parallel plates of different media to be separated by a plate of air whose thickness is T; and let the vibration in the plane of reflexion, of an incident stream of light within the first medium, be represented by $a \sin \frac{2\pi}{\lambda}(vt-x)$ where x is the equivalent in air to the actual distance of a particle from some fixed point, (the light being supposed polarized in a plane perpendicular to the plane of reflexion). Let ι be the angle of incidence on the last surface of the first medium; ι' the angle of refraction, which is the same as the angle of incidence on the first surface of the second medium; and ι'' the angle of refraction in the second medium. A part of the light will be reflected at the last surface of the first medium; a part will reach the first surface of the second medium, where it will be subdivided; and one portion will be reflected to the surface of the first medium, where it will be again divided; and one of its parts will enter in the same direction as that which was reflected at first. In this the phase of the undulation will be *behind* that which was first

* From the Transactions of the Cambridge Philosophical Society; before which body this paper was read, March 19. 1832, as noticed in the Philosophical Magazine and Journal of Science, vol. i. p. 400.

reflected by the quantity corresponding to the space $2\,T\cos\iota'$: or if $\frac{2\pi}{\lambda}(v\,t-x)$ be still taken as the measure of the phase of the ray first reflected, $\frac{2\pi}{\lambda}(v\,t-x)-\frac{4\pi}{\lambda}T\cos\iota'$ will be that of the ray which has been reflected at the surface of the second medium and then enters the first. The quantity $\frac{4\pi}{\lambda}T\cos\iota'$ we shall for abbreviation call V. Of the light which reaches the surface of the first medium, a part will be partially reflected at the surface of the second medium, and will partially enter the first medium: its phase will be $\frac{2\pi}{\lambda}(v\,t-x)-2\,V$; and so for succeeding reflexions.

Now suppose that at the last surface of the first medium, the coefficient of the incident vibration being 1, that of the reflected vibration is e, and that of the refracted f; at the first surface of the second medium, suppose the coefficient of the reflected vibration to be g; and for light incident from air on the surface of the first medium, suppose the coefficients of the reflected and refracted vibrations to be h and k. Then, the coefficient in the incident light being a,

That in the first reflected light is ....$a\,e$
that in the refracted light is $a\,f$
that in the light reflected at the second medium is $a\,f\,g$
and that in the light refracted into the first medium is ...$a\,f\,g\,k$
that in the light reflected from the first medium is $a\,f\,g\,h$
that in the light reflected from the second medium is $a\,f\,g^2\,h$
and that in the light refracted into the first medium is...$a\,f\,g^2\,h\,k$
and so on; the coefficients after the first following a geometrical progression whose ratio is $g\,h$. Thus it appears that the whole vibration will be $a.e.\sin\frac{2\pi}{\lambda}(v\,t-x)+a.f\,g\,k$

$$\left\{\sin\left(\frac{2\pi}{\lambda}(v\,t-x)-V\right)+g\,h.\sin\left(\frac{2\pi}{\lambda}(vt-x)-2\,V\right)+\&c.\right\},$$

or $a.e.\sin\frac{2\pi}{\lambda}(v\,t-x)+a.f\,g\,k.$

$$\frac{\sin\left(\frac{2\pi}{\lambda}(v\,t-x)-V\right)-g\,h.\sin\left(\frac{2\pi}{\lambda}(v\,t-x)\right)}{1-2\,g\,h.\cos V+g^2\,h^2}.$$

Now in Fresnel's expressions,

$$e=\frac{\tan(\iota-\iota')}{\tan(\iota+\iota')},$$

$$f = \frac{\cos \iota}{\cos \iota'}\left(1 - \frac{\tan(\iota - \iota')}{\tan(\iota + \iota')}\right),$$

$$g = \frac{\tan(\iota' - \iota'')}{\tan(\iota' + \iota'')},$$

$$h = \frac{\tan(\iota' - \iota)}{\tan(\iota' + \iota)},$$

$$k = \frac{\cos \iota'}{\cos \iota}\left(1 - \frac{\tan(\iota' - \iota)}{\tan(\iota' + \iota)}\right).$$

Hence $fk = 1 - e^2$, and $gh = -ge$; and the expression becomes

$$a e \sin \frac{2\pi}{\lambda}(vt - x) + a g (1 - e^2).$$

$$\frac{\sin\left(\frac{2\pi}{\lambda}(vt - x) - V\right) + ge \,.\, \sin\left(\frac{2\pi}{\lambda}(vt - x)\right)}{1 + 2ge\cos V + g^2 e^2}.$$

Resolving this into the form

$$P \sin \frac{2\pi}{\lambda}(vt - x) + Q \cos \frac{2\pi}{\lambda}(vt - x),$$

the intensity or $P^2 + Q^2$ becomes

$$a^2 \frac{g^2 + e^2 + 2ge\cos V}{1 + 2ge\cos V + g^2 e^2}.$$

The maxima and minima of this correspond to the maxima and minima, or the contrary, of $\cos V$. When $V = 0$, 2π, &c. that is when $T = 0$, or $= \frac{\lambda}{2\cos \iota'}$, or $= \frac{2\lambda}{2\cos \iota'}$, &c. the intensity of the reflected light is

$$a^2 \left(\frac{g + e}{1 + ge}\right)^2,$$

and when $T = \frac{\lambda}{4\cos \iota'}$, $\frac{3\lambda}{4\cos \iota'}$, &c. the intensity is

$$a^2 \left(\frac{g - e}{1 - ge}\right)^2,$$

and the excess of the latter above the former is

$$-a^2 \,.\, \frac{4eg(1 - e^2)(1 - g^2)}{(1 - e^2 g^2)^2}.$$

This is the difference of intensity of the brightest and of the darkest parts of the rings: and when it is positive, the centre of the rings is dark.

Now $\tan^2(\iota + \iota')$ is always greater than $\tan^2(\iota - \iota')$, and

$\tan^2(\iota' + \iota'')$ is always greater than $\tan^2(\iota' - \iota'')$: so that $(1 - e^2).(1 - g^2)$ is always positive. Consequently the central spot is black when e and g have different signs, and bright when they have the same sign. Or as $\tan(\iota - \iota')$ is always negative, and $\tan(\iota' - \iota'')$ always positive, the central spot is black when $\tan(\iota + \iota')$ and $\tan(\iota' + \iota'')$ have the same sign, and bright when they have different signs: that is, it is dark when $\iota + \iota'$ and $\iota' + \iota''$ are both less or both greater than 90°, and bright when $\iota + \iota'$ is less than 90° and $\iota' + \iota''$ greater than 90° (or vice versâ). From this it follows that while the angle of incidence is less than the polarizing angle of the first medium, the central spot is black: at that polarizing angle the rings disappear (as $e = 0$): from that angle to the polarizing angle of the second medium the central spot is bright: at the polarizing angle of the second medium the rings disappear (as $g = 0$); and beyond that, the central spot is again dark.

Now let us estimate the intensity of the light at the central spot when the first ring is black (the angle of incidence being between the two angles of polarization). If the first ring is black we have $\frac{g-e}{1-ge} = 0$, whence $g = e$: and the intensity in the central spot becomes $a^2.\left(\frac{2e}{1+e^2}\right)^2$. The condition $g = e$ gives

$$\frac{\tan(\iota' - \iota'')}{\tan(\iota' + \iota'')} = \frac{\tan(\iota - \iota')}{\tan(\iota + \iota')},$$

whence $\sin^2 2\iota' = \sin 2\iota . \sin 2\iota''$:

$$\text{or } \cos^2 \iota' = \frac{1}{m\,m'} \cos \iota . \cos \iota''.$$

where m and m' are the refractive indices of the two media. Without attempting to solve this equation generally, suppose $m = 1{\cdot}53$ and $m' = 2{\cdot}45$ (which correspond nearly to plate glass and diamond). The values of ι' at the polarizing angles are 56° 49′ 54″ and 67° 47′ 48″; and the value of ι' which makes the first ring black is 63° 19′ 4″; the values of ι and ι'' corresponding to this are 35° 43′ 57″ and 21° 23′ 21″: whence $e = g = 0{,}083215$; and the intensity of the light at the central spot $= a^2 \times 0{\cdot}02732$.

But to obtain a practical idea of the import of this expression we must compare it with the intensity of light in the rings in some other position. Now when the incidence is perpendicular, the expressions above give for the difference of the light in the dark spot and bright rings, $a^2 \times 0{\cdot}28159$. Consequently the intensity of light in the rings seen between the two polarizing angles is less than one tenth of that in the rings seen at a nearly perpendicular incidence. As the latter are by no means vivid, we must expect the former to be faint.

The intensity of the rings which would be produced at the same angle of incidence by light polarized in the plane of reflexion, found in the same way, $\left(\text{putting } e' = \frac{\sin(\iota - \iota')}{\sin(\iota + \iota')} \text{ and } g' = \frac{\sin(\iota' - \iota'')}{\sin(\iota' + \iota'')}\right)$ is $a^2 \times 0{\cdot}66487$; and is consequently about twenty-four times greater than that of the rings of which we are treating.

This shows that much care will be necessary to make the rings visible. Suppose for instance that the incident light is polarized by a plate of tourmaline, or (which amounts to the same thing) that the reflected light is examined by a tourmaline, with its axis perpendicular to the plane of reflexion. Few tourmalines are so perfect as to transmit no more than one twenty-fourth part of the light polarized perpendicular to their axis. If then the rings are examined with one of these, the rings of which we are in quest (whose centre is bright) will be mixed with rings produced by light polarized in the plane of reflexion (whose centre is black) of at least equal intensity: and their character will therefore be entirely destroyed. If instead of a tourmaline we use a doubly-refracting prism, with which both sets of rings are exhibited, separated from each other, there will be no fear of confusion of the rings, but a sheet of bright light (from the rays polarized in the plane of reflexion) will be spread over the faint rings that we are seeking, and will effectually make them invisible.

The plan which I have successfully adopted is, to combine a tourmaline and doubly-refracting prism. By means of the tourmaline (with axis perpendicular to the plane of reflexion) the brightness of the sheet of light, which would otherwise cover the rings that we have to examine, is so far diminished, that it offers no serious obstacle. At the same time the other set of rings is seen, and serves very well as an object of comparison.

To destroy the reflexion at the upper surface of the imposed lens is a matter of importance. I have used a plano-convex lens of 5·8 inches focal length with an obtuse-angled prism placed upon its plane side, the obtuse angle being over the centre of the lens. A drop of water was placed between them. Though its refractive index differs sensibly from that of the glass, yet the reflexion at the common surface of the prism and lens is almost totally destroyed, for the following reason. The surface of the lens is I suppose very slightly convex, and when the drop of water is interposed, and the air-bubbles are rubbed out, Newton's rings are seen, very large though slightly irregular, with the black spot in the centre. The rings in question are seen through this black spot, and consequently are not injured by the effects of reflexion. The water seems to have the power of bringing the lens and prism into closer contact than is otherwise attainable*: for I am well convinced that no force that could be applied without injuring them would bring them so near together as to exhibit the central black.

For the denser medium I have used a diamond with a surface of about $\frac{1}{12}$ inch in diameter, mounted in a ring: for the use of which I am indebted to the politeness of William John Broderip, Esq. Vice-President of the Geological Society. When the lens and prism were placed on this, a small system of rings was seen perfectly distinct and well formed, the diameter of the fifth ring not exceeding $\frac{1}{2}$ of the diameter of the surface.

These rings were examined with the combination of tourmaline and doubly-refracting prism that I have described. When the angle of incidence was small, the rings formed by light polarized perpendicular to the plane of reflexion were seen sufficiently vivid, with black centre, accompanied by the other set of rings which were faint. When the angle of incidence reached the polarizing angle of the glass, the first set of rings disappeared. On increasing the angle, the first set of rings was again seen with centre white. In the most favourable state, the first set of rings was much more faint than the second, but not so faint that there could be the slightest doubt upon the fact of the existence of the rings and the whiteness of the centre, as I saw them repeatedly with every change in the arrangement of the apparatus, and saw a succession of several rings. The white spot appeared larger than the dark spot in the other set of rings, but this I imagine is owing merely to the undefined nature of the spots, and to the circumstance that, in appreciating their comparative extent, the eye always gives credit to the brightness for a greater surface than it can properly claim. In respect of dimensions of corresponding parts, I could see no difference. On increasing the angle

* I may here mention a curious circumstance which occurred to me in the use of this combination. After leaving the prism, with the lens hanging to its lower surface, for one or two days, the water contracted itself to a spot (having partly gone off, I suppose, by evaporation) of about $\frac{3}{4}$ inch in diameter, its outline following most accurately the course of one of the rings (I think the third) even in its deviations from symmetry. In this state I was not able to move the lens upon the prism, though I applied a force parallel to the surface of the prism sufficiently great to shiver large splinters from the lens. On dipping them into water they instantly dropped asunder.

of incidence, the first set of rings again disappeared, and reappeared in great brilliancy, the centre being now black.

I am willing to think these experiments important, because they bear immediately upon a part of Fresnel's theory which has always appeared to me most liable to objection, namely, the formulæ for the extent of vibration in reflected and refracted rays. On the truth of Fresnel's general theory as a mere geometrical representation, namely, that light consists of transversal vibrations, and that polarized light is light in which all the vibrations are perpendicular to the plane of polarization, I shall say nothing, because I do not think it will be doubted by any one who is well acquainted with the experiments and has examined their agreement with calculation. But on the theorems for intensity in reflected rays, &c. involving points of the greatest obscurity, and supported only by very forced suppositions, any one may I think with reason be sceptical. The phænomena described here and those described in a former paper (On a remarkable Modification, &c.) depend entirely, in theory, upon the changes of sign of certain quantities which enter into Fresnel's expressions for these intensities. With respect to the absolute measure of the intensities I can say nothing, except that the general appearance of the brightness is sufficiently in accordance with the law. On the whole I think that these experiments give great probability to the truth of the formula considered as a general law: and that they establish with certainty that part of it which implies that, after passing a certain angle, the direction of the vibration in the reflected ray (considered with respect to that in the incident ray) is reversed.

Observatory, Feb. 4, 1832. G. B. Airy.

Postscript.—Since the above account was written I have (with a favourable sky) seen the white-centred rings many times, and several times with a doubly-refracting prism only, unassisted by a tourmaline. In examining one part of the phænomena, I find that there is a discordance of a most curious kind from what the strict theory had led me to expect.

When the light is incident at the polarizing angle of the glass, the rings, so far as I can see, vanish totally. Though I have looked several times with the most scrutinizing attention, I have not been able to see the least trace. If the angle of incidence is gradually increased till it exceeds the polarizing angle, the black-centred rings disappear gradually without altering their size (a considerable quantity of light being still reflected from the diamond) and white-centred rings of the same size appear in their place, without any intermediate stage except a total absence of rings. From the agreement of this with theory I conclude that the polarization of light at the inner surface of glass is (to the senses) complete. But at the polarizing angle of the diamond the case is perfectly different. On increasing the angle of incidence till it exceeds this angle, the white-centred rings do not disappear, but the first black ring contracts so as to leave no central white, and becomes itself the black centre. After this there is no material change: I find, however, that the black centre of the rings produced by light polarized perpendicular to the plane of reflexion is always (beyond tbe polarizing angle of the diamond) sensibly larger than the black centre of the rings produced by light polarized in the plane of reflexion.

The nature of this transition from rings of one character to rings of the opposite character appears to me to be, theoretically, extremely curious. As the rings do not disappear, it is plain that if light polarized perpendicular to the plane of incidence (or whose vibrations are entirely in that plane) is incident at what is called the maximum polarizing angle of the diamond, a portion of it is still reflected. Still, however, on increasing the angle of incidence the character of the rings is changed: and this takes place at an angle where (so far as we are entitled to conclude) there is nothing peculiar in the reflexion from the glass; and we are therefore compelled to admit, that the incident vibration being $a \,.\, \sin \frac{2\pi}{\lambda}(vt - x)$, when the angle of incidence is increased so as to exceed that angle, the reflected vibration is changed from $+ p \,.\, \sin \frac{2\pi}{\lambda} (vt - x)$ to $- q \,.\, \sin \frac{2\pi}{\lambda}(vt - x)$. A similar change takes place at the polarizing angle of the glass: but there, as we have seen, the transition from $+p$ to $-q$ is effected by passing through 0, or by the entire cessation of reflexion at one angle of incidence; which is not the case at the polarizing angle of the diamond. How then is the gradual change from $+ p \sin \frac{2\pi}{\lambda}(vt - x)$ to $- q \,.\, \sin \frac{2\pi}{\lambda}(vt - x)$ to be explained? I answer that the phænomena prove that it follows from a *gradual change of phase*, while the coefficient is not much altered. In other words (neglecting the trifling alteration in the coefficient) the quantity $+ p \sin \frac{2\pi}{\lambda}(vt - x)$ is changed to $- p \sin \frac{2\pi}{\lambda}(vt - x)$, not by the disappearance of p, but by the expression assuming the form $p \sin\left\{\frac{2\pi}{\lambda}(vt - x) - \theta\right\}$, where θ increases from 0 to π. This may be popularly explained in the following manner. The common Newton's rings, formed between two lenses, are produced by the interference of the light reflected from the lower surface of the upper lens with that reflected from the upper surface of the lower lens. Now if the upper lens be raised a little, or the lower depressed a little, the rings contract. As the only immediate effect of depressing the lower lens is to cause the light reflected from it to describe a longer path, or to have its phases retarded, it appears that a contraction of the rings may be considered as the effect of a retardation in the phase of the light reflected from the lower surface. The contraction of the rings then in passing the polarizing angle of the diamond requires us to admit that the phase of the reflected light (the incident light being polarized perpendicular to the plane of the reflexion) is, on increasing the angle of incidence by a few degrees, retarded nearly 180°.

The retardation, however, is not quite 180°. For if it were, the character of the rings would be exactly changed, so that the proportion of the size of the central black spot to that of the first white ring would be the same as that of the central white spot (before the change) to the first black ring. But as the central black spot formed by rays polarized perpendicular to the plane of reflexion is distinctly larger than that formed by rays polarized in the plane of reflexion, it seems that the black ring has not contracted completely, or that the alteration of phase is not quite 180°. This reasoning it must be confessed is not certain, as the same thing would be explained by supposing a small alteration of phase in the light polarized in the plane of reflexion. I may mention here, that in the Newton's rings formed between two lenses of the same kind of glass, the central black spot in those formed by light polarized perpendicular to the plane of reflexion is larger than in those formed by the light polarized in the plane of reflexion.

If, while the white-centred rings are under examination, the tourmaline and doubly-refracting prism are turned round, the rings become faint, but do not disappear, and are changed into black-centred rings by the contraction of the rings. This is exactly similar to what takes place when a lens is placed on a metallic surface, and it proves that (as in the former paper), while the angle of incidence is a few degrees less than the maximum polarizing angle of the diamond, the phase of light polarized perpendicular to the plane of reflexion is more retarded than the phase of light polarized in that plane.

I have not found any variation in these results from changing the position of the plane of reflexion on the diamond surface.

The result of these experiments and reasonings may be thus stated.

1. When the angle of incidence is less than the maximum polarizing angle of the diamond, the nature of its reflexion is similar to that of metallic reflexion: the phase of vibrations in the plane of reflexion being more retarded than that of vibrations perpendicular to the plane of reflexion, but perhaps by a smaller quantity than in reflexion from metals.

2. In the neighbourhood of the polarizing angle, the nature of the reflexion is different from any that has hitherto been described. The vibrations in the plane of reflexion do not vanish, but on increasing the angle of incidence by three or four degrees the phase of vibration is gradually retarded by nearly 180°. In the reflexion of light whose vibrations are perpendicular to the plane of reflexion, there is no striking difference between the effects of diamond and those of glass.

3. For angles of incidence greater than the polarizing angle, there is no sensible difference between the effects of diamond and those of glass.

I may remark that the extent of vibration in the plane of reflexion may be represented thus (the formula being purely empirical and given only for illustration). The vibration in the incident light being $a \sin \frac{2\pi}{\lambda}(vt - x)$, that in the reflected light is

$$\frac{\tan(\iota' - \iota'')}{\tan(\iota' + \iota'')} a \sin \frac{2\pi}{\lambda}(vt - x) - b\, a \cos \frac{2\pi}{\lambda}(vt - x),$$

where b is always small but never $= 0$, and is perhaps constant.

The conclusions at which I have arrived are at variance with one of Sir David Brewster's (Phil. Trans. 1815). Sir David Brewster's character as an experimental philosopher stands deservedly so high, and my estimation of his accuracy (as observed by myself in the repetition of many of his experiments) is so great, that I think it necessary to point out distinctly the nature of this disagreement.

Sir David Brewster states that homogeneous light is completely polarized by the diamond at the proper angle. I have made no experiments here with homogeneous light, and I know that, on account of its extreme faintness, however obtained, little confidence can be placed in results which depend only on the evanescence of the reflected light. But the phænomena observed by me are entirely inconsistent with this supposition. If homogeneous light were used, then (on this supposition) the bright-centred rings would disappear and black-centred rings would succeed them as at the polarizing angle of the glass. If white light were used, the rings in the neighbourhood of the polarizing angle would be wholly coloured, and on changing the angle the intensity of the different colours in each ring would alter, but there would be nothing like contraction. Thus at a certain angle the brightest part of the red would be at the centre of the spot, and its faintest part would be in the first ring; while for the blue the places would be reversed: on increasing the angle the brightest parts of both would be in the first ring. Whereas in my experiments there was no discoverable alteration in the colours of the rings, there never was seen a bright red centre surrounded by a bright blue ring; but the rings, without changing their character as to colour, diminished steadily till the central spot was as it were squeezed out. Whether the only diamond which I have used may possess any peculiarity which distinguishes it from those used by Sir David Brewster, I cannot say. Meantime I may observe, that the singularity in the reflexion at the surface of the diamond makes it not improbable that there may be some singularity in the refraction also, and renders a more extended inquiry into the laws both of its reflexion and of its refraction highly desirable.

Observatory, Feb. 16, 1832. G. B. Airy.

VII. *On certain Defects in the British Almanac.* By B. Bevan, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IS it not worthy of remark, that an Almanac published under the patronage of so learned a Society as that established for the Diffusion of Useful Knowledge, should continue to be published without giving the *sun's declination*? It cannot surely be owing to a want of room, when two pages per month are appropriated to the calendar.

The patrons of this publication must be aware of the importance of this information to all persons who may wish to become acquainted with practical astronomy. The declination of the sun is independent of the latitude of the place, and therefore will serve for all the British dominions; whereas the table of "sunrise" and "sunset" can be true only for the particular latitude of the place for which it is calculated, and can be of little, if any practical use;—at least, a general table for any year in the miscellaneous matter after the calendar would be quite as useful as the two columns now occupying part of each monthly space: for many reasons this supplementary table would be better, as it might be given for different latitudes, instead of being confined to one latitude. Is it probable that one person in ten thousand of all the inhabitants of London ever observe the rising sun? or would without considerable inconvenience be able so to do if inclined?

Every astronomer knows that the time of the sun's rising is influenced by many circumstances; such as the elevation of the horizon, the density and temperature of the atmosphere, and upon the latitude of the place. It might be asked, what part of the sun's disc is to be the index of its rising or setting? In a popular view, the first and last appearance of the sun would be considered the time to be observed; whereas I presume it is intended by the tables to give the time of the centre passing the sensible horizon, which differs considerably from the first and last appearance. If any person should rely upon setting their watch or clock to true time by the use of the tables published in the British Almanac, they would be deceived; whereas if they gave, like other almanacs, the sun's declination for each day, any person with a little knowledge of astronomy would be able to obtain his time by an observed altitude of the sun, at any time of the day.

If room is wanted, it might be obtained by removing much of the present contents of the first page of the month to the Supplement.

The places of the planets might also be given in a more useful form than in the vague and general manner now done at the head of the first page.

The constellations in which the planets are to be found, occupy a portion of each monthly department. We might inquire the use of this vague information. Will any of the readers of the British Almanac be able to see either Mercury or Uranus from such a notice? Whereas if the right ascension and declination of the planets were given four or five times in the month, any person might be able to find them at proper seasons.

Want of room cannot properly be urged as a reason for the omission of useful matter; as many things are given in the calendar part of the almanac which would be quite as well in the Supplement, or in the Companion.

I would not be understood to assert that the planet Mercury cannot be seen with the naked eye, although it is an object seldom seen, in consequence of its proximity to the sun; yet there are generally a few days in the year when this planet may be easily seen; and it would be no discredit to the British Almanac to point out those days, and the position in which it will be visible.

Dec. 14, 1832.

VIII. *On the Theory of Magnetic Electricity.* By Mr. William Sturgeon, *Member of the British Association for the Promotion of Science: Lecturer at the Hon. East India Company's Military Academy, Addiscombe, &c. &c.* *

[With Figures: Plate I.]

THE original plan which I had prescribed to myself for the publication of my investigations on the distribution of magnetic polarity in metallic bodies, was that of first describing all those experiments which appeared to me to be the most interesting, with such explanatory remarks and practical rules for their exhibition as were necessary to their being properly and easily understood; and afterwards to offer such theoretical inferences, with observations, as naturally presented themselves to my mind whilst contemplating the curious and novel phænomena which these inquiries elicited: and in order that the arrangement might be the more regular, uniform, and intelligible, I placed the experiments on iron in the earliest part of the detail. According to that plan, there would have been another communication previous to that which I am now writing, which would have continued, and perhaps completed the detail of my former original experiments. Since sending my last communication to the press†, however, I have had an opportunity of perusing a paper containing the detail of the more recent experiments of Mr. Faraday, published in the Philosophical Transactions for the present year; and finding that several of the experiments there detailed, although performed with somewhat different arrangements of apparatus, are intimately connected with those of mine already published, and consequently with those also which I have not yet described, I have been induced to deviate from my original plan, and to offer more early in the series than I had intended, those *theoretical elements* of this new branch of physics, of which all the *rules* hitherto advanced for the exhibition of the phænomena, however important they may appear in a practical point of view, are but the mere consequent subordinate results.

Before proceeding further, however, with the principal object of this communication, I must beg permission to observe, that notwithstanding the *title* under which I have hitherto published my investigations on this subject is perfectly unobjectionable, and also sufficiently comprehensive and explanatory for all

* Communicated by the Author.

† Phil. Mag. and Journ. of Science, vol. i. p. 31.

the phænomena exhibited by the deflections of the magnetic needle; the more recent discoveries of the electric spark, and other electrical phænomena by the same mode of excitation, (which have completely verified my anticipations as to the real character of the excited force which operates on the needle,) require to be arranged under another, and a very different head. *Magnetic Electricity* is an appellation which comprehends, and may very conveniently serve to express *generally*, every class of phænomena hitherto developed by magnetic excitation of the electric matter, whatever may be the character or form of the metal employed. It will therefore be more consistent with simplicity to confer on the whole that general appellation; and to designate, if necessary, each individual class of phænomena by its respective characteristic properties. Precedents of this kind, distinguishing various classes of phænomena, are abundant in scientific nomenclature, and cannot in this instance be reasonably objected to.

Considering therefore that *Magnetic Electricity* is an appellation at once emphatic, intelligible, and expressive of the exciting agent, I have been induced to publish my *theoretical* views of this subject under that general head. Moreover, it so happens that the laws of this species of electric excitation are not peculiar to the display of one class of phænomena only, but are applicable to the development of every fact hitherto discovered in this branch of physics. It does not therefore require that one mode of excitation should be observed for the production of the electric, and another mode for the production of the magnetic effects; but merely a diversity in the arrangements of the apparatus: for whatever be the character of the phænomena to be exhibited, the same laws of excitation are uniformly to be observed,—a circumstance which affords another and very powerful argument in favour of the adoption of the general significant appellation *Magnetic Electricity.*

Researches in magnetic electricity have hitherto been confined to the disturbing of the natural equilibrium of the electric fluid residing in metallic bodies, and perhaps other conductors of electricity, by means of certain movements of those bodies, with regard to natural or artificial magnets; and some very curious facts have been discovered by these modes of experimenting.

It is certainly something to discover new facts, and something more to point out rules by means of which the novel phænomena may be uniformly exhibited. It very often happens, however, that in this stage of the inquiry, the development of the most beautiful and interesting part of the science is but half accomplished. There is still something more to be done. A process of ratiocination has yet to be exercised, frequently above the sphere of the mere experimenter, which conveys the ideas far beyond the simple exhibition of phænomena. Such sublime investigations, if successful, unfold and penetrate into the more recondite recesses of nature; transport the mind to the very source from which emanate proximate and unerring fundamental laws, and display in superior radiance of philosophic light the *modus operandi* by which the dormant powers are impelled into activity, and exercise their dominion over the resulting obsequious phænomena.

I believe it is generally admitted by writers on magnetism, that a steel bar in a state of polarization is surrounded on every side by the magnetic matter, frequently called the *magnetic effluvium*, which forms to the bar a species of magnetic atmosphere. This point being granted, it will be a matter of no consequence to the present undertaking, whether this effluvial matter be stationary as regards the magnet, or whether, as some have imagined, it be continually flowing from pole to pole: it will be sufficient for the present purpose to consider it as consisting of exceedingly minute, polarized particles, emanating immediately from the surface of the steel;—concessions of no novel character, and such, I imagine, as but few will be found willing to deny.

With regard to the *distribution* of the virtual intangible magnetic particles in the vicinity of the bar, we cannot perhaps be more correctly directed for information than by examining attentively the arrangement of fine particles of iron, when gently and promiscuously scattered on paper, beneath which is placed a magnetic bar: for, notwithstanding the magnetic matter itself,—in consequence, perhaps, of the exceeding minuteness of its particles,—escapes the cognizance of vision, the distribution of the ferruginous particles being accomplished by its polarizing efficacy, may very justly be considered as the true representative of the distribution of the virtual intangible magnetic matter enveloping the surface of the steel.

Now, as those elemental magnetic intangibles are polar, their poles will necessarily be arranged according to the immutable laws exhibited by visible tangible magnets; to which they are the main-spring of all their energies, and the only active agents by which their mysterious phænomena are called forth, as displayed in the silent motions of the passive obedient steel. Regular concatenations of alternate *north* and *south* poles will, by their mutual attraction, pervade every part of the magnetic effluvium as decidedly and as uniformly as in a consecutive series of polarized ferruginous bars.

Under these considerations it will readily appear, that all the elemental magnetic particles enveloping the *north portion* of a regularly magnetized bar of steel will have their *south poles* directed towards the surface of the metal, and consequently all their *north poles* will be directed outwards in every part of the arrangement. Precisely the reverse of this distribution of poles will take place in the magnetic matter enveloping the *south portion* of the steel; so that in this case the *north poles* will be directed towards the south portion of the metal, and consequently all the *south poles* will be turned outwards.

If now we contemplate the arrangement which would take place in the vicinity of *one* polar portion only, of a piece of steel, supposing it to be uninfluenced by a pole of the other kind, we shall discover, by the laws of magnetics, that the polar affections of the enveloping magnetic matter will arrange the particles of which it is composed, into radial *polar lines*, emanating from every part of the steel surface; for, as each individual line will be formed by the attachment of a consecutive series of dissimilar poles of elementary particles, the remote extremities of all these virtual *magnetic lines* will become similarly polarized; in consequence of which, they will have a constant tendency to diverge from each other. Hence if we be contemplating the *north* polar portion of the steel, the remote extremities of the virtual *magnetic lines* will be *north polar*; but if it be the *south* portion of the steel which comes under consideration, the remote extremities of the *magnetic lines* will be *south polar*. Hence also, the lines of magnetic action which envelop a bar of steel displaying two poles only, may be divided into two distinct classes or systems; one of which may be called *north polar*, and the other *south polar*. If it were possible that either of these systems of *magnetic lines* could be displayed separately and independently of the disturbing force of the other system, those lines would be perfectly straight, or without flexure in every part of their course; that is, they are naturally *right lines*; and if the magnetized body were a sphere, the virtual *polar magnetic lines* would radiate in right lines from every part of its surface. (See fig. 1. and 2. Plate I.)

Hitherto I have endeavoured to explain what I consider radiating *magnetic polar lines*, emanating without obstruction, from a magnetized piece of steel, under the supposition of its being unipolar on every part of its surface: but as no piece of steel, of whatever form it may be made, has yet been known to exhibit one uniform polar state, but on the contrary, each piece of magnetized steel invariably displays a plurality of poles, and one, at least, of each description,—it will next be necessary to take into consideration in what manner the two systems of polar matter affect each other, and in what manner the elementary polar lines of each system become deflected out of their natural rectilinear course by their mutual attraction of each other.

If fine steel or iron filings be gently scattered on a sheet of card paper, under which is placed a bar magnet, they will immediately become polarized by the influence of the magnetic matter enveloping the bar; and if they be slightly agitated by hitting the paper a few gentle taps with a pencil or other such light body, they will become arranged in multitudes of exceedingly fine lines, some of which will be straight, and others curved, as in fig. 3. Conspicuous lines, each with a dash across one of its extremities, are drawn, to show their general positions in each system.

In this arrangement of the ferruginous particles we have perhaps a pretty correct picture of a longitudinal section of the distribution and arrangement of the intangible magnetic matter enveloping the steel bar. Near to, and around the extremities of the bar, the two systems of *polar lines* proceed in their natural rectilinear direction; but those *polar lines* of each system which are more vicinal to the neutral point, or to the neutral line *e q*, which crosses the centre of the bar, in consequence of presenting poles of different characters outwards, do, by their mutual attractions, aberrate from their natural course, and bend or incline towards each other; forming curves of different degrees of flexure, according to the powers of their reciprocal forces, and their distances from each other. If the steel bar be cylindrical, and uniformly magnetized on every side, then, whatever longitudinal line of this magnet be turned upwards, or towards the paper, a similar arrangement of *polar lines* will be exhibited, demonstrating in the most satisfactory manner that the virtual polarizing magnetic matter completely envelops the ferruginous cylinder. Fig. 1. and 2. represent the distribution of fine particles of iron when strewed on paper above the ends of the cylindrical magnet.

Fig. 4. is a representation of the arrangement of fine particles of iron, strewed on paper above a horse-shoe magnet, which affords a tolerably exact idea of the direction of the invisible *polar magnetic lines* as they are distributed in the plane of the magnet. Fig. 5. represents the arrangement and distribution of iron filings, scattered over a transverse section, or over the poles of the same magnet.

In fig. 4. it is observable that the *magnetic polar lines* exhibit the greatest degree of aberration from their natural rectilinear direction, in front of the metallic poles; whilst but very trifling deflections of the *polar lines* are to be seen, along the edges of the magnet: even on the outside of the limbs the aberrations are much less than those exhibited on the surface of the bar magnet fig. 3. at similar distances from the poles.

In this case the *magnetic polar lines* maintain their natural rectilinear direction, even at considerable distances from the extremities of the metal, and particularly between the limbs of the magnet; in consequence of the two systems emanating from the metallic surface in diametrically opposite directions, and meeting each other, as it were, in the same rectilinear path. On the outside of the limbs, the aggregate of the two systems of polar lines, in the plane of the magnet, are not only so far separated from each other as to be little affected by their mutual attraction, but are also so situated with regard to the transverse curvilinear forces (see fig. 5.), that they form a series of resultant right lines in the plane of the magnet. These lines have, however, a small degree of flexure from their natural course, arising from their mutual attractions in the direction of the metal, which bend them a little towards the centre of the magnet.

Having thus illustrated what I consider to be the virtual *polar magnetic lines*, and also their most usual arrangements in the vicinity of steel, or other ferruginous magnets, I now propose to show that the *excitation* of magnetic electricity, and also the *direction* of the currents excited, are referable to the *agency* and *position* of these *polar magnetic lines* alone; without any regard whatever to the poles, figure, or position of the steel which they envelop; any further than as those lines are casually arranged on its surface, by the diverse arbitrary forms and proportions it is so frequently made to assume.

[To be continued.]

XI. *Notes on the History of English Geology.* By William Henry Fitton, *M.D. F.R.S. &c**.

[Concluded from vol. i. p. 450.]

[With a Plate.]

THE geological publications of Mr. Smith are now so well known, and the progress of the author's researches has been sketched with so much truth and spirit by Mr. Sedgwick, in one of his addresses from the chair of the Geological So-

* [The whole of the subsequent pages, as well as some passages in the preceding parts of this paper, which are between brackets, are recent additions to the original as reprinted in 1821.]

ciety *, that the following pages will be little more than a statement of dates and circumstances connected with that diffusion of his views, which is known to have had great effect in the advancement of the subject in this country, before the appearance of his geological map in 1815: and such a statement the writer of these pages is enabled to give upon the best authority, through the kindness of Mr. Phillips of the York Institution, who has entrusted to him several original maps and other papers, of very early date, prepared chiefly by Mr. Smith himself at various times. The collection however, unfortunately, is but a part of what it was originally; several of the documents having been lost on Mr. Smith's removal from his residence in London in 1819.

The best mode of introducing the information contained in these papers, will be to prefix a brief notice of Mr. Smith's progress, drawn up in very unpretending language by himself, about the year 1804.

" In 1787, at the age of eighteen, I became an assistant of ' Mr. Edward Webb, land-surveyor, and was employed in the ' survey of estates and the inclosure of extensive commons and ' open fields in the counties of Oxford, Warwick, Worcester, ' Gloucester, Wilts, Hants, and Somerset; which embraced ' all the strata, from the red-marl at Inkborough, and Rugby, ' near Alcester, to the sand and gravel over chalk at Dibden, ' between the New Forest and Southampton.

" In 1789 I first saw the red-marl at Inkborough, and ' made many inquiries respecting it and the lias, and its clays ' contiguous. The latter were further noticed on setting out ' the allotments of inclosure at Great Kinton, Warwickshire, ' and also the red-marl on the road to Warwick. On the lat- ' ter site of lias, there had recently been an experiment for coal.

" In 1790 I particularly noticed a boring for coal on the ' very different soils of the New Forest. All the varieties of ' soil in so many surveys were particularly entered; and, from ' still more juvenile habits, some of the organized fossils, as the ' *anomia*, and *quoitstone*, or flat echinus of the under oolite: ' and employed in the fields, I observed no stone in those parts ' would set an edge to a knife. The chalk, with which I wrote ' and drew rude figures, and the black flints used in striking ' fire with steel, I then also learnt came by the drivers of stage- ' waggons from Stokenchurch Hills; which chalk hills I pass- ' ed in my way to and from London, when between twelve and ' fourteen years of age. The surface of the country from ' London to Bath, and from Warwick to Southampton, being ' familiar to me before I settled in Somersetshire, I was struck

* See Phil. Mag. and Annals, N.S. vol. ix. p. 272.

'with the coincidence of certain parts thereof, and the similar 'nature of its soil and rocks; and particularly with the re-'gular beds of lias-limestone in the quarries between Bath 'and Stowey, an estate of Lady Jones, which I went to sur-'vey; and was surprised to find the red-marl of that place 'and High Littleton,—so evidently the same as that of War-'wickshire, not similarly used for marling land. Coal was 'worked at High Littleton beneath the red earth. I was de-'sired to investigate the collieries, and state the particulars to 'my employer. My subterraneous survey of these coal veins, 'with sections which I drew of the strata sunk through in the 'pit, confirmed my notions of some regularity in the matter 'of the hills above the red earth, which they were in the ha-'bit of sinking through;—but on this I began to think for 'myself.

"My observations on the superposition and continuity of 'the strata were greatly extended in 1792; and in the following 'year, by taking levels for the proposed Somersetshire canal, 'I proved the red-marl, lias, blue-marl, and inferior oolite on 'the tops of the highest hills to be generally inclined towards 'the east: and this notion of a general declination appeared 'to hold through all the varieties of strata in the considerable 'extent of country before noticed; and other levels down two 'parallel valleys in the same strata seemed further to confirm 'the general notion. It then became a consideration how I 'could best represent this order of superposition,—continuity 'in the course, and general eastern declination, of the strata 'successively terminating at the surface of the earth.

"After attending the Somersetshire Coal Bill in Parliament, 'I was appointed, with two gentlemen of the Committee, to 'go on a tour of observation on canals. Both these gentle-'men were coal-owners, and workers thereof, which induced 'them, as curiosity did me, to keep separate memoranda of all 'the collieries in our route: but my more eager object was the 'verification of the preconceived order of superposition con-'tinuity and declination of the strata. I had generally the 'look-out seat in the chaise; and on a journey of upwards of '900 miles, commenced in August, and extended to New-'castle-upon-Tyne by one route, and back by another, I re-'turned to Bath the end of September 1794, without com-'municating to my fellow travellers any of my numerous ob-'servations, which confirmed the general principles before 'entertained.

"For six years I was the resident engineer on the Somerset-'shire coal canal, which put my notions of coal stratification 'to the test of excavation; and I generally pointed out to con-

'tractors and others, who came to undertake the work, what 'the various parts of the canal would be dug through. But 'the great similarity in the rocks of oolite, on and near the end 'of the canal toward Bath, required more than superficial ob-'servation,—to determine whether those hills were not com-'posed of one, two, or even three, of those rocks, as by the di-'stinctions of some parts seemed to appear. These doubts 'were at length removed by more particular attention to the 'site of the organized fossils, which I had long collected. 'This discovery of a mode of identifying the strata by the 'organized fossils respectively imbedded therein,—the sharp-'ness of those in their primitive sites, contrasted with the 'same fossils rounded and water-worn in gravel, led to the 'most important distinctions; which at once seemed to clear 'away the rubbish and common stumbling-blocks in geology.

"Thus stored with ideas, which I knew not how to make 'publicly or privately useful, on being introduced to Dr. An-'derson, then at Bath, and to the Rev. Benjamin Richardson, 'and the late Rev. Joseph Townsend, I was induced by the 'late Mr. Davis to make known to them some of my dis-'coveries. Dr. Anderson pressed me for a map and some ge-'neral account of the stratification of England, to publish in his 'Recreations on Agriculture, and sent me the first and second 'parts of that work: but getting into a variety of business, in 'draining land, &c. in remote parts of England and Wales, 'my correspondence with the Doctor ceased, without my com-'plying with his wishes.—I became intimately acquainted with 'the two other gentlemen, and opened my mind fully to them; 'and at the Rev. J. Townsend's house drew up the first tabular 'account of the order of strata, with the organized fossils by 'which they are respectively identified, in all the hills around 'Bath. From hence the information spread like a circle upon 'water; for my two sanguine friends thought remuneration for 'my discoveries was sure to follow the publicity of information 'so useful and important. I found, however, I had still to 'work my way, against that stream of difficulties which must 'ever attend the pursuit of such objects by a man like me, 'who had not property sufficient to publish it.

"The utility of such discoveries in draining and otherwise 'improving land, induced some gentlemen of the neighbour-'hood to put them to the test. This new occupation threw me 'in the way of T. W. Coke, Esq., who, during his stay at Bath, 'visited the agricultural improvements on Thomas Crook's 'estate at Tytherton, where I was employed. From hence I 'went into Norfolk; and thence soon after to Woburn, where I conceived the complete drainage of Prisley Bog, the site of

'Mr. Elkington's unsuccessful experiments, and the further 'converting much of it into excellent water meadow, would 'have insured me the Duke of Bedford's patronage. But here 'my hopes of remuneration vanished with the public loss of 'that great man.

"Business of all sorts connected with the stratification of the 'country pressed upon me; and some of my friends thought 'the retaining of such information would insure me more 'advantages in my profession than I should derive from the 'publication thereof; and the late Duke of Bedford himself, in 'a long interview with him on the subject but a fortnight be- 'fore his death, said, "the publication would be better deferred "a few years, as I should have the more opportunity of per- "fecting my system, and the public mind would be better pre- "pared to receive it." Mr. Farey was then the Duke's agent, 'and was anxious to become acquainted with the subject of 'strata; which, at the Woburn, Holkham, Smithfield, and 'Bath agricultural meetings, I scrupled not to explain very 'freely; and to elucidate by general and local maps of the 'stratification. Mr. Farey, and his friend Mr. Bevan of Leigh- 'ton-Beaudesert, who immediately after became an engineer, 'had very extensive practical lessons, at the Duke's request, 'as I was informed, in the vicinity of Woburn, the Dunstable 'chalk hills, and on other strata of the vale of Aylesbury,— 'confirmed by a collection of the organized fossils by which 'all these strata are respectively identified.

"Thus before 1803, I had fully taught, in the field, the 'practice of tracing all the strata, and of identifying them by 'the organized fossils, from the highest in the series over chalk 'down to the coal."

In another paper, which seems to have been intended for publication, as part of a narrative of his earlier progress, after alluding to his surveys during the years from 1787 to 1790, Mr. Smith thus states more fully the result of his proceedings at High Littleton.

"—— But the discoveries of regularity in the strata, which 'more particularly induced me to pursue the subject of geology 'to such an extent, chiefly originated in 1790 and 1791, in sur- 'veys of estates and collieries in Somersetshire, where I found 'at High Littleton the same red earth sunk through for the 'coal. The order of superposition in the coal-measures or 'strata perforated at each pit in that neighbourhood, seemed 'well known to some of the colliers; and on drawing a sec- 'tion thereof, with nine veins of coal, I was naturally led to ask, "—Whether the superincumbent strata, rising into hills from "200 to 300 feet above the mouths of their coal-pits, were not

" also regular?—I was constantly told—there was "nothing re-" gular above the red ground," which in their sinkings varied ' much in thickness; nor could they tell which way the coal ' would pitch, until the red earth was sunk through. This did ' not deter me from pursuing my own thoughts on the subject; ' and in 1792 and 1793, the general declination of the superior ' strata to the east or south-east was verified, by a survey and ' levels continued many miles through the adjoining country, ' for a canal purposed to be made in the vicinity of Bath. ' Ascertaining this fact by my spirit-level, in three parallel ' vales some miles apart,—that the lias and freestone of the ' Stone-brash Hills, which were previously well known to ' me, had such a general declination,—I soon applied these ' notions to all the extent of country before mentioned; and ' began to delineate on maps the courses of the strata; and ' constantly traced and retraced the order in which they would ' be intersected in making the canal.

" The superintendence and execution of the canal I had ' before surveyed confirmed the notions previously formed of ' the strata; and the canal excavations, and the new quarries ' opened, produced organized fossils, for the identification of ' several strata, which could not have been otherwise distin-' guished.

" These fossils were collected, written upon, and preserved ' in the order of strata, as vouchers thereof; and in June ' 1799, a written account of these discoveries, in a tabular ' form, was given to three scientific gentlemen, the Rev. Ben-' jamin Richardson, the Rev. Joseph Townsend, and William ' James, Esq., from some of whom manuscript copies were ' multiplied and extensively circulated.

" This paper, printed in the original form in the Memoir ' which accompanies the map of the strata, shows also the dis-' covery of regularity in the courses of springs; which soon be-' came an important branch of my mineral surveying. Thus ' knowing how to distinguish upon the surface the courses of ' the impervious strata;—and that the water which falls from ' the heavens is collected in the cavities of rocks and other po-' rous strata, on the subterrene surface of the impervious, and ' thus forced to run out on the soil, I considered myself qua-' lified for the business of a drainer and general improver of ' land; and in the extensive prosecution of such works, many ' of the very best local observations have been made."

It appears, therefore, that Mr. Smith's researches began among the Coal-tracts; and no better school can be imagined for instruction in the phænomena and relations of strata. Not that in many other portions of the series of secondary rocks, equal regularity may not be observed, with equal care; but simply because the commercial value of the coal is great enough to justify a large expenditure of capital in those operations of surveying and levelling, which are indispensable to the perfection of geological maps and sections. Among the documents connected with this early period of his inquiries, is a section of strata sunk through for coal, at Pucklechurch in Gloucestershire, in which a considerable depth of lias and red-marl is represented nearly in a horizontal position reposing directly upon coal-strata, which are highly inclined. The drawing is very well executed, and could not fail to suggest to Mr. Smith the important fact, that these two groups of strata are, generally, unconformable in that part of England:—but in this we have already seen that he was distinctly anticipated by Mr. Strachey.

Another document still preserved, of about this date, is a part of a coloured section, from the chalk-downs near Salisbury, to the coal-measures near Bristol. What remains of this drawing goes down to the red marl, and gives in detail, with very little room for correction even at the present day, the outcrop of the several beds, as they appear along the main road through Norton, Hinton, Broadfield, and Mitford. A single transverse section of this kind on a well chosen line, it is obvious, is sufficient to unveil the whole structure of any stratified country.

By his introduction to the Rev. Benjamin Richardson, of Farleigh near Bath, in 1799, he acquired one of his most steady and disinterested friends. When this gentleman, who had been himself a zealous geological inquirer, first showed his collection of fossils to Mr. Smith, the latter began immediately to place them in the order of the strata, to the extreme astonishment of the collector, at the new light thus suddenly thrown upon a subject which he had long and successfully studied in other points of view. By Mr. Richardson, Smith was made known to Dr. Anderson, who, forcibly struck by the novelty and importance of his discoveries, urged him to prepare an account of them for publication in a periodical work on Agriculture, and its kindred branches of knowledge, in which the latter was at that time engaged*; and with this request Smith made some preparations for complying. But the task of composition was new to him, and by no means acceptable. If De Saussure (with all the advantages

* "Recreations in Agriculture, Natural History, Arts, and Miscellaneous 'Literature: by James Anderson, LL.D." London 1799, &c.

of education and leisure) felt the imperfections of his own clear and eloquent style*, a man like Smith, engaged in laborious business, and struggling with difficulties, may well be allowed to tremble at the prospect of appearing, for the first time as an author, and on a subject upon which he felt his reputation must ultimately depend. He says himself, in one of his letters,—'Many of the better learned in the world 'might deem it the height of folly, for a man who has never so 'much as received a common grammatical education to attempt to instruct the public.' The work of composition, therefore, went on but slowly; and in reply to pressing letters from Dr. Anderson during the autumn of 1799, there is a draft of an answer from Smith apologizing for this delay, and stating that without aid and instructions he was much at a loss to arrange his papers in such order as would make them fit for publication: 'But,' he continues, 'had not business 'and a multiplicity of concerns diverted my attention more 'than usual from the pursuit of my favourite subject, you 'might before this have been in possession of such remarks 'as I shall be happy to consign to your care, for the good of 'the public; *conscious that they may at some future period be* '*found of much more value than may at first be perceived, by* '*those who have not been accustomed to view things in the same* '*light as I have done for some years past.* Yet,' he says, in a draft of another letter, 'notwithstanding all the time and 'thought I have bestowed on the subject, *and the ease with* '*which I can trace each stratum distinctly from the chalk hills* '*of this country down to the coal,*—I find it still difficult to be 'described in writing, without entering into the minutiæ of 'the subject much further than I fear would be consistent 'with your plan.'

The expected memoir never made its appearance: But the suggestion of it seems to have had the effect of inducing Mr. Smith to put his materials into somewhat better arrangement, and to hasten the preparation of his maps and papers, several of which bear date soon after this period†.

In 1799 also, he was introduced by his friend Richardson

* "Quant à mon style, je n'en ferai point l'apologie: je connois ses imperfections; mais, plus exerçé à gravir les rochers, qu'à tourner et polir 'les phrases, je ne me suis attaché qu'à rendre clairement les objets que 'j'ai vus, et les impressions que j'ai senties."—*Voyages dans les Alpes.* 4to. Discours preliminaire: tom. i. p. xx.

† A letter from Mr. Crawshay of Merthyr, in the beginning of 1804, states that Dr. Turton of Swansea was at that time ready to become the editor of Smith's works; but nothing farther appears to have been done in that direction.

to the Rev. Joseph Townsend, of Bath; at whose house the former wrote, from Smith's dictation, and at the suggestion of Townsend, that "Tabular View" of the order of the strata in the vicinity of Bath, with their respective organic remains, of which the original is now in the museum of the Geological Society. A copy of this very remarkable document is inserted in the present paper (see the TABLE in p. 46 and 47); and it is unquestionably one of the most striking examples of elaborate and successful research which the history of geology affords. It will be perceived, as Mr. Sedgwick observes*, that the successive groups from the chalk to the coal-measures inclusive, are here denoted by series of *numbers*; the author not having then decided upon those names for them which he subsequently adopted, and which still form a part of the geological nomenclature of England.

From such interviews, and from excursions with Smith himself in the neighbourhood of Bath, Mr. Townsend became fully possessed both of his principles, and of the detail of his results: and the knowledge thus acquired he published subsequently (in 1813) in a volume, already mentioned;—which, notwithstanding the incongruity of its title, and the introduction of a great deal of extraneous matter, is the best exposition of Smith's labours that has appeared. But it is to be lamented, if Mr. Townsend's purpose was to place the works of Smith effectually before the public, that he did not choose for his book a title and ostensible subject more congenial: since in its actual form, there was nothing to attract an unlearned reader desirous of obtaining geological information;—and much to repel those who were acquainted with geological history, and with the unhappy results of that alliance, which it is professedly the object of Mr. Townsend's volume to support.

At this period, 1799, Mr. Smith had coloured geologically the old county survey of Somersetshire, and a small circular map of the country around Bath; both works of great merit;—the latter especially, giving proof of extraordinary tact in detailing the minuter divisions of strata. The original of this circular map has been presented to the Geological Society, and is now in their Museum.

Mr. Richardson now justly felt, that the time was come when Smith was called upon to assert his claims to his own discoveries; and on the pressing suggestion of that excellent friend, a Prospectus was published and extensively distributed,

* Geological Soc. Proceedings, 1831; and Phil. Mag. and Annals, N.S. vol. ix. p. 276.

Order of the Strata in the Vicinity of Bath;—drawn up by Mr. WILLIAM SMITH *in* 1799.

[From the Original in the possession of the Geological Society.]—*Referred to, p. 45, line 7.*

Strata.	Thickness.	Springs.	Fossils, Petrifactions. &c, &c.	Descriptive Characters and Situations.	Names and Numbers in the Map, 1815.
1. Chalk	300	Intermitting on the Downs.	Echinites, Pyrites, Mytilites, Dentalia, funnel-shaped Corals, and Madrepores, Nautilites, Strombites, Cochliæ, Ostreæ, Serpulæ....	Strata of Silex, imbedded.	1. *Chalk* (5).
2. Sand	70	......	....	The fertile vales intersecting Salisbury Plain and the Downs.	2. *Greensand* (6). *Firestone.*
3. Clay	30	Between the Black Dog and Berkley.			3. *Blue Marl* (7).
4. Sand and Stone	30				4.
5. Clay	15	Hinton, Norton, Woolverton, Bradford Leigh.	....	Imbedded is a thin stratum of calcareous Grit. The stones flat, smooth, and rounded at the edges.	5. *Forest Marble* (17—18).
6. Forest Marble	10	......	A mass of Anomiæ and high-waved Cockles, with calcareous Cement................	The cover of the upper bed of Freestone, or Oolyte.	6.
7. Freestone	60	......	Scarcely any Fossils besides the Coral	Oolyte, resting on a thin bed of Coral.—Prior Park, Southstoke, Twinny, Winsley, Farley Castle, Westwood, Berfield, Conkwell, Monkton Farley, Coldhorn, Marshfield, Coldashton.	7. *Great Oolite of Bath* (20).
8. Blue Clay......	6	Above Bath.			8.
9. Yellow Clay..	8				9.
10. Fuller's Earth	6	......	....	Visible at a distance, by the slips on the declivities of the hills round Bath.	10.
11. Bastard ditto, and Sundries	80	......	Striated Cardia, Mytilites, Anomiæ, Pundibs, and Duck-muscles.		11.
12. Freestone	30	......	Top-covering Anomiæ with calcareous Cement, Strombites, Ammonites, Nautilites, Cochliæ, Hippocephaloides, fibrous Shell resembling Amianth, Cardia, prickly Cockle Mytilites, lower Stratum of Coral, large Scollop, Nidus of the Muscle with its Cables	Lincombe, Devonshire Buildings, Englishcombe, Englishbatch, Wilmerton, Dunkerton, Coomhay, Monkton Combe, Wellow, Mitford, Stoke, Freshford, Claverton, Bathford, Batheaston, and Hampton, Charlcombe, Swainswick, Tadwick, Langridge.	12. *Under Oolite* (22—23).
13. Sand	30	......	Ammonites, Belemnites.................	Sand Burs.	13.

14. Marl Blue	40	Round Bath .	Pectenites, Belemnites, Gryphites, highly-waved Cockles........	Ochre Balls.—Mineral springs of Lincombe, Middle Hill, Cheltenham.	14. *Blue Marl* (25).
15. Lias Blue	25	......	Same as the Marl with Nautilites, Ammonites, Dentalia, and Fragments of the Enchrini	The fertile Marl lands of Somersetshire. Twerton, Newton, Preston, Clutton, Stanton Prior, Timsbury, Paulton, Marksbury, Farmborough, Corston, Hunstreet, Burnet, Keynsham, Whitchurch, Salford, Kelston, Weston, Pucklechurch, Queencharlton, Norton-malreward, Knowle, Charlton, Kilmersdon, Babington.	15. *Blue Lias* (26).
16. Ditto White ...	15				16. *White Lias* (27).
17. Marl Stone, Indigo and Black Marl	15	......	Pyrites and Ochre	A rich manure.	17.
18. Red-ground ...	180	......	No Fossil known	Pits of Ruddle. Beneath this bed no fossil shells, or animal remains are found: above it no vegetable impressions. The waters of this stratum petrify in the trunks in which they are conveyed, so as to fill them, in about fifteen years, with red Watricle, which takes a fine polish.—High-Littleton.	18. *Red Marl and Gypsum* (28, 29).
19. Millstone.					19.
20. Pennant Stone	...	......	Impressions of unknown Plants resembling Equisetum.		20. *Coal Districts* (30).
21. Grays	...	......	....	Fragments of Coal and Iron Nodules.—Hanham, Brislington, Mangotsfield, Downend, Winterbourn, Forest of Dean, Pensford, Publow, Chelwood, Cumptondando, Hallatrow near Stratford-on-Avon, Stonebench on-the-Severn, four miles from Gloucester.	21.
22. Cliff	...	......	Impressions of Ferns, Olive, stellate Plants, Threnax-parviflora, or Dwarf Fan Palm of Jamaica	Stourbridge, or Fire-clay.	22.
23. Coal					23.

in June 1801, for a work, to be entitled, '*Accurate Delinea-* '*tions and Descriptions of the natural order of the various* '*Strata that are found in different parts of England and Wales,* '*with practical Observations thereon:*'—and an agreement was made for its publication with a London bookseller. The subscription filled readily; and the author appears at this time to have applied himself seriously to the task of publication; several different sketches of memoirs, and coloured maps of various sizes, still existing, which bear the date of that year (1801). One of these, a coloured copy of the Index to Carey's England, which has been presented to the Geological Society, is alone sufficient to prove the great extent to which the order of the strata had been ascertained at that period: and among other documents of this date in Mr. Smith's possession, are two copies of Carey's larger map, on a scale of fifteen miles to an inch;—one of them, uncoloured, having the lines of outcrop of some of the strata cut through, so that they can be raised above the general level of the paper, the other coloured geologically, and differing very little, even in the more complex parts in the interior of the south of England, from the more finished map, published several years afterwards. In the North also, the line of the oolites in Yorkshire, derived principally from notes taken during the author's excursion of 1794, deserves, in the opinion of a very competent judge, to be contrasted with a less accurate colouring of that country, subsequently published by Mr. Smith himself in 1821.

One of the chief defects in all these maps is, that the tract between the North and South Downs of Kent, Sussex, &c. is erroneous: being coloured, in some copies, of the same hue with the beds above the chalk; while in others, although the outcrop of that stratum towards the Wealds is expressed, the subjacent beds are not detailed. Nor do the colours, in any of the maps now referred to, extend to Cornwall, the greater part of Wales, or the north-western counties of England. With these exceptions, the maps of this early date do not suffer by comparison with any of the more recent publications; and the great difficulty of the oolitic tracts in the interior seems to have been at that time completely overcome.

Among Mr. Smith's remaining papers, are several fragments intended for a Memoir to be connected with the map and sections; one especially, in his own writing, which contains a "PLAN" in detail *, with several pages of a preface

* The paper here referred to is as follows: the date is 1801.

'PLAN of the work: To be divided into Two Parts.—

"The *First* of which should treat of the structure of the earth, or general 'disposition of the most remarkable known strata, collected from the best 'authorities, and arranged according to the order discovered in England;

and introduction: and these, with some slight defects or rather peculiarities of style, are of such value, that it is much to be lamented that the undertaking was not completed.

The failure of the bookseller who was to have published the intended work, most unfortunately defeated this project of publication: but it is clear that Mr. Smith was then in posses-

'and the *Second* should enter into the particulars of each stratum, with 'the fossils and minerals that have hitherto been discovered, with their 'connection and dependance one upon another. Though it is impossible 'for the labours of an individual ever to accomplish a thousandth part of 'what is proposed by this section; yet when a system is established which 'has Nature for its prototype, every one will be enabled to contribute his 'mite, and carry it on from time to time, till after ages may get a tolerable 'description of the habitable world,

"*Many* sections of the strata, in different directions, will be necessary 'to show their various inclinations. In the general section, each principal 'stratum should be numbered; with progressive numbers, beginning at 'the eastern strata of the kingdom; or, till that can be accurately ascer-'tained, at some stratum that forms a grand feature therein. As for in-'stance, the chalk which I would call No. 1; and those lesser strata, 'which are contained within it, or generally attached to it, or form any 'subdivisions therein,—I would call 1. a., 1. b., 1. c., &c. If any thin stra-'tum should be omitted, or a new one discovered, it may be brought into 'those numbers, by making it 1 a a., &c.

"After the general section of a country or district, should follow a 'large section of each stratum, with its concomitant small strata: with 'drawings and descriptions of such peculiarities as the principal stratum, or 'those connected with it, are found to contain; whether the exuviæ of 'marine animals, vegetable impressions, or fossil wood, coal, and metal of 'every description.

"The same numbers which refer to the section, may refer to an explana-'tion of the chemical properties of each substance, so far as discovered. 'This may be placed at the end of the book, or make a separate volume; 'where those properties may be more minutely examined than can con-'sistently be done in the body of the work,—which is intended to form a 'true representation of the order of *Nature*, with no more digressions from 'the main subject than are absolutely necessary to make it intelligible. 'Plates should be bound up at the end of each volume, in a peculiar man-'ner; these, as well as the strata, to make them more striking, should be 'coloured.

"The Second Section of the work may be divided into chapters, each 'stratum making a chapter or division, to which its name in conspicuous 'characters should stand as a title. The names of particular substances 'described in this division should also appear conspicuous and striking 'as well as the places they are found at, or near to; and a more particular '*section* will accompany each part of the work, with the map divided into 'squares, or published in parts; which may be united together, and form a 'complete map and general section on a large scale.—[Query, Map of *each* 'stratum?']

"The chemical part, which refers to the other by the numbers, may be 'arranged under the heads Iron, Coal, Limestone, &c. By this means 'those veins which lie very distant from each other, will admit of an easier 'comparison. This should form a summary of the more useful minerals."

sion of such documents, in a state fit for their appearance, as to embrace all the leading facts, and a great part of the detail of what has since been made known on the stratification of England. Had he published, as he could have done, at this period, he would have stood alone, and anticipated all competition by several years; and his work would have given an impulse to the subject, the effect of which it is now impossible to appreciate. But it is equally clear that, long before the map and sections did issue from the press, quite enough had been done by Mr. Smith himself, and by many of those to whom he communicated his observations, to diffuse a knowledge of his principles, so widely and effectually, as nearly to amount to a publication of them in England.

Though defeated in his purpose of making his discoveries public, in the best and least disputable form, by the unforeseen and critical event above mentioned, he does not seem to have been dispirited, or to have changed his habit of imparting his knowledge without reserve. He continued to exhibit his maps, sections and specimens as usual, and to explain his views to all who were desirous of becoming acquainted with them. Full of his subject, overflowing with information long familiar to his own mind, but in the then existing state of geology quite new to his contemporaries, he could not help, in fact, diffusing what he knew: and in some instances about this period he was very fortunate in these communications.

"I think (Mr. Bevan mentions, in a letter to the writer of this paper) 'the first of my acquaintance with Mr. Smith was 'in June 1801, at the sheep-shearing of the late Duke of Bed-'ford at Woburn Abbey.

"After dinner I observed a person at the table exhibiting 'some papers with sketches of the stratification of England. 'I did not observe any of the company appear to notice with 'much interest or attention the sections, or attend to Mr. 'Smith's theory; and he was on the point of folding up his 'papers, with little hopes of engaging the attention of any of 'the company, when I requested him to allow me to examine 'them, which he seemed pleased to do. From that time our 'acquaintance has continued to the present time.

"In the course of about half an hour, I learned from him 'the outlines of his discovery:—pretty nearly equal to all that 'has been since made known, except as to detail.

"In the evening of that day I called at Woburn, on my 'friend Farey; and explained to him the theory of Smith, 'and assured him that I had compared it with many facts 'within my own knowledge in the neighbourhood, and found 'it fully to agree with them. Mr. F. did not coincide with 'me at that time; but soon afterwards he entered into the 'system with great pleasure.

"In January 1802, at the Duke of Bedford's request, Mr. 'Smith came to Woburn, to investigate the stratification of 'the southern parts of this country, and the parts of Bucking- 'hamshire adjoining. The letter announcing the arrival of 'Mr. Smith, is dated 23rd January 1802, inviting me to join 'them on the following Monday, to spend three or four days 'on a geological survey, to commence near Dunstable, at the 'foot of the chalk hills, and thence to Wendover, Aylesbury, 'Quainton, Winslow, Leighton, &c. In this survey we loaded 'ourselves with fossils and specimens;—and thus commenced 'our geological experience under the guidance of the founder 'of the system.

"More investigations of this nature would have followed, 'but for the death of the Duke of Bedford, under whose kind 'patronage, and at whose expense, the first survey was 'made."

The picture given in the beginning of these passages is very striking. Those who are acquainted with the imperfect diffusion of geological knowledge in England even at the present day, can imagine what it must have been to deliver a lecture on a new geological system, after dinner, to an assembly of English farmers at an agricultural meeting, more than thirty years ago; nor can they be surprised at the result described by Mr. Bevan. Yet even there this zealous devotion and enthusiasm were not without reward: for Smith then made one convert of such value as to compensate for the inattention of many of his reluctant hearers.

Mr. Farey was another pupil who became known to Mr. Smith about the same time: and in a letter to Sir Joseph Banks, giving an account of the geological excursions mentioned by Mr. Bevan, Mr. Farey states, that he had then seen in Smith's possession a coloured geological map, of very large size, (about 7½ feet by 5 feet,) on which the outcrop of the several strata was delineated; and that Smith had given him information of peculiar value on the distinctions between the superficial accumulations of clay, loam, and gravel, and the same substances in the form of regular strata.

In May 1804, he attended a meeting of the Board of Agriculture in London, for the purpose of exhibiting his maps and sections, and of explaining his views, and was requested by the Board to prepare a specific statement of his researches. At the Woburn sheep-shearing of the same year, a paper was drawn up by Sir Joseph Banks, and a sum of money raised, by subscription from the Duke of Bedford and other eminent and patriotic persons, for the double purpose of publishing Smith's works, and of compensating the author,—on the ground that 'the expenses he had incurred in travelling and in sacrificing 'time that ought to have been altogether devoted to his pro'fessional duties, were not likely to be repaid to him by the 'profits of their publication.' In the course of this year also his collection of specimens was removed to his house in London; where it was seen and examined by many of the persons interested in the subject; among whom were several of those who afterwards became leading members of the Geological Society, on the institution of that body in 1807. The 1816 collection was subsequently (~~1806~~) purchased by Government for the British Museum, where it now remains.

In the mean time, this continued delay in the publication of works so long announced, must have appeared unaccountable to those who were not acquainted with the circumstances; and as the hopes of a communication from the author himself had been so often disappointed, it was not to be expected that other persons should abstain from applying the general principles which had been so freely diffused. Those who, from indolence or a fastidious desire of perfection, or—as in the case of Mr. Smith, from a combination of unfortunate events,—too long delay the appropriation of what they have done, must be content to run the risk of seeing their discoveries brought forward by other persons. Nor would it be just or reasonable, without the strongest evidence, to doubt the fairness of such rival claimants. The history of every science abounds in examples of coincidence in discovery, produced by accident,—by the natural course of inquiry,—or by the effect of hints so very slight, as not to be appreciable even by those who act upon them.

During the latter years of Mr. Smith's progress in England, the French naturalists had been intently occupied in the examination of their own country: and in 1810, Cuvier and Brongniart published an abstract of their celebrated work on the environs of Paris*, which was followed during the next year by the volume itself†. Of a work so well known, it is

* *Essai sur la Geographie Minéralogique des Environs de Paris.—Annales du Museum,* tom. xi. p. 293, &c. This abstract is stated (p. 294–5) to have been read to the Institute in 1810:—and the authors expressly say that their abridgement was published before the completion of their work, —which had been commenced four years before, in 1806,—for the purpose of "taking date" for their researches.—*Quelques circonstances nous obligent de presenter aujourd'hui cet abrégé, et de prendre date pour des recherches aussi longues, &c.'*

† Paris 1811. 4to. pp. 278.

sufficient to say, that no publication has given a greater impulse to geological science;—bringing into view distinctly, and for the first time, that great class of deposits which connects the secondary strata with the products of still subsisting operations, establishing on impregnable ground the importance of zoological inquiries to the history of the earth, and affording some of the most masterly examples of the investigation of local details. The principles on which this memorable work is framed, are precisely those to which Smith had previously been conducted, and which there can be no question he had made known extensively in England, so far back as in 1799,—superposition of strata, identified by the fossils which they contain: and to these principles it is plain the French philosophers must have been led, by the independent inquiries which had been long going on in France, and by the better acquaintance of the French naturalists with Werner's doctrines as to the order of formations. After mentioning the steadiness in the order of the strata throughout the tract which they describe, the authors have thus distinctly announced the principle of which they availed themselves in recognising them.—

'Cette constance dans l'ordre de superposition des couches les plus 'minces, et sur une étendue de 12 myriamètres au moins, est, selon nous, 'un des faits les plus remarquables que nous ayons constatés dans la suite 'de nos recherches. Il doit en résulter pour les arts, et pour la géologie, 'des consequences d'autant plus intéressantes qu'elles sont plus sûres.

'Le moyen que nous avons employé pour reconnoître au milieu d'un si 'grand nombre de lits calcaires, un lit déjà observé dans un canton très 'eloigné, est pris da la nature des fossiles renfermés dans chaque couche; 'ces fossiles sont toujours généralement les mêmes dans les couches cor-'respondantes, et presentent des différences d'espèces assez notables d'un 'système des couches à un autre système. C'est un signe de reconnois-'sance qui jusqu'à présent ne nous a pas trompé*.'

It was not till the summer of 1815,—after an interval during which the author had to struggle with many severe difficulties and trials, that Smith's Geological Map of England at last made its appearance, and was followed by the publication of the other productions enumerated at the commencement of this paper. Of works now so long in the hands of the public it is needless here to speak in detail; but for the purpose of illustrating the progress of the subject, within the limits to which these pages are of necessity confined, a reduced copy of the Geological Section from London to Snowdon† has been inserted in the plate annexed to this pa-

* *Essai, &c.—Annales du Museum,* xi. pp. 307, 308.

† This Section, though not published till 1817, had been long before prepared.

per; so that the series of figures may serve to place before the eye of the reader some of the most important steps which geological science has made in this country. The first figure of the plate represents a real section, by Mr. Strachey, of a colliery in Somersetshire,—in which the relative position of the red-marl and superior strata, and of the coal-measures, is distinctly seen. The second figure proves that Strachey was acquainted also with nearly the whole of the English series of strata, though he suffered himself to be called aside from the facts, by a fanciful and extravagant hypothesis as to their inclined position. The third figure* is that by which Mr. Michell illustrates his masterly exposition of the structure of the globe. It is in truth an abstract section, on a very small scale, of what really exists in nature. The fourth figure is Smith's section of England just mentioned; and was at the time of its publication not only the first and most perfect display of the strata of this island ever published, but unquestionably one of the most perfect sections of any portion of the globe so complex, which ever had been produced. It is necessary, for the purpose of rendering the numbers on this section intelligible, to subjoin a List of the strata to which they refer, and this has been taken from the engraved table in Mr. Smith's memoir connected with his map. [See the List at page 55.] The reader will thus be in possession of the state of our knowledge respecting the English series at the date of its appearance; and a comparison with the recent list of the strata, on the same page, will show the changes which the farther researches of the last fifteen years have introduced †.

One remark only may be offered here, with respect to the lowest members of the series in Smith's maps and sections, which are confessedly the most imperfect portion of his work;—and though derived from theory, it may perhaps be deserving of attention. It will be seen, even from the section, (fig. 4.) that the lines of stratification above the coal-measures, and on the east of the districts occupied by the slate and other transition rocks, are nearly parallel; all declining uniformly towards the south of east. But no such lines are visible to the westward: the strata thereabouts are contorted and confused; and it has been doubted whether any such permanence really exists among them, as will bear a comparison with that which has been shown to prevail among the higher members of the secondary series. May it not be inquired, Whether this incon-

* From the Phil. Trans, 1760. vol. li. p. 566.

† The coincidence of the list of 1815, with the 'Tabular view' of 1799, (see pages 46 and 47) proves very remarkably the accuracy of the observations of that earlier date.

	No.	Smith—1815. Names of the Strata on the Map and Sections.	1832.	Present Names.
		[These upper beds above the chalk not noticed by Smith.]		Marine and Fresh-water Strata of the Isle of Wight and Dorsetshire, &c.
Plains.	1	London Clay		London Clay.
	2	Clay, or Brick Earth		Plastic Clay.
	3	Crag		*[The Crag of Suffolk, &c. is superior to all the Strata here enumerated.]*
	4	Sand and light Loam		
Chalk Hills.	5	Chalk { Upper / Lower	Chalk	Upper. / Lower.
	6	Green Sand	Green-sands	Upper Greensand.
Clay Vales.	7	Blue Marl		Gault.
	8	Sand		Lower Green-sand.
		[The Wealden group not distinguished by Smith.]	Weald-en	Weald Clay. Hastings Sands. Purbeck Limestone.
	9	Portland Rock	....	Portland Oolite.
	10	Sand	....	Sand, beneath the Portland Stone.
	11	Oak-tree Clay	....	Kimmeridge Clay.
				Weymouth Sands and Grit.
	12	Coral Rag and Pisolite	....	Coral Rag (Oxford Oolite.)
	13	Sand and Sand-stone	Oolitic Forma-tions..	Sands and Grit.
	14	Dark Blue Shale		Oxford Clay.
	15	Kelloway Stone		
Stonebrash Hills.	16	Cornbrash		Cornbrash.
	17	Sand and Sand-stone		
	18	Forest Marble Rock	....	Forest Marble.
	19	Great Oolite { Clay	....	Bradford Clay.
	20	Great Oolite { Upper Oolite	....	Great Oolite (Bath).
	21	Under Oolite { Fuller's earth & rock	....	Fuller's Earth.
	22	Under Oolite { Oolite	....	Inferior Oolite.
	23	Under Oolite { Sand	....	Sand and Grit.
	24	Under Oolite { Marlstone		
Marl Vales.	25	Blue Marl	....	Marl.
	26	Lias { Blue	Lias	Blue Lias.
	27	Lias { White		White Lias.
	28	Red Marl and Gypsum	New Red sandstone	Red Marl.
	29	Magnesian Limestone / Soft Sand-stone		Zechstein, Conglomerate of Exeter.
Coal Tracts.	30	Coal Districts	....	Coal-measures. Millstone Grit.
Mountainous.	31	Derbyshire Limestone	....	Mountain, or carboniferous Limestone.
	32	Red and Dun-stone	....	Old Red Sandstone.
	33	Various; Killas or Slate	....	Transition Slates, &c.
	34	Granite, Sienite, Gneiss	....	Granite, &c.

gruity is not *apparent* only?,—resulting, not from any want of regularity in the arrangement of the lowest beds themselves, but from the difficulty of detecting their relations, and our very imperfect acquaintance with them?. If the hypothesis be true, which supposes all the stratified rocks to have been produced by deposition, there is no obvious reason why the order should be more constant and regular in one portion of the series than in another:—and if (to advance still farther in theory) the change in the character of the lower secondary rocks has been produced by their proximity to the crystalline, and perhaps at one time incandescent, masses beneath them, may not distinctive characters still survive, if sought for by researches sufficiently acute and persevering, to enable us to detect those proofs of order in their deposition,—which must have been obvious, or at least discernible, at the time when they were deposited, and must have remained so, till their characters were partially changed?

This sketch of the progress of geology in England has now been brought down to the period of Mr. Smith's publications; beyond which it was not the intention of the writer to extend it. In the course of these remarks, conflicting claims may possibly have been weighed with too much exactness, against observations not in the first instance derived from study, but suggested by sagacity, or almost spontaneously arising from the facts as they came into view. It may therefore be right to repeat, that nothing has been stated here with any intention to question the consciousness of originality, in those inquirers whose observations we have shown to have been anticipated. And after such a list of authorities as it has been our duty to bring together, no better conclusion for this paper can be adopted, than a passage from the eloquent and affecting address delivered from the chair of the Geological Society, in conferring upon Mr. Smith the first mark of public gratitude which it was in the power of that body to bestow.—Mr. Sedgwick, while exercising upon that occasion, what he justly calls the 'high privilege' of rewarding distinguished merit, has thus adverted to the labours of preceding inquirers:—'The works of these authors were, however, entirely unknown to Mr. Smith during his early life, and every step of his progress was made without any assistance from them. But I will go further, and affirm, that had they all been known to him, they would take nothing from the substantial merit of his discoveries. Fortunately placed in a country where all our great secondary groups are brought near together, he became acquainted in early life with many of their complex 'relations: he saw particular species of fossils in particular 'groups of strata, and in no others; and giving generalization 'to phænomena, which men of less original minds would 'have regarded as merely local, he proved, so early as 1791, 'the continuity of certain groups of strata, by their organic 'remains alone, where the mineral type was wanting. He 'made large collections of fossils; and the moment an op-'portunity presented itself, he arranged them all stratigra-'phically. Having once succeeded in identifying groups of 'strata by means of their fossils, he saw the whole import-'ance of the inference,—gave it its utmost extension,—seized 'upon it as the master-principle of our science;—by help of 'it disentangled the structure of a considerable part of Eng-'land,—and never rested from his labours till the public was 'fairly in possession of his principles. If these be not the 'advances of an original mind, I do not know where we are 'to find them: and I affirm with confidence, after the facts 'already stated, that the Council of the Geological Society 'were justified in the terms of their award; and that Mr. 'William Smith *was* the first in this country to discover and 'to teach the identification of strata, and to determine their 'succession, by means of their imbedded fossils*.'

X. *On a New Oxy-hydrogen Jet.* *By* J. F. Daniell, *Esq. F.R.S. Prof. Chem. King's College, London.*

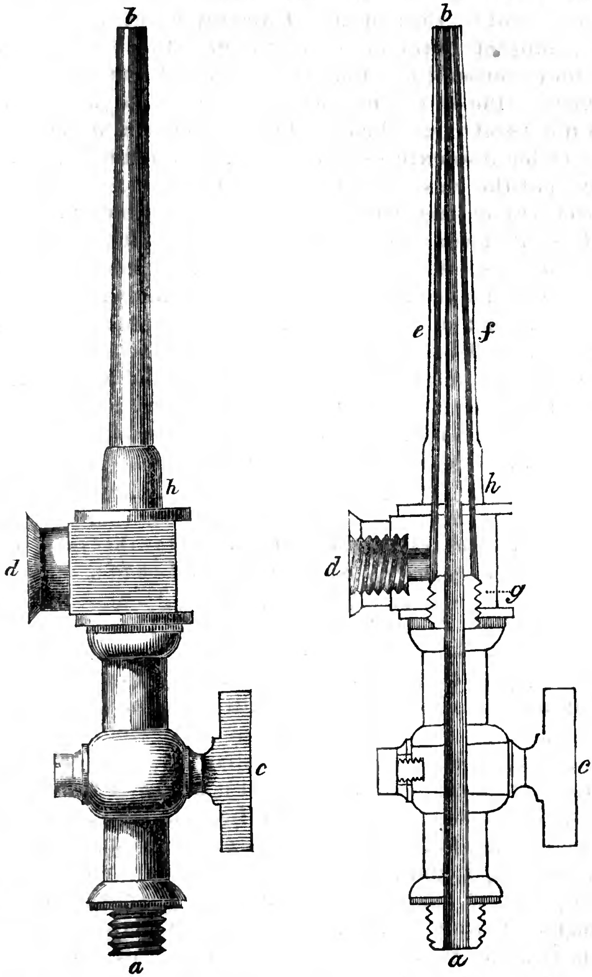
To R. Phillips, Esq. &c. &c.

My Dear Sir,

I SEND you herewith a drawing of a jet for the combustion of any inflammable gas with oxygen, which I have found extremely useful, both in original research, and in commodiously exhibiting a variety of instructive class experiments. Several of my friends have adopted it at my suggestion, and are equally pleased with it; and I have in consequence been so often applied to for a description of the arrangement, that, simple as it is, I cannot but think that an account of it may be acceptable to many of the readers of your Journal. The drawing represents it entire, and in section, and of two thirds of the original size: *a b* is a jet of brass, to be connected, by means of the stop-cock *c*, and a flexible pipe, with a gas-holder of oxygen gas. This is fixed by means of a screw *g*,

* Address of Mr. Sedgwick, as President of the Geological Society, on awarding the first Wollaston medal to Mr. William Smith.—Proceedings of the Geological Society, 1831, pp. 273, 274.—See Phil. Mag. and Annals, N.S. vol. ix. p. 275.

in the centre of another jet *e f*, (only seen in the section,) and connected by means of the lateral arm and screw *d*, with another gas-holder of hydrogen gas; or with, what is better still, the pipe of a coal-gas burner. The second jet thus forms an



exterior coating to the first; and when the inflammable gas flows through it, and is ignited at the orifice, a stream of oxygen may be directed into the interior of the flame by means of the latter with any required degree of force.

All the effects of the oxy-hydrogen blowpipe may thus be produced with the greatest convenience and safety; and that upon a scale which it would not be prudent to adopt with that instrument. When coal-gas is used, the change in the colour of the flame indicates with great precision the exact amount of oxygen which is sufficient to insure perfect combustion; and by these means I readily effected the fusion of 100 grains of clippings of platinum into a perfect button, with an expense of less than three pints of oxygen gas. The aperture of the jet did not become, during the process, hotter than the hand could bear. The combustion of the gases is thus rendered so extremely manageable and œconomical, that I have not the least doubt that by the proper arrangement of three or four such jets, the waste cuttings of platinum which are formed during the working of that metal, and which at present can only be worked up by redissolving them in acid, might be readily melted together, and applied to profitable use. I have succeeded in melting together a considerable quantity of the grains of crude platinum, after digestion in nitric acid; but the button was perfectly brittle under the hammer.

By placing the end of the jet *b h* within a lantern provided with a parabolic reflector, and exposing to the flame upon a pin of platinum a small fragment of lime, I succeeded perfectly in exhibiting the beautiful experiment of Lieut. Drummond's light; and can produce a prismatic spectrum almost equal in brilliancy to the solar. By concentrating the rays of light from the same source by means of the lenses of a solar microscope, phosphorus may be inflamed, and chloride of silver blackened; affording a beautiful illustration of the conversion of heat, *which will not pass through glass* into heat *which will pass through glass*, with all the properties of solar heat, by the radiating power of a solid undergoing no chemical change whatever.

The same jet when supplied with common air from the gas-holder, instead of oxygen gas, serves the purposes of a very convenient blowpipe; and I have taken the opportunity which this apparatus has afforded me, of trying upon a small scale the experiment of heating the current of air which supplies the combustion, upon the principle which has lately been applied with such great success at the blast-furnaces of the Clyde Iron-works, according to Mr. Dunlop's patent.

For this purpose I connected the jet *b a* with a brass tube, seven inches long, which I heated nearly to redness; but upon passing a current of air through it, it issued at the orifice at a temperature, I think, below 300°. When the flame of coal gas was supplied with this current, the temperature of a piece ofplatinum exposed to it visibly exceeded the temperature of the same piece when the flame was fed with cold air; but I did not succeed in melting it. A piece of platinum foil, however, showed signs of fusion upon its edges. By heating the current of air to a still higher degree, by exposing it in its course to a larger and a hotter surface, I should not despair of accomplishing this object.

Indeed, since I have had the good fortune to show that the highest temperature of our furnaces does not, probably, exceed 3500° Fahrenheit, instead of 22,000°, at which it has been, till very recently, estimated *, it is easy to understand how a supply of air at 600° or 700° may increase their efficiency; and that a like augmentation of temperature bears a considerable proportion to the melting point of cast iron reckoned at 2800°, which would be perfectly insignificant if the same point were 18,000°.

I remain, my dear Sir, yours very truly,

King's College, Dec. 6th, 1832. J. F. Daniell.

XI. *On the Existence of a Real or Imaginary Root to any Equation.* By R. Murphy, *Esq. M.A. Fellow of Caius College, Cambridge* †.

Let $f(x) = 0$ be any given equation: put $x = p + q\sqrt{\overline{1}}$. Then giving to p and q all possible values, there must be, amongst the values of $f(x)$ which result, some one exactly $= 0$.

For if not, if we reject all the imaginary results, there must be some one amongst the real ones nearer to zero than any other; let the values of p and q be then P and Q, and let R be the value of the function.

Let h be an indefinitely small quantity, then changing P into $\text{P}+h$, R would be changed into $\text{R}+\text{A}\,h^n$ (retaining only the lowest power of h).

But if we change simultaneously P into $\text{P}+h\cos\frac{\pi}{n}$, and Q into $\text{Q}+h,\sin\frac{\pi}{n}$, the whole increment to n or $\text{P}+\text{Q}\sqrt{-1}$ is then $h\left(\cos\frac{\pi}{n}+\sqrt{-1}\sin\frac{\pi}{n}\right)$; and consequently $f(x)$ or R would become $\text{R}+\text{A}\,h^n\left(\cos\frac{\pi}{n}+\sqrt{-1}\sin\frac{\pi}{n}\right)^n$ $= \text{R}-\text{A}\,h^n$.

* See Prof. Daniell's papers on his new Register Pyrometer, in Phil. Mag. and Annals, vol. xi.; and Phil. Mag. and Journ. of Science, vol. i.—Edit.

† Communicated by the Author.

If, therefore, the change first effected does not give a result nearer to zero than R, the second one will, and *vice versâ*; contrary to hypothesis.

Hence the proposition must be true.

Cambridge, Dec. 20, 1832.

XII. *Notice respecting the Determination of the Geographical Positions of the Village of Chamouni, and the Convent of the Grand St. Bernard. By* J. D. Forbes, *Esq. F.R.S. L. & E.**

AMONG the instruments with which I provided myself before leaving Britain, was a portable altitude and azimuth circle, constructed for me upon Captain Kater's principle, by Mr. Robinson, precisely similar to that which accompanied Captain Parry upon his last journey towards the Pole. It is considerably larger than the instruments usually made by Mr. Robinson of this form, and proportionably more perfect. The circles have a diameter of $4\frac{1}{2}$ inches, and are divided to 15″. The azimuth circle has three verniers; that for altitude, two. Two telescopes accompany the instrument, with three simple eye-pieces, and a diagonal one. The level is an excellent one, and there are various other appendages which I shall not at present particularize.

I intended to use this circle rather as a theodolite than an astronomical instrument; but my friend Professor Gautier, of Geneva, to whose kindness I have been much indebted during my stay in that place, suggested that I should make it the companion of my journey in Savoy, and determine the position of two points of interest, (for neither of which have we any direct observations,) Chamouni, and the Convent of the Grand St. Bernard; the last in particular is interesting, from the importance which attaches to it as a meteorological station. I readily adopted his suggestion; and had reason to be satisfied with the capability of the instrument to resist shocks, having been carried during the greater part of the journey upon a mule. The weather at Chamouni was far from favourable, and I only obtained two series of simple altitudes of the sun on different days, for the time, and three altitudes of *Polaris* at intervals during a cloudy evening. At St. Bernard I was more fortunate; I obtained corresponding altitudes on the evening of the 30th of August, and the succeeding morning, which gave the time of apparent midnight: and I took eight successive altitudes of *Polaris*, half with the face of

* Read before the Royal Society of Edinburgh, December 3, 1832; and communicated by the Author.

the instrument east and half west. The piercing cold of the night rendered these observations somewhat difficult, being a transition of no less than 60° Fahrenheit, from the heat under which I had been suffering in the plain a few days before. The longitudes which I have deduced chronometrically cannot of course be depended upon to great exactness, considering the trial to which the instrument is subjected in travelling on the mountains. The rates which I shall give below will show the degree of confidence which may be placed upon them. The chronometer was constructed for me with extreme care by Mr. Whitelaw, of Edinburgh, an artist whose ingenuity and practical skill will, I hope, in due time be generally appreciated*. I have been perfectly satisfied with the performance of this instrument, under a great variety of trials to which it has been subjected: the rates I am about to give were influenced of course by the incessant jolting of a very rough journey, made chiefly on foot, and are only to be considered in reference to such circumstances.

Geneva, 1832.—Mean daily rate of chronometer, August 16—18	$+20^{s}\cdot 3$
From August 18 to September 5 (including the journey in Savoy)........................	$+26\cdot 7$
From September 5 to September 6	$+26\cdot 0$
Chamouni.—From August 24 to August 26...	$+26\cdot 1$

N.B. This uniform rate is surprising, when I state that in the interval (August 25) I made the excursion to the "*Jardin*," with the chronometer in my pocket. I should certainly have left it behind, had I not required it for magnetical observations which I made at the "*Jardin*."

Observations.

Geneva, Aug. $18^{d}\ 0^{h}$. Error of chronometer upon mean time at the Observatory.................. slow $11^{m}\ 46^{s}\cdot 2$.

Chamouni. Le Prieuré. Aug. $24^{d}\ 4^{h}\ 31^{m}$. Error of chronometer upon mean time by eight simple altitudes of the sun; four with the limb of the instrument in each direction .... slow $12^{m}\ 10^{s}\cdot 0$

Aug. $26^{d}\ 4^{h}\ 48^{m}$. By five simple altitudes slow 11 17·8

Aug. 26. Determination of the latitude by altitudes of *Polaris*.

Time by Chron.	Face of Instrument.	Altitude.	Latitude.
$11^{h}\ 18^{m}\ 27^{s}\cdot 5$	West.	46° 59′ 30″	45° 55′ 14″
40 19	East.	47 7 30	45 56 40
47 49	East.	47 9 22	45 56 26

* A description of his beautiful escapement has been given by Mr. Robison in the Edinburgh Transactions, vol. xi.

Face of instrument, West.	Latitude	45° 55′ 14″
———— East.	——	45 56 33
Mean or latitude of Le Prieuré......		45 55 54

Longitude: E. of Geneva ... $2^m\ 47^s{\cdot}6$ = 41′ 54″
———— Paris...... 18 4
———— Greenwich 27 25

St. Bernard. (At the Hospice) Aug. $30^d\ 4^h\ 28^m$, and $19^h\ 12^m$. Six pairs of equal altitudes of the sun gave for the error of the chronometer at midnight............slow $9^m\ 50^s{\cdot}2$.

Aug. 26. Determination of the latitude by altitudes of *Polaris*.

Time by Chron.	Face of Instrument.	Altitude.	Latitude.
$8^h\ 57^m\ 22^s$	West.	46° 8′ 45″	45° 50′ 11″
9 3 56	——	46 11 37	45 50 21
9 15 25	East.	46 16 30	45 50 35
9 19 51	——	46 17 52	45 50 10
9 25 10	——	46 20 0	45 50 11
9 28 17	——	46 21 30	45 50 26
9 35 59	West.	46 24 7	45 50 0
9 43 20	——	46 25 15	45 48 16

Mean with face of instrument, W. (excluding the last value as differing too much from the mean	45° 50′ 11″
Mean with face East.................................	45 50 20
Latitude of the Hospice of St. Bernard	45 50 16

Longitude: E. of Geneva ...... $3^m\ 41^s{\cdot}5$ = 55′ 22″
———— Paris......... 18 58
———— Greenwich .. 28 19

Geneva, Sept. $5^d\ 0^h$. Error of chronometer upon mean time ....slow $3^m\ 46^s{\cdot}0$.

The following are some of the positions previously assigned to these stations.

	Prieuré de Chamouni. Latitude.	Long. E. of Paris.
Raymond. Map of the Alps......	45° 54′ 52″	4° 31′ 40″
Map of the kingdom of Sardinia, published under the direction of Government. Turin, 1819	45 58 45	
Map attached to the new Survey of the Italian engineers. Milan, 1827	45 52 30	4 33 0
Map of Savoy, by Chaix, 1832...	45 55 0	4 32 15
New determination	45 55 54	4 31 0

	Hospice du St. Bernard. Latitude.	Long. E. of Paris.
Raymond. Map of the Alps......	45° 52′ 30″	4° 49′ 48″
Map of the kingdom of Sardinia, published under the direction of Government. Turin, 1819	45 56 30	
Map attached to the new Survey of the Italian engineers. Milan, 1827	45 48 45	4 54 0
Map of Savoy, by Chaix, 1832...	45 52 30	4 50 0
New determination	45 50 16	4 44 30

In reducing my observations I have employed the convenient tables of Mr. Baily, except for refraction, for which I took Dr. Young's table, given in the Nautical Almanac.

Geneva, 3rd Nov. 1832.

XIII. *Description of a Species of Natural Micrometer; with Observations on the Minuteness of Animalcula. In a Letter addressed to* Sir David Brewster, *by* George Fairholme, *Esq. F.G.S.*

My dear Sir,

IN the course of a series of microscopical observations, in which I have been of late engaged, accident has thrown in my way a species of *natural micrometer*, an account of which I now beg leave to send you. For though it may possibly not be new to one so well acquainted with every thing relating to such subjects as yourself, yet I think it worthy of description, from its having brought home to my own mind the conviction of a fact in nature, which, though we are assured of its reality by numerous authors who have written on animalcula, is yet almost beyond the range of credibility, unless proved by actual demonstration.

All authors who have treated of microscopic objects, have said that there are some animalcula so inconceivably minute, that it would require *many thousands* of them to form the size of a grain of sand. Now, although we may be satisfied that the extent of created objects appears quite boundless, in whichever direction of the scale we may direct our thoughts, yet the powers of the human understanding are so much more limited, that though the tongue may *express* it, the mind fails in its attempt to *conceive* defined ideas of organized beings so much below the scale on which our common conceptions are formed.

I have not found, in any author, the mode by which he arrived at his conclusions respecting the comparative size of the two objects above named. Leuwenhoek calculates, probably from conjecture, that the size of some animalcula is to that of a mite, as a bee is to that of a horse. I think that the following observations will demonstrate the truth of that remark; but in a manner more conclusive and convincing than mere conjecture.

In the course of last winter, having observed on a dry and frozen gravel walk a variety of small hollows, of a greenish colour, it occurred to me that that tint might have been occasioned by the *scum* upon water during the summer rains; and if so, that it would probably contain animalcula. I accordingly scraped off a little of the frozen surface, and mixed it with water which had been boiled, and in which I had previously ascertained that there were no animalcula. In a few hours I examined a drop of this water, and found, as yet, no animalcula; but I discovered a number of minute transparent fibres, apparently vegetable, and to the existence of which, the green tint I had first remarked was probably owing. I found these fibres transparent; and when viewed in a certain degree of shade, I observed them to be marked throughout their whole length, in the most delicate and regular manner, with divisions like globules in a hollow tube, each of which was separated from another by a space of exactly similar dimensions. In the course of a day or two I again examined the water, and found in it a variety of animalcula, some of which were the most minute I had ever observed, except perhaps those found in an infusion of pepper. The highest powers of a good microscope gave me no information as to their form or structure, except that they were of an oval or round form, and moved about with considerable activity.

Having near me at the time some sea sand which I had been examining, I put a few grains of it into the drop, with the view of forming some idea of the comparative size of these minute creatures; and I then began for the first time to conceive the possibility of what has been stated by Leuwenhoek and others, who have described to us the result of their observations in the minute walks of animated nature. The difference of size, however, was so great, and the angular figure of the grains of sand so rude, that I despaired of ever advancing beyond conjecture as to their actual comparative measurement.

It happened, however, that a straight piece of the above-mentioned graduated fibre lay near one of the grains of sand; and as the globules or marks in the fibre were as nearly as possible of the same size and shape as some of the animalcula swimming around, it occurred to me to use this fibre as a base on which to measure the comparative size of the two objects. I had then an exactly graduated scale for this particular calculation; and by taking the square and cubic measure of a variety of grains, of different shapes, and striking the mean of the whole, I found that instead of many *thousands* of animalcula for the size of a grain of sand, there were *from one to three millions*, necessary to make up the solid bulk. For I found the mean of ten measurements to be, 50 of the globules, which, with their 50 equal intervals, made 100 for the side of a square: the matter therefore stood thus;

$$100 \times 100 \times 100 = 1,000,000.$$

But in this calculation I had by no means taken the smallest of the animalcula discernible in the fluid. Many were much smaller than those I calculated upon; so that I had thus a simple means of proving to demonstration the existence of animated beings *from one to three millions of times less than a grain of sea sand!*

By means of a species of micrometer of my own construction, of a very simple character, but sufficiently correct for all common purposes, I consider the graduation of this natural fibre, with the intervals between the globules, to be about 6000 to an inch; and as the animalcula on which the above calculation was made were of exactly similar size and form, we thus find that the space of a common half-inch die would require 27,000,000,000 of these organized beings to compose its bulk! And when we consider that others were distinctly visible in the same fluid not more than *one third part* so large, the calculation mounts far beyond the mental powers of distinct conception.

When the microscope thus discovers to us wonders in the lower part of the scale of creation as incomprehensible as those which the telescope has disclosed to us in the upper; and when we consider that we have no ground for supposing that either of these instruments has been yet brought to its highest powers,—the boundless extent of the works of the Almighty is strongly presented to the mind. We thus find that all our ideas of magnitude and minuteness are merely comparative; and that when we endeavour to extend them beyond a certain point, we soon become lost in boundless obscurity, and find our views of creation terminate in *infinity* on either hand.

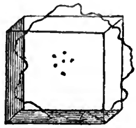
I inclose a small quantity of the earthy matter, in which you will find specimens of both the fibre and the animalcula. To observe the former in perfection, it may be examined very soon after being put in water; but the latter will not appear in any numbers, or with much motion, for several days. I have at present a glass containing some of the earth, which I have kept for about eight months, supplying pure boiled water as evaporation takes place. In the course of frequent examination during that period, I have found many different species of animalcula succeed each other; and I have even reason to think that different generations of the same species have been produced and have disappeared during that time.

I beg you will excuse my troubling you with details, which may possess none of the merit of novelty to you.

Yours, &c.

Ramsgate, August 18, 1832. GEORGE FAIRHOLME.

P.S. I find the light of a lamp much superior to that of the sun for microscopical observations on transparent or very minute objects.



A grain of the sand alluded to is about the size of one of the small dots in the annexed figure, and the zigzag line represents their irregular form.

XIV. *Proceedings of Learned Societies.*

LINNÆAN SOCIETY.

Dec. 4.—READ the Description of a species of Thrush, killed at Heron Court, Hants, in January 1828. By the Hon. Charles A. Harris. Communicated by John Curtis, Esq. F.L.S.

This account was accompanied by a letter from Mr. Yarrell, containing some observations on the bird, which appears to be identical with the Java Thrush (*Turdus varius* of Dr. Horsfield), a native of the Indian Islands, and also of New Holland. The specimen shot was in perfect plumage, and had no appearance of ever having been in confinement; but Mr. Yarrell is disposed to think that the species may likewise prove to be African, which, if confirmed, would account for its appearance on our southern coast.

Read also a continuation of Dr. Nees von Esenbeck's paper on East Indian *Solaneæ.*

Dec. 18.—A paper was read, entitled "Observations on the *Tropæolum pentaphyllum* of Lamarck; by Mr. David Don, Libr. L.S."

The *Tropæolum pentaphyllum* is a native of the countries bordering on the Rio de la Plata, where it was first discovered by Commerson; and from specimens collected by that indefatigable naturalist, the figure and description published by Lamarck were derived. The trivial names of *pentaphyllum* and *quinatum*, given to it by Lamarck and Hellenius, are, as Mr. Don justly observes, misapplied to a plant whose leaf is merely deeply lobed. Notwithstanding its having been described and figured by various authors, and but very recently in the Botanical Magazine and Register, Mr. Don appears to be the only one who has seen the ripe fruit, which proves to be a black juicy berry, resembling in appearance and flavour that of *Vaccinium Myrtillus*. This remarkable character, together with the valvate æstivation of its calyx, Mr. Don considers as amply sufficient to entitle the plant to be regarded as the type of a new genus, for which he proposes the name of *Chymocarpus*, with the following essential character:

CHYMOCARPUS. *Calycis æstivatio* valvata.
Petala 2. *Pericarpium* baccatum!

The author avails himself of the opportunity of entering into details on the natural affinities of the *Tropæoleæ*, which he is disposed to place near to the *Capparideæ*, and he points out the many characters which distinguish them from the *Geraniaceæ*, with which they have usually been associated. On comparing them with *Hippocastaneæ*, the author remarks, many striking analogies present themselves; but although the latter family is chiefly distinguished from *Tropæoleæ* by its arborescent habit, opposite leaves, and terminal inflorescence, he is not inclined to admit that there exists any intimate affinity between them.

Read likewise the conclusion of Dr. Nees von Esenbeck's paper on East Indian *Solaneæ*.

This paper is entirely descriptive, and its chief object is to give an account of the *Solaneæ* comprised in Dr. Wallich's collections. The species, with the exception of *Anisodus luridus*, are referrible to old and well-known genera, and principally consist of 22 of *Solanum*, 6 of *Physalis*, and 5 of *Datura*. The characters and synonyms appear to be drawn up with much care, and display the research and skill of the learned author.

ZOOLOGICAL SOCIETY.

Proceedings of the Committee of Science and Correspondence.

July 24.—A Letter was read, addressed by Sir F. Mackenzie to the Secretary of the Society, and dated July 16: it related to the breeding of some *Woodcocks, Scolopax rusticola*, Linn., at Conan on the eastern coast of Ross-shire, the estate of that gentleman.

For several years past, two or three of these birds have occasionally been seen in the woods, and about five years since a couple were shot just before St. Swithin's-day: these were, however, old birds, and from their being covered with fat, it was evident that they had not nestled. The keeper, in fact, had never been able to find one of their nests or to see a young bird, until the present season. In two small woods near his house he this year discovered four *Woodcocks'* nests, one having four, and the others three eggs each, all of which were hatched and ran. The young birds he repeatedly saw before they took wing; and now five or six couple may every evening, towards dusk, be observed flying about the lodge as they pass to their feeding grounds. The old birds give notice of their approach by a sharp cry of *twit-twit-twit*, repeated as rapidly as possible, and heard at three or four hundred yards distance; while the young ones are less noisy and more flagging in the motion of their wings. Than the flight of the *Woodcock* before and after incubation, Sir F. Mackenzie states that he knows nothing more rapid, as for an hour or two about dusk he (probably the male, though two have been seen together pursuing each other) flies in large circles over the tops of the trees, uttering his sharp and piercing cry, a whistle which sportsmen may have occasionally heard weakly when cocks are first flushed in the back flight in March. Sometimes his sudden flight will be arrested and changed into a sailing slowly, like a *pouter Pigeon*, his cry being at the same time varied to a purr or bleat resembling that of the *Ptarmigan*: then he will dart away with greater rapidity than a *Pigeon* in full flight, moving his wings, however, with a different action from that of the *Pigeon*, and with inconceivable rapidity.

The soil where the nests were found is gravelly and rather dry; the grass tolerably long, without underwood; and the trees, oak, birch, and larch not exceeding thirty years' growth. The situation is warm, and not 150 feet above the level of the sea; it is not far distant from the river. The woods are kept quiet, and several pheasants' nests were hatched in their close vicinity.

It is probable that the parent birds sought this spot for the purpose of breeding, as they must have arrived in the spring from other localities: for those who shot in the covers till February declare that they did not know of a single *Woodcock* being then left in them; and had there been two or three, the keeper must have been aware of it.

The skeleton was exhibited of the *Weasel-headed Armadillo, Dasypus 6-cinctus*, Linn.; and Mr. Owen read some Notes on the osteology of that species; which are given in full in the Proceedings of the Committee. Among other particulars mentioned were the following:—

The spines of the 1st, 2nd, and 3rd dorsal *vertebræ* are the longest, and slope considerably backwards; the rest of the spines, together with those of the lumbar *vertebræ*, also incline in the same direction, but in a less degree. "Every one who has seen the living *Armadillo* running about the open plot of ground in the Society's Gardens must have been struck with the machine-like manner in which the body is carried along. The short legs are almost concealed, and their motions are not accompanied by any corresponding inflections of the spine, the two extremities of the trunk not being alternately raised and depressed as in the *quadrupeds* which move by bounds. Hence there is no centre of motion in the vertebral column, or point towards which the spinous processes converge, but all these have a direction towards the *sacrum*. The relation which the structure of the vertebral column bears to the mode of progression of a *quadruped* is extremely interesting, and enables us to judge in some degree from the spine alone of the locomotive faculties of a fossil species."

The small processes that intervene between the *manubrium* and the sternal ends of the clavicles in the young animal, are afterwards anchylosed to the latter bone, and being joined together form a part superadded to the *manubrium.* This part is evidently a rudimentary form of the Y-shaped bone placed anterior to the *manubrium* of the *Ornithorhynchus,* which Cuvier regards as analogous to the *os furcatorium* of birds; it thus affords an additional and very interesting example of the affinity of the *Edentata* to the *Monotremata,* and supplies a step which was wanting in tracing the recedence of the latter, in their remarkably constructed *sternum,* from the mammiferous to the oviparous type of the *Vertebrata.* The *manubrium* itself also presents a peculiarity observable in that of the *Monotremata,* viz., a mesial longitudinal ridge on the anterior surface. This appearance in the *Ornithorhynchus* is regarded by Cuvier as indicative of an original division in the bone itself, 'Ossemens Fossiles,' v. pt. 1, p. 149; but Mr. Owen has examined the fœtus of the *ninebanded* species, and find that ossification commences in the *manubrium* by a single central *nucleus,* and not by two lateral depositions. The other bones of the *sternum* appear on an anterior view, to be almost deficient, being wedge-shaped, with the *apices* anterior; their number is four, exclusive of the ensiform cartilage.

The caudal *vertebræ,* like the cervical, present in *Dasypus* a peculiarity which is also found in the *Cetacea,* viz. that of having inferior spines, or V-shaped bones. These are present beneath all but the two last *vertebræ*; they are of a triangular form, but are articulated, not by their bases, as in the *Whale,* but by their *apices*; or rather the part which corresponds to the *apex* is flattened, and produced into two lateral processes.

August 14.—Specimens were exhibited of the following *Fishes* collected on the coast of Madeira by the Rev. R. T. Lowe, and presented by him to the Society:

Alepisaurus ferox, Lowe; *Box Salpa,* Cuv. & Val.; *Raja clavata,* Linn.; *Torpedo marmorata,* Risso; *Rhombus Maderensis,* Lowe; *Caranx* Cuv.; *Pagellus breviceps?* Cuv. & Val.; *Acarna,* Cuv. & Val.

At the request of the Chairman, the Rev. L. Jenyns exhibited an immature specimen of a second species of *crested Wren,* not hitherto recorded as having been met with in England; the *Regulus ignicapillus,* Temm. The individual exhibited was killed by a cat at Swaffham in Cambridgeshire.

Mr. Jenyns also exhibited a specimen of *Sorex remifer,* Geoff., killed in a corn-field at the distance of half a mile from any water. Its chief interest was the confirmation afforded by it of the existence in England of this species, which has recently been added by Mr. Yarrell to the British Fauna on the authority of a specimen exhibited by him at a late Meeting of the Committee (see vol. i. p. 460).

Specimens were exhibited of a species of *Woodpecker,* hitherto undescribed, which had recently been obtained by Mr. Gould from that little-explored district of California which borders the territory of Mexico. The exhibition was accompanied by a communication from Mr. Gould, in which, after some general remarks on the *Picidæ* and their geographical distribution, he referred to the species before the Committee as possessing the characters of the genus *Picus* in their most marked development, together with the greatest size hitherto observed in that group. In this respect it as far exceeds the *ivory-billed Woodpecker* of the United States, *Picus principalis*, as the latter does the *Pic. Martius* of Europe. Mr. Gould described it as the *Picus imperialis:* this species is readily distinguishable from the *Pic. principalis* by its much larger size; by the length of its occipital crest, the pendent silky feathers of which measure nearly 4 inches; by the absence of the white stripe which ornaments the neck of that bird; and by the bristles which cover its nostrils being black, whereas those of the *Pic. principalis* are white.

August 28.—Mr. Owen read some Notes on the Anatomy of the *Flamingo, Phœnicopteris ruber*, Linn.: they were derived from the examination of an individual which died about three months since in the Society's Menagerie.

"The principal *diseased* appearances were in the lungs, which were filled with tubercles and *vomicæ*. I was much struck with finding the inner surface of the latter cavities, and that of most of the smaller ramifications of the bronchial tubes, covered over with a green vegetable mould or *mucor*. As the individual was examined within 24 hours after its death, it seemed reasonable to conclude this *mucor* had grown there during the life-time of the animal. Thus it would appear that internal parasites are not exclusively derived from the animal kingdom, but that there are *Entophyta* as well as *Entozoa* *."

No *Entozoa* were met with in the specimen dissected by Mr. Owen: but Col. Sykes permitted him to examine two *tape-worms*, which he found blocking up the *duodenum* of a *Flamingo* dissected by him in Dukhun. From the marginal disposition of the *lemnisci* and the general habit of this species, it evidently appertains to the true *Tæniæ*, and from the structure of the head ranks among the rostellate species with an armed *proboscis*. It does not accord with any of those described in the 'Synopsis Entozoorum' of Rudolphi, and is of so peculiar a form that Mr. Owen felt no hesitation in characterizing it as *Tænia lamelligera*.

September 11.—Dr. Weatherhead communicated to the Committee several extracts from a letter which he had recently received from Lieutenant the Hon. Lauderdale Maule of the 39th Regiment, now in New South Wales. They referred to the habits and œconomy of the *Ornithorhynchi*.

"During the spring of 1831," writes Lieut. Maule, "being detached in the interior of New South Wales, I was at some pains to discover the truths of the generally accepted belief, namely, that the female *Platypus* lays eggs and suckles its young.

* The fact here stated must be regarded as a very interesting and remarkable one; there is no reason *a priori* why *Entophyta* should not exist; but in the case now before us,—as a certain number of hours did intervene between the death and the examination of the *Flamingo*, and we have reason to believe that *mucor* will occasionally form very rapidly on dead animal substances, while the *vomicæ* and bronchial tubes of the animal must have contained matter in a high degree susceptible of being organized (whether by seeds and *ova* or otherwise) into either *mucor* or *animalcula*,—it is equally probable, perhaps, that the formation of the *mucor* did not take place until after death.—Edit.

"By the care of a soldier of the 39th Regiment who was stationed at a post on the Fish River, a mountain stream abounding with *Platypi*, several nests of this shy and extraordinary animal were discovered.

"The *Platypus* burrows in the banks of rivers, choosing generally a spot where the water is deep and sluggish, and the bank precipitous and covered with reeds or overhung by trees. Considerably beneath the level of the stream's surface is the main entrance to a narrow passage which leads directly into the bank, bearing away from the river (at a right angle to it) and gradually rising above its highest water-mark. At the distance of some few yards from the river's edge this passage branches into two others which, describing each a circular course to the right and left, unite again in the nest itself, which is a roomy excavation, lined with leaves and moss, and situated seldom more than twelve yards from the water, or less than two feet beneath the surface of the earth. Several of their nests were, with considerable labour and difficulty, discovered. No eggs were found in a perfect state, but pieces of a substance resembling egg-shell were picked out of the *debris* of the nest. In the insides of several female *Platypi* which were shot, eggs were found of the size of a large musket-ball and downwards, imperfectly formed however, i. e. without the hard outer shell, which prevented their preservation."

In another part of his letter Mr. Maule states, that in one of the nests he was fortunate enough to secure an old female and two young. The female lived for about two weeks on worms and bread and milk, being abundantly supplied with water, and supported her young, as it was supposed, by similar means. She was killed by accident on the fourteenth day after her capture, and on skinning her while yet warm, it was observed that milk oozed through the fur on the stomach, although no teats were visible on the most minute inspection: but on proceeding with the operation two teats or canals were discovered, both of which contained milk.

The body of the individual last referred to (together with several others) has been preserved in spirit to be transmitted to Dr. Weatherhead, who stated his intention of examining it anatomically on its arrival, and of laying before the Committee the result of his observations on this interesting subject.

It was remarked, that the existence of milk in the situation described by Lieut. Maule is fully confirmatory of the correctness of the deductions made by Mr. Owen from the minute dissection of several individuals (including one in the Society's collection presented by Capt. Mallard, R. N., Corr. Memb. Z. S.), that the glands discovered by M. Meckel are really mammary. This opinion, with the anatomical reasons on which it was founded, have been lately laid by Mr. Owen before the Royal Society in a paper which is published in the second Part of the Philosophical Transactions for 1832, and which has been noticed in the first volume of Phil. Mag. and Journal of Science, p. 384. Mr. Owen's dissections, however, though they established the existence of numerous minute tubes leading from the glands in question through the skin where it was covered by the wool, did not enable him to detect any canals so large as would appear to be indicated in Lieut. Maule's letter.

A specimen was exhibited of a claw obtained from the tip of the tail of a young *Lion* from Barbary, recently presented to the Society's Menagerie by Sir Thomas Reade, His Majesty's Consul at Tripoli. It was detected on the living animal by Mr. Bennett, and pointed out to the keeper, in whose hands it came off while he was examining it.

Mr. Woods, to whom the specimen had been submitted for description, communicated to the Committee an enlarged representation of it, with other illustrations of the subject, and gave a detailed account of previous observations bearing upon this curious formation.

He commenced by referring to the writings of Homer, who remarked (erroneously, however,) that the Lion when angry lashes his sides with his tail; a remark which was repeated by many of the ancient poets both Greek and Roman, and was carried by Lucan to a yet greater extent, when he stated that the Lion lashes himself into rage: Pliny also indicates his belief that by this means the animal increases the anger already kindled in him. None of these writers, however, advert to any peculiarity in the tail of the Lion to which so extraordinary a function might, however incorrectly, be attributed. The discovery of the existence of such a peculiarity was reserved for Didymus Alexandrinus, one of the early commentators on the Iliad, who found a black prickle, like a horn, among the hair of the tail, and immediately conjectured, it must be allowed with some degree of plausibility, that he had ascertained the true cause of the stimulus to the animal when he flourishes his tail in defiance of his enemies, for he remarks that when punctured by this prickle the Lion becomes more irritable from the pain which it occasions.

For centuries after this announcement the Lion's tail and its mysterious prickle were consigned to oblivion, the discovery of the learned commentator being either unnoticed, or disregarded, or doubted, until about twenty years since, when M. Blumenbach, in his 'Miscellaneous Notices in Natural History,' revived the subject, having verified the accuracy as to the fact, though not admitting the induction, of Didymus Alexandrinus. He describes a small dark-coloured prickle in the very tip of the Lion's tail, as hard as a piece of horn, surrounded at its base by an annular fold of the skin, and adhering firmly to a singular follicle of a glandular appearance. All these parts were however, he remarks, so minute, and the little horny *apex* so buried in the tuft of hair, that the use attributed to it by the ancient scholiast cannot be regarded as any thing else than imaginary. Blumenbach's description was accompanied by a figure, which was copied in the 'Edinburgh Philosophical Journal,' in the 8th volume of which a translation of his paper was given.

The subject appears to have again slumbered until 1829, when M. Deshayes announced, in the 'Annales des Sciences Naturelles' (vol. vii. p. 79), that he had found the prickle on both a Lion and Lioness which died in the national Menagerie of France. It was described by him as a little nail or horny production, about two lines in length, presenting the form of a small cone, a little recurved upon itself, and adhering by its base only to the skin and not to the last caudal *vertebra*, from which it was separated by a space of 2 or 3 lines.

From the period when M. Deshayes' discovery was announced Mr. Woods has suffered no opportunity to escape him of examining the tails of every Lion, living or dead, to which he could gain access; but n no instance has he succeeded in ascertaining the existence of such an organ; nor had he ever observed it until the specimen now before the Committee was placed in his hands, within half an hour after its removal from the living animal, and while yet soft at its base where it had been attached to the skin.

It is formed of corneous matter like an ordinary nail, and is solid throughout the greater part of its length towards the *apex*, where it is sharp; at the other extremity it is hollow and a little expanded. Its shape is rather singular, being nearly straight for one third of its length, then slightly constricted, (forming a very obtuse angle at the point of constriction,) and afterwards swelling out like the bulb of a bristle to its termination. It is laterally flattened throughout its entire length, which does not amount to quite $\frac{3}{8}$ths of an inch. Its colour is that of horn, but becoming darker, nearly to blackness at the tip. Its appearance would lead to the belief that it was deeply inserted into the skin, with which, however, from the readiness with which it became detached, its connexion must have been very slight. The slightness of its adhesion is noticed by M. Deshayes, who attributes to this its usual absence in stuffed specimens. The same cause will account for its absence in by far the greater number of living individuals; for, as Mr. Woods remarks, its presence or absence does not depend upon age, as the Lions at Paris in which it was found were of considerable size, while that belonging to the Society is very small and young; nor upon sex, for although it is wanting in the female cub of the same litter at the Society's Gardens, it existed in the Lioness at the Jardin du Roi.

Mr. Woods, considering it probable that a similar structure might exist in other species of *Felis*, had previously examined the tails of nearly the whole of the stuffed skins in the Society's Museum, but failed in detecting it in every instance but one. This was in an adult Asiatic *Leopard*, in which the nail was evident although extremely small. It was short and straight, and perfectly conical, with a broad base. It is stated in a note in the 'Edinburgh Philosophical Journal,' that a claw or prickle had also been observed by the editor of that work on the tail of a Leopard. No such structure was however detected on a living individual in the Society's Menagerie. In the Leopard, therefore, as in the Lion, it appears to be only occasionally present. In both it is seated at the extreme tip of the tail, and is altogether unconnected with the terminal caudal *vertebra*. From the narrowness and shape of its base, the circumference of which is by far too small to allow of its being fitted like a cap upon the end of the tail, it appears rather to be inserted into the skin, like the bulb of a bristle or *vibrissa*, than to adhere to it by the margin as described by M. Deshayes. Neither the published observations of that zoologist nor the present discovery, can throw any light on the existence or structure of the supposed glandular follicle noticed by Blumenbach.

Mr. Woods concluded his communication by remarking, that it is difficult to conjecture for what purpose these minute claws are developed in so strange a situation, that of stimulating the animals to anger being of course out of the question. It is at least evident, he observes, that they can fulfil no very important design in the animal œconomy, from their smallness, their variable form, their complete envelopement in the fur, and especially from the readiness with which they are detached and consequently the majority of individuals deprived of them for the remainder of their lives.

XV. *Intelligence and Miscellaneous Articles.*

ACTION OF SULPHUROUS ACID ON THE PERSALTS OF IRON.

IT is well known that sulphurous acid when added to the solutions of those metals which possess a weak affinity for oxygen, such as platinum, gold and mercury, precipitates them in the metallic form.

I find it stated also in the last (fourth) edition of Dr. Turner's excellent work, the Elements of Chemistry (p. 274), that when sulphurous acid is mixed with peroxide of iron in solution, it deprives that compound of part of its oxygen, and converts it into protoxide.

This is an effect which it would be natural to anticipate; and yet I apprehend that such is not the case; for when a solution of sulphurous acid is added to one of persulphate of iron, the colour of the solution, instead of being changed from red or reddish yellow to blueish green, becomes a very deep red; and I have found that if a grain of protosulphate of iron be converted by nitric acid into persulphate, its presence in a pint of water may be detected by adding sulphurous acid, the solution becoming slightly yellowish *. When the solution of persulphate of iron is moderately strong, the intensity of the colour is so much increased by sulphurous acid, as to resemble the effect produced by sulphocyanic acid; so that I think it extremely probable that what has been caused by the former may have been attributed to the latter. It is however to be observed, that the colouring effect produced by the sulphurous acid disappears in a few hours, which is not the case with that derived from sulphocyanic acid. R. P.

IMPROVEMENT IN THE QUALITY OF IRON AND STEEL, FROM THEIR BECOMING RUSTY WHEN BURIED IN THE EARTH.

The following "extract from the Chronicles of Old London Bridge," is sufficiently curious in itself to merit insertion in the Philosophical

* The red colour of the solution is presumptive evidence that the peroxide of iron is not reduced to the state of protoxide by the sulphurous acid; and this conclusion is strengthened by considering the action of ferrocyanate of potash, which gives prussian blue, even when the sulphurous acid is greatly in excess, and after the red colour which it had produced has disappeared.

Magazine and Journal of Science, and as an instance of observation, ingeniously applied.

An eminent London cutler, Mr. Weiss of the Strand, to whose inventions modern surgery is under considerable obligations, has remarked, that steel seemed to be much improved when it had become rusty in the earth, and provided the rust was not factitiously produced by the application of acids*. He accordingly buried some razor blades for nearly three years, and the result fully corresponded to his expectation; the blades were coated with rust, which had the appearance of having exuded from within, but were not eroded, and the quality of the steel was decidedly improved. Analogy led to the conclusion, that the same might hold good with respect to iron under similar circumstances; so with perfect confidence in the justness of his views, he purchased, as soon as an opportunity offered, all the iron, amounting to fifteen tons, with which the piles of London Bridge had been shod. Each shoe consisted of a small inverted pyramid, with four straps rising from the four sides of its base, which embraced and were nailed to the pile; the total length from the point which entered the ground to the end of the strap being about 16 inches, and the weight about 8 lbs.

The pyramidal extremities of the shoes were found to be not much corroded, nor indeed were the straps; but the latter had become extremely and beautifully sonorous, closely resembling in tone the bars and sounding pieces of an Oriental instrument which was exhibited some time since, with the Burmese state carriage. When manufactured, the solid points in question were convertible only into very inferior steel: the same held good with respect to such bolts and other parts of the iron work as were subjected to the experiment, except the straps; these, which in addition to their sonorousness, possessed a degree of toughness quite unapproached by common iron, and which were in fact imperfect carburets, produced steel of a quality infinitely superior to any which in the course of his business Mr. Weiss had ever before met with; insomuch, that while it was in general request among the workmen for tools, they demanded higher wages for working it †. These straps, weighing altogether about eight tons, were consequently separated from the solid points, and these last sold as old iron. The exterior difference between the parts of the same shoe led at first to the supposition that they were composed of two sorts of iron; but, besides the utter improbability of this, the

* This enterprising artist has informed me, that "some years since he sent with Captain Parry, in his voyage to the North Pole, some steel, which was constantly exposed on deck in the Northern latitudes without being in the slightest degree rusted; but on arriving in a warmer and moister atmosphere it became so. This steel he found very good, but not equal to that from London Bridge."

† A successful application of genius or observation is rarely heard of without some one endeavouring to reap the benefit or the credit of the discovery, whileentitled to neither, nor perhaps understanding the principle on which it depends. The fame of Mr. Weiss's steel soon spread, when another person immediately purchased the bolts and fastenings of the old Bridge: the articles manufactured from them will answer equally well as *relics*.

contrary was proved by an examination, which led to the inference that the extremities of the piles having been charred, the straps of iron closely wedged between them, and the stratum in which they were imbedded, must have been subjected to a galvanic action, which in the course of some six or seven hundred years gradually produced the effects recorded in the present paper. T. J. H.

CAOUTCHOUC.

Few persons are perhaps aware of the comparatively late introduction of Indian Rubber into this country. The following notice is appended by Dr. Priestley to the preface to his *Familiar Introduction to the Theory and Practice of Perspective,* printed in 1770; and it will be observed that no name is given to the substance described: "Since this work was printed off, I have seen a substance excellently adapted to the purpose of wiping from paper the marks of a black-lead pencil. It must, therefore, be of singular use to those who practise drawing. It is sold by Mr. Nairne, Mathematical Instrument Maker, opposite the Royal Exchange. He sells a cubical piece, of about half an inch, for three shillings; and he says it will last several years."

FORMATION OF ÆTHER BY FLUORIDE OF BORON.

MM. Wöhler and Liebig in order to examine the formation of æther as stated by Desfosses, passed fluoboric gas into absolute alcohol. Much of it was absorbed, great heat was excited, and the solution became gelatinous, transparent and fuming. A small portion was saturated with potash; no æther separated, but the liquid had a peculiar odour, totally different from that of æther, but very agreeable; when distilled after dilution with water, it yielded more alcohol. That which had been saturated with fluoboric gas, when distilled gave a colourless liquor, from which water separated a notable quantity of pure æther.

Fluosilicic gas when passed into alcohol to saturation, gave no gelatinous mass, nor the smallest quantity of æther; as Berzelius has already stated. The action which has been attributed, in the formation of æther by sulphuric acid, to the sulphovinic acid produced, appears therefore to be very problematical.—*Ann. de Chim.* tom. xlix. p. 30.

PEROXIDE OF BARIUM.

The hydrated peroxide of barium employed to prepare peroxide of hydrogen, may be obtained, according to MM. Wöhler and Liebig, with the greatest facility by the following process. Heat caustic barytes in a platinum crucible, by means of a spirit-lamp, until it is nearly red hot, and then throw in, by small quantities at a time, chlorate of potash; incandescence takes place, and the protoxide becomes peroxide of barium. When the mass is cold, dissolve the chloride of potassium by solution in cold water; the peroxide becomes a hydrate during this operation, and remains in the state of a white powder. It may be dried by exposure to the air, without heat. It appears to contain six atoms of water.

The yellow oxide of lead fused with chlorate of potash, is readily converted into peroxide. Green oxide of chrome treated in the same manner, gives neutral chromate of potash, attended with a copious evolution of chlorine.—*Ann. de Chim. et de Phys.* tom. xlix. p. 30.

ANALYSIS OF PARAFFINE.

M. Jules Gay-Lussac has analysed this substance (for an account of which see Lond. and Edin. Phil. Mag. and Journ. vol. i. p. 402.) by means of oxide of copper, and obtained such quantities of carbonic acid and water as showed that it consisted of

Carbon	85·23
Hydrogen	14·99
	100·22

These, he remarks, are in the same proportions as form olefiant gas; and he considers this compound as equivalent to one atom of carbon and two atoms of hydrogen: but adopting the atomic weights usual in this country, it is a compound of one atom of each, or of

Carbon . . .	6	85·7
Hydrogen . .	1	14·3
	7	100·0

Ibid. tom. l. p. 78.

RED OXIDE OF PHOSPHORUS.

M. Pelouze observes, that there remains after the combustion of phosphorus, a red insoluble residue, which has generally been regarded as an oxide of phosphorus. M. Pelouze prepared this substance in the mode proposed by Berzelius, which consists in melting phosphorus in boiling water, and passing a current of oxygen gas into it; the phosphorus burns under water, phosphoric acid is formed and dissolves, and the oxide of phosphorus floats in the liquor in the form of cinnabar-red flocks. When the combustion ceases, the clear acid liquor is poured off, the oxide is washed and put into a retort; water passes over first, and afterwards the phosphorus which had adhered to the oxide; this remains in the retort, and is to be separated from a little phosphoric acid by water.

The oxide thus obtained was exposed for three days to a vacuum over sulphuric acid to dry it. The properties of this oxide are as follows: it is red, inodorous, and tasteless; it is denser than water, and completely insoluble in it; as also æther, alcohol, the fixed and essential oils. It is not luminous in the dark, even when quickly rubbed between two rough bodies, such as pieces of cork.

When heated in contact with the air nearly to dull red, it inflames; when put into a tube placed in boiling mercury, it does not burn; when it does burn it is converted entirely into phosphoric acid; when heated in a closed tube, it separates into phosphorus, which distils, and phosphoric acid. Cold sulphuric acid does not act upon it; when they are heated together, the results are the phosphoric and sulphurous acids. The nitric and nitrous acids attack it with extreme energy; they inflame it suddenly, and convert it into phosphoric acid: this is remarkable, considering that the red oxide resists the influence of heat and air much better than phosphorus, upon which cold nitric acid has scarcely any action.

It is probable that the extreme energy of this combustion depends upon the minute division of the oxide, and that phosphorus would exhibit similar phænomena if it were possible to obtain it in very fine powder.

The red oxide of phosphorus thrown into either dry or moist chlorine suddenly inflames, and phosphoric acid and perchloride of phosphorus are formed. It detonates with extreme violence when put in contact in the cold with chlorate of potash. Sulphur decomposes it only when near its fusing point, and without detonation. The oxide was analysed by converting it into phosphoric acid by means of the nitric acid, and ascertaining the quantity of oxygen it absorbed. M. Pelouze gives as its composition,

Phosphorus	85·5
Oxygen	14·5
	100·0

Now if, with Dr. Thomson, an atom of phosphorus be reckoned 16, a compound of three atoms of phosphorus and one of oxygen would consist of

Phosphorus	85·7
Oxygen	14·3
	100·0

—*Ibid.* tom. l. p. 83.

HYDRATE OF PHOSPHORUS.

M. Pelouze states, that this matter, which was supposed to be hydrated oxide of phosphorus, is white, insipid, and insoluble in water. Its odour is similar to that of phosphorus, and it is also luminous in the dark. Its density is 1·515 at 60° Fahr.; when put into contact with cold sulphuric acid, it is decomposed, giving its water to the acid, and the phosphorus is set free.

At a temperature of 110° of Fahr. and even a little under, the hydrate of phosphorus decomposes also into phosphorus, which appears with all its physical and chemical properties, and water. It does not contain more oxide than phosphorus which has been distilled, and is perfectly transparent.

In order to determine the quantity of water contained in this compound, it was dried on filtering paper, and heated in a weighed tube placed in water heated to 112° Fahr. The water was absorbed by paper, and found to amount to 12·53 per cent. A compound of four atoms of phosphorus and one atom of water would consist of

Phosphorus	87·48
Water	12·52
	100·00

—*Ibid.*

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

Days of Month, 1832.	Barometer.					Thermometer.					Wind.			Rain.		
	London.		Penzance.		Boston $8\frac{1}{2}$ A.M.	London.		Penzance.		Bost. $8\frac{1}{2}$ A.M.	Lond.	Penz.	Bost.	Lond.	Penz.	Bost.
	Max.	Min.	Max.	Min.		Max.	Min.	Max.	Min.							
Nov. 1	29·926	29·591	29·822	29·775	29·04	61	43	57	47	56	W.	NW.	NW.	0·02	0·200	0·21
2	29·841	29·490	29·822	29·616	29·30	59	50	58	53	46	SW.	W.	calm	·06	·060	...
3	29·819	29·703	29·922	29·822	29·07	55	44	56	50	50·5	W.	NW.	W.	·02	·060	.19
4	29·871	29·654	29·928	29·928	29·26	51	31	53	48	40	W.	NW.	calm	·04	·085	·04
5	30·069	29·712	30·078	29·834	29·29	49	39	48	40	37	N.	N.	NW.	...	·150	...
6	30·410	30·279	30·308	30·290	29·87	46	38	48	40	44	NE.	NE.	NE.	·01	...	·21
7	30·439	30·295	30·434	30·426	30·00	47	36	48	40	41	NW.	NE.	calm	...	...	...
○ 8	30·100	29·953	30·193	30·134	29·67	41	35	46	38	43·5	NE.	NE.	calm	·02	...	·10
9	29·938	29·902	29·937	29·628	29·50	47	33	55	42	40	NE.	SW.	calm	...	...	·08
10	29·749	29·512	29·428	29·328	29·33	45	32	56	44	41	SE.	SW.	calm	·39	·975	...
11	29·639	29·605	29·678	29·551	29·16	54	34	50	44	41	SW.	NW.	calm	·08	·250	·37
12	29·762	29·642	29·684	29·678	29·32	43	35	48	42	34	SW.	W.	calm	·03	...	...
13	29·819	29·786	29·681	29·628	29·41	51	35	53	42	32	SE.	W.	calm	...	·200	...
14	29·795	29·739	29·728	29·551	29·26	52	41	50	48	47·5	S.	NE.	W.	·14	1·590	...
☾ 15	30·124	29·879	29·928	29·828	29·53	50	42	50	44	44	NE.	NE.	calm	·02	...	·23
16	30·410	30·313	30·328	30·228	29·88	50	32	48	42	36	NE.	NE.	calm	...	...	·22
17	30·482	30·424	30·331	30·128	30·00	48	37	52	40	30	E.	NE.	calm	...	...	...
18	30·207	30·050	30·031	29·822	29·80	45	41	51	44	40·5	SE.	SE.	calm	·02	...	...
19	29·976	29·862	29·795	29·619	29·58	50	43	54	45	41	SE.	SW.	calm	...	·280	...
20	29·759	29·680	29·522	29·466	29·26	53	39	54	50	45	E.	SW.	SE.	...	·440	...
21	29·666	29·618	29·402	29·402	29·24	48	43	54	48	44	NE.	SE.	E.	...	...	...
● 22	29·930	29·783	29·666	29·542	29·40	54	32	55	50	44	E.	S.	calm	...	·150	...
23	30·031	29·951	29·763	29·672	29·55	57	39	55	48	41·5	SE.	SW.	calm	...	·165	...
24	30·057	30·006	29·896	29·766	29·55	54	46	54	50	47	S.	S.	calm	·18	·500	...
25	29·912	29·789	29·925	29·582	29·44	51	38	51	40	48	W.	SW.	calm	·24	·590	...
26	29·751	29·567	29·722	29·625	29·11	51	30	48	42	45·5	SW.	NW.	SE.	...	·120	·53
27	29·751	29·469	29·528	29·522	29·27	45	38	54	42	37	S.	SW.	calm	·11	·750	...
28	29·772	29·653	29·728	29·622	29·15	51	39	53	44	40	W.	NW.	W.	·26	·145	·22
29	29·636	29·591	29·728	29·634	29·06	47	33	47	44	41	SW.	NW.	W.	...	·130	·13
☽ 30	29·882	29·778	29·914	29·908	29·34	52	41	54	43	38	SW.	NW.	calm	·30	·050	·06
	30·482	29·469	30·434	29·328	29·42	61	30	58	38	41.8				1·94	6·890	2·59

Remarks.

London.—November 1. Stormy and wet: fine at night. 2. Rain. 3. Fine: showery. 4. Fine. 5. Cloudy: fine. 6. Showery: clear at night. 7. Fine. 8. Overcast. 9. Foggy. 10. Rain: very clear at night. 11. Foggy: rain. 12. Hazy. 13. Foggy. 14. Hazy: rain. 15. Foggy. 16, 17. Fine. 18. Hazy. 19. Foggy. 20. Fine. 21, 22. Foggy. 23, 24. Foggy mornings: very fine. 25. Rain. 26. Clear, and fine. 27. Foggy: rain. 28. Clear: stormy and wet at night. 29. Clear: lightning at night. 30. Clear: rain.

Penzance.—November 1. Rain: showers. 2—4. Showers. 5. Showers, hail and rain. 6. Fair. 7. Clear. 8. Fair. 9. Fair: rain. 10. Rain. 11. Showers. 12. Fair. 13. Fair: showers. 14. Rain throughout. 15, 16. Fair. 17. Clear. 18. Fair. 19. Fair: rain. 20. Showers. 21. Fair. 22. Fair: rain. 23, 24. Rain: fair. 25. Fair: rain. 26. Showers, hail and rain. 27. Rain: fair. 28. Showers, hail and rain 29. Showers. 30. Misty: rain.

Boston.—November 1. Cloudy: rain early A.M. 2. Rain. 3. Fine: rain P.M. 4. Fine. 5. Fine: showery A.M. and P.M. 6. Cloudy. 7. Fine. 8. Rain. 9. Cloudy: rain and stormy P.M. 10. Rain: rain and stormy P.M. 11, 12. Fine. 13. Misty. 14. Cloudy. 15. Cloudy: rain early A.M. and again P.M. 16. Fine. 17. Rime frost. 18—20. Cloudy. 21—23. Fine. 24. Cloudy. 25. Rain. 26. Cloudy: rain early A.M. 27. Fine: rain A.M. and P.M. 28. Fine. 29. Fine: rain P.M. 30. Cloudy.

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XVI. *On the Modification of the Interference of two Pencils of Homogeneous Light produced by causing them to pass through a Prism of Glass, and on the Importance of the Phænomena which then take place in determining the Velocity with which Light traverses refracting Substances. By* R. POTTER, *Jun. Esq.**

[With Figures: Plate II.]

THE investigation which is the principal subject of the present paper, arose from my meeting with a peculiar phænomenon in interference, whilst repeating, in a new mode, an experiment first described by Professor Powell in a former Number of the Philosophical Magazine†. The experiment consists in placing a prism of glass in the direction of two pencils of light which interfere; these pencils arising from the reflection of the light which diverges from the image of the sun, given by a lens of short focus, by two mirrors slightly inclined to each other. Professor Powell, by using a prism with a refracting angle of only a few degrees, and common light, believed that he found the same parts of the luminous pencils to interfere, after refraction by the prism, which would have interfered if it had not been interposed, and that the only alteration was in the direction of the pencils, produced by the refraction. I saw that this experiment merited a more rigorous examination than Professor Powell had given to it; and I adapted to an apparatus for trying the experiment of M. Fresnel by two mirrors slightly inclined to each other, a piece carrying a small prism having its smallest refracting

* Communicated by the Author.

† See Phil. Mag. and Annals, N.S. vol. xi. p. 1; and the preceding volume of the present Journal, p. 433.—EDIT.

angle about 43 degrees. By using a homogeneous light produced by a coloured solution and two plane mirrors of speculum metal, or rather one mirror polished as for a Newtonian telescope and then cut into two across the middle, I obtained sufficient brightness to trace readily the whole phænomena. I immediately found that Professor Powell's conclusion from using the prism with a small angle, was premature, and that the same portions of the pencils did not interfere after refraction which would have interfered before, if the prism had not been interposed; but that interference then took place between rays which had passed at a greater distance from the angle of the prism. Another phænomenon which greatly attracted my attention was, that when the eye and eye-glass were withdrawn further from the prism, the interference occurred between other parts of the pencils which had passed at still greater distances; and that when the eye and eye-glass were withdrawn still further, all appearances of interference at length ceased. This last effect arose where the prism became too narrow to allow a sufficient breadth of the pencils to pass, or when the mirrors were not sufficiently inclined to each other to give the required overlapping of the pencils for interference to take place at such a distance from the prism. The inflected or diffracted bands produced by the edges of the mirrors give a certain criterion by which to judge of this other interfering light; and in the act of drawing the eye and eye-glass from the prism, we see the phænomena which we consider traverse gradually over those inflected or diffracted bands, and finally become as gradually lost in the single light of the other mirror. This appearance takes place whatever may be the angle at which the pencils emerge from the prism. Another fact which demands equal attention is, that the breadths of the fringes produced by the interference vary with the inclination of the light to the surfaces of the prism: thus from the angle of minimum deviation towards a perpendicular incidence on the first surface, the fringes become narrower and narrower, and on the contrary side of the angle of minimum deviation they grow larger.

The above will be more easily comprehended on referring to fig. 1, where a and b are the images of the luminous point o, produced by the two mirrors $f g$ and $g h$; a' and b' the secondary images after refraction by the prism $d c e$. Let $a i$ and $b k$ be the axes of the pencils drawn perpendicular to the line joining the points a and b. Let $m n$ be a line parallel to $a i$ and $b k$, and exactly intermediate between them. After refraction, these lines must be considered as in the directions $a' i'$, $b' k'$, $m' n'$. Now where the prism does not intervene, the central band produced by the interference of the light, which proceeds as from the points *a* and *b*, is always on the line *mn*, and the other bands are symmetrical on each side of it. After refraction by the prism, the central band is no longer on the new line $m' n'$, but follows another direction similar to *p q*, which, when we come to examine this subject mathematically, we shall find leads us to very important conclusions.

As I shall have to consider the points at which interference should take place according to the Newtonian hypothesis,—that light moves with greater velocity in passing through bodies, in the direct ratio of their refractive indices,—I shall first show how we may expect interference to take place, on the theory that the phænomena of light arise from the effects of a subtile matter which is emitted by luminous bodies. The discovery of interference renders certain conditions now essential in every theory of light. These are: 1st, That luminiferous surfaces expand around each luminous point, which expanding surfaces tend to as near a spherical form as the circumstances of the luminous body permit.

2nd, That these luminiferous surfaces, or shells, succeed each other at regular intervals, which differ for the different colours of the spectrum, and are for each colour exactly the double of Newton's fits of easy reflection and transmission.

Those who admit the material theory of light, generally allow that light and heat are mutually convertible; and many facts may be adduced which confirm this view. Now heat perpetually radiates without causing any impression of light in the eye, and we must suppose that interfering rays of light produce an effect on the organ of vision similar to radiant heat; so that when two rays arrive on the *same path* in juxtaposition, they cease, either by their combined bulks being too large, or by some other property unknown to us, to create that sensation which we call light. That the eye has only a very limited power of appreciating the impressions which may be conveyed to it, the advocates of every theory must allow.

There is a fundamental difference, however, between this way of viewing interference and that adopted on the undulatory theory; for on this view the rays would neutralize each other's effect, while on the undulatory theory they should strengthen it, and *vice versâ*; that is, there should be a difference in the effects equal to the half of an intervening space, or half the breadth of an undulation. In one theory the effect of brightness should ensue where darkness should arise according to the other. The unfortunate half undulation which has continually to be asked for by those who adopt the undulatory theory of light, to make their theoretical deductions agree

M 2

with nature, and for which they have offered but very unsatisfactory arguments, I must claim as strongly in favour of the view which I have proposed. There are, indeed, cases in which the undulatory theory affords a direct application, as in the *transmitted* colours of thin plates, and in the fringes within the shadows of narrow bodies: but on a careful review we must allow that these cases arise in less simple circumstances than those exceedingly numerous ones in which the claim of half an undulation has to be made.

Having shown how interference may arise according to the theory that light is caused by an emitted matter, I shall proceed to the mathematical investigation of the experiment with the prism, which has been before described.

To find the central points of interference after the refraction of the pencils by the prism, requires the previous consideration of three distinct questions; namely, 1st, The positions of the secondary images of the luminous point, or the centres of divergence of the rays after the two refractions: 2nd, The simultaneous positions of the luminiferous surfaces: 3rd, The figure of the curve of the principal section of these surfaces;—the plane of this section being common to both pencils.

The first question involves only the common rules of optics; the other two require, in addition, the application of the respective theories as to the velocity with which the light passes through the substance of the prism.

From the properties of the prism we cannot rigorously consider the light which emanates from a luminous point before refraction to do so afterwards, excepting for very small pencils near the angle of minimum deviation; for which reason I shall only investigate the principal part of the problem on this supposition, of exceedingly small pencils, incident on the prism near the angle of minimum deviation. I do not nevertheless consider that it would lead to objectionable results for other incidences: in making the corresponding experiments, however, some care would be requisite to be taken, to preserve the direction of the bands perfectly parallel to the line bounding the angle of the prism. We find that interference, according to the rules, is not prevented by even the great degree of aberration which exists in the focus of the lens we use to form an image of the sun, and which, though of considerable dimension, we consider in calculation as a luminous point. The effect of these imperfections *generally* tends to render the phænomena less distinct and less sharply exhibited; or, where it was required to determine whether the central band were a bright or a dark one, this aberration would present a serious obstacle.

To find the positions of the secondary images of the luminous point after the passage of the small pencils through the prism, we will suppose one of the primary images at o, fig. 2; then after refraction at the first surface the pencil will diverge as from another point p, such that r being put for the distance $o\,r$, and r' for the distance $p\,r$, we have

$$r' = r\,\mu\,\frac{\cos^2 i'}{\cos^2 i},$$

i being the angle of incidence on the first surface, i' that of refraction, and μ the refractive index of the glass. (This and several other equations which I shall have to introduce being demonstrated in Mr. Coddington's excellent later treatise on Optics, I shall here use them without further explanation.) Then t being put for the thickness of the glass passed through, we have for $r^{\backprime}$ or $q\,s$ this equation:

$$r^{\backprime} = (r' + t)\frac{\cos^2 i}{\mu\cos^2 i'} = r + t\,\frac{\cos^2 i}{\mu\cos^2 i'}.$$

Now, of the two interfering pencils we may take the axis of one, or the line perpendicular to the line joining the two primary images of the luminous point, as passing through the very angle of the prism where $t = 0$; and hence $r^{\backprime}_{\prime} = r_{\prime}$.

Then the incidence of the axes of the pencils being that of minimum deviation, and $2a$ being put for the distance $a\,b$, and i for the refracting angle of the prism, we have

$$t = 2 \times \text{distance } c\,d \times \tan\tfrac{1}{2}\,i$$

$$c\,d = \text{distance } a\,b \times \frac{\cos i'}{\cos i} = 2a\,\frac{\cos i'}{\cos i},$$

the angle $e\,r\,c$ being equal to the angle of incidence, and the angle $r\,c\,d$ equal to the angle of refraction. Hence

$$t = 4a\,\frac{\cos i'}{\cos i}\tan\tfrac{1}{2}\,i = 4a\,\frac{\cos i'}{\cos i}\tan i' = 4a\,\frac{\sin i'}{\cos i}, \text{ and}$$

$$r^{\backprime} = r + 4a\,\frac{\sin i'}{\cos i}\cdot\frac{\cos^2 i}{\mu\cos^2 i'} = r + 4a\,\frac{\cos i\,.\,\sin i'}{\mu\cos^2 i'} = a's \text{ in}$$

the figure.

Then, drawing $s\!f$ perpendicular to $b^{\backprime}c$ produced, we have the angle $c\,s\!f = i$, and $s\!f = 2a$. Hence

$$\text{the distance } c\!f = 2\,a\tan i,$$

and $g\,b'$ being drawn perpendicular to $b'c$ and $a's$, we have

$$\text{the distance } g\,a' = (r_{\prime} + 2a\tan i) - \left(r + 4a\,\frac{\cos i\ \sin i'}{\mu\cos^2 i'}\right).$$

From these equations we find the positions of the *secondary* images of the luminous point, a' and b'.

To find the simultaneous positions of the luminiferous surfaces on the axes of the pencils after the two refractions, which are supposed to depart simultaneously from a and b.

We have first, $e\,c$ or $c\,f = c\,r$ or $c\,s \times$ sine incidence, and $r\,d$ or $s\,d = c\,r$ or $c\,s \times$ sine refraction

$$e\,c = r\,d \times \frac{\sin i}{\sin i'} = r\,d \times \mu$$

and $e\,c = 2\,a \tan i$; hence $r\,s = 2 \times r\,d = 4\,a\,\dfrac{\tan i}{\mu}$.

Now let the velocity of light in air be to the velocity in glass as 1 to m. Then when the upper ray arrives at s, the lower one will be at a point in its path, with respect to the point f, represented by this expression:

$$(e\,c + c\,f) \backsim \frac{r\,s}{m};$$

$$\text{or, } 4\,a \tan i \backsim 4\,a\,\frac{\tan i}{m\,\mu} = 4\,a \tan i \left(1 \backsim \frac{1}{m\,\mu}\right).$$

On the undulatory theory m is supposed to be the reciprocal of the refractive index; or we have $m = \dfrac{1}{\mu}$. Then the above expression, which we may call the difference of the paths, or the difference in the simultaneous positions of the luminiferous surfaces counted on the axes of the pencils, becomes this:

$$\Delta \text{ paths} = 4\,a \tan i \left(1 - \frac{\mu}{\mu}\right) = 4\,a \tan i\ (o)\ 0.$$

On the Newtonian hypothesis, that the velocity is directly as the refractive index, we have $m = \mu$,

$$\text{and } \Delta \text{ paths} = 4\,a \tan i \left(1 - \frac{1}{m\,\mu}\right) = 4\,a \tan i \left(1 - \frac{1}{\mu^2}\right)$$

$$= 4\,a \tan i \left(\frac{\mu^2 - 1}{\mu^2}\right).$$

The last preliminary question to be examined, or that of determining the curve of the principal section of the luminiferous surface after refraction, requires the introduction of differentials and the method of polar coordinates. Taking two rays $a\,o$ $a\,p$ (fig. 3), as indefinitely near each other, and diverging from the point a, we may take the indefinitely small and perpendicular distances between the rays in the prism $p\,n$ and $r\,s$ as equal to h and h'; and now calling the variable angle of incidence on the first surface φ, the corresponding angle of refraction φ'; the angle of incidence on the second surface ψ', and the corresponding angle of emergence ψ:—

We have, as will be easily seen from what has preceded, the difference of the thickness of glass which has been passed through by the two rays, equal to

$$\text{the distance } (on + sq) = h \tan \phi' + h' \tan \psi',$$

and the differences in air equal to

$$\text{the distance } (mp + rt) = \mu h \tan \phi' + \mu h' \tan \psi'.$$

Now these rays diverging from another point a' after the refractions, their relative positions will depend on the velocity with which they have traversed the glass of the prism; and by an analogous procedure to that which we used in the last article, and considering $r`$ now to be variable, we find the differential of the radius vector $r`$ to be

$$d r` = \mu\,(h \tan \phi' + h' \tan \psi') \backsim \frac{h \tan \phi' + h' \tan \psi'}{m},$$

or, $$d r` = (h \tan \phi' + h' \tan \psi') \left(\varphi \backsim \frac{1}{m}\right).$$

We may now apply the theoretical values for m; and according to the undulatory theory where $m = \frac{1}{\mu}$, we have

$$dr` = (h \tan \phi' + h' \tan \psi')\left(\mu - \frac{1}{\frac{1}{\mu}}\right) = (h \tan \phi' + h' \tan \psi')(\mu - \mu),$$

and $\int d r` = r` = \text{constant};$

which is independent of the values of φ' and ψ', and we recognize the polar equation of the circle referred to the centre.

As this equation has been arrived at rigorously, without any approximate considerations, and as we cannot integrate in the same rigorous manner for the Newtonian hypothesis, I shall proceed in the first place to the examination where interference should arise according to the undulatory theory. Referring therefore to fig. 4, and taking the positions of the *secondary* images of the luminous point, as we found them, in a' and b', and the simultaneous positions of the undulations on the axes of the pencils as we found them to be, on the same perpendicular sf, we have, making the point m' the origin of the rectangular coordinates, $m'g = a' = m'b'$, $b'g = 2a'$, which will always bear a determinate ratio to $2a$ depending on the incidence, and at the angle of minimum deviation it will be that of equality or $2a' = 2a$, in which case we now take it: and as we found before that we may calculate the distance ga' in terms of $2a$, we will call this distance ma. Then m' being the origin, and the lines $m'y$, $m'x$ the axes of the coordinates, we shall have for the equations of these circles whose centres are in a' and b', as follows:

$$(x - m\,a)^2 + (y - a)^2 = r'^2 \qquad x^2 + (y + a)^2 = r^2,$$

$$\text{and } r = r' + m\,a$$

for the central points of interference.

Substituting, developing, and subtracting, we find

$$2\,y + m\,x = m\,r = m\,\sqrt{x^2 + (y + a)^2};$$

raising to the square, and bringing all the terms to one side, we have

$$(4 - m^2)\,y^2 + 4\,m\,x\,y - 2\,m^2 a\,y - m^2 a^2 = 0,$$

which we find to be the equation of an hyperbola. Differentiating this equation, we find the differential coefficient

$$\frac{d\,y}{d\,x} = -\,\frac{4\,m\,y}{2(4 - m^2)\,y + 4\,m\,x - 2\,m^2 a},$$

we see that this equation becomes zero when $y = 0$, but on account of the constant quantity in the equation of the curve, this can only take place at the same time that x is infinite, to fulfil the conditions: hence the axis of the abscissæ is tangent to the curve at an infinite distance, and one of its asymptotes, as we may also learn from the geometrical construction, fig. 5.

We learn from this, that the central band produced by the interference of two luminous pencils after passing through a prism of glass, should, according to the *undulatory* theory, nearly coincide with the intermedial line $m'n'$ (fig. 1. and 5), and slightly tend towards the angle of the prism instead of from it, as we find by experiment. Hence the undulatory theory gives no account of this phænomenon.

According to the other theory,—that light travels through bodies with a velocity which is directly as their refractive indices,—on recurring to the general equation

$$d\,r' = (h \tan \phi' + h' \tan \psi')\left(\mu \backsim \frac{1}{m}\right),$$

we have, by putting for m its value μ,

$$d\,r' = (h \tan \varphi' + h' \tan \psi')$$

$$\left(\mu - \frac{1}{\mu}\right) = (h \tan \varphi' + h' \tan \psi')\left(\frac{\mu^2 - 1}{\mu}\right).$$

On referring to fig. 3, we see that we may write for h its identical expression $r'\,d\,\varphi'$; then considering h' as equal to h, and noting that r' increases as φ and φ' decrease, and that hence $d\,\varphi'$ must be taken negative, our equation becomes

$$d\,r' = -\,r'\,d\,\varphi'\,(\tan \varphi' + \tan \psi')\,\frac{\mu^2 - 1}{\mu};$$

Differentiating the general equation $\sin\varphi = \mu \sin\varphi'$, we have

$$\cos\varphi \, d\varphi = \mu \cos\varphi' d\varphi'$$

and $$d\varphi' = d\varphi \, \frac{\cos\varphi}{\mu\cos\varphi'}.$$

Substituting this value of $d\varphi'$, and the value of $r' = r\,\frac{\mu\cos^2\varphi'}{\cos^2\varphi}$ we have

$$d r' = -r\,\frac{\mu\cos^2\varphi'}{\cos^2\varphi} \cdot \frac{\cos\varphi}{\mu\cos\varphi'}\, d\varphi\,(\tan\varphi' + \tan\psi')\,\frac{\mu^2-1}{\mu};$$

or, $$d r' = -r\,d\varphi\,\frac{\cos\varphi'}{\cos\phi}\,(\tan\varphi' + \tan\psi')\,\frac{\mu^2-1}{\mu};$$

r in this equation is still a variable quantity, but we may eliminate it by considering a perpendicular let fall from the image of the luminous point upon the first surface of the prism produced, as *a e*, fig. 3. Calling this perpendicular distance e, we have

$$r = \frac{e}{\cos\varphi},$$

and substituting this value

$$d r' = -\frac{e}{\cos\varphi}\,d\varphi\,\frac{\cos\varphi'}{\cos\phi}\,(\tan\varphi' + \tan\psi')\,\frac{\mu^2-1}{\mu},$$

$$d r' = -e\,d\varphi\,\frac{\cos\varphi'}{\cos^2\varphi}\,(\tan\varphi' + \tan\psi')\,\frac{\mu^2-1}{\mu}.$$

This equation is most probably not integrable in the general form we now have; but by supposing the pencils very small, as they really are in cases of interference, we may substitute for $(\tan\varphi' + \tan\psi')$ a term containing only $\tan\varphi'$ and a constant; and we shall find, on recurring to numbers, that we may make this approximation, as well as the former one, without introducing any material error.

Thus at the angle of minimum deviation, we have

$$\tan\varphi' + \tan\psi' = 2\tan\varphi';$$

introducing this value, therefore, our equation becomes

$$d r' = -e\,d\varphi\,\frac{\cos\phi'}{\cos^2\phi}\,2\tan\varphi'\,\frac{\mu^2-1}{\mu}$$

$$= -e\,d\phi\,\frac{\cos\phi'}{\cos^2\phi}\,2.\frac{\sin\varphi'}{\cos\varphi'}\cdot\frac{\mu^2-1}{\mu}$$

$$d r' = -e\,d\varphi\,\frac{\sin\varphi'}{\cos^2\varphi}\,2\,\frac{\mu^2-1}{\mu} = -e\,d\varphi\,\frac{\sin\varphi}{\mu\cos^2\varphi}\,2\,\frac{\mu^2-1}{\mu}$$

or, $$d r' = - e \frac{\sin \varphi \, d \varphi}{\cos^2 \varphi} \, 2 \frac{\mu^2 - 1}{\mu^2} = - e \frac{-d \, . \cos \varphi}{\cos^2 \phi} 2. \frac{\mu^2 - 1}{\mu^2}$$

and $$\int d r' = \int - e \, d \, . \frac{1}{\cos \varphi} \, 2. \frac{\mu^2 - 1}{\mu^2};$$

or, $$r' = C - e \frac{1}{\cos \varphi} \, 2. \frac{\mu^2 - 1}{\mu^2},$$

which is the equation of the curve we seek; and from the circumstances in which we consider the experiments made, we may take r' and φ as the coordinates referred to the pole ζ or ε in fig. 5; and φ, which was originally the angle of incidence, being counted from the line $a \zeta y$, or a parallel to it, this line making with the axes of the pencils after refraction an angle equal to the angle of minimum deviation.

For the constant, let r' become R when $\varphi = 0$, we have then $$R = C - 2 e \frac{\mu^2 - 1}{\mu^2} \qquad C = R + 2 e \frac{\mu^2 - 1}{\mu^2};$$

hence $$r' = R + 2 e \frac{\mu^2 - 1}{\mu^2} - 2 e \frac{\mu^2 - 1}{\mu^2} \cdot \frac{1}{\cos \varphi},$$

which we may write thus,

$$r' = R + \varkappa - \frac{\varkappa}{\cos \varphi}, \quad \text{or } r' = Q - \frac{\varkappa}{\cos \phi};$$

and we see that our equation holds good for any values of R, as the motion of the rays of light requires.

To calculate the points of interference on the transcendental curves given by the light supposed to set out simultaneously from the images of the luminous point, we must return as before to rectangular coordinates, and follow an analogous process.

Then ε and ζ being the secondary images of the luminous point and the poles of the curves, we will take the lines $a \zeta y$ and $a \varepsilon x$ for the axes of the rectangular coordinates; and it will be required to find the distance $a \zeta$, which we will put $= \alpha$, and the distance $a \varepsilon$, which we will call β.

Now writing the equation of the upper curve

$$r' = Q - \frac{\varkappa}{\cos \varphi},$$

and that of the lower one

$$r'_{,} = Q' - \frac{k}{\cos \chi},$$

counting y' from the point ζ, and x' from the point ε, we shall have $$y' = y - \alpha, \quad x' = x - \beta,$$

and $r'^2 = y'^2 + x^2$, $r_{\prime}'^2 = y^2 + x'^2$ for the points at which the curves intersect; and also $y' = r' \cos\varphi$, $y = r_{\prime}' \cos\chi$.

Eliminating $\cos\phi$, $\cos\chi$, r' and $r_{\prime}'$ by means of these equations, we find

$$Q^2 = (y'^2 + x^2)\,\frac{(y' + x)^2}{y'^2},$$

$$Q'^2 = (y^2 + x'^2)\,\frac{(y + k)^2}{y^2}.$$

It now remains to establish the requisite relation between Q and Q′. For this purpose, putting for the difference of the paths $g'y$ the letter y, which we lately found the means of determining, letting fall the perpendicular $\zeta\,\delta$ upon the axis of the lower pencil, and calling the distance $\varepsilon\,\delta = \delta$, of which we easily get the value,
we have on the axes of the pencils

$$r'_i = r_{\prime}'_i - \delta + y,$$

and
$$Q - \frac{x}{\cos i} = Q' - \frac{k}{\cos i} - \delta + y$$

$$Q' = Q + \frac{k - x}{\cos i} + \delta - y = Q + C,$$

by putting
$$C = \frac{k - x}{\cos i} + \delta - y.$$

Returning to our former equations, we have

$$Q^2 = (y'^2 + x^2)\,\frac{(y' + x)^2}{y'^2},\qquad (Q + C)^2 = (y^2 + x'^2)\,\frac{(y + k)^2}{y^2};$$

we may now eliminate Q, and obtain an equation containing only x, y and constants, and which will represent generally the curve in which the central points of interference should take place.

Eliminating, we arrive at this equation:

$$2\,C\sqrt{\frac{(y' + x)^2}{y'^2}\,(y'^2 + x^2)} = x'^2\,\frac{(y + k)^2}{y^2}$$
$$+ (y + k)^2 - x^2\,\frac{(y' + x)^2}{y'^2} - (y' + x)^2 - C^2.$$

To get quit of the sign of the square root, it is necessary to raise both sides to the square; and putting for x' its value $x - \beta$, we have

$$4\,C^2\,\frac{(y' + x)^2}{y'^2}\,x^2 + 4C^2\,(y' + x)^2 = \left(x^2\left(\frac{(y + k)^2}{y^2} - \frac{(y' + x)^2}{y'^2}\right)\right.$$
$$\left. - x\,2\,\beta\,\frac{(y + k)^2}{y^2} + \beta^2\,\frac{(y + k)^2}{y^2} + (y + k)^2 - (y' + x)^2 - C^2\right)^2.$$

It will be seen that the involution to the second power is still only indicated on the second side; but from the complexity it assumes there is no means of using it, except by changing it into a numerical equation, by adopting some numerical values for y, and calculating the corresponding values for x by extracting the required roots. On this account the above is, I believe, the simplest form in which it can be used, and the calculation is, nevertheless, still sufficiently laborious. We may compare the equation to the following:

$$(Ax^2 - Bx + D)^2 = Ex^2 + F;$$

or, $A^2x^4 - 2ABx^3 + x^2(B^2 + 2AD - E) - 2BDx + D^2 - F = 0.$

For the data, I have taken $e = 40$ inches, $a = \cdot 06$ inch, and the refracting angle of a new prism (which I prepared with the intention of making micrometrical measurements if the phænomena had come under any known theory) $= 33° \ 18'$; the refractive index of the glass being $1\cdot 500$ very nearly.

From these I find for the angle of minimum deviation or $i - 25° \ 27' \ 14''$ nearly.

For the lower curve $e' = e + 2\,a \sin i = 40\cdot 0515744$.

$$\varkappa = 2\,e\,\frac{\mu^2 - 1}{\mu^2} = 44\cdot\dot{4}.$$

$$k = 2\,e'\,\frac{\mu^2 - 1}{\mu^2} = 44\cdot 50174.$$

$$r_i = \frac{e}{\cos i} = 44\cdot 297241 \ (= d\,l).$$

$$r_{ii} = \frac{e'}{\cos i} = 44\cdot 354361 \ (= b\,m).$$

To the point on the same perpendicular $- r_{ii} + 2\,a \tan i$

$$= r'_{ii} + 2\,a \tan i = 44\cdot 411479 = r'_{ip} \ (= \varepsilon\,o).$$

$$r'_i = 44\cdot 342340 \ (= \zeta\,r)$$

$$r'_{ip} - r'_i \text{ or } \delta = \cdot 069139 \ (= \varepsilon\,\delta)$$

$$\Delta \text{ paths or } y = \cdot 0634655 \ (= y\,g).$$

$$\alpha = \cdot 1140052 \ (= a\,\zeta)$$

$$\beta = \cdot 0786127 \ (= a\,\varepsilon)$$

$$C = \cdot 0691293.$$

Calculating with these, I have taken for y three different values; namely, $y = 41$ inches, $y = 45$ inches, and $y = 50$ inches, and arrived at the following equations:

For $y = 41$ inches,

$$\cdot 000045968\,x^4 + \cdot 00927181\,x^3 - \cdot 0128883\,x^2 - 40\cdot 053245\,x + 718\cdot 6541 = 0.$$

For $y = 45$ inches,

$$\cdot 000024167\,x^4 + \cdot 006115092\,x^3 + \cdot 00971671\,x^2 - 38\cdot 13274\,x + 787\cdot 2133 = 0.$$

For $y = 50$ inches,

$$\cdot 000112091\,x^4 + \cdot 003760774\,x^3 + \cdot 03045574\,x^2 - 36\cdot 35336\,x + 877\cdot 2895 = 0.$$

I have sought the roots of these equations which have the values nearest to that of x' on the line $a\,n'$, for the corresponding values of y, by the method of approximation; and accordingly, $y = 41$ gives $x = 19\cdot 78$ inches

$y = 45$ —— $x = 22\cdot 86$ ——

$y = 50$ —— $x = 26\cdot 92$ ——

The points on the line $a\,n'$ for these ordinates are found by the equation $x' = y \tan i$.

Hence $y = 41$ gives $x' = 19\cdot 515$ inches

$y = 45$ —— $x' = 21\cdot 419$ ——

$y = 50$ —— $x' = 23\cdot 799$ ——

We see that the points at which interference should take place according to the Newtonian hypothesis,—that light moves with a velocity in passing through refracting substances, which is directly as the refractive index,—are still further from the truth than according to the undulatory theory. The central band ought to have been seen, according to this hypothesis, following a direction similar to $t\,u$, fig. 5.

This investigation is not, however, entirely lost labour; for in addition to knowing the effect of the view we have followed, we see also where we must seek for the true solution; and it is clear that these phænomena can only arise by light really moving still slower in passing through refracting substances, than it is supposed to do even on the undulatory theory.

The experiment of Professor Powell must be allowed to be an important as well as an elegant one, drawing a clear boundary between the claims of rival theories, and pointing with an analysing precision to the true theory, which no reference to measurement alone would probably ever have discovered.

Since I learned the tendency of the Newtonian theory of refraction, I have examined the displacement of the coloured bands produced by causing one of the pencils to pass through a very thin slip of mica, and the displacement is undoubtedly in the direction which indicates the light to have passed through it with *diminished* velocity, and which, if we knew the *exact* thickness of the slip, might be determined. Perhaps the only resource will finally be,—either the method which M. Arago practised, of causing the pencils to pass through two similar pieces of glass of which he knew the inclinations to the directions of the pencils, and consequently the difference of the thickness passed through by the rays; or a method analogous to this.

M. Arago believed that he found the relative velocity in glass to be exactly as indicated by the undulatory theory: if the results were not widely different from this, he would undoubtedly refer the difference to error of experiment. We see that the experiment with the prism draws a clear line of distinction; but from what I have observed, I believe the velocity will not eventually be found extensively different from that according to the said theory. The slightest difference is, however, of fatal consequence; for the ratio ought, according to common consent, to depend rigorously on the refractive index, which is one of the fundamental principles of the theory.

XVII. *Experiments on Platina.* By RICHARD PHILLIPS, *F.R.S. L. & E. &c.*

THE third volume of the Quarterly Journal of Science contains a paper, by Mr. J. T. Cooper, *On some Combinations of Platinum.* In this communication the author states, that when a neutral solution of tartrate of soda is heated with one of muriate of platina, a blackish powder is precipitated: this substance after being washed, was dried on a sand-bath at 300°, in order to free it from uncombined water; it lost afterwards 2·8 per cent. by exposure to a red heat; and as nothing could be procured from the black powder but platina and water, Mr. Cooper considers it to be a hydrate of the metal, composed of 44·328 = 2 atoms of platina + 1·125 = 1 atom of water: these proportions agree tolerably well with the results of the experiment.

It is singular that Mr. Cooper does not particularly advert to the interesting fact which he announces; for this is, I believe, the first instance on record of the combination of water with a metal, not previously converted into an oxide; and it is almost as remarkable, that of the numerous authors whom I have consulted on the subject, no one mentions this compound.

Although, with some particular views, I have repeatedly formed this black powder, it is only lately that I have investigated its properties. Having dissolved some platina and precipitated it in the manner described, I duly washed the powder and dried it at 212°; after this I gradually heated it to redness, and found that it diminished 1·41 per cent. in weight. This experiment, slightly varied, was repeated with a difference of only 0·14 per cent. in the weight lost. It will be observed that although Mr. Cooper dried the precipitate at 300°, while I subjected it only to 212° before heating it to redness, yet I found the diminution of weight, caused by the subsequent and higher temperature, to be but little more than half of that which occurred in his experiment.

Supposing, therefore, the black precipitate to be a hydrate of platina, it would appear by my experiments to be a compound of about four atoms of the metal and one atom of water. Now the existence of a hydrate so constituted is not only of itself extremely improbable, but is rendered still more unlikely by the supposition that a metal without previous oxidizement should form a hydrate at all; for it is quite as contrary to experience that a metal and water should combine, as that a metal and an acid should unite, without the intervention of oxygen.

I am therefore disposed to consider the 1·41 per cent. of water which remains with the black powder, after being heated to 212°, as in a state of mixture, and not of combination. There are other circumstances which strengthen this conclusion: If the black powder be strongly pressed or rubbed in a glass mortar, the metallic appearance of platina is as perfect before as after the application of a red heat; and I conceive this could scarcely occur if it were chemically combined with water. That the platina is in the metallic state in the black powder, is proved by its total insolubility in nitric or muriatic acid, even when first precipitated, and before it is dried. Another circumstance which induces me to believe that it is not a hydrate, is its answering most perfectly the purpose of spongy platina in firing a jet of hydrogen gas, and detonating a mixture of oxygen and hydrogen gases; indeed it appears to me to be an excellent preparation for these purposes, and it is procured with very great facility.

It is probable that some other metals whose affinity for oxygen is weak, may also be precipitated in the metallic state by the tartrates; and I have found this to be the case with gold: when tartrate of soda is added to the muriatic solution of that metal, no effect is produced until heat is applied, and then the precipitation of metallic gold is as rapid, and quite as remarkable as that of platina.

Tartrate of soda being a salt not usually kept, I have sometimes employed tartrate of potash: when the solutions are cold, the well-known double salt of potash and platina is precipitated; but on the application of heat the black powder is very quickly formed; the same effect is produced by tartrate of lime and tartrate of ammonia; but neither tartaric acid nor bitartrate of potash determine precipitation until an alkali is added.

It appears to me that muriate of platina may be very advantageously employed as a test of the presence of tartaric acid, provided it be first saturated or supersaturated with an alkali.

During the formation of the black powder, there is an evident evolution of some gas: suspecting it to be carbonic acid

I passed it into lime-water and obtained a plentiful precipitate; it is probable therefore that the hydrogen of the tartaric acid combines with the oxygen of the platina, and thus reduces it to the metallic state; whilst the carbon and oxygen of the tartaric acid form carbonic acid gas.

I am at present engaged in some researches upon the oxide of platina, thrown down by the action of protonitrate of mercury.

XVIII. *Notice of the Arrival of Twenty-six of the Summer Birds of Passage in the Neighbourhood of Carlisle, during the Spring of* 1832, *together with some of the scarcer Species that have been obtained in the same Vicinity from the* 10*th of November* 1831, *to the* 10*th of November* 1832; *with Observations, &c. By* A CORRESPONDENT*.

No.	English Specific Names.	Latin Generic and Specific Names.	When first observed.	No.
1	Quail	Coturnix vulgaris	May 12	6
2	Swallow	Hirundo rustica	April 12	35
3	House Martin	——— urbica	—— 16	36
4	Sand Martin	——— riparia	March 29	36
5	Swift	Cypselus Apus	April 27	37
6	Goatsucker	Caprimulgus europæus	May 10	38
7	Pied Flycatcher	Muscicapa atricapilla	April 22	41
8	Spotted Flycatcher	——— Grisola	May 7	42
9	Ring Ouzel	Turdus torquatus	April 1	49
10	Wheatear	Saxicola Œnanthe	—— 1	53
11	Whinchat	——— Rubetra	—— 16	54
12	Redstart	Sylvia Phœnicurus	—— 18	57
13	Grasshopper Warbler	Curruca Locustella	March 31	58
14	Sedge Warbler	——— salicaria	May 3	59
15	Greater Pettychaps	——— hortensis	—— 6	62
16	Wood Wren	——— sibilatrix	April 24	63
17	Blackcap	——— Atricapilla	—— 27	64
18	Whitethroat	——— Sylvia	May 3	66
19	Yellow Wren	Regulus Trochilus	April 12	70
20	Yellow Wagtail	Motacilla flava	—— 18	75
21	Field Lark or Titling	Anthus trivialis	—— 19	78
22	Cuckoo	Cuculus canorus	—— 16	121
23	Wryneck	Yunx Torquilla	—— 22	125
24	Corncrake or Land-Rail	Ortygometra Crex	—— 14	129
25	Dottrel	Charadrius Morinellus	May 7	164
26	Common Tern	Sterna Hirundo	—— 18	235

[*Note.*—The figures contained in the column on the right in the above Table, as well as those affixed to the species not included in it, refer to the numbers in Fleming's History of British Animals, which we have inserted, in order that the reader who wishes to have a description or to see the various synonyms of any of the birds here alluded to, may find the species at once, should he possess that highly useful and very excellent work.]

* Communicated by the Author.

Quail.—On the 29th of December a Quail was killed near Kirkbride, a small village within a short distance of Solway Firth; and notwithstanding the time of the year, it was in most excellent condition, and proved, upon dissection, to be a male.

We are induced to record this circumstance, as it confirms the statement we made in our notice of this species for the year 1829, that "a few have been known to remain over the winter*."

9. *Turtle Dove* (*Columba Turtur*).—A Turtle Dove was killed in Blackwell Wood on the 18th of September. It was a young bird, being entirely destitute of the black patch on each side of the neck. This is the first specimen we have seen that has been obtained in this neighbourhood, although we have been informed that it has been met with at Woodside, and one or two other places; it is however of very rare occurrence in this county.

17. *Honey Buzzard* (*Pernis apivorus*).—We were shown a specimen of this elegant bird on the 17th of November, by a dealer in this place, who states that it had been killed about the middle of October last near Raughton Head. It was evidently a very young bird, and agreed in every respect with the description given by Temminck, of *les jeunes de l'année*, of this species, having a few very small buff-coloured spots sparingly scattered over the head and neck. So little was the possessor aware either of its value or rarity, that he had allowed it, through neglect, to become putrid before it had been skinned, and it was in consequence materially injured. This is the second specimen that has been obtained in this county; and as the Honey Buzzard is decidedly one of the rarest of the British *Falconidæ*, the following particulars of its capture, &c. in this country may possibly interest some of our ornithological readers.

2. A female, at Selborne, Hampshire, in the summer of 1780. —White's Nat. Hist. of Selborne, vol. i. p. 186.
3. A female, near Carlisle, Cumberland, June 13th, 1783.—Hutchinson's Hist. of Cumberland, vol. i. p. 5.
   In the cabinet of the writer of this notice.
4. A specimen, supposed to be a female, at Highclere, Berkshire.—Montagu's Orn. Dict.
   In the British Museum.
5. A specimen, seen on Slapton Ley, Devonshire, a few years previous to the year 1813.—Montagu's Sup. Orn. Dict.
6. A specimen, near Yarmouth, Norfolk, a few years prior to 1825. —Linnæan Transactions, vol. xv. p. 6; *Fauna Boreali-Americana*, p. xii.

* Phil. Mag. and Annals, N.S. vol. vi. p. 276.

Formerly in the cabinet of Joseph Sabine, Esq. F.R.S. &c.; but now we believe in the Andersonian Institution, Glasgow.

7. A specimen, in Staffordshire: sex, time when, or the locality where killed, not mentioned.—Mag. Nat. Hist. vol. ii. p. 273.

In the Manchester Museum.

8. A specimen, at Gipping Hall, Suffolk, in the summer of 1821?--Mag. Nat. Hist. vol. v. p. 280.

9. A specimen, near Thrunton Wood, Northumberland, some years before 1829.—Transactions of the Nat. Hist. Society of Northumberland, vol. i. part iii. p. 247.

In the Ashmolean Museum, Oxford.

10. A male, in Thrunton Wood, Northumberland, 31st of August 1829.—Nat. Hist. Society of Northumberland, vol. i. part iii. p. 247.

In the cabinet of the Hon. H. T. Liddell, Ravensworth Castle, Durham.

11. A male, in Tendring Hall Park, Suffolk, October 12th, 1831.—Mag. Nat. Hist. vol. v. p. 280.

In the cabinet of J. D. Hoy, Esq. Stoke Nayland, Suffolk.

12. A specimen, at Spetchly, Worcestershire, in the autumn of 1831.—Mag. Nat. Hist. vol. v. p. 379.

Independent of the above, Latham, Pennant, Boys, Donovan, Selby, &c. mention others, but have not stated the time when, or the place where, the specimens they allude to were obtained; and in all probability two or three more may be noticed in local catalogues, or preserved in the cabinets of private individuals.

39. *Greater Butcher Bird* or *Cinereous Shrike* (*Lanius Excubitor.*) —During the months of November and December three of these birds were obtained in this vicinity. A female, killed on the 24th of November, had been feeding greedily on the larva of *Scotophila porphyrea* (6252), and *Anarta Myrtilli*, (6390), several of which had been swallowed entire, and with little or no injury; the stomach also contained a very fine specimen of *Carabus hortensis* (12), two or three of *Phosphuga atrata* (820), and the elytra of several species of *Agonum**.

119. *Hoopoe* (*Upupa Epops*).— On the 4th of September two Hoopoes were observed on the high road near Lingy Closehead, a short distance from the village of Dalston; and on the following day one of them was shot at the above-mentioned place; the other we believe made its escape.

* The figures attached to the names of these insects refer to the numbers in Stephens's Systematic Catalogue of British Insects.

Upon referring to our communication for the year 1831, it will be seen that one of these handsome birds, so rarely met with in the North of England, was killed at Middlescugh, only a few miles from the village of Dalston, on the 8th of September *.

131. *Spotted Rail*, or *Gallinule* (*Gallinula Porzona*).—A very beautiful female of this remarkably pretty species was killed on Wragmire Moss on the 5th of October, the very same locality from whence we obtained a male the preceding year †.

140. *Green Sandpiper* (*Totanus Ochropus*).—Three or four Green Sandpipers were seen in this district in the months of August and September, two of which were obtained; one about the 6th of the former month, near Richardby; the other on the banks of the river Esk, within a little distance of the iron bridge at Garris Town, on the 23rd of the same month.

144. *Greenshank* (*Totanus Glottis*).—During the month of August, three or four of these birds were occasionally seen on Brugh and Rockcliff Salt Marshes, and on the 25th a young male and female were procured. These two birds had been feeding upon Sparlings or Smelts (*Osmerus Eperlanus*), and Shrimps (*Crangon vulgaris*). A third specimen, which was killed about ten days previous to the above, on the banks of the river Eden, near Botchardby, had recently swallowed a bearded Loche (*Gobitis barbatula*).

The Greenshank, which we believe is nowhere common in any part of England, is rarely met with near Carlisle.

148. *Common Snipe* (*Scolopax Gallinago*).—On the 18th of October we received a specimen of this bird, which had the three first primaries in both wings perfectly white.

150. *Black-tailed Godwit* (*Limosa ægocephala*).—A very fine young male of this scarce species was shot on Rockcliff Salt Marsh on the 25th of August; and, as we have every reason to think, the first specimen of this bird that has been captured in this part of the county ‡.

154. *Cuneate-tailed Sandpiper* (*Tringa pusilla*).—A pair of this rare species of Sandpiper were killed on Rockcliff Salt

* Phil. Mag. and Annals, N.S. vol. xi. p. 84. † Ibid. p. 85.

‡ The reader who has an opportunity of referring to Shaw's General Zoology (vol. xii. part i. pp. 73—77.), will find a very singular error relative to this bird, and its congener the Bar-tailed Godwit (*L. rufa*). The engraving there stated to represent the Black-tailed Godwit, is in fact a miserable figure of the Bar-tailed species, with the exception of the bill:—on the contrary, the one given for the Bar-tailed Godwit, has the long legs, black tail, and plumage of *L. ægocephala*. In short, these plates, in a scientific point of view, are of the most wretched description.

Marsh on the 1st of September, the only specimens we have hitherto heard of that have been captured in this vicinity. Both were young birds, in all probability not more than nine or ten weeks old, and their plumage was in almost every respect very similar to the young of the Common Sandpiper (*Totanus hypoleucos*) of the same age. They proved, upon dissection, to be of different sexes, and were exceedingly fat.

155. *Double Fork-tailed Sandpiper* (*Tringa minuta*).—Two of these scarce birds have been procured in this neighbourhood; namely, an old male, on the 18th of November, in a small fresh-water creek or inlet on Brugh Marsh, and an adult female, on the 1st of September, in company with the preceding species. The former had acquired its winter livery, and had black legs; the latter still retained the greater part of its summer dress; the legs of this bird were of a pale olive-green.

Previous to the publication of Temminck's justly celebrated *Manuel d'Ornithologie*, these two diminutive species of Sandpiper were almost invariably confounded together by the best ornithologists; and there is reason to believe that even at this time their specific marks of distinction are often overlooked by many, especially by those who have not had an opportunity of examining them alive or when recently killed. In either of these states their appearance is so very different that they may be recognised without the least difficulty. The very weak slender bill of *T. pusilla* is then very visibly bent or curved, but becomes, as Temminck very correctly remarks, quite straight when perfectly dry:—independent of this specific characteristic, the shorter tarsi, and cuneiform tail of this species, will at all times point out this bird from *T. minuta*. The flight as well as whistle or notes of these birds are also very different.

We have subjoined the comparative weights and dimensions of the four birds above alluded to.

Name.	Sex.	Weight.		Total Length.	Extent of Wings.	Beak to the Front.	Tarsi.
		Dr.	Gr.	Inch.	Inch.	Inch.	Inch.
Tringa pusilla	male	7	30	$5\frac{9}{10}$	$11\frac{8}{10}$	$\frac{11}{20}$	$\frac{13}{20}$
	female	8	14	6	12	$\frac{12}{20}$	$\frac{14}{20}$
Tringa minuta	male	5	17	6	$11\frac{1}{2}$	$\frac{14}{20}$	$\frac{8}{10}$
	female	8	29	6	12	$\frac{15}{20}$	$\frac{9}{10}$

It perhaps may not be amiss to observe that the various English specific names given to these two birds by recent authors, appear to us to be by no means either characteristic or appropriate; and each succeeding writer, as if dissatisfied with those adopted by his predecessor, has substituted others, but we really think with very little or no improvement. In order that the reader may judge for himself, we have given the names applied to these small Sandpipers, by Stephens, Selby, and Richardson.

	Tringa pusilla.	*Tringa minuta.*
Stephens* ...	Little or Temminck's Dunlin.	Minute Dunlin.
Selby†........	Little Tringa.	Minute Tringa.
Richardson‡.	Diminutive Sandpiper.	Pigmy Sandpiper.

Now, the size and weight of *T. pusilla* and *T. minuta* are so nearly the same, that the trivial names of Little, Pigmy, Minute, and Diminutive, may with equal propriety be given to both, and certainly do not point out any of their decided specific marks of distinction; and as Dr. Fleming has not given either of these birds an English name, we have ventured, after some hesitation, to call the former the Cuneate-tailed Sandpiper, and the latter the Double Fork-tailed Sandpiper; names which we trust will induce ornithologists to examine this bird with greater attention, and consequently the more readily lead to the detection of these two species.

At the same time we are fully aware that even these names are not altogether free from objection; as one or two of the extra-European species of diminutive Sandpipers are stated to have tails of very similar formation§.

158. *Ruff* (*Tringa pugnax*).—Two young females of the year, of this species, were shot on Rockcliff Marsh on the 23rd and 25th days of August. From various sources of information we are strongly inclined to think that a few young Ruffs annually resort for a short time to the Salt Marshes in the vicinity of Solway Firth, during their autumnal migration.

160. *Common Lapwing* (*Vanellus cristatus.*)—A rather sin-

* Shaw's General Zoology, vol. xii. part i. pp. 101, 102, 105.

† Selby's Illustrations of British Ornithology, 4to edition, No. 9. Second Series, pp. 127, 128.

‡ *Fauna Boreali-Americana,* part ii. pp. 385, 386.

§ It is very evident, from an examination of the synonyms, that ornithologists entertain doubts whether the *T. pusilla* of Wilson's American Ornithology (vol. v. p. 32) is identical with the *T. pusilla* met with in Europe; although we rather suspect that they will ultimately prove to be the same birds. Wilson observes that the little Sandpiper found in the United States, "resides chiefly among tho sea marshes, and feeds among the mud at low water; springs with a zigzag irregular flight, and a feeble twit,"—which is a very accurate description of the flight, note and locality, of the two birds recently obtained near Carlisle.

gular variety of the Lapwing was obtained near the canal reservoir on the 13th of March. This specimen, which was a male, had the whole of the back and wing-coverts white, interspersed here and there with a few feathers of the usual colour.

204. *Razor Bill* (*Alca Torda*).— A remarkably fine adult specimen of this bird was killed on the river Eden, near the village of Beaumont, on the 18th of April. Young Razor Bills are occasionally met with in this district; but the old birds are exceedingly rare.

211. *Northern Diver* (*Colymbus glacialis*).—On the 13th of December a young male of the year of this bird was shot on the river Eden, within a very short distance of Carlisle, and nearly upon the very same spot where a very similar Northern Diver was killed on the 21st of January 1789*.

219. *Fork-tailed Petrel* (*Procellaria Bullockii*).—The first specimen of this bird that has been detected in this neighbourhood, was found dead upon the coast near Cardurnock, on the 17th of December. Not very many years ago the Fork-tailed Petrel was considered one of the rarest of the British birds, but within these last few years several have been met with from time to time in various parts of the kingdom; and during the latter end of the year 1831 it appears to have visited this country in very considerable numbers, and is stated to have been obtained amongst others at the following places; viz.

One found dead near Chipping Norton, Oxon; two on the banks of the river Severn, Gloucestershire; several in the vicinity of London.

One found dead not far from Rington, Hertfordshire; four found dead a short distance from York; three or four in the vicinity of Halifax.

One shot on the river Tyne, near Newcastle; two or three in the neighbourhood of Plymouth; one or two on the coast of Cornwall; one at Thirsk, Yorkshire.

One contiguous to Hanbury, Worcestershire; one or two near Hull, &c. &c.†.

A few Meteorological Remarks on the Spring and Summer of 1832 at Carlisle.

In this neighbourhood we were favoured with some delightful weather for the season, from the 27th to the 31st of March,

* Hutchinson's History of Cumberland, vol. i. p. 21.

† Mag. Nat. Hist. vol. v. pp. 282, 283, 380, 388, &c. &c.

and again from the 4th to the 12th of April; the remainder, however, of this month was for the most part gloomy and exceedingly cold, and on the 25th the summit of Cross Fell was pretty thickly covered with snow. During the first three weeks of May, keen cutting winds prevailed generally from the East; on the 2nd, there was snow several inches in depth in the vicinity of Tindal Fell, and on the 15th and 16th we had several smart hail-showers; in short, it was not before the 22nd that the weather became at all warm and seasonable.

Vegetation, as might naturally be expected, made but little progress; and upon the whole the spring of this year was perhaps more backward than the very late one of 1829.

The summer and autumn which followed, however, were fine and remarkably dry, the harvest early, and the crops in this district, generally speaking, were exceedingly good, more particularly on cold elevated grounds, where the farming produce was scarcely ever recollected to have been more abundant, or to have been secured in finer condition.

Carlisle, November 10, 1832.

XIX. *Abstract of the principal Demonstrations of* M. Fourier, *relative to the Mathematical Law of the Radiation of Heat. By Baron* Maurice, *Member of the Institute of France, and Professor of Analytical Mechanics in the Academy of Geneva. Translated by* James D. Forbes, *Esq. F.R.S. L. & Ed. F.G.S. &c. Communicated in a Letter to* Sir David Brewster.

My Dear Sir,

THE *précis* of Fourier's Demonstrations, from which the following translation is taken, was put into my hands in manuscript, by my friend Baron Maurice of Geneva, last month. It was written for the Supplement to M. Prevost's work on Heat, which has since appeared. As I have not observed any account of this part of Fourier's labours in English works (and, indeed, the writings of that distinguished man are too little known in this country), I think the present notice may not be unacceptable; more especially as the original Memoir of Fourier, in the fifth volume of the Memoirs of the Institute, is really obscure. In this tract M. Maurice has reduced the theory to a few simple propositions, which he has given with all that copiousness of reasoning which distinguishes the writings of Fourier, when he is establishing fundamental propositions upon which a complex superstructure is to be raised: the first demonstration in particular is quite in the Newtonian style. I have adhered closely to the text of M. Maurice, though I have not always translated it word for word.

I am, my Dear Sir, yours most faithfully,

Greenhill, Edinburgh, Dec. 6th, 1832. JAMES D. FORBES.

Abstract, &c.

1. *Law of Radiation.*—"The rays of heat which issue under different angles from the same point of the surface of any body, have an intensity which decreases proportionally to the sine of the angle formed by their direction with the plane tangential to the surface, at the point of emission."

Demonstration.—Let AB, (fig. 1.) be the mathematical surface of the body; and AC the thickness of its *physical* surface. We shall consider a normal ray of heat CA, and conceive that C is the point furthest from the surface capable of emitting any heat whatever by radiation. Consequently, as weadvance from C to A, the particles radiate heat more copiously.

Fig. 1.

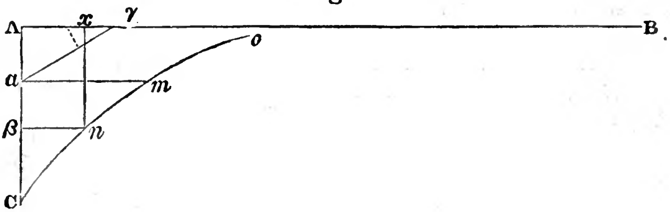
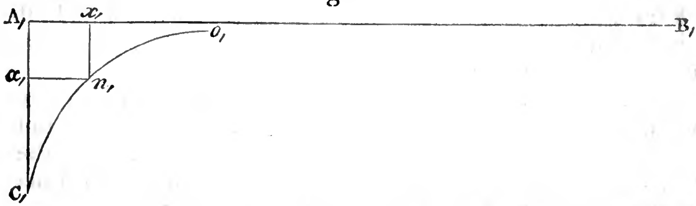


Fig. 2.



Let us *assume* the quantity of heat to be known which is furnished by each point α β......situated in the normal direction, and that it is represented by the respective abscissæ β n . α m . . . . referred to the axis A B: we may conceive such a curve as C n m o . . . . passing through their extremities; and the sum of these abscissæ, or the area of the curve, will express the intensity of the ray of heat normal to the surface.

We must next consider the intensity of a similar ray in the direction $\alpha\gamma$, inclined to the mathematical surface at an angle φ. Let us consider first the point α of the normal ray.

The quantity of heat which reaches γ from α will be identical with that which reaches A from β, provided $A\beta = \alpha\gamma$. Now, by the hypothesis, the abscissa βn represents this quantity. Next let us take (fig. 2.) $A_{,}$, $B_{,}$, $C_{,}$ and $\alpha_{,}$ as before, and erect the abscissa $\alpha_{,} n_{,}$ equal and parallel to βn, and let us repeat a similar operation for all the other points of the line $\alpha\gamma$; we shall then have another curve $C_{,} n_{,} o_{,} \ldots$ of which the surface will represent the intensity of a ray inclined to the normal.

In order to compare the surfaces of the curves fig. 1 and 2, we have only to observe, that for the same abscissa $A x = \beta n$ (fig. 1.), or $A_{,} x_{,} = \alpha_{,} n_{,}$ (fig. 2.), if the ordinate (fig. 1.) is represented by $A\beta = \alpha\gamma$, and we make $\alpha\gamma = 1$, the ordinate $A_{,} \alpha_{,}$ (fig. 2.) equal to $A\alpha$ will be represented by $\sin\varphi$. But when two curves have the same origin and axis of abscissæ, their surfaces comprised between the origin and a common limit are evidently in the relation of the respective ordinates: therefore, the surface $A C n o$: surface $A_{,} C_{,} n_{,} o_{,} = 1 : \sin\varphi$. Wherefore the respective intensities of a ray of heat μ normal to the surface, and one, ν inclined at an angle φ have the same ratio, and $\nu = \mu \sin\varphi$. Q. E. D.

2. This demonstration is equally applicable to curve as to plane surfaces. For the thickness of the physical surface CA being extremely small, the portion of the mathematical surface included between the extremity of the normal and that of the oblique line (which at most can only be equal to the length of CA), will always be sufficiently small to be confounded with a plane tangential to the point of emission.

3. The *absolute* intensities which are supposed to be known in the preceding demonstration are in no respect wanted for the determination of the intensity of the oblique rays *relatively to the normal ray.*

4. Having demonstrated the law of radiation upon these simple principles, we proceed to show, that did this law not exist we should arrive at conclusions at variance with the simplest experiments. But we must first introduce a distinct conception of the radiating power of a given surface.

5. Let a be the temperature of a heated surface, and h its radiating power*. Each infinitely small portion of the surface may be viewed as the centre of a hemisphere which is filled by the radiant heat emanating from it. If then we consider a small portion of the surface taken as unity, the quan-

* This coefficient h depends on the nature of the radiating body, and is what Fourier calls "Conducibilité extérieure" in his *Traité Analytique de Chaleur.*—Translator.

tity of heat radiated by it will be proportional to the product $a\,h$; and if we could know how much heat traversed in unity of time the surface of a hemisphere of radius 1, having for its centre an element of surface taken for unity, we should have the value of h by dividing the expression for that quantity by the product $a\,.\,2\,\pi$.

In order to determine this quantity, let us designate by g the constant coefficient which represents the intensity of a ray of heat normal to the surface. If this intensity varies with the inclination φ of the rays, we may represent it by $g f(\varphi)$, where $f(\varphi)$ denotes an unknown function of the inclination. Hence $a\,g\,f(\varphi)$ will represent the heat afforded by a ray making an angle φ with the surface.

Let us next consider upon the hemisphere of radius 1, an elementary zone which has for height the element $d\,\phi$ of the arc φ, and for base the circle $2\,\pi\,.\,\cos\varphi$: it is evident that the product $a\,g\,.f(\varphi)\,.\,2\,\pi\,\cos\varphi\,d\,\varphi$ will express the quantity of heat which in unity of time will traverse the surface of the elementary zone; and consequently the integral of this expression taken from $\varphi = 0$ to $\varphi = \frac{1}{2}\,\pi$, will express the quantity of heat which proceeding from unity of surface will traverse in unity of time the hemispherical surface $2\,\pi$. But this quantity ought also to be exactly represented by $a\,h\,2\,\pi$.

Hence $$2\,a\,\pi\,.\,h = 2\,a\,\pi\,.\,g\int d\,\varphi\,\cos\varphi\,.f(\varphi)$$

or, more simply, $$h = g\int d\,\varphi\,\cos\varphi\,.f(\varphi),$$

taking the integral between the limits first assigned. Such will be the general expression of the radiating power of a given surface.

Thus, for example, if the intensity of the rays be the same for all angles of inclination, we have $f(\varphi) = 1$, and integrating the expression for h between the given limits, we have $h = g$, as it ought to be upon this supposition.

If, on the contrary, as we have seen in article 1, the intensity is proportional to the sine of the angle of emission, we shall have $f(\varphi) = \sin\phi$, which gives $h = \frac{1}{2}\,g$. Hence in the case of nature, in which the general intensity of the rays is expressed by $g\,.\sin\varphi$, it has for extreme values *zero* and g; and the mean value of h, the radiating power, is $\frac{1}{2}\,g$. Such would be the intensity of rays emitted at an angle of 30°, for,

$$g\,.\sin\frac{\pi}{6} = \tfrac{1}{2}\,g.$$

We also see that if all the rays were similar to those normal to the surface, the resulting effect would be double what it really is; for it would then be represented by $a\,g$ instead of $\frac{1}{2}\,a\,g$ or $a\,h$.

6. From these principles may be deduced some curious and important consequences: but we proceed at present to consider a particular case, which puts in a strong point of view the *necessity* of the law of the sines.

7. Let us inquire what would be the final temperature acquired by a spherical molecule placed in the centre of a spherical surface having a radius R, which we conceive to be constantly kept at the temperature a; and continuing to call h the radiating power of the surface both of the spherical inclosure and of the molecule, of which we may call the radius r, we shall have, as we have just seen,

$$h = g \int d\varphi \cos\varphi \,.\, f(\varphi) \quad \dots\dots\dots \quad (\alpha)$$

denoting by ϕ the inclination of the rays as before.

Let ω be an infinitely small portion of the interior spherical surface. It will constantly emit rays of heat which may be conceived in unity of time to fill a hemisphere having a radius R: now the rays normal to the interior spherical surface will necessarily fall upon the central molecule, and will occupy upon the surface $2\pi \,.\, R^2$ of the hemisphere a space equal to $\pi\,r^2$. Hence these normal rays, all which have the intensity g, and of which ω is the base, will transmit to the central molecule a quantity of heat expressed by

$$\omega \,.\, a\,g \,.\, \frac{\pi\,r^2}{2\,\pi\,R^2} \quad \dots\dots\dots\dots \quad (1)$$

If in this expression we put for g its value found by equation (α), it becomes

$$\omega \,.\, \frac{a\,r^2}{2\,R^2} \,.\, \frac{h}{\int d\varphi \cos\varphi \,.\, f(\varphi)} \quad \dots\dots\dots \quad (2)$$

and as the ratio of the whole spherical surface to ω is expressed by $\frac{4\,\pi\,R^2}{\omega}$, if we multiply this ratio by the expression (2), we shall have the whole quantity of heat received by the molecule, denoted by $2\pi \,.\, a\,r^2 \,.\, \frac{h}{\int d\varphi \cos\varphi \,.\, f(\varphi)}$, the limits of the integral being always 0 and $\frac{1}{2}\,\pi$.

Let us now suppose that the final temperature acquired by the molecule is represented by b; it follows that the molecule will dissipate from its surface a quantity of heat equal to

$4\pi r^3 . b h$. We shall then have, for unity of time, the equation

$$4\pi r^2 . b h = 2\pi r^2 . a h . \frac{1}{\int d\varphi . \cos\varphi . f(\varphi)},$$

or, simplifying,

$$b = \tfrac{1}{2} a . \frac{1}{\int d\varphi \cos\varphi . f(\varphi)}.$$

But if the intensity of the rays does not vary with their inclination, we shall have $f(\phi) = 1$, and taking the integral between the proper limits, $b = \frac{1}{2} a$; so that the central molecule could only acquire a temperature equal to half that of the spherical inclosure!—a result which is absurd, being constantly contradicted by experience. If on the contrary we make $f(\phi) = \sin\varphi$, we find rigorously $b = a$; that is, that the final temperature of the molecule is equal to that of the inclosure,—agreeably to experiment.

8. It is easy to explain the rather singular result at which we have just arrived; namely, that if the intensity of the rays of heat emitted were independent of the angle of emission, the central molecule would only acquire half the temperature of the inclosure in which it is placed, even after an indefinite time. For whilst the inclosure from its spherical form can only transmit to the central molecule such rays as are normal to its own surface, its calorific energy being thus independent of the angle of emission of the other rays, the molecule itself dissipates heat in all directions, and (according to the hypothesis) with equal intensity; it is evident (see art. 5.), from the equation $h = g$, which is then applicable, that it will lose in unity of time twice the quantity of heat which it receives; its temperature therefore will only be half that of the inclosure*.

9. We shall next proceed to show the necessity of the mathematical law of radiation by proving that its existence is essential, in order to account for the uniformity of temperature pervading a space of which the limits are kept during a sufficient time at a constant temperature,—a fact which experience demonstrates.

10. Let us consider in the interior of the bounding sides

* These facts tend also to establish the rigorous connexion between the *absorptive* and *emissive* powers of bodies for radiant heat. If in the experiment above described, the central molecule had one of these properties in the slightest degree in excess over the other, it might acquire an infinitely high or an infinitely low temperature. Thus by these elementary views the *necessity* of these two fundamental laws discovered experimentally by Professor Leslie is illustrated.—TRANSLATOR.

of the given space, which have the common temperature a, two extremely small portions of the surface, which are plane and homogeneous, and which may be denoted by s and s'. Let δ be the distance between s and s', which is finite, and therefore incomparably greater than the dimensions of these very small portions of surface:—we have to find how much heat the surface s', for example, receives from s, in unity of time; neglecting, as we have hitherto done, the portion of heat reflected, since we shall do the same when we come to consider how much heat s receives from s'.

Let us call p the angle which δ makes with s, and ϕ that which it makes with s'. We may reckon the distance δ from any points of the surfaces s and s', since from their small size no sensible variation could be introduced into the length of δ, or the angles p and . Each infinitely small portion ω of the surface s will be the base of a ray of heat falling upon s'; and if, to know how much heat this ray contains, we make through a point of s' a section of the ray perpendicular to its direction, we shall obviously have $s' \sin \varphi$ for the area of that section; a quantity of which we must take the ratio to the whole surface $2\pi\delta^2$ of the hemisphere traversed by all the rays emanating from ω, when we wish to measure the quantity of heat which falls from s upon s'.

Now, denoting by $f(p)$ the unknown function of the inclination p of the ray which determines its intensity, we shall have the product $\omega . a g . f(p)$ for the heat of the pencil emitted from ω at an inclination p; g representing, as before, the intensity of a ray normal to the surface. Then multiplying this by $\frac{s}{\omega}$, and also by $\frac{s' \sin \varphi}{2\pi\delta^2}$, the ratio of the surfaces, we have the expression

$$\frac{a g}{2\pi\delta^2} . s f(p) . s' \sin \varphi$$

for the total amount of heat passing from the surface s to the surface s'.

But it is evident that, reasoning in a similar manner, the quantity of heat passing from s' to s in the same unity of time will be

$$\frac{a g}{2\pi\delta^2} . s' f(\varphi) . s \sin p.$$

It follows from the comparison of these expressions, that if the unknown function $f(p)$ or $f(\varphi)$ be the sine of those angles, the action of s will be the same upon s' as that of s' is upon s; and that if this function does not represent the sine, these two actions cannot be equal.

Hence it is easy to see that unless this condition be fulfilled, an equilibrium of temperature cannot be established after any lapse of time.

11. *Corollary.*—Since (art. 5.) we have found $g = 2h$ in the case of nature, we shall have for the expression of s upon s', or of s' upon s,

$$\frac{a\,h}{\pi} \cdot \frac{s \sin p \,.\, s' \sin \varphi}{\delta^2}.$$

But if the surfaces have unequal temperatures a and b, and supposing, for example, that a is greater than b, the result of their mutual action will be proportional to

$$(a-b)\,h \,.\, \frac{s \sin p \,.\, s' \sin \varphi}{\delta^2},$$

conformably to experience.

XX. *Notice of a remarkable Deposition of Ice round the decaying Stems of Vegetables during Frost.* By Sir John F. W. Herschel, *K.H. F.R.S. &c. &c.*

[With Figures: Plate II.]

To R. Phillips, Esq. &c. &c.

Dear Sir,

SOME years ago during the first days of a sharp frost, my attention was attracted by the unusual accumulation of ice round the roots and stumps of some dry and decaying thistles in the fields; while at the same time comparatively little hoar-frost was deposited on wheat-stubble and other vegetables in the neighbourhood. On examination I found it to incrust the stalks in a singular manner in voluminous friable masses, which looked as if they had been squeezed, while soft, through cracks in the stems. It was chiefly or entirely confined to the immediate neighbourhood of the root, the upper parts of the taller unbroken stalks being quite free from it. This peculiarity of situation, and the comparative absence of hoar-frost elsewhere, induced me at the time to attribute it either to some different cause from hoar-frost, or to some singular modification of that atmospheric deposition by local and temporary circumstances.

The above observation was recalled to my recollection by a similar phænomenon noticed on the morning of the 11th instant, after a night of sharp frost, of the kind vulgarly called "a black frost;" there being little deposition of hoar-frost from the air, which was during the night extremely tranquil, with a scarcely perceptible current from the north-east. The stumps of a bed of heliotropes, which had been left out to try the chance of the season, were found to be affected with a similar and very copious accumulation of ice to what I had before observed round the thistle-stalks. Fig. (A), Plate II. shows the general appearance of this accumulation; while the mode in which it was attached to the stem, and seemed to emanate in a kind of riband- or frill-shaped wavy excrescence,—as if protruded in a soft state from the interior of the stem, from longitudinal fissures in its sides,—is exhibited at fig. (B). The structure of the ribands was fibrous, like that of the fibrous variety of gypsum, presenting a glossy silky surface; the direction of the fibres being at right angles to the stem, or horizontal.

Although, as remarked, the icy sheets appeared to have been protruded from the interior of the stem, yet on examination they were found to terminate sharply at its surface, adhering to it so lightly as to render it impossible to handle a specimen without detaching them, and in no instance connected with any formation of ice within; on the contrary, the majority of the stems were sound and solid, and many of them still green when cut. The point of attachment of the ice was, however, always on the surface of the *wood*, beneath the outer bark or epidermis, which the frozen sheets had in every instance stripped off, and forced out to a distance. Where the fringes were large and well developed, the bark had quite fallen off; but in those cases where it adhered more strongly, it seemed to have prevented their free expansion; and in such instances the stem presented the singular appearance (C) of a thick massive coating of ice interposed between the wood and its integument, which was swollen and rifted.

The appearances above described are quite at variance with any idea of the deposition of these icy fringes from the store of aqueous vapour in the general atmosphere, in the manner of hoar-frost; and the only quarter to which we can look for their origin is in the plant itself, or in the comparatively warm earth beneath, to whose exhalations the decaying stems may form a kind of chimney. In the present instance, the season had been, up to the night in question, uncommonly mild and open; and the frost of a single night being insufficient to penetrate far into the soil, it may easily be supposed to have been giving out moisture through every open spiracle. What share the physiological functions of the plant may have in the phænomenon, or whether it be connected with the vitality of the stem at all, it is for botanists to decide.

I remain, Dear Sir, your very obedient Servant,

January 12, 1833. J. F. W. Herschel.

XXI. *On the Phænomena presented by Light in its Passage along the Axes of Biaxal Crystals. By the Rev.* HUMPHREY LLOYD, *A.M. M.R.I.A. Fellow of Trinity College, and Professor of Natural and Experimental Philosophy in the University of Dublin**.

IT is well known that when a ray of light is incident upon certain crystals, such as Iceland spar and quartz, it is in general divided into two pencils, one of which is refracted according to the known law of the sines, while the direction of the other is determined by a new and extraordinary law first assigned by Huyghens.

These laws were long supposed to apply to all doubly-refracting substances; and it was not until the subject was taken up by Fresnel, that the problem of double refraction was solved in all its generality. Setting out from the hypothesis, that the elasticity of the vibrating medium within the crystal is unequal in three rectangular directions, Fresnel has shown that the surface of the wave is neither a sphere nor a spheroid, as in the Huyghenian law, but a surface of the 4th order, consisting of two sheets, whose points of contact with the tangent planes determine the directions of the two rays. From this construction it follows that neither of the rays, in general, obeys the law of Snellius, or that of Huyghens, but that they are both refracted according to a new and more complicated law. Such crystals have two optic axes, and are said to be *biaxal*. When the elasticity of the medium is equal in two of the three directions, the equation of the surface of the wave is resolvable into two quadratic factors, which give the equations of the sphere and spheroid of the Huyghenian theory. The two optic axes in this case coincide in one; and the law of Huyghens is thus deduced from the general solution, and proved to belong to the case of *uniaxal* crystals. Finally, when the elasticity is equal in all the three directions, the surface of the wave becomes a sphere; and the refraction is single, and takes place according to the ordinary law of the sines.

There are two remarkable cases, however, in this elegant and profound theory, which its author seems to have overlooked, if not to have misapprehended. In a communication made, some months since, to the Royal Irish Academy, Professor Hamilton has supplied these omissions in the theory of Fresnel, and has thus been led to results in the highest degree novel and remarkable.

To understand these conclusions, it will be necessary to

* Communicated by the Author.

examine for a moment the form of the wave. Its equation, referred to polar coordinates, is

$$(a^2 \cos^2 \alpha + b^2 \cos^2 \mathrm{B} + c^2 \cos^2 \gamma)\, r^4$$
$$- \left[a^2 (b^2 + c^2) \cos^2 \alpha + b^2 (a^2 + c^2) \cos^2 \mathrm{B} + c^2 (a^2 + b^2) \cos^2 \gamma\right] r^2$$
$$+ a^2 b^2 c^2 = 0$$

in which α, B, γ, denote the angles made by the radius vector with the three axes of coordinates. If now we make $\cos \gamma = 0$ in this equation, so as to obtain the section of the surface made by the plane of $x\,y$, the result is reducible to the form,

$$(r^2 - c^2) \left[(a^2 \cos^2 \alpha + b^2 \sin^2 \alpha)\, r^2 - a^2 b^2\right] = 0;$$

so that the surface of the wave intersects the plane of $x\,y$ in a circle and ellipse, whose equations are

$$r = c, \quad (a^2 \cos^2 \alpha + b^2 \sin^2 \alpha)\, r^2 = a^2 b^2.$$

Now if c, the radius of the circle, be intermediate between a and b, the semiaxes of the ellipse, the two curves will intersect in four points, or *cusps*; and the angle which the radius vector drawn to the cusp makes with the axis of x, is found by eliminating r between the two equations, by which means we obtain

$$\sin \alpha = \pm \frac{a}{c} \sqrt{\frac{c^2 - b^2}{a^2 - b^2}}.$$

At each of the points thus determined, there will be two tangents to the plane section, and therefore two tangent planes to the surface; and consequently a single ray, proceeding within the crystal to one of these points, will at emergence be divided into two, whose directions are determined by those of the tangent planes.

Such seems to have been Fresnel's conception of this case. Professor Hamilton has shown, however, that there is a cusp at each of these points, not only in this particular section, but in every section of the wave-surface passing through the line whose direction has just been determined; or that there are, in fact, *four conoidal cusps* on the general wave-surface at the points of intersection of the circle and ellipse. So that there must be an infinite number of tangent planes at each of these points, and consequently, a *single ray*, proceeding from a point within the crystal in any of the above-mentioned directions, ought to be divided into an *infinite number of emergent rays*, forming a cone of the 4th order.

It is evident, further, that the circle and ellipse which thus intersect must have four common tangents. Fresnel has shown that the planes passing through these tangents, and parallel to the 3rd or mean axis, are parallel to the circular sections of a curved surface which he calls the surface of elasticity; and he seems to have concluded that these planes touched the wave-surface only in the two points just mentioned; and consequently that a single ray, proceeding from a point without a biaxal crystal, and refracted in the direction of the optic axis, would necessarily be divided into two, determined by the points of contact. Professor Hamilton, however, has shown that the four planes in question touch the wave-surface, not in two points only, but in an *infinite number of points*, constituting each a small *circle of contact*, whose plane is parallel to one of the two circular sections of the surface of elasticity; and that, consequently, a *single ray* of common light, proceeding from an external point, and refracted in the required direction, ought, if the theory be true, to be divided *within* the crystal into an *infinite number of rays*, constituting a *conical surface*.

Here then are two singular and unexpected consequences of the undulatory theory, not only unsupported by any phænomena hitherto noticed, but even opposed to all the analogies derived from experience. If confirmed by experiment, they would furnish a new and almost convincing proof of the truth of that theory; and if disproved, on the other hand, it is evident that the theory must be abandoned or modified.

Being naturally anxious to submit the theory of waves to this delicate test, and to ascertain how far these new theoretical conclusions were in accordance with actual phænomena, Professor Hamilton requested me to undertake a series of experiments with that view. I accordingly applied myself to this experimental problem with all the attention which the subject so well deserved, and have fortunately succeeded in verifying the first-mentioned species of conical refraction. I hope before long to be able to make similar researches on the second*.

The mineral I employed in these experiments was *arragonite*, which I selected partly on account of the magnitude of the cone which theory indicated in this instance, and partly because the three elasticities in this mineral have been determined, apparently with great care, by Professor Rudberg, and therefore the results of theory could be applied to it at once without further examination. The specimen I used was one of considerable size and purity, procured for me by Mr. Dollond, and cut with its parallel faces perpendicular to the line bisecting the optic axes. If we suppose a ray of common light to pass in both directions out of such a crystal, along the line connecting the two cusps in the wave, it is evident that it must emerge similarly at both surfaces: consequently

* Since we received this paper, we have been informed by the author that he has now *observed* phænomena corresponding to the *second* species of conical refraction, and of which an account will be given in our next Number.—Edit.

the ray which passes along this line, and forms a diverging cone of rays at emergence at the second surface of the crystal, must arise from a converging cone incident upon the first surface. Having therefore nearly ascertained the direction of the optic axis by means of the rings, I placed a lens of short focus at the distance of its own focal length from the first surface, and in such a position that the central rays of the pencil might after refraction pass along the axis. Then looking through the crystal at the light of a lamp placed at a considerable distance, I observed, in the expected direction, a point more luminous than the space immediately about it, and surrounded by something like a stellar radiation. Fearing that this appearance might have arisen from some imperfection in the crystal, I examined it with polarized light, and was happy to find the system of rings in the same direction. This was afterwards confirmed by numerous observations on different parts of the crystal.

This result is of some interest in itself, independently of its connexion with theory. It has been hitherto supposed that the only means of determining experimentally the direction of the optic axes, in substances of weak double refraction, was by observation of the rings which appear around them, when the incident and emergent light is polarized. Here, however, it is seen that common or unpolarized light undergoes such modifications in the neighbourhood of the optic axes of biaxal crystals, that the apparent direction of the axes may be at once determined, and with the simplest contrivance.

But to examine the emergent cone it was necessary to exclude the light which passed through the crystal in other directions. For this purpose a plate of thin metal, having a minute aperture, was placed on the surface of the crystal next the eye; and the position of the aperture so adjusted that the line connecting it with the luminous point on the first surface might be, as nearly as possible, in the direction of the optic axis. The exact adjustment to this direction was made by subsequent trial. The phænomenon which presented itself, on looking through the aperture, when this adjustment was complete, was in the highest degree curious. There appeared a luminous circle with a small dark space round the centre, and in this dark space (which was also nearly circular) were two bright points divided by a narrow and well-defined dark line. When the aperture in the plate was slightly shifted, the appearances rapidly changed. In the first stage of its change the central dark space became greatly enlarged, and a double cone appeared within it. The circle was reduced to about a quadrant, and was separated by a dark interval from the cone just mentioned. The remote cone then disappeared, and the circular arch diminished; and, as the obliquity of the line to the axis was further increased, these two luminous portions merged gradually into the two pencils into which a single ray is divided in the other parts of the crystal.

The same experiments were repeated by bringing the flame of the lamp close to the first surface of the crystal. In this case the lens was removed, and the incident cone of rays formed by covering the surface of incidence with a thin metallic plate perforated with a minute aperture. The results were perfectly similar to those obtained in the former case.

But to apply a yet more palpable test to this theory, I substituted a narrow linear aperture for the point, in the plate next the lamp; and fixed it so that the plane passing through the line on the first plate and the point in the second, should be the plane of the optic axes. In this case, according to the received theory, all the rays transmitted through the two apertures should be refracted doubly in the plane of the optic axes, so that no part of the line should appear enlarged in breadth on looking through the second aperture; whereas, according to Professor Hamilton's beautiful deduction from the same theory, the ray proceeding in the direction of an optic axis should be refracted in every plane passing through that line. In accordance with this conclusion I found, on looking through the second aperture, that the luminous line was undilated, except in the direction of one of the optic axes; and that in the neighbourhood of this direction its boundaries ceased to be rectilinear, and it swelled out into an oval curve.

This experiment seems to go far in affording a general verification of the principle. I was anxious, however, to observe the emergent cone more directly. After some trials I effected this with the sun's light, and received the rays, emerging from the aperture in the second plate, on a screen of roughened glass. I was thus enabled to observe the phænomenon at various distances, and with all the advantages of enlargement. The light was sufficiently bright, and the appearance distinct, when the plane section of the cone of rays on the screen was even two inches in diameter.

Examining the emergent cone with a tourmaline plate, I was surprised to observe that one radius only of the section of the cone vanished, in a given position of the axis of the tourmaline; and that the ray which disappeared ranged through 360°, as the tourmaline plate was turned through 180°. From this it appeared that all the rays of the cone are polarized in different planes.

On examining this curious phænomenon more attentively, I discovered the remarkable law,—that "the angle between the planes of polarization of any two rays of the cone is half the angle contained by the planes passing through the rays themselves and its axis." This law accounts for the disappearance of one radius only of the section of the cone, the opposite radius being in fact polarized in a plane at right angles to the plane of polarization of the first. The law itself can be easily shown to be a necessary consequence of the general theory applied to this particular case; it is, however, but approximately true, and holds only on the assumption that the biaxal energy of the crystal is small,—an assumption justified by the phænomena of all crystals hitherto examined.

The general phænomena being observed, it remained to take measurements, and to compare them with the results of theory. For this purpose I determined the magnitude of a section of the cone, at a considerable distance from the crystal, by observing, with the assistance of a small telescope, the points at which the aperture ceased to be visible by means of the transmitted light. The distance being then accurately measured, the angle of the cone could be obtained from a table of tangents. This angle was thus found to amount to 6° 14′ in the plane of the optic axes, and to 5° 46′ in the perpendicular plane,—the mean being exactly 6°. I then placed the flame of a wax taper at the centre of this section, and removing the plate from the second surface of the crystal, placed a mark at a considerable distance on the line of the reflected ray. Then placing a Hadley's sextant with its centre in the place of the crystal, I measured the angular distance between the flame and the mark. This angle was found to be 31° 56′, and consequently the angle of emergence corresponding to the axis of the cone was 15° 58′.

Now assuming the three indices for arragonite to be 1·5326, 1·6863, 1·6908, which are the indices for the mean ray E, as determined by Professor Rudberg*, Professor Hamilton has shown that the direction of the emergent rays in the plane of the optic axes will be given by the formulæ

$$\sin R_o = 1{\cdot}6863 \, . \sin I$$
$$\sin R_e = 1{\cdot}68708 \, . \sin (I - 1^\circ\, 44'\, 48'')$$

in which I is the internal angle of incidence, or the angle which the cusp ray makes with the normal to the surface of emergence; and R_o, R_e are the corresponding angles of refraction in air. But in the present instance the normal to the surface of emergence bisects the angle of the optic axes, and therefore I = 9° 56′ 27″. Consequently R_o = 16° 55′ 27″,

* See Lond. and Edinb. Phil. Mag. and Journ., vol. i. p. 140—141.—Edit.

and $R_e = 13° 54' 49''$. Now the difference of these angles, or $3° 0' 38''$, may be called the angle of the cone; and half the sum, or $15° 25' 8''$, is the mean angle of emergence. The angle $15° 58'$, found above, differs from this by $33'$ only; but the observed angle of the cone is about double of that given by theory.

I also measured the angle of the cone by receiving it on a screen of roughened glass at different distances, and tracing the outline of the section on the screen: the diameter of this section and the distance being then measured, the angle was determined. Three measurements taken in this manner gave for the magnitude of that angle respectively $6° 24'$, $5° 56'$, $6° 22'$, the mean of which, $6° 14'$, agrees very nearly with that determined by the former method.

Conceiving that the difference between experiment and theory arose chiefly from the rays which were inclined to the optic axis all round at small angles, and which were transmitted at the second surface in consequence of the sensible magnitude of the aperture, I determined to try the effects of apertures of various forms and dimensions.

When the aperture was at all considerable, two concentric circles were seen to surround the optic axis, the interior circle having about double the brightness of the annulus which surrounded it. The light of the interior circle was unpolarized, while that of the surrounding annulus was polarized according to the law already mentioned. When the aperture was diminished, the inner circle contracted in diameter, the breadth of the outer annulus remaining nearly the same; until the former was finally reduced to a point in the centre of the exterior circle. When the aperture was still further diminished, a dark space sprung up in the centre, which enlarged as the aperture decreased; until finally, with a very minute aperture, I succeeded in rendering this space about $\frac{3}{4}$ of the whole, or of reducing the breadth of the luminous annulus to about $\frac{1}{8}$ of its exterior diameter.

With this diminished aperture I examined the appearance produced by a line of light on the first surface parallel to the plane of the optic axes. The swelling curves, which it has been already remarked, surrounded the optic axis in this case, were reduced to a breadth corresponding to that of the annulus in the former experiment, and were separated by a considerable dark interval. When the plane passing through the two apertures deviated a little from the plane of the optic axes, the phænomena underwent many beautiful changes, the curves assuming in all cases the form of the conchoid, whose pole was the projection of the optic axis, and asymptote the line on the first surface.

Finally, when the apertures on the two surfaces were transposed, I found that no change was made in the resulting phænomena; and that they seemed, in fact, to be in all respects similarly related to the surfaces of incidence and emergence.

It is easy to render an account of these various appearances. When the aperture on the second surface is considerable, the rays proceeding to its circumference from a point on the first surface will be sensibly inclined to the optic axis, which we shall suppose to be in the line connecting the point with the centre of the aperture. Consequently the interior, as well as the exterior rays, into which each of them is divided, will be inclined *outwards*; and it is obvious that there will be a central bright space, every point of which is illuminated by one interior and one exterior ray. This space therefore will have double the brightness of the surrounding space, each point of which is illuminated by one ray only; and as the rays which combine to form it are polarized in planes at right angles to one another, the resulting light will be unpolarized.

When the aperture is diminished, the inclination of these interior rays to one another decreases; until finally they become parallel, and the central bright space is reduced to a point. When the aperture is still further diminished, the interior rays become inclined *inwards*, and cross; and it is obvious that beyond the point of junction there will be a dark space illumined by no ray whatever. As there is no meeting of rays oppositely polarized in this case, the whole of the light will be polarized, and according to the law already explained. Finally, when the aperture is still further diminished, the interior rays at one side approach to parallelism with the exterior at the other; and the central dark space enlarges, and approaches to equality with the outer and limiting cone. Thus the annulus of light is indefinitely diminished in breadth, and the cone approaches to a mathematical surface.

It will be easily seen that the angle of the true cone is, nearly, half the sum of the angles of the exterior and interior limits of the observed conical annulus; and that when a bright space appears in the centre, as is the case when the aperture is considerable, the true angle is half the difference of the angles of the interior and exterior cones. When the whole cone is of uniform brightness, and the central dark space reduced to a point, the observed cone is just double of that sought.

Now this last was very nearly the case in the experiments from which the measures already mentioned were taken; and consequently the corrected angle, being in this case half the observed, coincides very nearly with that deduced from theory.

As there must be an equal cone of rays incident upon the first surface of the crystal, I took other measurements with a view to determine its magnitude. For this purpose I placed a rough micrometer, consisting of two moveable metallic plates, immediately before the lens, and closed the plates, until on looking through the aperture on the second surface I could see them just touching the opposite sides of the circular images. The same thing was done for the interior circle of the annulus, and the focal length of the lens accurately measured. In this manner the extreme dimensions of the conical annulus were ascertained, and the true angle calculated. The mean of three such measurements gave 3° 47′ for the corrected angle of the cone.

It has been observed that the theoretical angle of the cone has been computed from the three indices of refraction as determined by M. Rudberg. Now a very small error in the determination of these indices, or a very minute difference between their values in different specimens of the same mineral, would make a considerable change in the angle. On the other hand, the effects of diffraction must in some degree modify the experimental results. And hence, though the measures were not taken with all the means to insure accuracy of which they are susceptible, it will be seen that their correspondence with theory is as close as could be reasonably expected.

XXII. *Of the Structure of Living Fabrics. By the Rev.* PATRICK KEITH, *F.L.S.*

[Continued from p. 16.]

(*External Structure continued.*)—*The Branches.*

THE Branches are the divisions of the trunk, or *caudex ascendens*, originating generally in the upper extremity, but often also along the sides. The primary divisions are again subdivided into secondary divisions, and these again into still smaller divisions till they terminate at last in slender twigs. In their insertion or distribution they are opposite, or alternate, or verticillate, or scattered. In their position they are vertical, that is, lying close to the stem; or spreading, that is, forming a conspicuous angle with the stem; or divergent, that is, expanding horizontally; or deflected, that is, hanging down so as to form an arch, as in the Weeping Ash, or Willow. In their size they are proportioned to the dimensions of the trunk, expanding in trees of large growth to a great distance from the centre, and forming a sort of secondary trunk. The horizontal branches of a full-grown Calabash-tree are said to be from forty to fifty feet in length*; while the horizontal branches of what is called the Live-oak, of East Florida, are said to extend to upwards of fifty paces†.

The most beautiful specimens that England affords, of plants with wide-spreading branches, are undoubtedly those of the Beech-tree; and the finest we have ever had an opportunity of admiring, are those in Eastwell Park, Kent, a seat of the Earl of Winchelsea. Expanding their aged and venerable arms in the full maturity of their growth, dignified in their elevation, and clothed with the pleasing verdure of their glossy leaves, they excite in the breast of the spectator emotions approaching to those of the sublime; and yet they are not so conspicuously remarkable for vastness of dimension as for beauty and symmetry of contour. The measurement of one of the handsomest of them was as follows: girth of the trunk, close to the soil, from eighteen to twenty feet; height, clear of branches, from seven to eight feet; horizontal growth of the lower branches, thirty-six feet, which, with half the width of the stem, gives a semidiameter of thirty-nine feet, and consequently a diameter of seventy-eight feet; slanting extent of the upper branches, such as to give to the group a sort of globular contour, as regular as if it had been clipped with shears, with an estimated elevation of from fifty to sixty feet. Think of the cooling and delightful shade afforded by this ample expansion, as filled up with its summer garniture of leaf and flower, and you have a type before you similar to that from which Virgil drew when he said or sang,

> Tityre, tu *patulæ* recubans sub tegmine *fagi*,
> Sylvestrem tenui Musam meditaris avena.—*Eclog.* i. 1.

Indeed we are of opinion that no reader of Virgil is competent to form a correct or adequate idea of the beauty of the distich now quoted, till he has seen some such trees as those now described. The Beech-trees of Knowle Park, near Seven Oaks, are said to be of dimensions still larger; and the far-famed Beeches of Knockholt are said to be the largest in England.

The Leaf.—The leaf, which belongs to the division of the temporary parts of the plant, is a thin and flat substance, of a green colour, issuing generally from the extremity of the branches, but sometimes also immediately from the stem or root, and distinguishable by the sight or touch, into an upper and under surface, a base and an apex, with a midrib and lateral nerves. Yet leaves are not always thin and flat, nor are they always green. The leaves of the Aloe are thick and fleshy; and the leaves of the several species of Beet-root are

* *Famil. des Plant.* Pref. ccxii. † Bartram's Travels.

of a dark and dull purple. Neither are they always furnished with transverse or lateral nerves. These are proper to Dicotyledonous plants only, for in Monocotyledonous plants the nerves are all parallel. The point by which the leaf is attached to the plant, is the base; the opposite and terminating point is the apex; the intermediate body of the leaf is the expansion, and the boundary of the leaf is the margin. It often happens that the base of the leaf is prolonged into a sort of semi-cylindrical pedicle, by which the expansion is removed to some distance from the point of attachment, as in the Vine and Poplar. This pedicle is denominated the footstalk or petiole, entering the expansion generally by the margin, but sometimes also by the centre, as in Nasturtium. In *Populus tremula* it is compressed in a line at right angles to the expansion, which peculiarity some phytologists regard as the cause of the leaf's mobility*.

The figure of the leaf or expansion has been found to be of great use to botanists in the discriminating of the several species of a genus; and hence they have spared no pains to determine by observation and description its varieties of form. Linnæus enumerates more than a hundred†. If the expansion is flat and membranaceous, the most frequent forms are the circular, the oval, the oblong, the triangular; if thick and succulent, the most frequent forms are the cylindrical, the semicylindrical, the sword-shaped, the compressed. The apex is acute, or obtuse, or bitten, or truncated, as in the leaves of the Tulip-tree. The margin is entire, or notched, or toothed, or serrated. The expansion is entire, or cleft, or lobed; yet the figure of some leaves is altogether anomalous, and cannot be brought under any of the foregoing divisions. The leaf of *Nepenthes distillatoria*, which is itself lanceolate, terminates at the summit in a sort of thread-shaped pedicle, supporting a hollow and cylindrical, or rather pitcher-shaped appendage, to which there is attached the curious and peculiar process of a lid opening at one side. This appendage secretes a fluid which is said to be very pleasant to the taste. The leaf of *Dionæa Muscipula* is furnished with a process issuing from the apex, which has a slight resemblance to a steel trap with the wings expanded. This singular appendage is so highly irritable, that if it is but touched with the point of any fine or sharp instrument, or if an insect but alights upon it, the lobes immediately collapse as if eager to seize their prey, and detain the insect captive, so that it resembles a trap, to which it has been compared, not only in form but in function.

In their size, leaves exhibit as much variety as in their

* *Cours de Phytol. Séance*, i. 24.

† *Phil. Bot.*, sect. 83.

figure. But it is not always the largest plant that has the largest leaf. The leaf of *Caltha palustris*, though a humble herb, is larger than the leaf of the oak, though a lofty tree. The largest leaf produced by any species of British plant, is, as I believe, that of *Arctium Lappa*, or of *Tussilago Petasites*, which is often to be met with of the dimensions of upwards of twenty inches in length by eighteen at the greatest breadth. The leaves of *Strelitzia Reginæ* grow to a length of three or four feet, with a breadth of eighteen or twenty inches at the widest. The leaves of the Plantain-tree (*Musa paradisiaca*) have been known to grow to the extent of ten feet in length by two feet at the basis*; so that, owing, perhaps, to their extraordinary dimensions, some writers have supposed them to be the leaves of which Adam and Eve are said to have made themselves aprons, when they first felt the want of clothing†. But the largest of all simple leaves is, doubtless, that of the Talipat-tree (*Corypha umbraculifera*), a native of Ceylon, said to be often met with of such a magnitude as to measure not less than eleven feet from the base to the apex, by sixteen feet across at the widest part, giving, thus, an ample circumference of nearly forty feet, and forming, when fully expanded, a most capacious and efficient parasol.

The leaves of trees, from their size or number, are naturally well calculated to form an agreeable and cooling shade, amidst the sultry heats of the intra-tropical regions,

.... where broad Palmettos shower
Delicious coolness in the shadowy bower;—(*Montgomery's West Indies.*)

and even in countries that are not within the tropics the shade afforded by the leaves of trees is still extremely desirable during the heats of summer. Hence the soft and balmy slumbers which an ancient poet experienced under the cool and delightful shade of the Plane-tree;

Αὐτὰρ ἐμοὶ γλυκὺς ὕπνος ὑπὸ πλατάνῳ βαθυφύλλῳ.—*Mosch. Idyll.* v.

Hence also the celebrity of the groves of Academus, where Plato and his successors delivered their lectures in philosophy, and instilled into the minds of their youthful followers the love of truth:

Scilicet ut possem curvo dignoscere rectum,
Atque *inter sylvas Academi* quærere verum.—*Hor. Epist.* II. lib. ii. 44.

The odour of many plants, which is extremely grateful to the smell, as well as their virtues, whether medical or dietetical, is very frequently contained in the leaf. Lastly, as the leaf is merely a temporary or deciduous part, it dies in the autumn or winter, and is regenerated in the succeeding spring,

* Lour. *Flor. Cochin.*

† Gen. iii. 7.

exhibiting an apt and edifying emblem of the succession of human generations, according to the beautiful remark of the greatest of all poets:

> Οἵη πὲρ φύλλων γενεὴ, τοίηδε καὶ ἀνδρῶν.
> Φύλλα τὰ μέν τ' ἄνεμος χαμάδις χέει, ἄλλα δέ θ' ὕλη
> Τηλεθόωσα φύει, ἔαρος δ' ἐπιγίγνεται ὥρη·
> Ὣς ἀνδρῶν γενεὴ, ἡ μὲν φύει, ἡ δ' ἀπολήγει.—*Homer. Iliad.* vi. 146.

While it exists, however, it forms one of the principal ornaments of the plant, clothing it with verdure and covering it with grace; and even in its decay and fall it ceases not to gratify the eye, assuming, by slow degrees, a paler and a milder shade, and tingeing the forest and the plain with an infinite variety of hues.

The Bulb.—The bulb, an appellation borrowed from the Latin term *bulbus*, a round or bulbous root,—

> Candidus Alcathoï qui mittitur urbe Pelasga
> *Bulbus*, et ex horto quæ venit herba salax,—(*Ovid. Art. Am.* lib. ii. 42.)

is a soft succulent substance, of an oval or globular figure, containing the rudiments of a future plant, and situated upon the root, stem, or branch, from which it ultimately and spontaneously detaches itself, and forms a new individual. If situated upon the root, it is said to be radical; if upon the stem or branches, it is said to be caulinary. The radical bulb therefore is not a root; but it is, as Linnæus has well defined it, "the winter quarters of the future plant*," furnished with a root suitable to its peculiar structure; that is, with an apparatus of radical fibres issuing from its base. It is solid, as in *Crocus sativus*; or coated, as in *Allium Cepa*; or scaly, as in *Lilium candidum*.

The caulinary bulb originates in the axis of the leaves, as in *Dentaria bulbifera*; or at the base of the umbel of flowers, as in *Allium carinatum*. In either case it is nourished by the parent plant till it reaches maturity, when the bond of connexion is dissolved, and the bulb falls to the ground, endowed with the capacity of striking root in the soil, by sending out fibres from the base, and so converting itself into a new plant. The flowers of bulbous plants have great beauty,—a property of which poets, as well as florists, have always known how to avail themselves. If Anacreon has a wreath or a garland to weave, he is sure to insert into it a due proportion of lilies; and so are also the modern sons of song.

> Ὅρα, κἂν στεφάνοισιν
> Ὅπως πρέπει τὰ λευκὰ,
> Ῥόδοις, κρίνα πλακέντα.—*Anac.*, Ode xxxiv.

Some bulbs are useful as articles of food, or rather as giving

* *Phil. Bot.*, p. 50. sect. 85.

a seasoning to food, such as the Common Onion; others are useful in medicine, such as the Squill or Sea Onion; and all of them are peculiarly tenacious of potential life if excluded from the action of the atmosphere. An Egyptian mummy that was lately unswathed in this country, was found to grasp in its hand a bulbous root. When exposed to the atmosphere, it germinated; and when placed in the soil, it grew with great rapidity*. It could not have been less than two thousand years old.

The Bud.—The bud is a small and ovate or conical-shaped substance, issuing from the axis of the leaves, or extremity of the branches, and containing the rudiments of future branches, leaves, or fruit; but not detaching itself spontaneously from the plant and forming a new individual. It is composed externally of a number of concave and overlapping scales, that protect the inclosed germ from the injuries of the atmosphere, and is connected with the stem or branch by means of a short and fleshy pedicle, in which the scales originate. The bud of the American Walnut is said to be the most magnificent of all known examples, though the bud of the Horse Chestnut (*Æsculus Hippocastanum*) is, as I believe, but little inferior to it.

Buds produce leaves only, or flowers only, or leaves and flowers together. The two former varieties may be seen in the buds of the Peach-tree; the latter, in those of the Horse Chestnut. Yet all plants are not furnished with buds. Annuals and many shrubs have none; and even trees and shrubs to which they are proper, do not produce buds in hot climates. But in this country and in all cold countries, trees and shrubs are universally furnished with buds; and without the intervention of a bud, no new part is added to the plant. Buds have not been found to be of much use to botanists in the discrimination of species, though they may serve occasionally to distinguish plants in the winter; and gardeners do, in fact, distinguish almost all their plants by the bud.

The Flower.—The flower, which, like the leaf, belongs to the division of the temporary parts of the plant, is an organ that issues generally from the extremity of the branches, but sometimes also from the root, stem, and even leaf; being the apparatus destined by nature for the production of the fruit, and being distinguishable, for the most part, by the brilliancy of its colouring, or the sweetness of its smell. It has been happily styled by Pliny, The joy of plants,—*Flos gaudium arborum*†; of which the Lily, the Tulip, and the lovely Rose, so sweetly sung by Anacreon of old, are magnificent examples:

* Journ. of Royal Instit., Oct. 1830.

† *Hist. Nat.*, lib. xvi. cap. xxv.

Ῥόδον ὦ φέριστον ἄνθος,
Ῥόδον ἔαρος μέλημα,
Ῥόδα καὶ θεοῖσι τερπνά.—Ode v.

One of the most splendid of all known flowers is that of the Laurel Magnolia, of East Florida, which, when fully expanded, gives a width across measuring not less than from six to nine inches*. But a much larger flower still is that of *Aristolochia cordifolia*, which is said to give a breadth across of at least sixteen inches. This is enormous; and yet it is little in comparison of the extraordinary and gigantic dimensions of the fully expanded flower of *Rafflesia Arnoldi*, which displays a diameter, as ascertained by actual measurement, of not less than three feet†.

Flowers, in their mode of attachment, are either sessile, as in Agrimony, or supported upon a peduncle, as in the Cowslip. In their direction they are upright, or bending, or nodding, or unilateral, that is, attached to one side only of the stem, as in Lily of the Valley. If they issue from the root, they are radical; if from the stem, caulinary; if from the branch, ramial; if from the leaf, foliary, as in Ruscus. But in all their varieties, they are obviously divisible into the following distinct parts,—the calyx, the corolla, the stamens, the pistil.

The Calyx,—an appellation borrowed from the Greek term κάλυξ, which signifies an unexpanded blossom or its covering,—is the exterior envelope of the flower, encompassing and protecting the interior parts. It may be perceived very distinctly in a Rose not yet fully blown, or in a Poppy beginning to open. Yet it is not to be regarded as altogether essential to the idea of a flower, for in many flowers it is wanting. The Tulip has no calyx. But in the flowers of perfect plants it is very generally present under the modification of perianth, glume, or scale. The perianth is a calyx that encircles the flower completely, and often assumes the similitude of a cup. In the case of the Acorn, the similitude is perfect. It is either proper to a single flower, as in Primula; or common to several flowers, as in Tragopogon. It consists of a single and undivided piece, as in Primula; or of several distinct pieces, as in Rumex. It is caducous or deciduous, or permanent and persistent, as in St. John's Wort. The glume is a chaffy and membranous substance, accompanying the flowers of the Grasses, and constituting their calyx, but not so formed as to resemble a cup. Yet, if it be true that there is no rule without an exception, a cup-shaped glume must exist. The outer covering of the flowers of *Cornucopia cucullata* has been thought to present that exception. But botanists seem

* Bartram's Travels. † Linn. Trans. vol. xiii. Part I.

now agreed to regard it as an involucrum. The Scale is a thin, chaffy, and membranaceous substance, forming part of the fructification of a variety of plants that produce incomplete flowers, and constituting their calyx. It may be seen in the catkins of the Willow and in the cones of the Fir. In the former it is a proper calyx, in the latter it is a common calyx.

The Corolla is the interior envelope of the flower, investing the central parts, but invested by the calyx. It is generally of a finer and more delicate texture than the calyx, and is of all the parts of the fructification the most showy and ornamental, being always, or with but few exceptions, that which is the most highly coloured, as well as that from which the flower imparts its rich and fragrant perfume,—its *croceos odores*,—delighting at the same time both the sight and smell. To this most elegant part of the fructification, the term Corolla has been very happily applied by Linnæus, signifying as it does in the original, a crown or chaplet of flowers.

> Et modo solvebam nostra de fronte *corollas.*—*Propert.* i. 5. 21.

Like the calyx, the corolla consists either of a single piece called a petal, or of several distinct pieces called petals. In the former case it is said to be monopetalous; in the latter case it is said to be dipetalous, tripetalous, or tetrapetalous, according to the number of distinct petals. The monopetalous corolla is regarded as divisible into three parts,—the tube, the mouth, the border. The tube is the lower portion, cylindrical and inflated. The mouth is the middle portion, often beset with fine hairs, or with small projecting scales, so as nearly to shut it up. The border is the upper and expanding extremity. In its general contour it is bell-shaped, or club-shaped, or funnel-shaped, or wheel-shaped. In the polypetalous corolla, each petal is regarded as divisible into a claw and border, the aggregate contour assuming many forms, amongst which the most remarkable are the cruciform and the papilionaceous. Like the calyx the corolla is not to be regarded as absolutely essential to the botanical notion of a flower, because, in some flowers, it is wanting. Yet, where one only of the two envelopes is present, it is sometimes a matter of considerable difficulty to say which of them it is. Is it a calyx, or is it a corolla? Botanists have laid down several rules on this subject, but no one that is quite satisfactory. In cases of doubt we must be guided by analogy.

The stamens,—an appellation borrowed from the Latin term *stamen*, a thread,—

> Et gracili geminas intendunt *stamine* telas,—(*Ovid. Met.* vi. 54.)

are substances of a very slender fabric, and of a thread-shaped figure, surmounted with a small bag or *viscus*, and situated immediately within the corolla, to which they are sometimes attached. A very good example of them may be seen by opening up the blossom of a Tulip or of a Lily. They are apparently of no importance in the eye of the vulgar spectator, but are essential to the botanical notion of a flower, because indispensable to the formation of perfect fruit. The calyx is sometimes wanting, and the corolla is sometimes wanting; or the calyx and corolla both, as in Euphorbia; but the stamens are never wanting, except through adventitious or accidental causes. On the number of stamens, Linnæus has founded the first twelve classes of his artificial method; so that if any flower is furnished with but one stamen, it is to be referred to the first class; if with two, to the second class; if with three, to the third, and so on in succession. The remaining classes are founded on other peculiarities.

Stamens are usually regarded as consisting of two parts,—a filament and an anther. Yet the filament is not an indispensable part of the apparatus of a flower. There are many flowers without a filament, but no flower without an anther. It is a viscus of one or more cells containing a powder, which botanists denominate the pollen, and which, at the period of the maturity of the flower, bursts its integuments and explodes.

The pistil is a small and column-shaped, but often pestle-shaped substance, occupying almost invariably the centre of the flower, and encompassed immediately by the stamens,—that is, when the plant is hermaphrodite. In monœcious and diœcious plants, this arrangement cannot take place. It is solitary as in the Cherry, or multiplicate as in the Apple and Pear; and is divisible, at least, into two, but very often into three distinct parts,—the ovary, the style, and the stigma. The Ovary is the lower extremity of the pistil, supporting the style and stigma, and containing the rudiments of the fruit. In its attachment, it is sessile or stipitate, inferior or superior; and in its figure it is globular, or egg-shaped, or oblong, or compressed, as in the Vetch. The Style, the middle portion of the pistil, is a prolongation of the substance of the ovary, issuing generally from its upper extremity, and supporting the stigma. It is deciduous, and falls when the ovary is ripe or permanent, and adheres to the fruit. The Stigma is a small and glandular-looking substance, crowning the style, and hence also denominated the summit. Yet it is sometimes, though rarely, lateral, as in Scheuchzeria. In its figure it is globular, or hemispherical, or conical, or petaloid; and in its duration it is like the style, sometimes deciduous, and sometimes persistent.

Flowers are often found to be furnished with certain additional and supernumerary parts, denominated appendages; such as the involucre, the spathe, the bracte, the nectary. The first three are analogous to leaf or calyx, and demand no particular notice in our present brief view. The latter is analogous to corolla, and is peculiar both in its form and function. It is defined to be an appendage of the flower secreting or containing a honied juice. Its function was detected by Malpighi, but he gave it no name*. This was reserved for Linnæus, who applied to it the very appropriate appellation of *nectarium*—the nectary or honey-cup, from *nectar*, the drink of the gods.

> Illum ego lucidas
> Inire sedes, ducere *nectaris*
> *Succos*, et ascribi quietis
> Ordinibus patiar Deorum.—*Hor.* lib. iii. Ode iii.

It assumes a great variety of shapes and situations in different genera of plants, and resembles a horn, or a cylinder, or a slipper, or a cowl, or a petal, or a pore, or a gland. It is attached generally to the corolla, but occasionally to the receptacle, or calyx, or stamens; and even to the anther or pistil, as in Adenanthera, and Cheiranthus.

The Fruit.—In the progress of fructification, when the several organs of the flower have discharged their respective functions, the petals, the stamens, the style, and often also the calyx, wither and fall.

> Nec viola semper, nec hiantia lilia florent,
> Et riget amissâ spina relicta rosâ.—*Ovid. De Art. Amat.* lib. ii. 115.

The ovary alone remains attached to the plant, and swells and expands till it reaches maturity. It is now denominated the fruit. In popular language the term is confined chiefly to such fruits as are esculent, as the Apple, the Peach, and the Cherry; but with the botanist, the matured ovary of every flower, with the parts contained, constitutes the fruit. Hence the position or distribution of the fruit upon the plant will be the same with that of the flower which preceded it; radical, if the flower was radical; terminal, if the flower was so. The figure of the fruit exhibits a very great variety of modifications, which it would be tedious to enumerate. But the spherical, or elliptical, or cylindrical forms are, perhaps, the most common. The size of the fruit is also very various, yet not at all in proportion to the plant producing it. The Oak and the Ash, though among the largest of trees, produce a fruit that is comparatively very small; while the Gourd, whose stem is but herbaceous and creeping, produces a fruit of a most enormous bulk. The surface of the fruit is very generally smooth,

* *Anat. Plant.* 47.

and in many cases exquisitely coloured; so that the beauties of the departed flower have but given way to the beauties of the ripened fruit; the mellow tints of autumn being equally pleasing with the bloom of spring, and the complexion of the Peach and Apricot being nothing inferior to that of the blossom which preceded them.

> ....Cum decorum mitibus pomis caput
> Autumnus arvis extulit;
> Ut gaudet, insitiva decerpens pyra,
> *Certantem et uvam purpuræ.—Hor. Epod.* ii. 17.

Fruits may be regarded as composed of two distinct and constituent parts,—the pericarp, and the seed. The Pericarp is the exterior portion of the ripened ovary, constituting, for the most part, its principal mass. Pericarps are distributed by botanists into the following species, though several of the terms are applied to the fruit also,—the capsule, the pome, the berry, the drupe, the silique, the legume, the cone. The capsule is a dry and membranaceous pericarp, separating when ripe into valves. The pome is a fleshy pericarp, inclosing a capsule, as in the familiar case of the Apple or Pear. The berry is a soft and pulpy pericarp, containing one or more seeds, but not inclosing a capsule. The drupe is a soft and pulpy pericarp, inclosing a nut; it is exemplified in the Cherry and Peach. The silique and legume are pods of different species, the one exemplified in Shepherd's Purse, the other in the Pea. The cone or strobile is the scales of the catkin, as exemplified in the genus Pinus.—The Seed, the last and most noble part of the fruit, is the interior portion of the ripened ovary, contained within the pericarp, and containing the rudiments of a new plant similar to that from which it sprang. In the Pea and Bean it is that part of the fruit which is eaten. In the Apple it is that part which is rejected and lodged within the core. Its figure, like that of the flower and fruit, is very much diversified. It is globular, or egg-shaped, or oblong, or kidney-shaped, or lenticular. Its magnitude is estimated by four cardinal points, instituted by botanists, and serving as a gauge or standard, through the application of which it is regarded as being large, middle-sized, small, or minute. It is smooth as in Linum, or furrowed as in Vinca, or wrinkled as in *Dianthus inodorus*, and is susceptible of the same modifications of shade as the flower and fruit. In Pæonia it is of a deep or dark purple; in *Croton cayanospermum*, it is of an azure blue; in *Abrus precatorius* it is of a rich scarlet; and in Coix, it is white as snow. On the surface of the seed, and at the point of its attachment to the pericarp, there is always to be found a mark or scar, differing in colour and in grain from the rest of the surface. It is the scar left by the natural fracture of the umbilical cord. Linnæus gave it the appellation of the *Hilum*, which the term scar translates.

The number of seeds produced by a single flower is extremely different in different plants. In some plants a flower produces only one seed; in others two, in others three, and in others many. But the great fertility of some species is altogether astonishing. A single stalk of *Zea Mays* will produce two thousand seeds*; a single plant of *Inula Helenium*, three thousand; and a single spike of *Typha major*, ten thousand. A single plant of Tobacco has been found, by calculation, to produce the almost incredible number of 360,000†; a single stalk of Spleen-wort has been thought, by estimation, to produce at least a million of seeds.

Like bulbs, seeds are also extremely tenacious of potential life. If well preserved from the action of the atmosphere, they will retain their vitality for thousands of years. Among the mummies lately unswathed in this country, one was found to grasp in its hand some grains of Egyptian wheat. When put into the soil they germinated, and grew, and sprang up, as if they had been the produce of the year preceding.—So much for the external structure of perfect plants.

[To be continued.]

XXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Address of His Royal Highness the President, delivered at the Anniversary Meeting, Nov. 30, 1832.

Gentlemen,

This is the Second Anniversary of my election to the Chair of the Royal Society, and I gladly avail myself of the opportunity which it affords me of renewing the expression of my gratitude to you for the distinguished honour conferred upon me in electing me to it, and still more for the continued kindness and support which I have received from you in the execution of the duties of my office. I can only assure you, Gentlemen, that if it be your pleasure that I should continue to fill this Chair, I shall feel an additional motive to induce me to devote my most earnest attention to the promotion of the interests of the Society, in the perfect reliance which I place upon your cooperation and assistance, and in the confident expectation which I entertain, that in case I should fail in the due and efficient discharge of any of my duties, I shall experience from you the most kind and liberal interpretation of my motives and conduct.

In making my acknowledgments to the Fellows of the Society at

* Linn. *Phil. Bot.*, sect. 132. † *Hist. Plant. Raii,* lib. i. cap. xii.

large for their uniform kindness and support, it would be injustice and ingratitude on my part were I not to return my public and especial thanks to the Vice Presidents, Treasurer, Secretaries, and the other Members of the Council—

To the Vice Presidents, as well for their general services as also for their kindness in supplying my place in this Chair, when I have unfortunately been compelled to be absent from the state of my health, or from the immediate necessity of discharging other most pressing public duties.

To the Treasurer, for his vigilant attention to the finances of the Society, and to every arrangement which may in any manner tend to promote the usefulness of the Institution, and increase the accommodation of its Members.

To the Secretaries, for their courteous discharge of their various and very laborious duties: and to the Members of the Council collectively, for their regular and punctual attendance at all the meetings to which they have been summoned, and for the zeal and readiness with which they have undertaken any labour, however considerable, which the interests of the Society might require them to perform.

The Report of the Council which will be read to you by one of your Secretaries, Dr. Roget, will make known to you various matters connected with the administration of the Society, and also the arrangements adopted for supplying the deficiencies of the Library in different departments of science, and for rendering it more generally accessible, and therefore more useful, by means of complete and well classed catalogues. I must refer you likewise to the same Report for a statement of the grounds upon which two Copley Medals have this year been adjudged, one to Mr. Faraday, and the other to Mons. Poisson. There is, however, one arrangement, admirably calculated, in my opinion, to increase the usefulness and to uphold the credit of the Royal Society, which that Report does not notice; I mean the Resolution adopted by the Council to allow no Paper to be printed in the Transactions of the Royal Society, unless a written Report of its fitness shall have been previously made by one or more Members of the Council, to whom it shall have been especially referred for examination. This Resolution has been acted upon for the greatest part of the last year, and some of those Reports of a favourable nature have been read before the Society, and printed in the Abstracts of our Proceedings. When the number of papers which come before the Society in the course of a year is considered, as well as the great diversity and occasional difficulty of the subjects which they embrace, it will be at once seen how greatly the labours and responsibility of the Members of the Council must necessarily be increased by the rigorous adoption of such a system. It is in consequence of the important influence which this plan is likely to have upon the well-being of the Society, that I am induced to enter somewhat in detail into the reasons which have led to its adoption.

It has long been the custom of many Foreign Societies, and particularly of the Academies of Science and of Medicine at Paris, to require written Reports upon every paper submitted to them, from a Committee of their Members: as the persons who are selected for this duty are frequently veterans in their respective sciences, who have earned by their labours an European reputation, the Reports which are thus produced prove often more valuable than the original communications upon which they are founded, and the collections of them, as is well known, form a most important part of the stock of modern science. Many other advantages also have been found to result from the adoption of this practice. The decisions of men who are elevated by their character and reputation above the influence of personal feelings of rivalry or petty jealousy, possess an authority sufficient to establish at once the full importance of a discovery, to fix its relations to the existing mass of knowledge, and to define its probable effect upon the future progress of science. They thus operate as a powerful stimulus to the exertions of the genuine cultivators and lovers of science, who feel assured that their labours will be properly examined and appreciated by those who are most competent to judge of their value; whilst, at the same time, they tend to keep under the obtrusive and turbulent pretensions of those who presume to claim a rank as men of science, for which they possess no just title or qualification.

It was from a conviction that many similar advantages would result from such a system of Reports in the Royal Society, that the Members of the Council were induced to agree to its adoption; and it is to be hoped that, when a longer experience has given to such a plan a more complete organization, and has shown the practical extent to which it can be conveniently carried, it will then become a permanent law of the Council.

In order, however, to secure its full advantages, it will be necessary that the Council should, in all cases, include men eminent for their proficiency in all those branches of science which usually come, or are likely to come, under the notice of the Society. That such men may be found, I feel satisfied, both from my past experience and from my knowledge of the many distinguished persons who adorn the lists of this Society; and that such men would generally be ready to undertake the performance of a duty, requiring the occasional sacrifice both of time and labour, I cannot venture to doubt, without imputing to them a charge of indifference to the interests and the usefulness of the Royal Society, and even a want of proper sympathy with the scientific honour of their country.

I think myself justified in using such strong language, Gentlemen, because I believe the scientific character of this country to be most intimately associated with the scientific character and estimation of the Royal Society. One of the most illustrious of modern mathematicians and philosophers, himself a foreigner, has said that the Royal Society has contributed more to the progress of science than the combined labours of all other similar institutions; and though it would be unfair to interpret too literally the language of a compliment, yet it would not be very difficult to vindicate its general truth and justice.

It was this Society which fostered and encouraged the early labours of Newton, and under its auspices was published the work which constitutes, and probably ever will constitute, the proudest monument of the genius of man: and from the period which immediately followed its foundation, the age of Wallis, and Newton, and Wren, and Hook, and Halley, and Taylor, to that of Herschel and Cavendish, and Wollaston, and Young, and Davy, its Transactions contain records of almost every important discovery in natural philosophy; of almost every experimental inquiry which has been most remarkable for its difficulty, delicacy, or importance, and of almost every original speculation which has most contributed to the advancement of science.

It becomes us therefore to guard these national archives of the progress of knowledge, with the reverence which is due to them as monuments inseparably connected with our own national honour; and to watch with our utmost care, lest any addition should be made to them, which can be considered as unworthy of the character of the stock upon which it thus becomes engrafted; and it therefore is the bounden duty of every Fellow of this Society, whether it be considered as imposed on him by the terms of the Obligation which he signed at the period of his admission as a Member, or as derived from the still higher and more comprehensive ties which bind every friend of the great institutions of his country, to maintain their efficiency and credit, and to allow no private or personal cause of jealousy or discontent, no trivial or unfounded plea of want of leisure from business, or occupations, to interfere with the devotion of his best exertions to uphold the character and promote the interests of the Royal Society.

There are some reasons which I know may, and very probably will be urged against the reasonableness of expecting that any considerable number of men of science, should be able, however willing they might otherwise be, to devote any large portion of time or labour to the service of any Society, let its claims upon them be ever so strong.

In this country, where wealth is the general measure of the social rank of families at least, if not of individuals, men of science must either possess an independent fortune, or they must pursue it, as is most generally the case, in connexion with a laborious profession; for we have few establishments which afford them support, independently of other employments; and even in those very rare cases, the provision which is made is so small, that no man of superior education can look forward to the attainment of the advantages which science and learning offer, in forming his scheme of life, unless he be prepared to make the most serious sacrifices. It is for this reason, that the learned professions, presenting as they do the most brilliant prospects of rank and wealth, generally absorb, in the progress of life, the studies and exertions of young men of the highest scientific education and promise; for, however strong may have been their attachment to the studies of their youth, and however ardent their ambition to obtain the honours of science, they soon find that such pursuits retard their professional advancement.

In other countries, however, where the learned professions are neither richly paid nor highly honoured, and where the exclusive cultivation of particular branches of literature and science presents the readiest access to the possession of competence and social rank, we find large bodies of men who have no professional engagements whatever to divert them from their literary and scientific labours, which are thus made to constitute the business of their lives. I am fully sensible of the great advantages which other countries possess in these respects above our own, and that it is quite impossible for us to command an equal concentration of attention to the advancement of particular branches of science, or to the concerns of a particular Society; still less so when it is considered that those services must with us, be afforded gratuitously, which, in other countries, are remunerated by the State, or are required as part of the duty of a salaried office:—we are not less called upon, however, on this account, to make the best and most efficient use of the means in our power, and the assistance which we cannot command as due from a sense of official or professional obligation, we may receive as rendered from a higher feeling of devotion to the promotion of the general interests of science, and with it of our national fame.

However much I may lament the want of establishments, in this country, for the exclusive and liberal support of men of learning and of science, and however anxiously I may look forward to the time when our Government and Legislature may take this subject into their most serious consideration, with a view to the remedy of so great an evil, yet I rejoice to observe amongst all ranks of society so zealous and so ardent a feeling in favour of the cultivation of every branch of science, of art, and of literature; so general and so deep an anxiety, in fact, that our country should advance in the front rank in the rapid march which European nations are making in knowledge and improvement.

It would be very easy for me to produce evidence of the existence of this spirit in the foundation of literary and other Societies in so many of our provincial towns, and in the active and general support which they receive; but it is sufficient for my purpose to appeal, for the complete confirmation of the truth of the opinion which I have expressed, to the noble manner in which the British Association has been supported by the eager concurrence of the friends of science from all quarters of the kingdom: and the splendid reception which has been recently given to this Association by the University of Oxford; the judicious and well merited honours conferred upon four of its most illustrious Members*; the eager attention which was given to its proceedings by crowds of intelligent and admiring auditors, the great variety and excellence of the Reports which were there produced upon the present state and recent history of various branches of philosophy, will constitute a proud epoch in the scientific history of this country, and one which is full

* Brewster, Brown, Dalton and Faraday, on whom the degree of LL.D. was severally conferred.

of promise with respect to the future state and fortunes both of science and its cultivators.

It becomes my duty now to advert to the heavy and severe losses which the Society has sustained during the last year, including, I regret to say, many celebrated names, more particularly in our foreign list. I shall begin, however, with the mention of those names upon our home list, whose labours in the cause of literature or of science, appear to entitle them to particular notice.

Sir Everard Home, Bart., was born at Hull on the 6th of May 1756. He was the youngest son of Robert Home, a surgeon in the army, and descended from the Barons of Polwarth, the ancestors of the Earls of Marchmont in Scotland; he was educated at Westminster School, and though elected off as a scholar to Trinity College, Cambridge, in 1773, he never went there, having abandoned his prospects in college upon the invitation of the celebrated John Hunter, who had recently married his eldest sister, and who offered to superintend his education in surgery and human and comparative anatomy, and gave him the free use of his unrivalled collections. Under his auspices he continued to study for several years, availing himself at the same time of the lectures and instructions of the most eminent anatomical and medical teachers of his day. He went to the West Indies upon the medical staff in 1780, where he remained for four years; upon his return to England in 1784, he continued to assist Mr. Hunter in the arrangement and completion of his museum, and also in his various official duties until his death, which took place in 1793. Mr. Home was elected a Fellow of this Society in 1785; in 1808 he was made sergeant-surgeon to the King, and in the same year he received the Copley Medal for his various papers on Anatomy and Physiology, printed in the Philosophical Transactions. In 1812 he was created a baronet, being the first surgeon in actual practice upon whom that honour had been conferred.

In 1821 he was appointed surgeon to Chelsea Hospital, and in the following year he was elected President of the College of Surgeons. In the year 1827 he began to retire from the practice of his profession, and from most of his official employments; and he died at his residence in Chelsea College in August last, in the 77th year of his age.

Sir Everard Home was the author of 107 papers in the Transactions of this Society, a number exceeding that of any other contributor. He published Lectures upon Comparative Anatomy, in six volumes quarto; the two first in 1814, the third and fourth in 1823, and the two last in 1828. They consist chiefly of the results of his papers in the Transactions of this Society, with a republication of the splendid plates, by the permission of the Society, by which many of them were illustrated. He was also the author of several other works upon different subjects of anatomy and surgery; and he published in 1797, Memoirs of John Hunter, who had bequeathed to him all his papers.

Sir Everard Home must be considered as the successor of John Hunter, and in every way most closely connected with him. He aided greatly in the formation of his noble collection; he was a witness of, and a sharer in, his most important investigations; he was also the depository of his literary treasures; and if we regard either the number or the nature of his anatomical or physiological researches, and the importance of his discoveries, we must be compelled to declare that he followed closely and worthily in the footsteps of his illustrious predecessor: but though he was a most diligent observer and collector of facts, and fully qualified, by his extensive knowledge of anatomy and physiology, to collate them with existing materials of those sciences, and to reduce them, as he has done in his lectures, to a regular and well connected system, yet we should be unjust to the memory of that great man who was his instructor and patron, if we ventured to place him in the same rank with him. But what name in modern times, if that of Cuvier be excepted, can be put in competition with that of John Hunter, for careful and philosophical induction, and for the power of concentrating facts derived from most extensive observations upon every part of the animal kingdom, in illustration and confirmation of his physiological theories? It would be unfair to the memory of Sir Everard Home to subject his merits and his fame to be tried by so severe a test; rather let us ask, when the vast range of his knowledge and investigations is considered, who were his rivals or his superiors among his contemporaries, or amongst his survivors?*

Sir James Hall, Bart., the author of several important papers in the Edinburgh Transactions, in illustration and in defence of the Huttonian Theory, and of a very ingenious and speculative book on the Origin of Gothic Architecture, is another considerable name, whose loss we have to deplore.

In considering the present state of geological science, we are too apt to forget the fluctuations of opinions and of theories through which we have passed in order to arrive at our present state of comparative repose. It is little more than twenty years since the partisans of Hutton and of Werner divided between them the geological world, and we rarely hear their names now pronounced; not that their names have passed into oblivion, but that their theories and their speculations have become a portion of the history of the science, and no longer form a part of the debateable materials of which it was, or was not, to be constructed. Sir James Hall, in con-

* The following papers by Sir E. Home, reprinted from the Philosophical Transactions, and other works in which they were originally published, will be found in the Philosophical Magazine, First Series: "On the mode of hearing when the Membrana Tympani has been destroyed," vol. viii. p. 365; "On Pithing Cattle," vol. xxi. p. 72; "On two Children born with Cataracts," vol. xxviii. p. 203; "On Calculi," vol. xxxii. p. 239; "On Animal Secretions," vol. xxxv. p. 108; "On the Bite of a Rattlesnake," vol. xxxvi. p. 209; "Experiments to prove that Fluids pass directly from the Stomach to the Circulation," vol. xxxviii. p. 37; "On the progressive motion of Snakes," vol. xl. p. 241; "On the formation of Fat in the Intestines," vol. xliii. p. 35; "On the beneficial effects of Colchicum autumnale, &c." vol. xlviii. pp. 337, 340; and "On the black Rete Mucosum of the Negro," vol. lviii. p. 31.—Edit.

junction with his friend Professor Playfair, was, in the early part of the present century, an ardent vindicator of the opinions of Dr. Hutton; and it was with a view to the removal of some of the more popular and startling objections to his theory, that he undertook, and continued during several years, those memorable experiments upon the effects of compression in modifying the action of heat, which have contributed so greatly to the termination of the controversies which were then agitated with so much warmth and severity. These experiments, most happily conceived, and executed with singular boldness and perseverance, completely proved that the most refractory substances may be made fusible by confining the elasticity of the gaseous parts contained in them. Thus, pounded carbonate of lime or chalk could be rendered fusible, without calcination, and became, upon cooling, a compact stony mass, and even crystalline, like marble; it thus appeared that the effect of heat, acting under enormous pressures, would not necessarily dissipate the gaseous and evaporable parts of the strata of the earth, but would leave them to form such new combinations or modifications of existence as might be determined by the laws of crystallization or of chemical affinities;—a most important fact, and one apparently so difficult to establish in a form which might bring into action those gigantic forces which present themselves in the great operations of nature, as would have checked the attempts of any man who was not urged onward by the most determined enthusiasm in the defence of a favourite theory.*

Sir James Hall's work on the Origin of Gothic Architecture cannot be considered as a serious archæological inquiry, but rather as an agreeable exercise of his fancy. The development however of his theory is singularly ingenious and elegant; it proves him to have possessed no mean talents as an artist, and shows a mind alive to all those beautiful combinations of nature which seem to be rendered fixed and permanent in the naves of our Gothic cathedrals, and in the tracery of our decorated windows.

Sir James Mackintosh was born in Morayshire in Scotland, in 1765; he was the son of an officer, of good family, but of very limited fortune; his first destination was for the profession of medicine, and with this view he took the degree of M.D. at Edinburgh, in 1787. Upon his removal, however, to London, shortly afterwards, he abandoned his medical prospects, and gave himself up entirely to the study of the law, and of moral and political philosophy. In 1789 he went to Leyden, where he studied for some time, and afterwards to Liege, where he was a witness of the memorable struggle between the Prince Bishop and his subjects, as well as of many other ebullitions of popular feelings which preceded and foreboded the French Revolution. It was, probably, the contemplation of scenes like these, as well as the observation of the corruptions and abuses

* Sir James Hall's "Account of a Series of Experiments, showing the Effects of Compression in modifying the Action of Heat," and his "Catalogue of Specimens," showing the results of those experiments, will be found in Phil. Mag. vols. xxiv. and xxv.—Edit.

of many of the continental governments of Europe, which made him, like many other ardent young men of that period, an admirer of the principles of that great national movement; and the *Vindiciæ Gallicæ*, a work of great force and eloquence, was the most powerful answer which appeared in that age to Mr. Burke's celebrated Reflections, and gained for him, at once, both at home and abroad, a distinguished reputation. The atrocities, however, which marked the more advanced stages of the French Revolution, his own increasing experience and knowledge of mankind, and still more his frequent intercourse with his illustrious adversary, for whose genius he had always professed a chivalrous admiration, however much he had opposed his views and his reasonings, combined to sober down the fervent enthusiasm of his own youthful speculations and hopes; and the principles which he avowed and vindicated in his celebrated defence of Peltier in 1802, must be considered as those which he adopted as the result of the convictions of his maturer age, and which he continued to maintain through life. In 1803 he was appointed Recorder of Bombay, where he resided for seven years, and where he secured the affection and admiration both of natives and of foreigners, by the able, impartial, and considerate discharge of his judicial functions. Upon his return from India in 1811, he was elected Member of Parliament for Knaresborough, a place which he continued to represent for the remainder of his life.

Few persons of his own age had read so much as Sir James Mackintosh, or remembered what they had read so well. His conversation was singularly instructive and brilliant, without being overbearing; his manners were conciliating; his temper excellent; and he was entirely tolerant of opinions which were different from his own. He was one of the most distinguished Members of the House of Commons; and his speeches upon all the great questions which were agitated in his time were remarkable, not merely for their eloquence, but the large and comprehensive views of national policy, which were supplied by his almost unrivalled knowledge of history and political philosophy.

Sir James Mackintosh, besides his *Vindiciæ Gallicæ*, was the author of Lectures upon the Laws of Nations; of A Sketch of the History of England; of an incomplete Essay on the Principles and the History of Moral Philosophy; and of many admirable Reviews. It is to be lamented that he should have dissipated his extraordinary powers upon occasional and desultory publications, instead of concentrating them upon some great work which might have transmitted, undiminished, to posterity the reputation which he enjoyed among his friends and cotemporaries. There were, however, many circumstances which might sufficiently account for his failing to leave behind him a monument for future ages, which would have been worthy of his genius and his learning. He brought home with him from India a shattered constitution, which disqualified him for continued and laborious exertion; he had many Parliamentary as well as official duties to perform; and the pressure of his pecuniary necessities compelled him to seek, too frequently, for the immediate remuneration, which was supplied by means of contributions to the perishable periodical literature of the day.

Colonel Mark Wilks went to Bengal in 1783, and served in different military and civil capacities, in various parts of India. In the year 1804 he was appointed principal Resident at the Court of Mysore, and in the following year he published a very able Report upon the financial condition, resources, and many other subjects connected with the administration of the government of that country. He was the author of "Historical Sketches of the South of India, in an attempt to trace the History of Mysore to the Extinction of the Mohammedan Dynasty in 1799,"—a work of great learning and authority: he was afterwards appointed Governor of St. Helena, and he died in England in the course of the present year.

Colonel Wilks must be considered as one of those distinguished men who have been formed by the system of our Indian Empire. The possession of great commands, upon which the happiness and misery of considerable nations are dependent, and the intense feeling of responsibility which is connected with the administration of trusts so important, is well calculated, under all circumstances, to call forth into action the highest powers of the human mind; and particularly so, when they have been previously exercised and fortified, as in our Indian service, by the severe study of Oriental languages, and by the successive occupation of different offices, with a great diversity of duties: it is to such causes that we are to attribute the frequent union which we observe in this service of the greatest civil and military talents with the most profound acquisitions in Oriental learning; it is to this system that we are indebted for the production of a Duncan and a Monro, an Elphinstone and a Raffles, a Colebrooke and a Malcolm, and a crowd of great men who have done so much honour to our Indian Government.

Alexander Barry, Professor of Chemistry to Guy's Hospital, and the author of a short paper in our Transactions for 1831, "On the Chemical Action of Atmospheric Electricity*," fell a victim to the imprudent pursuit of his chemical inquiries. He was making experiments upon some gases in a highly condensed state, when an explosion took place, by the effects of which he was so much injured as to occasion his death shortly afterwards. He was elected a Fellow of this Society in the course of the last year.

John Shaw, Architect, is advantageously known to the public by several works in the Metropolis, particularly the great hall in Christ's Hospital, and the new church of St. Dunstan in Fleet Street: works which are extremely effective, and well adapted to their objects and positions.

Stephen Groombridge, Esq., was the author of two papers in our Transactions for 1810 and 1814, of considerable interest and value, upon the subject of astronomical refractions†: his observations were

* An abstract of Mr. Barry's paper appeared in the Philosophical Magazine and Annals, N.S. vol. ix. p. 357.—Edit.

† Abstracts of Mr. Groombridge's papers were given in Phil. Mag. vol. xxxv. p. 303, and vol. xliii. p. 306.—Edit.

made at his house at Blackheath, with a four-feet transit circle, which has acquired no small degree of celebrity from its being the first instrument, after the Westbury Circle, to which Mr. Troughton applied his method of division, which he has described in our Transactions. Mr. Groombridge made many thousand observations, which have been reduced by order, and published at the expense of, Government,—a circumstance well deserving to be known by all astronomers, as he was an able and faithful observer, and possessed more advantages for making meridian observations, than are commonly enjoyed without the walls of a regular observatory.

Sir Richard Hussey Bickerton was a very distinguished naval officer, who was employed in the service of his country for the greatest part of his life, and who was for some time second in command to Lord Nelson in the Mediterranean and elsewhere, and enjoyed his entire confidence and esteem. He was one of the Lords of the Admiralty from 1805 to 1812, a circumstance which brought him into frequent communication with the Royal Society, and led to his election as a Fellow in 1810.

In our list of Foreign Members, we have to record the deaths of Cuvier and of Chaptal in France, of the Baron de Zach in Germany, and very lately likewise those of Oriani and of Scarpa in Italy; five celebrated names, which have long been intimately associated with the progress of science. The limits of this address must confine me to a very brief and imperfect notice of their merits and their labours.

The Baron Cuvier, the most illustrious naturalist of modern times, was born at Montbelliard in Alsace, in 1769, and died on the 13th of May last, in the 63rd year of his age: it is not necessary for me to detail any of the circumstances of the life of one whose name has been long known and reverenced in every region of the globe which has enjoyed the blessings of European civilization; suffice it to say, that he was honoured and even courted by every Government in France from the period of the Convention to the present day; that he held the most lucrative and distinguished appointments which the wise policy of that great nation has provided for the honourable support of its men of science and literature; that after the death of Laplace he was universally regarded by his countrymen as the most illustrious of their men of science, and as one of the most distinguished of their men of literature; that funeral orations were pronounced over his grave by men of all political parties, however much opposed to him during his life; and mathematicians and naturalists, geologists, historians and poets, all felt themselves impelled to pay this last tribute of homage to the genius of one, who in so many capacities had done so much honour to his country.

M. Cuvier was in every respect a most extraordinary man: his very presence was calculated to command respect, his countenance bearing that impress of a powerful intellect, which all men recognise when seen, however difficult it may be to define its character: his manners were dignified and polished, and his conversation possessed that happy ease and subdued gaiety which characterized the best age of French society. He was well acquainted with ancient literature, and familiar with the principal languages of modern Europe. His memory was singularly accurate and retentive; and his knowledge of facts, not merely in those sciences which he especially cultivated, but likewise in all other departments of knowledge, and particularly history, was a subject of surprise and admiration to all who knew him. He was also eminently distinguished as a writer of his own language, and his numerous *éloges* delivered in his capacity of *secretaire perpetuel* to the Institut, of which three volumes have been published, if considered as specimens of composition merely, have equalled, if not surpassed, the best examples of a species of eloquence of which the French nation has just reason to be proud; but if they be considered as specimens of correct and precise discrimination of the merits of the persons commemorated, as determined by their writings and discoveries, and by the influence which they have exercised upon the progress of knowledge, they may justly be pronounced to be unrivalled. It was to this publication that he was indebted for his place amongst the *forty* of the *Academie Française*, an honour which he alone, in his own age, enjoyed in conjunction with his place in the *Academie des Sciences*.

It is, however, chiefly as a naturalist that Cuvier must be viewed, when we seek to determine his permanent rank amongst the few great men who have effected great revolutions in the sciences which they have cultivated, or have left ineffaceable traces of the influence of their discoveries behind them. The whole animal kingdom, from the most obscure indications of the separation between inanimate and animate existence to the mighty monsters of a former world, has assumed under his hands a systematic arrangement, not founded upon superficial and unimportant external characters merely, but upon a most careful and laborious observation of the analogies of internal structure. By tracing every organ successively through the whole series of animals; by carefully determining the functions of such organs and their relations to each other; and by considering them in every animal in the first place as an individual, and in the second place with reference to others, he has been enabled to distribute them into species and genera, and families and classes, where every successive step in their arrangement is the result of a legitimate and inductive generalization. It is by such means that he has been enabled to convert the science of natural history, at least in the animal kingdom, from being little more than a systematic classification, formed for the purpose of identifying genera and species and with no higher view, into a science of strict and severe induction, founded upon a careful observation and comparison of every fact which anatomical and physiological science can detect, and thus to confer upon it a dignity which is only inferior to that of the physical sciences.

It has resulted also, from his researches, that every animal considered as one of the same genus or species, is not only an individual considered as a whole, but also when considered in all its parts; in other words, that every bone, every muscle, every organ, and every part of its structure is *essentially* distinguished from the corresponding parts of an individual of any other genus or species. To a perfect naturalist, therefore, the inspection of a bone, or any other part of an animal, would bring to his mind the entire animal itself, and would identify it as perfectly as if it was exhibited entire to his eye: this would be a triumph of science to which our limited knowledge and faculties can never completely attain; but it was to this point that Cuvier approximated, when he reconstructed as it were the fossil animals of an antediluvian world from the imperfect fragments which remained of them; when he showed in what such animals must have differed, and in what they must have agreed, whether in magnitude or in kind, from the animals which exist at present; when he ventured in fact to define their habits, and to write as it were the natural history of a former world, by throwing upon its obscure and half-obliterated records, the powerful light of science and philosophy. The *Histoire des Ossemens fossiles* must ever remain a classical work to geologists; and the discoveries which it contains, and those to which it has led in the hands of others, are some of the most interesting and extraordinary with respect to the past ages of the world, which observations upon the surface of the globe have ever enabled us to ascertain.

The last great work upon which he was engaged was the *Histoire Naturelle des Poissons*, a prodigious undertaking, of which eight volumes have been published, and which he expected to extend to twenty-five; it was undertaken in conjunction with Messrs. Valenciennes and Laurillard, to whom also he has bequeathed the task of completing it. It will contain the description of 6000 species of fish, 4000 of which had not been noticed in any other work.*

Jean Antoine Chaptal, Compte de Chanteloup, was born in 1756, and died in April last in the 76th year of his age. He was Professor of Chemistry at Montpelier before the Revolution, and was one of the most active cultivators of chemical science before that event, in conjunction with Monge, Fourcroy, Berthollet, Guyton de Morveau, and the illustrious Lavoisier. In the year 1793, upon the threatened invasion of France by the Allies, when saltpetre was not to be procured in sufficient quantities for the manufacture of the powder wanted by the French armies, he was invited by the Com-

* A translation of the "Memoir on the Mineralogical Geography of the Environs of Paris," by Cuvier and Brongniart, which is an abstract of the work on that subject prefixed to the *Hist. des Oss. foss.* (see our last Number, p. 52,) appeared in Phil. Mag. vol. xxxv. p. 36; and translations of the following memoirs, also forming part of Cuvier's great work, were given in that and other volumes; viz. "On the Fossil Bones of Horses and Wild Boars," Ib. p. 215; "On the Osteology of the one-horned Rhinoceros," vol. xix. p. 350; "On the Osteology of Living and Fossil Elephants," vols. xxvi. and xxx.; and "On the Osteology of Reptiles," vol. lxv. p. 447. A Report, by Haüy, Lelievre and Cuvier, on M. André's "Theory of the actual Surface of the Earth," was given in vol. xxxiii. p. 170; Cuvier's "Report on Delaroche's Memoir on the Air-bladder of Fishes," in vol. xxxv. p. 291; and his Report "On a paper by M. Flourens on the Nervous System," in vol. lxi. p. 114.—Edit.

mittee of Public Safety to superintend the establishments for that purpose; and his chemical knowledge so greatly improved the method followed in its manufacture, as in a very short time to make the produce greatly exceed the demand. He was made *Ministre de l'Interieur* by Napoleon, and continued under the Empire to fill many important situations. He was the author of considerable works on chemistry, on the application of chemistry to the arts, on the application of chemistry to agriculture, on the art of making wines, and on the art of dyeing cotton and wool, which are written in a very perspicuous and elegant style, and which have enjoyed a very considerable popularity in France. The labours of his whole life, in fact, were devoted to the improvement of those manufactures whose perfection depended more or less upon the most correct and economical application of chemical principles; and, after his distinguished countryman Berthollet, he must be placed in the first rank of those who have benefited the arts through the medium of chemical science.

François Xavier Baron de Zach was born at Pesth, in Hungary, in 1754. His taste for astronomy was decided at the early age of fifteen, by the interest which he took in the observation of the comet of 1769, and by the transit of Venus over the disc of the sun in the same year, a memorable event which served to make more than one important convert to the science of astronomy. After travelling with scientific views through different countries of Europe, and residing for several years in England, where he acquired for our manners and institutions an attachment which continued throughout his life, he settled at Gotha, in 1786, in the family of the Duke of Saxe Gotha, who charged him with the construction of the Observatory at Seeberg, over which he continued to preside for a considerable period. He published at Gotha, in 1792, Tables of the Sun, with a Catalogue of 381 Stars, and he subsequently published many other important astronomical Tables, particularly those on Aberration and Nutation. He became in 1800 the editor of the "Monatliche Correspondentz," a German periodical work on astronomy and geography, which was subsequently published in French under the title of "Correspondence Astronomique &c.," upon his removal to the South of France in 1813, and subsequently to Genoa in company with the Duchesse de Saxe Gotha. This was a most valuable Journal, containing records of the progress of astronomy in every country in Europe, and contributing more than any other publication to the great impulse which has been given for many years to the cultivation of astronomical science in Germany*. In 1814 he published his very interesting work on the "Attraction of Mountains." For many of the later years of his life he suffered severely from the stone, and he had established himself at Paris for the purpose of being con-

* In the Phil. Mag. vol. xvi. p. 49, will be found a paper by Baron de Zach, "On the Planet Pallas," from the *Monatliche Correspondentz*; and in vol. lxi. p. 353, a paper "On Repeating Circles," by the same astronomer, from his *Correspondence Astronomique*.—Edit.

stantly under the care of Dr. Civiale and experiencing relief by the operation of lithotrity, where he died from a sudden attack of cholera in September last. The Baron de Zach was a most zealous friend to astronomy, and throughout his long life contributed to its progress by his numerous publications, and by maintaining a most extensive and laborious correspondence with the principal astronomers in Europe. He was a man of warm and ardent affections, rapid and sometimes hasty in his conclusions, of the most lively and agreeable manners, and of the most indefatigable industry: and there are few persons of the present day whose loss will be more sensibly felt by the friends of astronomical science in every country in Europe.

Barnaba Oriani, Director of the Observatory of the Brera at Milan, where he has resided for fifty-five years as assistant and principal, was born at Garegnano near Milan, in 1753. He was the principal conductor of the measurement of an arc of the meridian in Italy, and of the great trigonometrical survey of Lombardy, which took place between the years 1786 and 1790; and throughout the course of a long life, he devoted himself to the cultivation of physical and practical astronomy. He was the first person who calculated the orbit of the planet Ceres after its discovery by Piazzi at Palermo. He published theories of the planets Uranus and Mercury, with Tables of their motions. He laboured with singular skill and perseverance in the improvement of the lunar Tables both by theory and observation. He was the author of an admirable treatise on spheroidical trigonometry: and the Astronomical Ephemeris of Milan was published for many years under his directions, by Carlini. Upon the whole, if the union of practical with theoretical science be considered, we shall be justified in pronouncing him to have been, after Bessel, the most accomplished astronomer of the present age.

Antonio Scarpa, one of the eight Foreign Members of the *Academie des Sciences* of Paris, and probably the most profound anatomist of the present age, was born in the year 1746, and died in October last in his eighty-seventh year. He was made Professor of Anatomy at Pavia in the twenty-second year of his age, and for the last half-century he has been placed by the common consent of his countrymen at the head of their anatomists and surgeons. He was the author of magnificent and classical works on "The Organs of Hearing and Smell," "On the Nerves," "On the principal Diseases of the Eye," "On Aneurism," "On Hernia," with Memoirs on many other subjects of physiology and practical surgery. He had accumulated a handsome fortune by the practice of his profession, and had collected in his palace at Pavia a considerable number of works of art, where he lived for the latter years of his life, surrounded by his pupils, reverenced by his countrymen, and in the enjoyment and contemplation of that brilliant reputation, the full development of which a great man can rarely live to witness.

In thus directing your attention, Gentlemen, to those distinguished Members of the Royal Society, who, unhappily for the interests of science, have been taken from us during the last year, there is one name remaining which I cannot notice without feelings of the most painful embarrassment. To what class shall I, or can I refer it; to the living or to the dead? Though my fears tend too strongly to make me decide upon the choice of the latter, yet I would fain indulge in the hope which is still afforded by the uncertainty, mournful though it be, which hangs over the fate of the gallant and adventurous Captain Ross. The object of his voyage, as is well known to you, was the solution of a nautical problem of the greatest interest and difficulty,—the discovery of a north-west passage. It is a problem which more than any other excited and baffled the adventurous spirit of our most daring seamen of the age of Elizabeth; and when subsequently resumed, chiefly upon the authority of the ingenious speculations of Daines Barrington, a distinguished Member of this Society, and of others of later date, the first attempt of Captain Ross himself and the memorable voyage of Parry, as well as the journey of Franklin, have shown how visionary were all hopes of its successful solution for the purposes of commerce, however interesting it might be for those of science. It was the failure of the first voyage of Captain Ross, and the apparent censure which he conceived rested upon him, in consequence of the greater success of the attempt of his immediate successor in this enterprise, which oppressed his high and manly spirit, and made him seek, with the greatest possible earnestness, for an opportunity of vindicating his professional character. With the assistance of some of his friends he planned another voyage, and nearly three years ago he proceeded to put it into execution. It is to dispel the mystery attendant upon that voyage, of which no tidings have been yet received, and to relieve the misery under which the friends and relations of Captain Ross and his gallant crew are lingering, that a vessel is now preparing, under the command of an able and experienced officer, to pursue the track which he probably followed. I have consented, at the request of the Royal Geographical Society, to be placed at the head of the Committee which has been formed for the aid and furtherance of this benevolent plan, and I confidently hope that the funds which are necessary to complete this undertaking will not be found wanting.

The name of nearly every distinguished foreigner who has been lost to science during the last year has appeared upon the Foreign List of the Royal Society, and I cannot help considering it as a circumstance which does honour to the Royal Society, that it should thus have associated with it whoever is most eminent in the great aristocracy of European science. It is my wish, Gentlemen, and I feel assured that it is yours also, that the Royal Society should embrace the name of every distinguished man of science in the British dominions. At the last Anniversary it was my pleasing duty to present the Copley Medal to Professor Airy,—a name which would do honour to any Society, but which does not yet appear in the list of our Members: and I lament that I am not allowed to commemorate the name of that very distinguished philosopher, Sir John Leslie, upon this occasion in the obituary of the Royal Society. I look forward with hope, Gentlemen, to the time when the Royal Society shall be so circumstanced as to be free from such a reproach, or rather from such a misfortune.

GEOLOGICAL SOCIETY.

Nov. 7, 1832.—The Society assembled this evening for the session.

A paper was first read, "On some Intersections of Mineral Veins in Cornwall," in a letter to Davies Gilbert, Esq. M.P. F.G.S. F.R.S. &c., by William John Henwood, Esq. F.G.S.

The chief object of this communication is to lay before the reader particular facts bearing upon certain theories respecting the dislocation of veins; and the author in pursuance of his intention first states the theory as a question, and then adduces his facts. The following are the principal propositions.

1. When one vein is dislocated by another, is it to be found on the side of the smaller or larger angle?

At Bulls, in the Herland mine, two veins are heaved by a cross-course; and one of them was rediscovered on the side of the smaller angle, and the other on the side of the larger angle.

2. When one vein dislocates several others, are all the latter to be found on the same hand?

In the Weeth mine, two cross-courses are traversed by the same E. and W. lode. One of them is heaved to the left, the other to the right, but both to the side of the larger angle.

3. When the same vein is dislocated by several others, do they all heave it to the side of the greater, or all to the side of the smaller angle?

In Huel Friendship mine, a lode has been heaved by three cross-courses, in each instance to the left; but in two, on the side of the smaller, and one on the side of the greater angle.

4. When a vein is dislocated by several others, do they all heave it to the right, or all to the left; or some one way, some the other?

In Carharrack mine the lode is heaved by two cross-courses, and by both of them to the side of the larger angle; but in one instance to the left, and in the other to the right.

5. When a vein is thrown or dislocated by a slide, is it to be rediscovered on the side of the greater or smaller angle?

In South Huel Towan mine, the vein was rediscovered on the side of the smaller angle, but at Bulls on that of the larger angle.

6. When the same lode is dislocated by various slides, do all the latter throw it to the side of the greater angle, or all towards that of the smaller; or some to one, and some to the other?

In Huel Peever mine are two lodes and two slides; both the lodes are thrown down by one of the slides, and towards the greater angle; but one of the lodes (*a*) on coming in contact with the other lode (*b*) is thrown upwards or towards the smaller angle, and the same lode (*a*) on meeting the slide (*d*) is again thrown upwards or to the smaller angle.

7. When various veins are thrown by the same slide, does it throw

them all upwards or all downwards, or some upward and some downward?

In Huel Trevaunance mine some of the veins on coming in contact with others are thrown down; but one of the veins (*a*) on coming in contact with the vein (*f*) is thrown upwards; and the vein (*b*) on coming in contact with the vein (*f*) is also thrown upwards, as is the vein (*f*) on coming in contact with the vein (*e*).

The author having thus "compared the general rules, which are so frequently discussed, with facts," says, "Notwithstanding there are numerous exceptions, it may be assumed that where a cross vein is found to have heaved two or three lodes towards one hand, the miner will not often be very far wrong if he excavate in the same direction to recover a dislocated portion of a fourth;" and the author further observes, that he shall not be surprised if a different rule be found to prevail in the districts where tin abounds, from that which obtains in a copper country. He next proceeds to combat the received opinion, that all interruptions or intersections in mineral veins are the effects of disturbances, and that the order of intersection of the various veins is the index of their relative age. He then enters upon the inquiry whether the phænomena of intersections and dislocations are explicable on the assumption of motion, the principal results of which inquiry are given in the abstract of the paper which is contained in the printed "Proceedings" of the Society.

A Notice of a Submarine Forest in Cardigan Bay, by the Rev. James Yates, M.A. F.G.S. and L.S. was afterwards read.

This forest extends along the coast of Merionethshire and Cardiganshire, being divided into two parts by the estuary of the river Dovey, which separates those counties. It is bounded on the land side by a sandy beach and a wall of shingles. Beyond this wall is a tract of bog and marsh formed by streams of water which are partially discharged by oozing through the sand and shingles. The author argues that, as the position of the wall is liable to change, it may have inclosed the part which is now submarine, and that it is not necessary to suppose a subsidence effected by subterranean agency.

The remains of the forest are covered by a bed of peat, and are distinguished by an abundance of *Pholas candida* and *Teredo navalis.*

Among the trees of which the forest consisted is the *Pinus sylvestris*, or Scotch fir; and it is shown that this tree abounded anciently in several northern counties of England. The natural order of *Coniferæ* may thus be traced from the period of the independent coal formation to the middle of the seventeenth century, although the Scotch fir is now excluded from the native Flora.

The amentaceous wood presents matter for reflection, in consequence of the perfect preservation of its vascular structure, while the contents of the vessels are entirely dissipated.

This tract is known to the Welsh under the name of *Cantrev Gwaelod,* i. e. the Lowland Hundred. The author refers to the Triads of Britain, and to other ancient Welsh testimonies, which prove that it was submerged about A. D. 520, and ascribe the disaster to the folly of "Seitheryn, the Drunkard, who in his drink let the sea over the Cantrev Gwaelod."

A paper entitled "Notices on the Geology of the North-west of the Counties of Mayo and Sligo," by the Venerable Archdeacon Verschoyle, and communicated by Roderick Impey Murchison, Esq. P.G.S. was also begun.

Nov. 21.—The reading of Archdeacon Verschoyle's paper, begun at the meeting held on the 7th of November, was concluded.

The author divides his memoir into two parts; in the first he gives a topographical description of the country, and in the second a detailed account of the different formations of which it is composed.

I. The district described is situated in the western part of the province of Connaught, and is bounded on the N. and W. by the Atlantic. Through the eastern portion a primary chain, called the Ox mountains, having a mean height of 1300 feet, extends in a N.E. and S.W. direction. The north side of the chain rises at a considerable angle, and terminates in a series of abrupt, rocky peaks; but the plane which forms the southern declivity is much more gradual in its inclination. The principal passes are at Colloony, Lough Talt, and Foxford. The formations of which the mountains consist are mica-slate, hornblende-slate, and quartz-rock. Their bases are covered by a conglomerate which the author considers to be the representative of the old red sandstone; and on it reposes alternating strata of sandstone, and shale, succeeded by carboniferous limestone. On the south of the chain the limestone stretches towards Roscommon and Galway, joining the great limestone field of Ireland; and on the N.W. it forms a plain, extending from Sligo to the barony of Erris, where the Nephin group rises from beneath it, being the commencement of the primary tract reaching northward and westward to the ocean. Immense ridges of water-worn pebbles occur in every portion of the district. The coast presents for the greater part bold, abrupt precipices, formed of gneiss, mica-slate, quartz-rock, and mountain limestone; but in some places it is low, and composed of a succession of sand-hills.

II. In describing the formations comprising the district, the author arranges them in the following descending order,—Carboniferous limestone with beds of oolite, calcareous shale and grit, old red sandstone or conglomerate, quartz-rock, gneiss, mica-slate, hornblende-slate, granite, trap-rocks, porphyry, and basalt. An abstract of his account of these formations is given in the Proceedings of the Society.

A communication was then read from the Rev. Adam Sedgwick, V.P.G.S. and Woodwardian Professor in the University of Cambridge, respecting certain fossil shells overlying the London clay in the Isle of Sheppey.

Mr. Sedgwick, in examining a series of fossils from the Isle of Sheppey, lately presented to him, found several specimens differing from the rest, both in their specific characters and state of preservation. These shells were derived by Mr. Crow of Christ College, Cambridge, from a bed in Warder Cliff, about 15 feet below the surface of the ground, and had lately been laid bare by a small land-slip. The bed in question is from 8 to 12 inches thick, and the part exposed is not more than 20 feet in length, though there can be little doubt that it extends considerably further. It rests almost immediately on the clays, containing the well-known suite of pyritous fossils with which the Isle so much abounds, and its level above the beach is stated to be about 140 feet. The specimens belong to the well-known English shells,—*Ostrea edulis, Cardium edule, Buccinum undatum, Fusus antiquus,* and *Turbo littoreus.*

Dec. 5.—A paper was read, entitled "Observations on the Remains of the Iguanodon, and other fossil Reptiles, of the Strata of Tilgate Forest in Sussex," by Gideon Mantell, Esq., F.G.S. R.S. and L.S.

The author, having noticed the various memoirs and works which have appeared on the organic remains of the fossil reptiles of Sussex, proceeded to give a summary account of all that was known upon the subject, and to add descriptions of the various interesting fossils which subsequent discoveries had brought to light. He observed that the strata of Sussex, with the exception of diluvial and tertiary deposits, were referrible to two series of formations only,—one, marine, including the chalk and green-sands; the other, freshwater, the Wealden: the former, containing fishes, zoophytes, and marine shells; the latter, herbivorous saurians, turtles, terrestrial plants, and fresh-water shells. He then described the teeth and other bones of the Crocodile, Megalosaurus, Plesiosaurus, Iguanodon, and *Phytosaurus cylindricodon.* The head, jaws, and teeth of the last animal were stated to have been found in the Keuper of Germany, and the teeth in the Tilgate beds of Sussex. On the Iguanodon the author offered many new anatomical details: he particularly noticed,—an ungual bone, a clavicle of a most extraordinary form, and the thigh- and both leg-bones of the same limb, which exhibited enormous dimensions. He then gave a statement of the results of a careful comparison of six different portions of the skeleton of the recent Iguana and the Iguanodon, and stated that from this investigation it appeared the length of the animal was 70 feet, the tail forming about two thirds of the whole. A new fossil reptile was then described, of which a considerable portion of the skeleton of the trunk had been lately discovered. The block of stone in which the bones were imbedded was 4½ feet by 2½ feet. It exhibited a chain of 5 cervical and 5 dorsal vertebræ, with corresponding ribs; and four other vertebræ detached from the column and lying on other parts of the stone. The coracoids and omoplates of both sides were visible, and exhibited a structure so peculiar as to warrant the separation of this new reptile from all recent and fossil genera. With the coracoids of a Lizard, it had the omoplates of a Crocodile. A still more extraordinary peculiarity of osteological structure was exhibited in a series of spinous bony apophyses, which, varying in size from 3 to 17 inches in length, and from 1½ to 7 in width at the base, maintained a certain parallelism with the vertebral column, as if they had been placed in a line along the back. This circumstance, together with other reasons, induced the author to suggest that they might be the remains of a dermal fringe, with which, as in some recent species of

Iguana, the back of the animal was armed; but at the same time he mentioned many anatomical peculiarities, which led him to hesitate in determining positively that these bones had formed such appendages. He next entered upon a careful examination of the reasons why they could not be processes of the vertebræ. Many dermal bones, which served to support the large scales, were discovered by the author in the stone. The author proposed forming a new genus for this animal, the characters of which would depend on the peculiarity of the sternal apparatus and the spinous processes; and he suggested the name of Hylæosaurus, or Forest-Lizard, to indicate its locality, the Forest of Tilgate. In the conclusion of this memoir, the author made some observations on the character of the district at the Iguanodon era. From the condition of the organic remains, which with the exception of the beds of shells, and the vegetable stems of the fossil *Equiseta Lyellii*, bore marks of transport, he contended that the river which had formed the ancient delta, the Wealden of geologists, must have had its source far distant from the beds which it had formed; and from the state of some of the specimens (and he instanced particularly that of the Hylæosaurus), he inferred, that the bones of the reptiles must have been broken and dislocated while covered with muscles and integuments, otherwise the broken parts and the displaced bones could not have maintained the relative situation in regard to each other which they are now found to maintain. He concluded with an eulogium on the late illustrious naturalist Baron Cuvier, many of whose observations, from his correspondence with the author, were introduced in various parts of the memoir.

XXIV. *Intelligence and Miscellaneous Articles.*

SINGULAR FOG-BOW SEEN ABOVE OLD MELROSE.

On the evening of Monday the 12th of November between 11^h and 12^h P.M. there was seen from Gateheugh near Bemerscale, a very luminous fog-bow, produced by the reflexion of the moon's rays from the globules of water suspended in the air, and constituting a very dense fog which extended over the whole valley of the Tweed from Old Melrose up to Abbotsford. Owing to the state of the moon, the colours of the bow could not be recognised.

The bow was extremely luminous, and every part of the arch was distinctly seen. It was thrown like an arch over the peninsula of Old Melrose, and, in one position of the observers, its two extremities appeared singularly luminous, which arose from their happening to coincide with the two branches of the river, above which the fog was more dense than above any part of the ground.

A little above the south side of the bow, the Eildon Hills raised their triple crown, entirely free from haze, and finely illuminated by the moon, like a distant island in the ocean of fog which had settled round their base.

Excepting between Berne and Thun, in an autumn morning in 1814, we have never seen a fog-bow. It was formed, however, by the rays of the sun; and in so far as we remember, a fog-bow formed by the lunar rays has never been described. D. B.

ON THE SENSATION PRODUCED UPON THE TONGUE BY MAGNETO-ELECTRICITY. BY MR. F. WATKINS.

To the Editors of the Phil. Mag. and Journ. of Science.

Gentlemen,

Among the several electrical effects exhibited by natural, artificial, steel, and electro-magnets, none are in my estimation more curious than that produced on animals. Dr. Faraday in his excellent paper on experimental researches in electricity, published in the Phil. Trans. of the past year, observes, in page 138, that when using an armed load-stone of great power, belonging to Prof. Daniell, he convulsed a frog powerfully, and adds with a diffidence so peculiarly his own, that "I thought also I could perceive the sensation upon the tongue, and the flash before the eyes." That this eminent philosopher did observe the phænomenon in a slight degree (at this moment) there can be no doubt; but as the effect was feeble, in consequence of the exciting cause not being sufficiently powerful, he alludes to it in the manner here quoted.

Through the kindness and liberality of the proprietors of the National Gallery of Practical Science, Adelaide Street, West Strand, I have seen and performed several magneto-electric experiments with great success, with their superb artificial steel magnet; and as I have not noticed in print anywhere else than in Dr. Faraday's paper, that the tongue had been electrified (if I may use the expression) by a magnet, I shall briefly recount what R. W. Fox, Esq., Mr. Saxton, and myself experienced on the 5th of June last. And should you consider the remarks worthy a space in your forthcoming Number, they are at your service.

Two slight copper wires were so disposed, that while one had an end connected with the ascending portion of the compound helical wire surrounding the armature or lifter of the large magnet, the other was joined to the descending portion, thus affording the means of completing the circuit in the mouth. When the free end of one wire was situated underneath the tongue, and the free end of the other placed above that organ, and in contact,—on breaking the connexion between the armature and magnet, a shock was felt; and when the process was repeated several times, the sensation was truly painful. Hence the original observation of Dr. Faraday was perfectly correct.

I remain, Gentlemen, your obedient humble Servant,

5 Charing Cross, 12th Jan. 1833. FRANCIS WATKINS.

OF THE POWER OF THE HOUSE SPIDER TO ESCAPE FROM AN INSULATED SITUATION. BY J. F. PHŒNIX.

Having in the course of my reading, years ago, when a boy, met with an account of the powers of the common house-spider (*Aranea* *domestica*) to escape from an insulated situation, I felt anxious to ascertain so curious a fact. I therefore proceeded by experiment to confirm or refute it.

Having procured a common basin, I placed a piece of clay at the bottom, into which I fixed a small stick, with a round card at about an inch from the top; then filling it with water up to the rim, I procured a spider; and placing him on the card, left him no chance of escape save swimming, which on my watching for some hours he declined, although exhibiting extreme activity in running up and down the mast, and frequently pressing his feet on the surface of the water. For three days his captivity endured, but on the morning of the fourth my little prisoner had escaped, by means of a web extended from the card to the outer edge of the basin.—The annexed sketch will exhibit his prison and means of escape. JAMES F. PHŒNIX.

Liverpool, Dec. 6, 1831.

SUBSTANCES CONTAINED IN OPIUM.

M. Pelletier in an elaborate memoir on opium printed in the *Annales de Chimie*, and which we propose to abridge in a future Number, mentions the following principles as contained in opium; viz. narcotine, morphia, meconic acid, meconine, narceine, caoutchouc, gum, bassorine, lignin, resin, brown acid and extractive matter, fixed oil, and a volatile but non-oleaginous principle, which rises in distillation with water.

Added to these substances M. Bebert announces (*Journal de Pharmacie*, April 1832,) another peculiar principle: it is bitter, crystallizable, forms salts with acids, especially with acetic acid, with which it gives crystals in the form of very white scales, and with sulphuric acid white silky crystals;—no name is given to this substance by its discoverer.

M. Robiquet, it also appears, has separated a new alkali from opium, which he calls *paverin*. Only a few details of its properties are as yet given (*Journ. de Pharm.* Nov. 1832.) It differs very remarkably from other vegeto-alkalies in being soluble in water. It saturates acids, is insoluble in potash, and contains much azote; it is very poisonous, and acts very particularly on the spinal marrow.

ANALYSIS OF CAMPHOR AND SOME VOLATILE OILS.

M. Dumas observes, that essential oils may be divided into—first, those which are entirely composed of carbon and hydrogen, such as the oil of lemon, turpentine, and naphtha; secondly, oxygenated oils, as camphor, oil of aniseed, and many others; thirdly, the essential oils which admit of a new element in their composition, as the oil of mustard, which contains sulphur, and that of almonds, which contains azote.

Some fine isolated crystals of camphor taken from the centre of a sublimed cake of that substance, yielded such proportions of carbonic acid and water as by M. Dumas' calculation gave per cent. very nearly

Carbon	79·28
Oxygen	10·36
Hydrogen	10·36
	100·

Neglecting on this, as on other occasions, the atomic weights adopted by M. Dumas, it will be seen that camphor is a compound of

Ten atoms carbon	$6\times10=60$	or	79
One atom oxygen	8	or	10·5
Eight atoms hydrogen	$1\times8=8$	or	10·5
	76		100·

Proust discovered the existence of camphor in oil of lavender; M. Dumas analysed some crystals of lavender camphor procured from the College de France: the results were perfectly similar to those obtained from common camphor.

M. Dumas considers camphor as composed of a peculiar carburetted hydrogen (to which he gives the name of *camphogen*), and oxygen, or ten atoms carbon + eight atoms hydrogen; or

Camphogen	68	89·4
One atom oxygen	8	10·6
	76	100·

The density of the vapour of camphor was found to be 5·468, and M. Dumas observes that supposing it to be composed of a volume of camphogen = 4·7634 + half a volume of oxygen = 0·5513, its density would be 5·3147, which comes very near to the experimental result. Oil of turpentine was found to be constituted precisely of the same proportions of carbon and hydrogen as camphogen:—the density of its vapour was by experiment 4·765 to 4·764, which agrees with the analysis.

When oil of peppermint is cooled to about 32°, it yields prismatic crystals, readily separable from the fluid part. These crystals pressed between folds of blotting paper, are colourless; fusible at 75° Fahr., volatile without decomposition, and again crystallize; they are very slightly soluble in water, but dissolve in alcohol, æther, and oils. They have the smell and taste of peppermint in a great degree. By analysis it appeared that this camphor was composed of 77·3 carbon, 12·6 hydrogen, and 10·1 oxygen in 100 parts; and it differs from common camphor in containing two more volumes of hydrogen.

The solid portion of oil of aniseed yielded by analysis, in 100 parts, 81·35 carbon, 8·26 hydrogen, 10·39 carbon: by adjusting these results, slightly, it appears that oil of aniseed contains two volumes less of hydrogen than common camphor.

M. Dumas conceives that essential oils are compounds of hydrogen and carbon, which, by oxidizement, produce camphors; this degree of oxidizement, however, is not to be confounded with the higher degree of it which occurs when the oils are freely exposed in thin strata to the air, for they are then converted into resins.

It appears from these experiments, that

10 carbon	10 hydrogen	1 concrete oil of peppermint.
10 ——	8 ——	1 oxygen, form common camphor.
10 ——	8 ——	oil of turpentine.
10 ——	6 ——	1 concrete oil of aniseed.
10 ——	4 ——	naphthaline

M. Dumas is disposed to believe that the absorption of oxygen by essential oils, produces different effects according as it occurs under the influence of water or when dry. The constitution of pure oil of lemons appears to be perfectly similar to that of oil of turpentine by M. Dumas's analysis, and it differs very little from that of M. Th. de Saussure. M. Dumas finds naphtha to be composed of 6 atoms carbon + 5 hydrogen; a conclusion which also agrees nearly with that of M. de Saussure; the density of its vapour should therefore be 2·870, which varies but slightly from that obtained by experiment.

Ann. de Chim. et de Phys. July 1832.

FORMATION OF ACETIC ACID FROM CARBONIC OXIDE AND HYDROGEN.

M. Matteuci succeeded in procuring acetic acid by passing oxide of carbon through water in which copper was suspended. The copper was prepared by passing a current of hydrogen over its oxide obtained by calcining acetate of copper; the oxide of carbon was procured by heating a mixture of one part of well calcined charcoal with three parts of carbonate of lime in a gun-barrel; in order to separate the small quantity of carbonic acid formed, lime was placed in a part of the barrel, and the gas passed over it.

The oxide of carbon was then passed into distilled water containing the copper: in a short time the water became greenish, and its intensity of colour was increased the longer the current of the gas continued to pass through the water. M. Matteuci therefore concluded that the substance dissolved was acetate of copper, derived from the decomposition of the water by the carbonic oxide and copper, the hydrogen uniting with the former to give acetic acid, and the oxygen with the latter forming oxide.

To verify this conclusion the solution was filtered, and a portion of it evaporated; to another portion a solution of ferrocyanate of potash was added, and an abundant red brown precipitate was obtained; sulphuretted hydrogen gave a black precipitate, and left an acid liquor, which, after being heated, was combined with oxide of lead, and formed a soluble salt. The solution when treated with iron produced a greenish soluble salt, which decomposed on exposure to the atmosphere, and deposited a reddish powder.

The evaporated liquor gave a small quantity of a crystallized substance of a greenish colour, which treated with sulphuric acid effervesced slightly, and disengaged vapours which had the properties of acetic acid.

When oxide of copper was substituted for the metal, no acetic acid was formed; but it was produced when a current of cyanogen was passed into water containing copper.

M. Matteuci found also, that when acetic acid, sulphuric acid, and peroxide of manganese are mixed, no formic acid is produced, as when some other vegetable acids are so treated; and he considers carbonic oxide as a compound analogous to cyanogen, and susceptible of forming an acid by combining either with oxygen or with hydrogen.—*Bibliotheque Univ.* June 1832.

DELPHIA AND SOLANIA.

Mons. M. O. Henry gives the following as the experimental analysis and theoretical constitution of these alkalies:

	Experiment.		*Theory.*	
Delphia.	Carbon....	74·240	26 atoms	74·62
	Oxygen ..	13·562	$3\frac{1}{2}$ ——	13·14
	Hydrogen..	8·870	38 ——	8·90
	Azote	3·328	1 ——	3·34
		100·		100·
Solania.	Carbon....	75·000	28 atoms	75·33
	Oxygen ..	12·778	$3\frac{1}{2}$ ——	12·32
	Hydrogen..	9·142	42 ——	9·22
	Azote	3·080	1 ——	3·13
		100·		100·

Journal de Pharmacie, Dec. 1832.

ON MECONINE.

Meconine was discovered in opium, by M. Couerbe in 1830. It is contained in opium in but small quantity. To prepare it, opium cut into thin slices is to be treated with cold water till it comes away colourless; the solution is to be filtered and evaporated till its specific gravity is about 1·060; then ammonia diluted with five or six times its bulk of water is to be added till precipitation ceases: this precipitate contains much morphia, and a little narcotine. When this precipitate has perfectly subsided, it is to be washed till the water comes off nearly colourless: let this precipitate thus washed be set aside, and add the washings to the ammoniacal liquor from which the precipitate was thrown down, and evaporate the mixture with a gentle heat till it has acquired the consistence of treacle; then set it aside for a fortnight in a cool place, and crystals will be formed. These crystals are brownish, and consist of meconine, meconiates, and other substances.—To separate the meconine they are to be boiled in alcohol of specific gravity 0·837, till it ceases to dissolve, and the spirituous solutions being mixed are to be distilled to about one third; by cooling, crystals are obtained; and by evaporating the solution a further quantity is procured. These crystals are to be dissolved in water, to be treated with animal charcoal, again to be crystallized, then dissolved in boiling æther, and by spontaneous evaporation meconine is deposited.

The properties of meconine are: that it is colourless and crystalline; the form of the crystal is a six-sided prism, two sides of which are larger than the others, and it is terminated by a dihedral summit. Meconine is inodorous, at first tasteless, but eventually sensibly acid. It is perfectly soluble in water, alcohol and æther, and crystallizes well from any of these fluids. It is composed of—

Carbon............	60·247
Oxygen	34·997
Hydrogen	3·746
	99·

Supposing it to be composed of the annexed atoms of its elements, its composition would be:

9 atoms carbon............	60·234
4 —— oxygen	35·023
9 —— hydrogen	4·742
	99·999

When heated to 194° Fahr. meconine begins to liquefy; and at one degree above it is entirely fluid, perfectly colourless, and limpid.— It retains its fluidity when cooled to 165°. At 311° it is converted into vapour, and may be distilled without losing any of its properties; and by cooling it becomes a mass resembling pure fat.

At a medium temperature 265·75 parts of water dissolve one part of meconine, while it requires only 18·55 parts of boiling water to dissolve the same quantity; alcohol, æther, and the essential oils dissolve it in much larger quantity. Acetate of lead produces no precipitate in a solution of meconine, but the subacetate does. The alkalies, potash and soda, dissolve but do not form determinate compounds with it; ammonia whether hot or cold does not dissolve it.—*Journal de Pharmacie*, Dec. 1832.

The action of sulphuric and nitric acid upon meconine, we shall give in a future Number.—Ed.

INQUIRY RESPECTING THE USE OF CLOCKS INSTEAD OF CHRONOMETERS AT SEA.

To the Editors of the Phil. Mag. and Journ. of Science.

Gentlemen,

I should be glad to learn from any of your readers whether any experiments have been made, and with what results, of using clocks instead of chronometers on board a ship.

For this purpose, two things only appear necessary; first, that the clock should be azimuth hung, the point of suspension of the clock coinciding exactly with the point of suspension of the pendulum; second, that the clock-case should be so loaded with an adjustible weight that the centre of oscillation of the clock-case should coincide with the centre of oscillation of the pendulum.

The clock had better have a main-spring instead of a weight. Possibly this reasoning may be erroneous; or the experiment may have been tried, and failed from unknown causes; of which I should be glad to be informed. S. S.

Preparing for Publication.

A Manual of Experimental Chemistry, by Richard Phillips, F.R.S. L. & E. &c.

COMMEMORATION OF THE CENTENARY OF THE BIRTHDAY OF PRIESTLEY.

In the fourth volume of the Philosophical Magazine and Annals (page 379) we announced the intention of the cultivators of Natural History to commemorate by a public meeting the second centenary of the birthday of Ray, whose extensive and accurate labours laid the foundation for the philosophical study of organic nature in this country; and in the following volume we had the pleasure of recording the highly satisfactory accomplishment of that intention. We have now equal pleasure in announcing that the cultivators of Chemical Science are about to commemorate in a similar manner, the centenary of the birthday of that distinguished Philosopher of a later period, to whom—whether we regard the invention of the apparatus requisite for researches on aëriform fluids, the investigation of the action of those fluids upon each other, or the number of them actually first discovered by him, and his determination of their relation to the functions of animal and vegetable life—must be awarded the merit of being the Founder of Pneumatic or Gaseous Chemistry. The object of this notice is to give additional publicity to the following circular:—

"*Commemoration of Priestley.*— A hundred years having elapsed since the Birth of the Philosopher whose extensive and successful researches entitle him to be considered as the Founder of Pneumatic Chemistry, it has been resolved that the event shall be commemorated by a Public Dinner on the 25th of March; and the Committee appointed upon the occasion, express a hope that all those who justly appreciate the high importance of Priestley's Discoveries, their influence upon the progress of Science, and the honour which they have conferred upon his Country, will heartily cooperate with them in carrying so desirable a plan into effect."

The following gentlemen have undertaken the office of Stewards:

Arthur Aikin, Esq. F.L.S.; John P. Atkins, Esq. F.S.A.; William Babington, M.D. F.R.S.; William Thomas Brande, Esq. F.R.S.; John Bostock, M.D. F.R.S.; J. G. Children, Esq. Sec. R.S.; Rev. J. Corrie, F.R.S.; J. F. Daniell, Esq. F.R.S.; Michael Faraday, Esq. D.C.L. F.R.S.; C. Hatchett, Esq. F.R.S.; Richard Knight, Esq. F.G.S.; J. Ayrton Paris, M.D. F.R.S.; W. H. Pepys, Esq. F.R.S.; Richard Phillips, Esq. F.R.S.; George Rennie, Esq. F.R.S.; Peter M. Roget, M.D. Sec. R.S.; John Taylor, Esq. F.R.S.; Richard Taylor, Esq. F.L.S.; E. Turner, M.D. F.R.S.; together with other Gentlemen attached to scientific pursuits who have been invited to join them.

It is gratifying to find in the list the names of men who are best qualified to estimate the value of Priestley's discoveries: and considering how much some of these are connected with physiology,—considering also his labours in various departments of physical science, it may be expected that the lovers of every branch of Natural Philosophy and Natural History will join the Chemists in doing honour to his memory, and asserting for our country the indubitable claim of being that in which were made "the first decided advances in the knowledge of elastic fluids."

LUNAR OCCULTATIONS FOR FEBRUARY.

Occultations of fixed Stars by the Moon, visible at Greenwich in the Year 1833. Computed by Thomas Maclear, *Esq.; and circulated by the Astronomical Society.*

*** The *angles* are reckoued *from* the northernmost point, and also *from* the vertex, *towards* the right hand, round the circumference of the Moon's image, as exhibited in an inverting telescope.

1833.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions. Sidereal time.	Immersions. Mean time.	Immersions. Angle from North Point.	Immersions. Angle from Vertex.	Emersions. Sidereal time.	Emersions. Mean time.	Emersions. Angle from North Point.	Emersions. Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
Feb. 1	15 Gemin.	6	799	9 35	12 47	63	100	10 34	13 46	293	334
	17 Gemin.	7	805	10 12	13 24	64	104	11 10	14 22	290	332
3	47 δ Cancri	4·5	1066	13 20	16 23	13	54	13 43	16 46	320	1
	(179) Canc.	7	1080	15 21	18 24	70	110	under the hori.			
4	(74) Leonis	7	1143	2 35	5 36	86	46	3 29	6 30	258	217
5	53 *l* Leonis	6	1284	13 38	16 33	74	104	14 40	17 35	238	274
8	80 l^3 Virgin.	6	1551	14 40	17 24	28	39	15 40	18 24	276	296
14	28 Sagittarii	6	2164	15 8	17 29	93	70	16 28	18 48	259	245
	35 ν^2 Sagitt.	5	2181	21 4	23 23	30	51	21 32	23 51	339	5
Mar. 3	(224) Canc.	7	1098	5 26	6 41	88	49	6 32	7 46	254	220
	78 Cancri..	7	1108	10 30	11 44	71	90	11 38	12 52	255	286
4	34 Leonis	6	1214	11 26	12 36	23	38	12 15	13 25	291	317

Extract from the Meteorological Journal kept at Penzance by Mr. Giddy.

ANNUAL RESULTS.

1832.	Barometer. Max.	Barometer. Min.	Barometer. Mean.	Register Thermometer. Max.	Register Thermometer. Min.	Register Thermometer. Mean.	Rain in Inches.	Wet Days.	Dry Days.	Prevailing Winds.
Jan.	30·446	29·134	29·8525	53	30	43·5	3·200	15	16	SE.
Feb.	30·528	29·131	29·9968	53	34	43·0	2·310	13	16	NE.
Mar.	30·292	29·334	29·9495	54	32	45·5	4·470	18	13	NE.
April	30·398	29·186	29·9405	63	36	48·5	2·905	11	19	NE.
May	30·419	29·172	29·9201	68	40	52·5	4·225	13	18	NE.
June	30·292	29·360	29·8185	73	45	58·0	2·520	11	19	NW.
July	30·407	29·654	30·0480	71	50	61·0	2·105	8	23	NE.
Aug.	30·195	29·254	29·8583	71	48	60·5	5·815	15	16	W.
Sept.	30·436	29·610	30·0660	65	47	57·0	0·985	7	23	NW.
Oct.	30·322	29·110	30·0151	64	44	54·0	3·625	13	18	SW.
Nov.	30·434	29·328	29·8091	58	38	48·0	6·890	22	8	NW.
Dec.	30·542	29·545	30·0012	56	37	47·0	6·095	27	4	W.
1832.	30·542	29·110	29·939310	73	30	51·65	45·145	173	193	NE.

The rain-gauge at the ground-level; the wet days comprehend rainy, showery, and misty days.

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

Days of Month, 1832.	Barometer.					Thermometer.					Wind.			Rain.		
	London.		Penzance.		Boston	London.		Penzance.		Bost. $8\frac{1}{2}$ A.M.	Lond.	Penz.	Bost.	Lond.	Penz.	Bost.
	Max.	Min.	Max.	Min.	$8\frac{1}{2}$ A.M.	Max.	Min.	Max.	Min.							
Dec. 1	29·854	29·803	29·925	29·922	29·22	57	53	56	46	53	SW.	W.	calm	0·06	0·115	0·08
2	29·681	29·458	29·792	29·646	29·05	54	40	53	50	53	SW.	NW.	calm	·19	·310	.32
3	29·681	29·527	30·002	29·798	28·92	47	41	50	42	45	W.	NW.	NW.	·02	...	·04
4	30·179	29·978	30·208	30·084	29·49	47	34	48	42	43·5	N.	NE.	NW.	·02	...	·04
5	30·239	30·116	30·220	30·128	29·76	36	45	52	37	36	NW.	W.	calm	·18	...	·02
6	30·170	30·031	30·178	30·084	29·64	45	36	48	38	45	N.	N.	calm	·01	...	·17
○ 7	30·396	30·283	30·328	30·284	29·85	45	34	50	40	38	N.	NW.	calm	...	...	·02
8	30·526	30·385	30·368	30·328	29·87	47	44	52	43	42	W.	NW.	calm	·01	...	...
9	30·426	30·399	30·368	30·322	29·86	51	48	52	46	45	SW.	W.	calm	...	...	...
10	30·379	30·358	30·422	30·322	29·83	47	35	51	48	46	W.	W.	calm	·03	...	...
11	30·555	30·427	30·542	30·511	29·85	46	34	51	44	43·5	W.	NW.	calm	...	...	...
12	30·495	30·415	30·471	30·372	30·00	49	32	49	41	46	SW.	NW.	calm	...	·310	...
13	30·279	30·086	30·181	29·972	29·47	47	36	51	43	43	S.	SW.	calm	...	...	...
☾ 14	30·026	29·884	29·981	29·828	29·48	55	34	53	45	43·5	SW.	W.	calm	·03	...	...
15	29·889	29·602	29·928	29·578	29·16	52	27	52	45	42	N.	NW.	NW.	·50	·480	·26
16	30·039	29·909	29·934	29·816	29·56	48	38	52	42	33	W.	SW.	calm	·02	·300	...
17	29·543	29·479	29·772	29·672	29·07	57	38	54	44	50	SW.	W.	W.	·10	...	·23
18	29·728	29·519	29·828	29·681	29·00	45	31	48	44	41	W.	NW.	W.	...	·335	·05
19	29·793	29·727	29·871	29·834	29·30	42	28	47	40	33·5	W.	NW.	calm	...	·210	...
20	30·042	29·974	29·987	29·778	29·54	40	29	52	40	31·5	SW.	N.	calm	...	...	...
21	29·836	29·718	29·828	29·678	29·25	52	40	52	42	38	S.	NW.	W.	·01	·765	·21
● 22	29·872	29·803	29·825	29·675	29·35	54	44	53	48	44	S.	W.	W.	·02	·330	·16
23	29·941	29·685	30·022	29·675	29·22	53	41	48	45	48	SW.	N.	calm	·07	0·100	...
24	30·050	29·819	29·978	29·672	29·55	53	45	54	40	42	SW.	SW.	calm	...	1·000	·12
25	30·012	29·674	29·966	29·675	28·98	54	31	51	47	54·5	SW.	NW.	W.	·22	0·150	·13
26	30·196	30·153	30·172	30·128	29·60	47	30	48	42	38	SW.	NW.	calm	...	...	·08
27	30·219	30·179	30·181	29·972	29·75	44	29	52	38	35	W.	S.	calm	...	·520	...
28	30·115	29·990	29·934	29·722	29·97	41	32	53	40	28	E.	W.	calm	·06	·300	...
29	29·889	29·560	29·622	29·545	29·46	40	34	47	44	38	E.	W.	SE.	·15	·700	·10
☽ 30	30·170	30·128	30·042	30·020	29·78	40	32	48	42	38	E.	SW.	E.	·06	·170	...
31	30·371	29·849	30·408	30·034	29·50	40	30	47	42	34	NW.	N.	calm	·12	...	...
	30·555	29·458	30·542	29·545	29·49	57	27	56	37	41·6				1·88	6·095	2·03

Remarks.

London — Dec. 1. Rainy. 2. Stormy: showers: fine: stormy, with rain and hail at night. 3. Clear and windy: at noon boisterous, with rain: fine at night. 4. Clear and cold. 5. Fine: rain at night. 6. Drizzly. 7—10. Foggy. 11. Very fine. 12. Foggy. 13, 14. Foggy in the mornings: fine. 15. Heavy rain. 16. Frosty: fine. 17. Cloudy: stormy and wet. 18. Stormy: showers. 19, 20. Frosty: clear. 21. Rain: clear. 22. Fine. 23. Drizzly. 24. Foggy. 25. Stormy and wet. 26. Frosty: clear. 27. Slight frost: fine: very dense fog at night. 28. Dense fog. 29. Cold and wet. 30. Clear and cold. 31. Sleet: cold rain: overcast at night.

Penzance.—Dec. 1. Rain. 2. Fair: hail, rain, thunder and lightning. 3. Fair: showers. 4. Fair. 5. Clear: showers. 6. Fair. 7. Fair: misty. 8—11. Misty. 12. Fair. 13. Misty. 14. Fair: rain at night. 15. Rain: fair. 16. Fair: rain. 17. Fair: showers. 18, 19. Showers: hail and rain. 20. Fair: rain. 21. Misty. 22. Misty: rain at night. 23. Rain: fair. 24. Rain throughout. 25. Rain: fair. 26. Fair: showers. 27. Fair: rain. 28, 29. Rain. 30. Misty: evening, rain. 31. Fair.

Boston.— Dec. 1. Fine: rain early A.M. 2. Fine: rain early A.M, and P.M. 3. Stormy: rain P.M. 4—6 Cloudy: rain P.M. 7—9. Cloudy. 10. Fine. 11—14. Cloudy. 15. Rain. 16. Fine: rain P.M. 17. Cloudy: rain P.M. early A.M.: stormy night. 18. Cloudy: rain P.M. early A.M. 19, 20. Fine. 21. Rain. 22 Fine: rain early A.M. 23. Cloudy: rain P.M. 24. Cloudy. 25. Rain, and stormy: rain P.M. 26, 27. Fine. 28. Rime frosts 29. Rain. 30. Cloudy. 31. Cloudy, snow A.M.

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JOURNAL OF SCIENCE.

[THIRD SERIES.]

MARCH 1833.

XXV. *Remarks on* Mr. Potter's *Experiment on Interference. By* G. B. Airy, *Esq. Professor of Astronomy and Experimental Philosophy in the University of Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the last Number of your Journal is a paper by Mr. Potter upon certain phænomena of Interference, which he considers to be inexplicable on the theory of undulations. These phænomena are, in fact, a confirmation of the truth of that theory, and might have been predicted from the theory. I should not therefore have troubled you with these remarks, if I did not feel that the public is much interested in seeing a distinct and correct interpretation put upon experiments and calculations like those to which I allude; and if I were not convinced that Mr. Potter, whose labours as an experimentalist I value most highly, would feel any thing but pain at my pointing out the error into which (I conceive) he has fallen as a theorist.

In Mr. Potter's experiment two pencils of light originating from a common source are made to interfere by falling upon two plane mirrors inclined at a small angle: the two reflected pencils fall upon a prism whose edge is parallel to the line of junction of the mirrors, and the interference-fringes, after emergence, are examined by means of an eye-glass in the usual manner. The light being supposed homogeneous, Mr. Potter finds (correctly) from the theory of undulations that the points, at which the paths of the pencils from the two images are equal, are almost exactly at the centre of the mixture of the lights: he infers that the centre of the mixture of lights ought to be the centre of the interference-fringes; instead of which it appears in experiment that on removing the eye and eye-glass further and further from the prism, the interference-fringes deviate from the centre of the mixed light more and more towards the thick side of the prism, and finally disappear. And this discordance is the point now to be explained.

Suppose now that the light is *not* homogeneous (the reasons for believing that this supposition is true will be given presently); and suppose only that the proportion of the differences of the lengths of the waves, or if you please, of the lengths of the fits, for all colours, from the length corresponding to some particular colour, is the same as the proportion of the differences of the deviation for those colours (produced by the prism of the experiment) from the deviation corresponding to that particular colour. This assumption is very nearly true if the light comprehends rays from no more than one half or one third of the spectrum. Let us now inquire what will be the position of the fringes.

Any one of the kinds of homogeneous light composing the incident heterogeneous light will produce a series of bright and dark bars, unlimited in number as far as the mixture of light from the two pencils extends, and undistinguishable in quality. The consideration, therefore, of homogeneous light will never enable us to determine which is the point that the eye immediately turns to as the centre of the fringes. What then is the physical circumstance that determines the centre of the fringes?

The answer is very easy. For different colours the bars have different breadths. If then the bars of all colours coincide at one part of the mixture of light, they will not coincide at any other part; but at equal distances on both sides from that place of coincidence they will be equally far from the state of coincidence. If then we can find where the bars of all colours coincide, that point is the centre of the fringes.

It appears then that the centre of the fringes is *not* necessarily the point where the two pencils of light have described equal paths, but is determined by considerations of a perfectly different kind. And this is the radical error into which Mr. Potter has fallen. The distinction is important in this and in other experiments.

To find the centre of the fringes in Mr. Potter's experiment, we must proceed thus:—First, we premise that the prism produces greater deviation for yellow rays than for red rays, and that the interference-bars are narrower for yellow rays than for red rays. Next, we allow that the centre of the mixture *for each colour* is a point which the interfering pencils of that colour have reached by equal (or rather equivalent) paths, and therefore is the place of a bright bar *for that colour.* Lastly, as the interfering pencils for different colours are made to deviate unequally by the prism, the centres of the mixture, for different colours, will not occupy the same place.

From the last consideration it follows at once that the centre of the mixture will not be the place of the centre of the fringes, inasmuch as similar bars of the different colours are not united there. To find the place where they are united we must consider that the centre of the red mixture, being the least deviated, has made the smallest progress towards the thick side of the prism, but that its bars are the broadest: the centre of the yellow mixture has made the greatest progress, but its bars are the narrowest. A number n of bars can therefore be found such that the linear deviation of the red centre $+ n \times$ breadth of a red bar $=$ linear deviation of yellow centre $+ n \times$ breadth of a yellow bar: (if the equality is not exact, n may be chosen so that the second side is the greater, but so that on putting $n+1$ for n the first side will be the greater). The nth red bar and the nth yellow bar, thus determined, will coincide; and their place will be the true centre of the fringes. It is plain that when the linear dispersion produced by the prism is small, that is, when the eye-glass is very near the prism, the centre of the fringes will not sensibly differ from the centre of the mixture for any of the colours; but that when the linear dispersion is great, or the eye-glass far from the prism, the centre of the fringes will be far from the centre of any of the mixtures.

Algebraically we may express it thus:—If d be the linear deviation of the centre of the mixture for one standard colour, $d+\delta d$ that for any other colour, b the breadth of a bar for the standard colour, $b+\delta b$ that for any other colour, then the distances of the nth bars from the origin of d will be $d+n\,b$ and $d+\delta d+n\,(b+\delta b)$ respectively; and these will be equal if $\delta d+n\,\delta b=0$, whence $n=-\dfrac{\delta d}{\delta b}$; which makes the distance of the place of coincidence $d-\dfrac{b\,\delta d}{\delta b}$. As δd and δb have different signs, this is greater than d; or the place of coincidence is removed from the origin of d in the direction of deviation, that is, towards the thick side of the prism. And as, upon receding from the prism, δd increases, while $\dfrac{b}{\delta b}$ is not altered, it follows that the place of coincidence or centre of the fringes advances further and further towards the thick side of the prism. If δd and δb have the same proportion for rays of different colours (which we have mentioned above to be very nearly accurate), then in a given position of the eye-glass the union of colours will be perfect at the place which we have found for the centre of the fringes; and the same place would be found for the centre of the fringes if a part of the colours were abstracted, or if the light were made more nearly homogeneous.

It appears then that, according to the theory of undulations, we ought to have precisely the phænomenon which Mr. Potter has observed, supposing the light heterogeneous. It appears also that a diminution of the heterogeneity will not alter the place of the centre of the fringes; its only effect being to make a greater number of bars visible on both sides of the centre; except, indeed, the light were strictly homogeneous, when the whole place of mixture of lights would be covered with bars, of which no one could be called the centre of fringes more than another.

But this will not apply to Mr. Potter's experiment unless we can show that the light used by him was heterogeneous. I believe that any person who has tried experiments of this kind sufficiently to know what brilliancy is necessary for them, and how faint is light of any reasonable degree of homogeneity, will at once allow it to be *probable.* But I will go further, and will assert that, on the face of the experiment itself, it is *certain* that the light was heterogeneous. And the reason is simple and unanswerable, that if the light had been homogeneous there would have been no distinguishable centre of fringes. And however small might be the heterogeneity, the centre of fringes would occupy the same place as if the light were (within certain limits) ever so heterogeneous. The phænomenon observed by Mr. Potter is therefore, as far as general explanation goes, completely in accordance with the theory of undulations.

The principle upon which I have determined the place of the centre of the fringes, though it has been used by Sir John Herschel in one experiment, has never (so far as I know) been distinctly stated. It applies, however, in a great number of instances, as well in the explanation of the phænomena of polarization by interference, as in the interference of common light. The following instructive experiment (which has frequently been alluded to) shows very clearly to the eye the application of the principle; and I cite it the more readily, because I infer from a passage in Mr. Potter's paper that he may not be unwilling to try it. I shall describe it in the form in which I have found it easiest in practice.

Make a small wooden frame (like a window-frame, or rather like the frame of a schoolboy's slate, but smaller); with a saw cut through the centres of two opposite sides and unite them by hinges; keeping the parts in one plane, fix in them a clear piece of glass with putty; and when it is well bedded, cut it with a diamond along the line of the hinges. The frame will now turn well, carrying two pieces of glass of the same thickness almost exactly in contact at the turning line. Fix one side of the frame so that the glass in it receives, perpendicularly, the light which is to fall upon one of the mirrors, and so that the line of the hinges divides the pencils which fall upon the two mirrors. One pencil will then pass through the fixed glass and one through the moveable glass. Now if the glasses are in the same plane, the interference-fringes are not altered; but if, while one is perpendicular to its pencil, the other is inclined, the fringes immediately shift towards the pencil which has passed through the inclined glass. Thus far the experiment has often been described; but the part which I wish particularly to point out is the following:—If, after looking with the glasses in the same plane, the observer should leave his eye-glass and change the position of one glass and then return to his eye-glass, he would find the centre of the fringes shifted, and might perhaps infer that the fringes had shifted bodily, and that the bar which was the central bar before is the central bar now. Nothing could be less true; as he would see if by a little additional apparatus (a string, and a weight to act in opposition to the string are quite sufficient,) he inclined the glass without taking his eye from the eye-glass. The fringes shift, but at the same time they change their character in such a manner that till they have been observed a few times the eye is completely bewildered. Supposing (for clearness of ideas) that the fringes shift to the right; the central white bar as it travels becomes blue at its right edge and red at its left edge, and when it has shifted about *four* bars, the bright white bar is the *fifth* from the original place of the bright white bar (I do not pretend to great accuracy in these numbers). This observation, which is easily made without moving the eye, shows clearly the difference between the *shifting of the bars* and the *shifting of the centre of the fringes*. In Mr. Potter's experiment, while (upon withdrawing the eye and eye-glass from the prism) the *centre of the fringes* shifts, the *bars* themselves (according to theory) remain nearly stationary; but it would not be easy to preserve that steadiness of eye which is necessary for these observations.

The other fact which Mr. Potter mentions,—namely, that the fringes become narrower as the prism is turned so as to increase much the angle of emergence,—is a direct consequence of the theory of undulations. In fact, the light after emergence comes from two virtual images more widely separated than the first images; and the breadth of the fringes is, *cæteris paribus*, inversely as the distance of the radiant images.

I have now made the remarks which I proposed to make upon Mr. Potter's experiment. I cannot, however, conclude without noticing an incidental expression, "the unfortunate half undulation which has continually to be asked for by those who adopt the undulatory theory of light." And this I do, partly because I have heard an objection something like Mr. Potter's, and partly because from Mr. Potter's way of stating it, I conclude that he must have derived it from some very imperfect or erroneous statement. I know of *no* case in which "half an undulation has to be asked for."

It happens sometimes unfortunately for a theory, that the words of its original proposer, which were necessary when the theory was new, are retained when they are not only unnecessary, but even mischievous. The propositions which in the early stages of a theory are necessary to point out the distinctions of different cases, add in no small degree to its obscurity when it is so far advanced that every case can be included in one general process. This has happened in regard to the propositions to which, I suppose, Mr. Potter alludes here.

The change of half an undulation is, in fact, a change of sign of the vibrations of which the undulation consists. The only thing to be explained then is a change of sign; and the only cases in which it occurs (so far as I know) are, certain cases of the interference of polarized light; and the interference of light forming Newton's rings. Perhaps I can best explain the apparent difficulty by referring to simple geometrical cases.

In describing to a student the relation between the versed sine and cosine, we might say, "the versed sine is formed by subtracting the cosine from the radius when the arc is less than a quadrant, and by adding it when it is greater than a quadrant." These are to him two distinct cases; but the accomplished mathematician considers them as one, connected by the theory of the change of sign. Thus it is with the interference of polarized light: in certain cases two resolved vibrations are added; on approaching a certain limit one of them disappears; on passing that limit it reappears, but in such a way that it must be subtracted from the other. But all these changes take place with as great regularity as the changes in the rule for forming the versed sine from the cosine, and in fact follow exactly the same law.

In explaining to a young student in mechanics the motion of elastic balls when one has impinged on another which was at rest, we might perhaps make two separate cases distinguished by the circumstances of the impinging ball being the greater, or the impinging ball being the smaller; and we should point out, that in the former case the motion of the impinging ball after impact was in the same direction as before; while in the latter case the motion after impact was in the direction opposite to its first motion. A more advanced student would perceive that both were included in the general formula $\frac{A-B}{A+B}v$. Thus it is with the reflexion of light from the inner or outer surface of glass: the mechanical conditions appear to be precisely similar to those which I have mentioned, and the theoretical result is similar; namely, that whereas in one case we are bound to suppose the remaining motion (which produces the reflected ray) to retain the same direction as before, in the other case we are equally bound to suppose that the remaining motion has a direction opposite to that which it had before. I lay smaller stress upon this part of the theory than upon the other, because I consider the mechanical part of the theory of undulations generally as less complete than the geometrical part: but what I have stated shows clearly that there is nothing arbitrary in this change of sign; but that it is absolutely required by theory as far as theory goes. I may add, that in making a complete mathematical investigation in any part of the theory of undulations,—for the explanation, for instance, of the most complicated phænomena of polarization,—the "demand of half an undulation," which has made so strong an impression on Mr. Potter, *never occurs.*

I rejoice that Mr. Potter has seriously undertaken to compare his experiments with the mathematical results derived from the theory of undulations. I hope (for the sake of the science) that he will continue his experiments; and I hope (more particularly for his own conviction) that he will continue the corresponding mathematical investigations. If the comparison between them be continued in the same philosophical spirit as that which marks his last paper, I can with confidence predict one result:—Mr. Potter will very soon become an *undulationist.*

I am, Gentlemen, your obedient Servant,

Observatory, Cambridge, Feb. 2, 1833. G. B. Airy.

XXVI. *Observations on the Action of Light upon the Retina; with an Examination of the Phænomena described by* Mr. Smith *of Fochabers.* *By* Sir David Brewster, *LL.D. F.R.S.*

In calling the attention of philosophers to this curious and important class of phænomena, I presume that the reader has perused my Observations on Mr. Smith's experiment*, and the ingenious paper which has been since published by that gentleman†, but which was written before the publication of my paper.

Mr. Smith has described so minutely the mode of performing his beautiful experiment, and has stated the general phænomenon so distinctly, that it may be sufficient to observe that when a candle is held near the right eye so as to be seen by it, but not by the left eye, and when both eyes look at a narrow stripe of white paper so as to see it double, the image of the paper seen by the right, or excited eye, will be *green*, and that seen by the *left*, or eye protected from the candle-light, will appear *reddish.*

Mr. Smith has concluded from a series of ingenious observations, that the light applied to the right eye actually influences the vision of the left eye in virtue of an action of the brain; that the *green* and *red* colours are complementary to oneanother; and that the *green* colour is owing to a diminished sensibility of the *right* eye to *red* light, and the *red* colour to an equally increased sensibility of the *left* eye to *green* light. From these results he has deduced the existence of two "functions hitherto unknown," which are excited in the brain by "indistinctness of vision," and the object of which functions is to "remove more or less the exciting cause, and produce distinct vision."—"I forbear," says Mr. Smith, "from making any observations on the singular nature of the *cerebral functions* thus detected, or on the perhaps still more singular nature of their *exciting causes,* thinking it due to truth, in a case that appears to involve principles entirely new, to wait the observations of competent inquirers, with whom it remains to confirm or refute, by an impartial scrutiny, the results which I have obtained."

Having had occasion to pay considerable attention to this class of phænomena, and having, in the paper already referred to, arrived at a result opposite to that obtained by Mr. Smith, I feel it incumbent upon me to undertake the scrutiny which

* See this Magazine, vol. i. p. 171. † Ibid. pp. 249 and 343.

Mr. Smith, in the true spirit of philosophy, invites, and I hope I shall do it with the impartiality which truth requires.

In performing the fundamental experiment in candle-light, I find that the colour of the paper seen by the excited eye varies with the distance of the image from the excited part of the retina. When the image of the paper is at the furthest possible distance from the luminous or exciting image, the colour is yellowish, becoming *greenish yellow*, *green*, *blue*, *dirty purple*, and finally disappearing as the image approaches to the point of maximum excitation.—The cause of these changes is obvious: the part of the retina least excited, because furthest removed from the exciting cause, becomes insensible to the *red* rays; nearer the point of excitation it becomes insensible to the *orange* also, nearer still to the *yellow*, nearer still to the *green*, and so on, till close to that point it is insensible to all light whatever. Hence we have the paper first *yellowish*, or a mixture of all the rays except *red*; next *greenish yellow*, or a mixture of all the rays except *red* and *orange*; next *green*, and so on with the other colours.

Let us now attend to the *red* image in the unexcited eye. This *red* image does not change its colour, while the other image is passing from *yellow* up to *dark purple*; which it ought to do if the colours were complementary; nor does the *red* change its intensity as it ought to do if the sensibility of the one eye to red light was increased in the same proportion as the sensibility of the other eye to that light is diminished.

But even the *red* colour of the unexcited eye is very undecided. If when we see it brightest we quickly shut the *excited* eye, its *redness* becomes instantly much less decided, and in like manner if we shut the unexcited eye, the *greenness* of the other image is much less brilliant. This diminution of tint does not arise in the first case from the eye being shut to the exciting light, for the colours do not disappear with such rapidity; and in the second case the exciting light still acts. The true cause of the diminution of tint in both cases is the want of contrast, in virtue of which the *green* image becomes *greener* in the presence of the *red* one, and the *red* image *redder* in the presence of the *green* one.

In the paper already referred to (see this Journal, vol. i. p. 172.), I have stated that *red* light predominates in candle-light; and I have found that in a spectrum from a candle the intensity of the blue rays is much less, and that of the red rays much greater, than in the solar spectrum. Hence as this *red* tinge is increased by contrast with the *green* image, we obtain a more simple explanation of the apparent affection of the left eye, than by resorting to an action of the brain.

Mr. Smith has remarked (page 251, Exp. 1.), "that the same phænomena occur, and are even more vivid, if one of the eyes is exposed to the rays of the sun either direct or reflected." According to my observations, however, the phænomena are quite changed in the latter case. The *green* image becomes a bright *blue*, and the *reddish* image almost colourless or *white*. The *colours* are therefore not complementary, and there is not even the appearance of an influence upon the unexcited eye, whatever tinge there may be of redness in the one image being merely the effect of contrast.

This result enabled me to make a very decisive experiment. I took *two* slips of paper, and having illuminated the one with *day-light*, and the other with *candle-light*, I excited the right eye with the light of a candle, and doubled the images. The slip illuminated with *day-light* gave one image *blue* and the other *white*; while the slip illuminated with *candle-light* gave one image *green* and the other *reddish*. Now, in this experiment the exciting cause was the same, and yet the colours were different,—obviously proving that the colours depend upon the nature of the light which falls upon the slip of paper,—that they are not complementary, and that when pure white light is used the unexcited eye sees the paper colourless.

These results may be confirmed by examining the *red* image when the *green* image has become a *dark purple* by bringing it close to the excited spot. In this case the redness instead of being increased is greatly diminished, and the slip appears *yellowish* or *cream*-coloured. This arises from the want of contrast, the *green* colour which heightened the *red* having almost wholly disappeared.

I now took two candles of equal brightness, and having placed them at the distance of about four inches from each other, and about two inches from the eyes, I held a slip of paper between them, and having doubled it by looking at a more distant object, I found that both images were yellowish green, not only when thus seen separately, but also when combined into one. Now if the colours were complementary, and if there was a balance of increased and diminished sensibility, the image must have been *white*.

Before quitting this part of the subject I may mention the curious fact, that when the eyelid of the excited eye is closed completely, and the eyebrow kept raised, before the exciting light is applied to it, then when the light is applied, there will be only one slip of paper seen, as there is only one eye open; and the colour of this slip is certainly white as usual: but the moment we open the right eye, withdrawing the exciting light at the same time, the slip seen by the newly opened eye is a *bright green*, (though the light passed only through the eyelid,) and fully as *green* as if the eye had been open;—and what is interesting, the other white slip instantly becomes *reddish*, which now can only happen from contrast.

I have mentioned in a former paper (vol. i. p. 172.), that a stick of red sealing-wax seen by a highly-excited part of the excited eye was of a *dark liver* colour; while to the other eye its colour was brilliantly red. By substituting the following coloured fluids in place of the slip of paper, and illuminating them by transmitted light, I observed them to change their tints, as shown in the following table.—The experiments were made in candle-light.

	Colour.	*Colour to the excited Eye.*	*Colour to the unexcited Eye.*
Nitrate of copper* ..	Greenish blue.	Deep blue.	Pale bluish white.
Carbazotic acid†	Yellow.	Green yellow.	Orange yellow.
Sulpho-cyanate of iron	Orange red.	Yellowish red.	More orange red.
Meconate of iron ...	Ditto.	Ditto.	Ditto.
Oxalate of chromium and potash	Port-wine red.	Pink red.	Red.
Bichromate of potash and arsenious acid	Green.	Bright green.	Yellowish green.
Chloride of cobalt ...	Pale red.	Paler red.	Brighter red.
Ammoniuret of copper	Blue.	Indigo.	Blue.
Nitrate of nickel	Green.	Bright green.	Yellowish green.
Muriate of copper ..	Whitish green.	Bright green.	Whitish yellow.
Purple glass	Purple.	Pink red.	Brick red.
Green glass	Green.	Bright green.	Yellowish green.

In all these experiments, where the effect is well marked, the image in the unexcited eye receives as it were an addition of the less refrangible rays, while the other image loses a portion of the same rays,—results which are entirely accordant with our previous observations.

Mr. Smith next proceeds to a very delicate and somewhat speculative task, into which we are not much disposed to follow him; namely, to ascertain the causes which excite the eye to see the slip of paper *green* and *red*, and the purposes for which these causes are called into action. He conceives the exciting causes to be the *indistinct* vision, and the heterogeneous or *white* colour of the exciting light; that is, if the exciting light is homogeneous, and seen distinctly, the complementary *red* and *green* colours are no longer visible.

Mr. Smith's experiment, No. 8, p. 344, is destined to prove that when the exciting light is seen distinctly, the images of

* This solution gave a spectrum in which there were no red or orange rays.
† No blue in its spectrum.

the slip of paper are colourless. A bright light is held as near the eye as possible, so as to be seen with perfect distinctness, and a slip of paper illuminated by a candle held above the head is placed between the exciting light and the eye, and so near the latter as to be seen double. In this experiment, which he has repeated often with the utmost care, the images of the slip of paper are perfectly colourless, the one seen by the exposed eye being only a little darker than the other. "In performing this experiment," says Mr. Smith, "*great caution* is required that the exposed eye be adapted correctly to the distinct vision of the flame; for by much observation I have found that a *small error* in this respect, such as occurs when the eye becomes dazzled, is sufficient to excite those changes in the sensibility to red light which have been proved to be the causes of the green and red appearances of the white paper."

Now, supposing this experiment and its result to be exactly described by Mr. Smith, four observations present themselves to us.

1. As the exciting bright light must have been about five or six inches from the eye, and was, I presume, that of an Argand lamp or large candle, it must have subtended an angle of 8° or 10°, so that a great part of the exciting light must have been seen indistinctly, as the eye sees only with perfect distinctness a small point in the axis of the eye. In order, too, to see this large light with perfect distinctness, Mr. Smith must have fixed his eye upon the margin of it, so that the other margin of the flame must have been exciting the eye by indistinct vision, at the distance of 8° or 10° from the axis of the eye.

2. If the exciting light were a *small* and highly luminous object, so that the whole of its margin could be seen pretty distinctly, then its image would fall upon the *foramen centrale* in the retina, where there is no nervous matter to be excited.

3. When the exciting light, whether large or small, does fall in a distinct image upon the retina round the *foramen centrale*, it acts upon the part of the retina, which being continually exposed to the action of light, is less easily excited, and a part too which can be proved by direct experiment to be less sensible to calorific impressions. Hence we have a distinct reason why an exciting light falling on the central part of the retina does not produce the same insensibility to red light which is produced by the same light acting upon a less used portion of the same membrane.

4. To these reasons we may add a fourth; namely, that the light in this experiment is necessarily much fainter from its being held at a greater distance; though this may be balanced

by an increase of intensity. If at the side of the eye we hold the light within *two* inches of the retina, and in the front of it within *six* inches, then the degrees of illumination are as 4 to 36; so that a light nine times greater should be used in front of the eye to produce, *cæteris paribus*, the same effect.

In repeating Mr. Smith's experiment No. 8, I do not find that the two images are colourless. The green of the one is comparatively faint, but the yellowish red colour of the other is distinctly visible, though it also is less decided than before, owing to the causes which I have explained above. In order, however, to prove that this diminution of effect is not owing to the *distinctness* of vision, let the experiment be made exactly as described by Mr. Smith, and let the eyes be adjusted to vision more remote than the exciting light, every thing else remaining the same; *the tints of the two slips will remain unchanged;* whereas, according to Mr. Smith, the slips ought instantly to appear *red* and *green*, as in the original experiment.

Mr. Smith next proceeds to show in his 9th experiment, that homogeneous coloured light will not excite the eye to see the *red* and *green* colours in question; and that in this case the image in the unexcited eye is *white* instead of red; while that in the excited eye has a colour complementary to the exciting light. This result is exactly conformable to what might have been expected, the phænomena exhibited by the excited eye being those of accidental colours. I have found, however, that the image seen by the unexcited eye is not always white. When the homogeneous light is red, the colour of the image seen by the unexcited eye is fully as red as it is when the exciting light is white. The reason of this is, that as the complementary colour of the homogeneous red is *green*, the natural reddish colour of candle-light is heightened by contrast. When other homogeneous colours are used as exciting lights, the colour of the image in the unexcited eye varies as might be expected, but is always less decided than in the case of red light. It is proper here to observe, that Mr. Smith's experiments were made with *coloured papers*; but such colours are surely far from homogeneous, and therefore it is not safe to deduce inferences respecting a peculiar action of homogeneous light from experiments in which homogeneous light was not employed. Mr. Smith, indeed, mentions that he used the yellow flame of the monochromatic lamp; but even if he succeeded in cutting off the red and blue rays with which that flame is generally accompanied,—it is a colour of so peculiar a character, and of so little brilliancy, that no satisfactory result could be obtained with it.

These observations lead to practical results of some utility.

When the eyes are exposed to strong lights, objects cannot be seen of their true colours, and even lights of ordinary intensity produce a decided deterioration in the tints of a fine picture. Hence it is that we see paintings to most advantage when we view them through two blackened tubes held close to the eye. By this means the colours are not only more brilliant, but faint lights are brought out which would otherwise have been overpowered by the action of lateral light upon the retina. If we turn a picture upside-down, and look at it with the head inverted, a similar effect is produced, because the image is received upon a part of the retina which is not so frequently used; and it is for the same reason that the colours of the sky and of the landscape near the horizon are so beautifully seen by looking at them either between the legs, or beneath the arm with the head inverted.

It is well known that the human complexion is seen to greater advantage in candle- than in day-light, unless the complexions are very ruddy. This arises from there being so much more red in candle- than in day-light. There are certain states, indeed, of the atmosphere, when dark blue clouds prevail, in which the ordinary complexion appears to great disadvantage; and persons in variable health are often described as looking ill, when the change arises from the prevailing colour of the clouds.

When gas-lights were first introduced, it was a common complaint among those who frequented the theatre, that they injured the personal appearance of the audience. This bad quality made them so unpopular, that a red colour was communicated to the light by inclosing it in a reddish-coloured glass. The effect, however, arose from the great quantity of light which was used, and from its influence upon the retina; and if the same intensity of light had been obtained either from oil or from candles, the same effect would have been produced. Our eyes are now so much accustomed to the use of strong lights, that the retina is not so easily rendered insensible to the red rays, and the blue colour of the light is no longer complained of. It is, however, still observed, by those who have been for the first time exposed to gas illumination,—and the eyes of such persons must therefore serve an apprenticeship before they learn to see objects in their true colours.

The blue colour of gas-light was ascribed to the badness of the gas; and the apparent removal of this injurious quality has been attributed to its increased purity, and to improved methods of burning it: but the truth is, that bad gas, or an imperfect combustion of good gas, produces a much redder light than good gas burnt in the best manner.

The smoke which is produced in the former cases invariably reddens the flame, and its perfect removal causes the gas to approximate to the light of the sun, which is always bluer than that of the whitest flames from wax, oil, or tallow.

There is a very pretty experiment illustrative of some of the preceding observations, which is easily made. Place two candles at the distance of *three* or *four* feet from the eye, and about *one* foot from each other, and having closed one eye, fix the other intently upon either of the candles, as if it were examining with attention some point of the wick. The other candle will be seen by indirect vision, and after a little time it becomes much brighter and bluer than the first, in consequence of the part of the retina on which its light falls being more susceptible than the more frequently used portion in the axis of the eye, upon which the light of the second is incident. The higher degree of excitation of the retina produced by the candle seen indirectly, renders that portion of the membrane less sensible to the red rays; and if the excitation is continued, the image will become actually *blue*, and will be surrounded with a halo of *yellow* nebulous light. The blue image, indeed, will sometimes disappear, and leave nothing in its place but a nebulous halo.

Allerly, Jan. 30th, 1833.

XXVII. *On the Law of the Diffusion of Gases. By* Thomas Graham, *Esq. M.A. F.R.S. Ed. Professor of Chemistry in the Andersonian University, Glasgow**.

It is the object of this paper to establish with numerical exactness the following law of the diffusion of gases:

"The diffusion or spontaneous intermixture of two gases in contact, is effected by an interchange in position of indefinitely minute volumes of the gases, which volumes are not necessarily of equal magnitude, being, in the case of each gas, inversely proportional to the square root of the density of that gas."

These replacing volumes of the gases may be named *equivalent volumes of diffusion*, and are as follows: Air, 1; Hydrogen, 3·7947; Carburetted hydrogen, 1·3414; Water-vapour, 1·2649; Nitrogen, 1·0140; Oxygen, 0·9487; Carbonic acid, 0·8091; Chlorine, 0·6325, &c.; numbers which are inversely proportional to the square roots of the densities of

* Read before the Royal Society of Edinburgh, December 19, 1831; and now reprinted from the Edinb. Phil. Trans., with an Appendix.—Communicated by the Author.

these gases, being the reciprocals of the square roots of the densities, the density of air being assumed as unity.

If the two gases are separated at the outset by a screen having apertures of insensible magnitude, the interchange of "equivalent volumes of diffusion" takes place through these apertures, being effected by a force of the highest intensity; and if the gases are of unequal density, there is a consequent accumulation on the side of the heavy gas, and loss on the side of the light gas. In the case of air, for instance, on the one side of the screen, and hydrogen gas on the other, a process of exchanging 1 measure of air for 3·7947 measures of hydrogen, through the apertures, is commenced, and continues till the gases on both sides of the screen are in a state of uniform mixture. Experiments on this principle can be made with ease and precision, as will appear in the sequel, and afford an elegant demonstration of the law.

There is a singular observation of Dœbereiner, which chemists seem to have neglected as wholly inexplicable, on the escape of hydrogen gas by a fissure or crack in glass-receivers, which belongs to this subject, and from which I set out in the inquiry. Having occasion, while engaged in his researches on spongy platinum, to collect large quantities of hydrogen gas, he accidentally made use of a jar which had a slight crack or fissure in it. He was surprised to find that the water of the pneumatic trough rose into this jar one and a half inches in twelve hours, and that, after twenty-four hours, the height of the water was two inches two-thirds above the level of the water-trough. During the experiment neither the height of the barometer, nor the temperature of the place, had sensibly altered.

In other experiments, he substituted glass vessels of very different forms, tubes, bell-jars, flasks, all of which had fissures. In every one of these vessels, filled with hydrogen, the water rose, after some hours, to a certain height. On covering one of these vessels, containing hydrogen, by a receiver—or on filling the vessel with atmospheric air, oxygen or azote, instead of hydrogen—he never observed a change in the original volume of the gas. He thinks it probable that the phænomenon is due to the capillary action of the fissure, and that the hydrogen only is attracted by the fissures, and escapes through them on account of the extreme smallness of its atoms*.

This explanation is rendered improbable by the circumstance, that hydrogen, of all the gases, was condensed and

* *Sur l'Action capillaire des Fissures, &c. Annales de Chimie et de Physique*, tom. xxiv. pp. 332—334. 1823.

absorbed with greatest difficulty, and in smallest quantity, by charcoal and the other porous substances, tried by Saussure. And we have no reason to suppose that the particles of hydrogen are smaller than those of the other gases.

On repeating Dœbereiner's experiment, and varying the circumstances, it appeared that hydrogen never escapes outwards by the fissure, without a certain proportion of air returning inwards. In the experiment, however, as originally performed, it is evident, that, as soon as the water rises in the jar above its outer level, air will begin to be forced into the jar mechanically through the fissure, by the pressure of the atmosphere, independently of what we shall suppose enters by diffusion. But if we press down the jar of hydrogen to a certain depth in the water-trough, so that the level of the water without is kept constantly higher than the level of the water within the jar, then, on the contrary, a portion of the hydrogen will be forced out mechanically, by the pressure to which the gas is subject. In the last circumstances, however, no air can enter by the fissure, and mix with the hydrogen, except by diffusion, or in exchange for hydrogen. Now, in a great number of experiments of this kind, the air which entered by diffusion amounted to between one fifth and one fourth of the hydrogen, which left the receiver at the same time. But when the circumstances were reversed, and the column of water allowed to rise in the jar above the level of the water-trough, the quantity of air which entered by diffusion was increased by a portion which entered mechanically; and varied from a third to a fourth part of the hydrogen, which escaped at the same time. The results, therefore, oscillate, as they should do, about our theoretical number. One volume air should replace 3·7947 volumes hydrogen; or the whole hydrogen, on escaping from the jar, should be replaced by little more than one fourth of its bulk of air, and a very great contraction ensue.

But it is unnecessary to detail experiments made with the jar with the fissure, as with every precaution they were not precise, although at all times compatible with, and indeed illustrative of, the law. Thus a sensible contraction always took place in the bulk of the gaseous contents of the jar when filled with carburetted hydrogen of marshes, or with coal-gas, which, like hydrogen, are lighter than air, and ought therefore to be replaced by less than equal volumes of air. With olefiant gas and carbonic oxide, which approach closely to the density of air, no contraction was perceptible, not attributable to other causes, although the gases as usual wholly escaped. In the case of carbonic acid, which is heavier than air, a slight, but positive, expansion appeared to take place, the experiment being performed over mercury.

But the same fissure or opening never allows the process of diffusion to go on with the same degree of rapidity in two successive experiments, principally, I believe, from its size changing with variations in its condition in regard to humidity. The fissures appear to be extremely minute, for we cannot cause either air or the gas employed to flow through them mechanically, at the same rate as it passes by the agency of diffusion, without the application of considerable pressure. Artificial chinks such as that obtained by pressing together ground glass-plates, or in phials fitted with accurately ground glass-stoppers, allow gas to pass through under the slightest pressure, and do not answer for the experiment.

The effects were made much more striking, in some respects, by the discovery that Wedgewood stoneware tubes, such as are used in furnace experiments, admit, from their porous structure, of being substituted, instead of jars with fissures. When shut at one end, as they are sometimes made, they may be managed like other cylindrical gas-receivers. Those which are unglazed are most suitable; but do not answer the purpose, if either very dry or too damp, being permeable by a gas under the slightest pressure in the one case, and perfectly air-tight in the other*. The following experiment illustrates the force and rapidity with which diffusion proceeds. A stoneware cylinder was entirely filled with hydrogen gas over water, and transferred to the mercurial trough: in forty minutes the mercury rose to a height of $2\frac{1}{2}$ inches in the receiver above the level of the mercury in the trough; half of the hydrogen had escaped, and had been replaced by about a third of its volume of air.

But these modes were superseded by the use of Paris-plaster as the porous intermedium.

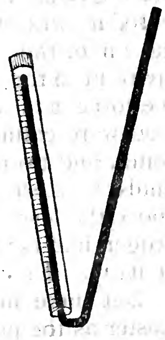
A simple instrument, which I shall call a Diffusion-tube, was constructed as follows. A glass-tube open at both ends was selected, half an inch in diameter, and from six to fourteen inches in length. A cylinder of wood, somewhat less in diameter, was introduced into the tube, so as to occupy the whole of it, with the exception of about one-fifth of an inch at one extremity, which space was filled with a paste of Paris-plaster of the usual consistence for castes. In the course of a few minutes the plaster set, and, withdrawing the wooden cy-

* Various facts demonstrative of the permeability to gaseous matter of substances of this description, had previously been recorded by Mr. Faraday, in his Bakerian Lecture on the Manufacture of Glass for Optical Purposes. Phil. Trans. 1830 p.26.—Edit.

linder, the tube formed a receiver closed with an immoveable plug of stucco. The less water employed in slaking the Paris-plaster, the more dense is the plug, and the more suitable for the purpose. In the wet state the plug is air-tight; if was therefore dried, either by exposure to the air for a day, or by placing the instrument in a temperature of 200° Fahr. for a few hours; and thereafter was permeable by gases, even in the most humid atmosphere, if not positively wetted. The tube was finally graduated by means of mercury into hundredths of a cubic inch, and the notation, as is usual with gas-receivers, counted from the top.

When such a diffusion-tube, six inches in length, was filled with hydrogen over mercury, the diffusion, or exchange of air for hydrogen, instantly commenced, through the minute pores of the stucco, and proceeded with so much force and rapidity, that within three minutes the mercury attained a height in the receiver of upwards of two inches above its level in the trough. Within twenty minutes the whole of the hydrogen had escaped.

In conducting such experiments over water, it was necessary to avoid wetting the plug. With this view, before filling the diffusion-tube with hydrogen, the air was withdrawn by placing the tube upon the short limb of an empty syphon (see figure), which did not reach, but came within half an inch of the plug, and then sinking the instrument in the water-trough, so that the air escaped by the syphon with the exception of a small measure, which was noted. The diffusion-tube was then filled up, either entirely, or to a certain extent, with the gas to be diffused.



The ascent of the water in the tube, when hydrogen is diffused, forms a striking experiment. In a diffusion-tube fourteen inches long, the water rises six or eight inches in as many minutes. The column of water attains in a short time its maximum height, at which, however, it is never long sustained; for as in Dœbereiner's experiment, air is all along entering mechanically through the porous plug in such circumstances, from the pressure of the atmosphere; and after the diffusion is over, the water subsides, in the course of several hours, to the general level. In experiments made with the purpose of determining the proportion between the gas diffused and the return-air, it was therefore necessary to guard against any inequality of pressure, which was managed much more easily when the tube was standing over water than over mercury.

The capacity of a mass of stucco to absorb and condense in its pores the various gases, was made the subject of experiment, as this property might interfere with the results of diffusion. The mass was previously dried at 200° Fahr. It absorbed at the temperature of the atmosphere, which at the time was 78°.

6·5	volumes	ammoniacal gas,
0·75	—	sulphurous acid gas,
0·5	—	cyanogen,
0·45	—	sulphuretted hydrogen,
0·25	—	carbonic acid.

Oxygen, hydrogen, nitrogen, carbonic oxide, olefiant gas, coal-gas were not absorbed in a sensible proportion, even when the temperature was 58°. It is evident, therefore, that the absorbent power which stucco enjoys, as a porous substance, is inconsiderable. Placed in humid air, the same mass of stucco absorbed $1\frac{1}{2}$ per cent. of hygrometric moisture. In setting, 100 parts of the stucco had retained 26 parts water uncombined, which escaped on drying at a moderate temperature, so as to avoid decomposing the hydrated sulphate of lime. It can be shown from this, that the vacuities must have amounted to one third of the volume of the mass.

I shall treat in succession of the escape of the different gases from a diffusion-instrument into air. As the contained gas bears no proportion in quantity to the external air, the gas escapes entirely, and is wholly replaced by air. It is of the utmost importance to determine the proportion between the volume of gas diffused, and the replacing volume of air eventually found in the instrument. We thus obtain the *equivalent diffusion-volume* of the gas, which it will be convenient to state in numbers, with reference to the replacing volume of air as unity. I shall begin with hydrogen gas, although attended with peculiar difficulties, as it introduces in a distinct manner to our notice several circumstances which may slightly modify the results of diffusion.

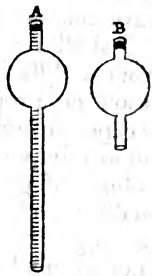
1. *Diffusion-volume of Hydrogen Gas.*

I shall in this paper adopt the specific gravities of the gases generally received in this country. Of hydrogen the specific gravity is 0·0694 (air = 1), of which number the square root is 0·2635. Now, according to our law, 1 volume hydrogen should be replaced by 0·2635 air. But to have the replacing volume of air = 1, $0{\cdot}2635 : 1 :: 1 : 3{\cdot}7947$;

or, $\dfrac{1}{0{\cdot}2635} = 3{\cdot}7947$; that is, 1 air should replace 3·7947 hydrogen. With the specific gravity of hydrogen adopted by Berzelius, namely, 0·06885, the equivalent diffusion-volume of hydrogen is 3·8149.

In a diffusion-tube standing over water, temperature 65°, 88 volumes hydrogen were replaced by 26 air; 84 hydrogen by 25 air; and in another tube, 130 hydrogen by 38 air. The quantity of return-air is here related to the hydrogen diffused, as 1 to 3·38, 3·36, and 3·42, numbers which approach to, but fall short of, the theoretical diffusion-volume of hydrogen, namely, 3·79. But the hydrogen in these experiments was saturated with vapour at 65°, which would make its density 0·0809, and reduce its diffusion-volume to 3·5161; while the air without, being comparatively dry, would be somewhat expanded *after* it entered the diffusion-tube, by the ascent of vapour into it. This would occasion the quantity of return-air to appear greater than it should be; but it is difficult to find elements for a proper correction, as not only the quantity of vapour in the atmosphere must be taken into account, but also the hygrometric state of the plug itself. The increased return-air, however, evidently lowers the diffusion-volume of the hydrogen gas.

With the view of increasing the capacity of the instrument, and the number of its divisions, and of obviating the interference of vapour, the mode of performing the experiment was varied. On a tube, four tenths of an inch in diameter, a bulb of two inches in diameter was blown, as in figures A and B. The tube above and below the bulb, in the case of A, was graduated into two-hundredths of a cubic inch. The upper end of the tube was closed by stucco, as in the case of the simple diffusion-tube. The general mode of proceeding will be best conceived from the recital of the details of a particular experiment.



The diffusion-instrument employed in the following experiment contained 855 measures, and was of the form A. The stucco plug was unusually large, being 0·6 inch in length, which occasioned the diffusion to be slow. At the commencement of the experiment the thermometer stood at 68°, and the barometer 29·73 inches. The bulb being sunk in water with the air-syphon in it, the whole air was withdrawn, with the exception of 12 measures, and the instrument filled up with newly made hydrogen gas. So that at the outset we had in the instrument,

Air with its vapour	12·
Hydrogen	823·83
Vapour (accompanying the hydrogen at 68°)	19·17
	855·00

As soon as it was filled, it was placed in a glass-jar, of about the same height, with a little water left in the bottom, and in proportion as the water rose in the tube of A, from the subsequent contraction, the jar was filled up by repeated additions of water, so as to keep the surface of the water, within and without the tube, as nearly as possible at the same level. With the view of having the external air in a constant state in regard to humidity, means were taken to saturate it. A small cone of damp paper was inverted, like an extinguisher, over the upper part of the instrument; the jar containing the instrument was placed on the shelf of the pneumatic trough, and a bell-jar with an opening at the top, which could be shut at pleasure, inverted over the whole. The return-air must therefore have been in the same state, in regard to humidity, as the hydrogen itself. Aqueous vapour would diffuse neither outwards nor inwards, as it existed in the same proportion on both sides of the plug; but dry hydrogen only would be exchanged for dry air, in the proportion of their equivalent diffusion-volumes.

In the first thirty-four minutes, the gaseous contents of the bulb were diminished by 95 measures, and ultimately, in twenty-six and a half hours, they were reduced to 227 measures, which were common air. The contraction in this and other cases, in which the water rose into the bulb, was determined by weighing, at the end of the experiment, the water which had entered; a mode which admits of even greater nicety than measuring the bulk of residuary gas in a graduated vessel.

With the view of obtaining elements for a correction for any change in the bulk of the gas, which might take place during the continuance of the experiment, from changes in temperature, pressure, or from solution of the gas in water, a receiver was made of the same tube, with a bulb of nearly the same capacity as the diffusion-instrument, but close at the top. This receiver was also nearly filled at the commencement of the experiment with hydrogen gas, and the quantity of gas noted, the tube being graduated.

The hydrogen in this standard receiver contracted $\frac{1}{82}$nd part during the experiment. We have therefore to increase the quantity of air found ultimately in the diffusion-receiver by $\frac{1}{82}$nd part. In this way the residuary air is increased to 229·8 measures, 12 of which, or, more correctly, $11{\cdot}85 (= 12 - \frac{1}{82}(12))$, were present from the beginning.

The temperature was also 68° at the end of the experiment, the same as at the beginning. The ultimate contents of the diffusion-instrument may be stated with sufficient accuracy as follows:—

Air and vapour originally present	11·85
Dry air which has entered	212·84
Vapour in ditto	5·11
	229·80

The conclusion is, that 823·83 measures dry hydrogen have been replaced by 212·84 dry air. Now,

$$\frac{823{\cdot}83}{212{\cdot}84} = 3{\cdot}87 = \text{diffusion-volume of hydrogen.}$$

The diffusion-volume of hydrogen comes out above the theoretical number in this experiment; but an addition of not more than 2 per cent. to the quantity of return-air, would reduce it below the theoretical number. The quantity of vapour which was supported by the hydrogen at the commencement of the experiment was 19·17 measures, but at the end of the experiment we find only 5·11 measures vapour; the difference has condensed, from the loss of a permanently elastic fluid necessary to support it.

As the quantity of hydrogen and of return-air is amplified in the same proportion by vapour, provided the temperature be the same at the beginning and end of the experiment, it is unnecessary to know the absolute quantity of vapour in either case, in determining the diffusion-volume of hydrogen. We may simply divide the gross amount of hydrogen gas diffused, by the gross amount of return-air, the quotient is the diffusion-volume of hydrogen.

Experiment 2.—The thickness of the stucco-plug in the instrument used above, was reduced from six tenths to two tenths of an inch, by cutting away the upper portion. The instrument, of the same capacity as before, was now entirely filled with hydrogen gas. This was effected, by first filling up with hydrogen, leaving a small quantity of air in the upper part of the instrument as in the previous experiment, then withdrawing this impure hydrogen by the air-syphon, and filling up a second or third time with the same gas, whereupon the proportion of air remaining ceased to be appreciable. The apertures of the plug were closed, by pressing the finger upon its upper surface; and in this manner any diffusion of the hydrogen was carefully guarded against, till the process of filling was completed. The diffusion was so rapid in the case of the thin plug, that this additional precaution was absolutely required. Care was taken to have the return-air saturated with moisture in this and every other experiment of the same kind, and inequality of pressure was avoided.

At the beginning of the experiment, the instrument contained 855 measures hydrogen, saturated with vapour at 62°; in three minutes a contraction of 95 measures took place, and in the course of an hour the diffusion was sensibly at an end. The instrument, however, was exposed for two hours longer, that the diffusion might certainly be complete. During intervals so short uniformity of temperature might be counted upon, with certain precautions; and the variations in atmospheric pressure were generally so minute, that they might be neglected with impunity. Corrections for temperature and pressure might therefore be dispensed with, which was a great advantage. 855 measures hydrogen were found eventually to be replaced by 226·5 measures air, both saturated with vapour at 62°.

$\frac{855}{226 \cdot 5} = 3 \cdot 774 =$ diffusion-volume of hydrogen. This determination is somewhat below the theoretical diffusion-volume, 3·79, while the preceding determination was in excess.

Exp. 3.—Another diffusion-instrument of the form B, with a dense plug, one tenth of an inch in thickness, was filled with water, which was then poured into a counterpoised phial, and found to weigh 1085·7 grains. When filled over water, 1085·7 grain-measures of gas are therefore introduced into this instrument, and in this way we express most correctly its capacity. The instrument, after the plug was dried, was entirely filled with hydrogen gas, as in the preceding experiment, thermometer 61°. The bulk of the diffusion appeared to be over in an hour and a half, but five hours were allowed to the experiment. Thereafter the water which had entered the instrument was poured into a counterpoised phial, and found to weigh 800·6 grains. This last quantity represents the contraction, and subtracting it from 1085·7, we have the return-air equal to 285·1 grain measures. Now,

$$\frac{1085 \cdot 7}{285 \cdot 1} = 3 \cdot 808 = \text{diffusion-volume of hydrogen gas.}$$

Exp. 4.—Same bulb, circumstances the same, but thermometer 62°. Time allowed for the diffusion four hours.

1085·7 measures hydrogen were replaced by 286·1 measures air.

$$\frac{1085 \cdot 7}{286 \cdot 1} = 3 \cdot 795 = \text{diffusion-volume of hydrogen.}$$

Exp. 5.— Same bulb, &c. thermometer 61°. Time five hours.

1085·7 measures hydrogen were replaced by 278·4 measures air.

$$\frac{1085\cdot7}{278\cdot4} = 3\cdot900 = \text{diffusion-volume of hydrogen.}$$

Exp. 6.—Same bulb, but in this and the succeeding experiment, the bulb was attached to the end of a balance, and counterpoised, so that it adjusted itself spontaneously in the jar filled with water, in which it floated. Thermometer 60°.

1085·7 measures hydrogen were replaced by 279·1 measures air.

$$\frac{1085\cdot7}{279\cdot1} = 3\cdot890 = \text{diffusion-volume of hydrogen.}$$

Exp. 7.—Same repeated. Thermometer 61°.

1085·7 measures hydrogen were replaced by 282·2 measures air.

$$\frac{1085\cdot7}{282\cdot2} = 3\cdot847 = \text{diffusion-volume of hydrogen.}$$

The results of these five last experiments, with the same instrument are, in one view,

Measures of Return-Air.	Diffusion-volume of Hydrogen.
285·1	3·808
286·1	3·795
278·4	3·900
279·1	3 890
282·2	3·847
Mean 282·2	Mean 3·848

New hydrogen gas was made for each experiment by the moderate action of dilute sulphuric acid on zinc, and it was collected in the diffusion-instrument from the beak of the retort. The observations could not be made with so much accuracy as to entitle us to place any reliance on more than two decimal places of the calculated diffusion-volumes. A great variety of experiments were performed on the diffusion of hydrogen with the diffusion-bulbs employed above, and several others of similar construction, principally with the view of discovering the cause of the slight variations in the results, and why the quantity of return-air was pretty uniformly somewhat less than the theoretical quantity, which has the effect of increasing the proportion of the hydrogen diffusion-volume.

It appears, that when the stucco-plug is in a parched state, the quantity of return-air is uniformly greater than it should be. Thus 3·65 and 3·69 were the diffusion-volumes of hydrogen deduced from an experiment, in the one case with a plug which had been dried at 100°, and subsequently exposed for several hours to the air, and in the other case, with a plug merely dried in air, temperature 68°. The obvious cause of this is, that the air is dried in passing through the plug, and is subsequently expanded while in the diffusion-instrument by the ascent of vapour into it. Hence, the first time a diffusion-bulb is tried, it generally gives the diffusion-volume of hydrogen below the truth.

On the other hand, I apprehend, that when the pores of the stucco are saturated with hygrometric moisture, which, from the circumstances of the experiments, must be almost always the case, the hydrogen, in making its way through the plug, actually avails itself to a small extent of this moisture, inducing it to vaporize, and exchanging places with it instead of air. Hydrogen which escapes in this way will not be represented by return-air, the quantity of which is thus diminished. This process, however, is extremely intricate, and has not yet been fully investigated. Its effect is insensible in the case of the other gases, of which the diffusion-volumes approach more closely to that of air.

The more dense and compact the plaster-plug, the more correct appear to be its general indications. On this account I compress the plug, while moist, before it sets. When the plug is of a loose structure, and probably contains sensible vacuities in its substance, diffusion goes on with increased rapidity; but I have observed, that the proportion of return-air is notably diminished in the case of the diffusion of hydrogen. Thus, in a set of experiments with a diffusion-bulb, having a plug of this description, and little more than one-tenth of an inch in thickness, I obtained, as the diffusion-volume of hydrogen, 4·05, 4·04, and 4·00. This plug had been somewhat thicker at one time, and then gave 3·93 as the diffusion-volume of hydrogen. These experiments exhibit an extreme case of this deviation. It appears to depend upon some physical property of hydrogen gas which is peculiar to it. To obtain light upon this subject, I was led to investigate the rate at which air, hydrogen, and the other gases flow through the stucco-plug into a vacuum, under the influence of mechanical pressure.

A small bell jar, with an opening at top, was used, which opening was closed with a plug of Paris plaster of half-an-inch in thickness, over which a brass cap and stopcock were fitted and cemented. This receiver was placed on the plate of an air-pump in perfect order, and exhausted. When the stop-cock of the receiver was closed, nothing entered the exhausted receiver; but on opening it, either air entered, forcing its way through the pores of the stucco, or any gas which might be conducted to it, by means of a flexible tube from a proper magazine.

The time was noted in which the mercury of the gauge-barometer, in communication with the receiver, fell two inches, always setting out with gas of the tension of one inch mercury in the receiver, and stopping exactly when it attained a tension of three inches.

Air entered, according to eight or ten experiments made on different days, in within ten seconds, more or less, of ten minutes, and so whether the air was saturated with aqueous vapour or dry.

The same volume of different gases entered in the times expressed in the following table, under the same pressure, or beginning at a pressure of 29 inches mercury, and terminating with a pressure of 27 inches:

	Min.	Sec.
Air, dry	10	0
Air, saturated with moisture at 60°	10	0
Carbonic acid	10	0
Nitrogen	10	0
Oxygen	10	0
Carbonic oxide	9	30
Olefiant gas	7	50
Coal gas	7	0
Hydrogen	4	0

In repetitions of the experiments, the numbers oscillated 10, or 12, sometimes 20 seconds, on either side of the numbers given in the table, from circumstances which could not easily be appreciated. As the mercury in the gauge fell not continuously, but by leaps, from adhesion to the glass, the experiments are not susceptible of the greatest accuracy.

The greater the pressure the more rapidly are gases forced through the pores of the plug; but the quantity of gas which penetrates in any given time is not exactly proportional to the pressure, at least in the case of air and hydrogen. By doubling the pressure, we do not quite so much as double the quantity of gas forced through; or a fixed quantity of gas does not enter in half time under double pressure, as will be evident from the following table of observations. Pressure of atmosphere 30 inches.

Height of Gauge Barometer in Inches of Mercury, or Pressure.	Air. Interval of Time in falling one Inch by Gauge.		Hydrogen. Interval of Time in falling one Inch by Gauge.	
	Min.	Sec.	Min.	Sec.
29	0	0	0	0
28	5	0	1	50
27	5	23	2	0
26	5	15	1	55
25	5	30	1	55
24	5	35	2	0
23	5	45	2	2
22	6	0	2	13
21	6	5	2	10
20	6	30	2	35
19	6	35	2	30
18	7	3	2	40
17	7	12	2	50
16	7	35	3	10
15	8	10	3	30
14	8	40	3	35
13	9	10	4	5
12	9	55	4	10
11	11	0	4	15
10	11	40	4	30
9	12	30	5	20
8	14	15	7	40

The ratio of the times, in hydrogen and air, is not greatly different at different pressures. Thus, the mercurial column was depressed 18 inches, or from 29 to 11 inches.

By air, in............ 7283 seconds,
By hydrogen, in... 3025 seconds,

$$\frac{7348}{3025} = 2{\cdot}408 = \text{ratio of hydrogen,}$$
$$1{\cdot} \qquad = \text{rate of air.}$$

It was found that the kind of gas in the receiver made no difference on the velocity with which hydrogen entered under a certain pressure. Hydrogen entered as rapidly against hydrogen in the receiver of a certain tension, as against air of the same tension. Thus,

Barometer Gauge. Height.	Hydrogen entered against Hydrogen, (*From preceding Table.*) Time.		Hydrogen entered against Air. Time.	
Inches.	Min.	Sec.	Min.	Sec.
15	0	0	0	0
14	3	37	3	35
13	3	56	4	5

It is evident from this, that the air does not diffuse out against so strong a pressure and the inward current of hydrogen.

When this jar, of which the capacity was 65 cubic inches, was used as a diffusion-instrument, and filled over water with hydrogen, one fourth of the hydrogen which it contained escaped by diffusion into air in the first hour. Now, we find by the table, (p. 188.), that hydrogen penetrates the plug with greater velocity when passing into a vacuum or into the exhausted receiver. The exhausted receiver was filled one fourth in about fifteen minutes; hence a certain quantity of hydrogen passed through the same porous plug, by the pressure of the atmosphere, into a vacuum in fifteen minutes; by spontaneous diffusion into air in sixty minutes; or the velocity of diffusion was one fourth the velocity of mechanical pressure.

This was a dense and excellent plug; and in others of a looser texture, the velocity of diffusion was much less than a fourth.

Dried bladder answers for showing the diffusion of hydrogen when stretched over the open end of the tube receiver. The diffusion, however, through a single thickness of bladder, is effected at least twenty times more slowly than through a thickness of one inch of stucco. While, on the other hand, either air or hydrogen, under mechanical pressure, passes more readily through bladder than a great thickness of stucco. Goldbeaters' skin is even more permeable by gases under a slight pressure than bladder, and less suitable for diffusion.

The superior aptitude of stucco for exhibiting the unequal diffusion of gases of different densities, seems to depend upon its pores being excessively numerous, but exceedingly minute, making in the aggregate a considerable channel. In the bladder, or goldbeaters' skin, the pores I suppose to be few in number but wide, making, however, when added together, but a small channel. Air passes through them but little impeded by friction.

Dry and sound cork answers exceedingly well as a substitute for the stucco-plug. The diffusion takes place slowly, but is not apt to be deranged by a slight mechanical pressure. So do thin laminæ of many granular minerals, such as the flexible magnesian limestone, &c.; charcoal also, and woods, if not too porous, may be applied to the purpose.

It might occur, in explanation of our experiments with the diffusion-instrument, to take Mr. Dalton's hypothesis, and suppose, in the case of hydrogen, the external air to be a vacuum to the hydrogen, and the hydrogen a vacuum to the air, and that the *inequality* of the diffusion depends upon the hydrogen *being least resisted in passing through the plug*. The experiments on the permeability of the stucco by gases under pressure, above detailed, were projected with a view to settle this point among others; and they are evidently incompatible with such an application of the theory, for hydrogen passes 2·4 times more swiftly, and not 3·8 times, as in the diffusion experiments. Carbonic acid, too, permeates the plug, under pressure, as rapidly as air does, or even somewhat more rapidly, for our results inclined to this side rather than to the other; whereas carbonic acid diffuses through the plug more slowly than air does, or is replaced by more than an equal volume of air, as will presently appear.

Those experiments, previously narrated, are perhaps sufficient to establish the law in regard to hydrogen, particularly when we find it hold in the case of other gases.

As hydrogen is a very light gas, I was anxious to establish the law also in regard to a heavy gas, such as carbonic acid.

[To be continued.]

XXVIII. *Notice of the Occurrence on a Stone Wall of a remarkable Deposition of Ice, similar to that described in the preceding Number of the Philosophical Magazine. By Professor* Rigaud.

To Sir David Brewster.

Dear Sir,

EVERY thing connected with an unusual fact is interesting in itself, and variations may assist in leading to the explanation of it—no further apology seems requisite for offering the following notice to your consideration.

The account in the last Number of the London and Edinburgh Philosophical Magazine, of a remarkable deposition of ice, immediately recalled to my recollection that I had once observed the same phænomenon, though not on vegetables. The description there given answers precisely to what I saw: the engraving, indeed, gives me the idea more of snowy whiteness than of the semi-pellucid icy appearance which occurred in the instance which I witnessed; but this may have arisen from accidental circumstances*.

On looking back to a memorandum which was made at the time, I find that in 1821, between 11 and 12 at noon on the 18th of February, I observed the fact on a stone wall, with an eastern aspect, in a lane of this place. It occurred in many parts, which were from three to six or seven feet from the

* The engraving represents the appearance imperfectly. It was, as Mr. Rigaud describes it, semi-pellucid.—(B.)

ground. The portions of ice (with a single exception) were formed at the edges of the stones,—indifferently at the tops, the bottoms, or the sides, but the curvature was uniformly turned inward from the mortar. There was a single instance of formation on the mortar itself, in which case the threads of ice were formed in an horizontal line, and I think (for of this I made no memorandum) parallel to the layer of mortar.

There was a considerable extent of old wall in the same situation, but the ice was only found on a part, which had been recently built. In looking back to the meteorological journals kept at the Observatory, I find the following entries:

1821. Feb. 17.	12^h	30′	Thermometer out of doors	31°	thick fog.
	10	30	— — —	29	cloudy.
18.	8	30	— — —	34	cloudy.
	12	30	— — —	38	cloudy.

May not, therefore, the new mortar be supposed to have supplied the moisture which was congealed by the radiation from the sharp edges of the new stones? and as water expands in freezing, though ice contracts after it is frozen, may not the curling form have been produced by the greater cold retained in the stone from the frost of the preceding night?

I remain, dear Sir, yours truly,

Oxford, Feb. 2, 1833. S. P. RIGAUD.

XXIX. *On the Effect of Aberration in prismatic Interference. By* WILLIAM R. HAMILTON, *Esq. Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland*.*

THE experiments and reasonings of Mr. Potter respecting the phænomena of prismatic interference, published in the last Number of the London and Edinburgh Philosophical Magazine (for February 1833), deserve attention; for, if correct, they would furnish a formidable and, perhaps, fatal objection against the undulatory theory of light. I have not repeated the experiments, but I have endeavoured to examine the mathematical part of the question, and have obtained results which differ from the mathematical results of Mr. Potter, and which appear to show that the phænomena described by him are consistent with the undulatory theory. It may, therefore, be useful to state briefly some of my results, in a form adapted for comparison with those of which they profess to be corrections. In stating them, it cannot be supposed that I intend any personal attack on Mr. Potter, for whose talents and industry I feel a sincere respect.

* Communicated by the Author.

Mr. Potter believes it to be a mathematical consequence of the undulatory theory of light, that when rays, in a plane perpendicular to the edge of a prism of glass, diverge from a luminous point *in vacuo*, and emerge from the prism after refraction, into a vacuum again, the locus of the points simultaneously attained by the emergent light is a circle;—either rigorously, or at least with an accuracy sufficient for the investigation of the positions of the central points of interference of two emergent streams of homogeneous light, which had set out together from two near luminous origins, namely, from the images of a luminous point formed by two plane mirrors inclined at a small angle to each other:—from which he concludes that these central points of interference, in the given plane perpendicular to the edge, are situated on a certain hyperbola, tending *towards the angle of the prism*, whereas he found by experiment a tendency *from that angle*. I find, however, that in consequence of the *prismatic aberration* (which is greater than the aberration of a lens), the section of an emergent wave differs sensibly from the circular form, and the time of arrival of the light at any proposed point of interference requires a sensible correction; by allowing for which I find, as the locus of the points of central interference in the plane perpendicular to the edge, a curve not hyperbolic, and *not tending* towards *but* from *the angle of the prism:* so that the phænomenon observed by Mr. Potter is a consequence of the undulatory theory.

To simplify the question I shall suppose, with him, that the line joining the two near luminous origins is perpendicularly bisected by a line which, if considered as an incident ray, would undergo the minimum of deviation, and would emerge in a certain direction, which I shall take, as he does, for the axis of x; supposing also, with him, that this emergent line passes through, or very near the edge, and measuring the positive ordinates y towards the thickness of the prism, while the positive abscissæ x are measured from the incident towards the emergent light. The problem is then to find, at least approximately, the equation in x, y, of the locus of points of central interference, or of simultaneous arrival of the light from the two luminous origins, with the undulatory law of velocity; and, in particular, to examine whether this locus tends *to* or *from* the angle of the prism, by examining whether the ordinate y *decreases* or *increases*, while the abscissa x increases from its value at the prism.

Denoting, as Mr. Potter does, the coordinates of the prismatic focus or image corresponding to one luminous origin, by the values $x = m\,a,\ y = a,$

and those of the prismatic image of the other luminous origin by

$$x = 0, \quad y = -a,$$

in which a is half the interval between the two near luminous origins, and m is a positive number depending on the angle and index of the prism. Mr. Potter finds for the difference of times of arrival of the two streams of emergent light at any point x, y, not far from the axis of x, the expression

$$\sqrt{x^2 + (y+a)^2} - \sqrt{(x-ma)^2 + (y-a)^2} - ma \ldots (1)$$

and equating this expression to zero, he finds for the locus of the points of central interference, the equation of a common hyperbola, which may be put under the following approximate form,

$$y = \frac{m a^2}{4 x} \ldots\ldots\ldots\ldots\ldots (2)$$

If then this analysis were sufficient, it would show, as Mr. Potter has concluded, that y *decreases*, and that the locus tends *towards the angle* of the prism; whereas the experiment showed a contrary tendency.

But I find, that on account of the prismatic aberration, the expression (1) for the difference of times of arrival, requires this correction, namely,

$$\frac{m l}{4} \left(\frac{y+a}{x}\right)^3 - \frac{m l}{4} \left(\frac{y-a}{x}\right)^3 \ldots\ldots (3)$$

in which l is a positive quantity, namely, the length of the path traversed by the light in arriving at the edge of the prism; and after allowing for this correction (3), the equation of the sought locus, of the points of central interference, gives the following approximate expression for the ordinate y,

$$y = \frac{m a^2}{4 x} - \frac{m a^2 l}{4 x^2} \ldots\ldots\ldots\ldots (4)$$

the second term being introduced by aberration, but being of the same order as the first. And taking account of this new term in the expression of the ordinate, we have, by differentiation,

$$\frac{dy}{dx} = -\frac{m a^2}{4 x} + \frac{m a^2 l}{2 x^3} = \frac{m a^2 (2l - x)}{4 x^3} \ldots\ldots (5)$$

so that while x increases from its value l at the prism to the value $2l$, the *ordinate* y *increases* from 0 to $\frac{ma^2}{16.l}$, and the *curve tends towards the thickness of the prism*, as it was found in the experiment to do. Indeed, when x increases still further, that is, when the eye is withdrawn from the prism to a distance greater than the length of the incident path, that is, greater than the distance of the prism from the two near luminous origins, the curve begins to tend the other way, though much more slowly; but the experiments of Mr. Potter do not seem to have been made at so great a distance from the prism, and therefore the phænomenon, which he observed, appears to be explained by the undulatory theory.

Dublin Observatory, Feb. 12, 1833.

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

In a Catalogue of Comets, more is required than a compilation of those only of which modern industry has calculated the elements. In proportion as the periodic times of a greater number are ascertained, is it requisite to search in the pages of history for intimations of their former appearance; but this is an undertaking of no ordinary difficulty, as regards both its nature and extent. To sift the details of the journalist of a convent, or the annalist of a kingdom, or the meagre narrative which is dignified with the name of history, is a task sufficiently irksome, and too often fruitless;—when no acuteness of discrimination can positively decide between the notice of a meteor and the record of a comet; can reduce the exaggerations of superstitious terror to their true proportions, and separate the visions of the mystic, or the fictions of the astrologer, from the ill-understood and more imperfectly registered phænomena of nature. So far as it is possible this has been executed by Pingré; and if he has added nothing to the theory of comets, his history of them is an unrivalled monument of industry, fidelity and judgement. This opinion is the result of a close and extensive examination and verification of his work, which has served as the foundation of the present Catalogue.

The best Catalogue of Comets of which the elements have been computed, is probably that contained in the third volume of Delambre's Astronomy†: this has been extended from various sources, which must be familiar to every scientific reader, and brought up to the present year; thus, it is to be hoped, supplying a desideratum in Astronomy.

* Communicated by the Author.

† Olbers's *Abhandlung*, published at Weimar in 1797, contained a short Catalogue of 89 Comets which had been observed previously to that year.

Part I.—The comets of which the appearance prior to the Christian æra rests upon sufficient authority.

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

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

Number.	Year and Appearance B. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
1	1770±	A ?	......	....	Varro.	
2	1194±	A ?	......	....	....	Freret has established the appearance of this comet by a comparison of the ancient poets, &c.
3	975 ±	B ?	......	....	Pliny.	
4	618 or 619	A ?	......	....	Sibyll. Orac.	
5	612	...	August.........	Seven stars of Ursa Maj.	Chinese Records.	
6	533± ?	...	Winter solst.	West. hand of ♒ and tail of ♑	Chinese Records.	
7	531	..	Winter.........	Aquarius.....	Chinese Records.	
8	524	...	Winter........	Scorpio	Chinese Records.	
9	481	...	End of the year	Scorpio......	Chinese Records.	
10	479	...	October	......	Pliny.	Seen during 75 days according to a quotation in Plutarch.
11	465±	...	......	......	Chin. Rec., Pliny, Diog. Laert., &c.	
12	432	...	......	......	Chinese Records.	
13	426 or 402	...	Winter solst.	Arctic Pole..	Aristotle.	
14	371	...	Winter........	Cancer......	Diod. Sic., Seneca, Aristotle, Pliny, &c.	Motion retrograde. Inclination of the orbit 30° +. Descending node in Cancer or Leo. Place of the perihelion Libra or Virgo. *Pingré.*
15	370	...	.....	Canis	Aristotle.	
16	360	...	......	....	Chinese Records.	
17	345	...	......	....	Pliny.	
18	344	...	......	In the North.	Diod. Sic., Plut.	
19	340	...	......	Near the Equ.	Aristotle.	
20	304	...	......	....	Chinese Records	
21	302	...	......	....	Chinese Records.	
22	295	...	......	....	Chinese Records.	
23	239	...	June or July	Northerly ...	Chinese Records.	
24	237	...	April, May	Sagittarius ..	Chinese Records.	
25	233	...	February	....	Chinese Records.	
26	213	...	......	....	Chinese Records.	
27	203	...	August.........	Near Arcturus.	Chin. Rec., Julius Obsequens.	
28	202	C ?	......	....	Julius Obsequens.	
29	171	...	......	....	Chinese Records.	
30	168	...	......	....	Julius Obsequens.	
31	166	...	......	....	Julius Obsequens.	
32	165	...	......	....	Julius Obsequens.	

Number.	Year and Appearance B. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
33	156	...	October	Shoulders of Aquarius, Equuleus, neck of Pegasus....	Chinese Records.	
34	154 or 153	...	Jan., Sept. 154, Feb. Mar. 153	....	Chinese Records.	
35	148	...	April (Nov. 147?)	Head of Orion.	Chinese Records.	
36	147 ?	...	......	....	Chinese Records.	
37	146 ?	...	......	....	Jul. Obsequens, Seneca.	
38	137	...	March	Hydra	Chinese Records.	
39	...	...	June..........	Hercules	Chinese Records.	
40	... ?	...	Autumn	....	Chinese Records.	
41	136 ?	...	......	....	Julius Obsequens.	
42	134	...	July, Aug. Sept.	....	Justin. Chin. Rec.	Seen during 72 days; passed its perihelion about the middle of August.
43	127	C?	......	....	Julius Obsequens.	
44	120 or 119	...	Spring	....	Chinese Records.	
45	118	...	May	....	Ch. Rec., Just. &c.	
46	109	...	May	Gemini.......	Chinese Records.	
47	...	...	May	Ursa Major ..	Chinese Records.	
48	108	...	......	Between Procyon and Castor and Pollux	Chinese Records.	
49	102 ±	...	......	Near γ Bootis.	Chinese Records.	
50	99	...	......	....	Jul. Obs., Pliny.	
51	93	...	......	....	Julius Obsequens.	
52	91	...	......	....	Julius Obsequens.	
53	86	...	Autumn	....	Pliny, Chin. Rec.	
54	84 or 83	...	Spring	To the N.W.	Chinese Records.	
55	75	...	......	....	Pliny.	
56	68	...	July and Aug.	Head of Herc.	Chinese Records.	
57	62	...	......	....	Dio. Cass. Jul. Ob. Chin. Records.	
58	55	...	......	....	Dio. Cassius.	
59	52	D?	......	....	Dio. Cassius.	
60	48	...	......	Cassiopeia ...	Pliny, Chin. Rec.	
61	...	A?	September	Leo., Virgo ..	Pliny, Sueton., Seneca, &c.	
62	43	...	May, June	Orion.	Chinese Records.	
63	42	...	......	....	Pliny, Manilius.	
64	41	...	......	....	Pliny, Manilius.	
65	31	...	February	Pegasus	Dio. Cass. Chi. Re.	
66	29	...	......	....	Dio. Cassius.	
67	11	E?	August	Lower part of Gemini.	Dio. Cass. Seneca, Chin. Rec.	
68	4	B?	Vernal Equin.	Head of Capri.	Chinese Records.	
69	3	...	April or May	Aquila........	Chinese Records.	

Hayes, Kent, January 1833.

[To be continued.]

XXXI. *Analysis of some Combinations of Platina.* By R. J. Kane, *M.R.I.A. Professor of Chemistry to Apothecaries' Hall, Dublin*: with Observations by* R. Phillips, *F.R.S. &c.*

MR. KANE prepared iodide of platina by adding a solution of hydriodate of potash to a dilute solution of permuriate of platina, the latter being in excess; a black precipitate was formed, which consisted of iodide of platina mixed with the double chloride of potassium and platina; the latter was dissolved by putting the precipitate into a large quantity of water, and keeping it at a temperature of 200°. The insoluble residue was iodide of platina, which when cautiously dried had the following properties: its colour was dull black, rather heavy, insoluble in warm water, but by long boiling in water, traces of iodine were perceptible, arising from the decomposition of a minute portion of the iodide. Alcohol and æther did not appear to act upon it; when heated to 250° it began to give out iodine copiously, and below a red heat it was totally decomposed, leaving metallic platina. Neither sulphuric, nitric, nor muriatic acid acted upon it when cold; but a mixture of the two latter dissolved it, permuriate of platina being formed, and iodine expelled. Solution of potash dissolved the iodide of platina, the colour of the solution was yellow; when saturated with nitric acid it became of a claret colour, more acid rendered it colourless, and by alcohol it was resolved into a mixture of metallic platina and iodine.

A solution of hydriodic acid dissolved the iodide of platina, the solution was red: it was dissolved also by a solution of hydriodate of potash, and its colour was deep claret; when the iodide was put into a solution of ammonia, it became first greenish, then brown, and finally of a clear Indian red; the supernatant liquor was yellow, it contained excess of ammonia, and by evaporation deposited minute red crystals.

To determine the composition of the iodide it was decomposed by heat; 100 grains left 35 of platina, and consequently 65 of iodine were dissipated: on repeating the experiment there was but a slight variation in the result. Now a compound of 3 atoms of iodine (126×3) 378, and 2 atoms of platina (96×2), $192 = 470$, would give 66·3 of iodine, and 33·7 of platina per cent. It appears, therefore, that this substance is a sesqui-iodide, composed of

$1\frac{1}{2}$	equivalent of iodine $(126+63)$	189 or	66·3
1	—— platina	96	33·7
		285	100·

* From the Dublin Journal of Medical and Chemical Science, for July 1832.

Mr. Kane prepared the iodide of potassium and platina, by adding an excess of the former in fine powder to a strong solution of permuriate of platina; effervescence took place. Some æther was immediately poured on the mass, and the whole agitated for a few minutes; a black powder was formed, which when separated by the filter was the double iodide, mixed with some chloride of potassium. This double salt, when pure, is very soluble in water, the solution is of a magnificent claret colour; it is not decomposed by evaporation, but yields a soft crystalline mass; the form of the crystal could not be determined. It is soluble in alcohol, but not in æther, and when it is added to a strong aqueous solution, the æther precipitates the salt in the state of a black powder; solution of potash dissolves it.

This double iodide of potassium and platina was thus analysed:—Twenty grains were heated until the iodine was expelled from the iodide of platina, and there remained metallic platina mixed with iodide of potassium; the latter was dissolved in water; the solution by evaporation left 7·75 grains of iodide of potassium, and the platina weighed 4·75 grains, consequently 7·5 grains of iodine were expelled by heat. It appears therefore to be composed of

Iodide of potassium	7·75
——— platina	12·25
	20·

Mr. Kane observes, that "7·5 iodine to 4·75 platinum is very nearly in the ratio of 1½ atom of iodine to 1 atom of platinum,—thus proving the accuracy of the previous analyses;" and he regards the true composition of the double salt to be 1 atom of each iodide. The results above mentioned certainly prove the *inaccuracy* of the previous or of the present analysis; for 7·5 iodine + 4·75 platina form a compound exactly intermediate between a sesqui-iodide, and a prot-iodide.

The next compound which Mr. Kane formed, he terms Iodo-platinate of hydrogen; it was formed by adding an iodide of platina to a strong solution of hydriodic acid. The solution had a fine claret colour; by cautious evaporation small grains were obtained which were soluble in water, and the solution was red. From the facility with which it was decomposed, Mr. Kane could not ascertain its composition.

The last compound is called Iodo-platinate of ammonium. It was procured by adding solution of ammonia to that of the above-named iodo-platinate of hydrogen. The ammonia caused a black precipitate, which in a few minutes passed through various shades of brown, and finally became a fine clear Indian red. The solution is stated to contain much ioduret of ammonium:—this iodo-platinate of ammonium is stated to consist of

5 atoms of [*sesqui*] iodide of platinum (285 × 5). .	1425
1 atom of ioduret of ammonium	144
	1569

Observations on the above Compounds.—In the Dublin Journal for January last, Mr. Kane has published a notice of a paper by M. Lassaigne, announcing that he also had prepared and analysed two iodides of platina; and Mr. Kane expresses his anxiety to secure what he considers to be his prior claim to the discovery of this compound. M. Lassaigne's paper is contained in the *Ann. de Chim. et de Phys.* for October last, it has not however long appeared. Supposing that the compounds obtained by Mr. Kane and M. Lassaigne were similar (which they are not), the priority unquestionably belongs to M. Lassaigne: his paper in the *Ann. de Chim. et de Phys.* just alluded to, begins thus: "Les combinaisons du platine avec l'iode n'avaient pas encore été obtenues ni étudiées, lorsque j'annoncai en 1829, dans le numéro de Juillet du *Journal de Chimie médicale et de Pharmacie*, qu'on pouvait préparer un iodure de platine à proportions définies en faisant agir la solution d'iodure de potassium sur celle de bi-chlorure de platine." M. Lassaigne then states, that the iodide of platina, which he had formed, appeared to consist of 4 atoms of iodine and 1 atom of platina, and that he declared his intention of trying to procure an iodide containing less iodine. In this, as I shall presently show, he has since succeeded.

It is evident that Mr. Kane never saw the *Journal de Chimie médicale* for 1829; for if he had, he could not have made the following statement, headed PRIORITY OF DISCOVERY OF THE IODIDE OF PLATINUM:—"I would direct the attention of my readers to a paper, published in this Journal in July, 1832, on the iodide of platinum and its saline combinations, in which I described that substance at length, developed the history of the compounds it forms with the iodides of the basic [basic?] metals, and enumerated all the important facts in its history. It is a source of the highest gratification to me, that so eminent a chemist as Lassaigne has followed the same train of research, and fully established the accuracy of my investigations by their close coincidence with his results." Mr. Kane adds, "there is but one point on which we differ:"—now the following comparative statement will show that there is no one point on which they agree.

The compounds analysed by Mr. Kane are,

Sesqui-iodide of platina, composed of

$1\frac{1}{2}$ atom of iodine (126+63)	189	or	66·3
1 ——— platina	96		33·7
	285		100·

Double iodide of platina and potassium, composed of

Sesqui-iodide of platina	61·25
Iodide of potassium	38·75
	* 100·

Iodo-platinate of ammonium, consisting of

5 atoms of sesqui-iodide of platina (285 × 5). .	1425	or	90·83
1 atom of ioduret of ammonium	144		9·17
	1569		100·

The following are the results of M. Lassaigne's analyses; in stating which I have accommodated the atomic weights to those above given.

Prot-iodide of Platina.

1 atom of iodine	126	or	56·76
1 ——— platina	96		43·24
	222		100·

Bi-iodide of Platina.

2 atoms of iodine	252	or	72·42
1 atom of platina	96		27·58
	348		100·

Iodide of Platina and Potassium.

1 atom of bi-iodide of platina	348	or	67·71
1 ——— iodide of potassium	166		32·29
	514		100·

Hydriodate of Ammonia and Platina.

2 atoms of bi-iodide of platina	696	or	82·86
1 atom of hydriodate of ammonia . .	144		17·14
	840		100·

Hydriodate of Bi-iodide of Platina.

1 atom of bi-iodide of platina	348	or	73·27
1 ——— hydriodic acid	127		26·73
	475		100·

In a future Number I shall give further extracts from M. Lassaigne's paper; at present I would only ask Mr. Kane to

* This is according to Mr. Kane's analysis; but if the compound were really what he states it to be, it would consist of

Sesqui-iodide of platina......	63·2
Iodide of potassium..........	36·8
	100·

point out the similarity between these results and his. M. Lassaigne's iodides of platina are two,—the prot-iodide and the bi-iodide; Mr. Kane's only iodide is a sesqui-iodide; M. Lassaigne's iodide of platina and potassium contains 1 atom of bi-iodide of platina, and 1 atom of iodide of potassium; Mr. Kane's (of questionable accuracy), 1 atom of sesqui-iodide of platina, and 1 of iodide of potassium. M. Lassaigne mentions an hydriodate of ammonia and platina, consisting of 2 atoms of bi-iodide of platina and 1 of hydriodate of ammonia. Mr. Kane has what he calls an iodo-platinate of ammonium, containing 5 atoms of sesqui-iodide of platina, and 1 atom of ioduret of ammonium.

Mr. Kane seems to claim great credit for the more philosophical manner in which he views the nature of the iodides of platina than M. Lassaigne does. M. Lassaigne "examined the compounds of iodide of platinum with iodide of potassium, &c. as double iodides; whilst I investigated them as iodine salts, in which the iodide of platinum is the electro-negative (acid) element," &c. &c. I confess I wish Mr. Kane would return to the simpler views entertained by M. Lassaigne; for I am afraid that at the rate at which innovation in nomenclature is proceeding, every month will produce a new language; for if when two iodides combine, one must be an acid and the other a base, I do not see why any compounds whatever may not be at the same time acids, alkalies, and salts. I had intended to make some further remarks on Mr. Kane's nomenclature, but these I shall postpone. In concluding I would observe, that in line 7, p. 310, vol. i. of the Dublin Journal, in Mr. Kane's paper, iodide of potassium is printed instead of iodide of platinum; and in p. 311, line 6 from the bottom, the sentence "its formula $(I\frac{1}{2}\,I + Pl.) + (I + Pl)$, and its atomic weight $= 471$," should be $(I\frac{1}{2}\,I + Pl) + (I + K)$, and its atomic weight $= 451$; for Mr. Kane has just before mentioned the composition to be a compound of 1 atom of sesqui-iodide of platina, and 1 of iodide of potassium.

XXXII. *On the Theory of Magnetic Electricity.* *By* Mr. Wm. Sturgeon, *Member of the British Association for the Promotion of Science; Lecturer at the Hon. East India Company's Military Academy, Addiscombe, &c. &c.*

[Continued from p. 37.]

THE theory of electric excitation by magnetic agency will be embraced in the following Positions:—

Position 1.—Magnetic electricity may be excited in all the metals, and perhaps in some other conductors of electricity.

Position 2.—The excitation depends upon a disturbance of the equilibrium of the electric fluid natural to the metal; by its impinging on the exciting *polar magnetic lines*; and is accomplished by mechanical motion, either of the metallic body to be excited, or of the magnet,—or of both at the same time. For simplification, however, we will suppose the magnet to be stationary, and the metallic body alone to be put into motion.

Remark.—As the electric fluid by this process has not as yet been recognised in any other state than that of motion, the phænomena are necessarily displayed upon the principles of *electro-dynamics*. Hence the term "*excitation*" in this place is to be considered not only expressive of a process for simply disturbing the electric fluid, but as one which is capable of communicating to various quantities of it an infinite variety of velocities. And as the *quantity* of fluid in motion, and the *velocity* with which it moves, will, conjointly, constitute an *electro-momentum*, which at all times will be proportional to the product of its constituent elements; it is therefore the production of the *electro-momentum* which is to be understood, when we speak of various degrees of excitation.

Indeed, whatever may be the nature of the exciting agent, or the mode of its application, it is in this sense only that the term *excitation* can, with any degree of propriety, be applied when electric currents and their effects are the phænomena under contemplation. *Electro-momentum* is an expression which at once conveys to the mind the author's meaning,—that it is the production of the *velocity* multiplied into the *quantity* of electric matter which, by the process, whatever may be its character, is impelled into motion from its previous statical repose.

Electric currents generated by a voltaic battery are constituted of distinct alternate charges and discharges of the electric matter, or of *electro-pulsations*; and may be assimilated to the currents of blood through the animal system, which are produced by the alternate charges and discharges at the heart. And it is very far from being improbable that both are actuated upon the same principle. The electric fluid called forth by a voltaic battery is, therefore, alternately accumulating and discharging during the whole time the instrument is in action. In the former case the *intensity* is exalting; but it is in the latter alone that the force is exhibited; which force is the production of the *quantity* of fluid discharged, and the *velocity* with which it moves conjointly; which may very clearly be understood by the term *electro-momentum*.

As, however, the *electro-pulsations* in most cases are produced too rapidly to be separately considered, it is the *aggregate* of the multitudinous *electro-pulsations* constituting the general discharge, that is to be understood by the term *electro-momentum* when a voltaic battery is the instrument employed for generating the electric currents.

Thermo-electric currents are also, in some cases, of a pulsatory character; for, as several of the metals are constituted of crystals, and those crystals of distinct elementary metallic films (see my paper on the *Thermo-magnetism* of simple metals, Phil. Mag. and Annals, vol. x.), the heat, which in this case is the impelling agent, must necessarily arrive at a certain degree of *concentration*, or of *intensity*, if you please, in *one* film, or distinct *metallic element*, before it can possibly take possession of the next. Consequently, however small and inappreciable may be the interruption in each stage of its progress, each interruption must necessarily produce a virtual *pause*; the very existence of which in the advances of heat from film to film will constitute a pulsatory progression.

In the Marechausian* (*colonne pendule*), or dry electrical column, the electro-pulsations are, in consequence of the very great number of interrupting papers, less frequent than in either the process of Volta, or in that of Seebeck. Notwithstanding which, the instrument produces slow pulsatory currents.

The favourite term *intensity*, so frequently *pressed* into the service of some writers, appears to have no definite meaning in the vague manner which it is generally employed. It occurs, *sine discriminatione*, in electro-statics and electro-dynamics as if no real difference existed in the two distinct conditions of the electric matter; and as it is very far from being expressive of either of them, it can never be intelligibly employed in that double capacity.

When first introduced as a technical term in electricity, it appears that *intensity* was intended to express *degree* of an electro-statical charge; and it has never yet been employed to denote distinctly an electric *force* constituted of *quantity* and *velocity*. *Intensity*, therefore, cannot be considered as synonymous with *momentum*, which admits not of being warped into electro-statics, nor of being dispensed with in electro-dynamics.

The term *induction* is in precisely the same predicament as that of intensity, and may very justly be considered as a fellow slave, variously, and often unintelligibly, employed.

Position 3.—When the metallic body moves in any given

* M. Marechaux appears to have constructed the first dry electric column.—*Ann. de Chim.* for January 1806.

direction with regard to the *polar magnetic lines*, the more *rapid* the motion the greater will be the degree of excitation, or *electro-momentum* produced; and *vice versâ*, the slower the motion the less will be the degree of excitation. Consequently, when the velocity is at a minimum or nothing, no excitation whatever can exist.

Illustration.—If the excited body be of such dimensions as to have the whole of its natural electric fluid put into motion by the process, the *electro-momentum* would always be proportional to the *velocity*, because of the *quantity* of fluid in motion being constantly the same: and as by Position 2. the motion of the fluid depends upon the motion of the excited metal, the velocity of the former will at all times depend upon that of the latter; and consequently the *electro-momentum* or extent of excitation will be proportional to the velocity of the moving body under excitation.

There may possibly, however, be a limit to the extent of excitation by an increase of motion, when the velocity is very great, in consequence of the yielding of the exciting *magnetic lines* to the force of the moving body, or to its electric fluid whilst striking them with great rapidity. But as far as my experiments and observations have been conducted, I am led to believe that the *electro-momentum* may be exalted by an increase of motion until the velocity becomes exceedingly great.

Position 4.—When the velocity of the moving body, and the energy of the exciting *polar magnetic lines* are constant, the *maximum* of excitation will be accomplished by the body moving at *right angles* to those lines against which it impinges.

Position 5.—When the direction in which the body moves is inclined to the axis of the exciting *polar magnetic lines* at any other angle than 90°, it receives no more excitation than what is due from the quantity of its motion taken in the direction perpendicular to that axis.

Illustration.—As the excitation of the body, or of the electric fluid which it contains, depends upon its collision with the *polar magnetic lines*; the greater the number of those lines against which the body strikes in a given time, the greater will be the number of exciting impressions accomplished in that time.

Let *a b*, and *a c*, (fig. 6.) be two directions in which a piece of metal is caused to move; the former perpendicular, and the latter oblique to the axis of the group of *polar magnetic lines*, represented by the vertical lines dashed across their heads in the figure. If now $ab = ac$ represent the velocity in each direction, then those two lines will also represent the spaces through which the body moves in two equal portions of time. Now it is evident, by mere inspection of the figure, that whilst the body moves from *a* to *b*, in the direction *perpendicular* to the axis or general direction of the *polar magnetic lines*, it will have to impinge against a greater number of those exciting lines than whilst moving in the oblique direction from *a* to *c*. Or, the body will impinge on no greater a number of *polar magnetic lines* whilst passing obliquely from *a* to *c*, with the velocity *a c*, than it would strike by moving with the less velocity $a\,d = f\,e$, the quantity of its motion taken in the perpendicular direction *a b*.

But, as the velocity is supposed to be constant in both directions, then the same number of exciting impressions will be accomplished by the body being in motion during a *part*, *a d*, only of the time, *a b*, in the perpendicular direction *a b*, as will be accomplished by its being kept in motion the *whole* of the time $a\,c = a\,b$, in the oblique direction *a c*.

Corollary.—Hence it is evident, that if a metallic body were to move in the direction of the axis of a group of parallel *polar magnetic lines*, it would suffer no excitation whatever. The position is also conformable to experiment.

Position 6.—The natural or primitive *channel* of an electric current generated by magnetic agency is at *right angles* to the axis of the exciting *polar magnetic lines*, whatever may be the direction in which the excited body moves.

Remarks.—The current may, however, be led or conducted in various other directions, according to the figure and dimensions of the metal employed, and the various directions in which it may be put into motion; notwithstandiug which, the *primitive* channel of the current will be constantly the same,—at *right angles* to the axis of the exciting *polar magnetic lines*.

Position 7.—The *direction* in which the current *flows* with regard to the exciting *polar magnetic lines*, is constantly the same, whatever may be the direction in which the metal is put into motion, or to whatever extremity or other part of a magnet the metal may be applied.

Illustration.—Let *a*, *b*, *c*, *d* (fig. 7.) be a ring of metallic wire, placed with its plane horizontal, and embracing a bundle or group of *polar magnetic lines*, the axis of which passes through the centre of, and at right angles to, the plane of the ring.

Let those *magnetic lines* emanate from the *south* magnetic pole of a bar of steel placed beneath the paper on which the figure is drawn. Consequently, their *south* poles (marked poles) will be upwards, and may very conveniently be represented by the group of small crosses embraced by the ring. (Fig. 8. is an oblique view of fig. 7.)

If now the ring be put into motion in its own plane, it will be a matter of no consequence which side advances towards the centre; the electric current thus excited will *flow* in every part of the ring in *one* and the *same* direction; which direction is indicated by the four exterior arrows, fig. 7.

Now, as the group of *polar magnetic lines* is stationary, and encompassed by the ring, it will be *that part* only of the ring, which *advances* towards the centre or axis of the group, which will receive the exciting impressions. The opposite side, instead of impinging on the *polar magnetic lines*, absolutely recedes from them, and operates in no other capacity than that of conductor to the excited current in the advancing side. So that whether it be *a*, *b*, *c*, or *d* which advances towards the centre, their opposite sides *c*, *d*, *a*, or *b* will respectively recede from the axis of the group, and become conducting parts of the ring, whilst the former correlative parts are receiving the exciting impressions.

Fig. 9. represents the ring cut open in four places, and each part made perfectly straight to represent four separate pieces of wire.

Let any one of these wires advance towards the centre of the group of *polar magnetic lines*. Then as the excitation in this case is under precisely the same circumstances as in the former, the electric current in the advancing wire, or part of the ring, is also constant and uniform in its primitive direction, flowing in one and the same invariable course, relatively to the exciting *polar magnetic lines* which gave it birth and activity. (See the arrows in fig. 9.)

To familiarize still further this beautiful law of magnetic electricity: Let any man suppose himself to be placed in the axis of a group of *polar magnetic lines*, similarly situated to those in fig. 7. and 8. Let him now stand or suppose himself to be standing in the centre of a hoop or ring of metal.

Whilst in this position, let him permit the ring to move in its own plane. Consequently, some part of it will advance towards him, whilst the opposite part will recede from him. The former will receive the exciting impressions, and the latter will become a portion of the conducting circuit.

Let him now look to whatever side of the ring he pleases, the current *before* him will be flowing from his right to his left hand.

If it be the excitation of a straight wire which he is contemplating, let him consider it as a portion of the original ring, or as one of the straight pieces in fig. 9, permitting it to advance towards his front; his *left hand* will be the unerring index to point out the direction of the passing electric current.

A walking-stick, or any other such article, may very well represent the metal to be excited; then a person standing in the position of the *polar magnetic lines*, as represented in fig. 7, 8, and 9, and holding the stick before him, by its extremities, one in each hand, and at *right angles* to the axis of his person, or to a straight line drawn from his head to his feet, will, by pulling the stick towards him, show the proper direction of motion for effecting the greatest degree of excitation under the conditions laid down in Position 4; and by the *illustration* of Position 7. the current would flow through the stick from the right to the left hand.

The preceding positions will, if I have not deceived myself, exhibit a correct view of one class at least of the natural elements of magnetic electricity; viz. those *secondary* theoretical laws which govern its excitation, and give direction to its polar streams. They are those *proximate* laws by which the display of the phænomena is accomplished and regulated, and by which it may very simply be explained, and easily understood. By these laws the experimenter may be directed in his manipulation, and with precision he may foretell the direction of the resulting electric streams.

[To be continued.]

XXXIII. *Further Experiments on the Phænomena presented by Light in its Passage along the Axes of Biaxal Crystals. By the Rev.* HUMPHREY LLOYD, *A.M. M.R.I.A. Fellow of Trinity College, and Professor of Natural and Experimental Philosophy in the University of Dublin*.*

I STATED in a former communication†, that by a new development of the undulatory theory of light, in its application to the laws of double refraction, Professor Hamilton had arrived at the remarkable conclusion, that in two cases of refraction in biaxal crystals, a single incident ray ought to be divided into an infinite number of rays, constituting a refracted cone. The first of these cases of conical refraction will take place at the emergence of the ray into air, when it has proceeded from a point on the surface of, or within, the crystal, and in the direction of the line‡ joining two opposite cusps in

* Communicated by the Author. † Page 112, et seq.

‡ It is much to be desired that these lines,—the normal to the circular section of the *surface of elasticity*, and the normal to the circular section of the *ellipsoid* of Fresnel's theory,—were distinguished by some appropriate

the wave. The second takes place within the crystal, when a single ray has been incident externally in such a manner, that one of the refracted rays may coincide with the normal to the circular section of the surface of elasticity, or the optic axis*.

In the article alluded to, I have entered into an account of some experiments, undertaken at the request of Professor Hamilton, which establish the existence of the first case of conical refraction; and go far, therefore, to support the theory of which it is a consequence. I have only to add, on this part of the subject, that additional measurements, taken since that paper was written, indicate a nearer agreement between the observed and computed cones than was at first obtained.

I have since succeeded in observing also the second species of conical refraction; and I now propose to give a brief sketch of the results of my experiments, referring for further detail to the forthcoming volume of the Transactions of the Royal Irish Academy.

It has been already mentioned, that the existence of this phænomenon depends upon the mathematical fact,—that the wave surface is touched in an infinite number of points, constituting a small circle of contact, by a single plane parallel to one of the circular sections of the surface of elasticity. When a ray is incident upon the crystal externally, in such a direction that one refracted ray may be normal to the plane just mentioned, it will be divided into a cone of rays within the crystal, determined by lines connecting the centre of the wave with the points of the periphery of the circle of contact. The angle of this cone $= \text{tang}^{-1} \dfrac{\sqrt{a^2-c^2}\,\sqrt{c^2-b^2}}{c^2}$, c being the mean axis; and its value in the case of arragonite, calculated from the elements of this crystal as determined by Professor Rudberg, is $1° \; 55'$.

Since the rays which compose this cone will be refracted at emergence in a direction parallel to the incident ray, they will form a small cylinder of rays in air; the base of the cylinder being the section of the cone formed by the second surface of the crystal. This cylinder is in all cases extremely small, and the experiments necessary to detect its existence and ascertain its magnitude require more care than those hitherto described.

nomenclature. Fresnel calls the former, the *optic axis*, when he is defining the term; but he subsequently applies the same name to the other. I fear that I have also made the same double application of the term in my former communication on this subject; though I have generally, with Professor Hamilton, used the word *cusp-ray* to designate the latter.

* See last note.

The light first employed, was that of a lamp placed at some distance; and in order to procure an incident ray as minute as possible, this light was made to pass through two small apertures; one of which was in a screen placed near the flame, and the other perforated in a thin plate of metal close to the first surface of the crystal. Observing the two rays into which the incident ray is generally divided, I turned the crystal slowly, so as to alter the incidence very gradually. After some trials, in which I was partly guided by the changes in the relative position of these rays, I at length succeeded in obtaining an incidence at which the two rays were seen to spread into a continuous circle; the diameter of which was apparently equal to the interval between them when near the ultimate position.

The emergent light in this instance was received directly by the eye, assisted by a lens. On repeating the experiment with the sun's light, I was enabled to receive the emergent cylinder upon a small screen of silver paper, and to see that there was no sensible difference in the magnitude of the section at different distances from the crystal.

When the adjustment was perfect, the light of the entire annulus was white, and of equal intensity throughout. But on a very slight deviation from the exact incidence, two opposite quadrants of the circle appeared more faint than the two others; and the two pairs were of complementary colours.

The theoretical incidence is easily calculated. The ray which proceeds within the crystal in the direction of the optic axis being a normal to the wave-surface, the direction of the corresponding incident ray will be given by the ordinary law of the sines, assuming as the refractive index the mean index of the crystal. The angle which the optic axis makes with the axis of x, or with the perpendicular to the surface of incidence, is equal to $\tan^{-1}\sqrt{\frac{c^2-b^2}{a^2-c^2}}$; and its value in the case of arragonite is $9° 1'$, assuming the values of the three indices as determined for the ray E by Professor Rudberg. The corresponding angle of incidence is $15° 19'$, the refractive index being 1·6863. Now the *observed* angle of incidence, which was obtained by measuring the angle between the incident and reflected rays, was $15° 40'$; differing from the computed angle by $21'$.

In order to determine the angle of the cone, I measured the diameter of its section made by the second surface of the crystal; and found it to be ·016 of an inch. The thickness of the crystal was ·49 of an inch, and the inclination of the conical pencil to the perpendicular about 9°. The angle of the cone, computed from these data, was found to be 1° 50′; differing by 5′ only from that assigned by theory.

Examining the emergent rays with a tourmaline plate, I found that they were polarized, and according to the law already observed in the former case of conical refraction. The result was in this case predicted by theory; in the former instance it was first discovered by observation.

XXXIV. *On the Theory of Voltaic Action.* *By Mr.* John Prideaux*.

Sect. I. *Of the Relation between Voltaic and Common Electricity.*

1. DISTINCTIONS have been drawn between electricity from the machine, and that from the voltaic apparatus; difference of tension having been considered insufficient to account for the difference of their effects. Dr. Hare is, I believe, the only chemist who has offered an explanation of this distinction, which he does by regarding voltaic as a compound of common electricity and caloric.

Because *a*) It warms all bodies through which it passes, unless very good and sufficient conductors, of caloric as well as of electricity.

b) A wire may be made, by a strong voltaic current, to continue radiating caloric for an indefinite time; which caloric, unless indefinitely contained in the wire, must be supplied by the current.

c) By passing through charcoal, or other bad conductors of heat, the caloric may actually be separated from the electricity, which is thus deprived of its heating power.

And hence Dr. Hare names the operation of his apparatus "calorimotion," in contradistinction to electromotion.

2. Whether Dr. Hare has abandoned this theory, I do not know. If not, it might be argued in reply,—

a) That reducing the diameter of a good conductor, disengages the heat as effectually, or even more so, than substituting a bad one.

b) That in reference to the wire, it may be made to give out *light* indefinitely, by heat; supplied invisibly by hot air †, or in any other mode, even that of the voltaic current; whence,

* Communicated by the author.

† Over a flame of hydrogen gas, as near as possible without touching it, hold a slip of platinum foil, edge downwards. The foil will continue to glow so long as it and the flame are kept steady. The little platinum spiral lamp, in vapour of spirit, is a more striking but less unequivocal evidence of the same thing.

by parity of reasoning, *light* should be contained in both heat and the voltaic fluid, unless it also be indefinitely contained in the wire; and heat may be indefinitely continued by friction, where its source is yet more obscure.

c) That common electricity passed through a water tube is deprived of its power of deflagration, or of even affecting Mr. Harris's delicate air-electrometer*, more effectually than voltaic electricity is by passing through charcoal, which may be thus proved:—

A pair of Leyden jars, connected in both coatings, were set on an insulating stool; their outer and inner coatings communicating respectively with two balls, set at $\frac{1}{4}$ inch apart. Between the outer coating and its ball, Harris's thermotest was interposed. The prime conductor being put in communication with the inner coating, the outer having of course a communication with the ground, the machine was turned until the jars discharged themselves through the balls. The thermotest rose 12°, or 1·2 inch; and so repeatedly, at the 64th turn of the machine, with a smart explosion at each discharge.

A glass tube, about 7 inches long and $\frac{5}{8}$ calibre, filled with water, and wired through a cork at each end, was now interposed between the inner coating and its ball. The discharge now took place with a sharp hissing sound, a blue spark, and *no effect whatever* on the thermotest, although it required 70 or more turns of the machine to make the spark pass; and very little electricity was left in the jars.

The water tube was next placed between the prime conductor and the inner coating, a similar one being made to communicate between the outer coating and the ground;—thus the jars were charged both inside and out through the water tubes; and if, in the case just quoted, the inaction on the thermotest was caused by abstraction of caloric in passing through the water, that caloric being now abstracted in *charging* the jars, no heat could be produced in their discharge. But on discharging them through the metallic circuit, as at first, the thermotest rose in the same manner; and on repeating and varying the experiment, the same effect was always produced by any given number of turns of the machine thrown on the jars, whether *charged* through water or metal, provided they were discharged through metal; and the effect was uniformly null when the *discharge* took place through water.

* To avoid circumlocution or ambiguity in the frequent repetition of the words "electrometer," "galvanometer," &c., I shall take the liberty of distinguishing Mr. Harris's instrument by the term "thermotest"; and the galvanometer of Professor Cumming by the name "magnetest."

To put Dr. Hare's charcoal experiment to a test as nearly parallel as the cases seem to admit,—

A card was placed between two copper plates, bound round with wire, and set in the fire until the card was well charred. It was then taken out and put between two plates of polished copper, to each of which a wire was soldered; they were then bound round tightly with many turns of waxed thread, and a wooden wedge afterwards thrust under the thread, on each side, so as to insure the charcoal being firmly and uniformly pressed between the plates. The two wires were then made the connexion between a thermotest and a magnetest, one connected with each pole of a large voltaic pair, weakly charged. Neither instrument was distinctly affected.

A multiplier being substituted for the magnetest, the needle deviated a few degrees. A slip of bright copper was now doubled so as to pinch the plates containing the charcoal, and thus complete the metallic communication. The needle was set spinning, and the thermotest rose 10° an inch.

The charge was now increased to about $\frac{1}{60}$th of nitric acid, a smaller pair being employed (30); the charcoal and slip of copper forming the communication alternately; and the multiplier being again superseded by the magnetest. The results are given in the following table.

(The column headed "Mag." is the deflection of the needle.—"Curr." is the intensity of the current, calculated from the deflection, Becquerel's table.—*Ann. de Chim. et de Phys.* for August 1829.—"Ther." is the rise of the thermotest.)

	Through Charcoal.			Through Copper.		
	Mag.	*Curr.*	*Ther.*	*Mag.*	*Curr.*	*Ther.*
	25°	16	1·5	48°	60	8
	30	23	2	50	68	8
	25	16	1	48	60	8
	25	16	2	46	54	7
Mean	27	18	1·6	48	61	8

Here the *electric* current is obstructed by the charcoal, as well as the *calorific*; and the calorific effect is, in each case, in such proportion to the current as would be expected, considering that weak currents which pass through the wire without resistance do not heat it at all; whence the heat must increase at a greater rate than the current.

Thus it is seen that charcoal obstructs the electric as well as the calorific current; and that common electricity is more effectually deprived of its heating power by passing through water, than voltaic by passing through charcoal; and it seems to follow, that bad conductors act, in case of the shock, by diminishing its impetus; of the current by reducing its quantity; and thus allowing of their passing tranquilly through the thermotest wire.

I think I am prepared to show, on a future occasion, that the heat of electricity is proportionate to, and consequently dependent on, the resistance it encounters in the substance heated; the shock presenting some analogy with percussion, the current with friction.

Dr. Hare seems also to have regarded voltaic electricity as acting on particles, and not on masses; an opinion which has of course long yielded to the whole body of facts belonging to electro-magnetism.

3. Common electricity, produced on non-conductors, is transmitted through a greater or less thickness of air, and *must* therefore possess a certain degree of tension; by which, when transferred to conductors, it is forced to their surface, and retained *there* only by the non-conducting power of the surrounding air.

4. Voltaic electricity is produced through the medium of semi-conductors, and cannot acquire *great* tension, because the resistance to its return is not sufficient. But by multiplying the strata of imperfect conductors through which it must force its way to return; that is, by increasing the number of alternations,—this tension may be increased to a limit not yet ascertained.

Its production is perfectly continuous, and its quantity such, that Van Marum found his great battery charged, by instantaneous contact with 100 pair of 2-inch plates, as high as the pile itself,—a charge which would have required several turns of his gigantic machine.

And on this continual flood seem to depend the chemical and (in case of voltaism, where the impetus is small) the calorific effect; for weak piles are said by Senger to have their effects remarkably increased, in these respects, by being let off, as it were, over the large conducting surface of a coated jar; whilst by reducing the magnitude of the plates, confining their points of contact with the liquid (17), or even obstructing the conducting power of the latter, the current may be so restricted as to produce tension *only*.

5. Thermo-electricity, being usually produced on perfect conductors, may be expected to be still more free from tension than voltaic electricity; its quantity and continuity being marked by its powerful action on the needle. Yet even this may be made to affect the gold leaf electroscope, by using a metal of

difficult conduction, as platinum; connecting the other end with the ground, through an imperfect conductor, as hot glass, or the finger*.

6. Though these observations leave unanswered many of the objections to the identity of voltaic and common electricity, those objections do not seem to me sufficient, after the striking experiment of Dr. Wollaston†, to establish any other distinction between them than difference of tension; nor will any other distinction be considered, in the following pages, between the electrical and chemical effects of the pile. Tension is even well known to expedite and facilitate the decomposition of water and salts by the voltaic current.

Sect. II. *Of the received Voltaic Theories.*

7. The insufficiency of the theory adopted by the acute philosopher, whose name stands for ever enshrined in this branch of electrical science; by which the action of the pile was referred solely to the electromotive force of the metals; that is, its inadequacy to account, without noticing the decomposition of the charge, for the copious flood of electricity which distinguishes its operation,—was early perceived. And it has since been shown by experiment, varied in a multitude of ways, that decided electrical action may be produced by two pieces of the same metal, in the same fluid (40, 41, 45); and that the efficacy of the liquid charge depends more on its facility of decomposition than on its conducting power.

8. To Dr. Wollaston is generally attributed another theory, which imputes the effects of the pile to decomposition of the liquid charge, and its chemical action on the zinc. He has been followed by Professor De la Reve, of Geneva, who goes further than Wollaston probably ever intended to do, attributing the effects *entirely* to the chemical action; and disputing the validity even of Volta's leading experiment, the production of electricity by contact of zinc and copper‡. But the electromotive effects of the metals are established by nume-

* Becquerel, *Ann. de Chim. et de Phys.* for August 1829.

† Dr. Wollaston's experiment here alluded to, is that in which copper was deposited on the negative wire, and redissolved, by reversing the connexion. A gross analogy with voltaic decomposition may be also noticed in an experiment where the difficulty of obtaining continuity in common electricity is obviated:—

Pass a negative ball over the face of a cake of resin, in fanciful lines of any kind, and a positive ball over the same face in other lines. Dust the plate with a mixture of powdered sulphur and red lead. The powders will separate according to their electrical character, and mark out the lines described by the respective electricities, in red and yellow.

‡ *Ann. de Chim. et de Phys.* for November 1828.

rous and unequivocal experiments, amongst which the following may be cited as well suited to contrast it with the chemical source of electricity.

9. Professor Berzelius took 12 glasses, each 3 inches deep and ½ inch wide, half filled them with alkalescent muriate of lime, the other half with dilute nitric acid. Into each he put a copper wire with a zinc foot, the zinc lying in the alkalescent liquid, the copper bending over into the acid of the succeeding glass. This formed what is commonly called the circle of cups.

So long as the poles were unconnected, the copper dissolved in the acid, and the zinc remained unacted on; but as soon as the connexion was made (through solution of salt), the solution of copper ceased, and the zinc began to oxidate, the zinc becoming positive (in the liquid) and the copper negative, as usual in voltaic arrangements.

Here the chemical action, which was in full play on the copper, before the electrical circulation was opened, instead of directing that circulation, was checked, and even reversed by it.

A simpler variation of this experiment will be described (14). And it will also be seen (13), that placing the zinc in an alkaline, the copper in an acid solution, is more efficacious than the contrary arrangement, although the acid acts much more freely on the zinc than on the copper.

It is also generally known, that of neutral salts, none (except sal-ammoniac) makes a more efficacious charge than sulphate of zinc, which is very unlikely to undergo decomposition.

10. Sir Humphry Davy's theory assumes that* "chemical and electrical attractions are produced by the same cause; acting in one case on particles, in the other on masses: and the same property, under different modifications, is the cause of all the phænomena exhibited by different voltaic combinations." A view so comprehensive, embracing every modification of chemical as well as electrical action, seems to include the other two, and every one that has been, or can be, attempted on the subject. But what it gains in extent it wants in distinctness.

11. It may, however, be limited by the word "attraction," and by the statements that a plate of platinum in solution of potass is positive to a plate of the same metal in acid, in consequence of the *attraction* of the *oxygen* of the acid for the one plate, and of that of the *metal* of the potass for the other; —a consequence not very evident. Also, that the cause of a

* Philosophical Transactions, 1826, Part IV.

current when two plates of zinc act in the *same* acid, is, one being *tarnished* and therefore "negative"* to the other; an explanation inconsistent with an experiment of Mr. Sturgeon's (41), derived probably from this very paper of Davy's.

Sect. III. *Of the initial Electromotion in the Metals.*

12. Volta, finding that he could not exalt his electromotive power by piling pairs of zinc and copper one upon another, thought of interposing flakes of wet paper between the pairs, and succeeded. Other investigators have since varied the form of the apparatus, and every one of its elements;—have still found zinc and copper the most convenient metals for the purpose, but have made great improvement in the liquid. Nitric acid, properly diluted, seems, under all circumstances, the most efficacious of these; but for the electromotive influence *only*, the most effective arrangement is zinc, alkali, acid, copper.

13. With whom or when this discovery originated, I do not know; but it is stated in Berzelius, *Traité de Chimie*, i. 152, that the little apparatus described (9) filled, half with alkaline, half with acid solutions, the zinc plunged in the alkali, the copper in the acid; produces more decided effects than when the zinc is placed in the acid, the copper in the alkali. In repeating this experiment I was annoyed with the negative effect of the copper stem, passing up from the zinc through the acid, from which more hydrogen was often given off than from the little copper plate which came over from the adjoining glass. This rendered the results variable; and to obviate it the plates were bent off at right angles, so as to lie flat, and shallow cups were employed instead of glasses; the depth of the strata of alkali and acid being thus reduced to $\frac{1}{4}$ or $\frac{3}{8}$ inch each, the stems had less interfering effect. The steadiness of result thus produced, suggested a simpler form of the experiment.

14. A glass tumbler being filled to the depth of $\frac{1}{2}$ inch with solution of common salt alkalised with potass†; an inch deep of diluted muriatic acid‡ was poured on carefully, so as to float above the alkali, without disturbing it. The latter having been coloured with logwood, the surface of separation was as distinctly visible as that of the glass was from the table.

A plate of zinc, and another of copper, each 2 inches

* The term "negative" here, and in other parts of the first section of Davy's paper, seems to be misapplied.

† Salt $\frac{1}{2}$ ounce, water $\frac{1}{2}$ pint, liq. potassæ 1 ounce.

‡ Water 1 pint, muriatic acid 3 drachms.

square, being each furnished with a copper wire soldered on, the wires 18 inches long, and amalgamated at the further ends; these plates were alternately placed flat on the alkali, the zinc being put in the acid just above the alkaline surface, when the copper was in alkali, and *vice versâ*.

The following table gives the deflections of the magnetest needle.

	Experiments 1	2	3	4	5	*Mean.*	*Curr.*
Zinc in alkali ...	26°	25	25	26	26	25·5	17
Zinc in acid......	9	7	8	7	8	8	4

The zinc was soon covered with gelatinous white oxide in the alkali; and the copper in the acid gave off hydrogen gas copiously, but without giving the slightest tinge to the liquor.

The above table gives only about the average of many experiments, some of which differed considerably from the numbers there given, influenced by the strength and depth of the alkaline solution, of which more (21).

15. If two glasses be filled with solution of common salt or sulphate of zinc, and connected by a syphon of water; and a pair of zinc and copper, connected through the multiplier, be plunged one plate in each glass,—divergence of course ensues. Now add some acid to the glass containing the zinc, no increase takes place in the divergence. Change the glasses so that the copper shall be in the acidulated one, the divergence is decidedly increased.

16. In both these cases (14, 15.) we have the zinc and copper in metallic connexion through the magnetest, and in liquid communication through the same fluids; the only difference being in the order of arrangements of the liquids.

Hence the increased effect is not attributable to varied electrical relation between the metals; to different conducting power in the liquid, still less to chemical action, that being greatest when the zinc is in acid, where the voltaic action is least.

17. At an early period in voltaic history, Mr. De Luc analysed the pile, separating, by interposing a brass wire tripod, one of the members from each group of zinc, copper, wet cloth. His first pile was, silver, tripod, zinc, cloth; silver, tripod, &c., and this produced all the effects of a good pile; the tripod forming a regular metallic connexion between the zinc and silver, and the wet cloth being in full communication with the other face of each.

His second pile was, silver, tripod, cloth, zinc; silver, tripod, cloth, &c., and this produced electrical, but no chemical effects. The zinc and silver were in good contact, and so were the zinc and cloth; but the cloth communicated with the silver only through the points of the tripod, a contact with the liquid insufficient to convey a current capable of any chemical action.

His third pile was, zinc, tripod, cloth, silver; zinc, tripod, wet cloth, &c.; and this produced no effects at all. The pile was inert. Yet (as noticed 16.) the communication here was the same as in the previous experiment, except that the tripod intercepted the contact of the *zinc* with the liquid, instead of that of the *silver*. But the tripod was of brass, which is negative to zinc, and was in contact with it on one side; whilst the silver, also negative, was in contact with it on the other. And De Luc attributes the inaction of this pile to the neutralizing effect of the brass and silver on opposite sides of the zinc.

18. To see how far this explanation, which seemed very probable, would apply, the pair of plates described (14.) were connected with a multiplier; the copper placed on the table, a flake of wet paper laid on it, and the zinc plate pressed gently on the paper.

Deflection . . . 30°

A slip of sheet copper $\frac{1}{8}$ inch wide, and 3 long, bent into a ring 1 inch diameter, was then placed between the paper and the zinc plate (as in De Luc's third pile). The effect was (as then) null, 0, although greater pressure than before was applied on the zinc.

A slip of sheet zinc, of similar dimensions to the copper, and similarly bent, was now substituted for it.

Deflection . . . 27°

The paper was now taken up from the copper, and the zinc ring laid on the copper plate; then wet paper, then the zinc plate.

Deflection . . . 0°

The zinc ring was now removed, and the copper one substituted for it, between the paper and the copper plate.

Deflection . . . 15°

So that interception of the contact between either of the metals and the liquid, by a ring of the electro-opposite metal, entirely intercepts the current.

19. The action when the wet paper was in full contact with the copper, and only with the ring of zinc, gave deflection 27°.

When in entire contact with the zinc plate, but only with the ring of copper,

Deflection . . . 15°

15 to 27° deflection being in the proportion of 8 to 19 of electrical current. And this held good (not as to exact pro-

portions,) whether water, a saline solution, or an acid was employed.

But on soaking the paper in a strong alkaline solution (liquor potassæ), the rate was inverted, and the maximum effect produced when the liquid was in *full* contact with the *zinc*.

Alcohol produced, as might be expected, no effect whatever.

In these experiments, the effect of the copper is weakened by the liquid in contact with it being electro-positive (alkaline), and destroyed by the interposition of a ring of positive metal (zinc) between it and the liquid. And the effect of the zinc is reduced by negative character in the liquid with which it is in contact, or annulled by the interposition of a ring of negative metal between it and the liquid.

20. This similar effect of homo-electric metals and liquids, and *vice versâ*, in their contact with the voltaic plates, seems to offer a clue to the theory of the cases, (9, 13, 14, 15, 45,) and of other unexplained phænomena of the pile. If a negative metal, placed between the zinc and the liquid, neutralizes the effect of the silver on the other side, has not a negative property in the liquid itself a similar tendency?—modified and impaired, of course, by the mobility and unsteadiness of liquid particles.

If it be so, when a negative liquid is in contact with the zinc of a voltaic pair, the effect should be (except chemical action, 40.) the *difference* between the influence of the negative metal and the negative liquid. On the other hand, when the negative liquid is in contact with the copper, the effect should be, the influence of the zinc, plus that of the negative liquid. And if we possessed a liquid of good conducting power, which should be electro-positive to zinc, the condition of that metal might be likewise exalted. But here we are restrained by the non-conducting power of oils, naphtha, alcohol and the like; and as water is negative to zinc, and alkaline solutions seem to lose in conducting power faster than they gain in electro-positive condition, a solution of neutral salt slightly alkalized seems to answer the best purpose with this metal: for—

21. In experiment (14.) when the alkaline liquid contained a double quantity of potass, and was poured in nearly an inch deep, the effect was remarkably weakened, the deflection of the needle not exceeding 10° or 12°; and very little oxide formed on the zinc, or hydrogen on the copper. But—

22. When the liquid was of the strength given (14, note) and only ½ inch deep,—as the zinc became coated with gelatinous white oxide, so the alkaline stratum was gradually becoming shallower, by the electrical current carrying it up into the acid; and thus what was lost in conducting power by the

coating of oxide, was gained, on the other hand, by the reduction of the alkaline stratum; and the consequence was a singular steadiness of action, the needle continuing for an hour within a degree or two of its maximum deflection.

This durability of action offers the means of a new class of voltaic researches, on which I am not yet prepared to make any report.

23. From multiplied experiments of the kind above quoted, it may be inferred that the metals partake of the electrical character of the liquids in contact with them; their electric condition being exalted if that liquid is similar, and depressed if it is of the opposite kind. And hence the fact observed by Morichini, that an addition to the quantity of copper increases the power of a voltaic pair, the charge being always negative, and therefore homo-electric with copper.

24. Whatever be the nature of electricity, it would seem to be connected with material particles by something analogous to affinity; inducing bodies which are naturally positive, to withdraw positive electricity from those which are naturally negative, when brought into mutual contact. In what way they do this I am unable to conceive in a manner consistent at once with the phænomena, with chemical analogy, and with probability; nor are experimental indications very easily found. I may perhaps venture the surmise, that repulsion is the stimulative, attraction the suppressive, principle of voltaic agency.

[To be continued.]

XXXV. *Further Demonstration of the Existence of a real or imaginary Root for any proposed Equation.* By R. Murphy, *Esq., M.A., Fellow of Caius College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In your Number for January, you favoured me with the insertion of a simple demonstration relative to the existence of a real or imaginary root for any proposed equation. I beg leave to reproduce that proof in a more distinct point of view in the present Number.

When the equation $f(x) = 0$ is of odd dimensions, it is known from the simplest principles that there exists a real root.

When the function $f(x)$ is of even dimensions, put $p + q\sqrt{-1}$ for x; where p and q are real quantities, the result R will evidently be of the form $\mathrm{P} + \mathrm{Q}\sqrt{-1}$. Where

P, Q are real functions of p and q, and the latter of odd dimensions in Q; therefore a real value of q corresponding to any proposed real value of p may always be found so as to make $Q = 0$, and then we have $R = P$, a quantity always real.

By assigning to p real values indefinite in number, the real values of R thus obtained must either pass through zero, or else some one result is nearer to zero than any other.

In the latter case putting $p + h$ for p, and supposing such a value assigned to q that $Q = 0$, we should obtain a result

$$R = P + A h^{h} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1)$$

in which the lowest power of h alone is included, since we may suppose h very small.

But if we put for p, $p + h \left\{ \cos\frac{\pi}{n} + \sqrt{-1}\sin\frac{\pi}{n} \right\}$ this substitution will lead evidently to the same result as if we put for p, $p + h\cos\frac{\pi}{n}$; and for q, $q + h\sin\frac{\pi}{n}$, and a real value may therefore be found for $q + h\sin\frac{\pi}{n}$, and consequently also for q such that Q shall still vanish, and we get

$$R = P + A h^{h} \left\{ \cos\frac{\pi}{n} + \sqrt{-1}\sin\frac{\pi}{n} \right\}^{n}$$

$$= P - A h^{h} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (2)$$

One of the real values of R marked (1), (2) must necessarily be nearer to zero than P, contrary to supposition. ∴ Hence it follows that R passes through zero; that is, the equation $f(x) = 0$ always admits a root of the form $p + q\sqrt{-1}$ where p and q are real quantities.

Caius College, Jan. 11. R. MURPHY.

XXXVI. *Suggestion regarding the Improvement of Lighthouses. By* JOHN ROBISON, *Esq. Sec. R.S. Ed.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

AS there are some circumstances which make it probable that a light of large volume, though of moderate intensity, should be recognisable at a greater distance *during foggy weather*, than one of small extent, though more brilliant, and capable of penetrating further in a clear atmosphere, it appears to be desirable that some person who has the means of doing it, should try whether a large blaze of pyrotechnic composition (the red light from strontian would perhaps answer well), or the ordinary illumination of a lighthouse apparatus, be seen at the greatest distance in a dense fog.

If it should prove that the blaze, by illuminating the clouds or fog, possess any considerable advantage in such cases, it would obviously be expedient to make occasional exhibitions of such signals on lighthouses.

I am, Gentlemen, very truly Yours,

JOHN ROBISON.

XXXVII. *Proceedings of Learned Societies.*

LINNÆAN SOCIETY.

Jan. 15, and Feb. 5. A PAPER was read on the Lycium of Dioscorides, by J. Forbes Royle, Esq., late Superintendant of the Hon. East India Company's Botanic Garden at Seharampore.

From the curious researches of the author, it appears that the substances known in the East under the names *Hooziz, Rusot*, and *Dar-huld*, are the produce of a species of Barberry: and that the words *loofyon* or *lookyon* are given by Arabic writers as the Greek synonyms of *Hooziz*, followed by the description given by Dioscorides of *Lycium, Λυκιον*, which is doubtless the word given by the Arab writers as *lookyon*, or corruptly *loofyon*, their character for *f* and *k* differing only by a diacritical point placed over the letter. He therefore concludes this substance in the Materia Medica to be the product of the wood or root of a species of Barberry. It is now applied in cases of inflammation of the eyes.

ROYAL ASTRONOMICAL SOCIETY.

November 9, 1832.—The following communications were read:—

1. Observations of Biela's Comet, by Sir J. F. W. Herschel.

"The moon being sufficiently removed from the presumed place of this interesting object, and the promising aspect of the evening of the 22nd September last holding out hopes of detecting it by the powerful light of the 20-feet reflector, I directed that instrument, with a newly-polished mirror, to a point of the heavens determined by taking a mean of the right ascensions and declinations, calculated by M. Santini from his own and from Damoiseau's elements. A haze, however, which, as the evening advanced, rapidly increased to a decided cloud, disappointed my expectations for that night; but on the following evening, the sky being perfectly clear, after sweeping to and fro for about five minutes over the place similarly calculated for the time of observation, I had the satisfaction of seeing the comet enter the field as a conspicuous nebula of about $2\frac{1}{2}'$ or $3'$ in diameter, and of such a degree of brightness as would entitle it to a place in my father's first class, or 'bright nebulæ.' It had no tail; and though its rate of increase of density towards the centre was rather more rapid and decided than about the circumference, yet there was nothing in the least entitled to the name of a nucleus. As no nebula exists in that part of the heavens which could be confounded with an object of this description, it was of course immediately recognised as the object sought: but had I entertained any doubt of its identity, it would have been dispelled by its motion, which, during an hour and a half, or thereabouts, that I kept it in view, was very considerable. At the commencement of my observation, it formed an irregular trapezium with three pretty bright stars, A, B, C; with two of which I measured its angles of position, and, on repeating the measures after a short interval, found them sensibly altered. The change of place speedily became remarkable to the eye, and was such as to carry the comet towards two or three other pretty bright stars, D, E, in a distant part of the field. In approaching these, it passed directly over a small cluster or knot of minute stars, of the 16th or 17th magnitude, which occupied a space about a minute or two in diameter; and, when on the cluster, it presented the appearance of a nebula resolvable, and partly resolved, into stars, the stars of the cluster being visible through the comet.

"A more striking proof could not have been offered of the extreme translucency of the matter of which this comet consists. The most trifling fog would have entirely effaced this group of stars; yet they continued visible through a thickness of the cometic matter, which, calculating from its distance and apparent diameter, must have exceeded 50,000 miles, at least towards its central parts. That any star of the cluster was *centrally* covered is, indeed, more than I can assert; but the general bulk of the comet may certainly be said to have passed centrally *over the group.*

"The reflector being at that time in a great measure dismantled, for the purpose of making some changes in the sweeping apparatus, I was unable to determine, as I could have wished, the place of the comet by an extra-meridian sweep; I was, therefore, obliged to have recourse to the 7-feet equatorial (aperture 5 inches), with which, at best, I could only hope to procure an approximate place; and from my knowledge of its comparative inefficiency in showing nebulæ, I had very slender expectation of being able to see it at all. However, by carefully noting in the finder of the large instrument the exact *locality* of the spot with respect to the stars θ, ν, and τ *Aurigæ*, I was enabled, with no great difficulty, to find the telescopic constellation consisting of the stars A, B, C, D, E, with which I had compared the comet in the reflector; and, after very long and careful attention in a field totally dark, and without the smallest light in the observatory, I succeeded in obtaining a sight of the comet in the place where, from its motion during the interval, I expected to find it. Under such circumstances, any determination of its place must be exceedingly vague; and, in fact, on reducing three comparisons which I obtained, directly and indirectly, of its place with that of θ *Aurigæ*, it became evident that in one of them I must have mistaken a retinal spectrum produced by nervous excitement, for the comet. For their results when reduced gave as follows, (calling α and δ the right ascension and declination of θ *Aurigæ,* and α' δ' those of the comet):

By the first comparison...	$\alpha' = \alpha - 8^m\ 4^s$	$\delta' = \delta - 53'\ 30''$
——— second	$\alpha' = \alpha - 9\ \ 9$	$\delta' = \delta - 59\ \ 0$
——— third................	$\alpha' = \alpha - 7\ 51$	$\delta' = \delta - 55\ \ 48$

The sidereal times of observation (corrected for the clock's error) being respectively $3^h\ 53^m\ 41^s{\cdot}5$, $4^h\ 32^m\ 40^s{\cdot}5$, $4^h\ 51^m\ 36^s{\cdot}5$. The second of these results must evidently be rejected for the reason above mentioned; whence the mean of the two others gives, for $4^h\ 25^m\ 29^s{\cdot}5$ sid. time,

$$\alpha' = \alpha - 7^m\ 57^s{\cdot}5 \ ......\ \delta' = \delta - 54'\ 39''.^*$$

"At $4^h\ 51^m\ 36^s{\cdot}5$, sid. time, the comet was compared with a small double star, whose approximate place for 1830·0 was found, by comparison with θ *Aurigæ,* to be

Æ $5^h\ 40^m\ 54^s{\cdot}5$; Decl. $+\ 36^\circ\ 13'\ 31''$.

The stars composing this double star are about the 8th and 9th magnitudes; their distance, roughly taken, about 65 parts of the micrometer $= 15''$; and their angle of position $13^\circ\ 20'$ *sf* $= 103^\circ\ 20'$. This description will enable any observer easily to identify and re-observe the double star in question. The comet, at the time above stated, *preceded* the double star 38 seconds of time, and was found, by a micrometric estimation (for I cannot call it a measure), to be about 550 parts $= 2'\ 12''$ *North* of it.

"On the next night, I again observed the comet with the 20-feet reflector, and again found it without the least difficulty. It was very little, if at all, perceptibly brighter or larger than the preceding night; but had entirely deserted its former place, and was now near a single, pretty bright star, of fully the 10th magnitude, insulated in the field, and easily identifiable. The approximate place of this star for 1830·0 I found, by comparison with θ by the equatorial, to be Æ $5^h\ 45^m\ 18^s{\cdot}0$; Decl. $+\ 36^\circ\ 13'\ 31''$.

"I procured, through the intervention of this star, two indirect comparisons of the comet with θ *Aurigæ*; in both of which I was, however, obliged to *estimate* the difference of declinations of the comet and star at 2', by reason of the excessive feebleness of the former, which rendered all measurement impracticable. I also compared it twice *directly* with θ; but of these comparisons, one, when reduced, proved to have been an illusion of the kind above mentioned; I therefore suppress its result. Those of the others were as follow:—

	h m s		m s	° ′ ″
By intervention of the small star, 1st comparison	$t = 2\ \ 1\ \ 4$	sid. time;	$\alpha' = \alpha - 2\ 28{\cdot}5$;	$\delta' = \delta - 1\ \ 0\ \ 0$
By ditto, 2nd ditto	$t = 3\ \ 1\ \ 4$		$\alpha' = \alpha - 2\ 14{\cdot}5$;	$\delta' = \delta - 1\ \ 0\ \ 0$
By direct comparison with θ ...	$t = 2\ 30\ 46$		$\alpha' = \alpha - 2\ 25{\cdot}0$;	$\delta' = \delta - 1\ \ 1\ \ 0$
Mean......	$t = 2\ 30\ 58$		$\alpha' = \alpha - 2\ 23{\cdot}5$;	$\delta' = \delta - 1\ \ 0\ 20$

* The mean place of θ *Aurigæ,* brought up to Jan. 1, 1832, by the precession assigned in the Astronomical Society's Catalogue, is

Æ $5^h\ 48^m\ 15^s{\cdot}6$; Decl. $+\ 37^\circ\ 11'\ 33''$.

"The following night it proved cloudy, and my attention being distracted to other objects, I have not since pursued the comet; nor should I have thought it worth while to communicate observations confessedly so imperfect as the present, to the Society, but for the high interest attaching to the object, as well as for an impression left on my mind, by the excessive feebleness of its light as seen in the equatorial, that it may have escaped the notice of most observers at this early period of its appearance, for want of light, as I am certain I could never have found it by any degree of attention with that instrument, had not its precise place been given by the 20-feet reflector.

"I know not whether I may be pardoned the mention of a conjecture in this place, as to the origin of a very striking phænomenon in the history of comets, which seems to have been satisfactorily established, at least in some instances, by positive observations; which is, their dilatation of volume as they recede from, and concentration within a smaller compass as they approach, the sun. This phænomenon has been attempted to be accounted for by a supposed *pressure* of the æther, whose density is assumed to increase in the sun's vicinity. But, not to mention that the effect would not follow from the cause without supposing the matter of the comet impermeable to the æther (as a sponge is not compressed by lowering it in water unless inclosed in a water-tight case), it appears to me that the phænomenon is explicable on a much less gratuitous supposition; viz. that of the extremely feeble attractive force by which the matter of a comet must be held together, owing to the probable extreme minuteness of its mass. *Cohesion* can hardly be supposed to exist in a gaseous or nebulous body of such tenuity; so that the only bond of union between its molecules must be their feeble gravitation to each other, which is hardly more than mere juxtaposition in space. Hence we must regard each molecule as constituting almost a separate, independent projectile, describing its own parabola about the sun. Now, the interval between two or more parabolas described about a common focus, and having their axes coincident, is a minimum at the perihelion, and increases as we recede from it, in a ratio easily calculable. The volume, on this view of the subject, ought to increase in the sesquiplicate ratio of the radius vector. The observations of Encke's comet, cited by M. Arago, indicate, no doubt, a much more rapid law of increase; but, not to mention the difficulty of obtaining any positive measures of a body so ill-defined as a comet, the circumstances under which the observations must necessarily be made have a powerful tendency to exaggerate the effect; since, in proportion as a comet recedes from the sun, it is continually seen projected upon a darker and darker part of the heavens as it emerges from the twilight; in consequence of which, exterior strata of the nebulosity become perceptible which were incapable of affecting the eye before. Those who are at all conversant with the observation of nebulæ, will not fail to have remarked the rapid rate of the obliteration produced by very trifling degrees of illumination, natural or artificial, of the field of view. Now, it is precisely on comets or nebulæ, whose condensation towards the centre is feeble and which (like the comets of Encke and Biela,) have no nuclei, that this action is most powerful, and its effect most extensive. I would not, however, be understood by any means to deny the reality of a more rapid law of variation than that which results from the foregoing consideration. If it subsist, of course we must look out for another explanation. At all events, the conclusion we have arrived at, regarded as a geometrical result, is not devoid of a certain degree of interest; and the cause I have assigned, having the character of a *vera causa*, cannot be excluded from some share in the production of the observed effect, even should it not be found sufficient to account for the whole.

"There is, however, another way in which the apparent dimensions of a comet may be conceived to vary with its proximity to the sun, while its real volume may remain unaltered, or even undergo a contrary change. The nebulous portion of the comet, or that which reflects the sun's rays, is not improbably of the nature of a fog, *i. e.* a collection of discrete particles of a vaporizable fluid floating in a transparent medium. Now, as these molecules, during the comet's approach to the sun, absorb its rays and become heated, a portion of them will be constantly passing from the liquid and visible to the gaseous and invisible state. As this change must commence from without and be propagated inwards, the effect will be a diminution of the comet's visible bulk. On the other hand, in its recess from the sun, it will part with the heat thus acquired by radiation, which, in conformity with the general analogy of radiant caloric, will take place chiefly from the unevaporated or nebulous mass within, whose dimensions will therefore begin and continue to increase by the precipitation immediately above it of fresh nebulous matter, just as we see fogs in cold still nights forming on the surface of the ground, and gradually extending upwards as the heat near the surface is dissipated. The comet will thus appear to enlarge rapidly in its visible dimensions, while the real *volume* is in fact slowly shrinking by the general abstraction of heat from the mass.

"This process might go on in the entire absence of any solid or fluid nucleus; but supposing such a nucleus to exist, and to have acquired a considerable increase of temperature in the vicinity of the sun, evaporation from its surface would afford a constant and copious supply of vapour, which, rising into its atmosphere and condensing at its exterior parts, would tend yet more to dilate the visible limits of the nebula. Some such process would naturally enough account for the appearances which have been noticed in the heads of certain comets, where the stratum, void of nebula, has been observed interposed, as it were, between the denser portion of the head or nucleus and the coma*. It is analogous to the meteorological phænomenon of a definite *vapour plane*, so commonly observed; and, in certain cases, may admit of two or more alternations of nebula and clear atmosphere.

* See my father's observations on the comet of 1811.

"If, however, after all, we should prefer to call in an æthereal medium surrounding the sun, as the sole or partial cause of the remarkable phænomenon in question, it will not be necessary to have recourse to the idea of condensation arising from its mechanical pressure, which, as we have seen, is repugnant to what we know of the mode of propagation of pressure in fluids. A less repugnant explanation offers itself in the presumable habitudes of the æthereal fluid with respect to heat. Fourier has rendered it not improbable, that the region in which the earth circulates has a *temperature* of its own, greatly superior to what may be presumed to be the *absolute* zero, and even to some artificial degrees of cold; and in my *Essay on the Study of Natural Philosophy**, (p. 157,) I have shown, I think satisfactorily, that if this be the case, such temperature cannot be due simply to the radiation of the stars, but must arise from some other cause, such as the contact of an æther possessing itself a determinate temperature, and tending, like all known fluids, to communicate this temperature to bodies immersed in it. Now, if we suppose the temperature of the æther to increase as we approach the sun, —which seems a natural, and, indeed, a necessary consequence of regarding it as endued with the ordinary relations of fluids to heat,— we are furnished with an obvious explanation of the phænomenon in question. A body of such extreme tenuity as a comet may be presumed to take very readily the temperature of the æther in which it is plunged; and the vicissitudes of warmth and cold thus experienced may alternately convert into transparent vapour, and re-precipitate the nebulous substance, just as we see an increase of atmospheric temperature dissipate a fog, not by abstracting or annihilating its aqueous particles, but by causing them to assume the elastic and transparent state, which they lose, and again appear in fog when the temperature sinks."

In a subsequent letter to Mr. Baily, Sir J. Herschel gives an account of a further observation obtained on the morning of the 4th November. The following is an extract:—

"After watching in vain for an opportunity of renewing my observations of the comet on the mornings of the 2nd and 3rd instant (Nov.), the cloudy state of the sky on both mornings from 2 A.M. till day-break precluding all possibility of observing it, I succeeded, on the morning of the 4th, in getting a very satisfactory observation. Having set the 20-feet reflector on the spot indicated by Mr. Henderson's Ephemeris, I had hardly made two or three azimuthal sweeps of the tube to and fro, when it made its appearance in the field of view as a large and very bright nebula. Judging of its light as compared with that of two nebulæ of the 2nd class which occurred in the first azimuthal sweep, I should say that its impression on the eye was at least 100 times that of one of the nebulæ. I judged its diameter to be full 4′. The condensation towards the middle was considerable, and the centre itself was occupied by a bright point about equal to a star of the 13th

* See also my paper on the astronomical causes of geological phænomena.

magnitude. It had no decided tail, but only a feeble trace of some extension of its nebulosity in a direction about 40° N.P. from the parallel.

"At $5^h 53^m 21^s$ sidereal time at Slough, the comet preceded a star *x* of the 10th magnitude $11^s{\cdot}0$ of time, and its position from the star, taken by the position-micrometer used in my observations of double stars (for which its own light afforded ample illumination), was 274°·0, so that the comet's centre was at that time $11''{\cdot}4$ north of the star.

"A large star of 5·6 mag., which proved to be *z Leonis* (Fl. 43), preceded the comet about a minute and a quarter of time, by rough estimation, and was judged to be about 13′ or 14′ to the south of it; but as my field of view would not take in both objects, I was about to have recourse to the equatorial, when it clouded. At this time the comet was rapidly approaching the star *x*, which had even begun to be involved in the extreme borders of its nebulosity. Its path, as nearly as I could judge, would have carried its centre about 40″ south of the star.

"The next night (Nov. 4–5) was unfavourable, clouds persisting obstinately in resting on and about the comet's place, while the horizon elsewhere was generally clear. However, as a star of the 10th magnitude is visible in my reflector through a pretty thick cloud, I succeeded in ascertaining the place of the star *x* of last night's observation with sufficient exactness to secure its identification, should any one be inclined to re-observe it. It follows 43 (*z*) *Leonis* $1^m 32^s{\cdot}2$ of time, and is about 12′ 47″ north of that star; so that its approximate place for 1832·0 will be Æ $10^h 15^m 44^s{\cdot}8$; Decl. + 7° 36′ 23″; which I apprehend to be within 1^s of the truth in Æ, and 1′ in decl.; and hence the approximate place of the comet at the epoch of the preceding night's observation must have been, Æ $10^h 15^m 34^s$; Decl. +7° 36′ 34″. The place for the same epoch, interpolated from Mr. Henderson's Ephemeris, computed from Damoiseau's Elements, is Æ $10^h 12^m 30^s$; Decl. + 8° 17′.

"It was not till about 8^h sid. time that the clouds were sufficiently dispersed from the comet's place to allow a view of it. Being then, however, at a much greater altitude than when seen last night, it was proportionally brighter, and was, indeed, a very fine and brilliant object. The trace of a tail or branch in the same direction as last night, though extremely feeble, was now unequivocal, and the central point not to be overlooked. It had not, however, the appearance of a star, but seemed more analogous to the central point in some nebulæ, such as that in *Andromeda*, which is probably only nebula much more condensed than the rest. The comet's diameter could not be estimated under 5′, and I suspected some degree of nebulosity even beyond that limit.

"At $8^h 8^m 29^s$ sid. time at Slough, the comet's centre followed a star *w* in the field, almost exactly $1^s{\cdot}5$ north of it, (a good observation). The comet being then allowed to run along the middle wire, so as to traverse a diameter of the field while the star traversed a chord, I found the times occupied to be respectively $57^s{\cdot}5$ and $25^s{\cdot}5$, which gives for their difference of declination $6' 24''$, the comet being south of the star. The star *w* is a conspicuous one, of fully the 9th mag., and there is no other which can be mistaken for it. A star of the 10th mag. precedes it about 20^s of time, and is about $2'\frac{1}{2}$ south of it; but except this there is no other considerable star within *at least* $10'$ in all directions. The comet's daily motion (obtained per Ephemeris) may therefore be safely used to identify this star. Admitting, then, that in the interval (26^h 15^m S.T.) elapsed since the last observation the comet had moved over $+6^m$ 20^s in Æ, and $-1° 10' 3''$ in decl., we get for the approximate place of the star *w* for 1832·0,

$$Æ = 10^h\ 21^m\ 52^s;\ \text{Decl.}\ +\ 6°\ 32'\ 55''."$$

Mr. Best presented a diagram of the path of the comet from Oct. 30 to Nov. 14.

II. On a Method of Ascertaining the Rate of Chronometers; especially when a strict examination of their performances is required. By Mr. James Epps, Assistant Secretary of the Society.

III. Observations of the Transit of *Mercury* of May 5, 1832; by M. Quetelet of Brussels; Mr. Henderson, at the Cape of Good Hope; Capt. Bayfield; Capt. R. Owen, at Crooked Island; Capt. Belcher, at Cavalho Island, Bijoogas; and M. Cacciatore.

IV. Observations of the Occultation of 104 *m Tauri* of October 13; and of *μ Ceti* of November 7, 1832. By Mr. Snow. Accompanied by the transits of the stars which were used to correct the clock.

1832. October 13. Observed in Saville Row.

Immersion of 104 *m Tauri* (5th mag.) 3^h 35^m $44^s{\cdot}7$ sid. time.

The star vanished gradually, as if it had been a small planetary body. Telescope, 45-inch refractor, power 180; adjusted to the star.

1832. November 7. Observed in Saville Row.

Immersion of *μ Ceti* (4th mag.) 23^h 35^m $41^s{\cdot}10$ sid. time.

Telescope 45-inch refractor, power 40, adjusted to the star. The star was very sharp and distinct; and although the immersion took place at the moon's dark limb, (the moon wanting about 12 hours of being full,) the star did not disappear until it actually reached the enlightened portion of the moon's disk: the star then vanished, *not quite instantaneously*, but suffered no previous diminution of light or change of colour.

V. Observations of the Occultation of *Saturn* of May 8; by Professor Airy and Mr. Maclear, at Cambridge; and by M. Cacciatore, at Palermo: the latter observed that on emerging, *Saturn* shone faintly for 20^s, and then resumed its usual splendour.

VI. Stars observed with the Moon at the Royal Observatory, Greenwich, in the months of June, July, and August, 1832.

VII. Observations of the Transits of the Moon with Moon-culminating Stars, made at Cambridge Observatory in the months of August, September, and October, 1832.

ZOOLOGICAL SOCIETY.

Proceedings of the Committee of Science and Correspondence.

September 25, and October 9, 1832.—Colonel Sykes resumed the exhibition of the collection of *Birds* formed by him in Dukhun. On previous evenings he had brought under the notice of the Committee the *Raptores* and *Insessores*; and on the present he submitted the remaining orders in the form of a "Catalogue of Birds (systematically arranged) of the Rasorial, Grallatorial, and Natatorial Orders, observed in the Dukhun by Lieut. Colonel W. H. Sykes, Bombay Army, F.L.S., F.Z.S., &c. &c." which is given in the "Proceedings" of the Committee. In this catalogue are enumerated 95 species, making together with 137 enumerated in the former catalogue 232, among which 14 of those enumerated in the present catalogue are new to science. Of these new species the specific characters, dimensions, and localities are given, throughout; together, in many instances, with observations on their habits, food, affinities, &c. Of the species already described by naturalists, specimens of which are contained in Colonel Sykes's collection, the synonymes and in some instances the native appellations are given; also references to figures, with remarks on their fidelity; in many cases the dimensions and the localities of the birds; and various remarks on their characters and habits, &c. We proceed to extract from Colonel Sykes's present Catalogue some articles which appear to possess peculiar interest.

142. *Columba risoria*, Linn. *La Tourterelle à collier du Sénégal*, Buff., Ois. 2, 550 & 553. pl. 26. Pl. Enl. 161 & 244. Le Vaill., Ois. d'Afr. 6. pl. 268.

Length, inclusive of tail, $13\frac{5}{10}$ inches: tail 5 inches. Gregarious, and common in the open country. Sexes alike. In spite of the proverbial gentleness of the Dove, Colonel Sykes has seen these birds fighting with the most inveterate hostility; seizing each other by the bill and rolling upon the ground together. Outer webs of 2nd, 3rd and 4th quill-feathers hollowed.

146. *Pavo cristatus*, Linn. *Peafowl. Mohr* of the Mahrattas.

The wild *Peafowl* is abundant in the dense woods of the Ghauts: it is readily domesticated, and many Hindoo temples in the Dukhun have considerable flocks of them. On a comparison with the bird as domesticated in Europe, the latter is found, both male and female, to be absolutely identical with the wild bird of India. *Irides* intense red brown.

181. *Phœnicopterus ruber*, Linn., 1. 230. *Le Flammant*, Buff., Ois. 8. 475. Pl. Enl. 63. *Red Flamingo. Rajah Huns* of the Hindoos.

Irides light yellow. Length, inclusive of tail, $43\frac{1}{2}$ inches: tail 6 inches.

In the *duodenum* of a female were found two thick, remarkable white worms composed of *annuli*; one 7 inches long, the other $4\frac{1}{2}$ inches; and filling up the intestinal canal, so that liquid food only could have passed; nevertheless the bird appeared quite healthy.

188. *Ibis religiosa*, Cuv., Règne Anim. 1. 483. *Sacred Ibis. L'Ibis sacre*, Cuv., Recherches sur les Ossemens Fossiles, 1. 161. *Tanta-*

lus Æthiopicus. Ibis Macei, Cuv., Ann. Mus. 11. 125. *White Ibis with purple black secondary quill decomposed feathers,* Ind. Orn. 2. 706.

Col. Sykes carefully compared the descriptions and measurements of the larger *Mummy Ibis* of Cuvier; and is induced to believe the present bird is the same. Col. Sykes puts into juxtaposition the measurements of Cuvier's *Mummy Ibis* from Thebes and one of his own birds:

	Mummy Ibis. Inches.	*Dukhun Ibis.* Inches.
Length of beak and head together	8·27	8·15
Head	1·85	1·80
Tibia	5·90	5·80
Tarsus	4·01	3·80
Middle toe	3·81	3·50
Ulna	6·01	5·95
Hand	4·92	4·80

The individual of which the measurements are given has the two first quills tipped with violet, their shafts of the same colour, and four of the secondary quills are also violet and with their webs decomposed, according with Cuvier's description. The violet colour is not so deep as in the *Æthiopian Ibis*; but as in all Col. Sykes's specimens (nine in number) the violet feathers are in progress of development, the colour would no doubt subsequently be darker. Cuvier mentions that the *Mummy Ibis* varied a little in size. Col. Sykes has birds larger and smaller than that of which the measurements are given.

Appear in Dukhun in the cold weather only. Gregarious.

Irides narrow, lake colour. Food water-crickets, crabs, beetles, shrimps. Length, inclusive of tail, 30 to $35\frac{1}{2}$ inches: tail $5\frac{4}{10}$ to $5\frac{7}{10}$. Bill and head to occiput $7\frac{8}{10}$ to $9\frac{6}{10}$ inches. Bill to the gape $6\frac{4}{10}$ to $7\frac{8}{10}$ inches.

189. *Ibis ignea. Tantalus igneus,* Lath., Ind. Orn. 2. 708. 12. *Ibis falcinellus,* Temm., Man. d'Orn., 2nd Edit. 2. 596.

Col. Sykes's birds, male and female, are identical with two European specimens in the British Museum labelled *Ibis ignea,* and viewed as the immature birds of *Ibis falcinellus.* Col. Sykes however has seen so many of both in India, appearing in different flocks at the same period of the year, and not having, as M. Temminck describes the birds before they are three years old, "partie inferieure du cou, poitrine, ventre, et cuisses d'un noir cendré; haut du dos et scapulaires d'un cendré brun," but of a rich fuscous brown, with brilliant metallic reflections; differing also in the proportions of the internal organization; and Dr. Latham moreover describes even the youngest birds of *Ibis falcinellus* as characterized by reddish brown. Herodotus speaks of the *smaller Ibis* as entirely black, a description inapplicable to the *Ibis falcinellus,* but applicable to the present species, which at a short distance appears entirely black. Col. Sykes is therefore induced to adopt the opinion of those writers who considered the bird distinct from *Ibis falcinellus.* Its measurements correspond with those of the smaller species of *Mummy Ibis* given by Cuvier; and it agrees in plumage (intense blackish brown with metallic reflections, without any mention of chestnut or marone, the livery of the *Ibis ignea,*) with the descriptions of the ancients; it is therefore very probable, as M. Temminck suggests, that it is the sacred species worshiped and embalmed by the Egyptians.

Length (male), inclusive of tail, 25½ inches: tail 4¼ inches. Female 23½ inches: tail 4 inches.

Black beetles, *larvæ* of water insects, and numerous univalve shells found in the stomachs of these birds.

191. *Ibis falcinellus,* Temm., Man. d'Orn. 2nd Edit. 2. 599. *Tantalus falcinellus,* Linn., 1.241. Gmel., 1. 648. *Le Courlis verd,* Buff., Ois. 8. 29. *Courly d'Italie,* Buff., Pl. Enl. 819. *Marone Ibis.*

Sexes do not differ in plumage; but the female is somewhat smaller than the male.

Length, inclusive of tail, 26 to 26½ inches: tail 4¼ inches. Multitudes of black beetles and grasshoppers, and univalve fresh-water shells, found in the stomach. An *immature* bird in possession of the Zoological Society, unlike the supposed immature bird (*Ibis ignea*), is characterized by the marone livery of the *Ibis falcinellus.*

228. *Plotus melanogaster,* Gmel. 1. 580. *Anhinga noir du Senegal,* Buff., Ois. 8. 453. Pl. Enl. 960 & 107. *Black-billed Darter,* called the *Snake-bird* in Dukhun.

Irides bright yellow. Length, inclusive of tail, 37½ inches; tail 9½ inches. Solitary. Rare in Dukhun, but frequently met with below the Ghauts. This bird has the singular faculty of being enabled to swim with the whole of its body under water, the long neck and head alone being visible, looking like a snake. Colonel Sykes's limits do not permit him to enlarge on the very peculiar formation of the stomach, more resembling that of a ruminant than a bird. Seven small carp and much deep-green vegetable fibre were found in the stomach of a female.

Colonel Sykes states, that the *domestic Duck* (*Anas Boschas*) is extensively bred by the Portuguese in Western India, and that it is subject to a kind of apoplexy, which carries it off in a few minutes, although previously in apparent health. He has known a trader lose a flock of more than thirty in the course of one day; and he has himself had ten ducks struck simultaneously, stagger about for a short time as if drunk, run round in circles, fall on their backs, and die. He has not been able to discover any morbid appearances in the brain. In no instance, in the stomachs of the *Anatidæ,* were animal matters met with; the contents consisted of grains, seeds, vegetables, and gravel.

Colonel Sykes, in closing his Catalogue of the birds of Dukhun, mentioned that the details he had given resulted from personal observation of the specimens, in a living or recent state. With few exceptions, the whole were shot by himself; and, to guard against false impressions, he accumulated several individuals of the same species and of both sexes, and was rarely confined to a solitary bird.

XXXVIII. *Intelligence and Miscellaneous Articles.*

OBSERVATIONS ON TWO ARCHES OF AURORÆ BOREALES.—FROM R. POTTER, ESQ.

THE observations I have to offer on these two appearances of the aurora borealis, are not such as would be necessary for very accurate calculations of the height of the meteor; they will nevertheless suffice for determining this height approximatively if contemporaneous observations should chance to have been made in places properly situated for the purpose. Indeed, if we adopt the hypothesis, as already sufficiently demonstrated, that the arches are portions of luminous rings round the magnetic axis, we may reduce the observations of any two localities to a common magnetic meridian, when the arch has not exhibited any deviation from a regular curvature.

The last arch of those below having been nearly stationary for a considerable length of time, I hope it may have been observed in Scotland; and if Mr. Wharton of Dryburn near Durham shall have seen it, I am sure he will in such case not have failed to have ascertained its apparent altitude with an instrument. Though this gentleman's observation, combined with mine, would not be adequate, from the too great proximity of the places of observation, for even an approximate calculation of the height, yet by using the reduction mentioned above, Mr. Wharton's observation, combined with a similar one taken in the neighbourhood of Melrose or Edinburgh, would go far to settle finally the interesting question as to the region in which this meteor takes place.

Observation 1.—The Sunday, 23rd Sept. 1832, was remarkably calm at Smedley, situated about $1\frac{3}{4}$ English mile to the north of the spire of St. Mary's church in Manchester, of which the latitude and longitude are on record; in the evening the sky was generally clear, though somewhat hazy near the horizon,—there was an appearance of a faint aurora. A faint arch, which had appeared, had at fifty-five minutes past seven o'clock an altitude of about 20° for its *middle* breadth; at ten minutes past eight its altitude was about 25°. It afterwards rose still higher, but was become almost imperceptible at half-past eight. A few faint streamers were seen, and more light on the horizon. The arch was not complete, reaching only from about the true north to the western horizon: its great faintness prevented me taking a better observation and measuring its breadth. There had been an auroral light visible in the north for several evenings, but the sky was cloudy.

Observation 2.—On my return from a short visit to Sir David Brewster at Allerly, when I arrived on the 21st Dec. 1832, by the Chevy-chase coach, to within thirteen miles of Newcastle-upon-Tyne, I perceived, on looking towards the north-north-west, that there was a very distinct arch of an aurora; it was just seven o'clock in the evening when I first saw the arch, and I immediately set about to estimate the altitude of its highest point, which I called to be nearly 11° for its under edge, and its breadth to be about 4°. I was rather unfavourably seated for watching the arch continually, and it was only some time after that I perceived the arch to have become double; by looking more frequently I found this appearance to arise from another arch which was rising above the former, and which after attaining a considerable altitude became irregular in form and disappeared, as well as some other irregular appearances. I noticed the first arch during about an hour and a half, and though it was often very faint, and had almost entirely disappeared, yet when it became bright again, it always had very nearly the same altitude as at first: I, however, sometimes believed it to be a little less if at all different.

The next evening when I had arrived on the high ground of Stanedge, between Huddersfield and Oldham, there was a promise of a fine display, but the sky became immediately densely overcast, so that no after-opportunity of observing occurred. The appearance on this occasion was different to any I had before seen; the sky in the magnetic north appeared thickly covered to a considerable height with minute fleecy clouds of bright auroral light; this uncommon appearance made me regret having arrived within the influence of the cloudy atmosphere of Lancashire. I found that the evening of the 21st of December had also been cloudy in the neighbourhood of Manchester.

ANALYSIS OF GUMS.

In our Number for September last, we gave the analyses of several gums by M. Guerin; we now continue them.

Cherry-tree Gum. Specific gravity 1·475, colourless or coloured like gum arabic; it is in rounded pieces of various sizes, like those of gum Senegal, or stalactitic. It frequently contains fragments of wood in its interior; reddens litmus-paper, and has sometimes an acid taste. Heat, light and chlorine act upon it as they do upon gum arabic; and the action of alcohol, and of sulphuric and nitric acids, is also similar. When pieces of this gum are put into cold water, they swell and partially dissolve only, whatever may be the quantity of water; but by boiling for some hours they dissolve perfectly. According to Dr. Thomson, alcohol occasions no precipitation in cherry-tree gum, but M. Guerin asserts that precipitation always takes place.

One hundred parts cherry-tree gum heated with 400 parts of nitric acid, yielded 15·54 parts of mucic acid and oxalic acid. By analysis it yielded—

Arabin	52·10	and	Carbon	43·69
Cerasin	34·90	—	Oxygen	50·08
Water	12·00	—	Hydrogen	06·23
Ashes	1·			100·00
	100·00			

The ashes contain the same substances as those of gum arabic, except sulphate of potash.

M. Guerin prepares cerasin by exposing one part of cherry-tree gum to the action of 400 parts of water for twelve hours at the temperature of 65° Fahr. The mixture is to be frequently stirred, and the water is to be poured off, and the same quantity again added; this operation is to be repeated till all the soluble matter is dissolved. The residue is then to be drained on a cloth and dried in a water-bath. M. Guerin remarks that Dr. Thomson has confounded that part of cherry-tree gum and native gums which is insoluble in cold water, with gum tragacanth, under the common name of Cerasin; but they ought to be distinguished, for the insoluble part of native gums yields nearly the same proportion of mucic acid as arabin, whilst bassorin gives more; the former also is dissolved and changed into arabin by boiling water, whilst the latter suffers no alteration by it. The name of *cerasin* is reserved by M. Guerin for that part of native gums which is insoluble in cold water, and he thinks that it may be considered as isomeric with arabin.

One hundred parts of cerasin consist of

Cerasin	90·60
Water	8·40
Ashes	1·00
	100·

The ultimate analysis of cherry-tree gum, which contains nearly 35 per cent. of cerasin, differs scarcely at all from that of gum arabic. Cerasin is insoluble in alcohol, and does not undergo the vinous fermentation; it swells slightly in cold water, without dissolving. A quantity of cerasin was boiled in water during six hours; the transparent liquor obtained was evaporated to dryness in a platina capsule. The residue was composed of

Arabin	90·587
Water	8·402
Ashes	1·011
	100·

M. Guerin found that water at 65° Fahr. dissolved 13·15 per cent. of it, and at 212°, 19·03; and he considers the cerasin as converted into arabin by the action of the heat, and this transformation he thinks may be accounted for, by considering the circumstances under which gums are formed. Gum arabic and Senegal flow from certain trees in hot countries, which probably occasions the formation of arabin; while in colder climates cerasin is formed, which by the heat of boiling water becomes arabin.

The following are given by M. Guerin as the composition of the gum of the annexed trees.

	Apricot.	Plum.	Peach.	Almond.
Arabin and cerasin	89·85	82·23	82·60	83·24
Water	6·82	15·15	14·21	13·79
Ashes	3·33	2·62	3·14	2·97
	100·	100·	100·	100·

EXAMINATION OF SUGAR OF MILK.

M. Guerin remarks that gum and sugar of milk are the only substances which by being heated with nitric acid yield mucic and oxalic acids; and he thought it desirable to ascertain which of them yielded the most. Water at 65° dissolved 10·91 per cent. of sugar of milk, and at 212°, 96·70 per cent.; 100 parts of it heated with 600 parts of nitric acid gave as a maximum product 28·62 of mucic acid, mixed with oxalic acid; gum Senegal, which consists of arabin and water, gave only 16·70 per cent. of mucic acid mixed with oxalic; but then sugar of milk contains less than one per cent. of water, while gum Senegal contains 16·1 per cent. and 2·78 per cent. more of ashes.

Mucic acid, obtained either from sugar of milk or gums, when dissolved in boiling water, crystallizes on cooling in small scales, which present on their edges small crystals, which appeared to be prisms with a rectangular base.

SUPPOSED ARTIFICIAL MALIC ACID.

M. Guerin observes that Scheele obtained a peculiar acid, which he called Malic acid, by the action of nitric acid upon mucilage. Fourcroy and Vauquelin repeated these experiments, and described a new uncrystallizable acid, which they considered as identical with the malic acid of fruits, this acid not having then been obtained in a crystalline state.

In order to prepare this artificial malic acid, M. Guerin employed the following process: one part of gum arabic was treated with two parts of nitric acid, diluted with half their weight of water; the mixture was heated moderately, until all the gum was dissolved, and the solution was then slowly boiled for two hours. After dilution with water it was neutralized with ammonia; muriate of lime was then added to precipitate the oxalic acid formed, and the whole was thrown on a filter; the filtered liquor was yellowish red, and solution of nitrate of lead was added to it; a yellowish precipitate was obtained, which, after being well washed, was decomposed by a current of sulphuretted hydrogen gas, and the acid liquor was evaporated with a gentle heat; this was again saturated with ammonia, and decomposed by nitrate of lead; and the precipitate decomposed by sulphuretted hydrogen, gave an acid liquor, which, though evaporated to the consistence of a syrup, gave no crystals.

The properties of this acid are, that it is slightly yellow, reddens litmus, its taste resembles that of malic acid, is inodorous, and more dense than water. It is very soluble, both in water and in alcohol; it causes precipitation in lime, barytes, and strontia water, which is redissolved by excess of acid. The salts of lead give a bulky precipitate with it, which is insoluble in cold water, and in excess of the acid; boiling water dissolves a small portion, which crystallizes as the solution cools. When this acid is neutralized by ammonia, and heated, an acid salt is formed, which crystallizes in colourless prisms with a rectangular base. Its taste is slightly acid; cold water dissolves it sparingly, but boiling water readily. It is insoluble in alcohol. This acid may be obtained by treating one part of sugar or of starch with half a part of nitric acid, in the same manner as already described with gum. M. Guerin concludes that this acid is not the malic, as has generally been supposed, but that it is a new acid, perfectly distinct from all others.—*Ann. de Chim. et de Phys.* tom. xlix. p. 274.

ON THE CHEMICAL AGENCY OF WATER. BY M. PELOUZE.

Anhydrous alcohol, sulphuric æther and acetic æther, disguise, more or less completely, the properties of the strongest acids. Their solution does not redden litmus, nor decompose a great number of carbonates. A mixture of about 6 parts of absolute alcohol and 1 part of concentrated sulphuric acid does not act upon any neutral carbonate, but it immediately decomposes acetate of potash, and disengages abundant vapours of vinegar mixed with acetic æther.

It is well known, since the labours of Hennell and Sérullas, that sulphovinic acid is formed in the cold, in a mixture of alcohol and concentrated sulphuric acid, but whatever may be the excess of alcohol employed, free sulphuric acid remains in the mixture. It is therefore reasonable to conclude, from the above-mentioned experiment, that an alcoholic solution of sulphovinic and sulphuric acid is incapable of decomposing a carbonate; water must be added that the action may occur.

A solution of muriatic acid gas in alcohol, so concentrated that when diluted with several hundred times its volume of water it reddens litmus-paper, attacks artificial carbonate of lime and even marble itself with extreme violence. It also attacks, but less strongly, the carbonates of barytes, strontia, magnesia, and soda, even wheu they have been previously calcined; but on the contrary it does not decompose carbonate of potash. Concentrated nitric acid mixed with alcohol does not decompose carbonate of potash; it acts energetically upon the carbonates of lime and strontia; those of barytes, magnesia, and soda are also attacked, but much more slowly.

Vegetable acids produce similar effects; the tartaric, paratartaric, citric, and oxalic acids all dissolve in notable quantity in alcohol; the solution of the two first did not act upon any of the numerous carbonates with which it was put in contact. The alcoholic solution of citric acid does not act upon the carbonates of strontia, lime, or barytes, but it attacks the carbonates of potash and magnesia, but the latter with extreme slowness.

Oxalic acid, which disengages carbonic acid from the carbonates of strontia, magnesia, and barytes, does not act at all upon carbonate of potash or of lime. These facts show, that on many occasions in which alcohol is employed in chemical investigations, it will prevent the operator from discovering the presence of an excess of acid by litmus-paper.

M. Pelouze remarks that some of the facts cited may be satisfactorily explained, while others are quite inexplicable. What is the reason, for example, why concentrated acetic acid does not act upon carbonate of lime, while it combines so energetically with caustic lime? Why is water required in the first case and useless in the second? —for in both cases the same product is obtained. Thus acetic acid dissolved in alcohol, and acetic acid dissolved in water, may be considered, with relation to certain bodies, chalk for example, as acids entirely distinct from each other. Acetic acid dissolved in alcohol is to the carbonates, what carbonic acid is to the acetates dissolved in alcohol; that is to say, in one case there is no action, and in the other it is strong.

Chloride of strontium, chloride of copper, and nitrate of copper when dissolved in alcohol, were not decomposed by exposure to a long-continued current of carbonic acid gas.

The presence of water does not appear to be always necessary to chemical action; in many cases it may occur with other solvents. Oxalic acid dried under the receiver of the air-pump and dissolved in absolute alcohol, precipitates a similar solution of nitrate flame. Sulphocyanate of potash gives as deep a colour to permuriate of iron dissolved in alcohol as in water.—*Ann. de Chim.* tom. l. p. 434.

Summary of the State of the Barometer, &c. in Kendal, for 1832.

Months.	Barometer.			Thermometer.			Quantity of Rain in Inches.	Wet Days.	Prevalent Winds.
	Max.	Min.	Mean.	Max.	Min.	Mean.			
Jan.	30·14	29·18	29·70	47°	20°	36·17	2·278	14	W.
Feb.	30·34	28·86	29·80	51	25	36·96	4·258	10	W.
Mar.	30·06	28·97	29·61	56	28	42·47	3·549	14	W.
April	30·28	29·25	29·80	62	30·5	46·76	2·235	10	NE.
May	30·26	29·00	29·74	67	30·5	49·89	1·602	8	N.
June	30·21	29·22	29·63	69·5	43·5	57·50	4·643	16	W.
July	30·18	29·47	29·89	72	43	59·41	2·639	10	NW.
Aug.	29·98	29·13	29·63	68·5	41	58·16	4·433	21	W.
Sept.	30·30	29·46	29·86	64	42	54·62	2·295	8	W.
Oct.	30·22	28·77	29·79	62	35	49·79	8·346	22	SW.
Nov.	30·30	29·13	29·62	53	25·5	39·86	5·373	16	NW&W.
Dec.	30·18	29·08	29·66	50	25	39·59	8·037	18	SW.
Annual Means, &c.	30·20	29·13	29·73	60·18	32·42	47·68	49·688	167	W.

There have been very few meteorological peculiarities observed during the year 1832. The annual mean of the barometer exceeds in a trifling degree that of several years past. The annual average of the thermometer is less than that of 1831, but greater than the mean of several preceding years. The cholera made its appearance in Kendal about the beginning of July, or the latter part of June. From that time to its disappearance in August, the weather was not distinguished by any peculiarity, by any sudden changes, or any excess or diminution of temperature, beyond what is common in this place at the same season of the year. The average temperature of July, August, September, and October, in this year is 55°·99; and taking the average temperature of the same months in the preceding seven years, we find it to be 55°·23, showing that the difference in temperature is very trifling. We had no frost from the 5th of May till the 7th of November. The aurora borealis has not been seen this year so frequently as last, and was very rarely noticed till the last three months, when there were several splendid appearances of it. The most extraordinary was seen on the evening of the 1st of November, when several bows of light, accompanied with streamers, were observed; but contrary to any preceding observation, the bows, instead of crossing the *magnetic* meridian at right angles, and having the *magnetic* north as their centre, crossed the *true* meridian at right angles, and had the *true* north as their centre. As there were several bows seen during the course of the evening, it was deliberately noticed that each had the *true* north in its centre. This variation from the general law of the arches which sometimes accompany the aurora borealis is unprecedented, so far as the reading or observation of the writer can determine. Till the end of September, the quantity of rain measured was much below what is usually taken in that period of the year. In the last three months the quantity, as will be seen by inspecting the preceding table, amounts to 21·756 inches,—a quantity equal to the annual amount which falls in many parts of England. The average annual amount of rain for Kendal for the last eleven years is 56·309 inches,—a quantity which of course will vary every year; but the annual average for Kendal may be fairly stated at about 57 inches. The greatest quantity of rain measured, which had fallen in the preceding twenty-four hours, was 2·882 inches on the 25th of December, but the real quantity was more than 3 inches; as the bottle which receives the rain was running over when noticed, and had evidently lost much in that way. On that day the river was so much swollen that we had the highest flood known for many years, except on the 9th of February 1831, which flood was partly occasioned by the sudden melting of snow, the ground being too much frozen to allow of its draining gradually from the neighbouring country. The greatest quantity measured in this town for the last eleven years, before the date first mentioned, was scarcely $2\frac{1}{2}$ inches in any twenty-four hours. The most prevalent wind during the last year has been West.

LUNAR OCCULTATIONS FOR MARCH.

Occultations of fixed Stars by the Moon, visible at Greenwich in the Year 1833. *Computed by* THOMAS MACLEAR, *Esq.; and circulated by the Astronomical Society.*

1833.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions.				Emersions.			
				Sidereal time.	Mean time.	Angle from North Point.	Angle from Vertex.	Sidereal time.	Mean time.	Angle from North Point.	Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
Mar. 10	46 θ Libræ	4·5	1811	17 51	18 37	126	146	18 37	19 22	203	229
12	58 *D* Ophi.	5	2032	20 11	20 48	118	141	21 11	21 48	236	267
14	(61) Sagitt.	6	2241	15 31	16 1	97	72	16 49	17 19	259	242
27	125 Tauri	6	688	5 58	5 39	125	133	7 7	6 47	253	276
31	8 Leonis	6·7	1163	7 11	6 35	125	101	7 55	7 19	201	191
April 1	53 *l* Leonis	6	1284	14 37	13 56	69	104	15 38	14 57	241	279
4	80 β Virgin.	6	1551	14 56	14 3	86	100	15 59	15 7	218	241

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

Days of Month, 1832.	Barometer.					Thermometer.					Wind.			Rain.		
	London.		Penzance.		Boston	London.		Penzance.		Bost.	Lond.	Penz.	Bost.	Lond.	Penz.	Bost.
	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	8½ A.M.						
Jan. 1	30·581	30·404	30·440	30·388	30·03	45	35	51	39	30	SW.	W.	calm	0·06	0·300	...
2	30·201	30·052	30·308	30·084	29·65	47	40	49	43	41	S.	W.	calm	·08	·800	0·02
3	30·587	30·455	30·548	30·460	30·04	42	35	45	38	39	NE.	NE.	calm	...	...	.20
4	30·608	30·536	30·546	30·534	30·24	42	29	41	35	38	NE.	NE.	calm	...	...	...
5	30·441	30·377	30·346	30·300	30·06	38	26	41	34	33·5	NE.	NE.	calm	...	...	...
○ 6	30·604	30·480	30·428	30·346	30·04	36	24	44	35	32	W.	SE.	calm	...	...	...
7	30·682	30·602	30·528	30·496	30·20	36	32	46	38	32	SW.	SE.	calm	...	...	...
8	30·732	30·727	30·546	30·522	30·33	37	30	45	40	36	NE.	SE.	calm	...	...	...
9	30·660	30·512	30·460	30·328	30·21	32	26	47	40	36	S.	SE.	N.	...	...	...
10	30·333	30·116	30·084	29·828	30·00	34	25	48	40	32	E.	SE.	calm	...	·250	...
11	29·980	29·874	29·740	29·728	29·69	39	30	48	45	30·5	E.	SE.	calm	...	...	...
☾ 12	29·901	29·899	29·628	29·628	29·61	40	36	48	43	31·5	NE.	SE.	calm	·05	·280	...
13	30·136	29·948	29·928	29·690	29·65	41	38	50	43	38	E.	E.	calm	·04	·300	·09
14	30·255	30·149	30·048	30·031	29·94	42	34	49	44	39	E.	E.	calm	...	...	...
15	30·221	30·189	30·051	30·048	29·83	40	36	45	43	39	E.	NE.	calm	...	...	...
16	30·253	30·230	30·134	30·090	29·87	40	36	43	38	39	NE.	NE.	calm	...	...	...
17	30·196	30·119	30·146	30·146	29·78	42	34	43	36	36·5	NE.	NE.	calm	...	...	...
18	30·226	30·120	30·160	30·152	29·79	37	35	42	37	37	NE.	NE.	calm	·02	...	·09
19	30·304	30·300	30·160	30·160	29·94	37	35	42	38	38	NE.	NE.	calm	...	...	...
● 20	30·366	30·308	30·166	30·146	29·93	38	28	41	38	38	NE.	NE.	calm	...	...	...
21	30·377	30·352	30·155	30·146	30·10	38	29	40	36	34	SE.	SE.	calm	...	...	...
22	30·554	30·440	30·396	30·249	30·11	38	24	41	37	35	SE.	E.	calm	..	...	...
23	30·783	30·608	30·446	30·446	30·22	35	22	45	36	31·5	SE.	SE.	calm	...	...	...
24	30·550	30·484	30·343	30·290	30·22	37	25	45	36	25·5	E.	E.	calm	...	...	...
25	30·303	30·201	30·237	30·234	29·97	36	29	48	40	33	SE.	E.	calm	...	...	...
26	30·179	30·153	30·237	30·234	29·76	39	28	48	41	35	SE.	NW.	calm	...	...	..
27	30·211	30·066	30·266	30·234	29·74	42	35	47	40	36	SW.	NW.	calm	...	...	...
28	30·020	29·803	30·057	29·584	29·60	46	37	48	38	37	SW.	SW.	calm	·12	1·060	...
☽ 29	29·367	29·331	29·428	29·287	29·10	46	35	48	43	40	NE.	NW.	calm	·01	0·385	·24
30	29·794	29·573	29·884	29·696	29·22	39	28	45	40	36	N.	N.	NW.	...	..	...
31	29·788	29·443	29·690	29·584	29·35	37	34	48	38	30·5	S.	NW.	calm	·14	·350	...
	30·783	29·331	30·584	29·287	29·87			51	34	35·1				0·52	3·725	0·64

Remarks.

London.—Jan. 1. Foggy: fine: rain at night. 2. Drizzly: stormy and wet. 3. Hazy and cold. 4. Cold and Cloudy 5, 6. Clear and frosty. 7. Hard frost. 8. Dense fog. 9. Hazy. 10. Frosty. 11. Foggy. 12. Frosty and foggy. 13. Drizzly and foggy. 14. Foggy. 15—17. Cold and overcast. 18. Drizzly. 19, 20. Cloudy and cold. 21, 22. Clear and frosty. 23. Frosty with dense fog: barometer very high. 24. Frosty: fine. 25, 26. Frosty and foggy. 27. Fine. 28. Fine: rain at night. 29. Rain. 30. Fine: at noon, cold and wet: clear and frosty at night. 31. Hazy: rain.

Penzance.—Jan. 1. Fair: rain. 2. Rain: fair. 3. Clear. 4. Fair. 5. Fair: misty. 6. Fair. 7. Misty: fair. 8, 9. Fair. 10—12. Fair: rain. 13. Rain. 14. Misty. 15—22. Fair. 23. Fair: misty. 24. Clear. 25. Fair. 26. Fair: showers. 27. Showers: fair. 28. Fair: rain. 29. Rain. 30. Fair. 31. Rain: fair.

Boston.—Jan. 1. Fine: rain P.M. 2. Cloudy: rain P.M. 3, 4. Cloudy. 5. Fine. 6. Cloudy. 7. Fine. 8—10. Cloudy. 11, 12. Fine. 13. Rain. 14—17. Cloudy. 18. Cloudy: rain early A.M. 19, 20. Cloudy. 21—24. Fine. 25, 26. Cloudy. 27. Fine: brisk wind P.M. 28. Cloudy. 29. Cloudy: rain early A.M. 30. Snow. 31. Fine: snow P.M.

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XXXIX. *Notice of a Submarine Forest in Cardigan Bay. By the Rev.* JAMES YATES, *M.A. F.L. & G.S.**

ALTHOUGH several ancient Welch writers, as well as some modern antiquaries, have alluded to the submarine forest in Cardigan Bay, I believe no geologist has given any account of it, if we except a brief reference in Mr. Arthur Aikin's Tour in North Wales (p. 56.).

This forest extends many miles along the coast of Merionethshire and Cardiganshire, being divided into two parts by the estuary of the Dovey. The northern portion, extending towards Barmouth, is considerably longer than the southern, which extends in the direction of Aberystwith.

The coast is here covered with sands, dry at low water, which seem to owe their formation to the junction of the river Dovey with the sea. *Borth Sands*, to which I shall chiefly confine my description, constitute the southern and shorter of the two divisions.

These sands are bounded on the land-side by a wall of shingles, extending about two miles from Borth to Moel-Ynys. The waters of the river Lery partly discharge themselves into the sea by oozing through the wall, and are in part diffused over a tract of bog and marsh land, which forms the north-western angle of Cardiganshire.

The submarine forest consists of low stumps of trees, which evidently retain their original position. Some of them have long curling roots, which radiate over the surface of the shore. The stratum of soil, in which they grew, is now covered with a bed of peat, and the stems of many of them are seen rising

* Read before the Geological Society of London; and communicated by the Author, with the permission of the Council.

through it. The peat is extensively cut for fuel by the poor people on the coast. Both the decayed wood and the peat are penetrated very generally by the *Pholas candida*. I found the wood to be inhabited also by multitudes of the *Teredo navalis*, although I did not observe any specimen of the latter animal in the peat.

The wood is found in every stage of decay. The bark remains in many instances around the trunks, much less altered than they are by time.

Among the species of wood, the most easily distinguishable is that of the *Pinus sylvestris*, or Scotch Fir. This is so little decayed as to be sometimes used for the purposes of carpentry. Dr. Bostock informs me, that he has seen the wood of Scotch fir, together with birch and oak, taken out of Barton Moss, a few miles to the north of Liverpool. Of the occurrence of Scotch fir, in others of the Lancashire mosses, as well as in fens and submarine forests in Cheshire, Yorkshire, and Lincolnshire, abundant evidence is afforded by the testimony of the Rev. Abraham De la Pryme, (Phil. Trans. vol. xxii. p. 980, &c. vol. xxiii. p. 1073), of Evelyn in his Silva, (ch. xxii. p. 298, ed. Hunter,) of the Rev. John Whitaker in his History of Manchester (vol. i. p. 310), and of Dr. Correa de Serra in the Philosophical Transactions for 1799. The facts stated by these authors prove, that about the commencement of the Christian æra there were in the above-named counties extensive forests of Scotch fir. It appears, however, to have been confined to low marshy situations, as I find no evidence of its growth upon any of the more hilly parts of the country. In Hatfield Chace, in the south of Yorkshire, there were some of these native firs remaining until about the middle of the seventeenth century, and the last of them was cut down only thirty years before Mr. De la Pryme sent his paper to the Royal Society. Thus we see that the natural order of Coniferæ may be traced in the English strata from the geological æra of the Independent Coal Formation to within two hundred years of our own time, although the Scotch fir, the last of the tribe, is now excluded by botanists from the living Flora of the country.

Another kind of wood, which is found in the submarine forest of Cardigan Bay, and which seems to be amentaceous (either birch, alder, or willow), is much more thoroughly decayed. But the solvent and corroding powers of the water have acted only on the sap, resins, gums, or other contents of the vessels of the wood, and not upon the vessels themselves. The vascular tissue seems to remain quite uninjured. After breaking off a portion of this wood I could, with my hand, squeeze the water from it as from a sponge, and was struck with the small space it occupied after such a pressure. It seemed obvious, that if these trunks had been thrown down instead of standing upright, they would have assumed under the weight of superincumbent masses of earth the flat ribbon-like form, which is usual in the native charcoal of the carboniferous strata; and it appeared that the preservation of the delicate structure of the wood with its cells and vessels, notwithstanding the removal of all the vegetable principles contained in them, might throw light on the process of petrifaction.

I am informed that the appearances are exactly similar in the northern portion of the submarine forest, which I have not visited. A natural mound or wall of shingles in this, as in the portion above described, separates the sands and submarine forest from a tract of marsh and bog, which owes its present condition to the waters of a stream, partly arrested by the mound.

With regard to the origin of these appearances, I see no reason to have recourse to any subterranean movement in order to explain them. The wall of shingles, which now forms a natural rampart between the sandy shore with its peat and submarine forest on the west, and the tract of peat and marsh on the east, must be supposed, notwithstanding its great dimensions, to be liable to changes of position. Placed further seaward, it would inclose the tract, which is now submarine; and if after the growth of the forest, its destruction, and the formation of peat upon its remains, the sea made a breach in the rampart, the present state of things would be in no long time the result, as the difference of level is inconsiderable between the submarine portion and that which is remote from the influence of the waves, and the slight difference of level which exists may, if necessary, be accounted for from the percolation of the same waters which produce the marsh and bog, and which might gradually carry away portions of the substrata.

The tract which I have been describing is called by the Welch, *Cantrev Gwaelod*, which means *the lowland hundred.* According to their ancient memorials it was submerged by an irruption of the sea about the year 520 of the Christian æra. In the *Triads of Britain*, one of the most important records, this disaster is imputed to the folly of "Seithenyn, the drunkard, who in his drink let the sea over the Cantrev Gwaelod." The representations of the bards respecting the importance and opulence of this district are probably exaggerated; but I see no reason to dispute their testimony either respecting the fact of the irruption of the sea by destroying the ancient dykes, nor with regard to the period which they assign as the date of that event*.

XL. *Narrative of Experiments made with the Seconds Pendulum, principally in order to determine the hitherto unassigned Amount of the Influence of certain minute Forces on its Rate of Motion.* *By Mr.* JAMES SCRYMGEOUR †.

THE author's object in making the experiments detailed in the following narrative, was so much ramified in the course of his researches, that it will be best understood after their perusal.

Suffice it at present to say, that he had in view to ascertain the influence of the maintaining power, or escapement on the time of the vibrations of the pendulum in which no recoil is produced; and likewise the effect of the resistance of the air upon the vibrations, either when the pendulum is attached to a clock, or when it is in a detached state.

The results of these experimental researches will be found applicable to all the contrivances or escapements for giving the impulse to the pendulum without recoil. As recoil was considered to introduce sources of irregularity, it was deemed unnecessary to inquire into its effects, as they would be indefinite and would vary with the quantity of recoil; besides, such experiments would be applicable to the clock employed only.

In pursuing this inquiry, my first object was to make the suspending spring of two of my clocks (which had both dead escapements) such as to cause the long and short vibrations to be performed in equal times. For this purpose, I shortened the acting part of the suspending spring of one of them, which, for the sake of distinction, I shall call my shop time-piece. This clock has a common lenticular bob with a wooden rod; it had been going for five or six years previously, with the suspending spring made in the usual way, of a piece of middle-sized watch main-spring about $\frac{3}{4}$ of an inch in length. The bob is only about 4 pounds in weight, with a small brass ball below it, nearly $\frac{1}{2}$ of a pound in weight.

The action of the suspending spring was shortened by fixing a piece of steel on each side of the spring, and joining it to the rod, so as to reduce the acting part of the spring to

* See the Cambro-Briton, for June 1820, vol. i. p. 361; Edward Williams's Lyric Poems, vol. i. p. 78. note; Davies's Mythology and Rites of the Druids, p. 240, &c.; Meyrick's Hist. and Antiquities of the County of Cardigan, p. 50—80.

† Communicated by the Author.

about $\frac{1}{8}$ of an inch in length. I was so fortunate as to obtain my object on the first trial; for when the vibrations were made to vary from 1°·5 to 2°·5, on each side of the point of rest, they were equal to within less than half a second in 24 hours.

I next adjusted the suspending spring of the other clock, which had a mercurial pendulum, and a suspending spring nearly $\frac{1}{2}$ of an inch in breadth, and $\frac{1}{120}$ of an inch in thickness. A moveable clamp-piece was made to fasten near the top of the pendulum rod, so as to be shifted up and down at pleasure, and to clasp the suspending spring. By various trials I found, that for this strength of spring and weight of pendulum, the acting part required to be about $\frac{1}{20}$ of an inch in length.

After both clocks had been going for some time, I was surprised to find that each of them experienced a losing rate, but particularly the shop time-piece with the light bob. Since that period, which is about four years, I have had ample opportunities of satisfying myself that this loss of rate originated in the weakening of the spring.

The clamping or fastening of the spring in the manner described, without any shortening of the absolute length of the pendulum, occasioned the clock having the light pendulum to gain about 3 minutes in 24 hours. I therefore lowered the pendulum bob, and brought it to time. About a month afterwards, I found it losing on its rate, and screwed up the pendulum bob. It is now (at the distance of nearly four years) screwed up to about half of the distance it was lowered at first; a circumstance which shows how much the spring has weakened since that time. The clock frequently maintained a uniform rate for three or four weeks successively; after which it began to lose on its rate, particularly when any sudden change of temperature occurred.

Pendulums with thin suspending springs (which most of the best clocks have) will not be subject to this variation, after having gone for some time, at least as far as my experience enables me to judge.

The clock just mentioned was lately cleaned. Before cleaning, it lost about a second in 24 hours, and the extent of its vibrations was reduced to 2°; after cleaning, the extent of its vibrations was 2°·5, its original extent when clean; it then gained 5 seconds in 24 hours, making a difference of 6 seconds all together. The cause of this difference was, less friction on the pallets.

The impulse being given in the ascent occasioned all the friction that affected the time to be in the descent, which consequently retarded the motion and caused the time to be slower. The friction beyond the escaping point being equal in ascent and descent, occasions no difference in time.

I next put a lighter maintaining power on the clock, which reduced the extent of vibration to 2°, its extent before it was cleaned; but it still gained 5 seconds in 24 hours. This result shows that the spring was such as to cause the vibrations to be equal in time, and that the loss in its rate before cleaning was occasioned by friction.

I was now desirous of ascertaining the rate of the pendulum of this clock in a detached state. For this purpose, I suspended a detached pendulum, with an adjusted suspending spring, close in front of the clock, and adjusted it so as to vibrate with the clock pendulum. I then detached the pallets from the crown-wheel. The collet to which the pallets were riveted being fixed to the arbor by a screw, they could thus be put out of action, clear of the wheel, and fixed again upon the arbor. In this construction, the pallets, with the arbor and back-fork, form part of the pendulum.

The pendulum was put in motion by the hand, a little beyond its extent of vibration, so that the mean extent in the detached state should be the same as when attached to the clock. The experiment showed that it gained upon the detached pendulum. I then adjusted it to the detached pendulum, and put it in action with the work as before. It now lost about $2\frac{1}{2}$ seconds in 24 hours upon its former rate. This experiment, therefore, showed the influence of the escapement upon the time of the vibrations, which was occasioned, conformably to what has been already stated, by the remaining friction upon the pallets in the descent, in consequence of the impulse being given in the ascent.

It is proper to mention here, that the bob of this clock traversed close to the back of the case; and the resistance thus occasioned, accounts for part of the loss of $2\frac{1}{2}$ seconds, as will be shown hereafter.

The same experiment was next tried with the clock having the mercurial pendulum, of about $13\frac{1}{2}$ pounds in weight, and furnished with the strong adjusted suspending spring. The result showed a loss amounting to within 2 or 3 tenths of a second of the loss of rate exhibited in the former experiment.

In order to give the impulse in the descent instead of the ascent, the following means were adopted. The flanches of the pallets were hollowed by forming them thus:  with a small cylindrical lap of about $\frac{1}{8}$ of an inch in diameter. These pallets were made separate, and being jointed upon their axes of motion, the distance between them was regulated by a tangent-screw, so that they could be adjusted to any depth in the teeth of the wheel. This shape afforded the means of giving about three fourths of the impulse in the descent. Experiment showed that the clock now gained about 2 seconds in 24 hours. The extra friction in this case was in the *ascent*.

It is known that if a pendulum have at the point of rest impulse sufficient to reach 2°, with a certain quantity of friction, it will require a greater impulse with a greater quantity of friction. The greater impulse will of course cause the first increments of motion to be quicker in the latter case than in the former; but this incidental quickness will have gradually decreased, and vanished when the pendulum has arrived at the extent of 2°; and consequently, the entire vibration will be quicker. Pendulums having strong adjusted suspending springs require a maintaining power in weight about double of that which they would require with a thin spring, or on a knife-edge, in order to make them vibrate to the extent of 2°.

The change of rate occasioned by the use of strong suspending springs, led to the consideration of some other practical means for obtaining isochronism, cycloidal cheeks being admitted to be objectionable in practice. It occurred to me that by placing a cylinder, or a portion of it, at the point of suspension of a pendulum, and making it roll on a plane, the point of oscillation would move in a cycloid, at least as far as the extent of the vibrations of the pendulum required. For this purpose, I constructed an appropriate apparatus. I described the central portion of a cycloid on a sheet of brass, and fixed it on a wall, with a horizontal plane on the same, at a proper distance above for the length of the seconds pendulum. After several trials with cylinders of different sizes, it appeared that one of about 2 inches in diameter caused the point of oscillation to traverse in the curve, at least as far as could be sensibly determined.

These trials led to the construction of a cylinder the size of which was a very near approximation to the proper magnitude. This cylinder was then fixed to a convertible pendulum, which I had previously constructed; but in the present case it was used as a common pendulum. The result of more careful and repeated experiments showed that a cylinder of 1·8 inch in diameter caused the *seconds pendulum* to perform its vibrations in times more nearly equable than could be obtained by cylinders of any other size.

Thinking, however, that the property of isochronism might possibly depend on some minute size or proportion, I first shortened the pendulum, making use of the cylinder which gave the nearest approximation. I then gradually and successively lengthened it, even beyond its proper length, and tried the vibrations at every small interval; but the experiments failed to give a nearer approximation to isochronism than in the former case. In short, the vibrations thus obtained did not appear to be more isochronous than those which were obtained by employing a knife-edge.

Being disappointed in not finding the vibrations equal in time, I began to doubt the property of the cycloid in rendering the vibrations of the pendulum isochronous. For my own satisfaction on this point, I therefore instituted the following experiments. I described a circle with beam compasses (the distance of the points of which was double the diameter of the circle with which the cycloid had been described), so close by the central portion of the cycloid on the same sheet of brass, as to be within $\frac{1}{20}$ of an inch of that curve. To an extent of about 9° or 10° on each side of the middle of the curve, there was no perceptible difference between it and the circumference of the circle to the same extent.

My next step was to construct metallic cycloidal cheeks of the proper size for the seconds pendulum, and to fix them in a vertical position, so that their extreme points were at the distance of the base of the curve from each other; while the middle portions of the curve inverted served as cheeks for the vibrations of the pendulum, in the usual way prescribed in philosophical treatises on the subject. A ball was then suspended between the cheeks, by means of a piece of small gold wire flattened to about $\frac{1}{300}$ of an inch in thickness. As soon as the ball was made to vibrate, it was obvious that the cheeks would have no effect in equalizing the vibrations, even when the extent of vibration was 9° or 10° beyond the point of rest.

Subsequently, it occurred to me to try the experiment by employing the extreme portions of the curve as cheeks, and fixing them for this purpose in the manner above mentioned. The cycloidal cheeks formed in this manner were found to have a considerable effect in equalizing the times of the vibrations; for the short vibrations of 1°·5 were not above 8 or 10 seconds faster in 24 hours than the longer vibrations of 4° or 5°; and had all been as perfect as theory requires, the vibrations might have been isochronous.

In ascertaining the proportions of the times of the long and short vibrations, the detached pendulum was always adjusted to the clock pendulum, so as to make them vibrate alike. The fixture for the detached pendulum was a little in front of the clock; and the two pendulums were observed by viewing them from such a point that they were both seen in a line when at the extent of their vibrations; the experiment being always commenced by putting them in motion so as to vibrate exactly with one another. This method was considered not only more simple than that of *coincidences*, but fully as accurate. It is also more suitable for an experimenter who cannot conveniently have an assistant at all times; nor indeed would the method of coincidences have answered so well for the various experiments which I had in view.

Though the preceding experiments occupied upwards of 12 months, during all my spare hours from business, either in making the necessary apparatus or the experiments themselves, yet considerable practical advantage was thereby obtained.

The object of the following experiments was to ascertain the difference in time between the vibrations of a pendulum in air and *in vacuo*, &c. For the purpose of making experiments *in vacuo*, or in an exhausted vessel, the following apparatus was constructed.—An iron vessel was made, having circular apertures about 6 inches in diameter, near the bottom, at a proper distance for observing the extremity of the pendulum rod. These apertures were glazed with strong plate-glass, so as to be capable of bearing the great pressure consequent on a high degree of rarefaction. The vessel was placed close in front of the clock-case, which had sides made to withdraw, so as to admit more readily of the adjustment of the pendulum, &c. There was likewise in the vessel a contrivance for putting the pendulum in motion at any degree of rarefaction or exhaustion.

In his experiments on the pendulum, Captain Kater appears to consider that the buoyancy of the air causes the vibrations of a pendulum to be slower. "Thus," says he, "the specific gravity of water compared with that of air, may be known for the temperature and altitude of the barometer at the time of observation; and multiplying this by the specific gravity of the pendulum, the ratio of the weight of the pendulum compared with that of air will be obtained. This ratio will express the diminution of the force of gravity arising from the buoyancy of the atmosphere: and in order that the number of vibrations may be the same *in vacuo* as in air, the length of the pendulum must be increased in the proportion of this ratio to 1, the lengths of pendulums vibrating in the same time, varying directly as the force of gravity."

Buoyancy is here considered equivalent to a diminution of gravity, and proportional to the deduction of the weight of the pendulum's bulk of air from its own weight; consequently, it should appear as if a light pendulum would vibrate more slowly than a heavy one. Not being of this opinion, I made the following experiment to put it to the test. I mounted a light pendulum with a wooden bob, of a cylindrical form, about 4½ inches long and 2 inches in diameter; this bob was 10 ounces in weight, and, along with a brass ball and the pendudulum rod, weighed about 15 ounces. A piece of thin iron, about ¾ of an inch in breadth, was fixed to the end of the pendulum rod, edgewise to the direction of motion of the pendulum. The object in making it so light was, that the effect of buoyancy might be more readily observed, and also its difference of rate in air and *in vacuo*.

The pendulum thus mounted was suspended in the vessel in which the vacuum or exhaustion was to be produced. A small trough of mercury, about 6 inches long, 1 inch deep, and 1 inch broad, was placed at the bottom of the vessel, having its length parallel to the direction of motion of the pendulum. After frequent adjustments and exhaustions, I succeeded in making the lower edge of the piece of iron traverse slightly in the mercury, so that at the end of each vibration it was nearly free of the surface. Some dust upon this surface indicated a slight motion in the direction of the pendulum; but it almost ceased with the motion of the pendulum at its extent of vibration, and appeared to produce no effect upon the time in descending.

By such means, the pendulum was made to lose its extent of motion nearly at the same rate as a pendulum with a metallic bob does in air; the clock pendulum was adjusted to vibrate with the light pendulum. The trough with the mercury was afterwards removed, the vessel was exhausted to the same degree as before, and the same mean extent of vibration was employed; but careful experiment showed no perceptible difference in time.

In this experiment, I considered that the mercury should buoy up the pendulum as much as air; and if so, we must evidently conclude from the result, that buoyancy makes no difference upon the time of the vibrations of a pendulum. When the same pendulum used above was made to vibrate in air, it lost one vibration in 20 minutes upon the clock pendulum.

By this time I had begun to suspect the true cause why the pendulum lost time when vibrating in air, and my conclusion was as follows: A current was generated in the air by the motion of the pendulum, and in the direction of its motion; consequently, when the pendulum reached its height and its momentum was exhausted, the current thus generated now slightly suspended its motion, and retarded the first increments of its descent.

The current of air generated by the vibrations of the bob of a pendulum may be rendered distinctly visible thus: Fix a piece of rag at the bottom of the pendulum, set fire to the rag, and blow out the flame, still allowing the rag to smoke; then put the pendulum in motion, and the smoke will be seen passing along in the direction of the pendulum's motion, even before it can be observed that it has begun to descend.

Had Captain Kater been aware of this fact, he would have been at no loss to account for his pendulum losing its adjustment, when by the hygrometer he observed a great and sudden change in the air from moisture to dryness. This observation shows the accuracy with which his experiments were conducted; it also indicates that dry air is more dense than moist, as the current generated in the latter state offered more resistance to the return of the pendulum than in the former.

For, in his experiments, at the time when the smaller weight of the pendulum was down, the vibrations would be slower than when the greater weight was down; the smaller weight presenting a larger surface to the resisting medium, in proportion to its weight, than the greater weight, the current then generated would oppose its descent more than the current generated by the greater weight.

[To be continued.]

XLI. *On the Theory of Voltaic Action.* *By Mr.* John Prideaux.

[Concluded from p. 220.]

Sect. IV. *Of the Conducting Property of the Liquid.*

25. THE conducting power of the liquid is a main point in voltaic phænomena; and acid liquids are understood, generally, to be the best conductors (liquid metals of course left out of the question); alkaline liquids the worst, of aqueous solutions; and alcohol, oils, and the like, as non-conductors. Thus, from whatever kind of coincidence, the conducting and electro-negative properties seem to bear some mutual relation.

26. Whether this conduction in the voltaic battery be from particle to particle, or, like that of caloric in liquids, connected with transference of the particles themselves, becomes the next inquiry. Of such transference we have abundant evidence; but in what degree it is *essential* to the conducting process, it may be difficult to ascertain by direct experiment.

27. Separate glasses, filled with acid and alkaline solutions, and connected by a siphon filled with water, separated cells, similarly filled, and divided by bladder, each having a copper plate plunged in the alkali, a zinc plate in the acid, and con-

nected through a wire,—have their contents gradually altered by transference of the acid into the alkali, and *vice versâ*, until either neutralization has taken place, or the activity of the circuit is so far lowered as to have no longer power to communicate the requisite impetus. Oxygen goes to the zinc, and hydrogen to the copper, in most cases; and we have seen (22) that not only this took place, but that the alkali continued to rise, and the acid to descend, in opposition to their specific gravity.

28. It is seen (23) that addition to the quantity of copper augments the effect; but I do not find this happen unless the additional copper be in *immediate* liquid communication with the zinc.

a) A 3-inch zinc plate was set in a water-tight copper case (open at top), with due precautions against contact: the case was nearly filled with diluted nitric acid (1·60), and placed in a vessel of the same liquid, which just reached its upper edge. The case and zinc being both furnished with conducting wires, were put in communication with the magnetest;

Deflection . . . . 40°

b) An additional copper case, open at both ends, was then placed about the first, clear of contact, and also made to communicate with the same mercury box as the first case.

No increase of deflection.

c) A little more of the same acid was then poured on, to overflow the whole, and establish immediate liquid communication between the zinc and the external copper. As soon as it ran over the edges,

Deflection . . . . 42°

The conducting medium is the same in (*b*) as in (*c*); but the transference of particles from the zinc to the external copper is intercepted in (*b*), and no other evident difference appears. The communication in (*c*) being only over the edge of the inner case, the increase is proportionally small; 40° to 42°, or 38 to 43 current.

Thus we may imagine the copper exalting, by contact or by metallic communication, the positive character of the zinc; the zinc thus exalted decomposing the water with peculiar vivacity, attracting the negative oxygen, *charging* and repelling the positive hydrogen; the latter being at the same time attracted, and oxygen charged and repelled by the copper:—oxygen thus continually travelling from copper to zinc; hydrogen from zinc to copper; and each discharging its *excess* of electricity as it arrives. This attraction and repulsion being of course stronger, and the motion of the charged particles consequently quicker, the less the interval between the plates;

and we know how greatly approximation increases the activity of voltaic plates.

29. If this transference be the real conducting agency, it should follow, that when the poles are disconnected, as the electricity accumulates, and counterbalances those attractions and repulsions between the plates, the action between them should become gradually weaker, and at last cease, but be renewed on restoring the connexion. And such we know to be the fact.

Also, when the plates, and consequently the intermediate imperfectly conducting strata of liquid, are multiplied, resistance to the passage of electricity is increased. It must therefore accumulate, in degree, on each pair of plates; and the atom of hydrogen from the zinc of pair A should be unable to discharge itself into the copper of pair B, unless its charge be high enough to overcome the resistance forward, and *vice versâ*. Hence the electricity at the poles of a numerous voltaic battery, though greater in tension, should be less in quantity of current than at those of a battery equal in surface, but in fewer divisions.

And this also may be seen to be true by the following experiment.

30. For comparisons of this kind I employ a trough of wood, twenty-four inches long, seven wide, and four deep, divided by transverse partitions into six cells, and well lined with cement. Each of these cells contains a small calorimotor, nearly on Dr. Hare's plan, composed of 10 zinc and 11 copper plates, each three inches square, the zinc and copper working into separate mercury boxes (3, &c.) on the top of each calorimotor.

By passing connecting wires along 13, 23, 33, 43, &c., and 1 *c*, 2 *c*, 3 *c*, 4 *c*, &c., the whole six sets become a single pair; but connecting them 13, 2 *c*; 23, 3 *c*; 33, 4 *c*, &c., they become as many pairs as there are sets. And it is easy to understand how, by arranging the connexions, they become two or three pairs. It is this convenient divisibility which led to the preference of the number 6.

The following Table shows the deflections, in proportion to the manner in which the calorimotors were divided.

Connexion	Experiments 1	2	3	4	*Mean.*	*Curr.*
into 1 pair......	60°	58°	58°	59°	59°	75?
2 pair......	49	48	48	49	48·5	62
3 pair......	42	42	42	43	42	43·4
6 pair......	30	30	30	30	30	22·6

The number 75 is an estimate, and I think considerably below the truth. Becquerel's table does not go so high.

These facts do not appear so easy of explanation upon any other hypothesis of liquid conduction, as on that by transference of particles.

31. As, however, we are unacquainted with any standard of the *actual* quantity of electricity circulating in any given voltaic action, and as it would be difficult to measure even the relative quantity that a given portion of positive and negative liquid matter can convey by transference of particles, under a given tension,—we cannot ascertain by calculation, any more than by direct experiment, whether this transference is likely to be the chief, or even a partial agent, in conveying the electricity through the liquid. The discharge of a Leyden jar through a water-tube, though with great diminution of its impetus, yet exhibits a rapidity of conduction inconsistent with our notions of the transference of particles; and a discrepancy is occasionally found between the transmitting or conducting power, and the facility of decomposition, as in the case of dilute sulphuric and nitric acids.

Still such a notion materially assists our understanding the opposite electrical accumulations at the poles of the pile, and some of the phænomena accompanying them; and the high tension of a charged jar *may* enable the electricity to pass through liquids in a manner which could not be produced by the actions between the plates of a voltaic pile.

Sect. V. *Of the Loss of Power by continued Voltaic Circulation.*

32. However uncertain be the degree in which molecular transference in the liquid acts as an auxiliary, no doubt exists of the tendency of the negative particles toward the positive plate, and *vice versâ*: and if the leading principle of this paper be true (20), they should, when thus arranged in the order of electrical attraction, *after discharging* their acquired electricity, oppose, and gradually tend to neutralize, the electromotive action of the plates on each other.

33. Thus, whilst a voltaic pair, kept, the zinc in alkali, the copper in acid, retains its electricity unimpaired for a long time, we find them, when charged with solution of a neutral salt, become gradually weaker in action, until, after a short period, they hardly affect the multiplier. If we now take the plates out of the liquid, leave them exposed to the air, and replace them, the action is renewed with a vigour and permanence proportionate (to a certain extent) to the length of time the plates have been withdrawn from the liquid.

34. During this time of separation, the liquid particles at-

tached to the plates will gradually drain off, and those remaining in the liquid may reassume the arrangement due to their natural affinities. Thus the energy of action should be renewed on replacing the plates in the liquid; and this renovation should have more or less permanence, according as the re-establishment of the natural order of affinities were more or less complete, and freed from remaining electrical influence. And this, in an imperfectly conducting liquid, subject to the effects of combination and decomposition before noticed (28), may be an operation not quite instantaneous.

35. If such an electrical arrangement of molecules be the chief cause of decay of power in a battery (when the neutralization of acid or alkaline charges is not concerned), then washing the plates instantly, on their removal from the liquid (although kept beneath the surface of the water all the time they are out of the charge), should make them as effective, on being plunged into a fresh solution, as hanging for any length of time in the air. And keeping them in the second solution until the force be again much reduced, should give the first charge time to recover its natural state, by which the plates on being removed from the second, washed, and returned to the first, should have all their original energy. And thus the action should be renewable by washing and alternation, until the formation of a coat of suboxide on the plates should impede their contact with the liquid, and therefore require friction or an acid to cleanse them.

36. The apparatus described (30), in which the cells are nearly filled by the little calorimotors, and the whole liquid charge consequently subjected to their action, gave results corresponding so accurately with these anticipations, that doubts might have been excited of their fairness, particularly as the structure of the instrument is a little complicated, which would make the experiments troublesome to repeat; and as each calorimotor is bound by a wooden frame, which might be supposed to retain a portion of the water or acid employed in washing, the following simple arrangement was therefore substituted.

A pair of zinc and copper plates, 3 inches square, each provided with a conducting wire, were fixed together at the interval of $\frac{1}{4}$ inch, by short cylinders of sealing-wax at the four corners, with the aid of heat. The backs of the plates were then varnished; so that the polished faces, opposed to each other at an invariable distance, were the only parts capable of action. The liquid charge was 4 ounces sulphate of zinc, dissolved in a quart of water, and it was contained in two glasses, G and H.

The plates being dipped in water, to remove any foreign material, or fugitive impression at the first contact with moisture, the ends of the wires were connected with a multiplier, and the plates plunged into H,—Deflection . . 40°

Left in H until the needle had receded to . . . . . 25°,

Then taken out and plunged into G,—Deflection . 40°.

As the plates occupied only the middle of the glass, and the backs were varnished, it was probable that but little of the charge, perhaps only the part immediately between the faces of the plates, had suffered electrical change. The needle was therefore allowed to recede to 28°, when the plates were lifted out and replaced in the same liquid,—Deflection . . 38°, but unsteady. After receding to . . . . 30°, removed to H,—Deflection again . . . . 40°.

After receding to 20°, taken out and replaced,—Deflection . . . . 40°, but receded quickly.

Washed and placed in G,—Deflection . . . . . . 42°.

Taken out, washed, and left all night in the air. In the morning plunged into H,—Deflection . . . . 42°: So that washing produced the same deflection as hanging all night in the air.

Receded in 15 minutes to . . . . 20°.

37. It then became a question whether the mere discontinuance of electromotion, without moving the plates, might not allow the reaction of the natural affinities, and thus restore the action.

The needle having fallen back, as above stated, to . 20°, the connexion through the multiplier was severed. Re-connected after 10 minutes . . . . 42°, but fell back, in 10 minutes connexion, to . . . . . 18°.

Disconnected 5 minutes; on re-connexion . . . . 30°.

Again disconnected 10 minutes; on re-connexion . 30°. The face of the copper covered with small bubbles; doubtless hydrogen gas. Detached 2 minutes to wipe away the bubbles with a feather,—Re-connected . . . . 35°.

The plates had now continued two hours and a half in the solution, and the zinc was black with suboxide; yet simply washed and placed in H,—the Deflection was . . . 37°.

It is unnecessary to occupy more space with the further variations of this experiment, all which give the same result.

38. But one circumstance must not be passed over. Hanging in the air two or three hours gave a deflection of 45°, which fell back to 35° so rapidly as to allow only time for turning to the desk and writing the figures; whilst remaining

for twenty-four hours in the air gave only 35°, but steady. Hence some accumulation seems to take place in the air during the drying of the plates, perhaps communicated by the vapour.

39. These experiments go in confirmation of the supposition (35), that the electric attraction, arranging the negative particles of the liquid against the face of the positive metal, and *vice versâ*, and thus tending to saturate itself, should gradually extinguish its own action.

And from previous observations (14, 15, 19, 22,) it might be inferred, that the destruction of zinc by acid charges is waste.

Entire extinction of the electromotion by saturation is, however, prevented by the unsteadiness of liquid particles; and by the imperfection of liquid conducting power, whatever its mode of action, obstructing the effects on the particles not in immediate contact with the metals; which are sufficient to keep some action alive for a good while, where only neutral charges are employed. But when free acids are used, which dissolve the zinc, a more powerful compensating force comes into play.

Sect. VI. *Of the Effects of Chemical Action.*

40. It has long been shown by Becquerel*, that when a metal is acted on by an acid, and forms with it an oxide or a salt, the metal becomes negatively, the liquid positively, electric; and accordingly it is familiar to Voltaists, that when, into an acid liquid, two plates of zinc are plunged, the one new and bright, the other corroded, connexion being made through a multiplier, the corroded plate is positive (in the liquor) to the bright one.

41. It has also been shown by Sturgeon, and had been shown before him by Davy, that if two plates of iron with bright surfaces be plunged into dilute muriatic acid, and after a time one of them be withdrawn, and kept for some seconds out of the acid, on being replaced it acts as copper; and so alternately *either* plate withdrawn for a few seconds, acts as copper on reentering the liquor. And the case is the same with zinc, as any one possessing a couple of zinc plates may prove in a minute.

42. These two experiments (40, 41), and Becquerel's general principle (40) explain each other. When bright and corroded zinc are brought into contact with an acid, the latter yields most readily to its action, and gives off electricity to the liquid, which returns through the bright plate and the wire.

* *Ann. de Chim. et de Phys.* May 1829.

When of two similar plates, one is withdrawn from the action of the acid, the other remaining subject to it, the latter will give off positive electricity to the liquor, which the former, on reentering, must take up and convey back through the wire, as copper does.

So in a voltaic pair, when acid is brought by electrical attraction or otherwise into contact with zinc, the metal is attacked; the acid or water being decomposed on the one hand, and the zinc dissolved on the other. The positive electricity passes from the zinc into the liquid (40) in the direction of the galvanic current (and assuming the theory of two electricities, the negative passes from the acid to the zinc, also in the direction of the current). Thus the chemical action compensates, or surpasses, the neutralizing effect of the negative liquid particles on the zinc, in proportion to its facility of decomposition, and to the electrical character of its residual ingredients; whilst the copper is negatively excited by the acid to the highest degree (23).

43. Sulphuric acid decomposes the water, as does probably the muriatic; hydrogen gas being given off in both cases, and carrying with it* some of the positive electricity generated. But nitric acid gives rise to no gas (in moderate charges); and there being no waste of electricity, its action should be the greater, as is known to be the fact.

44. These three acids, employed in atomic proportions, in equal quantities of water, placed in three cells of the trough (30); one of the calorimotors being moved from one to the other and back again alternately, so as to give all the acids equal opportunity for action, gave the following deflections.

Acids.	Experiments 1	2	3	4	*Mean.*	*Curr.*
Sulphuric......	26°	20°	14°	10°	17·5	9·7
Muriatic	30	23	†	13	22·	13·3
Nitric	42	34	21	17	28·5	20·5

Why the muriatic acted so much more powerfully than the sulphuric acid is not evident, as the acids were pure. Possibly the muriatic acid may be itself decomposed, and the chlorine combine directly with the metal. No evidence occurred to me, when employing atomic proportions, of the less durability of nitric acid, as stated by Singer; but my experience is not to be set in competition with his.

45. An experiment quoted by Berzelius‡, and which I do

* Pouillet, *Ann. de Chim. et de Phys.* September 1827.
† This figure was not recorded, through oversight.
‡ *Tr. de Chim.* tom. i. p. 152.

not recollect to have encountered elsewhere, is in point here, and compares well with the stimulative action of the electric state of the liquid (20).

If a large and a small plate of zinc connected through a multiplier be plunged into a weakly acid liquor, the larger acts as copper; but increase the acidity, or warm the liquid, the larger acts as zinc.

Here when the acid is too weak to attack the zinc, the larger surface giving the greater field for the influence of the negative liquor, positive electricity sets away from it (20) through the wire, to the smaller plate, as happens with copper. But when the metal is acted on, either by increasing the acidity, or warming the liquor, the larger surface gives off the more positive electricity to the liquor, which then sets through the wire in the other direction, as in the case of zinc; thus not only compensating, but surpassing (42), the *negative* stimulation of the acid first demonstrated.

It must here be confessed, that my results in repeating this experiment have not been constant, though generally confirmatory. The authority of Berzelius is, nevertheless, abundantly sufficient; and proofs of the efficacy of chemical action, in augmenting the voltaic current, are too familiar to the experimentalist to need further exemplification here.

Conclusion.

The theory here advocated may be thus generally stated.

When zinc and copper come into contact, positive electricity passes from the copper into the zinc, until their mutual relation to that fluid be *in equilibrio*: this is initial electromotion; which may be continued, in the condition of circulation, through a conducting liquid (12). But if an electropositive liquid be placed in contact with the negative metal, and *vice versâ*, and the circulation kept up, the disposition of the liquids being in *counteraction* to that of the metals, the electro-motive action is obstructed (19). On the contrary, when the positive and negative liquids are in contact with the homo-electric metals, the tendency of the whole is in the same direction, and the electro-motive action is expedited (19).

When the electricity thus passes into the zinc, and on into the positive element of the liquid, the so *charged* liquid particle is repelled, and attracted by the copper, in proportion to the approximation of the plates; and having free motion, proceeds in that direction with proportionate velocity. The converse takes place between the copper and the electro-negative liquid particle; and the particles discharging themselves on their arrival, thus maintain the circulation.

When discharged of their *acquired* electricity, they remain attached to the anti-electric plates, and thus assume the counteracting character above mentioned, obstructing the current in proportion to the quantity collected on the surface of the plates (32). But when there is free acid, it no sooner comes in contact with the zinc, than it begins to act upon it, and the zinc, in the act of dissolution, gives out positive electricity to the liquid; *i. e.* in the direction of the current (40), thus balancing, or more commonly overcoming, the neutralizing influence of the negative liquid particles (42), whilst they act with their full effect in exalting the copper: and hence the superiority of acid charges (23).

The reader will perceive that the theory of two fluids is most conformable to these views. It has not been insisted on, because not fully received in this country, nor quite free from ambiguity in its application.

XLII. *Abstract of Meteorological Observations made at St. Petersburg, in* 1830, *at the Astronomical Observatory. By* MM. Wisniewsky *and* Tarkhanof; *and calculated by Professor* M. A. Kupffer*.

In the following observations the thermometer is divided according to Reaumur, and the barometer into French inches. The barometric heights have been reduced to the temperature of 14° of Reaumur, and the months are reckoned according to the New Style.

Table I. *Containing the Mean of the Thermometric Observations for every Month of* 1830.

Months.	7h A.M.	2h P.M.	9h P.M.	Means.
	°	°	°	°
January.. ...	− 9·46	− 8·03	− 8·44	− 8·64
February ...	8·54	6·55	7·24	7·45
March........	− 4·59	− 1·13	− 3·25	− 2·99
April.........	+ 0·83	+ 4·30	+ 1·18	+ 2·10
May	4·46	7·76	3·86	5·36
June	11·59	14·67	10·89	12·38
July	13·11	15·73	12·66	13·83
August.......	13·16	16·65	12·65	14·15
September...	6·08	10·77	7·33	8·06
October	+ 3·17	5·68	3·86	4·24
November...	− 0·36	+ 0·72	+ 0·28	+ 0·21
December....	− 4·76	− 3·64	− 4·11	− 4·17
Means.........	+ 2·6	+ 4·74	+ 2·47	+ 3·09

According to the tables communicated by Dr. Brewster†, we

* Communicated by Professor Kupffer.

† Edinb. Journal of Science, for June 1826.

must subtract 0°·11 from the mean results found above, in order to have the mean temperature of the year. We shall then have

The mean temperature of the year 1830... +2°·98 Reaum.
Or .... 38°·705 Fahr.

I need not remind the reader, that the table given by Dr. Brewster is probably applicable only to Scotland and to similar climates. I have employed it here because we do not yet possess for St. Petersburg meteorological observations executed upon the model of those which have been made with so much perseverance at Leith, under the care of Dr. Brewster.

TABLE II. *Extreme Variations of the Octogesimal Thermometer for every Month of* 1830, *and the Maximum of the Difference for each Month, between two Observations of the same Day.*

Months.	Maximum of Temperature at 2^h P.M.	Minimum of Temperature at 7^h A.M.	Difference.	Greatest Diff. between two Observations of the same Day.
	°	°	°	°
January......	− 1·2	−19·2	18·0	6·8
February	+ 0·3	16·5	16·8	7·7
March	4·3	17·9	22·2	7·2
April	13·2	− 5·2	18·4	9·9
May	16·0	0·0	16·0	7·5
June	22·0	+ 8·1	13·9	8·8
July	23·8	8·3	15·5	6·5
August	24·0	9·4	14·6	8·0
September...	14·5	+ 1·1	13·4	9·5
October......	12·7	− 2·2	14·9	7·4
November...	6·3	9·5	15·8	4·0
December...	+ 1·2	−12·1	13·3	7·1

This table does not give the greatest variations of temperature in the course of a month, or during 24 hours. We must admit that the maxima of temperature take place at 2^h P.M. But for the minima we know that they occur a few instants after sunrise; so that at 7^h A.M. the temperature during the greatest part of the year is considerably above the minimum.

TABLE III. *Mean of Barometrical Observations made at* 7^h A.M., 2^h P.M., *and* 9^h P.M., *for every Month of the Year* 1830.

Months.	Barometric Height in French Inches.	Months.	Barometric Height in French Inches.
January ...	28·454	July	28·044
February..	28·025	August....	27·937
March ...	28·077	September..	28·212
April	28·073	October....	27·991
May......	28·160	November..	28·315
June......	28·056	December..	28·051

Mean barometric height for 1830......28·116 inches.

TABLE IV.—*Extreme Variations of the Barometer at the Hours of Observation for each Month of* 1830.

Months.	Maximum.	Minimum.	Difference.
	In.	In.	In.
January ...	29·14	27·75	1·39
February...	28·67	26·93	1·74
March	28·83	27·33	1·50
April.	28·55	27·48	1·07
May.........	28·51	27·47	1·04
June...... .	28·64	27·53	1·11
July	28·46	27·70	0·76
August.....	28·17	27·70	0·47
September..	28·60	27·78	0·82
October.....	28·41	27·17	1·24
November..	28·99	27·36	1·63
December..	29·01	27·26	1·79
Means	28·67	27·46	1·21

TABLE V.—*State of the Winds whose Direction was observed Three Times a Day, at* 7h A.M., 2h P.M., *and* 9h P.M.

Months.	North.	North East.	East.	South-East.	South.	South-West.	West.	North-West.	Calm.
Jan. ...	1	14	0	8	13	41	12	0	4
Feb. ...	7	18	4	11	7	33	2	1	1
March.	0	7	4	6	29	38	8	0	1
April ...	1	13	2	10	14	43	2	0	5
May ...	8	27	1	6	5	25	17	1	3
June ...	4	18	9	5	9	27	11	0	7
July ...	7	13	2	6	7	40	12	4	2
August	0	4	5	14	15	41	8	0	6
Sept. ...	11	23	11	1	13	16	3	3	9
Oct. ...	12	19	3	12	10	30	1	2	4
Nov. ...	3	11	2	14	24	31	2	0	3
Dec. ...	2	8	0	13	48	15	3	0	4
Sums.	56	175	43	106	194	380	81	11	49

TABLE VI.—*Mean Height of the Barometer for each Wind.*

Winds.	Mean Height of Barometer.	No. of Observations.	Winds.	Mean Height of Barometer.	No. of Observations.
	In.			In.	
North.......	28·091	56	South-West	28·085	380
North-East	28·229	175	West.........	28·142	81
East.........	28·156	43	North-West	28·142	11
South-East	28·071	106	Calm.........	28·256	49
South.......	28·025	194			

General Observations.—Strong and very strong winds occurred on the following days: February 9, (New Style,) South-east; March 12, 13, 18, 30, South; March 31, South-west;

April 5, South; May 8, North-west; April 14, South; December 27, South.

In the course of the year 1830, there were at St. Petersburg: —90 days of rain; 68 days of snow; 10 days of thunder; 58 days during which the sky was entirely covered from morning till night; 218 days during which the sky was cloudy during the greatest part of the day; 143 days of fog (these fogs were commonly produced in the morning, but less frequently in the evening, and they very seldom lasted beyond noon); and 28 days during which the sky was entirely clear from morning till night.

The last frost took place on the 4th of May.—The first frost on the 14th of October.

The thermometer rose above zero,

For the first time on the 27th of February;—and for the last time on the 28th of December.

The day of the flood on the Neva, 21st of April.—The day of its being shut up, 1st of December.

Auroræ Boreales.—These meteors appeared on the evenings of the following days:—Feb. 24th; March 18th; May 5th; Sept. 13, 17, 18, and 19; Oct. 18 and 22; and Dec. 8 and 15.

XLIII. *On the Inflexion of Light. By* John Barton, *Esq.**

SOME time ago I had the honour to submit to the Royal Society an account of a variety of experiments and observations on the inflexion of light, which seemed to me strongly to indicate that light consists of material particles, endued with a force of mutual repulsion†. I have since had the satisfaction to find that the possibility of explaining the phænomena of inflexion by the help of the same principle had suggested itself to the mind of Sir David Brewster‡. In the paper just mentioned, I did not enter into any discussion respecting the theories of Young and Fresnel,—contenting myself with a simple detail of the results of my own experiments, accompanied by such explanatory observations as seemed needful to connect them together, and render them intelligible; but I wish now to state some considerations which appear to me to be decisive against those theories.

The fundamental principle common to them both is this:—If two equal waves, moving in opposite directions, come into collision, they will destroy each other, and all further movement will cease; whereas, if they coincide in their direction,

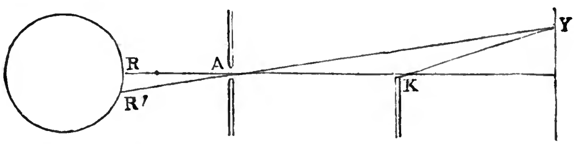
* Communicated by the Author.

† An abstract of the paper here alluded to was given in Phil. Mag. and Annals, N.S. vol. x. p. 300: it has also been noticed by Prof. Powell, in vol. xi. p. 2.—Edit.

‡ Life of Newton, p. 105.

they will form by their union a single wave of double force. Now light is supposed to consist of the undulations of an imaginary elastic æther, as sound consists of undulations of the air.

If therefore we suppose a ray entering into a darkened chamber through a small orifice at A (see the following figure) to proceed forward in a direct line to Y, when it falls on a sheet of white paper; while another ray proceeding in the direction AK, is inflected at K by touching the edge of a knife, or other solid body, and turned into the direction KY, so as to fall on the same point Y as the former ray;—then the effect produced by the joint action of these two rays will be different, accordingly as the lengths of their paths differ or not by an integral number of undulations. If the lengths of their paths differ by a half-undulation, or any odd number of half-undulations, they will destroy one another, and the spot Y will be dark. If the lengths of their paths differ by a whole undulation, or any number of whole undulations, they will coincide, and the spot Y will be of double brightness. And thus are explained the alternate bands of light and shade, which border the shadows of bodies placed in a small beam of light entering a darkened room.



The lengths of the two rays AY, AKY, are always computed by Young and Fresnel from the point A, which they denominate the "origin of the rays:" or the "luminous point." But it appears to me that the true origin of the rays is at two points R, R′, on the surface of the sun; and that instead of comparing AY with AKY, we ought to compare R′Y with RKY. Now that this comparison should give the same results as the former,—in other words, that the line RA should either be equal to R′A, or that their lengths should always differ precisely by an integral number of undulations,—is evidently impossible.

It will not be said, I presume, that the two rays RA, R′A, on entering through the small opening at A, exercise any mutual action on each other, so as to become in fact a single ray. Such a suggestion would be at variance with the whole theory of Huyghens, which necessarily assumes, as one of its fundamental principles, that any number of undulations may pass through each other without disturbance:—inconsistent, indeed, with well known facts, such as the perfect image of the crescent form of the sun during an eclipse, received through a pinhole in a darkened room; and the still more familiar fact of the distinct vision of a multitude of distant objects through a refracting telescope; since the rays proceeding from all those objects repeatedly cross one another in passing through the instrument.

Secondly. Though the theory of Fresnel agrees pretty well with the results of his own experiments, it is far from agreeing with the results of other experiments made by observers of acknowledged accuracy. Sir Isaac Newton and M. Biot have each of them recorded a series of observations on the inflexion of light, which may be employed as tests of the accuracy of Fresnel's theory. Newton, having admitted a beam of light through a hole the 42nd part of an inch in width, let it fall on a slit between the edges of two knives, at the distance of 8 feet 5 inches. Placing a sheet of white paper behind the slit, he observed that the shadows formed by the edges of the knives were bordered with a succession of coloured fringes. Varying the width of the slit, he observed at what distances the paper must be placed, so that the first of the dark intervals between these fringes, coming from either side, might cross one another in the centre of the spectrum *. Now, by the theory of Fresnel, any one of these observations should give us the length of the undulations supposed to constitute light; and of course that length will be the same when deduced from any other observation. The following Table will show how far this is from being the case.

Distances of the paper from the kuives in inches.	Distance between the edge of the knives.	Length of an undulation by Fresnel's theory.
$1\frac{1}{2}$	·012	·00001385
$3\frac{1}{3}$	·020	·00001763
$8\frac{3}{5}$	·034	·00002075
32	·057	·00001901
96	·081	·00001896
131	·087	·00001888

In all these observations the length of an undulation turns out smaller than it should be. According to Fresnel, the length of an undulation in red light is ...... ·00002512† inch.
According to Young, it is .... ·0000266
In yellow light, the most luminous part of the prismatic spectrum, the length of an undulation, according to Young, is ...... ·0000235
Whereas the highest value deduced from Newton's observations above, is no more than.. ·00002075

* Optics, Book III. Obs. ix. p. 105.

† ·000368 millimetres

Indeed, these values are so much at variance with one another as to destroy all confidence in the theory; since the differences are far greater than can be accounted for by any supposable errors of observation, even had the observer been less remarkable for accuracy than Newton. This will appear clearly from the following Table, in which I have taken the length of an undulation such as it is given by the first of the above observations, and then computed by Fresnel's method the distances of the knives from the paper in each of the succeeding observations.

Distances between the edges of the knives.	Distances of the knives from the paper: By observations.	By Fresnel's theory.
·012	$1\frac{1}{2}$	1·5
·020	$3\frac{1}{3}$	4·28
·034	$8\frac{3}{5}$	13·45
·057	32	49·8
·081	96	202·
·087	131	336·

Still more are these observations at variance with Young's theory, which supposes the phænomena to depend on the interference of the two rays reflected from the edges of the slit. In this case the centre of the spectrum would be always bright, the lengths of those two rays being equal.

M. Biot's observations were made in red light*. He has not mentioned the distance of the slit from the opening through which the light entered; but as the length of an undulation in red light is given by M. Fresnel, we may reverse the calculation, and compute what this distance must have been from the other data. It will be seen that the results given by the theory are impossible.

Width of slit: in millimetres†.	Distance at which the first dark band cut the central axis.	Distance of the slit from the opening by which the rays entered, by Fresnel's theory.
·25mm	12mm	− 86mm
.50	46	−263
·75	120	−2791
1·00	244	+2577
1·25	404	+2525
1·50	576	+3876
1·75	922	+2630
2·00	1071	+5325

In the first three observations, the distance of the slit from the opening in the window-shutter, as computed by Fresnel's hypothesis, here turns out to be a negative quantity. In other words, this distance is *greater than infinity!*

It may be proper briefly to state the method by which these

* *Traité de Physique*, tom. iv. p. 757. † Ibid. page 764.

computations are performed, referring necessarily to Fresnel's Memoir, in the 5th volume of the Memoirs of the National Institute, for further particulars; since it would scarcely be possible to explain the process at length without transcribing a considerable part of that memoir.

Assuming the distance of the slit from the opening in the shutter.. $= a$.
The distance of the slit from the paper on which the rays are received.. $= b$.
The width of the slit .... $= c$.
The length of an undulation.............................. $= \lambda$.

Then, according to Fresnel, the intensity of the light at the centre of the spectrum varies as

$$\left(\int d\,v \cos q\,v^2\right)^2 + \left(\int d\,v \sin q\,v^2\right)^2$$

q representing the fourth part of a circumference to radius 1, and the integrals being each taken from

$$v = \frac{c}{2}\sqrt{\frac{2\,(a+b)}{a\,b\,\lambda}}, \text{ to } v = -\frac{c}{2}\sqrt{\frac{2\,(a+b)}{a\,b\,\lambda}}.$$

As these expressions do not admit of being integrated directly, the author has given a table of their numerical values for each value of v *. Now we have to find the value of v when the first dark band falls on the centre of the spectrum; in other words, the smallest value of v at which the intensity of the light becomes a minimum. On reference to the Table, it will be found that this value of v is somewhere between 1·8 and 1·9; and by interpolation, for which purpose the theorem employed by the author † may be conveniently used, the exact value of v sought is 1·875; we have, therefore,

$$1{\cdot}875 = \frac{c}{2}\sqrt{\frac{2\,(a+b)}{a\,b\,\lambda}},$$

* Memoirs of the National Institute, vol. v. p. 408.

† "Supposing the curve which has for its ordinates the intensity of the light at three nearly adjacent points to coincide within that small space with a curve of the second degree, the position of the least ordinate will be given by the formula

$$z = \frac{'p\,''z^2 - ''p\,'z^2}{2\,('p\,''z - ''p\,'z},$$

where z' and z'' represent the distances of one of the extreme points from the two others; $'p$ and $''p$ the differences of their intensities, and z the distance of the same point from the minimum."—P. 435.

It may be observed that this formula is not analytically exact. The true value of z is

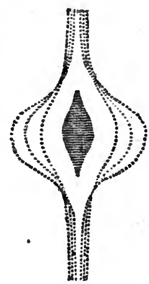
$$\frac{'p\,''z^2 - ''p\,'z^2 - 'p\,''p\,('p - ''p)}{2\,('p\,''z - ''p\,''z')}$$

but when the differences of intensity are not considerable, the last term may be neglected as evanescent.

an equation from which it is evident that the values of λ or of a may be determined, the values of the other quantities being known.

Thirdly. The theory of Fresnel is quite irreconcileable with another observation of Newton's, which I have frequently verified. When the interval between the two knife-blades is very much diminished, the spectrum thrown on the paper exhibits a dark space in its centre. "When the distance of the edges was about the four hundredth part of an inch," says Newton, "the stream of light parted in the middle, and left a shadow between the two parts. This shadow was so black and dark that all the light which passed between the knives seemed to be bent, and turned aside to the one hand or to the other. And as the knives still approached one another, the shadow grew broader, and the streams shorter at their inward ends which were next the shadow, until upon the contact of the knives the whole light vanished, leaving its place to the shadow*".

The most satisfactory way of performing this experiment is to employ two razor-blades, whose edges are slightly convex; for then the coloured bands will be seen running round the dark space on either side, as in the annexed figure. The position of the dark space, and of the greatest divergence of the coloured bands, answers of course to the point where the razor-blades approach one another most nearly.



Now by the theory of Fresnel, so far from the centre of the spectrum being occupied by a dark space, it should be the brightest and most luminous part of the whole; as will appear on calculating the intensity of the light at different distances by the rule which he has laid down. Suppose, for instance, the distance of the knife-blades from the hole in the window-shutter to be 30 inches; the distance from the paper 21 inches; the interval between the two blades one four hundredth part of an inch: then we have

$$c\sqrt{\frac{2(a+b)}{ab\lambda}} = \frac{1}{400}\sqrt{\frac{2(30+21)}{30\times 21\times \cdot 00002512}} = \cdot 2.$$

And if x represent the distance of any point from the centre of the spectrum, the intensity of the light at that point should be as
$$(\int dv \cos q v^2)^2 + (\int dv \sin q v^2)^2$$

* Optics, Book III. Obs. vi.

the integrals being each taken from $v = (x + \cdot 1)$ to $v = (x - \cdot 1)$. Computing the intensity for each value of x in this way, I obtain the following results.

Value of x.	Intensity of light.	Value of x.	Intensity of light.	Value of x.	Intensity of light.
0	·04	1·0	·0387	2·5	·0324
·1	·04	1·1	·0384	2·7	·0312
·2	·0399	1·2	·0381	2·9	·0300
·3	·0398	1·3	·0378	3·1	·0287
·4	·0397	1·4	·0375	3·3	·0274
·5	·0396	1·5	·0371	3·5	·0261
·6	·0395	1·7	·0363	3·7	·0248
·7	·0393	1·9	·0355	3·9	·0236
·8	·0391	2·1	·0345	4·3	·0208
·9	·0389	2·3	·0335	4·7	·0182
				5·1	·0156

It will be seen that the intensity of the light is a maximum at the centre of the spectrum; or when x, the distance from that centre, is equal to 0.

I have supposed $a = 30$ inches, and $b = 21$ inches, simply because these appear to have been about the distances of the knives, from the window-shutter and from the paper respectively, in Newton's observation. But if any other values are assigned to these latter, provided the interval between the two blades is very small, the same conclusion will be found to hold good.

I might refer to other phænomena, which appear to me no less irreconcileable with the undulatory hypothesis than those here considered. But this seems unnecessary; for if the preceding reasonings and computations involve no error, they are surely of themselves sufficient to overthrow that hypothesis.

XLIV. *On the Law of the Diffusion of Gases.* By Thomas Graham, *Esq. M.A. F.R.S. Ed., Professor of Chemistry in the Andersonian University, Glasgow.*

[Continued from p. 190.]

2. *Diffusion of Carbonic Acid Gas.*

THE most satisfactory experiments with carbonic acid gas were performed by confining it over a solution of common salt, saturated in the cold, which absorbs this gas very slowly, and, instead of the diffusion-instrument with bulb, a long diffusion-tube was found most suitable.

Experiment 1.—Thermometer 64°: dew-point 53°. Barometer 30·13. Left in diffusion-tube 17 air, and filled up over brine to 197 with carbonic acid gas, which gives 180 carbonic acid. As brine boils at 222° or 224°, that is 11° or 12° above the boiling point of water, we may suppose it to be proportionally less vaporous at low temperatures, and take the tension of its vapour at 64° to be that of water at 53°, which was also the dew-point. This was confirmed by confining 847 volumes of atmospheric air over brine at the time; the air was not expanded by vapour rising into it from the brine, nor did it contract.

The initial contents of the diffusion are therefore,

Air and vapour	17·
Carbonic acid gas	177·6
Vapour	2·4
	197·0

An expansion took place of 4 measures in ten minutes, and of 40 measures in five hours. A standard tube of the same diameter as the diffusion-tube, sealed at the top, had been filled with carbonic acid and placed over brine, to mark the absorption of the gas. One measure of gas was absorbed during the continuance of the above experiment. The expansion, therefore, in the diffusion-case has really been 41 and not 40, or, probably even more than 41, as undoubtedly a greater absorption of gas by the brine occurred in the diffusion-tube than in the standard-tube, from the motion of the liquid in the former during the course of the expansion of its gaseous contents, while the liquid in the other was quite at rest, and 177·6 −1, or 176·6 carbonic acid gas only have been exposed to diffusion. The diffusion was allowed to take place into the open air, which had the same proportion of vapour as the carbonic acid.

The specific gravity of carbonic acid gas is 1·527, of which the square root is 1·2360, and the reciprocal of the square root 0·8091. Hence one volume air should replace 0·8091 carbonic acid gas, which is the theoretical diffusion-volume of this gas.

In the experiment, 176·6 carbonic acid are replaced by 217·6 air.

Here, the expansion upon 176·6 carbonic acid being replaced by air is 41+ parts by experiment, while it is 41·68 parts by theory.

The diffusion-volume of carbonic acid gas is,

0·812 by experiment,
0·809 by theory.

Exp. 2.—In another experiment, conducted in the same manner, thermometer 64°, barometer 30·00, the initial contents of the diffusion-tube were, Carbonic acid and vapour, 201.

The final contents,

Air and vapour .... 245.

Correcting for loss of gas by absorption, the final contents would be, Air and vapour 246.

As the proportion of vapour in the gas at the first, and in the air finally is the same, we may say that carbonic acid is replaced by air in the proportion of 201 to 246.

$$\frac{201}{246} = 0{\cdot}813 = \text{diffusion-volume of carbonic acid.}$$

Exp. 3.—In a third experiment over brine, thermometer 62°, barometer 29·65, carbonic acid and vapour 169

Replaced by air and vapour 205

Or, allowing for absorption, by air and vapour...... 206

$$\frac{169}{206} = 0{\cdot}816 = \text{diffusion-volume of carbonic acid.}$$

But extreme accuracy is quite out of the question in the case of carbonic acid, from the vagueness of the small correction for absorption of the gas by the brine, and from the absorbent action of the plug, which affects, more or less, all the condensible gases.

The experiment in the case of this gas had been performed repeatedly over water itself, in different diffusion-tubes, and always with an eventual increase to the gaseous contents of the tube of within 2 per cent. of the theoretical quantity; but this mode, and the corrections for absorption, are decidedly inferior in precision to the preceding.

3. *Chlorine.*—This gas, from its high density, should afford a good illustration of the law, were other circumstances equally favourable, as the specific gravity of chlorine is about 2·5, of which the square root is 1·5811, and the reciprocal of the square root 0·6325. 100 measures of chlorine should be replaced by 158·11 air; or 1 air should replace 0·6325 chlorine, which is its diffusion-volume.

Experiment.—Thermometer 64°. To a diffusion-tube over water, with 5 measures air, 80 chlorine gas were added, making together 85 measures, which, diffusing into damp air, expanded 3 measures in the first eight minutes, 18 measures in eighty-two minutes, and, finally, 19 measures in one hundred and six minutes; but the same gas, in a close standard tube of the same diameter, contracted, owing to absorption of the gas by water, 5 measures in eight minutes, 15 measures in thirty-three minutes, and 18 measures in thirty-nine minutes, the rate of absorption diminishing evidently from the water in the tube becoming saturated and abiding in it. But the absorption of gas by water in the two experiments cannot be well compared; for, in the diffusion experiment, the chlorine is rapidly diluted with return-air, which protects it from absorption, and, indeed, before the end of the experiment, must occasion a portion of the dissolved chlorine gas to reassume the gaseous form, vapourizing away from the water which held it in solution, and rising into the upper part of the tube. The absorption in the diffusion-case would certainly be overrated at one half of what occurred in the comparative experiment in the same time. At the outset, however, we may presume that the same absorption took place in both cases. Hence the expansion in the diffusion experiment would be 3+5, or 8 measures to the first eight minutes. The absorption, however, would tell two ways in lessening the expansion; *first*, so much gas has disappeared by absorption, the quantity to be added to the expansion; *second*, so much less chlorine has really been submitted to diffusion: 80 parts have not been diffused, but 80 diminished by this quantity.

Merely adding the observed absorption in the first thirty-nine minutes, namely, 18 measures to the expansion observed of 19 measures, we have an expansion from diffusion of 37 measures, which approaches, as near as we can expect from the method, to 45 measures, the theoretical expansion on 78 measures dry chlorine. We may therefore presume that the diffusion of chlorine is not incompatible with the law.

4. *Sulphurous Acid Gas.*—Over mercury. To diffusion-tube with 7 measures air, 66 dry sulphurous acid gas were added, which were allowed to diffuse into dry air. An expansion occurred of

5	measures in	9 minutes
13	———	23
30	———	85
31	———	108

at which last expansion it remained steady.

Assuming the specific gravity of sulphurous gas at 2·222, its square root is 1·4907, of which the reciprocal is 0·6708.

67·08 sulphurous gas should be replaced by 100 air.

We have 66 sulphurous gas, and expansion 31, or,

66 sulphurous acid are replaced by 97·00 air, by experiment;
66 —— —— —— 98·39 air, by theory.

The diffusion-volume of sulphurous acid gas is,

0·68 by experiment,
0·67 by theory.

5. *Protoxide of Nitrogen.*—In an experiment with this gas, dry, over mercury, allowing for a quantity of nitrogen which

it contained, 51 measures were replaced in ninety minutes by 62 dry air. Taking the specific gravity of this gas at 1·2577, its root is 1·2360, of which the reciprocal is 0·8091.

Diffusion-volume 0·82 by experiment,
——— —— 0·81 by theory.

6. *Cyanogen.*—Also over mercury. First deprived of hydrocyanic acid by peroxide of mercury, and dried, an expansion always resulted from diffusion, but it never amounted to the theoretical quantity. Taking 1·8105 as the specific gravity of cyanogen, the square root is 1·3456, and the reciprocal of the square root 0·7432.

Hence, 1 cyanogen is replaced by 1·3456 air; and
1 air replaces .... 0·7432 cyanogen.

1st, 83 cyanogen were replaced by 99½ air; 2nd, 75 cyanogen by 90 air; 3rd, 50 cyanogen by 63 air. The last experiment is the most favourable. But 100 cyanogen are replaced, according to that experiment, by 126 air only, instead of 134. This deviation from the law, depends on the property of the plaster-plug, which it shares with all porous bodies, to absorb and condense a portion of all those gases which, like cyanogen, are easily liquefied. It is evident, that if a portion of the cyanogen is withdrawn in this way, a certain contraction is occasioned, and again really less of the gas is submitted to diffusion; and from both causes, the expansion is less than it ought to be. It is possible, also, that the cyanogen may have contained a little nitrogen.

7. *Muriatic Acid Gas.*—Specific gravity 1·28472; square root, 1·1334; reciprocal of square root 0·8823. Hence,

1 muriatic acid should be replaced by 1·2847 air; and
1 air should replace 0·8823 muriatic acid.

In the case of this gas, the expansion from diffusion was overpowered by the absorbent property of the plug.

94 measures contracted to 88 in ten minutes, and remained at that quantity for nine minutes, and then expanded to 90 measures in twenty-five minutes more. The plug, upon a subsequent examination, appeared to be injured, and rendered too permeable, by a chemical action of the muriatic acid upon the hydrated sulphate of lime.

8. *Ammoniacal Gas.*—Density 0·5902. Square root 0·76825; reciprocal of square root 1·3016. Hence,

1 ammoniacal gas should be replaced by 0·76825 air; and
1 air should replace 1·3016 ammoniacal gas.

But in the case of this gas, as with muriatic acid, the result of diffusion is altogether deranged by condensation of gas in the porous plug, which, in these experiments, was half an inch in thickness. It is remarkable, however, that when the tube was filled with ammoniacal gas in the usual way, the final contraction was by no means excessive, indeed, never quite so great as it should have been from diffusion alone, independently of the contraction from absorption. This was found to arise from the absorption by the plug being so rapid, that, during the progress of filling the tube with gas, the plug became nearly saturated with gas, taking up ten or twelve times its bulk, and consequently, a great deal more gas was introduced into the tube than its capacity.

9. *Sulphuretted Hydrogen Gas.*—Prepared from sulphuret of antimony, by the action of muriatic acid. Density, 1·1805, Root, 1·0855. Reciprocal of root, 0·9204.

In the case of this gas, 69 measures were replaced by 73 air. In this experiment, 100 air replaced 95 instead of 92 sulphuretted hydrogen. But we may refer the diminution to the absorption of the gas by the plug, and to its partial decomposition, as the mercury exposed to the gas became black. The air which entered contributed to this decomposition.

As carbonic acid is one of the gases condensed by the plug, like the preceding examples, but to a less extent, we can now understand why the return air was always a little under the theoretical quantity, in the careful experiments on that gas, of which an account was formerly given.

In the case of the gases which follow, the specific gravity approaches so closely to that of air, that their accordance with the law requires every precaution.

10. *Oxygen Gas.*—Specific gravity, 1·111. Square root, 1·0541. Reciprocal, 0·9487.

100 oxygen should be replaced by 105·41 air; and
100 air should replace 94·87 oxygen.

When confined in a straight diffusion-tube, there is uniformly an expansion; but it is unnecessary to recount experiments performed with the straight tube, as the divisions are not minute.

Experiment 1.—Thermometer 64°. Barometer 29·82 inches. Diffusion-instrument with bulb, divided into two hundredths of a cubic inch; also standard bulb and tube, close at top, to afford corrections for changes in temperature and pressure, as before explained. Both diffusion-instrument and standard were filled with pure oxygen from chlorate of potash, and placed in glasses over water, covered by a bell-jar, of which the inside was moistened. A few minutes were purposely allowed to elapse before the quantity of gas in either instrument was noted, as the quantity oscillated for a little. The diffusion-instrument contained 795 measures oxygen, and the standard 828, at the outset. In two hours the expansion in diffusion-instrument, corrected from the standard, was 6 measures; in four hours and a half, 13 measures; in fifteen hours, 29 measures; in twenty hours, 34 measures; in twenty-nine hours, 41 measures; in thirty-eight hours, the expansion was at a maximum, namely, 43 measures. In explanation of the long duration of this and the following experiments, it may be stated, that the plug was fully half an inch in thickness.

795 measures oxygen and vapour have therefore been replaced by 838 measures air and vapour.

$$\frac{795}{838} = 0{\cdot}9487 = \text{diffusion-volume of oxygen by experiment.}$$

This is the exact theoretic number; a coincidence, however, which we must view as accidental.

Exp. 2.—In a careful repetition of this experiment with another specimen of oxygen gas, the results approached very closely to the preceding; but the return-air was in slight excess above the theoretical quantity. Thus,

1 oxygen was replaced by 1·056 air, by experiment.
1 —— —— 1·054 air, by theory.

Oxygen, therefore, affords a most striking confirmation of the law.

11. *Nitrogen.*—Prepared by burning an excess of phosphorus in a confined portion of air, and allowing the residuary gas to stand over water for several days.

Specific gravity, 0·9722. Root, 0·9860. Reciprocal of root, 1·0140. 100 nitrogen should be replaced by 98·60; and 100 air should replace 101·40 nitrogen.

Thermometer, 66°. Barometer, 29·23. Diffusion into moist air as in the preceding experiments.

836 measures contracted 3 measures in two hours and forty minutes, as corrected by standard; and 13 measures in eighteen hours, which was the maximum contraction; for in twenty-three hours and a half from the beginning of experiment, a contraction of 12 measures was indicated. Taking the last as the true result,

$$\frac{836}{834} = 1{\cdot}0143 = \text{diffusion-volume of nitrogen by experiment.}$$

$$1{\cdot}0140 = \text{diffusion-volume of nitrogen by theory.}$$

12. *Olefiant Gas.*—Specific gravity likewise 0·972, &c. as in nitrogen. The gas was carefully made, collected in a low receiver, allowed to stand over water for twenty-four hours, and finally washed with caustic ley.

Thermometer, 59°. Barometer, 29·83. 800 measures of this gas were replaced by 785 measures of air, in twenty-five hours, correcting from standard.

$\frac{800}{785} = 1\cdot0191$ = diffusion-volume of olefiant gas, by experiment.

The contraction in this experiment is a little above the theoretical quantity. In another experiment with different gas, the contraction was even greater, indicating a diffusion-volume $= 1\cdot0303$; but the presence of a minute quantity of carburetted hydrogen, or some lighter hydro-carburet, was suspected, from the rapidity of the contraction in this case.

[To be continued.]

XLV. *A Reply to the Remarks of* Professors Airy *and* Hamilton *on the Paper upon the Interference of Light after passing through a Prism of Glass. By* R. Potter, *Jun., Esq.**

MY paper in the February Number of this Journal, On the Interference of Light which has been refracted by a Prism, having been noticed in the last Number by Professors Airy and Hamilton, I hasten to reply to their remarks; although I am very far still from being prepared to enter completely into the subject of the velocity of light in traversing refracting media. I must accordingly still refer solely to the experiment with the prism, leaving to another time the appeal to a more direct and less intricate experiment; with which I have been long occupied, but which I have not yet been able to get through, on account of its requiring apparatus which I did not before possess, and which I find still requires further additions to produce some minute adjustments.

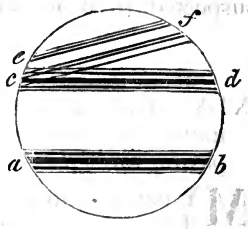
The papers of the two learned Professors have tended much to strengthen my previous expectation as to what will be found eventually to be the real velocity of light in passing through refracting bodies; for both of them have raised objections to my conclusions only upon points on which I had myself in the outset some misgivings.

Upon that which is proposed by Professor Airy, I was at great pains to satisfy myself experimentally. In using common light, or light considerably heterogeneous, and when the distance between the images of the luminous point is *too small* in comparison with the distance of the prism from them, the bands or interference-fringes might be supposed, from what we observe, merely to dilate. This appears to be the way in which Professor Airy has tried it, which I conceive he has done; and I placed the original paper in Mr. Coddington's hands at Oxford.

To satisfy myself upon this point, I used the red light given by the solution of iodine in hydriodic acid, which, when of

* Communicated by the Author.

proper strength, gives a much purer light than the red glass found in the windows of old churches, and which M. Fresnel considered sufficiently homogeneous for experiments where much greater delicacy was required. I also inclined the two mirrors so much to each other as to render the bands sufficiently narrow; and when I observed the bands, similar to *a b*, in the figure, come, by withdrawing the eye and eye-glass a little distance from the prism, into a position similar to *c d*, I had no danger of falling into Prof. Airy's error of supposing the change to arise only from the shifting of the centre of the fringes, whilst *the bars themselves remained stationary*. The dotted lines *e f* represent the diffracted fringes, caused by the edge of the lower mirror. I cannot imagine how Prof. Airy should represent me as describing the appearances by a shifting only of the centre of the fringes;—my description states distinctly the bars themselves to move; and Professor Hamilton has evidently thus rightly read it. By operating in the manner above described, and placing the prism so that its edge appears to touch the bands formed directly in the air, then looking with the eye-glass at a little distance from the prism, another complete set of bars may be seen in the prismatic light. I have repeated this experiment frequently, and believe Professor Powell and myself succeeded in so trying it at his residence in Oxford in June last.



The phænomenon of the shifting of the apparent centre of fringes must be noticed in the common experiment, without a prism, by every one who frequently tries it, as it occurs perpetually, when, from looking directly, we change the position of the eye, in the plane perpendicular to the direction of the fringes, and look obliquely through the lens.

From what I have just said, it will be seen that I consider it no compliment that the Plumian Professor should think me ready to advance the minute effect of the shifting of the centre of fringes with light slightly heterogeneous, as a strong and solid argument against the undulatory theory. I must also respectfully inform him, that I believe the probability of my becoming an *undulationist* becomes daily less and less; as, from the time of my having merely an opinion upon the general theory, from having read Dr. Young's Bakerian Lecture, I am now gradually come to see many serious and weighty objections against it, of which several have the greater influence with me from having arisen in my own experimental inquiries. Amongst the objections not generally raised, which occur to me at the moment, I may mention, that the heating properties of one end of the solar spectrum and the chemical effects produced at the other, seem very inadequately accounted for to a chemist, by a small difference in the lengths of the undulations. The whole connexion of light, heat, and electricity seems to him beset with extraordinary difficulties, by adopting the undulatory theory of light. The subject of the combined or latent caloric of bodies, and the circumstance of transparent solid bodies belonging entirely to the class of electrics, give him a presentiment that we must look to more varied and profound causes than the motion of a subtile æther for the explanation of the effects we witness. (I must here notice that I cannot, with many opticians, call the translucency of thin metallic leaves transparency.) The phænomena of the absorption of light by coloured media have justly been shown by Sir David Brewster to militate strongly against this theory. My own discovery of the law of reflection by metals offers also a strong objection; for that whilst a considerable portion of the light enters the substance of the metal as in transparent bodies, yet the law of the variation of the intensity of the reflection is essentially different. The chemical theory would remind us that both classes of bodies possess determinate specific heats, but that there is an essential difference in their electrical properties, the former being conductors, and the latter electrics. I hence learn to look beyond the results of a mechanical theory on the motions excited in a subtile æther, for the solution. The effects of chemical agency and of arrangement of atoms in crystallized bodies show also the connexion of the optical effects with chemical affinities, and which brings us again to the theory of chemical combinations, without which, it appears to me, we can never give a satisfactory reason for double refraction, which is so intimately connected with the polarized condition of light.

The failing of the deductions of the most talented men who have adopted the undulatory theory, of which many instances have fallen under my own observation, and several of which I have already published, give me also more than a distrust of the fundamental hypothesis having any basis in nature.

With respect to the claim of half an undulation, my manner of speaking of which appears unpleasant to Professor Airy, I must say, that, although we do not find any mention of it under this title in his undulatory theory of optics in the last edition of his mathematical tracts, yet he is no doubt aware that when he says "he must have derived it from some very imperfect or erroneous statement," I had in recollection

Dr. Young's article "Chromatics" in the Supplement to the Encyclopædia Britannica. Dr. Young there gives as follows: "In reflections at the surface of a rarer medium, and of some metals, in all very oblique reflections, in diffraction, and in some extraordinary refractions, a half interval appears to be lost."

Dr. Young most probably wrote the above before M. Fresnel had adopted his new theory of diffraction, and had attempted to disprove his first view. It will be found, however, in his still later writings (see Quarterly Journal of Science, &c. for 1827, p. 450.), that Fresnel did not entirely abandon the theory of the light reflected at the edges of bodies producing diffracted fringes. As to his experiments to determine between his two views, I believe I have only need to object to their sufficiency,—that the red light given by the glass from the windows of old churches is quite insufficiently homogeneous where so small differences are to be ascertained; and this was the light which he made use of.

That the undulatory explanation of Newton's rings is inadmissible we may infer from the following fact: If we press a lens against one side of a cube of glass, on looking through the opposite side we see the central dark spot surrounded with the rings: if we look through one of the adjacent sides we see the central black spot, but without the rings, in the midst of a surface giving total reflection. In this latter case it will be impossible to account for the black centre by interference; and the same solution must apply to it which applies to the other case. When the lens is of less refractive glass than the cube, and the light is incident on the second surface of the glass cube at the critical angle for the two surfaces, the dark spot, after taking various tints, commencing with purple, entirely disappears. The best mode to observe the phænomenon above offered to notice, is to press the lens against the hypothenusal side of a glass prism having two angles of 45 degrees each: we can then see the black spot either in the totally reflected light, or in the partially reflected light, by slightly elevating or lowering the eye; and we see that the *white* of the first ring *is the point* where total reflection first becomes perfect, and from which we ought to commence our measurements of the spaces between the surfaces and our calculations of interference.

The question of half an undulation is more directly and effectually to be settled with the simple experiment of the two mirrors. Those who maintain the correctness of the undulatory theory, invariably assure us that the central band is *always* a *white* one. The result of considerable experience with me is, that it may be seen both black and white, though with me it has much oftener been the former, especially when the bars have been well defined. I was so perplexed with this uncertainty, that some time ago I wrote to Sir John Herschel, though a stranger to him, to ask what mode of observing he had used, and what were his results. I eventually, as may be learned from the paper on Interference which I read at Oxford, concluded it arose from the aberration in the focus of the lens used to form an image of the sun; and I accidentally, lately, found, I think, the means of arriving at an unexceptionable result. My two mirrors of speculum metal happened to be so nearly parallel, that the images of the luminous point appeared to the naked eye as only one; and when I looked through the eye-glass of $\frac{3}{4}$ inch focus, I found that four bands covered the whole field of view. I immediately endeavoured to cause a *shifting of the centre*, which had been one of the causes of perplexity; but found from the great breadth of the bands that this effect did not now perceptibly take place, and that the central band was *undoubtedly*, *unquestionably*, a black one; as the colours were perfectly symmetrical on each side of it. If this mode of trying the experiment is not the most unexceptionable of any, I shall be glad if those who suppose the central band to be *always white* will set me right, and show in what mode we may try it, so as to settle so important a point.

Sir John Herschel has expressed in print his opinion, that if equal talents had been exerted on the corpuscular theory which have been exerted on that of undulations, it is probable that the phænomena of optics, supposed to be referrible only to the latter, would have been found to be well accounted for on the former. I have seen nothing in the course of my experiments, nor met with any thing in the course of my reading, which could lead me to dissent from this opinion, but rather to go beyond it.

The establishment of truth, alone, should be the object and pride of all engaged in scientific researches; and though it is always more pleasant for the time to find new truths chime in with old opinions, yet it is a matter of weakness to allow their discordance to give us any lasting vexation, or to prevent us from giving them openly and fairly to the world. I hope that neither my opponents nor myself will ever want sufficient courage to publish their researches, or to confess a change of opinion when it overtakes us; and that such change will not cause in any a relaxation of zeal in the prosecution of scientific inquiries.

With respect to the solution of the question of prismatic interference, proposed by Professor Hamilton, the effects which would arise, according to the formulæ he has given, are much too small, and would show that I was perfectly right in considering the irregularity arising in pencils of only a few minutes of a degree in breadth from aberrations to be correctly negligible.

To find the values of the ordinates for the abscissæ of values 45 and 55 inches, we have

$$y = \frac{m\,a^3}{4\,x} - \frac{m\,a^2\,l}{4\,x^2}$$

where, taking the numbers of my former paper, we have

$$m\,a^3 = \cdot 00414834$$

and $$l = 44\cdot 35 \text{ nearly.}$$

Hence for $x = 45$ inches...... $y = \cdot 000000332$

and $x = 55$ —— ...... $y = \cdot 000003651$

and the difference of these is ·000003319, or rather more than three millionths of an inch.

This difference is so small, that I am sure Professor Hamilton would never have given me credit for being so minutely acute an observer, if he had had recourse to actual quantities. It is also easy to determine, that in the experiment the ordinate y is not zero at the prism, as the formula indicates it should be. Without having an accurate measurement, I nevertheless know that the effect under consideration, from whatever cause it arises, is at least several thousand times the amount of the above calculated difference.

Whatever velocity be finally ascertained to belong to light in passing through refracting bodies, it is clear that a different view in the theory of emission must be taken from the Newtonian one of refraction. I recollect, a long time ago, hearing Dr. Dalton express his opinion in private conversation, that it was not the same light which impinged upon the first surface of transparent bodies that left the second surface. It is clear that this view would bring refraction to a similar consideration with that of undulations. I have frequently considered the consequences which this view would lead to, and must confess that I do not still see it to be entirely unattended with difficulties, though these might most probably vanish on further study; and I have no hesitation in stating my belief that it will be found to accord better with a long range of chemical facts than either of the two other theories.

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

[Continued from p. 196.]

Part II.—COMETS of which the appearance since the commencement of the Christian æra rests upon competent authority, with the elements of such of them as have been computed.

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

A, the comet of 1680. B, that of 1652. C (Halley's), that of 1682. D, that of 1758. E, that of 1661.

Number.	Year and Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
1	10	...	.....	....	Manil., Dio.Cass. Chinese Records.	Several comets were said to have been seen at once in China.
2	14	...	......	......	Di. Cass., Ch.Re.	Seen during 20 days in China.
3	19	...	......	......	Chinese Records	
4	22	...	December.....	Hydra.......	Chinese Records.	
5	39	...	March, April	Pleiad., Wing of Peg. Andr.	Chinese Records.	Seen during 49 days.
6	54	...	Aug & Sept.?	....	Di.Cass., Sueton., Seneca........	Chin. Rec.
7	56	...	End of March	N.E. of Cancer	Chinese Records.	Seen 113 days.
8	60	...	August.........	From N. of Perseus to feet of Virgo	Tacit., Chin. Rec.	Seen in China during 135 days.
9	62	...	......	....	Seneca.	
10	64	...	May to Oct...	S. of η Virginis	Tacit.Suet.Ch.R.	
11	65	...	July	Sextans......	Chinese Records.	Seen 56 days.
12	66	...	......	From ♑ to ♐ and ♏......	Chinese Records.	Seen 50 days.
13	69	...	......	....	Di. Cas., Joseph.	
14	70	...	December......	Leo...........	Chinese Records.	Seen 48 days.
15	71	...	March	Pleiades......	Chinese Records	Seen 60 days.
16	75	...	July	Between Leo, Virgo, and Bootes	Chinese Records.	
17	76	...	September	Towards the head of Herc.	Pliny, Chin. Rec.	Seen in China 40 days.
18	77	...	Winter.........	Near β Arietis	Chinese Records.	Seen 106 days.
19	79	...	June?	....	Dion Cass., Suet.	
20	110	...	January........	Near $\gamma\delta\zeta$ Erid.	Chinese Records.	
21	117	...	January........	Near Equuleus	Chinese Records.	
22	132	...	......	Near $\delta\lambda\phi$ Sagit.	Di.Cass., Ch.Rec.	
23	141	...	March, April	Peg., Gem., Leo	Chinese Records.	
24	149	...	October	Head of Herc.	Chinese Records.	

Number.	Year and Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
25	161	...	February	Scorpio	Chinese Records.	
26	—	...	June..........	α β Pegasi	Chinese Records.	
27	178	...	September	Near the head of Hercules ..	Chinese Records.	Seen during 80 days.
28	180	...	August	Hind paws of Ursa Major	Chinese Records.	Seen 20 days.
29	—	...	November.....	Near Syrius...	Chinese Records.	
30	182	...	March	Between Androm. & Pisc.	Chinese Records.	Seen 60 days.
31	—	...	August	Fore paws of Ur. Maj., Can. Leo, Virg., &c.	Chinese Records.	
32	188	...	March	......	Chinese Records.	
33	190±	...	......	......	Lamp. Herodian.	
34	192	...	Sept. or Oct. .	Virgo	Chinese Records.	
35	193	...	November....	Virgo; then towards the head of Herc.	Chinese Records.	
36	200	...	November....	Near δ Serpen	Chinese Records.	
37	204	...	November....	Gemini, Canc.	Di. Cass., Chi. Re.	
38	206	...	January......	7 Stars of Urs. Major	Chinese Records.	
39	207	...	November....	Leo	Chinese Records.	
40	213	...	January......	Gemini	Chinese Records.	
41	218	...	April	Taurus, Gem.	Di. Cass., Chi. Re.	
42	222	...	November	Between β ♍ & σ ♌	Chinese Records.	
43	225	...	December....	Leo	Chinese Records.	
44	232	...	December....	Crater near σ ♌	Chinese Records.	
45	236	} ?	November....	Scorpio..	Chinese Records.	
46	—	}	December....	Ophiuchus. ...	Chinese Records.	
47	—	...	December....	Herc., Ophiuc.	Chinese Records.	
48	238	...	September	Between Cor. Hydr. & Crat.	Chinese Records.	Seen during 41 days.
49	—	...	Nov. Dec.....	Between α ♒, ε & θ Pegasi; then between Cycnus and Cepheus to Aquila......	Chinese Records.	
50	240	...	......	......	Chinese Records.	The elements have been computed by Burckhardt*.
51	245	...	September	Near Cor. Hyd.	Chinese Records.	Seen 23 days.

* Passage through the perihelion in mean time at Greenwich: November $9^d\ 23^h\ 50^m\ 39^s$.—Long. of the perihel. $9^s\ 1^\circ\ 0'\ 0''$.—Long. of the ascending node, $6^s\ 9^\circ\ 0'\ 0''$.—Inclin. of the orbit, $44^\circ\ 0'\ 0''$.—Perihelion distance, 0·371000.—Logarith. of the mean motion, 0·605000.—Motion direct.

[To be continued.]

XLVII. *On the undulatory Time of Passage of Light through a Prism.* *By* WILLIAM R. HAMILTON, *Esq. Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland* *.

SINCE I communicated my little paper, On the Effect of Aberration in prismatic Interference, (p. 191.) I have seen Professor Airy's remarks on Mr. Potter's experiment; in which it is suggested, that the observed central points, which tended towards the thickness of the prism, were not the points of simultaneous arrival of two homogeneous streams. From the well-known experience and skill of Professor Airy as an observer, I think it likely that he has assigned the true physical explanation of Mr. Potter's instructive experiment; though I wish that this experiment were repeated, with careful micrometrical measures. But I continue to think the mathematical correction just, which I proposed in my recent paper. In that paper, I took Mr. Potter's own account of his experiment; namely, that he had found, in the plane perpendicular to the edge, a tendency *towards* the thickness, and *from* a certain intermedial line, in the locus of the points of simultaneous arrival of two near homogeneous streams: and I endeavoured to show, that according to the undulatory theory, this locus *ought*, during a considerable range, to tend in this direction and not in the opposite;—a mathematical result, which was contrary to Mr. Potter's conclusion. It is, I hope, unnecessary to repeat the expression of my sincere respect for the gentleman from whom I have found myself obliged to differ on this mathematical question. But as I only stated, in my former paper, a correction of Mr. Potter's formula for the difference of times of arrival of two homogeneous streams, arising from the prismatic aberration of figure, and showed the influence of this aberrational correction on the course of the sought locus, without showing how I obtained the correction itself,—it may be useful to give here an outline of the method which I employ, for the treatment of this, and of other similar questions; referring, for more full details, to the recent and forthcoming volumes of the Transactions of the Royal Irish Academy.

Let light be supposed to go, in a bent path ABCD, from an initial point A to a final point D, through any prism ordinary or extraordinary, undergoing a first refraction at the point of entrance B, and a second refraction at the point of emergence C, the prism being placed *in vacuo*, and its angle being small or large; let the position of the final point D be marked by three rectangular coordinates x, y, z, of which the

* Communicated by the Author.

origin is taken on the edge of the prism; and let the position of the initial point A be marked by three other rectangular coordinates x', y', z', having the same origin, but not necessarily the same axes; let α, β, γ, be the cosines of the angles which the emergent or final direction CD makes with the rectangular axes of x, y, z; and let α', β', γ', be the cosines of the angles which the incident or initial direction AB makes with the rectangular axes of x', y', z'; finally, let V be the undulatory time of propagation from the initial to the final point, measured by the equivalent path *in vacuo*; and let it be considered as a function of the initial and final coordinates, which, by the position that we have assigned to the origin, is homogeneous of the first dimension. We shall then have the two following equations, deduced from my general methods,

$$V = \alpha x + \beta y + \gamma z - \alpha' x' - \beta' y' - \gamma' z', \tag{1}$$

$$o = x\,\delta\alpha + y\,\delta\beta + z\,\delta\gamma - x'\,\delta\alpha' - y'\,\delta\beta' - z'\,\delta\gamma'; \tag{2}$$

that is, V is to be determined as a function of the extreme coordinates $x\,y\,z\,x'\,y'\,z'$, which I have called in my Theory of Systems of Rays the *Characteristic Function*, by the condition that it shall be the maximum or minimum, with respect to the quantities $\alpha, \beta, \gamma, \alpha', \beta', \gamma'$, of the expression (1): attending to the two general relations,

$$\alpha^2 + \beta^2 + \gamma^2 = 1, \qquad \alpha'^2 + \beta'^2 + \gamma'^2 = 1, \tag{3}$$

and to the two other relations between the final and initial cosines of direction $\alpha\,\beta\,\gamma\,\alpha'\,\beta'\,\gamma'$, which result, in each particular case, from the prismatic connexion between the incident and emergent directions. And when the form of the *characteristic function* V is known, the six extreme cosines of direction may be deduced from it, by differentiation, as follows:

$$\left.\begin{aligned} \alpha &= \frac{\delta V}{\delta x}, & \beta &= \frac{\delta V}{\delta y}, & \gamma &= \frac{\delta V}{\delta z}, \\ \alpha' &= -\frac{\delta V}{\delta x'}, & \beta' &= -\frac{\delta V}{\delta y'}, & \gamma' &= -\frac{\delta V}{\delta z'}. \end{aligned}\right\} \tag{4}$$

When the prism is ordinary, such as glass, or when being extraordinary its edge is an axis of elasticity; and when we take the edge for the axis of z and of z', and consider only rays in a plane perpendicular to this edge, we may make,

$$\left.\begin{aligned} &z = o, \quad z' = o, \quad \gamma = o, \quad \gamma' = o, \\ &\alpha = \cos\theta, \quad \beta = \sin\theta, \quad \alpha' = \cos\theta', \quad \beta' = \sin\theta', \end{aligned}\right\} \tag{5}$$

θ being the emergent inclination to the axis of x, and θ' being the incident inclination to the axis of x'; and the undulatory time V, corresponding to any given coordinates $x\,y\,x'\,y'$, is the maximum or minimum, relatively to θ, of the expression

$$V = x\cos\theta + y\sin\theta - x'\cos\theta' - y'\sin\theta', \tag{6}$$

in which θ' is to be considered as a function of θ, depending on the prismatic connexion between the initial and final directions.

For an ordinary prism *in vacuo*, having its angle $= \varpi$, and its index $= \mu$, so that

$$\sin i = \mu \sin \frac{\varpi}{2}, \tag{7}$$

i being the angle of external incidence corresponding to the minimum of deviation, the relation between θ, θ', is,

$$\mu^2 \sin \varpi^2 = \sin (i+\theta)^2 + \sin (i-\theta')^2 \\ +2 \cos \varpi . \sin (i+\theta) . \sin (i-\theta'), \tag{8}$$

if the positive semiaxis of x be an emergent ray of minimum deviation, and the positive semiaxis of x' the corresponding incident ray prolonged, while the positive semiaxes of y, y', lie on the same side of the axes of x, x', as the prism. The relation (8) may be put under the approximate form,

$$\theta' = \theta - \frac{m}{4} . \theta^3, \tag{9}$$

when the angles θ, θ', are small, that is, when we consider rays having nearly the minimum of deviation, m being the same positive number as in my last paper, namely,

$$m = \frac{8 \sin (i+\frac{\varpi}{2}) \sin (i-\frac{\varpi}{2})}{\sin 2\, i . (\cos \frac{\varpi}{2})^2}: \tag{10}$$

and if, besides, we consider the ordinates y, y', as small, that is, if we suppose the light to pass near the edge of the prism, and neglect terms of the fourth dimension with respect to the small quantities y, y', θ, we shall have the undulatory time or characteristic function V = the maximum or minimum, relatively to θ, of the expression,

$$V = x - x' + (y-y')\, \theta - \tfrac{1}{2} (x-x' - \frac{m}{2} y')\, \theta^2 - \frac{m}{4} x' \theta^3. \tag{11}$$

In this manner we find, with the same order of approximation,

$$V = x - x' + \tfrac{1}{2} . \frac{(y-y')^2}{x-x'} + \frac{m}{4} . \frac{(x y' - x' y)\,(y-y')^2}{(x-x')^3}, \tag{12}$$

a result which may also be thus expressed:

$$V = \frac{m}{2} y' + \sqrt{(x-x' - \frac{m}{2} y')^2 + (y-y')^2} - \frac{m x'}{4} \left(\frac{y-y'}{x-x'}\right)^3. \tag{13}$$

If we neglected the last term of this last expression, it would give, by (4) and (5), the following formula for the tangent of inclination of the emergent ray,

$$\tan \theta = \frac{\beta}{\alpha} = \frac{y-y'}{x-x' - \frac{m}{2} y'}; \tag{14}$$

it would therefore imply that all the rays which diverged before incidence from the luminous point or primary image x', y', diverge after emergence from a prismatic focus or secondary image, having for coordinates,

$$x = x' + \frac{m}{2} y', \quad y = y': \tag{15}$$

so that the last term, $-\frac{mx'}{4}\left(\frac{y-y'}{x-x'}\right)^3$, of the expression (13) for the undulatory time V, may be considered as an aberrational term, arising from and determining the aberration (of figure, not of colour) of the prism. Accordingly Mr. Potter, neglecting this aberration of figure, did not perceive this term in the expression of the undulatory time, and was led to the results respecting the locus of points of simultaneous arrival of two near homogeneous streams, which I attempted in my last paper to correct. In comparing my present notation with my former, we are to make $x' = -l$, and $y' = \pm a$; we are also to observe that the present origin of x and y is on the edge of the prism. It seemed useful to give the present outline of a proof of the results stated in my former paper; because the methods which I have introduced for the solution of optical problems differ much from those usually received; and because it would perhaps be difficult, by those usual methods, to investigate the influence of the prismatic aberration of figure, on the undulatory time of propagation of homogeneous light.

Dublin Observatory, March 12, 1833.

XLVIII. *On the Real Functions of Imaginary Quantities.* By R. Murphy, *Esq. M.A. Fellow of Caius College, Cambridge**.

THE following is a very remarkable property of this class of functions.

"The *real* functions of *imaginary* quantities do not generally admit of maximum or minimum values."

$$\text{Let } f(\alpha+\beta\sqrt{-1}) = \text{P} + \text{Q}\sqrt{-1}$$

where α, β, P, and Q are all real; then it is easily seen that

$$2\text{P} = f(\alpha+\beta\sqrt{-1}) + f(\alpha-\beta\sqrt{-1})$$

and $$2\text{Q}\sqrt{-1} = f(\alpha+\beta\sqrt{-1}) - f(\alpha-\beta\sqrt{-1})$$

Moreover, if we only consider *real* functions, we must put $\text{Q} = 0$, and thus establish a relation between α and β.

* Communicated by the Author.

The condition that the real part P should then be a maximum or minimum is expressed by the equation

$$\frac{dP}{d\alpha} + \frac{dP}{d\beta} \cdot \frac{d\beta}{d\alpha} = 0$$

and the condition $Q = 0$ gives

$$\frac{dQ}{d\alpha} + \frac{dQ}{d\beta} \cdot \frac{d\beta}{d\alpha} = 0;$$

also the first two equations give

$$\frac{dP}{d\alpha} = \frac{dQ}{d\beta}$$

$$\frac{dP}{d\beta} = -\frac{dQ}{d\alpha}$$

Eliminating the three quantities $\frac{d\beta}{d\alpha}$, $\frac{dQ}{d\beta}$ and $\frac{dQ}{d\alpha}$ between these four equations, we get

$$\left(\frac{dP}{d\alpha}\right)^2 + \left(\frac{dP}{d\beta}\right)^2 = 0,$$

an equation which it is impossible to fulfill, since P and consequently its partial differential coefficients are necessarily real.

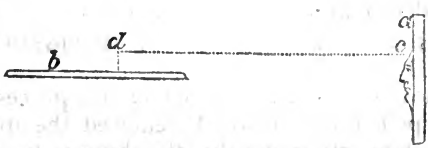
Caius College, March 5, 1833. R. MURPHY.

XLIX. *On an Improvement in Medal Ruling. By* R. B. BATE, *Esq.*

My dear Sir David, 21 Poultry, London, March 14, 1833.

ABOUT fifteen months ago, some printed representations of medals received from America were shown to me, with an inquiry into the manner in which they were produced: it was evident that they were the result of a ruling process; and one of my sons, having been engaged in ruling micrometer divisions, constructed an apparatus for performing this process, upon a scale sufficiently large for medals, and obtained the *same* results.

The following diagram represents the construction of the apparatus.



a. Being the medal.

b. The plate, covered with an etching-ground.
c. The tracer; and,
d. The etching-point, at right angles to it.

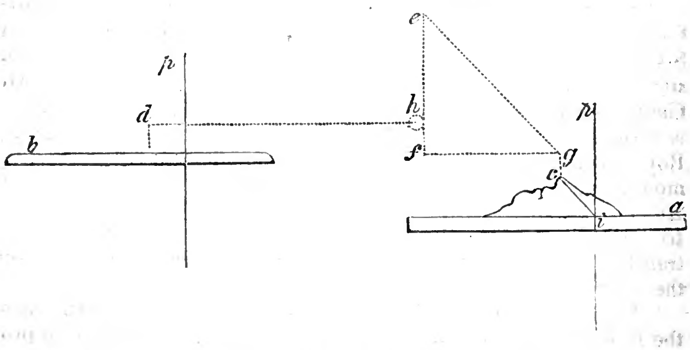
The arm *c, d* having a ruling motion horizontally across the surfaces of *a* and *b,* and likewise moving freely in the direction *c, d*; also, vertical motion being given to *a*, and horizontal motion to *b* by the same screw; a series of lines traced over the medal were described upon the plate in the following manner: so long as the tracer moved over the plane surface or ground of the medal, the point *d* described equidistant lines upon the plate; but so soon as the tracer touched a part of the raised surface or relief of the medal, it was raised above its plane a quantity equal to the height of such relief, and the line described by the etching-point was no longer equidistant, but deviated an equal quantity upon the horizontal plate; in the succeeding line, the tracer being raised off still further by the increasing height of the relief, the etching-point deviated still further from the former line described upon the plate: the continuation of this process produced a succession of deviating lines upon the plate, which, opening as the tracer rose above the plane of the medal and closing again as it approached that plane, gave the effect of light and shade, in the printed impression of the plate, so perfectly as, contrasted with the even tint produced by the parallel lines representing the plane surface of the medal, to convey to the mind almost a conviction that the impression was raised above the level of the paper.

However pleasing the effect of these impressions, they were all distorted representations of the original (just so much as the lines producing the representation deviated from the straight line upon the medal); and having ascertained that this method of producing such representations had been known to engravers upwards of fourteen years, as likewise that an account of it was given in a French work, called the "*Manuel de Tournure,*" it became evident that this distortion had suspended the use of the process; the most valuable and interesting subjects, those possessing the highest relief, being the most distorted.

It appeared to me, as it had no doubt appeared to others, that this defect was irremediable, because the distortion arose from the principle upon which the process was conducted; and although the quantity of distortion might be diminished by diminishing the motion of the etching-point, yet as this would reduce the effect of the relief, it would impoverish the effect of light and shade accordingly.

My son, however, whilst engaged in the process of ruling the large Napoleon medal, which required the application of three whole days, observing that the thing to be desired was a means of bringing the tracer down upon the medal, a quantity equal to the deviation of the etching-point from the straight line upon the plate; observing also that the process he was employing transferred vertical sections of the medal to the plate,—proposed taking *inclined* sections of the medal: a little consideration determined the selection of 45°, as being equidistant from the vertical and horizontal positions employed, and this inclination completely fulfilled the purposes required, removing the distortion altogether and, so far from impoverishing the effect of light and shade, improving that effect, inasmuch as, without diminishing its quantity, it threw the light upon the representation of the medal at the angle of 45° to its plane, instead of, as before, in the direction of the plane of the medal.

It was, however, soon found necessary to restore the tracing-point itself to the vertical position with respect to the plane of the medal, to allow of its being brought into every part of the surface which it had to rule, and, for the same reason, the mechanism which gave motion to it was obliged to be removed and the hand to be employed in its stead; the arrangement then became similar to the following diagram:



The tracer *c* being now attached to the right-angled triangle *e*, *f*, *g* and a friction-roller substituted for it at *h*, the triangle (the motion of which was *strictly confined to the plane of the diagonal* *e*, *g*) moved *d* a quantity always equal to the distance of the tracer *c* from the perpendicular *p*; so that the etching-point described precisely the same line upon the plate *b* as the tracer described upon the surface of the medal *a*, in the following manner:—so long as the tracer moved upon the plane surface of the medal, equidistant and parallel lines were described upon the plate, as before; but the moment the tracer rose above the plane surface of the medal, it began to deviate from the perpendicular p, and continuing to rise, described the diagonal i, c, parallel with g, e; now e, f, being always parallel with p, the deviation from p was always equal, as well upon the plate as upon the medal.

This improvement being, as I have no doubt you will immediately perceive, at once efficient and correct, I have procured my son a patent for it; and he is now constructing an apparatus for performing the process upon busts and statuary, and will, I expect, be able to produce a miniature representation of either, in which the resemblance will be faithfully preserved. I am, my dear Sir David,

Your obliged and faithful Servant,

R. B. Bate.

Sir David Brewster, F.R.S. &c. &c.

L. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 14, 1833.—The following announcement was made from the Chair:—

"His Royal Highness the President has received from Professor Gauss the abstract of a paper read by him at the meeting of the Royal Society at Göttingen, on the 15th of December last, entitled '*Intensitas vis magneticæ terrestris ad mensuram absolutam revocata.*' Mr. Gauss's views possessing considerable interest, His Royal Highness is desirous that they should be made known to the Fellows of the Royal Society; but as the original paper will not be printed for many months, and the abstract which appeared in the *Göttingische gelehrte Anzeigen* is in a language not generally understood in this country, His Royal Highness has requested your Foreign Secretary to translate it; and I am commanded to desire your Secretary to read the same to the present meeting.

"In deviating thus far from the usual routine of the business of the Royal Society, His Royal Highness is actuated by a wish to promote the reciprocal and early communication of new and important discoveries and views in science, between our own and the other Societies of Europe, devoted, like this, to '*the improvement of natural knowledge.*'

"Communications of this nature, however, cannot of course be admitted into your Transactions; but the publication, from time to time, of your Proceedings, affords a happy means of giving them general circulation; and thus the rapid propagation of much valuable information will be effected, which otherwise, if not absolutely lost to us, would, at least, long remain unknown to the British scientific public."

The following is the abstract of Professor Gauss's Memoir:—

Of the three elements which determine the manifestation of terrestrial magnetism in a given place, viz. Declination, Inclination, and Intensity, the first soonest engaged the attention of philosophers, the second much later, and the third has only at a very recent period become an object of investigation and experiment. This progressive interest is chiefly to be accounted for by the circumstance, that while the variation of the compass offered the greatest interest, as applied to the purposes of navigation and geodesic operations, the dip was looked upon as more nearly allied to it than was the intensity of terrestrial magnetism. To the natural philosopher, those three elements are absolutely of the same import, inasmuch as our knowledge of the general system of terrestrial magnetism will ever remain imperfect, until an equal share of attention has been bestowed on its separate branches.

For the first light thrown upon this subject we are indebted to the Baron Humboldt, whose attention was particularly directed to it during all his travels, and who has furnished a considerable series of observations, from which the gradual increase of this intensity, from the magnetic equator of the earth towards the magnetic poles, has been deduced. Many observers have since followed the footsteps of that great naturalist; and almost every part of the world to which, in recent times, travellers have penetrated, has furnished its quota of materials, from which already Hansteen (to whom this branch of philosophical inquiry is under great obligation) has been enabled to attempt the construction of an iso-dynamical chart.

The mode adopted in all these observations consists in disturbing the equilibrium of one and the same magnetic needle in places the comparative intensity at which is to be determined, and in exactly measuring the duration of its oscillations. This duration is indeed, *cæteris paribus*, dependent on the magnitude of the arc; but in such a manner, that however small the arc becomes, it still approaches a determined limit, loosely called the duration, and to which, the arc of oscillation being known, the really observed duration may easily be reduced. The intensity of terrestrial magnetism is thus inversely proportional to the square of the duration of oscillation of the same needle, or directly so to the square of the number of oscillations in a given time; and the result relates to the whole force, or to the horizontal portion of it, according as the needle has been caused to vibrate, in the plane of the magnetic meridian, round a horizontal axis, or, in a horizontal plane, round a vertical axis.

It is evident that the admissibility of this method entirely rests on the assumption of the unchanged magnetic state of the needle employed. If a properly-magnetized and carefully-preserved needle of good hardened steel be made use of for the experiments, and these do not take up too long a space of time, the danger to be apprehended from such alteration may not, indeed, be considerable; and the observer may rest the more satisfied in this respect, if, on returning to the first place, he find the time of the vibration to be the same; but experience teaches us that this result cannot by any means be calculated upon; neither can it be denied, that in resorting to such a proof we are only reasoning in a circle. It was known indeed, long ago, that both the declination and inclination in the same place are far from being invariable; that both of them, in the course of time, undergo very considerable progressive variations, independently of those periodical ones by which the nicety of observation is affected in different seasons and parts of the day. It is, therefore, no matter of doubt that the intensity of terrestrial magnetism must likewise be subject to them; indeed, the periodical diurnal variations are clearly perceptible in delicate observations. Hence, even if, after a considerable lapse of time, the same time of vibration is again observable in a given place, we are not, on that account, warranted in ascribing this circumstance to anything but a casual compensation of the variations which the intensity of the magnetism of the earth in that place, and the magnetic state of the needle itself, may have experienced during that interval. But even allowing the certainty of the comparative method to be only diminished to a certain degree, not entirely annulled, provided too long a space of time do not intervene, that mode, at all events, becomes entirely useless in cases where it is required to ascertain what changes the intensity of terrestrial magnetic force undergoes in a given place during a very long interval. This question, of considerable interest in a scientific point of view, must, therefore, remain unanswered until the merely comparative method shall be superseded by one which reduces the intensity of terrestrial magnetism to unities perfectly determined and manifest, and entirely independent of the individual nature of the needles employed in the experiments.

It is not difficult to lay down the theoretical principles on which such an independent method is to be founded. The time of oscillation of a given needle depends on three quantities; namely, the intensity of the terrestrial magnetism, the static momentum of the free magnetism in the needle, and the momentum of the inertia of this needle. The last of them may readily be ascertained by suitable methods; and thus, from the observed duration of the oscillation, is deduced, not the quantity of the intensity of the terrestrial magnetism, but the product of this quantity into the static momentum of the free magnetism in the needle. But it is impossible to separate these two factors from one another, unless observations of quite a different kind be superadded, that involve a different combination of them; and this end is attained by the use of a second needle, which, in order to ascertain the ratio of these forces, is subjected both to the influence of the magnetism of the earth and to that of the first needle. These two effects do, indeed, partly depend on the magnetic state of the second needle; but, by suitably conducting the experiments, the observer may eliminate that state, inasmuch as the *ratio* of both forces becomes the more independent of it, the greater the distance of the two needles from one another is assumed. Here, however, it is obviously necessary, at the same time, to consider the position relative to the magnetic meridian, of the magnetic axes of both needles, and of that of the straight line connecting their centres, as also the magnetic state of the first needle; all which cannot be subjected to computation unless we know the law of the force exerted on each other by two elements of free magnetism, or, in other words, with which, according as they are of the same or different denominations, they repel or attract each other. Tobias Mayer had already conjectured this law to be the same with that of general gravitation, *i. e.* that the force is in the inverse ratio of the square of the distance. Coulomb and Hansteen have endeavoured experimentally to confirm this conjecture; and the fact is now completely established by the experiments detailed in Professor Gauss's forthcoming memoir. This law, however, only relates to the elementary effect; for the computation of the total effect of a magnetic body on another, as soon as the nature of the distribution of free magnetism in these bodies is accurately known, becomes a problem purely mathematical, and consequently remains dependent on their casual individual nature; but the greater the distance, the less the influence of this individuality becomes; and if the distance be very great, we may, *cæteris paribus,* assume (as indeed follows from the above principle,) the total effect to be inversely proportional to the cube of the distance. The product of this cube into the fraction which expresses the ratio of the effect of the first needle, and of the terrestrial magnetism on the second needle, will therefore, as the distances continually increase, tend to a determined limit. A proper combination of observations at several judiciously selected distances will, being mathematically treated, make us acquainted with that limit, from which may be deduced the *ratio* of those two quantities the product of which was derived from the observed times of vibration. The combination of both results will then obviously give those two quantities themselves.

The experiments for comparing the effects of the magnetism of the earth, and of the first needle on the second, suspended by a thread, may be conducted in two different ways; inasmuch as the latter may be observed either in a state of motion or of rest. The former is best effected by placing the first needle in the magnetic meridian of the second, whereby the time of a vibration of the latter is either increased or diminished, according as poles of the same or of different names are opposed to each other. The comparison of the time of vibration thus changed, with that occasioned by terrestrial magnetism alone, or rather, the comparison of an increased with a diminished one (under opposite directions of the first needle), will then readily lead to the ratio sought. The second mode is that of placing the first needle in such a manner that the direction of its influence on the second makes an angle with the magnetic terrestrial meridian; when the angle of deviation from the meridian, in a state of equilibrium, will equally lead to the knowledge of the ratio sought. And here, too, it is more advantageous to compare with each other two opposite deviations, under opposite positions of the first needle. The most advantageous position of this needle is along a straight line drawn through the middle of the second and perpendicular to the magnetic meridian. The first mode agrees upon the whole with that proposed some years ago by Poisson; but the experiments, as far as we have any record of them, made by some natural philosophers with a view to apply that mode, have either entirely failed, or their results can at best be considered only as imperfect approximations.

Professor Gauss, who has made frequent trials of both those modes of proceeding, is satisfied that the second is, on many accounts, far preferable to the first.

The real difficulty consists in this, that other elements depending on the individual nature of the needles, enter, as well as the value of the limit, into the influences observed. That effect is represented by a series which proceeds by the negative powers of the distance, beginning from the third; where, however, the following terms become more considerable as the distance is smaller. Now those following terms are to be eliminated by means of several observations; but a slight acquaintance with the theory of elimination easily convinces us that unavoidable errors of observation will never fail to endanger the exactness of the results, as the number of co-efficients to be eliminated is greater; so that their number need not be very considerable to render the results of computation entirely useless. No precision, therefore, in the results can be expected, unless such considerable distances are employed as will make the series rapidly converge, and a few terms of it suffice. But in this case the effects themselves are too small to be determined with exactness by our present means of observation; and thus the ill success of the experiments hitherto made is readily explained.

However easy, therefore, in theory the methods of reducing the intensity of terrestrial magnetism to absolute unities may appear, yet their application will ever remain precarious until magnetic observations have attained to a much higher degree of precision than they have hitherto possessed. It is with this view that Professor Gauss has followed up several ideas long ago entertained by him relative to the improvement of our means of observing; confidently expecting that magnetic observations will, ere long, be carried to a degree of perfection nearly, if not altogether, equal to that of the most delicate astronomical observations. The expectation has been answered by the result. Two apparatus fitted up in the observatory of Göttingen, and which have been employed for making the observations, of which several are given in his memoir, leave nothing to desire but a suitable locality completely secured from the influence of iron and currents of air.

The following short abstract from the detailed description of the two apparatus and their effect, given in the memoir itself, will no doubt be acceptable to naturalists interested in this kind of research.

Professor Gauss has generally employed needles (if prismatic bars of such strength may be designated by that name) of nearly a foot in length, weighing each about one pound. They are suspended by an untwisted thread of $2\frac{1}{2}$ feet in length, composed of thirty-two threads of raw silk, and thus able to carry even double that weight without breaking. The upper end of the thread is tortile, and the degree of torsion is measured by means of a divided circle. To the south or the north end of the needle (according as the locality renders either the one or the other more convenient), a plane mirror is fixed, the surface of which, by means of two adjusting screws, may be placed perpendicular to the axis of the needle; but scrupulous attention need not be paid to this adjustment, as any deviation may most exactly be measured by the observations themselves, and taken into account as errors in collimation. The needle thus balanced is enclosed in a wooden cylindrical box, which, besides the small aperture in the lid for the passage of the thread, has a larger one in the side, which is rather higher and wider than the mirror already mentioned.

Opposite to the mirror, a theodolite is placed, the vertical axis of which is in the same magnetic meridian with the thread of suspension, and at a distance from it of about sixteen Parisian feet. The optical axis of the telescope is placed rather higher than the needle, and inclined in the vertical plane of the magnetic meridian, so as to be directed towards the centre of the mirror on the needle.

To the stand of the theodolite is fixed a horizontal scale of four feet in length, divided into single millimetres: it makes a right angle with the magnetic meridian. That point of the scale which is situated in the same vertical plane with the optical axis of the telescope, and which, for the sake of brevity, may be denominated the zero point, is marked out by a fine thread of gold depending from the middle of the object-glass, and charged with a weight. The scale is fixed at such a height that the image of a portion of it is seen in the mirror through the telescope, the eye-glass of which is adjusted accordingly. At the opposite side from the needle, in the same vertical plane, and at a distance from the telescope equal to that of the image, a mark is fixed, serving every instant to ascertain the unchanged position of the theodolite.

It is obvious, that if all these conditions be fulfilled, the image of the zero point on the scale will appear exactly on the optical axis of the telescope, and that, so far as an object of known azimuth is visible at the place of the theodolite, we may, by means of this instrument, immediately find the absolute magnetic declination. If, on the other hand, those conditions are only partially fulfilled, then, generally speaking, the image, not of the zero point, but that of another point of the scale, will appear on the optical axis; and if the horizontal distance of the scale from the mirror have been measured with exactness, it will be easy to reduce the amount of the divisions of the scale to the corresponding angle, and thus to correct the result first obtained. By turning the needle in the stirrup (so that the upper surface becomes the lower), the amount of the error of collimation of the mirror may be ascertained with great ease and precision. In both the apparatus, one part or division of the scale is equal to nearly twenty-two seconds; an interval which even the least practised eye may easily subdivide into ten parts.

By this mode of operating, therefore, the direction of the needle and its variations are determined with the greatest possible precision. It is by no means necessary always to wait till it is at rest; as the two elongations to the right and the left may be observed with great accuracy, and their combination, properly managed, will indicate the corresponding point of rest with equal precision. During the antemeridional hours, when the daily variation is most rapid, this may be followed almost from one minute of time to the other.

Of equal importance is this mode of proceeding for observing the duration of the vibrations. The passage of the vertical thread in the telescope before a fixed point of the scale (properly speaking, the reverse is the case), may, even if the whole deviation only amount to a few minutes, be observed with such a degree of precision as never to leave any uncertainty amounting to the tenth of a second in time. The considerable duration of a vibration (about 14 seconds in the most intensely magnetized needles), and the slow degrees by which the arc decreases, are productive of other important advantages: only a few vibrations are required to enable us to determine the time of one vibration with such accuracy, that, though the needle be left to itself for one or even several hours, no doubt will remain on the mind of the observer as to the number of oscillations performed during the interval of his absence. We may commence with vibrations so small (such, for instance, as those with which we generally leave off,) that the reduction to infinitely small vibrations becomes almost imperceptible; and yet, after an interval of six and more hours, the vibrations are still found sufficiently great to admit of having their beginnings observed with all requisite precision.

In cases where anomalies still appear in the observations (which, however will prove so trifling, that with the common means they would have been altogether imperceptible,) they are solely to be ascribed to the current of air which, in the locality where the experiments were made, could not be altogether avoided. To remedy this inconvenience the aperture of the box might be closed by a plane glass; but none perfectly true was within the author's reach, neither could it have been made use of without an inconvenient loss of light.

To the enumerated advantages of this method another may be added, which is, that the observer constantly remains at a great distance from the needle, while in the old mode of proceeding his proximity to it was unavoidable; so that, even if enclosed in a glazed case, it was exposed to the disturbing influence which might be exerted upon it by the warmth of the body, or that of the lamp, by the iron or even the brass which the experimenter might happen to carry about him.

The advantages of stout heavy needles over those of diminutive size, which have been made use of for most magnetical observations, particularly those relating to the time of vibration, are dwelt upon by Professor Gauss; he has since successfully employed one weighing upwards of two pounds, and expresses his conviction, that if needles of from four to six pounds in weight were used, on which slight currents of air would cease to exert any perceptible influence, magnetic observations might attain an exactness and precision unsurpassed by the most delicate astronomical observations. Much stronger threads would indeed be required for suspension, the torsion of which would produce greater reaction; but whatever the strength of the thread may be, the force of torsion must always, and may without any difficulty, be taken into account with the greatest exactness.

The two apparatus may likewise be made use of for another purpose, which, though not immediately connected with the principal subject of the memoir, may still be adverted to in this place. They are the most sensible and convenient galvanometers both for the strongest and weakest energies of the galvanic current. To measure the strongest, it is only required to bring the conducting wire single, and at a considerable distance (at least several feet), into the magnetic meridian below or above the needle; for very weak energies a multiplier is wound round the box containing the needle. Some of the experiments were made with a multiplier of 68 circumvolutions, producing a length of wire equal to 300 feet. No pair of large plates is requisite; a pair of small buttons, or even simply the ends of two different metallic wires dipped in acidulated water, produce a current indicated by the movement of the image along many hundred parts of the scale; but on using a pair of plates of very moderate dimensions, the image of the whole scale, as soon as the circuit is completed, is seen rapidly to dart through the field of vision of the telescope. It is obvious that by this method the measurement of galvanic forces may be conducted with a degree of ease and precision unattainable by the hitherto employed laborious modes by means of observed times of vibration; and it is literally true that by it we are enabled to follow from second to second the gradual decrease of the intensity of a galvanic current, which, it is well known, is more rapid in the beginning. If, in addition, instead of the single, a double (astatic) needle is used, no degree of electro-magnetic energy will be found too small to admit of being still measured with the utmost precision. Here, therefore, a wide field is opened to the naturalist for most interesting investigation.

Not a small portion of this unpublished memoir of Prof. Gauss is taken up by the developement of the mathematical theory; and also by various methods peculiar to the author, such as the determination of the momentum of inertia of the vibrating needle, independently of the assumption of a regular figure; by his experiments with a view to establish the above-mentioned fundamental law for the magnetic effects; and, finally, by the details of the experiments to determine the value of the intensity of terrestrial magnetism, of which last the following may be given as the results, as far as they relate to the intensity of the horizontal part of that force.

I.	May 21	1·7820
II.	May 24	1·7694
III.	June 4	1·7713
IV.	June 24—28 ..	1·7625
V.	July 23, 24 ..	1·7826
VI.	July 25, 26 ..	1·7845
VII.	Sept. 9	1·7764
VIII.	Sept. 18	1·7821
IX.	Sept. 27	1·7965
X.	Oct. 15	1·7860

For unities, the millimetre, the milligramme, and the second in time have been adopted. The manner in which the measurement of the intensity has been determined by them cannot here be specified: the numbers, however, remain the same, provided the unity of space, and that of weight (properly speaking, unity of masses), are changed in the same proportion. These experiments vary partly with regard to the greater or less degree of care with which they were conducted, partly with regard to the places in which they were made, and to the needles employed.

The experiments VII, VIII, IX, were in every respect performed with all the precision which the apparatus in the present state admits of, and the distances were measured with microscopic exactness. In experiments IV, V, VI, X, some operations have been performed with rather less care; and the first three experiments are still less perfect in this respect.

The needles employed in the first eight experiments were not indeed the same, but they were nearly alike in size and weight (the latter between 400 and 440 grammes); the principal needle in experiment X. weighs 1062 grammes; experiment IX. on the other hand was made, with a much smaller needle (weight 55 grammes), merely for the sake of ascertaining the degree of precision, which, all other precautionary means being alike, may be attained in using a needle of such small dimensions: the result of this experiment is therefore much less to be depended upon.

Experiments VII. to X. were made in one and the same place in the observatory; the preceding ones in other places in the same observatory, and in apartments of the author's dwelling-house. No perfectly pure results therefore could be derived from these latter experiments, inasmuch as the iron in those localities, and particularly in the observatory, becoming itself magnetic by the magnetism of the earth, would necessarily react upon the needle, and confound its influence with that of the terrestrial magnetism. Such places, indeed, were uniformly chosen in which neither fixed nor moveable masses of iron were near; nevertheless, even the more distant ones may not have been altogether without their effect upon the operations. However, on casting a look over the different results, it appears probable, that in no one of those localities, the modification of the terrestrial magnetism produced by extraneous influence exceeds the hundredth part of the whole. But results commensurate to the precision belonging to this mode can only be expected in a locality entirely free from the influence of iron.

In order to obtain the intensity of the *whole* force of the terrestrial magnetism, the numbers found are to be multiplied by the secant of the inclination. Mr. Gauss intends at a future period also to treat this element according to peculiar methods; in the mean time he merely mentions that on June the 23rd he has found 68° 22′ 52″ with the inclinatorium of the University collection of instruments,—a result which, as the observation was made in the observatory, and therefore not without the reach of local interference, may possibly require to be rectified by other observations.

GEOLOGICAL SOCIETY.

Dec. 19, 1832.—A paper was read, entitled "Report of a Survey of the Oolitic Formations of Gloucestershire." By William Lonsdale, F.G.S.

This survey was made in consequence of a resolution of the Council, confirmed by the Annual General Meeting of 1832, that one year's dividends of the Wollaston Fund should be applied to the continuing, northwards from Bath, the survey of the oolitic formations commenced by the author of the Report in the year 1827.

The district examined is bounded on the west by the escarpment of the oolitic hills from Toghill, $4\frac{1}{2}$ miles N.W. of Bath, to Meon Hill, near Chipping Campden; and on the east by the foot of the coral-rag-hills, from the neighbourhood of Chippenham to Farringdon, and thence by a straight line passing from Burford to Stow-on-the-Wold and Shipston-on-Stour. The formations examined are the marlstone, inferior oolite, Fuller's earth, great oolite, forest marble, and cornbrash.

The geologists to whose labours the author acknowledges himself much indebted, are, Mr. Smith, Mr. Cumberland, Mr. Weaver, the Rev. William Conybeare, Mr. De la Beche, Mr. Murchison, and Mr. Greenough: he also notices the great advantage which he possessed in having the Ordnance Maps for the base of his survey.

Marlstone.—This formation was originally established by Mr. Smith, and its geological position, as a member of the lias formation, has been subsequently proved by Mr. Phillips, in his valuable work on Yorkshire. In Gloucestershire, the formation consists of about 150 feet of marl and sand, containing, towards the lower part, a bed of calcareous or ferruginous sandstone, abounding with organic remains; and its superior stratum consists of blue micaceous marl, the representative of the alum shale of Yorkshire. The most characteristic fossils are *Gryphæa gigantea* and *Pecten æquivalvis.* The marlstone is co-extensive with the escarpment, and may be traced within it wherever the beds subjacent to the inferior oolite have been denudated.

Inferior oolite.—In the South of Gloucestershire this formation consists of nearly equal divisions of soft oolite and slightly calcareous sand; but in the northern portion of the county, the latter, for the greater part, is replaced by a yellow sandy limestone. The freestone beds, which are not to be lithologically distinguished from those of the great oolite, gradually increase in number and thickness, from the neighbourhood of Bath to the Cotteswolds, east of Cheltenham, where they constitute the whole of the escarpment. This vertical importance is retained through the north of the country examined; but to the eastward of the valley ranging from Stow-on-the-Wold to Barrington, near Burford, a change takes place, both in the structure and thickness of the formation. The freestone beds are there replaced by strata of nodular coarse oolite, containing numerous specimens of *Clypeus sinuatus:* the sandy portion consists of only a thin bed, and the thickness of the whole formation is diminished from 150 feet to about 50. The most characteristic fossils which were noticed by the author, are *Clypeus sinuatus, Terebratula fimbriata, Modiola plicata, Pholadomya fidicula, Trigonia costata, Gryphæa columba* (Sowerby), *Lima proboscidea,* and *Ammonites corrugatus.*

The formation occupies, in Gloucestershire, a much greater superficial importance than has been hitherto assigned to it. Besides forming the upper part of the escarpment, it constitutes, to the south of Cheltenham, the inclined plane which ranges between the crest of the hills and the ridge of Fuller's earth and great oolite, and, to the north of that town, the summit of the whole of the hills, with the exception of an occasional capping of great oolite.

Fuller's earth.—This argillaceous deposit is of much less importance in the district surveyed than in the neighbourhood of Bath. The mineral to which it owes its designation is wanting, or is represented by only an occasional bed of impure, useless Fuller's earth. Its greatest thickness in Gloucestershire is estimated not to exceed fifty feet: in the Cotteswolds it was found to be not more than twenty-five; and the deposit was ascertained to thin out to the north-east of a line passing from the neighbourhood of Winchcomb to Burford.

Great oolite.—The threefold arrangement of upper rags, fine freestone, and lower rags, into which this formation was divided near Bath, does not prevail through the whole of the district examined. The upper rags, consisting of soft freestone and hard shelly oolite, were traced to Cirencester; but to the north-east of that town they are replaced by a rubbly white argillaceous limestone. In the middle division, fine workable freestone is of partial occurrence; and the greater number of the beds are composed of hard oolitic limestone. The lower rags, consisting of coarse shelly oolites, resting upon closely-grained or crystalline limestone, extend from Bath to Wotton Underedge; but in the neighbourhood of that town a change occurs, and their position is occupied by beds of fissile calcareous limestone. These strata were traced through the whole of the north-east of Gloucestershire, and to the neighbourhood of Burford. They are extensively worked as a tile-stone; possess the lithological character of the Stonesfield slate; have their fissile property developed by exposure to atmospheric agency; contain *Trigonia impressa,* the characteristic fossil of Stonesfield; and, on comparing the strata of Burford with those which rest at Stonesfield on the slaty beds, it was found that an almost perfect identity of character and order of position prevailed at the two localities. The following table contains Dr. Fitton's accurate enumeration of the beds of Stonesfield (see Zoological Journal, vol. iii.), and a list of those wrought at the Windrush quarries near Burford.

Burford.		*Stonesfield.*
Top. Rubbly limestone	1 foot.	Top. Rubbly limestone.
Brownish marlstone ...	6 feet.	Clay.
Rubbly limestone	4 feet.	Limestone.
Pale sandy marl.........	3 feet.	Blue clay.
Rubbly marlstone	½ foot.	Oolite.
Light-coloured clay ...	½ foot.	Blue clay.
Rag and freestone	15 feet.	Rag, oolitic limestone.
Sandy laminated grit.		Sandy bed, containing the slate.

The author states that he was indebted to Mr. Greenough for the first suggestion that the slate of Gloucestershire would prove to be the equivalent of the slate of Stonesfield.

The author, in alluding to the fossils of the great oolite, remarks on the important changes which are effected by removing the Stonesfield slate from the forest marble to the bottom of the great oolite.

Forest marble.—The Bradford clay, which separates the great oolite from the forest marble in Wiltshire, was observed only in the most southern part of Gloucestershire. Of the forest marble itself, the survey afforded no new characters. It was found to consist of a thick stratum of laminated shelly oolite, interposed between beds of sandy clay, containing laminæ of grit; and to have, from Bath to near Fairford, for its uppermost stratum, a deposit of loose sand, containing large masses of calcareous grit.

Cornbrash.—This formation consists, through nearly the whole of its range, of a thin deposit of rubbly, hard, compact limestone; but in the neighbourhood of Malmsbury it is composed of thick strata of crystalline limestone, alternating at their lower extremity with beds of sand, and surmounted by a stratum of sandy clay, containing laminæ of grit.

The author, in conclusion, notices four faults which affect all the strata from the lias to the forest marble: they occur at Stow-on-the-Wold; Clapton, near Bourton-on-the-Water; Brookhampton, near Cheltenham; and between Tetbury and Cirencester.

January 9, 1833.—An Essay, entitled "Observations on Coal," by W. Hutton, Esq. F.G.S. was first read.

The author was led to the observations contained in this essay by pursuing the method of microscopic examination which has been so successfully employed by Mr. Witham. On examining, with the microscope, one of the thin slices of coal in which Mr. Witham lately discovered a distinct vegetable texture, the attention of the author was excited by the remarkable appearance of several cells in that part of the coal where the texture of the original plant could not be distinguished. Tempted to extend the inquiry, he procured an extensive series of slices, taken from the several varieties of coal found at Newcastle and the contiguous district.

The coal of the Newcastle district is considered by the author to be of three kinds. The first, which is the greatest in quantity and the best in quality, is the rich caking coal so generally esteemed; the second is Cannel or Parrot coal (Splent coal of the miners); and the third, the slate coal of Jameson, consists of the two former, arranged in thin alternate layers, and has, consequently, a slaty structure. In these varieties of coal, even in samples taken indiscriminately, more or less of the vegetable texture could always be discovered; thus affording the fullest evidence, if any such proof were wanting, of the vegetable origin of coal.

Each of these three kinds of coal, besides the fine distinct reticulation of the original vegetable texture, exhibits other cells, which are filled with a light wine-yellow-coloured matter, apparently of a bituminous nature, and which is so volatile as to be entirely expelled by heat before any change is effected in the other constituents of the coal. The number and appearance of these cells vary with each variety of coal. In caking coal, the cells are comparatively few, and those which do exist are highly elongated. Their original form the author believes to have been circular; and he attributes their present figure to the distention of gas confined in a somewhat yielding material, subject to perpendicular pressure. In the finest portions of this coal, where the crystalline structure, as indicated by the rhomboidal form of its fragments, is most developed, the cells are completely obliterated. In such parts the texture is uniform and compact: the crystalline arrangement indicates a more perfect union of the constituents, and a more entire destruction of the original texture of the plant.

The slate-coal, or the third variety above mentioned, contains two kinds of cells, both of which are filled with yellow bituminous matter. One kind is that already noticed in caking coal; while the other kind of cells constitutes groups of smaller cells of an elongated circular figure.

In those varieties which go under the name of Cannel, Parrot, and Splent Coal, the crystalline structure, so conspicuous in fine caking coal, is wholly wanting, the first kind of cells are rarely seen, and the whole surface displays an almost uniform series of the second class of cells, filled with bituminous matter, and separated from each other by thin fibrous divisions.

After describing these appearances, and illustrating them by drawings, the author proceeds to speculate on the origin of the cells in Cannel coal. He considers it highly probable that they are derived from the reticular texture of the parent plant, rounded and confused by the enormous pressure to which the vegetable matter has been subject.

The author next states, that though the crystalline and uncrystalline, or, in other terms, perfectly and imperfectly developed, varieties of coal generally occur in distinct strata, yet it is easy to find specimens which in the compass of a single square inch contain both varieties. From this fact, as also from the exact similarity of position which they occupy in the mine, the differences in different varieties of coal are ascribed to original difference in the plants from which they were derived.

The author next adverts to the escape of inflammable gas from coal, and cites various interesting facts, principally from the authority of Sir H. Davy and Mr. Buddle, in proof of the existence of inflammable gas ready formed in coal while contained in the mine; of the immense quantity which is sometimes emitted by blowers, indicating a free communication between the reservoirs in which it resides; and of the great pressure to which it is there subject. He ingeniously shows the probability of the gas existing within the coal in so compressed a state as to be liquid. A consideration of these circumstances induced the author, while engaged in his microscopic inquiries, to search for a structure in coal capable of containing gas; and he accordingly discovered a system of cells, different from any before mentioned, and apparently adapted for that purpose. These supposed gas cells are found empty, are generally of a circular form, occur in groups which communicate with each other, and each cavity has in its centre a small pellet of carbonaceous matter. The author establishes a clear distinction between these gas cells and those above described as being filled with bituminous matter; for the anthracite of South Wales contains the former, but is quite free from the latter. He also states, on the authority of Mr. F. Foster, that the anthracite of South Wales affords a free disengagement of inflammable gas when first exposed to the air.

A communication "On Ophiura found at Child's Hill, to the N.W. of Hampstead," by Nathaniel Thomas Wetherell, Esq. F.G.S. was then read.

After noticing the rare occurrence of Ophiura, and that in England they had hitherto been observed only in the chalk and the lower division of the oolitic series, the author states, that he discovered, in 1829, several specimens of a species of Ophiura in the septaria of the London clay of Child's Hill; that they were associated with some of the most characteristic shells of that formation; and that he had found fragments of the same Ophiura in a septarium from the Highgate Archway.

Jan. 23.—A Geological Memoir was read "On a portion of Dukhun, East Indies," by Lieut.-Col. W. H. Sykes, F.G.S. F.L.S. &c.

The author describes his track as bounded on the west by the range of mountains usually called the Ghauts by Europeans, from a misconception of the term Ghaut, which simply means a pass, the proper name being the Syhadree; on the north by the Mool river, on the east by the Seena river; on the south by a line drawn from the city of Beejapoor to the town of Meeruj, continued up the Krishna and Quina rivers to the hill fort of Wassota in the Ghauts; comprising an area of about 26,000 square miles, and lying between the parallels of north latitude 16° 45′ and 19° 27′, and east longitude 73° 30′ and 75° 53′.

The whole of this tract, whether at the level of the sea or at the elevation of 4500 feet, is composed of distinctly stratified, horizontal, alternating beds of basalt and amygdaloids, without the intervention of the rocks of any other formation. Similar stratification and structure is instanced in Malwa, and in the Vindhya, Gawelghur, and Chandore ranges of mountains.

The Dukhun (the mean elevation of the valleys and table-land of which is about 1800 feet above the sea) is described as rising very abruptly by terraces from the country at its base: to the eastward it declines by terraces; but these being low, and occurring at long intervals, excite little remark. On the top of the Ghauts there are numerous spurs or ranges of mountains extending to the E. and S.E. The valleys between them are either narrow, tortuous and fissure-like, or wide and flat; both ends being of nearly equal width. A river runs through each valley, having its source at the western end. The author does not think it physically possible for the present rivers to have excavated any of these valleys. Those of a fissure-like character might be referred to a period when the country was heaved up from below the sea, if such ever took place; but this explanation would not account for the broad flat valleys margined by scarped mountains.

The author notices successively the extensive occurrence of columnar basalt, and instances numerous localities of basaltic pavements of pentangular slabs; being, in fact, the terminal planes of basaltic columns. He also notices singular insulated heaps of rocks and stones, the loose parts of which manifest a disposition to geometrical forms. He witnessed repeated occurrences of nodular basalt, or *basalt en boules*; of stupendous escarpments; of dykes of great length, in some instances crossing each other; of strata of ferruginous clay under compact basalt, which, in different localities, pass from friable to jaspery; the occurrence of pulverulent [carbonate of ?] lime in seams; and minute nodular limestone on the surface and in the banks of rivers. Crystallized [carbonate of ?] lime was noticed as an imbedded mineral only. He observed numerous veins of quartz and chalcedony traversing the basaltic strata, and supplying the major part of the siliceous minerals abundantly strewn over the country, such as, agates, jaspers, hornstones, heliotrope, semiopal, stilbite, heulandite, mesotype, ichthyophthalmite, pseudomorphous quartz, &c. &c.; and he mentions the occurrence of muriate and carbonate of soda, of the ores of iron which are worked into the celebrated wootz steel, and of thermal springs. The author did not observe any conformation of the mountains resembling the craters of extinct volcanoes, nor did he find organic remains of any kind.

The paper concludes with some general observations (limiting their application to the 25th degree of north latitude) on the amazing extent of the trap, laterite, nodular limestone, granite and gneiss formations in the peninsula of India. From the geological papers of Capt. Dangerfield, Capt. Coulthard, Major Franklin, Dr. Voysey, and Mr. Calder, the continuous trap region would appear to occupy an area of from 200,000 to 250,000 square miles; and from the observations of the Rev. Mr. Everest, Mr. Royle, Mr. Babington, Mr. Calder, and Dr. Voysey, it may ultimately be found that the ramifications extend eastward to the Rajmahl trap-hills on the Ganges, and southward through Mysore to the extremity of the peninsula. With respect to the age of this formation, Major Franklin states, that in Bundelkhund it rests on a sandstone which he considers identical with the new red sandstone of Europe: the trap would therefore be posterior to the carboniferous series, and belong to the supermedial order. But the Rev. Mr. Everest adduces valid reasons for questioning the correctness of Major Franklin's opinion, and it would consequently be idle to speculate on an æra without sufficient data to assist in determining the question. The author suggests the manner of the formation of the horizontal beds of basalt and amygdaloids, with their *parallel, superior,* and *inferior planes* and *vertical edges,* as a subject of curious and interesting speculation.

From the observations of Mr. Calder, the Rev. Mr. Everest, Mr. Stirling, Dr. Davy, and the author, the laterite formation is found to extend for several hundred miles, with few interruptions, along both shores of the peninsula, and into Ceylon. Ample evidence is given of the occurrence of nodular and pulverulent lime all over Dukhun and Hindoostan. With respect to granite and gneiss, Dr. Voysey collected facts which led him to believe that these rocks constituted the basis of the whole peninsula, and, on this belief, must occupy an area, roughly calculated, of about 700,000 square miles.

The author is not aware of the occurrence of sedimentary rocks in Western India south of Baroach, excepting such as may have resulted from the consolidation of comparatively recent alluvium.

Finally, the author considers the characteristic geological features of the peninsula to consist in the amazing extent of the trap, and the horizontal position of its stratified beds; in the granitic basis of the whole country; in the existence of trap veins in granite; the absence, as far as is at present known, of that uniform series of rocks constituting the formations of Europe; in the extended and peculiar nodular limestone and laterite formations; the occurrence of pulverulent [carbonate of?] lime in seams; and in the non-discovery hitherto of the fossil remains of extinct animals.

The memoir was accompanied by a coloured map, two sections of the country, several sketches of its physical features, and numerous rock and mineral specimens.

A letter was afterwards read, addressed to the Rev. Prof. Buckland, D.D. V.P.G.S. by Joshua Trimmer, Esq. F.G.S., respecting the discovery of marine shells of existing species on the left bank of the river Mersey, and above the level of high-water mark.

Feb. 6.—A paper was read, entitled "Notes to accompany a Map of the Forest of Dean and the Country adjacent, coloured geologically," by Henry Maclauchlan, Esq. F.G.S., employed in the Ordnance Survey.

The author commences his memoir by acknowledging the aid which he received from his colleagues employed in the Ordnance Survey, Messrs. J. and R. Wright and Mr. Carrington; and the valuable assistance afforded him by the Rev. W. D. Conybeare, Dr. Buckland, Mr. De la Beche, Mr. Mushet, Mr. H. James, Mr. Bathurst, Mr. Ormerod, Mr. M. Teague, Mr. Bennett, and Mr. Hale.

The district coloured by the author comprises an area of about 1000 square miles. Its western boundary is defined by a line passing from Gold Cliff, near Newport, to Preston on the Wye, eight miles N.W. of Hereford; and its eastern by another ranging from Didmarton to Stroud, Gloucester, and Hanley Castle, four miles E. of Malvern.

The author successively describes the band of transition limestone which extends, with little interruption, from Shucknell Hill, four miles and a half N.E. of Hereford, to Flaxley, near Westbury-on-Severn; the old red sandstone of the district; the carboniferous limestone; coal-measures; new red sandstone; superficial gravel; and also the faults in the coal-field of the Forest of Dean.

LINNÆAN SOCIETY.

February 19, 1833.—A paper was read, entitled, "Observations on a new Genus of the Order *Musci*." By William Valentine, Esq. F.L.S.

This genus is founded on the *Phascum Stoloniferum* of Dickson; and the principal characters are, its lateral fructification, and the presence of Conferva-like shoots, which the author regards as real stems. These shoots are developed long before the fructification makes its appearance. After a time, the perichætia sprout forth from the sides of the stems and branches, in the form of buds, which, towards maturity, send down radicular fibres (like the perichætia of *Dicranum adiantoides*, and many of the creeping *Hypna*), which penetrate the soil to a great depth for so minute a plant. From these perichætia, young branches are sent forth, and then the parent stem is hardly discernible, or entirely decays. The structure of the stem and branches, which consist of a single series of elongated cells, is peculiar, and totally unlike that of all other Mosses.

The above observations were made on the *P. Stoloniferum* only; but from the presence of Conferva-like shoots, and the similarity in the texture of the leaves, the author thinks they will be equally applicable to the *P. serratum, cohærens,* and *crassinervium,* which will consequently be referrible to the same genus, which he has named *Cladoma,* with the following characters:—*Theca* integra, deoperculata. *Fructus* lateralis.

March 5.—Part of a paper on the *Myrsineæ,* by M. Alphonse De-Candolle, was read.

March 19.—An extract was read from a letter, addressed to R. H. Solly, Esq. F.R.S. and L.S., by Mr. William Griffith, Assistant-Surgeon in the Honourable East India Company's Service, containing some curious remarks on the change of insertion in the stamina of *Mirabilis.* Some time before the expansion of the flower, the stamina are found to be hypogynous; but as the contraction of the tube of the perianthium increases, the filaments are gradually separated, as if by ligature, at the strangulated part just above the ovarium, leaving their lower ends in the form of five setigerous glands; their upper portion becoming united to the tube of the perianthium for a considerable way above the contraction, and thence deriving their nourishment. Mr. Griffith could not find any vascular connexion between the filaments and the perianthium: he thinks the adhesion takes place by means of loose cellular tissue. Mr. Griffith remarks, that the stem in this plant deviates from the dicotyledonous structure, the centre being composed of cellular tissue, with bundles of woody fibre interspersed, thus approaching the *Monocotyledoneæ.*

A notice was read, extracted from a letter from Capt. King, R.N., stating that Mr. J. MacArthur, of Paramatta, had a specimen of *Ornithorhynchus,* from the mammæ of which he had squeezed a large quantity of milk. There were no nipples, but the milk oozed out through pores. This direct confirmation of the discovery of Professor Meckel appears to have been obtained in New South Wales, about the same time that Mr. Owen was demonstrating the same fact (as published in the Phil. Trans. for 1832) from the anatomical examination of specimens in England*.

A communication from Professor Buckland was read, "On the adaptation of the structure of the Sloth to its peculiar mode of life."

The opinion of mankind in general, as well as of all naturalists to the present time, has been, that the sloths are instances of imperfect organization. Differing in many respects in their plan of structure from other quadrupeds, every difference was considered to be a defect; and hence they were supposed to be condemned to lead a life of misery. Cuvier remarks, that the elephant alone varies in as great a degree as the sloth, from the general plan of nature in the formation of this class; but the variations in the elephant correspond with one another, so as to produce a harmonious result adapted to its habits, "mais dans le paresseux chaque singularité d'organization semble n'avoir pour resultat que la faiblesse et l'imperfection, et les incommodités qu'elle apporte à l'animal ne sont compensées pas aucune avantage." (*Ossemens Fossiles*, vol. v. part 1. p. 73.)

These notions have arisen from a want of attention to the habits of the animal, which are described by Professor Buckland, from the information of Mr. Burchell, and by quotations from the works of Piso and Marcgrave, and Mr. Waterton.

The sloths spend their whole lives in trees; and the forests of Brazil, and other parts of South America where they are found, are generally so dense, that they can move from tree to tree by the interwoven branches without descending. When obliged to come down, their passage on the ground, to reach the next nearest tree, is attended with much difficulty, as they are not formed for walking. Their food consists of the young leaves and buds; and these furnish them with sufficient moisture, for (as Mr. Burchell observed in the case of several which he kept) they never drink.

They climb trees with ease, and hang to the branches by their long and curved claws. In this position, suspended with their back downwards, they move along under the branch with facility, and sufficient rapidity to surprise those who have observed only their awkward attempts at progression on the ground. They generally sleep in the fork of a branch, with all their four legs clasped round it.

Professor Buckland examines in detail the anatomical structure of these animals, and quotes the accounts given by authors who have written upon them; from which it is shown, that so far from the peculiarities of their form and organization being sources of inconvenience and instances of imperfection, they are beautiful adaptations to their peculiar habits.

The position of the feet, and the form and size of the claws, are admirably suited for grasping a branch; and the length of the arm and fore-arm enables them to reach the end of the branches which are too slight to bear their weight.

It has long been observed, that the sloth differed from all the other mammalia in the number of the vertebræ of the neck, which is nine,

* A notice of Mr. Owen's paper will be found in Lond. & Edinb. Phil. Mag. & Journal, vol. i. p. 384.—Edit.

while all the rest of the class have only seven. Hitherto, this deviation from the general plan has seemed to be without a motive; as it appeared, that where length and flexibility were required, these qualities were perfectly attained by lengthening each vertebra, without adding to the number of them. The reason of the exception is now pointed out. The sloth, in its inverted position, and confined in its line of march to follow the branch on which it is placed, requires the power of looking directly backwards. As a long neck would be an incumbrance, it has received an additional number of short joints, which enable it to turn its head within a short space.

This remark appears to have been first made by Mr. Burchell, who has observed these animals, with great facility, to twist their heads quite round, and look in the face of a person standing directly behind them.

FRIDAY-EVENING PROCEEDINGS AT THE ROYAL INSTITUTION OF GREAT BRITAIN.

The evening meetings commenced on the 25th of January, when Mr. Brande delivered a discourse on Chemical Notation.

Mr. Brande stated that his objects in bringing the subject of chemical notation before the Members of the Royal Institution were twofold; to inform the readers of foreign chemical works of the meaning of the symbols generally adopted abroad, and to submit to the consideration of English chemists, and especially of teachers of chemistry, a system of symbolic notation, more consistent with algebraic notation, and not open to the inconveniences and misconstructions which the adoption of Berzelius's system would probably involve. He then adverted generally to the great advantages that had been conferred upon chemistry in its theoretical and practical relations, by the gradual development of the atomic theory; and as all notation was a mere concise statement and expression of the facts furnished by that theory, he proceeded, in the first instance, to offer a few preliminary remarks in illustration of its principles, taking for the purpose the several chemical combinations presented to us by the union of the elements of the atmosphere, nitrogen and oxygen, which, in the immense ocean of aëriform matter that surrounds our planet, are in a state of mechanical mixture, but which may be made to enter into chemical union, and then give rise to no less than five definite products. Mr. Brande then exhibited the character of the separate constituents of the atmosphere, and opposed them to the above combinations; namely, the nitrous oxide, the nitric oxide, the hyponitrous acid, the nitrous acid, and the nitric acid, and referred to a table, showing the relative proportions per cent. of the nitrogen and the oxygen in these several compounds. From this table it was obvious that they exhibited a striking instance of the law of multiple proportions; for the numbers were such, that 14 parts (by weight) of nitrogen, were shown successively to combine with 8, 16, 24, 32, and 40 parts of oxygen, these latter numbers being to each other as 1, 2, 3, 4, and 5; accordingly, in the language of the atomic theory, one atom of nitrogen was said to combine successively with one, two, three, four, and five atoms of oxygen; and we obtain the numbers 22, 30, 38, 46, and 54, as the equivalents or atomic weights of the compounds. In designating these by notation, foreign writers employed a capital initial letter to represent the base, and the respective proportionals of oxygen were indicated by dots over it: thus the

Nitrogen was represented by	N.
Nitrous oxide, by	$\dot{N}$.
Nitric oxide, by	$\ddot{N}$.
Hyponitrous acid, by	$\dddot{N}$.
Nitrous acid, by	$\ddddot{N}$.
Nitric acid, by	$\overset{.....}{N}$.

Mr. Brande then proceeded to point out several objections to this system when carried to its full extent and applied to practical purposes; and explained the suggestions of Mr. Whewell in reference to an improved and less objectionable method, in conformity with which he proposed to represent the above five compounds as follows:

Nitrogen..............	n
Nitrous oxide..........	$n + o$
Nitric oxide............	$n + 2\,o$
Hyponitrous acid	$n + 3\,o$
Nitrous acid	$n + 4\,o$
Nitric acid	$n + 5\,o$

The small n representing an atom, or equivalent (i. e. 14 parts by weight) of nitrogen, and the o an atom (or 8 parts) of oxygen; the attached number showing the number of such atoms in the compound.

Mr. Brande then proceeded to explain the principle upon which hydrogen, in the system of equivalent numbers, was adopted as unity, and represented water as a compound of an atom of hydrogen, and an atom of oxygen, or $h + o$; more concisely indicated by the letter q, as proposed by Mr. Whewell *. Thus the liquid nitric acid consists of an atom of oxygen, with five atoms of nitrogen and two atoms of water, and would be represented thus: $\overline{n + 5\,o + 2\,q}$. The line above, or parentheses, indicating that the elements are in chemical combination.

The compounds of carbon with oxygen were then adverted to (the equivalent of carbon being 6), and were as follows:—

Carbonic oxide............	$c + o$
Carbonic acid	$c + 2\,o$
Oxalic acid	$2\,c + 3\,o$

Ammonia was stated to be a compound of $n + 3\,h$ (one atom of nitrogen and three of hydrogen), and cyanogen of $n + 2\,c$ (or one atom of nitrogen and two of carbon).

With this preliminary information, Mr. Brande proceeded to show some applications of the system of notation to the explanation of chemical decompositions, such as that of oxalate of ammonia into cyanogen and water: $(\overline{2\,c + 3\,o} + \overline{n + 3\,h}) = n + 2\,c + 3\,q$: that of ni-

* See Phil. Mag. & Annals, N. S. vol. x. pp. 104, 405.—Edit.

trate of ammonia into nitrous oxide and water, $(\overline{n + 5\,o} + \overline{n + 3\,h}) = 2\,(n + o) + 3\,q$: and that of cyanic acid and water into bicarbonate of ammonia, $\overline{n + 2\,c + o} + 3\,q = (n + 3\,h) + 2\,(c + 2\,o)$.

More complex cases, some selected from mineral combinations, were then mentioned as susceptible of similar notation. The compounds of hydrogen and carbon (hydrocarburets) were also selected, as, from the peculiarity of their atomic constitution, peculiarly susceptible of such illustration. We subjoin the compounds that Mr. Brande selected; but our limits prevent the insertion of the entire table which is necessary to the full explanation of their composition :—

Olefiant gas	$2\,c + 2\,h$
Fire damp	$c + 2\,h$
Bicarburetted hydrogen............	$6\,c + 3\,h$
Quadricarburetted hydrogen........	$4\,c + 4\,h$
Naphtha..........................	$6\,c + 6\,h$
Naphthaline	$20\,c + 8\,h$

In conclusion, Mr. Brande adverted to certain symbolic abridgements, convenient where it was unnecessary to specify the whole of the elements : thus nitrous and nitric acids might be represented by $n^{\backprime}$, n'; the former characterized by a grave, the latter by an acute accent, as suggested also by Mr. Whewell; and the hyponitrous acid by $\underline{n}^{\backprime}$—a short horizontal stroke being in the last case added beneath the letter.

In further illustration of this subject, the following were suggested as the symbols of sulphur and its acids.

Sulphur	s
Hyposulphurous acid......	$\overline{2\,s + 2\,o}$, or $\underline{s}^{\backprime}$
Sulphurous acid..........	$\overline{s + 2\,o}$, or $s^{\backprime}$
Hyposulphuric acid	$\overline{2\,s + 5\,o}$, or $\underline{s}'$
Sulphuric acid	$\overline{s + 3\,o}$, or s'
Liquid sulphuric acid	$\overline{s + 3\,o, + q}$, or $s' + q$

The alkaline bases and other metallic oxides might be designated either in detail, or simply by their initial capital letters, always using the Latin names of the metals. Thus—

A	Ammonia..	=	$\overline{n + 3\,h}$	C	Calcia, or Lime	=	$\overline{cal + o}$
P	Potassa ..	=	$\overline{po + o}$	B	Baryta	=	$\overline{ba + o}$
S	Soda......	=	$\overline{so + o}$	Str	Strontia	=	$\overline{str + o}$
L	Lithia	=	$\overline{li + o}$	M	Magnesia	=	$\overline{mag + o}$

One or more secondary letters must be employed, as the above cases show, where requisite for distinction; but as Mr. Brande stated that he was about to publish a table of equivalents, with the whole of their corresponding symbols, for the use of the chemical students in the Royal Institution, we shall refer to that for details.

Feb. 1.—Mr. Faraday on the Identity of Electricity derived from different sources.—The development this evening was the matter of a paper lately read to the Royal Society, but not yet noticed in our "Proceedings of Learned Societies." In the present state of his experimental investigations, it became a matter of great importance with Mr. Faraday, to prove or disprove the identity of the common, voltaic, animal and other electricities, especially as of late many opinions have been put forth unfavourable to the view of their identity. On comparing them by their various powers, however, he saw no reason to doubt their being alike; and taking common and voltaic electricity, he proceeded to prove the matter as regarded the chemical and magnetic power of the agent in these forms. He first showed the chemical power of common electricity, in part confirming, and in part correcting Dr. Wollaston's results, and extending them so as to render the proof visible to the whole audience. He then did the same with the magnetic powers of common electricity, showing their nature and amount, and fully confirming Colladon's statement. Finally, by reference to chemical and magnetic action, Mr. Faraday compared the electricity from a common machine to that evolved by a single pair of plates, and found that two wires, one of zinc, the other of platina, $\frac{1}{18}$ of an inch thick and $\frac{5}{16}$ apart, immersed to the depth of $\frac{5}{8}$ of an inch in acid, consisting of 1 drop of oil of vitriol to 4oz. of water for $\frac{1}{18}$ of a minute, evolved electricity equal to that which could be supplied by a fine plate electrical machine 50 inches in diameter, and in full action during 30 revolutions.

Many modes were then shown by which common electricity could be converted as it were into voltaic; and in one case the charge of a large Leyden battery was passed through the tongue, producing no other effect than that occasioned by the contact of two pieces of zinc and silver.

Feb. 8.—Sir Anthony Carlisle gave an account of his views relative to the causes of supposed Hereditary Diseases. He denied their derivation from parent to child, and considered them as brought on by weakness of constitution, or improper food or clothing, or unhealthy situations as to climate, &c., and gave such directions with regard to the general course of procedure as to clothing, food, habits, &c., as he thought best fitted to counteract their production or development.

Feb. 15.—Professor Ritchie on a Peculiar Mode of Communicating Scientific Knowledge to Youth.

Mr. Ritchie stated, that to give effect to any system, the teacher must possess certain qualifications. Among these must be an accurate knowledge, not only of the particular branch which he teaches, but also of collateral subjects, that he may be able to draw his illustrations from the most interesting sources. In order to give a boy some confidence in his own strength, the teacher should carefully avoid using any epithet which might lead the boy to believe that he was stupid —the term 'Dunce' ought not to be known in schools.

The plan usually adopted in teaching elementary science in schools, is the synthetical mode of instruction. The plan recommended as having been found most successful, is strictly analytical. In arithmetic, for example, a system of rules is placed before the pupil, and he is told to do a certain number of sums by those rules. When the boy has succeeded in doing so he receives praise, and believes he has done all that is necessary, whereas he has scarcely done anything. He sees not the principle on which the rule is founded. His reasoning powers have scarcely been called into exercise. His memory is thus burdened with a load of rules without one connecting principle. It is a very common observation with a boy, when he cannot solve a question in arithmetic, that he has forgot the rule. Had he got principles instead of rules, arithmetic would have become as it were a part of himself. He could no more forget these principles than he could forget his own name.

When a boy has obtained an accurate view of the great leading principles of arithmetic, which may be attained in a few months, he should be gradually led on to geometry. The ordinary mode of teaching geometry in schools is to place the Elements of Euclid in his hands. Now this Work, notwithstanding its many excellencies, is not well adapted to the instruction of boys. The demonstrations are too verbose and perhaps too formal for youth; and the arrangement, however logical, is not the arrangement according to increasing difficulty of solution, which ought to be the arrangement for instruction: besides, it is entirely synthetical. Reading Euclid, as it is called, may improve the memory, and give a sort of mathematical precision to the language, but it certainly does not bring into play the reasoning and inventive faculties of youth. The analytical mode constantly employs the reasoning powers, and is the only mode which can rouse the dormant inventive powers of youth.

Boys are always anxious to see the use of what they learn. Now this may be done from the very commencement of their geometrical studies. Let them be taken out to the fields and shown the applications by taking the angles subtended by distant objects, which may be accomplished by means of very simple instruments that can be made for a few shillings. By taking a few angles, and measuring, by means of a tape or chain, a few lines, the boys would be delighted to construct the figure by means of their protractor and diagonal scales, and thus to ascertain by measurement the distances of remote objects, &c.

By pursuing this plan, Dr. Ritchie stated, he found that he could interest boys in their mathematical studies from the very commencement, and having done so, their progress must keep pace with the interest thus excited.

The whole discourse was illustrated by numerous striking examples.

Feb. 22.—Mr. Faraday on the Practical Prevention of Dry Rot. In order to preserve timber from the destructive ravages of dry rot and other species of decay, a gentleman (Mr. Kyan) has proposed and practised the application of a solution of corrosive sublimate. A solution in the proportion of a pound of this substance, (cost about three shillings,) in five gallons of water is prepared, and the timber immersed in it for a week. A load of fifty cubical feet is found to absorb about five gallons, and is then removed; and after drying in the air it is fit for use.

Timber thus prepared has stood extraordinary trials against parts of the same timber unprepared, having been introduced for several years into situations liable to rot, and resisted, whilst the other ran to decay.

Mr. Faraday having full trust in the preservative powers, had more particularly examined the state of the corrosive sublimate with reference both to its removal by washing or soaking, and so leaving the timber liable to decay after, and to its volatility, and the consequent production of a bad atmosphere, for he had found corrosive sublimate to be volatile at common temperatures.

Portions of canvass and calico, which had been prepared by Mr. Kyan, were therefore washed several times, until water could remove nothing more; but on digesting them in dilute nitric acid, mercury was found, proving that the substance had been previously in *combination.* Again, such washed portions, together with similar specimens unprepared, were put into a cellar to decay, and in eight weeks had undergone no change; whilst the others were covered with mould, mildew, &c.;—so that the preservative powers remain. Being in this state of combination capable of resisting the action of water, Mr. Faraday expressed himself free from any fear as to injurious effects arising from the volatility of the substance.

March 1.—Mr. Faraday; An Investigation of the Velocity and Nature of the Electric Spark and Electric Light. This investigation, though delivered by Mr. Faraday, has been carried on by Mr. Wheatstone. We refrain from entering into it here, as we expect to have the pleasure of submitting the whole to our readers.

March 8.—Mr. Wheatstone on some Properties of the Impressions produced by Light on the Organs of Sense. This also, we hope, will appear shortly in our pages.

March 15.—Mr. Donaldson on the Drainage and Sewerage of London, with Mr. Martin's Plan for its Improvement.

March 22.—Mr. Carpmeal on Recent Improvements in the Salt Works of Great Britain.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

Feb. 25.—The Rev. W. Whewell read a continuation of his Memoranda on the Architecture of Normandy.

After the meeting, Professor Airy gave an account (illustrated by models and diagrams) of his researches into the numerical value of the mass of Jupiter. He observed that, next to the elements of the planetary orbits, this value is the most important for the calculation of the phænomena of the solar system, on account of the magnitude of the perturbations which it produces. Its effects on the motion of Saturn are very considerable; those on the small planets still greater; and those on Encke's comet so great, that a few years ago the truth or falsehood of the theory of resistance depended upon the use of one or other of two values of the mass differing only one eightieth part. He then gave an historical account of the principal determinations previous to the present. Newton, from observations of the fourth satellite made by Pound, determined the mass to be $\frac{1}{1070}$ of the Sun's mass; Laplace, from the same observations, found nearly the same value; and Bouvard obtained nearly the same from the perturbations of Saturn; Nicolai, from the perturbations of Juno, found $\frac{1}{10539}$ (a value generally adopted by the German astronomers); Encke, in examining whether the absolute attraction of Jupiter on the Sun and on Vesta were the same, found $\frac{1}{1050 \cdot 4}$, remarking at the same time that Nicolai's value would satisfy his observations nearly as well. Professor Airy's observations were made on the elongation of the fourth satellite in right ascension. Thirteen sets of observations were made, in each of which the planet and satellite were observed upon twenty-four or thirty-six wires. The first set was never calculated. The second and third were rejected, because the axis of the declination circle of the equatorial employed in these observations, was not strictly perpendicular to the polar axis (all the other adjustments being tolerably exact). It appeared in the sequel, that in these two observations the instrument had been in opposite positions, and the mean of the determinations might therefore have been retained. The effect of such a want of adjustment was guarded against in the other observations, by reversing the instrument in the middle of the series. For calculation of the mass, the place of the satellite was computed from Laplace's theory, leaving only the mean distance indeterminate. In the course of these calculations, some remarkable numerical errors were discovered in the *Mécanique Céleste*: in particular the mean longitude of the satellite is in error by nearly one third of the circumference. On comparing the different results, it appeared that all the observations in which the satellite followed the planet indicated a greater mass than those in which it preceded the planet; this is attributed by Professor Airy to a constant difference in the mode of observing a satellite and a planet. The value of the mass which from these observations is most probable, is $\frac{1}{1048 \cdot 5}$; Encke's value, however, would satisfy the observations nearly as well. The value assigned by Nicolai is by no means so probable, as it makes the sum of the squares of errors nearly four times as great; and Laplace's mass, which makes the sum of the squares about thirty-four times as great, is exceedingly improbable.

LI. *Intelligence and Miscellaneous Articles.*

ACCOUNT OF AN AURORA BOREALIS, SEEN AT CAMBRIDGE ON THE 13TH OF MARCH. BY PROFESSOR AIRY.

A WELL marked specimen of Aurora Borealis was seen at Cambridge on the evening of Wednesday, March 13th. It was first noticed at about half-past eight (nothing remarkable having been visible a few minutes before that time), when its appearance was that of a dark cloud, with a broad bright upper edge; the boundary line of the upper edge passing a little below Polaris and below Venus. The impression upon three persons who saw it at this time was, that the dark part was opake. Shortly after (probably between 8^h 35^m and 8^h 40^m Cambridge mean time) it was noticed that the bright edge began to divide into streamers, and that stars were visible through the dark as well as through the bright part. At this time the upper edge of the bright band, or the upper extremities of the streamers, reached very little above Polaris, and perhaps two degrees above Venus. The length of the streamers did not at the utmost exceed six or seven degrees. A bright spot formed itself below Venus, and continued in that situation almost permanently during the whole appearance. The streamers, when first formed, were curved, with their convexity towards the west: afterwards they were straight, and their direction nearly perpendicular to the band (perhaps deviating from this direction towards the direction of a vertical). While watching the streamers east of Polaris, a shooting star was seen above them; the direction of its path coincided with the interval between two streamers produced, but we were not certain whether it approached to them or receded from them. The lateral motion of the streamers was very remarkable. Some of them remained stationary (changing their length only) till they disappeared; and no instance to the contrary is recollected in the streamers west of α Persei. But between Polaris and α Persei, most of the streamers, as soon as they were well formed, moved steadily and uniformly to the west (none of them in the opposite direction). I cannot undertake to say that any of them moved from Polaris to α Persei; but several of them moved through the greater part of that distance, and with a velocity which (judging from several estimations) would carry them from Polaris to α Persei in twenty-four seconds. The streamers generally travelled in companies of four or five. In one instance a streamer in motion came in contact with one which either was stationary, or was moving more slowly: they united, and formed a broad streamer, which travelled onwards. This was noticed distinctly (by one of the party) in one instance, and suspected in several instances. This lateral motion of the streamers has not, I think, been sufficiently noticed, but it is sometimes one of the most remarkable circumstances; and in one instance (Oct. 9, 1830,) I have seen a body of streamers, extending over 30 degrees of length (measured along the band or arch), travel over thirty or forty degrees of azimuth, but from west to east, without sensible alteration. During the whole appearance of March 13th, the phænomena extended little to the east of Polaris. A few minutes before nine the band began to divide into mases, each mass having a feathery structure. One of these (which soon faded) was about Polaris; another covered Cassiopeia; a third was on and above Venus. The two latter remained till about a quarter past nine, and then seemed to have gradually died away. A bright light remained in the N.W. horizon, but no arch or streamers arose.

The irresistible impression on the persons who saw this Aurora was, that it had some connexion with the clouds; but from the extreme fineness of the evening, and the total absence of clouds (except that from which the Aurora seemed to originate), it is probable that, if highly elevated, it has been seen by many persons at distant stations.

The wind was about E.S.E., gentle, with an appearance of a northern current in the upper regions of the air (before sunset). The barometer about 29·5, falling; the thermometer about 33°. The air had been extremely dry. At about half-past one in the morning (five hours after the commencement of the Aurora Borealis) the sky was very suddenly overcast; the stars were quite bright, when a cloud formed itself, which in one or two minutes blackened the whole sky. The next morning was cloudy, with black frost: some snow fell, with E. wind.

Observatory, Cambridge, March 14, 1833. G. B. Airy.

LUNAR RAINBOWS.

To the Editors of the London and Edinburgh Philosophical Magazine and Journal.

Gentlemen,—As I cannot find that any one has disproved the commonly received opinion (first propagated by Aristotle) that lunar rainbows are never visible except at or near the full moon, I beg to mention that I saw one last night, during a heavy shower, at half past eleven o'clock, *when the moon had not completed her first quarter* by fourteen hours. It was entire, and *very conspicuous*, but colourless.

I am, Gentlemen, your very humble Servant,

Redruth, 27th February, 1833. Richard Edmonds, Jun.

COMMEMORATION OF THE CENTENARY OF THE BIRTH-DAY OF PRIESTLEY.

In pursuance of the announcement in our Number for February, the Commemoration of the Centenary of the Birth-day of Dr. Priestley, considered as the principal founder of Pneumatic Chemistry, took place at the Freemasons' Tavern, on Monday, March 26th. The Chair was taken by Dr. Babington, at six o'clock, when about one hundred and twenty gentlemen, comprising many of the most distinguished cultivators of chemistry and other branches of science, as well as amateurs and patrons of science and literature, and others taking a strong interest in the reputation of Priestley, sat down to dinner. Among them we observed Mr. Lubbock, Dr. Bostock, Mr. Hatchett, Dr. Daubeny, Dr. Roget, Sir G. Cayley, Mr. G. W. Wood, M.P., the Rev. J. Corrie, Mr. M. Phillips, M.P., Hon. D. G. Halyburton, M.P., Mr. B. Hawes, Jun. M.P., Sir John Rennie, Mr. G. Rennie, Dr. Prout, Mr. Travers, Sir A. Crichton, Dr. Bright, Mr. Knowles, Mr. J. E. Gray, Mr. I. L. Goldsmid, Dr. Paris, Professor Cumming, Sir Francis C. Knowles, Mr. Fox, Dr. Ure, Mr. W. Smith, the Conde de Funchal, Mr. Bingley, Mr. Porrett, Mr. R. Knight, Mr. A. Aikin, Mr. C. R. Aikin, Mr. Children, Dr. Rees, Mr. E. Forster, Mr. Pepys, Mr. R. H. Solly, Mr. S. H. Christie, Dr. Copland, Mr. Hennell, Mr. Wheeler, Mr. Yates, Mr. Faraday, Dr. Turner, Dr. Ritchie, Mr. John Taylor, Mr. R. Taylor, Mr. Hudson, Mr. Dollond, Dr. Marshall Hall, Mr. Brayley, jun., and Mr. Bate. After dinner the assembly was addressed on the merits of Dr. Priestley by some of the most eminent men of science present. A very imperfect report of the proceedings having appeared in some of the newspapers, we hope to give a more full and correct account in our next

PRESENT WORK OF THE FIVE BEST STEAM-ENGINES IN CORNWALL.

The following is an extract from the Monthly Reports of the Steam-Engines in Cornwall, showing the lbs. weight lifted one foot high, by consuming a bushel of coal, by five of the best Engines, in each month, for six months.

1832. July.	Borlase's Engine, Wheal Vor Mine, Cylinder 80 in. diameter, T. Richards, Engineer. 80,886,732	Wilson's Engine, Wheal Towan Mine, Cylinder 80 inches, Sam. Grose, Engineer. 73,500,826	Trelawny's Engine, Wheal Vor Mine, Cylinder 80 inches, Tho. Richards, Engineer. 67,909,112	Shear's Engine, Consolidated Mines, Cylinder 65 inches, Hocking & Loam, Engineers. 66,260,617	Hudson's Engine, East Crinnis Mine, Cylinder 76 inches, Sims & Son, Engineers. 65,453,087
Aug.	81,367,402	73,604,324	66,730,721	Swan's Engine, Binner Downs Mine, Cylinder 70 inches, Greger & Thomas, Engineers. 65,413,607	65,296,325
Sept.	84,714,451	Trelawny's Engine, Wheal Vor Mine, Cylinder 80 inches, Thos. Richards, Engineer. 69,540,018	Wilson's Engine, Wheal Towan Mine, Cylinder 80 inches, Sam. Grose, Engineer. 68,304,819	Hudson's Engine, East Crinnis Mine, Cylinder 76 inches, Sims & Son, Engineers. 65,631,427	Shear's Engine, Consolidated Mines, Cylinder 65 inches, Hocking & Loam, Engineers. 63,378,374
Oct.	84,756,336	Wilson's Engine, Wheal Towan Mine, Cylinder 80 inches, Sam. Grose, Engineer. 68,860,098	Trelawny's Engine, Wheal Vor Mine, Cylinder 80 inches, Tho. Richards, Engineer. 65,640,849	Shear's Engine, Consolidated Mines, Cylinder 65 inches, Hocking & Loam, Engineers. 64,979,465	Leed's Engine, Great Work Consols Mine, Cylinder 60 inches, Thos. Richards, Engineer. 63,276,639
Nov.	84,193,703	67,738,946	64,790,281	Hudson's Engine, East Crinnis Mine, Cylinder 76 inches, Sims & Son, Engineers. 64,463,129	Powlet's Engine, Marazion Mines, Cylinder 60 inches, Sam. Grose, Engineer. 64,173,196
Dec.	91,353,246	Polgooth Engine, Polgooth Mine, Cylinder 66 inches, Sims & Son, Engineers. 71,031,031	69,274,225	Shear's Engine, Consolidated Mines, Cylinder 65 inches, Hocking & Loam, Engineers. 65,357,228	Taylor's Engine, Consolidated Mines, Cylinder 70 inches, Hocking & Loam, Engineers. 65,213,602

CAMBRIDGE MEETING OF THE BRITISH ASSOCIATION.

The meeting of the British Association for the Advancement of Science for this year is to be held at Cambridge; to commence on the 24th of June, and end on the following Friday. A notice respecting the arrangements for visitors, and inviting the attendance and communications of men of science, has been issued by the Cambridge Secretaries, Professor Henslow and the Rev. W. Whewell. All who are aware of the gratification, and the advantage to science, with which the former meetings have been attended, must, we are persuaded, feel a wish to be present. A full Report of the Proceedings of those meetings is nearly ready for publication, containing a large body of valuable scientific information.

CONTINENTAL ASSOCIATION OF PHILOSOPHERS.

The next meeting of German Philosophers, similar to those which have been already held at Berlin, Hamburg, Heidelberg and Vienna, and from which have been derived our York, Oxford, and approaching Cambridge Associations,—is appointed to be held this year at Breslau, the capital of Silesia.

LUNAR OCCULTATIONS FOR APRIL.

Occultations of fixed Stars by the Moon, visible at Greenwich in the Year 1833. *Computed by* THOMAS MACLEAR, *Esq.; and circulated by the Astronomical Society.*

*** The *angles* are reckoned *from* the northernmost point, and also *from* the vertex, *towards* the right hand, round the circumference of the Moon's image, as exhibited in an inverting telescope.

An Asterisk (*) annexed to the time of the phænomenon is intended to denote that the Star is *on*, or *near to*, the meridian, at that time.

1833.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions.				Emersions.			
				Sidereal time.	Mean time.	Angle from North Point.	Angle from Vertex.	Sidereal time.	Mean time.	Angle from North Point.	Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
April 6	30 o^2 Libræ	6	1744	15 2*	14 1	29	25	16 2	15 2	283	289
10	(255) Sagitt.	6·7	2195	16 37	15 20	139	126	17 29	16 12	217	213
22	61 δ^1 Tauri	4	488	7 46	5 44	96	134	8 51	6 49	288	327
	64 δ^2 Tauri	4·5	492	8 26	6 24	62	102	9 14	7 12	319	0
	68 δ^3 Tauri	5	499	9 31	7 28	171	213	9 49	7 47	208	250
23	(37) Tauri	7	624	7 38	5 32	100	138	8 50	6 43	272	213
25	63 *p* Gemi.	6	916	12 20	10 5	137	179	12 53	10 38	205	247
27	83 *q* Cancri	6	1130	8 1	5 39	123	100	8 51	6 29	207	199
28	37 Leonis	6	1222	7 57	5 31	71	52	9 10	6 44	245	240
May 6	2 Sagittarii	6	2020	13 29	10 31	104	76	14 32	11 34	226	205
	(195) Ophi.	7	2033	16 43	13 44	109	104	17 53*	14 54	231	236
25	37 Leonis	6	1222	16 44	12 31	125	165	under	the h	oriz.	
29	88 Virginis	7	1571	17 16	12 47	37	66	18 14	13 44	273	307
31	30 o^2 Libræ	6	1744	11 57	7 21			a near	approa	ch	

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

Days of Month, 1833.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Penzance. Max.	Barometer. Penzance. Min.	Barometer. Boston $8\frac{1}{2}$ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Penzance. Max.	Thermometer. Penzance. Min.	Thermometer. Bost. $8\frac{1}{2}$ A.M.	Wind. Lond.	Wind. Penz.	Wind. Bost.	Rain. Lond.	Rain. Penz.	Rain. Bost.
Feb. 1	29·676	29·476	29·696	29·584	29·17	48	31	50	40	35	N.	NW.	calm	0·18	...	...
2	29·021	28·869	29·184	29·078	28·55	54	41	54	42	43	SW.	SW.	S.	·20	1·295	0·80
3	29·574	29·100	29·578	29·387	28·50	53	40	51	43	44	SW.	NW.	W.	·06	...	·16
○ 4	29·975	29·714	29·878	29·781	29·25	56	49	53	46	41	SW.	W.	calm	...	0·150	...
5	29·986	29·909	29·878	29·866	29·35	56	41	53	50	50·5	W.	W.	calm	·02	·170	...
6	30·048	29·846	29·875	29·666	29·55	55	45	51	48	45	S.	W.	W.	·06	·040	...
7	29·954	29·813	29·872	29·666	29·35	55	42	53	44	46·5	SW.	SE.	calm	·03	...	...
8	29·760	29·740	29·678	29·666	29·17	55	40	54	45	47	SW.	W.	W.	·67	·400	...
9	29·820	29·672	29·678	29·566	29·20	51	31	50	44	42·5	W.	SW.	calm	·16	...	·23
10	29·430	29·261	29·378	29·219	28·94	54	45	51	43	45	SW.	SW.	W.	·28	1·300	·12
☾ 11	29·437	29·415	29·425	29·322	28·85	54	39	51	47	47·5	SW.	SW.	W.	·16	0·340	·10
12	29·656	29·516	29·578	29·172	28·87	53	44	52	40	40	SW.	SW.	calm	·26	·550	...
13	29·487	29·305	29·228	29·048	28·87	51	41	51	44	46	SW.	NW.	calm	·08	·560	·25
14	29·461	29·165	29·490	28·928	28·75	49	36	51	43	46·5	W.	SW.	W.	·50	·500	...
15	29·494	29·176	29·554	29·393	28·71	45	31	43	38	36	W.	NW.	N.	...	1·055	·90
16	29·832	29·683	29·793	29·778	29·21	45	29	48	35	35·5	W.	NW.	calm	...	...	·22
17	29·783	29·579	29·540	29·481	29·32	46	39	51	38	35	S.	SW.	calm	·14	·560	·17
18	29·700	29·561	29·778	29·684	29·03	49	37	48	43	42	W.	NW.	calm	...	0·135	·29
● 19	29·913	29·733	29·840	29·334	29·45	48	39	51	38	41	N.	SE.	calm	·16	1·000	...
20	29·466	29·061	29·884	29·417	28·65	45	38	46	36	46	W.	NW.	SW.	·32	...	·30
21	30·060	29·788	29·940	29·934	29·48	47	39	47	43	43	NE.	N.	E.	...	...	·22
22	30·070	30·002	29·900	29·834	29·67	45	35	49	42	41	NE.	NE.	E.	·02	0·180	...
23	29·918	29·738	29·790	29·484	29·53	44	31	47	43	39·5	N.	SE.	calm	...	·250	...
24	29·559	29·486	29·434	29·240	29·29	46	35	50	43	38	E.	W.	E.	·12	...	...
25	29·603	29·513	29·434	29·304	29·20	50	35	48	40	38	S.	SW.	E.	·10	·200	·58
26	29·369	29·233	29·187	28·978	28·95	46	35	47	40	45	S.	SW.	S.	·18	·500	·15
27	29·237	29·096	29·334	29·134	28·78	48	30	47	40	43	E.	NW.	SE.	·08	·170	·05
☽ 28	29·314	28·898	29·037	28·884	28·87	45	37	48	38	39	S.	NW.	calm	·20	·375	...
	30·070	28·869	29·940	28·884	29·09	56	29	54	35	42·1				3·98	9·730	4·54

Remarks.

London.—Feb. 1. Cloudy: clear and cold. 2. Boisterous, with heavy rain. 3. Clear and windy. 4. Drizzly and foggy. 5—7. Cloudy and fine. 8. Heavy rain. 9. Rain: frosty at night. 10. Stormy and wet. 11. Clear and windy. 12. Heavy rain, and stormy in the morning: fine. 13. Rain: cloudy and windy at night. 14. Stormy and wet: fine: heavy rain at night. 15. Boisterous, with rain: clear. 16. Clear and frosty: fine. 17. Slight haze: rain. 18. Fine. 19. Clear and fine: rain. 20. Stormy and wet. 21—23. Cold and cloudy. 24. Rain. 25. Rain: fine: showery. 26. Stormy and wet: fine. 27. Rain: showery: fine. 28. Overcast: stormy and wet: clear and fine.—A large quantity of rain has fallen this month.

Penzance.—Feb. 1. Showers: rain at night. 2. Rain. 3. Fair: showers. 4. Showers. 5. Fair: rain. 6. Fair: showers. 7. Clear: evening rain. 8. Fair. 9. Fair: rain at night. 10. Rain. 11. Fair: showers. 12. Fair: rain. 13. Heavy showers, hail, and rain. 14. Fair: rain. 15. Rain and hail. 16. Clear. 17. Rain: fair. 18. Showers, hail, and rain. 19. Clear: rain. 20. Showers: fair. 21. Clear. 22. Rain: fair. 23. Fair: rain at night. 24. Fair. 25. Fair: rain. 26. Rain. 27, 28. Showers.—The wettest month for 15 years.

Boston.—Feb. 1. Snow. 2. Rain and stormy. 3. Stormy. 4—7. Cloudy. 8. Fine. 9, 10. Rain. 11, 12. Fine. 13. Cloudy: rain early A.M. 14. Fine. 15. Rain and stormy: snow early A.M. 16. Cloudy: snow and rain A.M. 17. Fine: rain P.M. 18, 19. Cloudy. 20. Fine: rain & stormy early A.M.: rain P.M. 21. Cloudy: rain early A.M. 22, 23. Cloudy. 24. Cloudy: rain early A.M. 25, 26. Cloudy: rain early A.M. rain P.M. 27. Fine: rain early A.M. 28. Fine: rain P.M.

THE

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AND

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[THIRD SERIES.]

MAY 1833.

LII. *Some Remarks on the Granite found near Penryn, and on the Mode of working it.* By JOHN S. ENYS, *Esq.**.

THE chief intention of this paper is to give publicity to the opinions of the persons employed in the supply of granite shipped at Penryn, and to explain the mode of procuring the rectangular blocks of granite which are used in the construction of large bridges, docks, &c. &c. This hard and compact granite appears to run in parallel ranges through a coarser and softer variety, forming together a granitic district north-west of Penryn, of nearly a circular form, 7 or 8 miles in diameter. One of these ranges is situated on the northern side of the district: another near Penryn on the southern limits. The latter runs N.E. and S.W., and is about 5 miles long and from 1 to 1½ wide; it is marked throughout by large loose rocks on the surface, from whence for many years the quantity exported was obtained. An immense supply from this source is still obtainable; but about five or six years since a second bed was found under some of the larger rocks, and this in several places has been worked in quarries, locally termed "*bals*": these workings are so numerous as to afford strong proof of a continuous range of hard granite. The stone is found more free from stains in depth than at the surface, though the supply is too abundant to require the workings to be carried in any instance to a depth exceeding 14 or 15 feet.

To prevent any misunderstanding, it may be advisable to explain in what sense several words will be used. *Rock* will mean either a solid mass contained between the natural joints

* Communicated by the Author.

of the granite, varying in size from 10 to 500 tons, or loose rocks of equal size; many of these last, however, are probably in their original position, if any judgement can be formed from the coincidence both of the remains of the natural joints and of the "cleaving line," with the quarry masses: such seems the case with the Main Rock or Tolmen, a remarkable and well-known rock, situated near the centre of the hard granite line; it is supported on several large rocks by two points, and measures about 500 tons.

Granite is always measured, and 14 cubic feet are allowed to a ton; though $13\frac{1}{2}$ of fine grit will generally amount to that weight.

Cleaving is used to express splitting up a rock into blocks for sale.

The joints both of the hard and softer granite are very similar, but are more frequent and numerous in the latter variety. They have a strong tendency to meet at right angles, so as to form masses or rocks of a rectangular shape; these are often 50 feet long, and from 10 to 25 feet deep, and seem exactly similar to those at the Land's End and Cape Cornwall, and to the Logan Rock.

A small quantity of a softer stone is occasionally found between some of the joints, which is not observed to be present in exposed headlands. The Arris is likewise square, but on rocks long exposed to the influence of the weather it seems to have been rounded. The rectangular form, however, is as strongly marked as in the quarry, the joints of which are generally visible, though the granite seems in most instances to be in actual contact. Occasionally a rock has been split within two inches of a joint, which has only been found by the breaking of the blocks in working. One of these joints is nearly horizontal, and is called the Bed-way or Floor, and two are nearly perpendicular, one of which is found generally to run in a direction N.N.W. and S.S.E. with a variation of 15 or 20 degrees either way; the third being across. In some quarries this arrangement is extremely regular, though in others many of the joints are "unconformable." Fortunately, blocks for buildings are mostly required of rectangular forms, so that rocks are often split up to great advantage.

To form a cubic block, it is evident three ways of cleaving are required: these are termed by the cleavers or stone-cutters,

1st, Capping or Quartering:

This is a line parallel to the horizontal bed-way or floor. *Capping* is the term used for splitting off the top of a rock in its natural position, or of a block split off which lies as when found in the quarry. *Quartering* is the term when the block is placed on edge, as it usually is when the vertical cleaving has been first effected. The identity of these terms, in regard to the direction of the cleaving, must be attended to in speaking to stone-cutters; they refer only to the different positions of the blocks of granite. The granite splits most easily on this line, and its existence is strongly asserted to be found in some varieties in which the "cleaving line" is not so strongly marked: it is likewise notorious that it is found in elvan or porphyry; so that a workman by attending to this point, constantly gains more wages than others in breaking stones for roads at tutwork*. It is readily known in the quarry; but some skill and practice is required to find it in separated stones:

2nd, Cleaving:

The "cleaving line" is vertical, and is said by the stone-cutters to be in the general direction of the crystals of felspar: it is instantly pointed out by them, but is more readily seen in coarse than in fine grit. Near Penryn it runs N.N.W. and S.S.E., varying 15 or 20 degrees either way: it consequently generally coincides with one of the vertical natural joints, though in many instances it does not correspond, but crosses them, often at an angle of 30 or 40 degrees. In these cases it appears the cleaving line, or general direction of the felspar crystal, keeps it in a N.N.W. and S.S.E. direction, and the joints are "unconformable." Part of the Cornish parapet of London Bridge was supplied from a rock which was split up parallel to the joints, and diagonally across the cleaving line, to prevent waste of stone. The appearance is exactly similar to a common observer, though a practised cleaver would *probably* point out every stone from that rock, certainly if allowed to break the stone:

3rd, The tough way, or across the grain:

This is asserted by the stone-cutters to be a tranverse section of the felspar crystals, and at right angles with the cleaving line.

The power required to effect a fracture on these lines is very different, and has been estimated in the following proportions:—

1st. Capping or quartering .... = 2
2nd. Cleaving .... = 3
3rd. The tough way .... = 5.

In one trial of the tough way and capping way the power used was estimated as follows:—

* *Tutwork* is the term used in Cornwall to denote work contracted for with the workmen *by measure*.—Edit.

	Inches.		Inches.		Sq. In.		Power.	
The capping way measured	24	by	26	=	624	......	5	2 Wedges, 1 Ripper.
The tough ditto	24	by	15	=	360	......	3	...3 Wedges.

This, as far as one trial can do so, more than proves the stone-cutter's assertion;—the cleaver likewise added that the tough way was struck rather harder than the other. The cleaving can be effected on any diagonal line, but the fracture is untrue; it is almost invariably true both on the capping and cleaving lines, though less on the tough way.

In cleaving granite two varieties of gear are in use, Wedges and Cues,—the latter so called from the ox-shoes, which were first used for the purpose; and Rippers and Feathers. The ripper is sometimes called a Tearer; it is only a tapered bar of iron, in fact a circular wedge; it is used in deep holes formed by a borer or jumper; the last is well known in quarries, the former is beat by a man with a sledge-hammer, and turned in the hole by a boy who holds it. The wedge is placed in a groove cut about three inches deep. The cues and feathers are thin slips of iron acting in a similar manner, and are respectively placed against the sides of the groove or hole when the wedge or ripper is placed between them, and struck down with a sledge-hammer, generally of 30 pounds weight. One ripper is considered more than equivalent in power to three wedges: these last are chiefly used for cleaving small blocks of 2 or 3 feet. The holes are bored about 1 foot apart, so that a large rock requires a great number of rippers, which are struck alternately, and slide down without any great friction between the feathers, and cause a pressure sufficient to cleave down 20 or 25 feet in depth. For deep cuts of 24 feet or even less, it is most usual to drive down the rippers in the evening until the iron begins to feel warm, and leave them for the night; in the ensuing morning a fine hair-line fracture will be found, which is easily increased; whereas should it have been attempted to drive the rippers at once, the iron would be destroyed before the rock would be split.

On the other side of the Channel, wedges only were observed to be in use: perhaps this may account for the small size of the granite blocks used in the construction of the docks at Brest. Powder has been sometimes used near Penryn, particularly in capping, and 1 or $1\frac{1}{2}$ pound of it has been known in a 9-feet hole to have effected a true fracture 18 feet long by 9 feet wide.

It would be advantageous to the cleavers, or under-contractors for the supply of granite, if engineers, architects and building contractors would pay some attention to the cleaving of granite.

Various points could be arranged in the courses of stone for building, particularly in heavy work, which would prevent the present waste of stone and labour. Under the present system, of ordering blocks, it often happens a fine rock is split up into small blocks; then comes an order for a large block to be delivered instantly for the same building, when small rocks are obliged to be removed and wasted to obtain another fine rock. A little consideration and arrangement would enable the cleaver to split up his large and small rocks to greater advantage. The waste of granite, from the quantity in sight, may be immaterial, but the waste of labour has often occasioned a heavy loss to the cleaver. To convert rocks to the greatest advantage, a considerable number of blocks, with as much variation in their sizes as is suitable to building, should be ordered of each cleaver; and if possible, an allowance should be granted of 3 or 4 inches in the position of the upright joints, so that an error of cleaving a block 1 inch too short might be rectified by procuring the next 1 inch larger.

The *beating off waste*, that is, reducing the block with a pick of 20 pounds weight to the exact size required, is the most expensive part of the work; and the cleaver often attempts to cleave too nice, and the block is wasted. Should a good arrangement ever take place in ordering granite, a skill would soon be exerted in the conversion of masses of granite similar to that of a shipbuilder in the conversion of timber.

Blocks under 2 tons are carried on the axletree of a pair of wheels, which are loaded in a manner exactly similar to that proposed by Sir H. Stewart for the removal of large trees. Blocks of a larger size require four wheels, and a greater price, to meet the increased difficulty of loading and carriage. This price is regulated by the cubic foot of finished stone, and the contractor is obliged to deliver at Penryn a block 1 inch larger each way as an allowance for fine cutting; this extra inch is not paid for, and the shipowner complains that granite is the heaviest cargo, since the ton or 14 cubic feet of granite seldom weighs less than 22 cwt.

These statements have been made with a hope of attracting attention and inquiry to tracts which seem to have some interest both in a geological and œconomical point of view.

Attached is a map and description by R. W. Fox, Esq. of the hard and compact granite.

Enys, Dec. 1832. JOHN S. ENYS.

LIII. *A Geological Sketch of a Portion of the Granite District near Penryn, referred to in the preceding Paper.* By R. W. Fox, *Esq.**

[With a Map: Plate IV.]

Explanation of the Map.

THE unshaded part is intended to show the direction of the range of hard or compact granite near Penryn, which is nearly N.E. and S.W. Its length about 5 or 6 miles, and its breadth 1 to $1\frac{1}{2}$ mile.

The dotted part represents a coarser granite, less compact, and often friable near the surface.

The granitic district extends many miles towards the north-west, and includes, as there is reason to believe, other ranges of compact granite nearly parallel to the above.

The shaded part represents clay-slate, or "killas," resting on the granite.

Some of the quarries in which the cleavage has been examined, are marked thus, + ; and it appears that the average direction of the vertical cleavage is nearly N.N.W. and S.S.E. There seems to be a remarkable approximation to uniformity in this respect, although not so decided as in the horizontal, or almost horizontal cleavage.

The Main Rock, and some other very large rocks, which are above the surface, seem to correspond with the lines of cleavage; thus affording strong evidence of their being in their original position.

In reference to the statement in the preceding paper relative to the cleavage of granite near Penryn, it may be proper for me to say, that the horizontal cleavage, or "capping" as it is termed, is by no means confined to the granite in question; but on the contrary, it seems to be a characteristic of that rock in different parts of Cornwall; as I find it has been observed at Kit-hill, and other places near Callington, at Rough-tor near Camelford, and at Carnmarth near Redruth. In these districts likewise, the granite possesses natural joints in a horizontal direction, or nearly so, although they are often almost imperceptible, except where they have been enlarged by the action of the weather. Besides these joints, there are other similar ones at right angles to them; but I am not prepared to state whether, as in the Penryn granite, they have any tendency to uniformity in their bearing. In all the cases alluded to, there appears however to be a correspondence between the "capping," and the nearly horizontal joints; on which account it seems reasonable to infer, that the similar joints which abound in the granite at and near the Land's End,

* Communicated by the Author.

and at the Scilly Islands, indicate a cleavage, or tendency to split in the same direction.

I apprehend that granitic ranges occur in various parts of Europe possessing identical characters, as far at least as the joints are concerned, respecting which it seems desirable to obtain specific information bearing on the points in question; for I think it must be evident, that wherever a conformity in the cleavage, or indeed in the joints of different ranges of granite can be established, especially where such ranges are found in contact, there is good reason to consider them contemporaneous, however differently they appear to be circumstanced in other respects.

R. W. Fox.

LIV. *Particulars of the Measurement, by various Methods, of the Instrumental Error of the Horizon-Sector described in* Phil. Mag. vol. lix. *By Mr.* John Nixon.

[Concluded from the Lond. & Edin. Phil. Mag. and Journ. vol. i. p. 108: with Figures; Plate IV.]

By the Second Method.

Theory.—A RAY of light falling, at any angle of *depression*, on the surface of a fluid at rest, is reflected from it at the same angle of *elevation*. Hence parallel rays are reflected from a level surface at their previous *equal* inclination to the horizon. The elevation of any direct ray (A. fig. 1.) at any point (R) of a horizontal surface (HH′) will be the same as that of the reflected ray (C). Another ray B, parallel to A, intersecting C at any point* of it P, will be seen from that point at an elevation equal to that of A at R, and equal to the depression (at P) of the reflected ray C in the direction of R. PL being parallel to HH′, the angle ARH will be equal to BPL, and also to LPR, and the sum of the two latter angles equal to BPR. The rays A, B, although sensibly parallel, might have diverged from one point situated at a considerable distance. Rays from a (fixed) star would be of this description.

At P, a star observed in the direction of B, would also be seen by reflection from a level surface below, as HH′, in the direction of R. As the elevation of the star should equal the depression of its reflected image, an instrument having a *constant* error would not give the two angles equal. However, as the one would be exactly as much in excess as the other was in defect, half their sum would be the correct quantity, and half their difference the constant error, additive to, or

* The horizontal distance of P to R should not exceed a few feet, or the direction of gravity at the two points will not be sensibly parallel.

subtractive from elevations, according as the angle of depression exceeded or fell short of that of elevation.

As the rays of light from the point of intersection of the cross wires of a telescope issue from beyond its object-glass, placed at the sidereal focus, parallel to each other, it is obvious that we determine at once the error of a telescopic-level on measuring by it the elevation of the line of collimation of a similar telescope, and the depression of its reflected image.

Description of the Apparatus, and Method of Observation.

A cistern, formed of a block of wood 9 inches long, 5 inches high, and nearly equally broad, excavated with a narrow margin to the depth of a quarter of an inch below its upper surface, was glued lengthwise upon the horizontal plank (already described) at about the middle of its length. North of the cistern was placed the wooden stand, supporting within its Ys the collimator telescope, which had been lately fitted up by Mr. Dollond, with a stop (in lieu of the slip of pearl), consisting of a plate of brass, having in the centre a minute circular aperture, through which *exclusively* the light derived from the pasteboard, or transit lamp* fixed beyond the eye-glass, passed into the body of the telescope. South of the cistern, now filled nearly to overflowing, stood the sector, placed with its telescope in a line with the collimator, the middle of their object-glasses being about the height of, and almost close to the cistern†. On looking through the telescope of the sector, no reflection of the circular aperture (or luminous disc) could be discerned, as the collimator was gradually inclined from its original horizontal position, until the depression exceeded 12′; an angle quite beyond the limits of the levels. By making the line of sight inclined by at least this quantity to the axis of the cylinder, that axis, and with it the *adjusted* line of collimation (which lies in the same or parallel direction), would, however, be truly horizontal whenever the deviated line of sight, as the cylinder revolved within its Ys, should be found to bisect alternately the minute disc and its reflected image‡. In this case the difference between the observed position of the bubble of either of the great levels and its reversing point would give the constant error.

* The tube being deprived of its lens, the aperture was closed by a disc of gauze paper dipped in mastic varnish.

† Found by experiment to be better than when the cistern stood either lower or higher.

‡ *Demonstration.*—In fig. 1. let PB be the *deviated* line of sight bisecting a star, and PR its direction, and that of the reflected star subsequent to half a revolution of the cylinder. As the latter revolves without

Unfortunately the wire could not be displaced more than 15′, at which angle (of depression) the reflected disc appeared so foggy, indistinct, and even elliptical, that—instead of observing the lower or upper limb (or both),—the vertical diameter, which appeared under an angle of not less than two minutes, was bisected, vaguely no doubt, but as exactly as could be estimated. No reference level was made use of, although clearly proved in a succeeding attempt to be absolutely indispensable. So many sources of inaccuracy led, as might be anticipated, to most unsatisfactory results; the error coming out, not the same by both levels, as should have been the case, but 30″ by the one, and 38″ by the other. An extra horizontal wire, fixed just within the limits of the stop, would insure complete success to this novel method*.

The measurement by the graduated arcs of the sector of the elevation of the disc, and of the depression of its reflection at an angle sufficiently great to render the image well defined, although uncertain in an instrument not subdivided beyond 5″, was of necessity adopted. The inclination of the collimator being increased to about 1°, by means of a wedge fixed to the north end of the under surface of the stand, the latter was glued to the plank. Fortin's level, attached as well to the collimator as to the adjoining side of the Ys which supported it, served to indicate and rectify, during the subsequent operations, the minutest deviation in the inclination of the plank. After the lapse of a day or two, the sector being stationed south of the cistern, with its vertical wire bisecting the disc, the elevation of the upper and lower limbs† of the disc,

disturbing the direction of its axis, it follows that the line of sight was elevated above the axis at the same angle at which it is now depressed below it; its vertical range being BPR. Hence the axis must lie in the direction of the level line PL, which bisects the angle BPR.

Admitting the cylinder to be *flexible*, the line of sight would be elevated above its axis; but on depressing the cylinder by an angle equal to the deflection, the deviated wire would again bisect alternately the direct and reflected disc, and the line of collimation, adjusted *as usual*, would point level.

* The readiest plan of observation by this method may be briefly stated. —The sector being about level, alter the inclination of the collimator until either limb of the disc appears covered by the additional wire of the sector. Then invert the cylinder of the sector; and if the wire now points *above* the *same* limb of the reflected disc, diminish the inclination of the collimator by half the difference, or increase it equally if the image appears above the wire.

When the upper one of the three horizontal wires of a telescope bisects a low star, and the lower one (is moved to bisect at the same instant) its reflected image, the middle wire, when placed *equidistant* from the other two, will obviously point *level*.

† In fact, the horizontal wire was pointed a second or two below the upper limb, and the same quantity above the lower one.

and the depression of the same as seen by reflection, were measured in succession, first on one arc, and, immediately after inverting the cylinder, also on the other. The reversing points of the levels were well ascertained; the stand of the sector being previously glued to the plank, which was kept at one constant inclination by referring to Fortin's level. The measurements, of which the details follow, give on an average 26″ for the error.

January 24th, 1833. Temp. 48° Fahr.

			Means.
		′ ″	′ ″
Elevation of upper limb of disc	by right-hand arc	55 12¾	= 55 18⅜
	by left-hand arc	55 24	
Depression of ditto reflected	by right-hand arc	56 7¾	= 56 9⅛
	by left-hand arc	56 10½	
Difference			0 50¾
Half difference, or constant error			0 25½
Elevation of lower limb of disc	by right-hand arc	53 23	= 53 21
	by left-hand arc	53 19	
Depression of ditto reflected	by right-hand arc	54 17¾	= 54 15
	by left-hand arc	54 12	
Difference			0 54
Half difference, or constant error			0 27
Ditto by upper limb			0 25½
Mean			0 26¼*

By the Seventh Method.

Theory.—Let ES (fig. 2.) represent the elevated line of collimation of a telescope; L the axis of a perfectly cylindrical level-tube, moveable about a horizontal pivot fixed to any part of the telescope. The axis L, as the bubble is supposed to be

* *Remarks.*—1°. When the horizontal wire of the sector appeared on a level with the upper limb of the reflected disc, the height of the fluid in the cistern was increased nearly 0·2 inch, without diminishing in the slightest degree the depression of the image. 2°. A better image was obtained from water resting on quicksilver (the light being derived from white pasteboard) than from either of those fluids, or from treacle, alone. 3°. In every case the image appeared a little more distinct when a sheet of white paper extended horizontally about a tenth of an inch above the surface of the fluid. 4°. However modified the light or apparatus, the reflection of a horizontal wire could not be rendered visible. 5°. The image of the vertical pearl slip was not seen, perhaps from its length, except at a great depression of the collimator; but were the aperture of the stop considerably contracted, the line drawn across the diaphanous slip might be observed with greater certainty than either limb of the minute disc.

about the middle of the tube, must be horizontal. On inverting the telescope, without varying its direction, the level-tube, also inverted, will have its axis, now raised above the line of collimation, inclined to the latter at the same angle as when lying below it; MAS being equal to SAL. When the level is depressed until its bubble fixes about the middle of the (opposite) side of the tube, its axis must have described the arc ML (of the angle MAL), half of which is the elevation of the line of collimation ES.

As a perfectly cylindrical level may be considered impracticable, it will be necessary to substitute *two* levels of the usual construction, one (a) (fig. 3.) secured, as in the figure, to the upper, and the other (b) to the lower surface of an inflexible bar (c). This double level being fixed to the moveable index (bar) of a graduated arch attached to a telescope, which now points at an elevated star, alter the inclination of the index until the bubble of the level (a) comes to rest anywhere within the limits of its scale.—Having inverted the telescope, and bisected the star, depress the index until the bubble of the other level (b), now uppermost, settles wholly in view. Were the levels, with their bubbles at those points of their respective scales at which they became stationary, strictly parallel to each other, then would half the arc, passed over in depressing the index, be equal to the elevation of the star. Ignorant of the inclination of one level to the other, we have, however, but to repeat the observations with the double level reversed in direction, and the average of the two measurements will give the correct altitude of the star, and half their difference the inclination of the levels to each other. The demonstration will be greatly facilitated if we suppose the interior of each level a cylinder to which the exterior is parallel; in which case that surface of the wedge-shaped bar to which the level is soldered will become horizontal whenever the bubble remains in view.

Fig. 5. The inner surface AE of the bar C, as indicated by the level attached to it, is to be considered horizontal. On inverting the telescope, its present inclination to the elevated line of collimation ES will not be varied. The bar, moveable about E, being now depressed until the bubble of the other level, now uppermost, comes to rest at its mark, it is evident that the exterior surface BE, to which that level is attached, must have passed over the arc B′A, which exceeds A′A (= double elevation of ES) by the inclination of the surfaces of the bar to each other, or to the arc A′B′ (= AB). The bar is represented in Fig. 4. reversed in direction, its outer surface AS being horizontal. On inverting the telescope, the

inner surface BS of the bar must be elevated by the arc BA before the bubble of the level attached to it can attain its mark. This arc being in *defect* of double the elevation of ES (or arc A′A) by the arc BA, equal to the angle formed by the surfaces of the bar, whilst the previous measurement *exceeded* it by the same quantity, half their sum will be the correct elevation.

The elevation of a distant object, measured on this plan by a 30-inch telescope carrying a divided arch (of 18 inches radius), of which the moveable index was furnished with a double level, exceeded by $30''\cdot5$ the angle obtained by the corresponding observation with the sector.

When a double level is affixed to the face of a divided circle, move the latter until the bubble of the level uppermost settles with its extremities equidistant from the zero of its scale; then turn the circle exactly half round, and alter the adjustments of the other level (at present uppermost) until the ends of its bubble lie also equidistant from the zero of its scale. The two levels must now be parallel to each other, and the reversing of the connecting bar not requisite. The parallelism might be similarly effected with the bar fixed to the *telescope* of the circle.

By the Eighth Method.

Having secured the bar lengthwise to the tube (cylinder) of which the axis is considered to be horizontal, note the position of the bubble of the uppermost level of the bar, before, and that of the bubble of the other level, after inverting the tube within its Ys. Then take off the double level, and replace it, reversed in direction, with the bubble of the level which may be uppermost at the previous points of its scale. In the event of the tube being perfectly cylindrical (and inflexible), it will be found on inverting it that the other level (of the bar) will come up with its bubble in its original position. Were the tube conical, the bubble would be situated nearer the wider end of it by quadruple the constant error or inclination of the axis of the tube.

Some years ago two reserve levels of the sector were glued to a bar of wood, with the design of ascertaining the error in question; but the levels were repeatedly so much changed in inclination by the *setting* of the glue, as to render the apparatus unserviceable.

By the Sixth Method.

When the ring (or collar) of a telescopic-level nearest the object-glass exceeds the other in diameter, elevations are measured in *defect* by a constant angle. On repeating the observations with the object-glass placed nearest the lesser ring, the measurements, it is evident, will be in excess by the same quantity.

Having substituted Dollond's eye-tube in the place of the object-glass of the sector, the latter was removed to the other end of the cylinder. A comparison of two observations, made with the sector in its original and altered state, indicated an average defect in the former of 23″, or constant error of $\left(\frac{23}{2}=\right)$ 11″·5.

By the Third Method.

Theory.—The surface of the fluid in which a cube or other body floats, is always in contact with it, whatever its direction, at the same points of its sides. A section of the cube through these points would be a horizontal plane. A line, drawn on any surface of the floating cube, will be constantly parallel or inclined at one angle to the horizon during a revolution in azimuth of the cube. Consequently, if we fix a telescope to the floating body, its line of collimation must be either level, or inclined at a constant angle, whatever the (lateral) direction of the telescope. Let the line of collimation lie in the line drawn from the object at which it is directed to another situated diametrically opposite, and should it be found, on reversing the floating body, to point with equal exactness at the opposite one, it must be horizontal.

Captain Kater's floating collimator, constructed on the above principle, when used to prove a telescopic-level, should be placed between it and an auxiliary telescope, of which the line of collimation is directed at the horizontal wire of the level. Having pointed the collimator at this telescope, turn it half round in azimuth, and it will be found directed either exactly at the horizontal wire of the (adjusted) level, or at a small angle above or below it. In the former case the instrument must be perfect; in the next it measures elevations in defect by half that angle; and in the latter, in excess by the same quantity.

On the Effect of Flexure in the Cylinder.

Were the object-glass fixed within one ring, and the cross wires within the other, a slight deflection of the tube could not sensibly affect the parallelism of the line of sight to an axis passing through the centres of both rings. When the stop is fixed, as in the sector, between the rings, the effect of flexure must be to depress the horizontal wire below the level it would otherwise maintain. The line of sight, although still capable of being adjusted to bisect the same object with the tube direct and inverted, must be inclined, at an angle of elevation, to the horizontal axis passing through the centre of the rings. As this deflection evidently increases, or diminishes the error arising from an inequality in the rings, accordingly as the object-glass is fixed nearest to the wider or to the narrower ring, its precise amount should be ascertained.

The error, as measured by the fourth method, being $19''\cdot7$, or $5''\cdot7$, with the object-glass in the place of the eye-tube, the difference of $14''$ will be the sum of the two flexures. As the stop is fixed 6·3 inches from the narrower ring, and 10·8 inches from the wider one, $5''\cdot2$ will be the deflection of the line of sight in the former, and $8''\cdot8$ its value in the latter case. Hence the measurements by the first method, which requires the tube inflexible, will be in defect by $5''\cdot2$; whilst those by the eleventh method, which are not only exclusive of flexure, but also suppose the stop placed equidistant from the rings, must be augmented by half the sum of the two flexures. The peculiar situation of the stop of the eye-tube, made use of in the observations by the sixth method, renders the quantity of deflection uncertain.

The following list contains the instrumental error, as given by each of the different methods, corrected for flexure. The mean of the whole, $21''$, cannot possibly deviate from the truth by more than a second or two.

Method.					No. of Obs.
I.	Error	$16''\cdot5+5''\cdot2$	Flexure $=$	$21''\cdot7$	2
II.	......	....	....	26 ·2	1
IV.	......	....	....	19 ·8	4
V.	......	....	....	21 ·8	8
VI.	......	$11''\cdot5+5''\cdot2$(?)	....	16 ·7	2
VII.	......	....	....	30 ·5	2
X.	......	....	....	21 ·3	18
XI.	......	$13''\cdot7+7''\cdot0$	....	20 ·7	35

Arithmetical mean $22\frac{1}{4}$;—rational mean $21\frac{1}{4}$.

Were the terrestrial refraction unquestionably a constant ratio of the arc of distance, the error of the sector might be ascertained by comparing the observed refraction on short arcs, with its apparent value, for others of greater extent. This method applied to 50 arcs, amounting in the whole to upwards of 8°, and ranging from $3'\cdot0''$ to $25'\ 34''$, determined the true refraction to be $\frac{1}{16\cdot5}$, and the error in question $17''\cdot2$.

Leeds, Feb. 8, 1833. JOHN NIXON.

LV. *An Account of Test Objects for Microscopes. By* ANDREW PRITCHARD, *Esq.**.

[With a Plate.]

EVERY important advance in our knowledge of those bodies in the material universe, from which our earth appears as an atom, has been coeval with, and greatly dependent upon, some augmentation of the powers and effectiveness of telescopes. Before the discovery of the double stars and nebulæ, the goodness of these instruments was determined by their capability of showing the planets and their satellites. But, since our acquaintance with the former bodies, telescopes have to undergo more severe tests, and greater accuracy in their construction is required. What has been advanced in regard to the telescope will be found applicable to the microscope; and to the discovery of certain objects which may be considered as tests of the penetrating and defining powers of this instrument, we may justly attribute the grand and magnificent improvements which the microscope has recently received.

In the perusal of the works of Leeuwenhoek, Dr. Goring met with a passage describing the dust, or imbricated scales, which cover the wings of the silkworm (*Phalena Mori*), from which he was led to suspect there were some peculiar properties in the lines on the feathers and scales of insects, rendering them more difficult to be discerned than other microscopic objects; and the result of his investigation was the discovery of their properties as *tests*—a description of object before unknown in the annals of microscopic science.

Now, as it is undoubtedly of the highest importance to the naturalist that he should know the exact capabilities of his instrument, in order that he may not be led astray in his investigations, by placing undue confidence in it; and as these tests offer the best means of accomplishing this end, I conceive them to be of the greatest value and interest. As no complete account of them is extant, I shall endeavour to supply this deficiency in the present chapter, and illustrate the subject by accurate drawings of them, greatly magnified. * * *

Having ascertained that different test-objects require different degrees of perfection in the instrument used to develope their structure, it became an interesting pursuit to discover those which are best adapted for this purpose, and the peculiarities, in the illumination, &c. under which they are exhi-

* Abridged from the "Microscopic Cabinet." London: Whittaker, Treacher and Co. 1832. An account of this work was given in the Lond. and Edinb. Phil. Mag. and Journ. vol. i. p. 163.

bited with the greatest perspicuity. In this investigation, it was found that there were two distinct properties in a microscope, and that the instrument might possess a very considerable approximation to perfection in the one, and fall short in the other, or *vice versâ*, or might be perfect in both. The lines on the dust or feathers from the wings of the lepidoptera, and those on the scales from the body and limbs of the thysanuræous insects, offered the means of determining their goodness in one particular, viz. their *penetration*, and the structure of the hair of animals, certain mosses, &c. served to ascertain their defining power.

The analogy between telescopes and microscopes is so great, that I cannot be said to digress from my subject by stating that the aforesaid observations apply also to the former of these instruments, which seldom combines the two qualities of penetration and definition to any great extent. Thus, a telescope with a large aperture will frequently resolve clusters of stars, and exhibit nebulæ, while it will fail in defining the disc of a planet, or the moon, with precision; and, on the other hand, one of moderate aperture accurately figured will define the latter, but be wholly inert on the nebulæ and clusters. So a microscope with large aperture and high power will show the "active molecules" and lined objects, while it will not define a leaf of moss, or a mouse hair; and another with a smaller aperture will define the latter, but prove ineffective on the former. This is very manifest in single lenses which require different apertures for different objects*.

The *penetration* of a microscope has been shown to be dependent on its *angle of aperture*, and that whenever this was less than a certain quantity, the lined structure of the scales cannot be rendered visible, however perfect the instrument may be; and the *defining power* is inversely as the quantity of spherical and chromatic aberration.

A proof, or test-object, may be defined to be one which requires a certain degree of excellence or perfection in a microscope or engiscope for the development either of the whole, or some particular part of its structure.

Test-objects are separable into two great divisions; but as I intend only to treat on one of them, it is proper here to point out their distinction. In the first division I place those which operate out of focus, and tell us what the defects of an instrument are. The second, those which, if exhibited by a micro-

* I have a very beautiful sapphire lens (plano-convex of one fifteenth focus) that shows the lines on the *long* brassica very distinct and sharp, when its aperture is large, but will not define a moss satisfactorily with this aperture; but as stops behind the object have the effect of reducing it, with them it shows the latter.

scope, assure us that it possesses certain good qualities. The first division, as artificial stars, enamel dial-plate, wire gauze, &c.*, which inform us of the state of their aberration, achromatism, centering, adjustment, curves, &c., I shall pass over,—as many persons are not disposed to enter into a scientific scrutiny concerning the *causes* of their demerits, and because they are more applicable to engiscopes, or compound microscopes, than to single and compound magnifiers,—and shall content myself by giving some simple means of determining effectiveness by means of the second division.

(1.) *Lepisma saccharina.*—The insects of the families Lepismenæ and Podurellæ are comprehended in the order *Thysanoura* of Cuvier and Latreille; they are small, frequenting damp places, and are of various colours; they leap like fleas.

The scales of these apterous insects must be taken from fresh specimens, for, when long dead, they adhere so firmly to the insect, that they cannot be detached without injury.

Their longitudinal lines slightly radiate from the point of insertion; they are readily seen, and appear flat or square, like the indentations on some bivalve shells: these are the prettiest scales I am acquainted with. There are other lines in various directions, as shown in the drawing of a magnified scale at fig. 1. Plate III. When the candle is so placed as to bring out the latter strongest, and the scale is turned round in the axis of the microscope in certain positions, they will cease to appear connected. In this object it is the sharpness and cleanness of the spaces that chiefly evince the goodness of a microscope, for the longitudinal lines are easily developed.

(2.) The *Morpho Menelaus.*— This butterfly is indigenous to America, the wings are indented, and their superior surface of a highly-polished blue colour.

The imbricated scales from the centre of the superior side of the wing are of a pale blue, mixt with others almost black. The former are mostly broader than the latter, and are the test-objects required; they measure about one one-hundred and twentieth of an inch in length. When viewed in a microscope, they exhibit a series of longitudinal stripes or lines, as shown in the magnified drawing, fig. 2. Plate III. Between these lines are disposed cross striæ, which, with the lines, give it the appearance of brick-work.

The microscope or engiscope under examination should be able to make out these markings, with the spaces between them, clean and distinct. The cross striæ, which give the

* For a particular account of these objects, see Dr. Goring's Memoirs "On the Exact Method of, &c." p. 191. Mic. Cab.

brick-work appearance, are seldom to be seen all over the feather at once. The tissue that covers this scale or feather contains the largest portion of colouring matter, and is often destroyed in removing them from the wing, and along with it the cross striæ. In such cases, the longitudinal lines only can be visible. The damaged specimens are easily known by their paleness.

(3.) (*Alucita pentadactylus*, and *hexadactylus.*)—The ten and twenty Plumed Moths.—The structure of the wings, or, more properly, plumes of these insects, is so peculiar, that few persons acquainted with entomology are strangers to it.

The twenty-plumed moth is more delicate in its form than the other. The feathers or scales, employed as proof objects, must be taken from the body of the insect, and not from the plumes or wings. Their breadth is generally greater than their length, and their form is never symmetrical. They are transparent, and about one one-hundred and eightieth of an inch long. The scale is often partially covered by a delicate, uneven, membranous film, which obliterates the lines on those parts. The longitudinal lines are not difficult to resolve, but their proximity is such, that they require a considerable power and careful illumination to separate them distinctly. They are elegant microscopic objects, but rather scarce. * * *

(5.) The *Clothes Moth.*—(*Tinea vestianella.*)—These small brown moths possess very delicate and unique scales, requiring some tact in the management of the illumination, to resolve their lines distinctly. I should observe, that it is the small feathers only, from the under side of the wing, that must be considered as tests; the others are easy. A magnified view of a small one, about one four-hundredth of an inch long, is given in fig. 3. of Plate III. They are readily made out under the single and doublet magnifiers. This is a favourite object with some, who exhibit it as the standard of excellence. I do not consider it very difficult; though it must be admitted, to bring out the lines sharp and clean, requires an excellent instrument.

(6.) *Pontia Brassica** (Leach.)—The pale slender double-headed feathers, about one eightieth of an inch long, having brush-like appendages at their insertion, obtained from some portions of the wing of this large cabbage butterfly, afford an excellent criterion of the goodness of a microscope. Some connoisseurs prefer them to all others, and form an accurate judgement of an instrument by the manner in which it demonstrates this single object. They are easily detached from the

* This is the *Pieris Brassica* of Latreille.

wing by the point of a quill, but must be gently handled, for, like many others, they are soon mutilated; indeed I have seldom seen them perfect in the ordinary sliders. Those specimens which are easily resolved are readily distinguished, being short, broad, and more opake. There are also found, on the same wing, two or three other sorts, but they are unworthy of notice as proof objects.

In Plate III. at fig. 4*. is represented a sample of the regular proof feather. It is very transparent, and has a yellowish tint; the surface is seldom smooth, as indicated at the part *a*. In the engiscope these inequalities are not so observable, and therefore, when the lines appear strong, the surface is more uniform than in the microscope. This object requires the light more oblique than any other of the lined kind. On this account I have seldom been able to see the lines satisfactorily with Dr. Wollaston's illumination, unless the magnifier was much out of the axis of the perforation. If we throw the light of a candle (placed a few inches behind the stage) obliquely on them, they can be seen very sharp. I have seen them in this way with a simple jewel lens, of only one fifteenth of an inch focus.

(7.) The *Podura plumbea.*—(*Lead-coloured Spring-tail.*)—The body and limbs of these insects are covered with scales, which, from their extreme delicacy, require great care in removing. They are also very soft, and easily wounded. The fluid which exudes from the injury so completely adheres to the scales as to obliterate all their markings. Hence they must be cautiously handled. Those who are desirous of preserving these insects, should keep camphor along with them; through omitting this, I once had a large collection of them consumed by a species of mite (*Acarus*), which had insinuated itself into the box.

I have never been able to see the lines on them with a power much below 250 (that is, one twenty-fifth of an inch focus), and therefore microscopes of a lower power cannot be expected to show them, except of very superior quality; for it must constantly be kept in mind, that that instrument is the best which exhibits an object with the *least amplification*, all other things being equal.

It is affirmed, by a very acute experimenter, of these scales, that "all are difficult, and some seem to defy all power of definition." The latter part of this quotation is perfectly accurate; but I differ in the former, because many specimens,

* The reader should examine this and the other figures with a hand magnifier.

especially the French ones, are very easy, and unworthy the title of *proofs*; and, as they might be substituted for those I am describing, and thus a common instrument might pass for one of superior excellence, I feel justified in giving this caution *.

The size of these scales varies from one nine hundredth to one hundred and sixtieth of an inch in length, and, as they decrease in size, become more transparent. They are of different forms, but possess a general character, easily recognised, by the want of any sharp angles. Under a microscope not having sufficient penetration, the tissue appears devoid of structure or markings; but, when placed in a superior one, and the illumination properly made, they show a series of lines or cords on their surface, and present a much greater variety in their arrangement than the scales of any other species of insect. Some have the lines straight, as shown in the magnified scales, Plate III. fig. 5. and 6, and have two sets of oblique lines on them, similar to fig. 8 †; others are waved and curved, as shown in fig. 7, 9, and 10, while on some of the small ones, as fig. 11, nothing satisfactory has yet been developed. In these figures I have endeavoured to give the appearances which the objects present under the microscope; and it will be observed, on a careful inspection of them, that the lines on fig. 9, 10, and 13, (which are only portions of scales,) are very different from those on fig. 5. and 6, the former ones not being so sharp and defined as the latter.

As a general rule, it will be found that the smaller the scales the more difficult the test; those in fig. 6, however, cannot be included as tests, as they are very easily resolved. I must not omit to notice, also, that the cords on these scales are loosely attached to the tissue, and are often rubbed off in mounting. Of course it will be fruitless to examine such specimens. Those on which the greatest reliance may be placed are similar to fig. 5, though the same scale will assume all the appearances of fig. 8, 9, 10, and 13.

Before leaving the subject of the lined objects, I should notice, that all objects of similar structure are more or less tests, as the lines on the scales of some beetles, one of which, from

* It should be remembered that the exhibition of the lines on these scales is only proof of the penetration of a microscope; and unless the *outline* of the scale is sharp, the instrument is defective in definition.

† As in the scales of the *Pontia Brassica*, only one system of oblique lines can be seen at once; the other system is similar to those in fig. 8, but running in a direction at right angles to them.

the diamond beetle (*Curculio imperialis*) is shown at fig. 12. Plate III.* The lines and markings on certain vegetable tissues, and many others too numerous to name, may also be employed as proof objects. The reason for making a selection of those above described, has been in order to render the task of judging of the merits of an instrument by different individuals more simple and satisfactory, so that by the assistance of the drawings, and a sample of the objects, they may ascertain the quality of an instrument without the trouble of comparing it with others, which are often difficult, and sometimes impossible, to procure.

Definition.

The defining power of microscopes and engiscopes depends on their capability of collecting together all the rays from any one point of the object, or, in other words, their freedom from aberration, and is independent of their penetration; for, if we take an engiscope and view a lined object with the aperture of the objective, as it is usually sold in the shops, its defining power may be very fair; but if we enlarge the aperture so as to enable us to develope the lines which it will then accomplish, the defining power of the instrument will be injured to such an extent as to render the outline quite confused. The great desideratum, then, in microscopes and engiscopes, is to obtain these two qualities combined, which, however, is only rarely attained.

Cylindrical or spherical bodies appear the best suited for ascertaining the goodness of an instrument, as regards definition; and the following examples, which are prefaced by remarks on the method of illuminating them, I deem sufficient for this end.

In the preceding class of objects, *oblique diverging rays* ap-

* The scales from the body of the diamond beetle, either as transparent or opake objects, are by far the most brilliant, in point of colour, of any of the lined class. In viewing them as opake objects, with single lenses, in order to exhibit the lines, the scale must be brought a little within the focus, and the illumination carefully arranged. As you cannot exhibit them with single lenses of a one twentieth or one thirtieth of an inch focus without using silver cups, it is difficult to procure oblique light. As transparent objects, they are much easier managed. They present a mottled sort of colour, composed of the brightest carmine, mixed with purple, blue, and yellow, and their lines are distinctly seen, as shown in fig. 12. As the lines on some of these scales are of easy resolution, it will not be advisable to trust every specimen as a *test*. The small ones from the legs of the Brazilian beetle are the most difficult, and many of these require the most rigid adjustment of the focus and illumination to resolve the lines, and the slightest tremor, though not enough to occasion any sensible dancing (as a carriage at a distance), is sufficient to render them invisible.

pear to be essential for the development of their structure, the degree of obliquity varying, however, with different specimens of scales. The extremes of this variation are the *Podura plumbea* and *Pieris Brassica*, the delicacy of the former requiring almost central light, while the latter requires it very oblique. From this cause artificial illumination is to be preferred to day-light for this class of objects, as the light of a lamp or candle gives the rays diverging without any apparatus whatever. The same effect, however, may be produced in day-light, either with Dr. Wollaston's or Dr. Goring's illuminator, where the rays, after meeting at the focus of their illuminating lens, are permitted to diverge, and, by placing the object out of the centre, oblique vision is obtained. In the investigation of the class of objects now to be described, *direct parallel* rays are preferable, and, indeed, in most cases are essential; and on this account they are scarcely ever well defined by candle- or lamp-light. In these, therefore, clear day-light, directed through the axis of the instrument, should be employed.

1. The hairs of the common mouse (*Mus domesticus*) differ both in size and form; the principal varieties, with their relative diameters, are shown in Plate III. fig. 14, 15, and 16. These are drawn, as seen by transmitted light, and as proof-objects should have their transparent parts clearly and distinctly separated from the darker portions. This remark holds good for the whole tribe of hairs and mosses, and it is from the sharpness with which the parts are separated that a correct opinion of the goodness of an instrument can be obtained. When these hairs are seen by reflected light, that is, as opake objects, their appearance is altered, the dark solid parts reflecting more light than the transparent portion; hence they are lighter than the latter. A peculiar and interesting variety of a large hair viewed in this way is shown at fig. 17; it is engraved from a drawing made by Dr. Goring. * * *

3. The hair from the wing of the bat (*Vespertilio murinus*).—Although this creature is supposed to bear some affinity to that of the mouse, the structure of the hair of these two animals is entirely different: there are, however, great varieties, the principal of which are shown at fig. 18. and 19. The hair in the latter figure is spiral; the former like a succession of cones, the apex of one being inserted into the base of the following.

Many other kinds of hair might be enumerated for the purpose to which I have applied the above; but I deem these amply sufficient to illustrate this part of the subject. As, however, the diversities in the structure of different kinds of hair are worthy of investigation*, I have sketched a few of the most interesting varieties. They are all magnified in the same proportion as the mouse and bat's hairs, which accompany them in Plate III.

Fig. 20. is a hair from the larva of the common dermestes.

Fig. 21. is a white hair from a young cat.

Fig. 22. is the hair of a Siberian fox; and

Fig. 23. the hair of a common caterpillar†.

4. The *Lycæna Argus.*—Among the scales on the underside of the wing of this elegant blue butterfly, are some whose conformation is remarkably singular; their form is represented in fig. 24; their general appearance is not unlike a child's battledore, with its surface covered with spots. I have not been able satisfactorily to demonstrate its structure; but it appears to consist of two delicate tissues, having regular rows of conical spines on the upper one. As a test-object, these spots should be clearly and distinctly separated. When the light is thrown obliquely, they are blended together, appearing like a stripe of unequal breadth; similar to many of the other tests, it is the manner in which they are seen rather than the mere exhibition of them that should be observed. This object I employ for the same purpose as the leaf of an unknown species of moss belonging to the genus Hypnum, which, as it is difficult to procure, renders this substitute an acquisition to the microscopist.

Before I conclude this chapter, it may not be amiss to notice another class of objects, which by the vulgar are considered as positive proofs of the efficiency of an instrument; I allude to the animalcules. Nor does this opinion seem confined to those unacquainted with this subject; but we find it stated by Adams, in his quarto work on the Microscope, p. 430, that the *Monas Termo* (one of the most minute of all the animalcules very abundant in vegetable infusions), "eludes the power of the compound microscope, and is but imperfectly seen by the single." Now, all that is requisite for seeing this object, or any other of the same kind, is to cut off by stops, or otherwise, all superfluous light, so as to reduce the quantity and intensity‡ of the illumination; for, when too much light

* The serratures on the surface of the human hair, especially those from an infant, afford excellent tests, and are very beautiful objects.

† The form of these hairs varies in every species: in some they resemble the feather of the peacock's tail in miniature; others are furnished with tufts of fine hair, and beset with spines.

‡ The reader should observe that *quantity* and *intensity* are distinct from each other: thus, when we employ a small wax taper close to an object, it

is admitted, these minute and delicate bodies are completely drowned. All that is necessary for *seeing* these objects, even in the ordinary compound microscopes (engiscopes), providing they have sufficient magnifying power, is to employ a faint illumination. If, however, the observer is desirous of examining the structure and organization of them, of course he must use an instrument of superior quality; for in this case not only sufficient magnifying power and proper illumination are necessary, but penetration and definition.

LVI. *Narrative of Experiments made with the Seconds Pendulum, principally in order to determine the hitherto unassigned Amount of the Influence of certain minute Forces on its Rate of Motion. By Mr.* James Scrymgeour.

[Continued from p. 251.]

My next inquiry was to ascertain whether a pendulum of the same length made any difference in its time, accordingly as its weight was light or heavy. The only method of ascertaining this experimentally, is to employ the principle of convertibility adopted by Captain Kater in his experiments on the pendulum. The convertible pendulum which I employed was composed of two steel rods placed an inch separate from each other, and firmly joined by cross pieces at the ends as well as at intervals of their length. The knife-edges or portions of cylinders were fixed between the rods, at the points of oscillation and suspension.

In the following experiments the knife-edges were used, and the pendulum was adjusted, first with weights which, along with it, made the whole weight 8 pounds 10 ounces. When the heavy end of the pendulum was lowest, it vibrated exactly with the clock pendulum at an extent varying from 3° to 2°·5; when the light end was lowest, at an extent of vibration varying from 2° to 1°·5.

The pendulum was then transferred to the vessel in which the exhaustion was to be effected. When exhausted to a mean pressure of 1½ inch of mercury, the pendulum, with the light end lowest, vibrated with the clock pendulum at an extent of

will be *intensely* illuminated though the quantity of light is small; but if we employ the flame of a large lamp, &c. at some distance from the object, its intensity will be small, though the quantity of light be great. It will be found generally preferable to employ a small quantity of intense light, rather than a larger portion of weak light; and, if possible, avoid the use of lenses or mirrors, either for condensing or changing the direction of the light.

3°. When the heavy end was lowest, it equalled the clock pendulum at an extent of 3°·5.

The pendulum was next reduced in weight to 4 pounds 3 ounces. When the vibrations were adjusted so as to make them alike, either when the light end or the heavy end was lowest, they were found to be slightly slower than those of the clock, even at the extent of vibration of 0°·5. But in a degree of exhaustion the same as above, with the heavy end lowest, the pendulum equalled the clock at an extent of vibration of 3°. When the light end was lowest, it equalled the clock at an extent of vibration of 2°. The knife-edges were not altered in either of the experiments.

It appeared from these experiments, that when the heavy end of the pendulum was lowest, it vibrated faster by 4·5 seconds in 24 hours, when its weight was 9 pounds 10 ounces, than when its weight was 4 pounds 3 ounces; and when the lighter end of the pendulum was lowest, there was a greater difference between the times of its vibrations in the light and heavy states. When the adjusting weight was shifted towards the heavy end, a great effect was produced on the time of the vibrations of the pendulum when the lighter end was lowest, and but a small effect in the contrary case. When the pendulum was nearly adjusted in both these cases, the difference in the times of its vibrations when the heavy ends were lowest, may be safely taken as the real difference between the times of the vibrations in the light and the heavy states of the pendulum. This difference is no doubt considerable, and is sufficient to give some countenance to the supposition that a heavy pendulum is more influenced by gravity than a light pendulum.

If, however, the approximation in the times of vibration shown in the experiments, with the light pendulum vibrating in air and in the state of exhaustion, be taken into consideration, it will appear that if the pendulum had been properly adjusted and made to vibrate in a perfect vacuum in both cases, there would have been no difference in the times.

In the next experiment, my object was principally to show the effect of a current generated in the air by the motion of a pendulum. The convertible pendulum was adjusted symmetrically, by fixing brass weights at the one end, and the wooden patterns of these weights at the other, so as to make it vibrate with the clock pendulum when either end was lowest. When the heavy end was lowest the long vibrations were slower and the short vibrations quicker, as usual; but when the light end with the wooden patterns was lowest, there was no difference in the times of the vibrations, whether long or short. The explanation of this fact is, that the retardation arising from the current as the vibrations decreased, was equivalent to the acceleration resulting from the short vibrations; that is, that the current was stronger in proportion to the smaller vibrations, and likewise to the diminution of the momentum of the moving mass.

The difference in time between the long vibrations in air and *in vacuo* is less than it is between the short vibrations in air and *in vacuo.* The reason of this is, that as the velocity increases, the current is not so effectually formed in the long as in the short vibrations; besides, the air in front of the pendulum may be supposed partially condensed; and that behind it partially rarefied;—causes which both operate in producing a quicker return of the pendulum. Indeed I am of opinion, that if the vibrations were extended to 4° or 5°, they would be faster in air than *in vacuo,* at the same extent of vibration.

In the next experiment, the knife-edges were removed and replaced by portions of a cylinder, of which the diameter was 1·8 inch, in such a manner that the distance between them was the same as that between the knife-edges. The pendulum, which weighed 9 pounds 10 ounces, was adjusted so as to vibrate in the same time, when either the light or the heavy end was lowest. In this state, the clock gained upon the pendulum one vibration in 15 minutes, at the mean extent of 1°·3 vibration. The weight of the pendulum was then reduced to 4 pounds 8 ounces, and adjusted as before; the clock now gained one vibration in 13 minutes (being a mean of several experiments,) at an extent of 1°·3 vibration, either with the light or the heavy end lowest. These results give a difference, in the first experiment, of 1 minute 36 seconds in 24 hours, and in the second, of 1 minute 50 seconds, between the times of the vibrations on the knife-edge and the cylinder, those on the latter being slowest. The vibrations of the pendulum in the light state were also slower than in the heavy state; this result was occasioned by two circumstances; the current had more effect on the pendulum, because in the former state it presented a greater surface to the opposing medium, in proportion to its weight, than in the latter state; and because, in order to produce the adjustment, it was necessary that the weights should be nearer the end of the rod, when vibrating on the cylinders, than when vibrating on the knife-edges.

By experiments with cylinders of different diameters, I found that in order to make a simple pendulum, which vibrated seconds on a knife-edge, vibrate seconds likewise on a cylinder, the rod must be lengthened by a quantity equal to half the diameter of the cylinder; provided that the cylinder, or a small portion of it, be fixed in such a manner, that its surface shall be at the exact point of the rod previously occupied by the knife-edge.

In my experiments with the convertible pendulum, no part of the rod extended beyond the point of suspension at the light end, with the exception of a brass nut, which did not extend beyond the centre of the cylinder. Conceiving the cylinder to be a separate part of the pendulum, I equalized the matter on each side of the centre or axis, by making it equally heavy on balancing it with additional weight on both sides, in the direction of its length, and then adjusted the pendulum anew. I expected that this adjustment would cause the vibrations to be performed in the same time as when it was adjusted with the knife-edge.

The pendulum, however, now lost one vibration in 8 minutes, a quantity of loss on its rate nearly twice the amount of its former loss. From this result, I inferred that if all the weight or matter beyond the surface on which the cylinder rolls were removed, it would have the effect of producing vibrations synchronous with those performed by a pendulum vibrating on a knife-edge; and that the effect of the cylindric surface in quickening the vibrations of the pendulum is the same as that of shortening the rod of the pendulum by a quantity equal to half the diameter of the cylinder.

The conclusions to be drawn from these experiments are, that when a convertible adjusted pendulum is made to roll or vibrate on a cylinder instead of a knife-edge, the surface on which it rolls is the point of suspension, and the centre of the cylinder that is lowest is the point of oscillation, and that they reciprocally take the places of each other when inverted.

When the detached pendulum vibrated on a cylinder, the long vibrations were slower than when it vibrated on a knife-edge; I suppose the principal cause of this to be, the lengthening of the pendulum in proportion as the vibration extends. For the portions of the cylinder on each side of the vertical passing through the point of rest, become more distant from the point of oscillation in proportion as the vibration extends, and consequently the times of the longer vibrations are proportionably slower.

There is a vertical motion, as well as a small horizontal motion, both of which increase in proportion as cylinders of a larger diameter are employed;—but why cylinders of 1·8 inch, with a seconds pendulum, produce more isochronous vibrations than cylinders whose diameters are greater or less than that size, I leave to those to determine who are better acquainted with the mathematical theory.

The following experiments were made with a detached pendulum, both *in vacuo* and in air; a simple rod, with a knife-edge, being used; the clock pendulum was always adjusted to the detached pendulum for the different extents of vibration, and the rate of the clock was taken from another. In conducting the experiments in air, the pendulum was not removed, the air being merely readmitted into the vessel.

Set, No. 1.—With a brass bob, 5 inches long and 4 inches broad, of such a shape that a vertical section is rectangular, and a horizontal section oval.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	$+3^m\ 35{\cdot}7^s$	$1{\cdot}0^\circ$
In exhaustion, 1 inch	+3 44·0	1·0
In air	+3 34·0	1·5
In exhaustion, 1 inch	+3 40·8	1·5
In air	+3 30·5	2·0
In exhaustion, 1 inch	+3 37·5	2·0
In air	+3 26·3	2·5
In exhaustion, 1 inch	+3 33·0	2·5

Set, No. 2.—With a cylindrical brass bob of 5 pounds weight.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	$+3^m\ 11{\cdot}5^s$	$1{\cdot}3^\circ$
In exhaustion, 1 inch	+3 24·7	1·3
In air	+3 6·0	2·0
In exhaustion, 1 inch	+3 17·8	2·0

Set, No. 3.—With an oval leaden bob of $10\frac{1}{2}$ pounds weight.

Experiments.	Loss in 24 Hours.	Extent of Vibration.
In air	$-1^m\ 55{\cdot}6^s$	2°
In exhaustion, 1 inch	−1 49·5	2

There being but a small difference between the results of the experiments in air and in exhaustion, with the brass bob of 5 pounds, and the results of those with the leaden bob of 10·5 pounds, I was led to suspect that the surface of the lead might have carried more air along with it than that of the brass, and thus have caused a greater current. I therefore varnished the leaden bob, and the results of experiments with it were as follows:—

Set, No. 4.

Experiments.	Loss in 24 Hours.	Extent of Vibration.
In air	$-2^m\ 1^s$	2°
In exhaustion, 1 inch	−1 55	2

These results show that the varnishing of the bob made no difference in the time of the vibrations. The apparent difference between the results in air and in exhaustion arose from the bob not being set precisely in its former place after varnishing.

The following experiments were made in the open air, with the view of ascertaining the differences in time in the different arcs of vibration.

Set, No. 5.

Experiments.	Loss in 24 Hours.	Extent of Vibration.
In air	$-2^m\ 7{\cdot}6^s$	2·0°
Ditto	−2 12·3	2·5
Ditto	−2 18·2	3·0
Ditto	−2 24·2	3·5

Between the first experiments of Set, No 4. and Set No. 5. there is a difference of 6·6 seconds, although the extent of vibration was the same. I found afterwards that this was occasioned by the want of firmness in the fixture on which the pendulum vibrated, owing to its removal from the exhausted vessel. For although the fixture appeared to be firm, I considered it advisable to increase its stability by an additional fixture; this produced an acceleration in its rate of about 6 seconds in 24 hours. Thus it appears that a small change in the stability of a fixture will often produce a great effect on the rate of a pendulum. Owing to the slight instability of the fixture, in the above experiments, the difference in the times of vibration in the arcs of 2° and 3°·5 may be two or three seconds greater than it ought otherwise to be.

The next set of experiments were made for the purpose of ascertaining the effect of resistance or friction on the rate of a pendulum, in the case where no current was generated by the resistance.

The mercurial pendulum, having a suspending spring so adjusted as to cause the long and short vibrations to be nearly isochronous, was employed. A hair was stretched parallel to the motion of the pendulum so as touch the rod slightly, about 2 inches below the point of suspension.

Set, *No.* 6.—With a maintaining power of 5 pounds weight.

Experiments.	Loss in 24 Hours.	Extent of Vibration.
With hair............	$-2^m\ 7{\cdot}5^s$	1·8°
Ditto	$-2\ \ 7{\cdot}2$	
Without hair.........	$-2\ \ 6{\cdot}0$	2·0

The loss of 1 second in the rate of the pendulum arising from the increase of friction in consequence of its slight contact with the hair, may be explained as follows: The impulse given to the pendulum was all during its ascent; this caused all the friction that would affect the time to be in its descent. Since the friction in its ascent would fall into the impulse, it would occasion a diminution of its power only, but have no effect on the time of the ascent; it would, however, have its full effect on that of the descent, and produce the loss of 1 second, as shown by experiment.

[To be continued.]

LVII. *On the Mathematical Laws of Electrical Influence. By* R. Murphy, *Esq. M.A. Fellow of Caius College, Cambridge**.

FEW exact results, in the mathematical sense of the word, have been obtained with respect to the distribution of electricity when any number of electrized bodies mutually influence each other. M. Poisson has established a very simple principle for reducing such problems to analysis, and another may be announced by assuming the influences to be consecutive: thus if two spheres A and B mutually influence each other, we may suppose, first, that A alone influences B; secondly, that the disturbance thus produced in B influences A, and so on: by this means we may arrive at the final distribution of electricity on the surfaces of A and B.—The following *exact* results of influencing bodies may be noticed.

If any number of concentric shells, the thickness of any one of which is uniform, are charged with electricity, the quantity of electricity developed on the *outer* surface of any shell will be the sum of the charges of all the interior shells, including the individual shell itself; and on its inner surface, the same sum excluding that shell.

If a very remote body influence an electrized sphere, the section made by a plane through its centre perpendicular to the direction of the disturbing force, will contain those points on the surface which are not influenced, and the influence at

* Communicated by the Author.

any other point is as the sine of the angle which the radius drawn to that point makes with the above plane.

If an electrized point influence an electrized sphere, the influence at any point is as a constant quantity *minus* another quantity which varies inversely as the cube of the distance from the influencing point.

April 10, 1833. R. MURPHY.

LVIII. *On the Law of the Diffusion of Gases.* By THOMAS GRAHAM, *Esq. M.A. F.R.S. Ed., Professor of Chemistry in the Andersonian University, Glasgow.*

[Concluded from p. 276.]

13. *Carbonic Oxide.*

SPECIFIC gravity, 0·9722, &c. as in the case of nitrogen. Gas prepared by the action of sulphuric acid on crystallized oxalic acid, well washed with caustic ley.

On 803 measures carbonic oxide and vapour, a contraction of 11 measures in fifty hours, 12 measures in eighty-nine hours, 12 measures in ninety-seven hours; or 803 became 791. The diffusion was slower than usual, from the plug having been partially wetted in filling the instrument with gas.

$\frac{815}{803} = 1·0149 =$ diffusion-volume of carbonic oxide, by experiment.

$1·0140 =$ diffusion-volume of carbonic oxide, by theory.

In the case of the last three gases, when the experiment was performed over water in a diffusion-tube, with free exposure to the dry atmosphere, instead of any contraction ensuing, a positive expansion generally occurred, which was to be attributed to the return air, which was comparatively dry, being expanded after entering the receiver.

14. *Carburetted Hydrogen of Marshes.*— Specific gravity, 0·555. Diffusion-volume, 1·3414.

In an experiment with this gas, deducting a small quantity of air which it contained, 252 measures were replaced by 187 air.

$\frac{252}{187} = 1·344 =$ diffusion-volume, by experiment.

$1·341 =$ diffusion-volume, by theory.

These are all the permanent gases which could conveniently be submitted to diffusion. Vapours cannot be rigidly examined, as they are all condensible in the pores of the stucco. The following Table exhibits a summary of the results:

Table of Equivalent Diffusion-volumes of Gases; Air = 1.

	By Experiment.	By Theory.	Spec. Gravity.
Hydrogen................	3·83	3·7947	0·694
Carburetted Hydrogen	1·344	1·3414	0·555
Olefiant Gas.............	1·0191	1·0140	0·972
Carbonic Oxide.........	1·0149	1·0140	0·972
Nitrogen..................	1·0143	1·0140	0·972
Oxygen....................	0·9487	0·9487	0·111
Sulphuretted Hydrogen	0·95	0·9204	1·1805
Protoxide of Nitrogen	0·82	0·8091	1·527
Carbonic Acid...........	0·812	0·8091	1·527
Sulphurous Acid........	0·68	0·6708	2·222

In the diffusion-volumes of oxygen, nitrogen, and carbonic oxide, the correspondence between theory and experiment is as close as could be desired. Indeed, admitting our law, I believe that the specific gravity of these gases can be determined by experiments on the principle of diffusion, with greater accuracy than by the ordinary means. But, to be of value, experiments performed with this important object in view, would require to be conducted with extreme care, in the most favourable circumstances, as regards uniformity of temperature, and to be frequently repeated. The diffusion-bulbs might also be considerably increased in size, and a greater minuteness of observation attained. Even in the most successful experiments recited in this paper, we cannot depend upon the absolute accuracy of the third decimal figure. In the case of carbonic acid gas, protoxide of nitrogen, sulphuretted hydrogen, and sulphurous acid, the process of diffusion is interfered with in a greater or lesser degree by the absorbent action which all porous bodies exercise upon gases. Fortunately, however, the absorbent power of stucco is very low in degree.

The density of any gas diffused into air, both being in the same state as to aqueous vapour, is obtained by the formula

$$D = \left(\frac{A}{G}\right)^2;$$

where G is the volume of gas submitted to diffusion, and A the volume of return-air. In operating upon gases lighter than air, the most useful instrument is a bulb of about two inches in diameter blown upon half-inch tube, of which about an inch may be left on either side of the bulb. The capacity of the instrument, *used as a gas-receiver over water*, is most simply determined by filling it with water, and weighing the water which it contains, and which can be poured from it into a counterpoised phial. Then, after any experiment, the return-air may be found from the weight of the water which has entered the instrument, determined in the same manner. By proceeding in this way, we avoid wetting the stucco after every experiment. A hood of damp paper may be inverted over the upper tube while the diffusion is going on, and the whole counterpoised in a tumbler of water, being suspended from one of the arms of the beam of a balance, the scale on that side being removed. An experiment with the bulb will generally occupy several hours. But with a plain diffusion-tube, a much shorter time will suffice.

A peculiar advantage of this mode of taking the specific gravity of gases, besides its simplicity, is, that we can operate upon a most minute quantity of gas: it is possible to come within 100dth of the specific gravity, operating upon no more than one cubic inch of gas.

It is to be regretted that this method is not so fully available in the case of coal-gas, as might be expected. The density of that gaseous mixture appears to depend, in no inconsiderable measure, upon the presence of a small quantity of the heavier hydro-carburets, such as naphtha-vapour; and these are apt to be absorbed and withdrawn in part by the water, during the continuance of a diffusion experiment. I have observed coal-gas to contract $\frac{1}{10}$th of its bulk by standing over water, without agitation, for forty-eight hours, and from the loss of the denser portion of it. But in the case of this gas, the experiment should succeed over brine, which absorbs much less of the gas than water does.

The process of diffusion may be managed so as to demonstrate relations in density. The short upper tubes of two diffusion-bulbs, not closed by plaster, but open, were connected by means of thick caoutchouc adopters, with the two ends of a short piece of straight tube, in which there was a diaphragm of plaster, $\frac{1}{5}$th of an inch in thickness, and equidistant from either end of the tube. The apparatus being proved air-tight, and the plug in a proper condition for diffusion, one of the diffusion-bulbs was filled with nitrogen gas, and the other with carbonic oxide, and the bulbs placed upright in separate contiguous glasses containing water. The quantity of gas in each was carefully observed at the beginning of the experiment, and after the expiry of twenty-four hours, when it was found to be identically the same as at first; at least, if a contraction or expansion took place, it was the same in both bulbs, and therefore entirely due to changes in temperature or pressure.

Now, the gases were found by analysis to be uniformly diffused through both bulbs; so that nitrogen and carbonic oxide are of the same density, or at least do not differ more than $\frac{1}{500}$dth part, which was the limit of observation in the case of these experiments. It appears, also, that inequality of density is not an essential requisite in diffusion.

I had occasion to remark, more than once, a singular accident to the stucco plugs. After being disused for some days or weeks, and left in the interval exposed to the air, which might be either dry or damp at the time, the plugs occasionally, on a new trial, did not permit diffusion to take place through their pores, at least immediately. Hydrogen, however, always opened a passage in the course of two or three minutes, and then the diffusion proceeded as rapidly as ever. Carburetted hydrogen, and the other gases, often required a longer period. A slight heat restored the action of the plug. The obstruction could not be attributed to moisture, nor to any thing but dust.

It may be mentioned, that there was nothing peculiar in a mixture of two gases, in the proportion of the numbers expressing their diffusion-volumes;—nothing that could be considered an indication of mutual saturation.

Evaporation, or the elevation of vapour from a liquid into air, or any other gas, comes now to be explained on the principles of diffusion. The powerful disposition of the particles of different gaseous bodies to exchange positions, may as effectually induce the first separation of vapour from the surface of the liquid, as a vacuum would do. Once elevated, the vapour will be propagated to any distance, by exchanging positions with a train of particles of air, according to the law of diffusion. The length to which this diffusion proceeds, in a confined portion of air, is limited by a property of vapour, namely, that the particles of any vapour condense when they approximate within a certain distance. Hence, the quantity of vapour which rises into air, has the same limit as that which rises into a vacuum, and is the same.

I may be allowed to mention an application of the law of diffusion, in explanation of the mechanism of respiration. The cavity into which air enters during respiration, consists, first, of a large tube, the windpipe; secondly, of smaller tubes, into which the windpipe diverges; and, thirdly, of a series of still smaller tubes, diverging from the last, themselves ramifying to an indeterminate extent, till at last the tubes cease to be of sensible magnitude, but are believed to terminate in shut sacs. The capacity of the whole cavity cannot easily be determined, but we may estimate it at 300 cubic inches. In a natural expiration, about 20 cubic inches, or $\frac{1}{13}$th of the contents are thrown out, from the application of a general pressure to the whole. But it is evident, that these twenty cubic inches will be the twenty cubic inches nearest the outlet, or the contents of the larger tubes. The contents of the second-sized tubes will advance at the same time into the largest tubes, but no further, and will recede again into their original depositories on the next inspiration, which will fill the larger tubes with fresh air; which identical quantity will again be expelled in the next expiration. This illustration is perhaps too strongly stated; but it is evident that, in ordinary respiration, the slight mechanical compression will have little or no effect in emptying the most distant tubes, or the ultimate air-cells, of their contents. The bulk of the air, also, is not altered during respiration, although, for a quantity of oxygen, carbonic acid gas is substituted. This substitution, which is the great end of respiration, undoubtedly takes place most abundantly in the minute and distant air-cells, which present the largest surface to the blood; and the carbonic acid there produced, must be moved along the smaller tubes by the diffusion process, (which we know to be extremely energetic, and also inevitable,) till it is thrown into the larger tubes, from which it can be expelled by the ordinary action of respiration. But the action of diffusion is always twofold: at the same time that carbonic acid is being carried outward from the air-cells, oxygen is carried inward in exchange, and thus the necessary circulation kept up throughout the whole lungs.

Further, by a forced expiration, from 160 to 178 cubic inches may be expelled, after which, there still remain in the lungs about 120 cubic inches, which are not under the control of the respiratory action.

There can be no doubt that much of this quantity occupies constantly and permanently the most minute tubes and air-cells, for it can scarcely be withdrawn by means of the air-pump. Now, the question has arisen, how these ultimate tubes and air-cells are so powerfully inflated; for they are not distended by the action of muscular fibre, of which they are known to be destitute. This state of distention must be highly useful, by exposing surface; and the law of diffusion enables us to account for it. The heavy carbonic acid which these minute cells may contain, is not merely exchanged for oxygen, but for a larger volume of oxygen, in the proportion of the diffusion-volumes of carbonic acid and oxygen; namely, 81 carbonic acid are replaced by 95 oxygen. The resistance to passage through the most minute tubes, is overcome by the diffusion action, as in the case of the pores of the stucco-plug, and there follows a tendency to accumulation on the side originally occupied by the carbonic acid. This accumulation is limited by the increased facility with which the air-vessels can empty themselves mechanically of a portion of their contents, from their distended state.

In the law of diffusion of gases, we have, therefore, a singular provision for the full and permanent inflation of the ultimate air-cells of the lungs.

But it is in the respiration of insects that the operation of this law will be most distinctly perceived. The minute air-tubes accompanying the blood-vessels to every organ, and like them ramifying till they cease to be visible under the most powerful microscope, are kept distended during the most lively movements of the little animals, and the necessary gaseous circulation maintained, wholly, we may presume, by the agency of diffusion.

In regard to the terms of the law of diffusion: "The diffusion, or spontaneous intermixture of two gases in contact, is effected by an interchange in position of *indefinitely minute volumes of the gases*." My experiments, published on a former occasion, on the diffusion of mixed gases (Quarterly Journal of Science, Sept. 1829), afford the first demonstration of the fact, that diffusion takes place between the ultimate particles of gases, and not between sensible masses, and therefore that diffusion cannot be the result of accident. For, in the case of a mixture of two gases escaping from a receiver into the atmosphere, by apertures of 0·12 and 0·07 inch in diameter, it was not so much of the mixture which left the receiver in a given time, but a certain proportion of each of the mixed gases, independently of the other, corresponding to its individual diffusiveness. The same separation of mixed gases occurred in diffusion through the pores of stucco, or the fissure of a cracked jar.

"Which volumes are not necessarily of equal magnitude, being, in the case of each gas, inversely proportional to the square root of the density of that gas." This may be demonstrated, when different gases communicate by very narrow channels, or by very small apertures, and when inequality of pressure is guarded against. In the case of a gas communicating with the air by a wide aperture, on the other hand, although the diffusion or intermixture takes place precisely in the same way, still the result is different; for where a contraction takes place from the process of diffusion, the air flows in mechanically through the aperture, wholly unresisted, and makes up the deficiency. A gas, however, of large diffusion-volume escapes, in these circumstances, *in a shorter time* than a gas of small diffusion-volume. Indeed, it was the conclusion of the former paper, that gases diffuse more or less rapidly according to some function of their densities, "apparently inversely as the square root of their densities." The advantage, in illustrating the process of diffusion, of minute apertures or channels of communication, such as we have in the stucco-plug, depends upon the circumstance, that when a contraction or expansion takes place in the gaseous contents of a diffusion-instrument, any current in an outward or inward direction is prevented by frictional resistance; so that the simple result of diffusion is exhibited, not complicated by the effect of any other force.

The law at which we have arrived (which is merely a description of the appearances, and involves, I believe, nothing hypothetic), is certainly not provided for in the corpuscular philosophy of the day, and is altogether so extraordinary, that I may be excused for not speculating further upon its cause, till its various bearings, and certain collateral subjects, be fully investigated.

Supplementary Observations on the Law of the Diffusion of Gases.

It is curious that intermixture takes place more rapidly in the case of some gases than in that of others, although still in conformity with the law of diffusion. Thus the process goes on with much greater activity in the case of hydrogen, olefiant gas and coal gas diffusing into air, than in the case of chlorine, carbonic acid, carbonic oxide, &c., diffusing into the same medium. This is very observable on comparing the times as stated in describing the experiments on each gas.

The circumstance of the apertures being in the upper part of the diffusion-instruments, and opening upwards, may be supposed to give the light gases an advantage in diffusing; but I am disposed to attribute little of the inequality in question to this cause. From a diffusion-bulb, in which the upper tube was curved and bent downwards, hydrogen gas was found to escape with its wonted rapidity.

This inequality in the velocity of diffusion is strikingly illustrated in the following results, obtained from experiments with different gases, submitted in turn to diffusion from the same instrument. In a certain time, the same in all the experiments, a quantity of air entered, by diffusion, which varied with the gas diffusing.

In a given time,

With chlorine in the diffusion-tube	0·302	vol. air entered.
With carbonic acid	0·623	——
With hydrogen	1·277	——

It appears, then, that the process of diffusion into air through stucco is four times more rapid in the case of hydrogen than in that of chlorine, and twice as rapid in the case of the former gas as in carbonic acid. The process of diffusion might be said to proceed at a uniform rate, if the same quantity of air entered the instrument in the same time, whatever gas was diffused, and although the quantity of gas which escaped was variable of course, and proportional to the respective diffusion-volume of the gas. But this exchange of diffusion-volumes takes place more rapidly, it appears, in the case of some gases than of others.

A table of experiments is given in the body of the paper (p. 187) on the rate of passage of different gases through the pores of stucco under the influence of pressure. The rate appears to be the same in the case of air, nitrogen, oxygen, and carbonic acid, from which carbonic oxide deviates in a small degree. But hydrogen, and, it is remarkable, olefiant gas and coal gas, which contain hydrogen, are less resisted than the preceding class. Upon reconsideration I am inclined to connect with this fact the apparent deviation of hydrogen from the law of diffusion, which is noticed in the paper. It is there shown that more hydrogen passes out than the exact quantity proportional to the return-air. The same deviation from the law may be remarked in the experiments detailed on olefiant gas. It is also very noticeable in the case of coal gas. But these are gases which, like hydrogen, are less resisted than common air in their passage through stucco. There appears to exist an inaptitude on the part of a stucco intermedium to exhibit the exact effect of diffusion, in the case of gases, on either side of it, which are not capable of permeating through it with equal facility; that gas which experiences least frictional resistance diffusing through in a quantity somewhat greater than it should do.

There can be no doubt that the velocity of diffusion noticed above, is likewise influenced by the variable resistance which the gases experience in passing through the stucco. But I am not prepared to say, that the variation depends entirely on this cause, and is therefore accidental to the mode in which the diffusion takes place. The diffusion or intermixture of *light* gases appears to take place in all circumstances with greater rapidity than that of heavy gases.

Glasgow, Sept. 7, 1832. THOMAS GRAHAM.

LIX. *Remarks on Chemical Changes of Colour.* *By* H. F. Talbot, *Esq. M.P. F.R.S.**

VERY little is known with certainty concerning the cause of those striking changes of colour which we so frequently witness in chemical experiments. No theory has yet been proposed which will by any means account for the whole of them, and we are often at a loss even for a plausible explanation. And yet these phænomena have probably some very close connexion with the ultimate constitution of bodies, and are therefore well worthy of our particular attention. I will therefore mention a few facts relating to this subject, hoping to engage others to pursue such inquiries further.

Water, being a colourless substance, ought, one would imagine, when mixed with other substances possessing no decided colour, to produce a colourless compound. Nevertheless it is to water only that the common vitriol or sulphate of copper owes its extremely vivid blueness; as is plainly evinced by a simple experiment. For if we calcine the vitriol at a low red heat, and pulverize it, we shall obtain a powder of a dull and dirty white appearance. Now pour a little water upon this, and a slight hissing noise is heard, accompanied by an evolution of heat, very similar to what happens in the slaking of quick-lime. At the same moment *the blue colour suddenly reappears.* Mr. Faraday, to whom I showed this experiment, informed me it was new to him. I therefore presume it is a fact little, if at all known, and may interest your chemical readers.

Under the microscope this is a very pretty experiment, for the instant a drop of water is placed in contact with the vitriol, the amorphous powder is seen to shoot into blue prisms.

Are we then to infer that water has a tendency to communicate a blue rather than any other colour, to bodies in general? By no means; for in other instances its operation is exactly the reverse, and it is a *destroyer* instead of a *promoter* of blueness. For instance: sulphate of molybdenum is a liquid of a very rich dark blue, when sufficiently concentrated: but a very small portion of water suffices entirely to annihilate this blue tint, and to produce a mixture which is perfectly colourless. Thus the action of water upon the two metals copper and molybdenum is of an entirely opposite character. Instead of water if ammonia be used, the same contrast is seen still more strikingly. Another remarkable and well-known instance analogous to this is the muriate of cobalt, which is

* Communicated by the Author.

entirely deprived of its fine blue colour by a very slight admixture of water.

Muriate of copper is described in most books of chemistry as a liquid of a bright green colour. But how imperfect an account of it this is, will be seen from the following experiment:—If sulphate of copper and muriate of lime (dry, or only slightly damp) be pulverized together in a mortar, muriate of copper is formed, *of a dark yellow colour*, or more frequently *of a yellowish brown.* If a few drops of water are now added, the yellow speedily changes to a bright green. If more water is added, the mixture becomes greenish blue, sky-blue, and finally colourless. If the water is evaporated by heat, the same colours reappear, in the reverse order*.

In Turner's Elements of Chemistry, it is said that nitric acid when containing a small portion of the orange nitrous gas, acquires a green tint; upon which he takes occasion to make the following remark (p. 193.):—

"It is difficult to perceive how an orange-coloured liquid should give different shades of green and blue merely by being diluted."

Now the above-mentioned property of muriate of copper seems to furnish an instance that is very analogous.

The mere application of heat often produces great change of colour, which disappears again when the substance becomes cold. Red lead, vermilion, and white oxide of zinc are instances well known, and never accounted for. Another curious example is furnished by the sulphate of molybdenum, which when warmed changes its fine blue tint to a pale yellow, again reverting to the blue when cold.

LX. *Observations on the Absorption of Specific Rays, in reference to the Undulatory Theory of Light.* By Sir David Brewster, *LL.D. F.R.S. &c.*

As Mr. Potter has referred in the last Number of this Journal to some opinions of mine respecting the absorption of light, I am anxious to state the views which I have taken of this class of phænomena, in reference to the undulatory theory. I have long been an admirer of the singular power of this theory to explain some of the most perplexing phænomena of

* The *yellow* state of the muriate of copper is best exhibited by warming a sheet of paper on which letters have been written with it. The writing disappears again on cooling, because it absorbs the atmospheric moisture. The use of this salt as a sympathetic ink has been already mentioned by chemical writers.

optics; and the recent beautiful discoveries of Professor Airy, Mr. Hamilton, and Mr. Lloyd afford the finest examples of its influence in predicting new phænomena. The power of a theory, however, to explain and predict facts, is by no means a test of its truth; and in support of this observation we have only to appeal to the Newtonian Theory of Fits, and to Biot's beautiful and profound Theory of the Oscillation of Luminous Molecules. Twenty theories, indeed, may all enjoy the merit of accounting for a certain class of facts, provided they have all contrived to interweave some common principle to which these facts are actually related.

On these grounds I have not yet ventured to kneel at the new shrine, and I must even acknowledge myself subject to that national weakness which urges me to venerate, and even to support, the falling temple in which Newton once worshiped.

That the undulatory theory is defective as a *physical* representation of the phænomena of light, has been admitted by the more candid of its supporters; and this defect, in so far as it relates to the dispersive power of bodies, has been stated by Sir John Herschel as a "*most formidable objection* *." That there are other objections to it, as a physical theory, I shall now proceed to show; and I shall leave it to the candour of the reader to determine whether they are more or less formidable than that which has been stated.

According to the Undulatory Theory, light consists in the undulations of an exceedingly rare and elastic medium called Æther, which pervades all space, and which exists in the interior of all refractive media, but with a diminished elasticity, the æther being least elastic in the most refractive substances. As, in sound, the pitch or note is determined by the frequency of the aërial pulses; so in light the colour is determined by the frequency of the *ethereal* pulses. Generally speaking, indeed, light differs from sound, according to this theory, only in the undulations being performed in media of very different elasticities.

If we transmit white light through the thinnest film, that can be detached, of transparent *native orpiment*, the light will be a bright greenishyellow; and if we analyse this light by the prism, we shall find that it contains none of the *violet* rays. Hence it follows,—and we find it so by direct experiment,—that this thin transparent film is *absolutely opake* to violet light, refusing to transmit a single ray of it through its substance. Now this film contains æther which is freely put into undulation by *red*,

* Treatise on Light, § 565.

yellow, and *green* light, and yet it is absolutely immoveable when acted upon by the undulations of violet light, which differ from the others only in their inferior length.

There are some substances in which the æther will undulate only to violet light; and there are others in which the æther will undulate only to *green* light, the body which contains it being absolutely opake to all red and violet rays.

That very remarkable salt the *oxalate of chromium and potash* (for fine specimens of which I have been indebted to Dr. William Gregory,) exercises a still more definite action upon light. While a certain thickness of it is absolutely opake to every ray except the red ones, it is also opake to a definite ray in the very middle of the red space! That is, it is absolutely transparent, or its æther freely undulates, to a *red* ray whose index of refraction, in flint-glass, is 1·6272, and also to another *red* ray whose index is 1·6274; while it is absolutely opake, or its æther will not undulate at all, to a *red* ray of *intermediate* refrangibility whose index is 1·6273!

When we consider that *green light* passes copiously through such a dense substance as a thin film of gold*, and that metallic salts of great density afford as free a passage to light as water or even atmospheric air, we cannot ascribe the preceding phænomena to any mechanical obstruction which the solid particles of bodies oppose to the free motion of the æther which they contain. But even if we could, by some new assumptions, avail ourselves of this principle in the case of dense bodies, it will not be applicable to those strange phænomena of definite action which I have discovered in the absorptive power of nitrous acid gas.

When we transmit light through a very small thickness of this gas, there are no fewer than *two thousand* different portions of the incident beam, which are absolutely stopped by the gas, while other two thousand portions are freely transmitted; and what is equally strange, the same body in the liquid state exercises no such power, but freely transmits all those *two thousand* portions which the gas stops. The æther in

* Mr. Potter has remarked "that he cannot, with many opticians, call the translucency of thin metallic leaves transparency" (present volume, p. 278). If he means that the light which such leaves transmit does not pass through the substance of the metal, but through small openings or pores produced by hammering, I beg leave to refer him to an experiment in the Phil. Trans. for 1830, p. 136, which, though it was not sufficient to give me a correct measure of the action of gold in changing the plane of polarization, was perfectly sufficient to show that the green light had its plane of polarization changed, while that which passed through the pores suffered no change: the metallic leaf had therefore the same kind of transparency as all other bodies, varying of course with the colour of the incident light.

the liquid undulates readily to all their rays, while the æther in the gas, in which we should expect it to exist in a much freer state, has not the power of transmitting the undulations of *two thousand portions* of white light!

Among the various phænomena of sound no such analogous fact exists, and we can scarcely conceive an elastic medium so singularly constituted as to exhibit such extraordinary effects. We might readily understand how a medium could transmit sounds of a high pitch, and refuse to transmit sounds of a low pitch; but it is incomprehensible how any medium could transmit two sounds of nearly adjacent pitches, and yet obstruct a sound of an intermediate pitch.

Such are the grounds upon which I stated to Mr. Potter that the absorption of light militated strongly against the undulatory theory.

Allerly, April 13, 1833.

LXI. *On a Modification of the Electrophorus of* Volta. *By* John Phillips, *F.G.S., Assistant Secretary to the British Association**.

HAVING for three years found considerable advantage in electrical experiments from the use of an electrophorus, which in one respect is of peculiar, and I believe, new construction, I am induced to offer a short description of it.—The ordinary electrophorus exhibits its action in consequence of a communication being established between the insulated cover, while it is applied to the excited surface, and bodies conducting to the earth. Usually this communication is made by the finger of the operator; and when there is occasion to accumulate the electricity developed by the instrument, or to procure its sparks in rapid succession, the trouble and tediousness of the operation is so considerable as to induce many persons to have recourse to a machine, for purposes to which the electrophorus is perfectly adequate.

Considering that the touch of the finger was of no other service than to establish the necessary communication between the cover and the earth, and that the same effect would result from permitting, under the same circumstances, a momentary connexion between the cover and the metallic basis of the resinous plate, I have adopted three methods of doing this. The *first* consists in raising from the metallic basis, above the edge

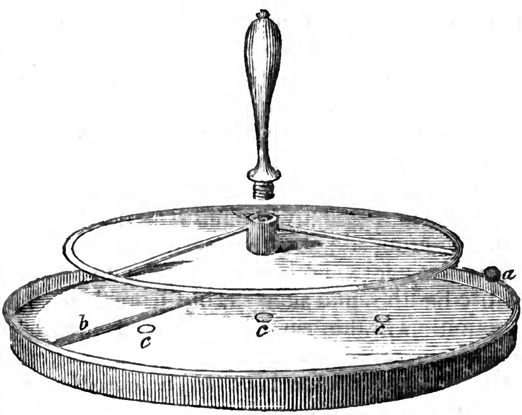
* From two papers on the subject of the Electrophorus, read to the Yorkshire Philosophical Society in 1830 and 1833.—Communicated by the Author.

of the resin, a brass wire and ball, to which the edge of the cover, or a brass ball upon it, may be applied: and this method succeeds extremely well, especially with small covers which can with ease and certainty be directed to any particular point of the sole. The *second* mode is to fix a narrow strip of tinfoil quite across the surface of the resinous plate, and unite it at each end with the metallic basis. This construction answers perfectly and instantaneously, and is especially convenient with large circles, the covers of which, though uneven, will thus be sure to touch some conducting point. The *third* method of construction is to perforate the resinous plate quite through to the metallic basis, at the centre, and at any other points which may be thought proper, and at all those points to insert brass wires, with their smoothed tops level with the resin. If the surfaces of contact were perfectly plane, a central wire would be sufficient, but this is seldom the case.

To those who have not *studied* the electrophorus it may appear extraordinary that the wires or the tinfoil, at the surface of the resin, do not when the cover is uplifted reduce its electricity to the natural state of equilibrium. They may be reminded that while the cover touches the excited electric, and at the same time a body conducting to the earth, it is put into a state of *forced* equilibrium with the electric, by the induction of that body; and that on being separated from it, and at the same time from bodies conducting to the earth, the conditions of this equilibrium are more and more impaired the further off the cover is removed. Therefore, at very small distances from the electric the cover has no sensible tendency to communicate with any conducting body (as may be proved by electroscopes), and at greater distances, when the conditions of equilibrium are proportionately diminished, the striking distance of the cover is not equal to the interval between the points supposed to communicate. On two of the largest electrophori which I have yet made, both the second and third methods have been tried with equal success, but I much prefer the latter construction. The largest instrument has a cast-iron basis 20·5 inches diameter, resinous surface 19·75 inches, cover 16·25 inches.—The resinous composition was made according to the directions in Mr. Faraday's work on Chemical Manipulation. The cover is made of a plate of thin copper, strengthened at the edge by a thick brass wire, from which three radial brass wires pass to the upper part of a central brass tube. In consequence of the angle they thus form with the plane of the plate, they act as pretty strong braces, to maintain its figure, and the whole is very light. This central brass tube receives a cylindrical piece of wood, into which the insulating glass handle, covered with sealing-wax, is screwed by its wooden foot.

With ordinary excitation, this instrument will yield loud flashing sparks, two inches long or more, and speedily charge considerable jars. The cover can be easily charged and discharged 50 or 100 times in a minute, by merely setting it down and lifting it up as fast as the operator chooses, or the hand can work. In charging a jar or plate, I place one knob of the connecting rods near the insulated surface of the jar or plate, and the other some inches above the cover; then the cover, being alternately lifted up and set down, the jar is very quickly charged.

One instrument, nine inches in diameter, which I have made upon the second plan above described, has very often surprised me by its remarkable power of retaining electrical excitation. The following example seems worthy of notice:—Early in September 1832, this instrument was moved from a house in York, where it had been for some time laid by, and brought to my present residence, distant $\frac{1}{3}$rd of a mile. It was placed on a shelf on my book-cases, where it remained untouched until the 23rd of March 1833, and was then taken down, *covered with dust.* It was found to be in a state of feeble excitement, so as to give sparks visible in the day-light nearly $\frac{1}{4}$th of an inch long.



Basis of the sole a cast-iron disk.
a. The place of a ball in the first method.
b. The slip of tinfoil in the second method.
c, c, c. Conducting wires, in the third method, which is preferred.

York, April 2, 1833.

LXII. *On the Theory of Magnetic Electricity. By* Mr. Wm. Sturgeon, *Member of the British Association for the Promotion of Science; Lecturer at the Hon. East India Company's Military Academy, Addiscombe, &c. &c.*

[Continued from p. 207.]

IT is probable, however, that other laws are in operation during this novel process of excitation, which are still more remotely situated from observation; and require for their development, experiments and a mode of reasoning of a very different order to those which have been employed for organizing the system of *proximate* laws already explained.

It appears to me that electric currents generated by magnetic agency are not the *immediate* effects of the magnet employed in the excitation. It is highly probable that there is a *mediate* or intervening agent called forth;—the magnetism natural to the excited metal, which, by being polarized by the exciting *polar magnetic lines* of the magnet, becomes the *immediate* agent in giving life and energy to the previously dormant electricity of the metal.

Remote and mysterious as the intermediate agency of the natural magnetism of the metal in this process of exciting electricity may appear in the present infantile stage of the science, I have much reason to suppose that such is the fact. The phænomena in magnetic-electricity, as well as those in electro-magnetism, are highly favourable to the hypothesis; and I am not aware of an exception that militates directly against it. Moreover, the facility with which the *modus operandi* might be explained upon the simple principles of *polar magnetic lines* alone, would, I am persuaded, establish a degree of plausibility at least, not easily shaken by any counter-reasoning likely to be advanced; and the illustrations which it would be possible to bring forward in support of such an hypothesis, might possibly be the means of fixing a basis on which the theory of excitation in this curious branch of physics is eventually and permanently to be established.

The same class of *remote* laws apply equally to electro-magnetism as to magnetic-electricity; and it would be very difficult, indeed, independently of those laws, to completely harmonize with each other the phænomena displayed by the two different modes of excitation.

With regard to electro-magnetic action, the idea can hardly be said to be novel. Mr. Buxton long ago asserted that the magnetism of the conducting wire becomes polarized, and is the intermediate agent between the transmitted electric current and the magnet employed; but the illustrations which have been advanced by that gentleman might possibly require considerable modification to establish a theory on those principles.

I have heard brought forward, as an argument against the hypothesis of magnetic polarity of the conducting wire, an experiment of Sir H. Davy's, which showed the deflection of an electric current passing through air between the charcoal points of a voltaic battery, by the presentation of a magnetic pole. Such arguments can have but very little force in discussions of this character; for the experiment develops nothing different to the generality of electro-magnetic phænomena. If an electric current be capable of rousing into activity the dormant magnetic powers of ferruginous matter, no doubt can possibly be entertained of its susceptibility of being put into motion by the energies of an already formidable polarized bar.

This is the extent of reasoning to which the experiment can be applied even under the supposition of the electric current being the immediate agent in the process of magnetizing iron or steel, and that no intervening polarization of the conducting wire is concerned in the operation; which, in fact, is no argument whatever, further than might be advanced from any other electro-magnetic experiment.

On the other hand, it might be inferred with a great deal of propriety, that if the electric current is capable of calling forth the latent magnetism of hard steel, in which it is pent up and retained with a degree of vigour which requires the greatest efforts of the exciting agent to extricate it and accomplish its polarity even to a comparatively small extent;—it is but reasonable to expect that in those metals which do not possess so exalted a degree of retention as hard steel, the *same* exciting agent would accomplish a polarity to a much greater extent.

This simple *induction* is beautifully illustrated and substantiated by demonstrable *facts*, by comparative experiments on soft iron and hard steel; and it was by the same mode of reasoning that I was first led to construct electro-magnets of soft iron*; since which time the practice has been pursued with more than anticipated success.

The facility of polarizing the magnetic matter, or of arranging it into *active polar lines* by any constant exciting force, appears to be inversely proportional to the retentive quality of the metal on which the process is performed.

* See Transactions of the Society of Arts, &c. vol. xliii.; Phil. Mag. and Annals, N.S., vol. xi. p. 194.

The *retention* of magnetic polarity is displayed to the greatest extent by very hard steel. After this the retentive faculty diminishes with various grades of hardness down to soft steel; thence by gradations downwards to the softest iron, which exhibits the faculty of retaining magnetic polarity, only in a very slight degree indeed. But the facility of magnetizing those bodies, and the extent to which their polarity is exhibited, are in precisely the reverse order.

Now, as the *retention* of polarity appears to result from a want of facility, on the part of the metal, to readmit the magnetic matter which the exciting agent has arranged into active *polar lines* on its surface and vicinal medium; and as those metals which display the retentive faculty in the greatest degree also offer the greatest resistance to the formation of those *polar lines*, or to the escape of the magnetic matter from its ferruginous prison;—this disposition evinced by the metal, of resisting both the egress and ingress of the magnetic matter, must necessarily arise from a natural tendency which it possesses to refuse the transmission of the magnetic element. Hence those metals which retain magnetic polarity in the highest degree may be called *inferior* magnetic conductors; and those which retain no traces of polarity after the exciting process has ceased to operate, may be called *superior* magnetic conductors, with as much propriety, and for the same reason, as similar terms are employed in electricity.

Under these considerations it will appear that *hard* steel is an exceedingly bad conductor of magnetism; because it offers a very great resistance to the motion of the magnetic matter. This resistance causes the process of magnetizing to become exceedingly tedious; and with very hard *cast steel* it very seldom terminates successfully, or to the satisfaction of the operator. Hence, in a practical point of view, it is interesting to know that magnets constructed of *cast steel* should never be harder than the *blue temper*.

Soft iron being the best ferruginous conductor of magnetism, offers a much less resistance to the flow of the magnetic matter than when in any other state. The vigorous *polar magnetic lines* are therefore speedily arranged, and to an extent of concentration never to be accomplished on the surface of very hard steel.

But the same conducting quality which gives to soft iron a facility of excitation, also gives a facility to the return of the magnetic matter into the metal when the exciting agent is withdrawn; for which reason the retention of polarity displayed by soft iron is exceedingly feeble, and easily deranged.

Hence it appears that, as far as ferruginous bodies are concerned, the vigorous retention of magnetic polarity exhibited by some of them, and the almost total absence of this quality in others, may very easily be explained upon the principles already advanced; and perhaps it would only require that we should consider copper and other non-ferruginous metals to be still better magnetic conductors than soft iron, to reconcile the sudden and total disappearance of polarity in them to the same principles, whether the exciting agent be magnetic or electric.

I have deflected a magnetic needle by an electric current traversing an ignited charcoal conductor, as was first shown by the very interesting experiments of Mr. Kemp; but as we are not aware of the total absence of the magnetic matter in charcoal, the experiment is inconclusive, any further than as an interesting fact, which has no particular bearing on the present discussion.

The energies of ferruginous electro-magnets are invariably exalted by multiplying, to a certain extent, the number of coils of conducting wire. My large electro-magnet, described in a former communication, requires twelve coils to accomplish its maximum of power (400 pounds). The general explanation of this fact is, I believe, that one wire alone is incapable of transmitting or conducting the whole of the electric force; and therefore a multiplicity of conducting wires becomes necessary in order that the battery may be enabled to give a full and complete display of its electric energies. And in order to accomplish this object the more completely, the extremities of all the wires are brought as close as possible to the voltaic plates. The wires of the large American magnet are even soldered to the plates of the battery.

I find, however, that although an addition of coils is attended with an accession of magnetic power until a maximum of polarity is accomplished, it is by no means essential that all those wires arrive immediately at the battery. A single copper wire may intervene between the coils round the iron and the poles of the battery without deteriorating the energies of the magnet, which will still be displayed to a maximum, as decidedly as if the whole system of wires were soldered directly to the plates.

My large electro-magnet is still capable of supporting its 400 pounds, notwithstanding the electric force has to traverse six inches of bell-wire *before* it arrives at the coils; and also six inches *more* from its quitting the coils till its arrival at the other pole of the battery;—in all, twelve inches of single bell-wire. There is a limit, however, to the dimensions of the intervening wires. If they be too long or too thin, the magnet will not display its maximum of power. With pretty stout bell-wire, and the length not exceeding twelve inches, I always succeed. The battery which I employ is a single pair of metals, sufficiently small to be placed in a pint pot.

This novel and curious fact is one of those which bears directly on the subject in question, and in a theoretical point of view is of a most interesting character. In practice, also, I find that it is exceedingly useful; giving a facility of manipulation so desirable in the management of very large electro-magnets, but which is not to be expected when all the extremities of the wires arrive immediately at the copper and zinc.

The theory of *polar magnetic lines* which I have advanced, requires not two magnetic fluids, nor indeed is it favourable to that doctrine; and if it be not fatal to the circulating currents of Ampère, it will at least require them to be in motion in a great variety of planes, which that distinguished philosopher never intended they should pursue. It is possible, however, that electric currents are naturally attended with magnetic polarity, independently of that which has been supposed to be excited in the wire; but it is by no means so probable that the existence of magnetic polarity is universally due to the permanency of electric currents. Electric currents may very possibly, either directly or indirectly, magnetize the terrestrial globe; but we have no reason whatever to believe that such currents are essential to give retention of polarity to steel.

The introduction of *polar magnetic lines* into the theory of electro-magnetism would simplify the explanation of the phænomena, and reduce them to the principles of magnetics; and experiments may be shown in both sciences which are favourable to such a conclusion, independently of any consideration that would reconcile to identity the electric and magnetic matter.

If it can be admitted as an universal maxim in nature, that when one species of matter is impregnated with, contains, or is charged with another, the charged body must necessarily be of a grosser texture than the substance with which it is charged, or that the latter should be more subtle than the former; then it is possible that the magnetic matter, which is the most subtle we are acquainted with in nature, may insinuate itself into the pores of the electric; and the latter become charged with the former, as decidedly, under some circumstances, as a piece of iron is naturally charged with them both.

I shall not, however, on the present occasion, advance further into speculative suppositions of this kind, which, however curious they may appear in themselves, are perhaps not of much interest in the present stage of our knowledge of physical operations.

[To be continued.]

LXIII. *Note on* Mr. Potter's *Reply.* *By* William R. Hamilton, *Esq. Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland* *.

FROM Mr. Potter's Reply, published in the April Number of the London and Edinburgh Philosophical Magazine, I collect some additional facts respecting his experiment of prismatic interference, which do not seem to have been stated in his first account of that experiment. In Mr. Potter's first paper, the stress of his objection to the undulatory theory of light seemed to be laid on the *observed direction* of a certain deviation; to which he opposed his *calculated decrease* of a certain hyperbolic ordinate. I showed that *this* observed fact, of deviation in the observed direction (towards the thickness of the prism), could be accounted for by the prismatic aberration of figure, which changed the decreasing hyperbolic ordinate to an *increasing ordinate* of a certain other curve. But I was of course aware that this prismatic aberration, though a cause acting in the observed *direction*, might not be energetic enough to account for the whole, or even for the greatest part of the observed effect; and that whether aberration was, or was not, an *adequate* as well as a *real* cause (on the undulatory theory of light), must depend on the comparison of my calculated formulæ with the *observed magnitude* of the deviation, of which Mr. Potter had not given any measure, or even any estimate. I am happy to have been the means of inducing Mr. Potter to bring forward some additional testimony on this important point: and willingly admit, that according to this new testimony, there remains, after allowing for my suggestions, a large residual phænomenon.

Dublin, April 13, 1833.

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

Journal of the Asiatic Society of Calcutta, Nos. 1, 2, *and* 3; *with Plates.* Calcutta, 1832.

WE are particularly desirous of calling the attention of our readers to this valuable monthly periodical, which we are afraid is by no means so well known in Europe as its merits entitle it to be. The present Numbers form the continuation of a scientific journal, published also

* Communicated by the Author.

at Calcutta, under the title of "Gleanings in Science;" a work intended to contain a mixture of original communications and of extracts from the best scientific journals of Europe. From the abundance and importance of the original communications, the "Gleanings in Science" soon became as replete with novel matter as any other publication of the same kind; and the success of the work was so considerable in India, that application was made to publish it under the auspices of the Asiatic Society. The request was immediately granted, with the understanding that the permission was to be continued as long as the publication should be under the charge of one or both of the Secretaries of the Society. Hence the change of title and present name.

Under the modest title of "Gleanings in Science," the first three volumes contain numerous and valuable papers on the Meteorology, Geology, Mineralogy, Zoology, Literature, and Statistics of India, together with numerous chemical analyses of Indian products, and criticisms on works of science relating to India. We would particularly draw attention to the papers of Messrs. Wilson, Herbert, Prinsep, Hodgson, Benson, Paddington, Everest, and Buchanan; many of whom are already well known to the European public.

The first Number of the "Journal" before us contains a paper by Mr. Wilson (now Professor of Sanscrit in the University of Oxford,) on the contents of the Dul-va; a memoir by Mr. Hodgson on the Native method of making Paper; an account of a new genus of Land Shells, by Mr. Benson; an examination of Minerals from Ava, by Mr. Prinsep; an account of a new Bridge near Hyderabad; a method of rectifying a Route Protraction; a comparison of the Indus and Ganges; a Summary of Meteorological Observations made at Calcutta in 1829, 1830, and 1831; an account of the Earthquake at Lahore in 1831; with a notice of the Population of Allahabad. To these are added, the proceedings of the Asiatic Society of Calcutta, of the Medical and Physical Society, and of the Natural History Society of the Mauritius; proceedings which are also noticed in the other Numbers.

The other two Numbers contain memoirs of equal interest; among which we may mention Mr. Royle's papers on his collections of Natural History made in the Himalayan Mountains, &c., and on the Botanic Garden at Seharanpore; Mr. Benson's remarks on the *Antelope Hodgsonii*; Mr. Wilson's analysis of the Poranas; an account of the progress of the Trigonometrical Survey of India; Hourly Observations on the Barometer in the Fortress of Cavita, &c.

Our readers will form a better judgement of the nature and objects of the Journal of the Asiatic Society from this sketch of its contents, than from lengthened detail. We must, however, more particularly call attention to the Summary of Meteorological Observations made at Calcutta in 1829, 1830, and 1831, drawn up, as we understand, by the editor, James Prinsep, Esq. F.R.S., as it contains the monthly and diurnal oscillations of the barometer and thermometer at Madras, Ava, Calcutta, Benares, and Seharanpore, or at five places situated on an inclined plane between the 12th and 30th degrees of north latitude.

We have little doubt that the cultivators of science in Europe will be greatly gratified by the evidence this Journal affords of the activity of the scientific men in India; and that the work itself will prove highly valuable to them, adding, as it does so materially, to our stock of knowledge, more especially as regards that most interesting portion of the world.

LXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

1832. Nov. 15.—A PAPER was read, entitled "On some Properties of Numbers in Geometrical Progression." By Charles Blacklewar, Esq. B.A. Communicated by J. G. Children, Esq. Sec. R.S.

Nov. 22.—A paper was read, entitled "Account of an Improvement in the Machine for producing Engravings of Medals, Busts, &c. directly from the Objects themselves, in which the Distortions hitherto attending such Representations are entirely obviated." By Mr. Bate. Communicated by J. G. Children, Esq. Sec. R.S.*

A paper was also read, entitled "An Account of the Construction of a fluid refracting Telescope of eight inches aperture and eight feet nine inches in length, made for the Royal Society by George Dollond, Esq. F.R.S." By Peter Barlow, Esq. F.R.S.

The author has, in former papers read to this Society, pointed out the great variety of cases included under the general formulæ relating to the operation of fluid refracting telescopes, and stated the difficulty of selecting, independently of experiment, the particular case which was likely to produce the best result. This subject is pursued in the present paper; and the principles and calculations stated at length which the author has applied in the construction of the telescope which the Council of the Royal Society directed should be made by Mr. Dollond, under the superintendence of the author, in order to put these principles to the test of experiment, and to decide the question of the expediency of proceeding in the construction of a similar telescope of much larger dimensions. When the experimental telescope was completed, it was found that its performance agreed in every respect with the computed results, as well in focal distance as in chromatic and spherical aberration. The arrangement of the lenses was such, that the corrections are all of them made in the transmission of the light through the fluid, and by the fluid only. The author abstains from offering any remarks on the performance of this telescope, leaving it to those whom the Council of the Royal Society may appoint, to decide upon its merits. He concludes by expressing his obligations to Mr. Dollond, for the readiness with which he complied with all the suggestions of the author, and for the accuracy with which he has executed every part of the instrument.

* A notice of this improvement, by Mr. Bate, will be found in our last number, p. 288.—Edit.

Report of the Council to the Anniversary Meeting on St. Andrew's Day, 1832.

The Council of the Royal Society have, during the past year, used their most earnest endeavours to render the Library as effective for the purposes of science, as the means at their disposal would enable them. They have been desirous, in particular, to make it as complete as possible in all those departments of science, which it is more especially the object of the Royal Society to cultivate and to advance. They have accordingly purchased, with the advice of the Library Committee, such books as were more immediately required for these purposes, at an expense of about £1600. It was evident, however, that the mere possession of these books by the Society would be of little avail to those who wished to use them, until they were arranged and catalogued according to some uniform and well-digested method. A Committee was therefore appointed to consider of the best plan of effecting this desirable object; and to suggest measures for obtaining a correct catalogue of the library, arranged under such specific heads as were best calculated to assist the inquiries of all those who might resort to it for information. Various plans for this purpose were proposed and discussed: and it was finally determined that in order to insure uniformity of execution, the whole labour of compiling the new classed Catalogue, and of conducting it through the press, should be confided, though still under the superintendence of the Committee, to one person only; provided a proper person could be found who was fully competent to so arduous a task, and also willing to undertake it. The Council have accordingly engaged Mr. Panizzi, of the British Museum, a gentleman of great literary attainments, and conversant with that kind of labour, to undertake this charge; and have no doubt that he will accomplish it to the full satisfaction of the Fellows of the Society at large, to whom the possession of such a classed Catalogue as the one proposed, will be advantageous in many ways, independently of its direct utility in reference to the employment of the library.

The whole of the sum at which the Arundel Manuscripts which have been exchanged for books, were valued, has now been received from the Trustees of the British Museum, and the account with them is thereby closed.

The Council have also directed the printing of an edition of the Abstracts made by the Secretaries and entered on the Journal Book of the Society, of such papers as have been read to the Society and ordered for publication in their Transactions, from the year 1800 inclusive, to the present time. They conceive that a collection of these Abstracts, which possess in themselves much intrinsic value, will form an useful sequel to the Abridgement of the Philosophical Transactions of which the public is already in possession, but which does not extend to a later period than the end of the last century. This work will form two thick octavo volumes, one of which is now completed and ready for delivery to subscribers. The proof sheets, at the desire of the Council, were read over by Mr. Lubbock and Mr.

Children, and no alterations were made except for the correction of errors obviously arising from inaccurate transcription. The Council have also directed a general Index to be made of the contents of the Transactions from the year 1821 to 1830 inclusive.

Documents relating to the periods and heights of the Tides having been furnished to the Society, at the request of the Council, by favour of the Lords Commissioners of the Admiralty, who have obligingly ordered these returns to be made from the principal sea-ports of England, a Committee has been appointed for the purpose of examining and digesting them, and for printing such of the observations or results as they may deem useful.

The Committee for conducting the Meteorological Observations have been anxious to arrange a plan for insuring their accuracy, and increasing their utility. They find that standard instruments are much wanted for furnishing correct data in this department of science. This deficiency they are endeavouring to supply; and have in particular been promised the kind assistance of Mr. Daniell and Dr. Prout in superintending the construction of a standard barometer of superior accuracy, on the indications of which they expect that the utmost reliance may be placed.

The telescope, which the Council, with the advice of a Committee, had requested Mr. Barlow to construct as an experiment, on the principles stated by him in his paper in the Philosophical Transactions, is now completed, and will soon be ready for trial.

The Council have awarded one of the Copley Medals to Mr. Faraday, for his discovery of Magneto-Electricity, as explained by him in his Experimental Researches in Electricity, published in the Philosophical Transactions for the present year.

Oersted's important discovery of the influence of voltaic electricity on a magnetic needle, was rapidly succeeded by a series of minor ones, all tending to establish the existence of an intimate connexion between magnetism and electricity. The evidence, however, of that connexion, resting, as it did, on the mutual influence of magnets and wires in which electric currents passed, and in the development or induction of magnetism by electricity, was positive on one side only; to render it conclusive, it remained to be shown that electricity could be excited by magnetism: and this, by a series of experiments as simple as they are beautiful, founded on a train of correct reasoning, Mr. Faraday has happily accomplished.

Although the Council consider that the discovery of magneto-electricity fully entitles its author to the Copley Medal, they by no means limit the value of the papers in which it is detailed to this discovery, however important. Even the preliminary facts, as they fully establish volta-electric induction, had they at the time led no further, would have been of the greatest value; but they were in hands in which they could not long remain barren, and the expectation they held out of important results was soon realized. Beyond the details of the discovery, the author rapidly but clearly establishes the laws according to which electric currents are excited by a magnet. He satisfactorily applies these laws to the explanation of a very interesting class of phenomena previously observed, namely, the reciprocal action of magnets and metals during rotation. He at the same time establishes an important distinction among bodies which had long been considered as associated by phenomena common to them all; and gives indisputable evidence of electric action due to terrestrial magnetism alone. An important addition is thus made to the facts which have long been accumulating for the solution of that most interesting problem, the magnetism of the earth.

The Council have awarded another Copley Medal to M. Poisson, for his work entitled *Nouvelle Théorie de l'Action Capillaire.* In this work a great variety of problems are solved relative to molecular attraction, some of which had not before been attempted; but the most remarkable feature of the work is, the conclusion which the author draws, namely, that the elevation and depression of liquids in capillary tubes are essentially dependent on the rapid variation of density which takes place at the surface of the fluid, and without which, according to the author, that surface would continue plane; this is at variance with the theory given in the *Mécanique Céleste,* although indeed Laplace notices this change of density at the surface, as a necessary consequence of the action of the molecules upon each other (Supp., x. livre, p. 74.) The theorems and expressions of M. Poisson do not differ in form from those of the *Mécanique Céleste*; but the constants which are involved in these equations are not expressed by the same definite integrals. No difference ensues in the consequences which are deducible from them, because the law of molecular attraction being unknown, it is impossible to arrive at the value of these constants *à priori,* or otherwise than by observation.

M. Poisson has calculated the vertical and horizontal pressures upon a solid body plunged in a fluid: the value of the latter does not agree with that given in the *Mécanique Céleste.* According to the expression of Laplace the body might take a motion of translation: to this objections were formerly made by Dr. Young, and it will be noticed with interest that these objections are now confirmed by M. Poisson. The Council have awarded the Medal to the author, in order to testify the high sense which they entertain of the importance of the researches contained in the work in question.

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

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

Other Members of the Council: Francis Baily, Esq.; Captain Francis Beaufort, R.N.; Mark Isambard Brunel, Esq.; Rev. William Buckland, D.D.; Samuel Hunter Christie, Esq. M.A.; William Clift, Esq.; Rev. James Cumming, M.A.; Benjamin Gompertz, Esq.; Joseph Henry Green, Esq.; George Bellas Greenough, Esq.; William George Maton, M.D.; Roderick Impey Murchison, Esq.; William Hasledine Pepys, Esq.; Stephen Peter Rigaud, Esq. M.A.; Rev. Richard Sheepshanks, M.A.; Rev. William Whewell, M.A.

LINNÆAN SOCIETY.

April 2, 1833.—A paper was read, entitled, "On the Modifications of Æstivation observable in certain Plants formerly referred to the Genus *Cinchona*." By Mr. David Don, Libr. L.S.

The æstivation of corolla is much more varied in monopetalous than in polypetalous flowers, for, with the exception of a portion of the *Rutaceæ*, principally from New Holland and South America, the imbricate form is found almost generally to prevail in the latter class. Among the monopetalous orders the form of æstivation is a character of such high value as oftentimes to afford the only palpable distinction to the limitation of families; but the *Rubiaceæ* present a striking exception, examples of almost every modification of æstivation being afforded by it; and although among them it is a character of less value, still it will be found materially to assist in distinguishing the genera of that extensive family.

The genera enumerated and described by the author are *Cinchona*, *Cosmibuena*, *Exostema*, *Hymenodictyon*, *Luculia*, *Pinckneya*, and a new one, founded upon the *Cinchona rosea* of the *Flora Peruviana*, which he has named and characterized as follows:—

LASIONEMA. Cinchonæ sp. *Ruiz et Pavon.*

Calyx 5-dentatus. *Corolla* tubulosa, limbo 5-fida, æstivatione imbricatâ. *Stamina* exserta: *filamenta* medio barbata: *antheræ* subrotundæ, peltatæ! biloculares: *loculis* basi solutis. *Stigma* bilobum. *Capsula* bilocularis, medio loculicido-dehiscens! polysperma. *Semina* exigua, samaroidea. Arbor (peruviana) *inflorescentiâ paniculatâ.*

1. L. roseum.

This constitutes a very distinct genus, differing from *Cinchona* not only in its imbricate æstivation, but likewise in the structure of the stamens and in the dehiscence of its capsule. So little has the æstivation of corolla been attended to among these plants, that we find the present genus included among the synonyms of *Cinchona lancifolia* in the *Nova Genera et Species Plantarum* of Professor Kunth.

April 16.—Read, "Remarks on a few rare Scottish Plants, chiefly from the Clova mountains." By Mr. David Don, Libr. L.S.

The mountains of Clova, which bound the upper part of Forfarshire, have long been famed in the annals of British Botany, as affording many of those interesting additions made to the Scottish Flora, by the author's father, the late Mr. George Don, of Forfar, and which the discoveries of more recent investigators have shown to be still far from being exhausted of novelties.

The vegetation of Clova is remarkable for its comparative luxuriance, many plants of similar species being found of a more gigantic size than are to be met with on Ben Lawers, Ben Nevis, and Cairngorum, and in general they are found at much lower elevations on the Clova range, which may perhaps sufficiently account for their increase in stature.

Lychnis alpina.

This elegant little plant was first discovered by the late Mr. Don in the summer of 1795; and again accidentally met with in considerable abundance by Sir John Ogilvie, Bart. in August last. The specimens from Clova agree in every respect with those in the Linnæan Herbarium. The capsule in this plant is uniformly five-celled, but the thin partitions in the advanced state are found occasionally partially obliterated.

Mulgedium alpinum. Sonchus alpinus *of Linnæus.*

The late Mr. Don was acquainted with several stations for this plant; and Dr. Graham's party found it in five different places, some of them being seven or eight miles apart. One or two of the stations are fortunately quite inaccessible, so that the plant is not likely to be entirely eradicated.

It is singular that of this, which is also a Lapland plant, there is no specimen in the Linnæan Herbarium; the two so named are North American species, and apparently from the Upsal garden; one of them being *Sonchus floridanus*, and the other example (which has also the number of the *Species Plantarum* attached to it,) *Sonchus spicatus* of Lamarck, the *Leucophæus* of Willdenow. Both these are also species of *Mulgedium*; and our late excellent President, misled by the number attached to the specimen in the Linnæan Herbarium, has been induced to publish this last as the real *alpinus*, substituting for the actual plant the name of *cæruleus*. All these plants more naturally associate with *Lactuca* than with *Sonchus*, only differing from the former in the less attenuated apex of the achenia.

ROYAL ASTRONOMICAL SOCIETY.

Dec. 14, 1832.—The following communications were read:—

I. Extract of a letter from Professor Santini to Professor Airy, dated Nov. 23, 1832. Communicated by Professor Airy.

M. Santini succeeded in making several observations of Biela's comet, which are given in the monthly notices of the Society.

On his own observations and those of Sir J. Herschel, M. Santini observes:—

"It is remarkable that its place should have been found nearly intermediate between my ephemeris and that of M. Damoiseau; from which it seems to follow, that the line of perihelion passage was badly determined in both, which is probably owing to the assumption of a constant orbit through the whole period, in the calculation of the co-ordinates for the computation of the perturbations; and as, in May of last year, the comet approached very near to Jupiter, a slight error in these co-ordinates may have had a sensible influence on the perturbation of mean motion, on which depends the return to perihelion.

"I looked carefully for Biela's comet on several evenings in September, with the equatorial of this observatory, but I could not discover any thing that had the least resemblance to a comet. The telescope is one of Fraunhofer's, of three inches aperture, and is very clear. I hear that it was observed at Rome on Sept. 24, with a telescope made by Cauchoix, of eight inches aperture, and that it was extremely faint. I was absent from the observatory from the middle of October to the 31st, on which evening I found it easily between the places given by the two systems of elements, as you inform me Sir J. Herschel saw it in September. At Milan it was observed on Oct. 24. On the last evenings it was very faint and very difficult to observe: in general, the least disturbance of the atmosphere extinguished it entirely."

II. Various observations made at the observatory of St. Helena by Lieutenant Johnson; consisting of,

1. Stars observed with the Moon from March to July, 1832.
2. Observation of the solstice of June 1832.
3. Observation of the Solar Eclipse of July 27, 1832.

III. Investigation of a rule for clearing the apparent distance of the centres of the Sun and Moon from the effects of parallax and refraction. By Charles Blackburn, B.A., late R.M. College.

In the Monthly Notice for May 1832 is given a formula of Baron Zach for the solution of this problem. The method proposed by Mr. Blackburn is similar in principle, so far as it consists in determining the true distance by the application of certain corrections to the apparent distance; but differs in the manner of obtaining the corrections, which are two in number instead of four.

IV. Observation of the Transit of Mercury of May 5, 1832. Made at Utrecht, by Professor Moll.

The author first notices the observations made in Holland at the preceding transits of Mercury in 1661, 1697, 1753, 1799, and 1802, and then proceeds to state the preparations that were made to observe the present transit. A table contains the result of the observations.

"Even with such small powers as 64 and 76 of the achromatic telescopes of Fraunhofer and Dollond," Dr. Moll observes, "I could plainly perceive a grayish spot on the dark disc of Mercury. As soon as I had perceived it, I asked my assistants whether they saw any thing particular on Mercury. One of them instantly replied, '*Do you mean the white spot?*' On applying higher powers (as 110 and 180 to the 42-inch, and 96, 144, 216, and 324 to the 6-feet,) the same appearance was always visible. Its periphery was not well defined, but seemed gradually to sink from a grayish white to the dark colour of the planet's disc. It constantly appeared on the same part of the disc.

"After the observation, looking in Schröder's *Hermographic Fragments*, I was much surprised to find that both Schröder and Professor Harding had observed a similar appearance during the transit of 1799. They even attempt a delineation of the object, in which there are two separate spots. I cannot say that I saw any thing resembling this, as it seemed to me and others that there was one grayish undefined spot on the black disc of Mercury.

"I must not here omit the circumstance, that this gray spot was most clearly visible when the eye had had some rest. On applying it then to the glass, it was most easily distinguished.

"The observers at Dr. Van Beek's house saw nothing of this singular phænomenon.

"Some observers state, that in former transits a coloured lighter ring seemed to surround the orb of Mercury on the Sun's disc. According to Plantade, an appearance of the sort was seen at the transit of 1736. Flaugergues saw the same in May 1786, in Nov. 1789 and 1799. He calls it an illuminated ring, *un anneau lumineux.* Nothing, however, of the kind is mentioned in Delambre's account of the transit of 1799, nor in that of Messier of that of 1786. Prosperin and Ferner, however, speak of it as observed during the transit of 1786. But the most circumstantial account of the phænomenon is that of Harding and Schröder, during the transit of 1799. They describe it exactly as we saw it, both with the 42-inch and 6-feet telescopes. *A nebulous ring of a darker tinge, approaching to the violet-colour, appeared to surround Mercury's disc.* Near the planet the colour was darkest. Now, what we saw, and Schröder and Harding saw, is exactly the reverse of what is mentioned by Plantade and Prosperin. They talk of *a luminous ring;* we saw, or at least think we saw, *a darker, more deeply-coloured zone surrounding Mercury.* Ljungberg saw also a dark nebulous ring round Mercury, at Copenhagen, in 1802*.

V. Occultation of Saturn by the Moon. Observed at Utrecht, by Dr. Moll and Mr. G. R. Fockens, 8th of May, 1832.

The same phænomenon was observed at Leyden, by Professor Uylenbroek and Mr. Kaiser.

VI. Remarks on the fifth Catalogue of Double Stars, communicated to the Society, June 7th, 1832. By Sir J. F. W. Herschel.

VII. Stars observed with the Moon at the Royal Observatory, Greenwich, in the months of October and November, 1832.

VIII. Stars observed with the Moon, at the Cambridge Observatory, in November and December 1832.

December 10, at $12^{h}\ 21^{m}\ 53^{s}{\cdot}86$ Cambridge sidereal time, or $19^{h}\ 1^{m}\ 32^{s}{\cdot}8$ Greenwich mean solar time, δ Cancri disappeared at the Moon's bright limb. The limb was uneven, and the star seemed to run along one long mountain slope before it disappeared. The time is uncertain two or three seconds.

There was laid on the table, amongst other presents, a MS. copy of Dr. Halley's astronomical observations made at the Royal Observatory at Greenwich, copied from the originals by order of the Lords Commissioners of the Admiralty, and by them presented to the Society.

PHILOSOPHICAL SOCIETY OF CAMBRIDGE.

March 11.—A Memoir by the Marchese Spineto was read, containing objections to the chronological system of Sir Isaac Newton; and reasons for preferring the more extended chronology, which is

* Zach. Monatl. Corresp. vol. viii. 1803, p. 335.

suggested by the study of Egyptian antiquities. After the meeting Dr. Jermyn exhibited various antiquities, found in association with bones partly interred and partly deposited in urns, which have been discovered at Exning and Bartlow in this neighbourhood.

Professor Sedgwick also gave an account, illustrated by drawings and sections, of the geology of North Wales. He stated, that by various traverses across Caernarvonshire and Merionethshire, it was ascertained that the strata of that district are bent into *saddles* and *troughs*, of which the *anticlinal* and *synclinal* lines occur alternately, and are all nearly parallel to the "great Merionethshire anticlinal line." The direction of these lines is nearly N.E. by N. and S.W. by S.; and they appear to pass through the following points:—(1.) Near Caernarvon; (2.) Mynydd Mawr; (3.) Garn Drws-y-loed; (4.) Moel Hebog; (5.) Moel Ddu; (6.) Between Pont-aber-glass-lyn and Criccieth; (7.) The Great Merioneth anticlinal; (8.) The west side of the Berwyns; (9.) The calcareous beds to the west of Llanarmon Fach. The bearing of these facts upon the general views of Elie de Beaumont was noticed; and it was observed that the approximate parallelism of the most prominent mountain chains of Wales, the Isle of Man, Cumberland, and the South of Scotland, corroborate the justice of his theory up to a certain point: although on a wider scale, these apparently parallel straight lines may be found to be portions of curves of small curvature.

April 22.—The following notice by Professor Miller was read:—

At the Oxford meeting of the British Association, Sir David Brewster announced the discovery of a series of fixed lines in the spectrum formed by light that had been transmitted through nitrous acid gas. As it does not appear that Sir D. Brewster examined the effects produced by any of the other coloured gases, I beg to offer the Society a short account of some experiments which I made conjointly with Professor Daniell, in the laboratory of King's College.

In these experiments the light of a gas-lamp, after having passed through a jar filled with the vapour to be examined, was made to converge to a focal line by interposing a tube filled with water. The line of light thus obtained was then viewed through a prism, with the assistance of a small telescope attached to the prism, in such a position that the incident and emergent rays made equal angles with the first and second faces of the prism.

When the air in the jar was slightly coloured with the vapour of bromine, the whole of the spectrum was seen interrupted by probably more than a hundred equidistant lines; as the vapour became denser the blue end of the spectrum disappeared, and the lines in the red part grew stronger.

When the light was transmitted through the vapour of iodine, a series of equidistant lines were seen exactly resembling those produced by bromine;—a new and unexpected analogy between two substances which have so many other properties in common. The density of the vapour of iodine did not appear to have any sensible effect upon the visible extent of the spectrum.

Chlorine extinguished the blue end of the spectrum without producing any lines. The total absence of lines cannot, however, be inferred from our observations, as the apparatus was not very carefully adjusted.

Euchlorine produced a number of broad lines at irregular intervals in that part only of the spectrum which was extinguished by chlorine.

Lastly, the vapour of indigo was tried, but without producing any lines. The near approach of the temperature at which indigo is decomposed to that at which it is volatilized, made it difficult to obtain enough of the vapour to give a decisive result in this case.

W. H. Miller.

COMMEMORATION OF THE CENTENARY OF THE BIRTH OF DR. PRIESTLEY.

Addresses delivered at the Commemoration of the Centenary of the Birth of the Rev. Joseph Priestley, *LL.D. F.R.S., regarded as the* Founder of Pneumatic Chemistry, *holden in Freemasons' Hall, London, March* 25, 1833.

[In our last Number we briefly noticed the Commemoration of the Centenary of the Birth of Dr. Priestley, giving the names of some of the patrons and cultivators of science who were present. We now redeem our promise of publishing a more complete view of the proceedings, by giving a report of the addresses which were delivered on this interesting occasion.]

At the commencement of the proceedings especially appropriate to the commemoration, William Babington, M.D. F.R.S., President, addressed the assembly to the following effect:—

Gentlemen,—I am very desirous to have it distinctly understood, that I have not presumed to be your Chairman, on the present memorable occasion, in conformity either with my own inclination, or my own judgement, but in compliance with the request of several much respected friends of our Committee of Stewards, with whose wishes I found it impossible not to comply. I therefore trust that you will have the kindness to make the necessary allowance; and I am not without hope that my best endeavours, aided by your friendly assistance, will effect the object of our present meeting to our entire satisfaction.

Gentlemen, you must already be sufficiently aware that the object for which we are now assembled is to commemorate the Centenary of the Birth of a very distinguished and highly gifted individual, Dr. Joseph Priestley, the principal founder of pneumatic chemistry. I had the gratification of knowing him personally, but not until he had himself, by the novelty and importance of his researches, become known to all the world. The principal founder of pneumatic chemistry he undoubtedly was, if not the sole inventor; for although it may be granted that he had, in a few isolated instances, been anticipated, yet I may venture to assert, without fear of contradiction, that, during the period in which he was actively engaged in that department of experimental science, he gave more evidence of original genius, exhibited more novelty, made more numerous experiments, and obtained more important and valuable results, than could be justly claimed on behalf of any of his cotemporaries.

To the correctness of this statement I have reason to expect that satisfactory testimony will be given, in the course of the evening, by several friends present, whose authority on this subject will not be disputed. If in estimating what we owe to the discoveries of this most sagacious and successful inquirer, I may be allowed to refer to the benefits which they have conferred upon mankind in reference to my own profession, I may ask what, before the time of Priestley, did we know of the constitution of the atmosphere,—of the composition of water,—of the nature of mineral springs? And were we not comparatively ignorant of the composition of metallic oxides, mineral acids, and many other of the most active articles of the Materia Medica? I may also be permitted to remind you, that for the purpose of applying certain of the elastic fluids, or gases, to the treatment of pulmonic diseases, Dr. Beddoes, of the University of Oxford, quitted his employment as professor, to settle himself at Bristol; and that it was to the necessity of his requiring assistance in this pursuit that we are indebted for the additional light thrown on chemical research by the genius of the immortal Davy. Had oxygen gas alone been the fruit of Dr. Priestley's investigations, the obligations conferred on our profession must have been indelible. Of this I might adduce a proof by referring to an occurrence, the particulars of which were made public by myself in the year 1807; and as they may be considered not altogether irrelevant, I will, with your permission, briefly state them.

Two persons, servants in a public-house in the neighbourhood of Aldermanbury, where I then resided, the one a lad of 13 years old, the other a man of 35, having gone to bed in a small room in which a brasier of lighted charcoal had been left burning, were found the next morning in a state of complete insensibility. The lad had fallen on the floor, and appeared quite lifeless; and all endeavours at resuscitation in his case proved unsuccessful. In the man some signs of life still remained. Before my arrival he had been removed into a large chamber, and a few ounces of blood had been taken from his arm. From these measures, however, no improvement had been effected. I found him still quite insensible; his countenance pale, his respiration imperfect, his pulse sinking, his tongue protruding, and his under jaw in a state of spasm. In this apparently almost hopeless state, it occurred that the most likely means of restoring vitality would be to produce artificial respiration, and at the same time to employ oxygen gas in place of atmospheric air. Having by good fortune a portable galvanic trough at my command, and being promptly supplied by my friend, Mr. William Allen, with the necessary quantity of oxygen gas, there was little or no loss of time in making the experiment. Nothing could be more satisfactory than the result. At every application of the galvanic conductor to the lower and anterior part of the chest, a muscular spasm ensued, by which the chest was expanded, and an opportunity was consequently given for the introduction of the oxygen gas with obvious effect. By a repetition and continuance of this proceeding, we had the gratification to find that our endeavours were completely successful, the man being in the end restored to health.

Not considering myself at liberty to trespass further on your time, I have now, Gentlemen, to request that you will rise from your seats, and, in reverential silence, drink "To the Memory of Dr. Joseph Priestley, the Founder of Pneumatic Chemistry."

The President next called upon the assembly to mark in a similar manner its respect for the memory of those other distinguished individuals of our country who laboured with Dr. Priestley in the same field of science, and who have since also paid the debt of nature. The list, he observed, would be long,—commencing with Black, Cavendish, and Kirwan, and ending with Wollaston and Davy.

The meeting was then addressed as follows by Dr. DAUBENY, Professor of Chemistry at Oxford, whose name had been connected by the President with an expression of respect for that University.

In the name of the University of Oxford, I beg leave to return you my best thanks for the honour you have done that body on the present occasion. I receive the toast with the greater satisfaction, as an evidence of the good feeling that subsists, and which I trust may ever be maintained, between men engaged in the common cause of discovering and of disseminating truth, whether they may chance to belong to the older institutions of the country, or to those which have more recently sprung up amongst us. I can assure you, on the part of the University, that the same feeling is reciprocally felt towards you; and that although we in Oxford are considered to be more intent on training and disciplining the minds of youth for the future reception of scientific knowledge than in inculcating the facts of any particular science, yet that for this very reason perhaps we are the more sensible of the debt of gratitude we owe to such individuals as the one we are this day met to commemorate, who acted as the pioneer in a new path of discovery,—fully aware that our theories and systems, however logical and ingenious they may be, would, without the assistance of the facts brought together by the labours of these experimentalists, turn out as baseless and as unsubstantial, as are those of the ancients on matters of physics, whose writings amuse us in our closets. Neither ought an ecclesiastical body like ours to be unmindful of the services rendered even to the cause of religion by one in particular of Dr. Priestley's chemical discoveries; I mean that of the carbonic acid exhaled by animals being found to constitute the food of *plants*,—a discovery which, more perhaps than any other within the whole range of chemistry, is calculated to evince the adaptation of one part of nature to another, and which, ripened and confirmed as it has been by the subsequent researches of Saussure and others, suggests to us the means by which the Creator preserves to the atmosphere, unchanged, even to the end of time, that identical constitution and those precise properties, which we know to be best adapted to the wants of the animal and the vegetable creation; ordaining that every blade of grass which we tread under our feet shall contribute to supply the waste of oxygen caused by the respiration of land animals, whilst the sea-weed, the "*inutilis alga*" as the poet terms it, and the conferva that grows on the surface of the stagnant pool, compensate for that occasioned by the aquatic tribes.

I think, therefore, this Anniversary well deserves to be attended by others besides professed chemists, and that its celebration will contribute to the prosperity not only of the branch of science which Dr. Priestley especially cultivated, but even of other departments of physics. It will show to those engaged in such pursuits, that the fame to which they may aspire, if it be less brilliant than that to be derived from other applications of their talents, is at least equally durable, and certainly less open to controversy. Had, I may remark, Dr. Priestley himself been known only for his theological or his political writings, this, his anniversary, would chiefly have been attended by persons who agreed with him in those respects. As it is, we see men of the most opposite views on those points cordially joining to testify their sense of his merits on particulars which admit of no dispute. Their attendance here involves no opinion on their part with respect to the doctrines of the person whom they are met to commemorate on other subjects; still less does it imply any wish to exalt the interests of science above those higher ones which concern men more vitally, considered as individuals, or as members of society; but it has been dictated by the deep sense they entertain of the unrivalled services he has rendered to Chemistry—unrivalled, I mean, considering the time in which he lived, and the circumstances under which he was placed;—from the confidence, in short, which they feel, that the Philosophical Character of Dr. Priestley, which, during the time at which he lived, was alternately depressed and elevated unduly by the waves of political and theological strife, now that the tempest has subsided, has attained its just and natural level, and is destined to float for ever down the stream of time, buoyed up by the fame to which his discoveries in science so justly entitle him.

A similar tribute of respect having been paid to the University of Cambridge, associated with the name of the Rev. J. Cumming, Professor of Chemistry in that University, the following address was delivered by that gentleman:—

When it was first intimated to me that a meeting would be held this day to commemorate the birth of a man on whom I have been accustomed to look with reverence, I considered that I should be unworthy the situation I hold if I did not use every exertion to be present on this occasion. The use of these exertions has produced an indisposition which I must be allowed to plead as an apology for my not having prepared myself to say anything on the merits and attainments of Dr. Priestley, such as you are ready to receive, and such as you are entitled to expect from me. But I regret this the less because I see before me many persons more capable of doing justice to his merits than I could possibly be, were my health and my faculties in a state superior to what they are at present. When

I see before me the man who has extracted electricity from the magnetic spark, surely he, of all others, is capable of doing justice to the character of Dr. Priestley; from him, and from persons inspired by his genius, we may expect to acquire knowledge in that particular branch of science, which we may term in a more elevated sense *pneumatic chemistry*, equal to that which we have acquired through the labours of Dr. Priestley and those who have followed him in the department of physical inquiry to which we at present apply that designation.

One point in the character of Dr. Priestley I wish to have the pleasure of noticing, because I feel myself called upon to vindicate him in this respect. Some persons speak of him merely as an experimenter, who tried results in a variety of shapes, and by accident attained some happy discoveries. But the persons I now address must be well aware that these results are only seen by persons of keen observation,—by persons who are able to appreciate what they observe. If facts have remained unnoticed before by common eyes, surely the man deserves credit who is not only able to see, but to point out to others the value of what he observes.

There is another point to be considered in the character of Dr. Priestley. It was probably owing to this acuteness of perception and intellect that he was less capable of systematizing the results of his researches than some men are; for our faculties are so limited, that different orders of them are required to make discoveries and to systematize those discoveries when made. In my own opinion the character of a discoverer stands far higher than that of a systematizer: and surely it can be no diminution to the transcendant talents of Dr. Priestley as an inventor, that he had not the subordinate character of a methodizer of the facts which he discovered.

It is not my intention to enter upon the merits of Dr. Priestley; but on one point I will make an observation. We recollect those disgraceful proceedings by which he was driven from this country; and I congratulate myself that such a circumstance, if it were not a matter of history, could now scarcely be credited. I see before me men of different religious, and of different political sentiments; but we none of us think that we compromise either our religious or political opinions by endeavouring to do honour to a man who was an honour to his age and to his country.

It only remains for me, in the name of the University of Cambridge, of which I am on this occasion the unworthy representative, to return you my sincere thanks for the honour you have done us.

On the part of the Royal Society and its President, His Royal Highness the Duke of Sussex, J. W. Lubbock, Esq., Treasurer and Vice President of that body, addressed the meeting as follows:—

I am very sorry that it devolves on so humble an individual as myself to return thanks to you, in the absence of His Royal Highness the President, for the honour now conferred on the Royal Society. Every one, I am sure, regrets that His Royal Highness has lately suffered so much from illness; and were he not still indisposed, I am convinced he would be present. There are many circumstances, I think, connected with the name of Priestley, which must be very gratifying to the members of the Royal Society. First, Dr. Priestley was one of its Fellows; and next, we have the satisfaction and the great pride, that the Transactions of the Royal Society are the depositories of his discoveries. We have also the satisfaction of knowing that the Council of the Royal Society at that time were not unmindful of the very great merit of his communications; for in the same year that his first paper on the Properties of Air was communicated, they gave to him the Copley Medal, which was the highest honour they could bestow, and which is, in the words of Sir John Pringle, who presented it to him, "the palm and laurel of our community." The paper for which the Copley Medal was given, was not that which has been so much admired, on the discovery of oxygen gas; but that which contained the analysis of atmospheric air with nitrous gas; and it contained also (which was not much considered at the time,) the relation of his having received muriatic acid gas over quicksilver, and introduced particles of water, showing that the gas was immediately absorbed by the water. These and many other papers he contributed to the Royal Society; which show the great use of the Society at that time, when there were not so many Societies as there are now, nor so many scientific journals.

I am convinced there is no one present who does not wish prosperity to the Royal Society; and I am sure, also, that although some persons may have censured its proceedings, their censure has only proceeded from a desire to render it more useful to the public. I can truly say that this feeling exists nowhere so strongly as with His Royal Highness the President, and with the Council. I hope the Society will long endure, and that it will be the means of recording even more brilliant discoveries than have ever yet been given to the world.

I may take the opportunity of remarking, that too much has been said of the decline of science in this country. It is not for me to dwell on discoveries which have been made in chemistry; but I may refer to one which must strike us all, I mean that of Mr. Faraday with respect to the magnetic spark, which is recorded in the Transactions of the Royal Society of last year. Geology is not on the decline; and there are now astronomers and mathematicians whose names do honour to this country, papers by some of whom are to be found in our Transactions. We have had a paper by Mr. Ivory extending the expressions of Mr. Jacobi in the theory of elliptic transcendants, and, more recently, a paper by Professor Airy on a new Inequality of Venus. I have ventured to introduce these instances, because they show that science, even in these branches, is not altogether neglected.

The President having proposed the health of Charles Hatchett, Esq., F.R.S., as greatly distinguished by the industry and talent with which he had prosecuted chemical researches, that gentleman returned thanks, addressing the company to the following effect:—

I can scarcely find words to express to you the deep sense I have of the honour you have conferred upon me, being really at a loss to account for it;—only as the friend of Watson, of Davy, and of Young, can I presume to receive such an honour from you. I feel great pleasure in meeting so many distinguished men of science assembled to do honour to the memory of that illustrious philosopher Dr. Priestley. All chemists owe him the greatest obligations; but I feel peculiar obligations to him; I am under personal obligations to him;—not that I ever knew him, or ever spoke to him more than once. In very early youth, by mere accident, I obtained possession of his first two volumes, entitled "Experiments and Observations on different kinds of Air." These two volumes caught my attention, and first directed my thoughts to chemistry. I studied it, with one other book which was lying about my father's house, the Dispensatory of Colville, to which is prefixed a short history of the Materia Medica. As to Dr. Priestley's work, it is no wonder that it should have caught my attention; there is everything in it to captivate the young mind. Let any person look into its pages, and he will not fail to observe the beautiful perspicuity with which he relates his experiments, and the simplicity of the steps by which he effected his extraordinary discoveries. I therefore feel very deeply indebted to him for making me a chemist. It is a very curious and extraordinary thing that some single circumstance generally fixes our determination as to our future pursuit. I will answer for it, that amongst this assembly of eminent men, highly distinguished as you all are in science, there is not one of you but can refer to some circumstance in the early part of your life which set you upon your career of science, and which has led to your present eminence. Sir Joseph Banks, when quite a youth, happened to open a book upon botany; he read some of it, it caught his attention; he read more of it, and from that moment he imbibed a love of the science. The love of botany led to the love of natural history in general. Next he imbibed a desire to acquire some more valuable scientific knowledge, and then a desire to visit foreign countries, in order to become acquainted with the history and manners of its inhabitants: that led to a voyage round the world. Then he was elected Fellow of the Royal Society, subsequently its President; and he became such a patron of science that his name ought not to be, never can be, forgotten.

"Prosperity to the Linnæan Society, and the health of Dr. Bostock" having been proposed, that gentleman returned thanks in the following address:—

Allow me to return my thanks for the honour you have done me. Dr. Priestley has been justly eulogized as having enlightened every branch of science, and amongst others, natural history, which is the province of the Society with which you have honoured me by associating my name. My friend Dr. Daubeny has, however, pre-occupied that point for which Dr. Priestley's character, as a scientific man, is more remarkable—with respect to vegetable physiology, the doctrine of the action of light on plants, by which nature supplies that which is equivalent to respiration in vegetation. I will, therefore, take the liberty of adverting to that part of Dr. Priestley's character which I think tends to illustrate his attainments as a chemist.

I had the honour of being Dr. Priestley's pupil in early life; I was much in his laboratory, took notes for him, saw his method of proceeding; and without having had the opportunity of seeing him as I did, I think no one can be fully aware of his merits, his enthusiasm, his originality, his quickness in meeting all difficulties that occurred, and in taking advantage of all points which started up under his notice. It has been said by some persons that his success in experiments was accidental, that he tried everything, and in fact was jumping about without aim or method. Now that was not the case: he had the eye and the mind of a philosopher; and the number of his experiments serves to show his zeal, his industry, and his perseverance.

It has been remarked this evening by a gentleman who has preceded me [Professor Cumming], that Dr. Priestley was not much celebrated for arrangement; but in contrivance, in meeting obstacles, in bringing out new facts by new combinations, he was unrivalled. Let any person take up any contemporary author of his day, consider what was known when he began his experiments, and compare it with the state of the science when he left it; and then he will be able to judge of what Priestley did: without that comparison we cannot be aware of the extent of his labours. He gave increasing motion to the great machine of discovery, which has been working with additional rapidity ever since; and therefore it is that at this day his discoveries appear in some degree, I may say, perhaps trifling, till we recollect that they were the commencement of the career. But why should I say this, when I reflect that he made such valuable discoveries with respect to oxygen gas; that he was the first person who was fully aware of the fact, that the respiration of animals and the combustion of inflammable matter were the effect of air; that he was the first person who procured, in a separate state, the muriatic acid; that he was the first person who applied electricity in operations with the gases; the first person who invented a method of transferring gases. And when I reflect also that he discovered the effect of air upon vegetation, the effect of air upon the blood and upon vitality, I must say, that the experiments which at this day we look upon as common, would be wonderful and astonishing forty years ago, when they were made by a man who began life without fortune, who taught himself philosophy, and who commenced his discoveries with only a few tumblers and glasses.

Allow me to make one further observation. It must give us all great pleasure to know, that not only in England but on the Continent also are his merits appreciated. About two years ago I had the pleasure and satisfaction of hearing the lamented Cuvier lecture in Paris; and I am sure no person was more sensible than he was of the value of Dr. Priestley's discoveries. I was astonished to find how fully aware he seemed to be of all he had done; and I was deeply gratified to hear this master in science give to Priestley that praise which the most zealous countryman of his own would have awarded him.

The assembly having testified their interest in the prosperity of the Royal Institution, connecting with it the name of Michael Faraday, Esq., F.R.S., the following address was delivered by that distinguished cultivator of science :—

I know no reason why I should be distinguished with this peculiar mark of your favour—a favour so distinct in its character that no one present can mistake it; for I presume that we have met this evening to honour chemistry as personified by Dr. Priestley. I know no reason why I am selected, except that of the absence of my superior, Mr. Brande, who is not here to represent the Royal Institution. I will, therefore, do my best to thank you for the honour, and say, that as far as we are concerned, the Royal Institution shall endeavour to follow in the path trod by Dr. Priestley, namely, by experiments to advance science and the knowledge of truth.

I think I may be allowed, on this occasion of meeting to do honour and service to chemistry, to quote Dr. Priestley as an example to be followed in one very important point,—I mean in that freedom of mind, and in that independence of dogma and of preconceived notions, by which men are so often bowed down and carried forward from fallacy to fallacy, their eyes not being opened to see what that fallacy is. Dr. Priestley was a most remarkable man for the facility with which he could change his views as he saw nature change before him: and it is delightful to observe in his various papers, especially in the prefatory part of that on the discovery of oxygen, how he points out, with the perfect ingenuousness of a high-minded man, the way in which philosophers should proceed. He begins by stating, for the encouragement of all young philosophers, how many discoveries in chemistry are made by what we call chance, that is to say, by the observation of facts which result from natural causes working before us; and how greatly these things are effectual in carrying forward science. And he remarks in a very curious way, but with great truth, that this would be seen very strikingly in those men who are most celebrated for their philosophical acumen, were they to write, not in the synthetical manner, but analytically and ingenuously. He goes on further to say in that paper, that it is wonderful how his mind was bowed down by preconceived notions, and how his eyes were shut at first against perceiving the truths which were attached to the discovery which he had made of oxygen gas. He says he lays all this to prejudice, which not only influences our judgement, but even the perceptions of our senses; for we receive maxims so strongly for granted, that when they are contradicted by facts we refuse to receive those contradictions; and the more acute a man is, the more strongly is he bound by the chains of error; for he only uses his ingenuity to falsify the truth which lies before him.

I am very anxious at this time to exhort you all,—as I trust you all are pursuers of science,—to attend to these things; for Dr. Priestley made his great discoveries mainly in consequence of his having a mind which could be easily moved from what it had held to the reception of new thoughts and notions; and I will venture to say that all his discoveries followed from the facility with which he could leave a preconceived idea.

Men have degraded Dr. Priestley, or tended to do so, by saying that there was a sort of uncertainty and unsteadiness in his mind. I say not so; and I may affirm, without any derogation from Dr. Priestley's talents, that the very point on which his mind was most firmly fixed was that where he has the least credit,—the doctrine of Phlogiston. His constancy there was a constancy in error; and where he changed, it was generally to truth. By that spirit he discovered, and advanced upon his discoveries to new results; and instead of remaining fixed like a nail, he ran on before his time, and gained a character which now, a hundred years after his birth, you are met to celebrate and applaud. I hope we shall be led by his example, in like manner, to carry on our various pursuits, not confining ourselves to this very moment of time, but having our thoughts directed forward. And inasmuch as there is abundant discovery before us, so there is also the greatest reason for caution and sound judgement: there are as many theories and false views put forth now as there were in Priestley's day. I trust we shall all consider him as a man whose example we ought in that respect to follow, holding our minds in a state ready to receive at any time new impressions of truth, and not of necessity chaining them down to what we conceive to be truth at any instant. For my own part I must acknowledge that I cannot but attribute much of my late experimental success to an endeavour to follow the candid method of investigation pursued by Priestley, and to apply the principles of philosophical logic which I found in Sir John Herschel's "Preliminary Discourse."

Prosperity to the 'King's College,' and the Health of Professor Daniell, having been drunk, J. Guillemard, Esq. briefly returned thanks on the part of that Institution, in the absence of Mr. Daniell.

A corresponding tribute of respect having been offered to the University of London connected with the name of Dr. Turner, the Professor of Chemistry in that Establishment, Dr. Turner addressed the company as follows.

I rise to return thanks for the honour now done to the Friends and Professors of the University of London. I can assure the company, in the name of the Professors, that they are engaged in promoting the best interests of science and literature; I can assure them that they are heartily engaged in communicating sound instruction to youth.

It would ill become me, occupying the Chair of Chemistry in that Institution, were I to offer a silent return of thanks on this occasion. I rejoice to have an opportunity of expressing my admiration of the scientific character of Dr. Priestley. I rejoice to express thus publicly my gratitude to Priestley, the discoverer of oxygen,—to Priestley, the founder of pneumatic chemistry. His various merits have been proclaimed this evening; but I would say a few words more on one of them,—namely, his merit in the character I have last mentioned. I would not be understood as wishing to pass by the great merits of others, his fellow-labourers in the field of science at that time. I would not deprive of one tittle of their well-deserved fame, our countrymen Cavendish and Black: I would not pass by without due mention those splendid names, Lavoisier and Scheele. No, Sir; they have done their part; they made discoveries; they deduced accurate inferences from facts which they observed. But Priestley *invented* a new department of science. He taught us to confine, to manipulate with, to collect gases; he invented the pneumatic trough; he invented the mercurial bath; he gave us our instruments; and from his time pneumatic chemistry has been cultivated with increasing zeal and success. It is with this explanation that I consider him as the founder of pneumatic chemistry.

It is delightful to think that that low estimate of Dr. Priestley's scientific character which was once entertained,—so low, indeed, that it was said his discoveries were made by accident,—it is pleasing to think, that not only in this room, not only in this country, are his merits fully recognised; but if we go to rival nations,—rivals now only, I may say, in science,—if we go to France, we find there due honour done to the scientific merits of Priestley by one whose recent loss science is now deploring, by one, of whose great talents the whole community of science unite in testifying and recording their admiration,—I mean the lamented Cuvier. He has passed a just eulogy on the merits of Dr. Priestley, which will never be forgotten by this country or by the world.

The merits of Dr. Priestley, then, are duly recognised. But I cannot sit down without drawing the attention of this Meeting to one other point in the character of Dr. Priestley. Mr. Faraday has held him up as a pattern for that facility of change of opinion, that openness of mind, which every philosophic spirit ought to possess. To every one who cultivates science I would further hold him up as a pattern of philosophic liberality and candour. Dr. Priestley, when he had made a discovery, did not lock it up in his closet, in order that he alone might pursue the inquiries suggested by it, and deprive others of the merit of assisting in the research: he brought it out before the public, and made it the property of every one. This trait in the character of Dr. Priestley cannot be too much praised. Allied to this was the ingenuous feeling which led Dr. Priestley to make known the very mistakes which he committed; these he gloried in telling. "I could," says he, "have kept this blunder to myself, but I publish it to tell the world with what little skill persons may make discoveries."

The Philosophical Society of Birmingham, and its President, the Rev. John Corrie, having received an expression of respect from the Meeting, the subjoined observations were addressed to it by that gentleman.

I beg to thank you for the honour you have done the Society with which I have the pleasure of being connected. I am very sensible that we owe that honour solely to our being established in the town which was the residence of the great man the centenary of whose birth we are this evening met to celebrate. I am afraid we cannot claim him for our founder; but I have no doubt that the Institution owes its origin to that attention to scientific pursuits which his discoveries produced among our townsmen.

We have heard most interesting accounts of the merits of Dr. Priestley: I will not call them eulogy,—simple truth is eulogy enough when spoken of such a man. Will you allow me to deviate a little from the chemical view of the subject? While Dr. Priestley resided near Birmingham, another very eminent man was resident there, the late Dr. Parr; and though there was a repulsion between those two great men on some subjects, yet the attractions of kindred genius overcame all those feelings, and they were friends and associates amid all the tumults of party. Dr. Parr stood forward as the advocate of Dr. Priestley when he thought he was unjustly treated; he stood forward to reprobate those who designed to defame the character of an injured man. I think I shall not displease this company if I repeat a very few sentences from an eulogy which Dr. Parr published in the year 1792 on the character of Dr. Priestley. After detailing the modes of attacking Dr. Priestley's principles, which had been adopted by his opponents, he points out what would have been justifiable modes of attack; and he concludes with saying, "Let no one depreciate the acquirements of Dr. Priestley;—they are numerous, and almost without parallel. Let no one ridicule his talents, for they are superlatively great. Let no one vilify his merits, for they are correct without austerity, and exemplary without ostentation, because they present to a common observer the innocence of the hermit and the simplicity of the patriarch; and on the basis of philosophic truth you can at once discern the solid edifice of virtuous character." This eulogy is marked with the peculiar characteristics of Dr. Parr's style, and many persons may admire it on that account; but all will allow that it was deservedly merited by the intellectual and moral qualities of Dr. Priestley.

There was another and a solemn occasion on which Dr. Parr came forward to honour the character and the memory of Dr. Priestley: he composed the inscription which is placed on his tomb, and it must gratify the feelings of Dr. Priestley's friends, and afford great satisfaction to his admirers, that this is the testimony of a man who was fully capable of appreciating Dr. Priestley's talents, and who at the same time was the most unexceptionable judge of his merits.

"Prosperity to the Society of Arts, and the health of Arthur Aikin, Esq." having been proposed, Mr. Aikin addressed the Meeting to the following effect.

I can assure you that it was quite unexpected by me to receive the honour which you have done me in drinking my health in connexion with the Society of Arts. The only connexion which I can claim with the business of this day is the circumstance of my having been a pupil of Dr. Priestley, whose illustrious character and memory we are now met to celebrate. I consider it to be one of the most fortunate circumstances of my life that I was a pupil of that eminent man; and indeed I may say that my father before me was also a pupil of his. It is rather a remarkable circumstance that there are two gentlemen present, Dr. Bostock and myself, who were pupils in the same class, and that our fathers were pupils likewise. I trust we have benefited by the instructions we received from that very eminent teacher of experimental philosophy. I can truly say, the circumstance of my life which more than any other I have the pleasure of looking back upon was the great satisfaction I had in being employed in assisting Dr. Priestley in his laboratory when he removed from Birmingham to Hackney.

The name of JOHN TAYLOR, Esq., having been similarly associated with that of the Smeatonian Society of Civil Engineers, the assembly was addressed as follows by that gentleman:—

In noticing the Society with which you have done me the honour of coupling my name, I shall have to introduce to your notice the names of many of the associates of Dr. Priestley. The Society of Civil Engineers, which bears the name of Smeaton, was founded at a time when general improvement was rapidly extending. Soon after the year 1760, when canals began to spread over the kingdom, and when many useful arts were put into motion, Smeaton, the great founder of that Society, proposed the formation of this association for discussing subjects of practical use and scientific interest. When I mention the names of some of the members of that Society, you will find there were many whose minds were congenial with the views which Dr. Priestley entertained. The Society originally included those of Smeaton, James Watt, Priestley, Whitehurst, Boulton, Rennie, Mylne, and Jessop: these were the professional engineers that were amongst the first members of the Society. To these were afterwards added, as honorary members, gentlemen who took an interest in the nature and objects of the Society,—Sir Joseph Banks, Captain Huddart, Dr. Hutton, Lord Morton, Ramsden, Troughton, and Sir George Shuckburgh Evelyn. This Society began with small means; and I may also notice that, with the exception of the Royal Society, it was the only one which Dr. Priestley belonged to in London. The evenings were spent in the discussion of some interesting philosophical questions. The Society has continued to enrol several distinguished names, such as Chapman, Mylne, Rennie, Walker, Jessop, Jardine, Stephenson, Giles, and Cubitt; and we have, as honorary members, Davies Gilbert, Troughton, Dollond, Watt, Ewart, Chantrey, Babbage, Colby, Baily, Beaufort, and Seppings, with other men well known to the scientific world. With such associates, and with a mind like Dr. Priestley's, which had a peculiar tendency to everything that was practically useful,—for I think that if we go through his most brilliant discoveries, we shall always find him taking plain and practical views, and inquiring to what useful end they might be employed for the good of mankind,—with such men, I say, it was no wonder that his attention was directed towards experimental philosophy. In the history of his life it is said, that coming to London, as was his custom, a month every year, his acquaintance with Dr. Watson, Dr. Price, Mr. Canton, and Dr. Franklin commenced about 1766; and then his first attempts at experimental philosophy began. I think, however, there is evidence of his attachment to experimental philosophy much earlier. I was looking this morning at a letter which Dr. Priestley wrote to my grandfather respecting two of his first pupils, my late revered uncle, and the father of my friend at my left hand, Dr. Rigby, and I find that he particularly mentions in that letter that out of school hours he directs the attention of his pupils to natural philosophy as an amusement, and that they have the use of his library and apparatus for this purpose, and were engaged in experiments. I also find that when it was proposed he should become a tutor at the Dissenting Academy at Warrington, the department assigned to him was that of languages: he then represented that he should have been more gratified to have undertaken that of natural philosophy. He however went to Warrington, and became Professor of languages; he taught Hebrew, Greek, Latin, French, and Italian; he lectured, I believe with distinction, on civil law: some time after that he contrived his Chart of Biography, and subsequently his Chart of History, which, I believe, are the types of everything of that kind which has since been produced. Dr. Priestley, besides all his other studies, was an astronomer, though we do not generally know him in that capacity. He was applied to to accompany Captain Cook as astronomer in his second voyage round the world; some objection was raised, however, and it was given to Dr. Foster, who was also a tutor at Warrington. On this occasion, Dr. Priestley, with his usual candour, says, "It is given to a man far better qualified than myself, as I know but little of natural history, though I think I could have worked myself up to the point had it been necessary."

With such men as Smeaton, and Watt, and Whitehurst, it was not unlikely that Dr. Priestley's attention should be turned to that line of science which he afterwards pursued with so much success. In his visits to London he became more acquainted with scientific men, and was excited to the pursuit of natural philosophy. His industry was most extraordinary; and, as an instance, I may mention that he proposed to Dr. Franklin to write a history of electricity. Dr. Franklin approved the plan, and offered to furnish the books necessary for the purpose. In less than twelve months he sent Dr. Franklin a printed copy of the work. He lectured during that year five hours a day; he made the experiments for that work, finished the drawings for it, and he apologizes in one of his letters for its being one of his most hasty productions. The work maintains, notwithstanding, a very high regard among those who follow that science.

Dr. Priestley had a particular aptitude in turning his knowledge to useful purposes. In executing this very work, he perceived that in the representation of apparatus all former delineations were uncouth and unpleasant to look at. He examined into the cause, found that the defect was in the perspective, and, setting to work, studied Dr. Brook Taylor on the subject. Now, we know what it is to wade through such an elaborate and abstract work; but Dr. Priestley applied himself to the task, learned the art, and applied it successfully; and, what is more, gave to the world the clearest treatise on perspective which has been seen to this day. It contains not only a perspicuous account of all that was known up to that time on the subject, but he actually gives a new method of delineating any object correctly with common instruments, and fully describes it in a few pages, so that no man can misunderstand it.

Dr. Priestley's attention to the useful application of knowledge may be traced in other instances. At Birmingham a process of gilding buttons was common which was exceedingly destructive to the health of the workmen; the process of amalgamation was gone through over a common fire; the atmosphere was thereby impregnated with mercurial fumes, and the consequence was that the lives of the people were shortened. Dr. Priestley saw at once that this might be altered, and invented a most simple plan for the purpose, by which not only the health of the workmen was ensured, but the quicksilver might be saved, and thus a great œconomy introduced into the manufacture. I saw an apparatus of this description which was erected under the direction of Dr. Priestley himself, and so completely has it answered, that the manufacturers have generally adopted it.

There was another instance in which he regarded the useful application of scientific discovery. He discovered that water might be artificially impregnated with carbonic acid gas. He thought this might probably be beneficial, particularly in long voyages, as a remedy for the sea-scurvy, a disease then much felt. He instantly stated his views to the Lords of the Admiralty; they referred him to the College of Physicians, who gave attention to the subject, and encouraged Dr. Priestley to proceed, to determine the best process for the purpose. The result was highly favourable; and then, as was his uniform practice, he communicated to the world all he knew. He therefore published, in a shilling pamphlet, the result of his experiments and discoveries in fixed air, and described the mode in which it might be thus employed.

In his preface to this little book, he says, "If this discovery (though it doth not deserve that name,) be of any use to my countrymen and to mankind at large, I shall have my reward. For this purpose I have made the communication as early as I conveniently could since the latest improvements I have made in the process, and I cannot help expressing my wishes that all persons who discover anything that promises to be generally useful would adopt the same method." He does not arrogate any merit to himself for this discovery; he attributes to Dr. Black the investigation of substances containing fixed air; to Sir John Pringle the observation that putrefaction was checked by fermentation; to Dr. Macbride the discovery that the effect thus produced was by the fixed air generated in the process: he attributes to Dr. Brownrigg the discovery that Pyrmont and other mineral waters contain this air; and to Dr. Percival some experiments on its medical uses. There is nothing that is contained in Dr. Priestley's book which he claims as a discovery, except that, by accident, he says, he found that water might be impregnated with carbonic acid gas: he supposes this may be useful, and therefore he gives freely to the public the result of his inquiries.

Perhaps, after all, one of the most extraordinary features in the character of Dr. Priestley was the unexampled industry with which he pursued the numerous subjects that came under his observation. It is really surprising that a man who was engaged in deep read theological and metaphysical research, who taught languages, who gave instruction in civil law, who illustrated perspective and other useful arts, and attended to astronomy, should be able to do all this, and conduct the numerous experiments which were requisite during the progress of his History of Electricity and that of the Discoveries relating to Vision, Light, and Colours, both being elaborate works, requiring immense labour and diligence. Add to these the subjects which have been so well noticed by those who have preceded me at this meeting, all pursued with a truly philosophic spirit, and with a single-minded purpose of being useful to the world; and we must consider it as a noble example of industry, and one which is calculated to inspire emulation in those who engage in similar pursuits, and to excite respect and gratitude for the memory of one who so ably led the way in the field of discovery and usefulness.

Dr. Roget, Sec. R.S., on the part of the Zoological Society and himself, delivered the following address:—

As you have done me the great honour of mentioning my name in connexion with the Zoological Society, of which, I must confess, I am a very unworthy member, I beg leave to return, in the name of that Society, our most grateful acknowledgments for the compliment you have paid us. It is quite unnecessary to adduce any argument to prove that the foundation of all zoological science must be laid in correct views of the physiology of animals. As cultivators of that science, we certainly owe a large tribute of gratitude and respect to the memory of Dr. Priestley, whose discoveries have contributed so largely to elucidate one of the most important of the animal functions, namely, that of respiration. It must be acknowledged by every person who has attended to the history of physiology, that, previous to his time, physiologists were in a state of the most profound ignorance with regard to the real nature and objects of that function: the only notions entertained were those of accounting for the phenomena on mechanical principles; and, if we except Mayow, no person had an idea that they were of a chemical nature; but Dr. Priestley, by the discovery of oxygen, and more particularly of its disappearance during its passage through the lungs, and of the evolution of another gas, threw greater light on the theory of that function than it had ever received before, and cleared away the obscurities which before prevailed. And it is very remarkable that the results which he attained, instead of being invalidated by subsequent experimenters, have been corroborated by every person who has examined into the subject. I may appeal to a gentleman whom I have the pleasure of seeing at this table on the present occasion, Mr. Pepys, who has contributed very largely to the knowledge we at present possess on this subject; and also to Dr. Bostock, who has also given his attention to that function:—I am sure they are ready to confirm what I have now stated. I think, therefore, that the discoveries of Dr. Priestley on the subject of respiration may be regarded as among the happiest efforts of his penetrating genius, inasmuch as they have contributed to establish and fix correct views of that function. Every cultivator of natural history must feel that Dr. Priestley is entitled to his warmest gratitude; and when we consider the great multitude of subjects connected with science which have derived advantage from the labours of Dr. Priestley, I think we may consider his name as reflecting honour upon our country, as holding out a bright example for the imitation of posterity, and as fully justifying the enthusiasm which I rejoice to see animates this assembly.

"Prosperity to the British Museum" having been associated with the health of J. G. Children, Esq., Honorary Secretary to the Committee by whom the preliminary measures for the Commemoration had been conducted, Mr. Children addressed the meeting nearly in the following words:—

I am not exactly aware what immediate connexion there is between the British Museum and Dr. Priestley, except that his volumes form one of the greatest ornaments of our library. I beg leave, however, to return my thanks for the honour you have done the Trustees and Officers of that establishment, and for the compliment you have paid to me, as Honorary Secretary on the present gratifying occasion. The labours of that office, which I am highly delighted in filling, have, through the kindness of many gentlemen interested in this meeting, (especially Mr. Taylor,) been of the lightest kind; but were it possible that such duties could have been irksome, your approbation would be ample compensation. But I must not omit this opportunity of paying my tribute of respect to the memory of the Philosopher, the hundredth anniversary of whose birth we are this day assembled to celebrate. My honourable friends who have already addressed you, have alluded with equal ability and eloquence to a variety of subjects to which the labours of Dr. Priestley were directed. I beg leave to call your attention to those which he devoted to electricity. A science so abounding in brilliant phænomena could not fail to excite the strongest emotions in a mind like that of Priestley: who not only fully appreciated the importance of the discoveries already made, but with an almost prophetic spirit seems to have anticipated still more important results reserved for a future period. I will not undertake to quote his exact words,—but, in the History of Electricity which has been justly eulogized by Mr. Taylor,—and it is impossible to conceive a more valuable history, one more perfect in its arrangement, more minute, and yet not tedious,—in that work he says, "By pursuing this new light, the bounds of natural science may possibly be extended beyond what we can now form an idea of: new worlds may open to our view, and the discoveries of Sir Isaac Newton himself and all his cotemporaries be eclipsed by the labours of a new set of philosophers in this new field of speculation." And he adds a little further: "What delight would it afford to a modern electrician, to exhibit, were the thing possible, some of his principal experiments to Sir Isaac Newton!" And here I may be allowed to ask, If in the estimation of Dr. Priestley, it would have excited admiration in the mind of Newton to witness the powers of the Leyden phial, or the repetition of Franklin's daring experiment, what would he himself feel at the progress which his darling science has made in later times? With what ardour would he behold the electric fluid deprived of its fugacious and impetuous character, and rendered the obedient servant of our will by the sagacity of a Volta! How would he rejoice to see the strong analogies that exist between Electricity and Magnetism, extended and confirmed by the discoveries of Œrsted!—and, finally, if not their *identity*, yet (as has been well expressed in a very recent work,) that they are merely "different aspects of the same agent," proved, beyond dispute, by the incomparable researches of our own highly gifted countryman [Mr. Faraday], the second "child and champion" of that noble Institution in whose bosom and at whose hand a Davy found that protection and assistance which enabled him to triumph over the most energetic attractions of matter, to disarm the fire-damp of its terrors; and, in a word, to raise an imperishable monument to his own and his country's glory! But to return to Priestley:—I shall not enter into a detail of all that he accomplished in the science; but two happy applications of electrical agency to chemical research I must not pass over in silence; since, though Priestley did not himself pursue the path he marked out to its full extent, it conducted others to results of primary importance. I allude to his discoveries of the formation of an acid, when electric sparks are taken for some time in confined portions of common air; and of the great increase of volume which ammoniacal gas experiences when similarly acted on. The first fact led Mr. Cavendish, as you know, to the successful investigation of the composition of nitric acid; and by little more than the mere repetition of the second experiment, Berthollet effected the analysis of ammonia. It would be impertinent in me to think of detaining you longer; for what more could I say of Dr. Priestley that you do not already know and acknowledge? My feeble eulogy can add nothing to his fame; it is as immortal as his labours were multifarious: and to him, *mutatis mutandis*, may be applied what was said of a celebrated literary character, "*Nullum ferè scientiæ genus non tetigit, nullum quod tetigit non ornavit.*"

The name of W. H. Pepys, Esq. was next associated with the toast "Prosperity to the London Institution," when the company were thus addressed by that gentleman:

I return you my most grateful thanks for the honour you have conferred upon me. I feel it as a mark of your esteem, which I shall remember as long as my days continue. The companion and friend of Davy, with whom I have passed many cheerful and happy days, I stand before you as a lover of chemistry, with a desire of promoting that science by every means within my humble power.

In reverting to Dr. Priestley, allow me to say that it was he who, in the early period of my life, brought me to consider the nature and the properties of the gases. It was from him, from reading his books, that I first formed the apparatus and the arrangement of my own laboratory. I was delighted with the simplicity of his apparatus, because I found in it so much facility and assistance. If I wanted hydrogen, or oxygen, or any one of the forms of gas that are useful in the laboratory, by Dr. Priestley's apparatus I was put in possession of it. It will always occur to any mind that loves science that certain improvements may be brought forward in arrangement; but the simplicity of Dr. Priestley's laboratory engaged the attention of all. His pneumatic trough, his arrangements for the preparation of every gas that I could speak of, were simplicity itself. It is true there may be additions made to the comforts of a laboratory, but, at the same time, the foundation was laid by Priestley; and I always think of his experiments with pleasure. As science advances, different improvements may take place: man is not contented with any operations, or any experimental researches, unless he attains to some sort of perfection in them. If we look with discerning eyes on the transactions of society, everything shows us that man is an improving being;—that he is always going on from one thing to another, in the discovery of what is useful and necessary. Man is one of the humble instruments in the hands of Divine Providence for showing forth the wonders of His works; for, after all, what are we and the best results of our researches intended for, but to bring out His great truths, and show His power, and His ascendancy?—and he, perhaps, who is the best observer is the best worshipper. I return you thanks for the honour you have shown me in coupling my name with that of the London Institution. I certainly did take a very active part in the formation of that useful establishment; perhaps no man ever gave more of his attention to any object than I did to that; and I trust I shall always feel a pleasure in doing what may be within my power to forward the interests of science.

The meeting was subsequently addressed by the President, to the following effect:

It must be to all of us a source of gratification that we have on the present occasion the company of the grandson of the distinguished individual on whose account we have this day assembled. The feelings of this meeting with respect to his grandfather, his descendant will no doubt be exceedingly pleased to witness. I have no doubt, therefore, that you will cordially unite with me, if it were on no other account than with reference to his worthy grandfather, in drinking with me his health and prosperity. I therefore propose the health of Mr. John Finch.

Mr. Finch returned thanks in the following words:

Gentlemen, I can truly say that this is the happiest day of my life; for I have now heard commemorated, in the addresses which have been made to you, the merits of my grandfather. I cordially tender to you my thanks for the honour you have conferred upon his memory. His career was, in some respects, that of every person connected with science. In his youth he had to struggle with many difficulties. In his advanced age his love for science was more admired, for science itself was more appreciated. In the United States of America I have seen the mansion in which his life rolled peacefully on. There he would have been perfectly happy, if he could have assembled around him some of the friends of the former years of his life, whose society would have delighted him. If I might be permitted, without presuming too much on the indugence of the meeting, to propose a toast, I would give that of one of his first friends. I am sure that, in an assembly of Englishmen, the name of Franklin cannot be mentioned without his being regarded as one of the most distinguished discoverers in science.

"The Memory of Dr. Franklin."

The meeting was addressed, in conclusion of the Proceedings, by J. A. Paris, M.D. F.R.S.

After what you have heard this evening, and which has thrown a delightful degree of interest around the meeting, I should not have risen did I not feel that you have yet an important duty to perform, which is to drink the health of our worthy friend the President.

Allow me to make a few observations on the object of this festival. It has been said that a man is no prophet in his own country. I will go further, and say that a man is no philosopher in his own time, and for this obvious reason—that it is the character of a genius to anticipate the age in which he lives. He sees by a species of mental refraction the great light of truth, which is as yet below the horizon. We can revert to the works of our departed philosopher for an illustration; since it is only lately that we have been able to appreciate the utility of those applications to which his discoveries have been subservient. This leads me to the relation of one simple fact. If gentlemen will refer to the first Bakerian lecture delivered by Sir Humphry Davy, they will see that, without the assistance of Dr. Priestley, he could not have arrived, at least so speedily, at those results which have crowned his memory with lasting honours. At this late period of the evening I will not enter into any chemical discussion, but will merely state to what I refer. It was well known that water under the influence of voltaic action gives out indications of alkaline and acid matter. It was obvious to Sir Humphry Davy, that the presence of this alkaline and acid matter must have been derived from some foreign ingredient, although Sylvester assumed that the matters in question were actually generated. By a series of experiments, unequalled in beauty, Sir Humphry Davy convinced himself that some foreign matter must have interfered; but he was at a loss to understand how the production of alkaline matter at a certain stage of the process was stationary, while that of the acid matter was progressive: and it was only by reference to the experiments of Dr. Priestley that he was enabled to clear up this doubt and difficulty.

I now venture to revert to the object I had in view when I rose, which is, to propose that we drink the health of our friend Dr. Babington. Let me observe, that in proposing his health as Chairman of this Meeting, you will do him very feeble praise if you regard merely the situation in which he now appears. Dr. Babington has been the friend, the bosom friend of those philosophers whose memory we have now commemorated. It is more than half a century since Dr. Babington ceased to lecture as a chemist. He was the first person in this country who excited anything like a love for mineralogy. He purchased a large collection of minerals, and published the first systematic catalogue that ever appeared in this country. It was in his house that the meetings of those gentlemen who afterwards formed the Geological Society took place. Dr. Babington has been the intimate friend of all those gentlemen whose memories we respect as scientific men: and if there be any truth in the observation that those asperities, those "*animis cœlestibus iræ,*" which are too often generated in the breasts of contemporary philosophers, be assuaged and softened down by intercourse with gentler spirits, I ask those gentlemen, who from experience are well able to judge, where these philosophers could have found another person like Dr. Babington for their associate? where could they have found a kinder or a more faithful Atticus? I have thought it necessary to say thus much in proposing his health; had he been absent, I should have said more.

The health of the President being drunk accordingly, that gentleman then terminated the proceedings of "the Commemoration of the Centenary of the Birth of Priestley, as the Founder of Pneumatic Chemistry," by the following expression of his thanks:

Gentlemen, I feel myself quite unable to offer anything in the way of return for the very handsome manner in which Dr. Paris has thought fit to express himself; I therefore content myself with offering you my best acknowledgments on the present occasion. I hope you will recollect what I said at the beginning, that I have not filled this situation in conformity either with my inclination or my judgement. I beg you will have the kindness to receive my warmest thanks for your assistance this evening. I am glad to perceive the feeling that has animated the Meeting, and that we have been so much gratified with what we have heard.

LXVI. *Intelligence and Miscellaneous Articles.*

EXPERIMENTS ON MINIUM. BY M. DUMAS.

M. DUMAS submitted massicot to 24 hours heat in a reverberatory furnace, by which it acquired 1·17 per cent. of oxygen; after a second exposure to heat for the same length of time, the oxygen added amounted to 1·22 per cent.; and after a third operation the addition was 1·36. The colour of these miniums was as fine as that of those obtained by a much longer exposure to heat. By exposure to 24 hours longer heat, the amount of oxygen gained was 1·5 per cent., by a fifth 1·55, and after eight days heating, the total amount acquired during the conversion of the massicot into minium was 1·75 per cent. M. Dumas remarks that the extreme slowness with which massicot acquires oxygen, appears to depend upon its physical properties; for when white lead is calcined in the same manner, the finest orange minium is obtained by three operations, and it acquires 2·23 per cent. of oxygen.

M. Dumas observes that in no one of these cases has the massicot in becoming minium combined with sufficient oxygen to convert it into a sesqui-oxide, which it is well known the common red lead is usually considered to be; for in that case it would give out 3·33 per cent. oxygen instead of only 2·23; when oxygen gas was passed over it, 2·4 per cent. was the largest quantity of oxygen absorbed. This minium M. Dumas found to consist of about 64·9 of protoxide and 35·1 of peroxide of lead, or two atoms of the former and one atom of the latter; and this he considers to be the essential composition of the miniums of commerce, mixed, as his experiments show, with variable quantities of yellow oxide.—*Ann. de Chim. et de Phys.*, tom. xlix. p. 398.

PREPARATION OF PEROXIDE OF HYDROGEN. BY M. THENARD.

Those chemists who have prepared peroxide of hydrogen or oxygenated water, know that the peroxide of barium which they employ always contains oxide of manganese; that this oxide comes from the porcelain retorts in which the nitrate of barytes is calcined, and that it occasions the decomposition of a large quantity of oxygenated water, at the moment of precipitating the solution. This decomposition increases the difficulty of the preparation; it is in fact the only real one. I attempted to neutralize the effects of the oxide of manganese, and accomplished it by the addition of a little phosphoric acid; phosphate of manganese is then formed, which does not act at all in decomposing the oxygenated water.

Having then dissolved the peroxide of barium in the muriatic acid, and obtained a liquor sufficiently charged with oxygen, 2, or at most 3 parts of concentrated phosphoric acid must be added for every 100 parts of oxide; the acid is to be supersaturated by hydrated and divided peroxide, which will precipitate the silica and alumina, the iron and oxide of manganese, the two latter in the state of phosphates. These four substances become immediately deposited in the state of flocks, and are to be collected on a cloth; the liquor will readily pass through, and may be filtered as wanted.

In order that the operation may succeed well, it is requisite that the saturation by means of peroxide of barium should be attempted only with a tolerably clear solution. The presence of much sulphate of barytes would be an obstacle to collecting the precipitate, and to the filtration through linen.

When the liquor is carried to such a point as to contain only water, peroxide of hydrogen and muriate of barytes, sulphate of silver is to be added in powder; this is to be very slightly acid, and may be obtained by calcining pure nitrate of silver with sulphuric acid in a platina crucible. The whole is to be stirred with a glass rod, and when the muriate of barytes is quite or nearly decomposed, it will be known by the liquor becoming clear; then, in order to attain the point of perfect decomposition, small quantities of a very dilute solution of muriate of barytes, or of sulphate of silver, are to be added, as either salt may be in excess. In this, as in the preceding case, the liquor ought to be again passed through linen, to separate the precipitated matters. It is certainly possible to obtain the liquor perfectly neutral, but it is better that the sulphuric acid should be slightly in excess; a little barytes water will afterwards precipitate it, and all chance of decomposing the peroxide of hydrogen will be avoided.

Lastly, having filtered the liquor through paper, it is to be concentrated as usual under the receiver of the air-pump. There is no difficulty in charging it very quickly with 60 to 80 times its volume of oxygen by the processes which have been described, before any concentration; only instead of dissolving the peroxide in a glass vessel, it is much more convenient to do it in a platina or silver vessel surrounded with ice, and to rub continually with a pestle the hydrated and divided peroxide.—*Ann. de Chim. et de Phys.*, tom. l. p. 80.

COMPOSITION OF CAFFEIN.

MM. Pfaff and Liebig have analysed caffein, and find it to be composed of, taking the mean of two experiments,

Carbon	49·86
Hydrogen	5·32
Azote	29·03
Oxygen	15·80
	100·01

which they consider as equivalent to

Four atoms carbon	3·05750 or	49·79
Five atoms hydrogen	0·31199	5·08
Two atoms azote	1·77036	28·83
One atom oxygen	1·00000	16·30
	6·13985	100·00

At the request of MM. Pfaff and Liebig, M. Wohler also analysed caffein, and with results almost precisely similar to those above stated. —*Ann. de Chim. et de Phys.*, tom. xlix. p. 303.

To the analysis, the authors have appended the following observations, which constitute a fine example of the confusion which chemistry is doomed to suffer from the wild theories and speculations which are now so rapidly rising into fashion:—

"According to its theoretic composition, caffein may be regarded as a combination of a cyanic acid, which contains one half less oxygen than the common acid, with æther analogous to cyanic æther. An æther formed of a problematical cyanous acid would be composed of, $Cy^2 \frac{1}{2} O + (C^2 H^4 + \frac{1}{2} O H^2) = C^4 H^5 N^2 O$; this formula is the same as that of caffein." R. P.

ANALYSIS OF THE SULPHO-PLUMBIFEROUS TELLURIUM.

M. Berthier remarks that the only ore of Tellurium which can be procured in Paris in sufficient quantity for the preparation of tellurium, is of the variety called in collections *Blatter-erz* and Auro-plumbiferous tellurium; but it is not the same as that analysed by Klaproth, and ought to form a peculiar species.

This mineral comes from Nagiag; it is in curvilinear and intercrossing laminæ, disseminated in crystallized rose carbonate of manganese and white quartz; its colour is iron black, approaching lead gray; it is shining. When it contains no gangue its specific gravity is 6·84. It gave by analysis,

Gold	6·7	or	Telluretted gold	19·7
Tellurium	13·0		Sulphuret of lead	72·9
Lead	63·1		Sulphuret of antimony	6·2
Antimony	4·5		Sulphuret of copper	1·2
Copper	1·0			
Sulphur	11·7			100·0
	100·0			

Ann. de Chim. et de Phys., tom. li. p. 150.

ACTION OF CHLORINE UPON GUM.

M. Simonin caused chlorine to act upon gum, and obtained results which differ from those described by M. Guerin. Four ounces of white gum Senegal were dissolved in about a quart of water, and the solution was put into a long-necked flask; in twenty-four hours more than 12000 cubic inches of chlorine gas were passed through it; the solution became gradually colourless, was afterwards turbid, and had an opaline tint, which it retained to the end of the operation. After repeated operations, and the separation of the muriatic acid formed, M. Simonin obtained an acid which he thinks was somewhat analogous to that obtained by M. Guerin from the action of nitric acid upon gum; and sugar yielded when treated in the same manner a similar acid.—*Ann. de Chim.* tom. l. p. 319.

MR. B. BEVAN ON COVENT GARDEN MEASURES.

Leighton, Jan. 15, 1833.

To the Editors of the Phil. Mag. and Journal of Science.

Gentlemen,

Expecting that one of your numerous contributors residing in London would have favoured the public, through the medium of your Magazine, with a reply to my inquiries respecting the measures used at Covent Garden Market, I deferred sending the result of my observations on that subject. But no person appears to have considered it of sufficient importance, although these measures are in very extensive daily use, and though almost unknown in the country, are referred to in most of the reports on the prices of articles sold at that market, both in the public newspapers and in the Gardeners' Magazine.

I therefore hope the subjoined statement of these measures will be acceptable, at least as approximations, until some person will take the trouble to determine them more correctly. This statement may serve in some degree as a guide to country gardeners, and enable them to compare the prices of articles in the country with those at Covent Garden, and to judge whether they can profitably contribute to its supply. The mode I adopted to ascertain the capacities of these measures, was that of purchasing a new set of them, of one of the most respectable vendors in the market, and from their dimensions calculating their capacities, and also by actually filling each of them properly *heaped.* I am aware that baskets formed of osiers, as these measures usually are, cannot be expected to prove very accurate; and had I been in London, I should have obtained an average of nine or ten instead of being guided by a single set.

I find there are four varieties of *Punnets,* which still leaves a source of uncertainty, unless in the report of prices the particular variety was indicated.

From my experiments, the *greatest* capacity

of the sieve	=	1644 cubic inches.
Half ditto	=	822
Quarter ditto	=	362
Largest punnet ..	=	248
Second ditto	=	228
Third ditto	=	90
Least ditto......	=	60

But as in practice they may not be filled to the maximum, we may infer, that, relative to a bushel, the proportion will stand as follows:—

2	sieves	=	1 bushel
4	half ditto........	=	1 peck.
8	quarter ditto	=	1 gallon.
12	large punnets.		
16	second ditto	=	1 pottle.
32	third ditto	=	1 quart.
48	small ditto.......	=	1 pint.

On looking over the list of articles quoted in the reports, I find several are sold by the *bunch.* The better way of making known the usual quantity which constitutes a bunch, will be that of giving the average weight of each, or when they are small the weight of a dozen. Information of this specific nature cannot fail of being duly estimated by many of your readers. Yours, &c. B. BEVAN.

The extract given in a late Number, from the Chronicles of Old London Bridge, respecting the improved quality of iron from exposure to rust or oxidation (p. 75), will in some measure confirm an opinion that prevailed in Bedfordshire near fifty years ago. I recollect the observations of a smith at that period, that nothing made so good a knife as a piece of steel which had been rusty; and it was generally asserted that a knife which had been long exposed to rust, proved always a good cutter, whatever might have been its quality originally.

I have now in common use a razor, which about forty-five years ago was rejected after many trials, and thrown amongst a heap of old iron as worthless. After lying in this place for some years, and becoming very rusty, I gave it a trial out of curiosity, and found it prove one of the best razors of my set.

I have also heard from a person much acquainted with the iron trade, that occasionally a quantity has been manufactured, which from some unknown cause turned out so bad as to be unsaleable, but which, after lying for a year or two in the stores, and becoming quite rusty, has proved of excellent quality.

I have also been informed by a person who deals in scythes, that the hardest tempered are preferred, which are exposed for ten or eleven months to all weather, upon the thatch of some building, until they become rusty;—after this they generally prove of excellent quality. B. B.

ELECTION OF MR. R. BROWN AS A FOREIGN MEMBER OF THE ROYAL ACADEMY OF SCIENCES OF PARIS.

The Royal Academy of Sciences of Paris, on the 4th of March, proceeded to the election of a Foreign Member, in the room of the late anatomist Scarpa.

The candidates were—

Bessel and Jacobi of Kœnigsberg; Robert Brown and Michael Faraday of London; John Herschel of Slough; Leopold de Buch and Mitscherlich of Berlin; Meckel of Halle; Oersted of Copenhagen; Plana of Turin.

The number of members voting was 47.

Robert Brown had 29 votes; Bessel 7; Oersted 7; Mitscherlich 2; Meckel 1; Herschel 1.—*Le Temps*, March 6, 1833.

LUNAR OCCULTATIONS FOR JUNE.

Occultations of fixed Stars by the Moon, visible at Greenwich in the Year 1833. *Computed by* THOMAS MACLEAR, *Esq.; and circulated by the Astronomical Society.*

1833.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions.				Emersions.			
				Sidereal time.	Mean time.	Angle from North Point.	Angle from Vertex.	Sidereal time.	Mean time.	Angle from North Point.	Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
June 5	[1324] Sagit'	6	2345	under	the ho	rizon		15 54*	10 58	239	211
10	30 *r* Pisciu.	4·5	2870	under	the ho	rizon		18 26*	13 10	274	235
	33 *s* Pisciu.	5	2877	19 22	14 5	127	91	20 31	15 14	281	250
11	20 *m* Ceti ..	5	86	21 0	15 39	43	10	21 24	16 3	6	335
13	73 ξ^2 Ceti. .	5	255	21 31	16 2	79	40	22 29	17 0	317	281
20	(180) Canc.	7	1081	13 33	7 38	49	91	14 23	8 28	279	320
25	80 *b* Virgin.	6	1551	18 39	12 23	56	95	under	the hor.		
29	(214) Scorp.	6·7	1924	12 26	5 56	58	28	13 34	7 3	262	239
30	(312) Sagitt.	6	2063	20 51	14 15	85	110	under	the hor.		
July 1	(225) Sagitt.	6	2183	20 54	14 14	94	114	22 7	15 27	258	284

* Rising at emersion.

Penzance, and Mr. VEALL *at Boston.*

Days of Month, 1833.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Penzance. Max.	Barometer. Penzance. Min.	Barometer. Boston $8\frac{1}{2}$ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Penzance. Max.	Thermometer. Penzance. Min.	Thermometer. Bost. $8\frac{1}{2}$ A.M.	Wind. Lond.	Wind. Penz.	Wind. Bost.	Rain. Lond.	Rain. Penz.	Rain. Bost.
March 1	29·480	29·029	29·584	29·196	28·50	50	33	48	38	39	W.	NW.	W.	...	0·220	0·12
2	29·874	29·544	29·778	29·684	29·25	51	46	54	40	41	W.	SW.	W.	...	...	·28
3	29·714	29·444	29·578	29·542	29·38	54	44	52	47	47	SW.	SW.	calm	...	·250	·09
4	29·903	29·717	29·772	29·573	29·35	57	32	49	45	46	SE.	NE.	calm	...	·210	...
5	30·132	30·027	30·078	30·054	29·56	53	38	50	43	47·5	N.	W.	NW.	...	...	...
○ 6	30·245	30·144	30·328	30·220	29·66	44	35	47	41	42	N.	NE.	NW.	...	·240	...
7	30·351	30·309	30·334	30·128	29·86	45	28	48	41	43	N.	SW.	calm	...	·350	·21
8	30·332	30·310	30·181	30·034	29·93	39	29	44	42	34·5	N.	SE.	E.	...	1·040	...
9	30·203	30·107	30·202	30·066	29·82	40	32	43	31	33	N.	NE.	calm	..	...	...
10	30·049	29·986	30·072	29·996	29·71	40	32	42	32	38	NE.	NE.	E.	...	...	...
11	30·149	30·086	30·066	30·002	29·70	41	29	44	32	35	NE.	NE.	NE.	0·01	...	...
12	30·104	30·001	30·072	29·996	29·73	41	24	41	31	34	NE.	NE.	calm	...	...	...
☾ 13	29·821	29·508	29·855	29·332	29·40	42	23	45	33	35	E.	S.	calm	...	0·200	...
14	29·411	29·354	29·202	29·140	29·12	44	28	42	35	34	E.	SE.	calm	...	2·010	...
15	29·488	29·430	29·208	29·202	29·20	45	35	42	32	36·5	E.	E.	calm	...	0·185	...
16	29·534	29·518	29·390	29·252	29·25	48	39	48	34	41	E.	NE.	E.	...	...	...
17	29·635	29·561	29·594	29·432	29·30	40	37	47	40	40	NE.	NE.	E.	·36	·250	...
18	29·844	29·751	29·798	29·693	29·44	42	35	44	38	40	NE.	NE.	E.	·03	...	·31
19	30·151	30·007	30·134	29·990	29·60	45	25	44	37	41	NE.	NE.	NE.	...	...	...
20	30·185	30·042	30·196	30·134	29·73	45	31	46	40	36	NW.	NE.	N.	...	·100	...
● 21	29·942	29·844	30·040	29·984	29·55	45	32	46	38	35	N.	NE.	N.	...	...	...
22	29 956	29·950	29·990	29·964	29·58	45	25	46	34	36	NW.	N.	calm	·18	...	...
23	29·957	29·917	29·976	29·958	29·64	39	27	46	37	36	NE.	NE.	calm	...	...	...
24	29·989	29·837	29·976	29·940	29·64	43	34	46	32	37·5	NE.	NE.	calm	·04	...	·58
25	29·874	29·793	29·946	29·940	29·47	44	32	47	35	38	NE.	NE.	E.	·34	·060	·29
26	30·039	29·873	29·952	29·940	29·49	44	31	46	38	37	SE.	NE.	E.	...	·175	·19
27	30·105	29·998	29·952	29·917	29·76	53	29	44	37	37	NE.	SE.	E.	...	·230	·11
☽ 28	30·041	29·925	29·940	29·940	29·61	54	29	49	35	40	S.	W.	calm	...	...	·08
29	29·971	29·782	29·946	29·801	29·53	55	34	48	37	39	S.	N.	calm	...	...	...
30	29·762	29·653	29·816	29·778	29·28	52	27	51	38	41	W.	NW.	calm	...	...	...
31	29·822	29·473	29·687	29·278	29·40	58	42	50	40	42·5	SE.	SE.	calm	·26	0·135	...
	30·351	29·029	30·334	29·140	29·49	58	23	54	31	38·7				1·22	5·655	2·26

Remarks.

London.—March 1. Overcast: clear and windy at night. 2, 3. Overcast. 4. Fine. 5. Foggy: fine. 6. Cloudy and cold. 7. Fine but cold. 8. Cold and dry. 9. Slight snow: windy and cloudy at night. 10. Cold and cloudy. 11. Slight showers of snow. 12. Cold and cloudy. 13. Frosty: hazy. 14. Frosty: cloudy. 15. Frosty, with fog. 16. Fine: cloudy. 17. Overcast: rain. 18. Heavy rain. 19. Cold and cloudy. 20, 21. Frosty: fine. 22. Overcast: fine: at night a heavy fall of snow, lodging heavily on trees and shrubs. 23. Snow-showers. 24. Frosty. 25. Fine: snow-showers at night. 26. Very fine. 27. Frosty. 28. Very fine. 29. Foggy: very fine. 30, 31. Fine.

Penzance.—March 1. Showers: fair. 2. Fair: rain at night. 3. Fair: rain. 4. Rain: fair. 5. Fair: evening rain. 6. Clear. 7. Fair: rain. 8. Rain: fair. 9. Clear: heavy showers of hail. 10, 11. Fair. 12. Clear. 13. Fair: rain. 14. Heavy rain. 15. Snow: rain. 16. Fair. 17. Showers. 18, 19. Fair. 20. Showers. 21—24. Fair. 25. Showers. 26, 27. Rain: fair. 28. Fair: a luminous halo round the moon. 29. Fair. 30. Clear. 31. Fair: rain.

Boston.—March 1. Stormy: rain P.M. 2. Fine. 3. Cloudy: rain early A.M. 4. Cloudy. 5. Fine. 6. Cloudy: rain P.M. 7. Fine: rain early A.M: snow P.M. 8. Cloudy. 9. Snow: hail-storm P.M. 10. Stormy. 11. Snow: ice A.M. 12. Fine: ice A.M. 13. Cloudy: ice A.M. 14. Cloudy: ice A.M.: snow P.M. 15.—17. Cloudy. 18. Cloudy: rain early A.M. 19. Cloudy: snow A.M. 20. Fine: ice AM. 21. Cloudy: snow A M. 22. Cloudy. 23. Fine: snow A.M. 24 Cloudy. 25. Rain A.M. 26, 27. Rain. 28. Cloudy. 29—31. Fine.

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LXVII. *Notices of certain Plants of Marocco, Specimens of which were transmitted to the Horticultural Society in* 1831; *with Remarks on the* Arar *or Gum Sandarach Tree, and an Inquiry respecting the Cedar of the Ancients. By* E. W. A. Drummond Hay, *Esq. His Majesty's Agent, &c. in Marocco**.

THE names of the following plants I have received from my scientific friend the Chevalier Schousboe, Consul-General for Denmark, at this place (Tangier), who has been for many years known by his writings to the botanical world.

1. *Passerina hirsuta*, Linn.: found in the fields near Laraiche, upon the western coast.

2. *Narcissus Bulbocodium*, Linn.: from the Atlas mountains. Of this I add a dozen bulbs, in hopes that some one may be made to grow.

3. *Artemisia:* from near to the Atlas range. The name of this plant is pronounced by the Arabs of Marocco *Shăch*†. I endeavour to show the sounds as nearly as possible according to the pronunciation of an educated Englishman. It is to the want of travellers having stated distinctly the pronunciation to which their use of the letters they employ in strange proper names is to be referred, that we may ascribe *much* confusion in ancient and modern history and geography; and some also, I doubt not, in other sciences. This plant *Shăch*

* Read before the Horticultural Society May 17, 1831; and now communicated by the Council of that Society.

† The *a* is to be pronounced like the *a* in our participle *made*.

شيح is used as a vermifuge by the Moors, who pound its leaves, flower and small stalks, all which are bitter, and make a paste of them with honey, which they administer every morning fasting, for three or four days, as they tell me. I am informed by the natives that it is abundant near a place called Taza (by some written *Tezza*), about two days' journey east, or rather perhaps north-east, from Faz; also in the region between Marocco and Mogodor; and so vastly does it abound, that the sheep in some of those districts feed almost exclusively upon it. I send a parcel marked *Shăch* A, obtained by me as a drug at this place. The packet marked B is not so fresh, and this specimen of the drug appears to be from a younger or smaller plant than the former. The little packet marked C contains the flower and seed, I presume, some of which are to be found in the parcel A. The whole is sold and used indiscriminately for the medicine.

4. *Narcissus viridiflorus:* rare. Grows in the plains of Showeea.

5. *Ornithogalum fibrosum:* very rare. See Desfontaines, "*Flora Atlantica*," tab. 84. It is found in the forest of Màhmōra, near Mahadēa.

6. *Narcissus serotinus*, Linn.: found in the plains of Showeea. Of this I send, besides the three pressed specimens with flowers, four bulbs; but much fear that they have been kept too long to grow.

7. *Jasminum fruticans*, Linn.: also from the plains of Showeea.

8. *Leucoium autumnale*, Linn.: grows in the plains of Al Gharb.

9. *Viola arborescens*, Linn.: from the district of the Shtooka tribe. The tribes of Shtooka and of Sheadma occupy the region of the coast to the north of Azamor.

10. A small branch with leaves and fruit of the *Arar* عَرْعَر (more correctly pronounced *Araroon*), which Mr. Schousboe kindly procured for me. He names it, after Desfontaines, *Thuja articulata*.

I have not seen the Work of Desfontaines; yet I conceive that he must, by the name he gives to the *Arar*, have suspected this tree to be the θύον of the Greeks, or *Arbor citri*, as Pliny calls it (lib. xiii. cap. 29. and 30.), and of which that extraordinary cyclopædist gives so highly interesting an account; as the material of large and beautiful tables used by the ancient Moors, and which had been brought to Rome as early as in the time of Cicero, who had a noble board of the sort.

Since writing my letter, I have perused again the whole passage on the subject in Pliny, and am much struck with this sentence:—"*Ancorarius* mons vocatur citerioris Mauretaniæ, qui laudatissimam dedit citrum, jam exhaustus."

Knowing how much the Latins, as the Greeks their predecessors in this careless custom, corrupted the proper names found in tongues foreign to their own, I am disposed to think that the etymon of this Latinized term *Ancorarius* may be found in the name of a stream and valley, called by the present inhabitants *Ă'ncor* أَنْكُرْ (*Wad Ă'ncor*), which an honest and intelligent mountaineer in my service, who is a native of the region near Melilia, describes as having its course at about three days' journey on this side that place; and that, where the stream enters the sea, a small island is seen at a short distance from the shore. This would appear to me to be a river, for which I have not indeed seen any name set down, but which is marked in the best charts as entering the Bay of Alhucemas, which is near the 4th degree of W. longitude; and, as a further coincidence with the best chart, my informant seems to agree with our geographers in the relative positions of the districts of Bockoya and Badis with that stream.

The Wad Ă'ncor is said to be an ample stream that runs through a broad valley, descending from high mountains, on which he believes there is much timber—much he says that he knows to have been cut in that district, but he never was upon *those* hills. He does not know the name of any *mountain* in his country of Reef that resembles the same word *Ancor*.

At the sources, nevertheless, of this stream I would presume to place that ancient habitat of the *laudatissimæ citri*, which is described by Pliny as being in *Mauretania citerior*.

My informant says he has seen forests of the Arar in his native mountains; that many of these trees are in girth more than two full-sized men can encircle with their arms, at the height of a man from the ground. This, however, would not seem to give more than about four feet in diameter. Their height he describes as enormous, but cannot give a definite idea of that.

Although the hills above the Wad Ă'ncor be now well clothed with large timber, the expression of "jam exhaustus," which in *his* time Pliny may have applied to that region, does not I think supply any fair objection to my theory; particularly when it would appear that the neighbouring coasts were more frequented in the time of the Jubas, and for ages before and after them, than they are now; and the stream of which we have been speaking, with others in the vicinity, would afford a facility for floating the timber to the sea.

My Reefian adds, that they have in his province in great number the large tree which, he says, *they* call Làris, and of which much fine timber is brought, not only from Reef, but from, I believe, all the higher Sierras of Marocco. It is sometimes of a delicate texture, generally I think of a dusky yellowish colour, and gives out an agreeable aromatic odour: it is rather soft, like some species of Pine, and easily worked. My servant describes this tree as having various characteristics of some of the Pine genus.

It seems as if the vulgar name *Laris* were a corruption of *Al A̓ris* or *Al A̓rs* أَرْز , which is in the Hebrew named אֶרֶז (pronounced *Āris*), which Golius translates "Arbor conifera, *nempe* Cedrus, Pinus, Picea," and adds, "usurpatur quoque pro عَرْعَر *ar ar*, Juniperus." Our Reefian says that his Laris bears a cone or pine.

I have been led to mention this tree, because seeing doubts expressed by the commentators on Pliny (lib. xvi. cap. 19.) with regard to the name Larix, the πευκη of Theophrastus, I fancy those interested in botanical nomenclature may discover in the name used not only by the Arabs, but in the language of the Brebers, the most antique race known in Marocco, a clue to extricate them from the difficulties of the scholiasts. I may note that the Cedar of Lebanon is named אֶרֶז *āris* by the Hebrews, in numerous passages of the Bible. See, among others, 1 Kings, v. 6., where the wood used for the Temple of Solomon is spoken of; and (xix. 6.) in the Book of Numbers (written, I suppose, many ages before that of Kings), I find the same wood used in ceremonies of religion; wherefore I conclude, the āris was an odoriferous wood like the Cedar, as is the L'aris of Marocco. This name אֶרֶז is translated by Abraham Mendes de Castro in his curious Spanish version (published with the Hebrew) sometimes, as in the First Book of Kings, v. 6., of our version (which he makes the 20th verse of the same chapter), *alarzes*; and in another place (the next chapter, vi. 16.) *cedros*. *Alarze* is the same as the modern Spanish word *Alerce*, corrupted evidently, as Cañes agrees, from the Arabic; and which the Spanish academicians describe as "Arbol corpulento, casi generalmente reputado por especie de cedro, por ser muy olorosa su madera:" but they then favour us with translating it by *Acer*, which my Latin tells me is the *Maple tree*!

It is curious that the wood of which the Ark of the Covenant was made (Exod. xxv. 10.; and xxxvii. 1. &c.) is named in our vulgar English version *Shittim*,—a very correct pronunciation indeed of the Hebrew שִׁטִּים, which in Arabic is سُنْط, pronounced *sent* (or *sant*, or more correctly *santoon*); and this is translated by Richardson "A kind of thorn, acacia;" and Golius (p. 1225.) makes it also "Acacia." This is given by De Castro as "*maderos de cedros*;" but a modern Spaniard, Father Philip Scio de S. Miguel, sets the thing down, in more prudent ignorance, as "*maderas de setún*"!

The names of different woods mentioned in the Bible seem to have strangely confounded the translators; for I find that of which the Noachic ark was ordered to be made, (Genesis vi. 14.) in Hebrew גֹּפֶר, that is, to be pronounced *Gopher*; so written in our received version, for want, of course, of a better acquaintance with the ancient botany of Syria. This Gopher-wood is written in the Chaldee קְדְרוֹס *Kadros*. May this be an etymon of κέδρος? the *cedrus* of the Latins (who by the way never, *most assuredly*, pronounced their *c* softly like an *s*, as we and some other Europeans think fit to do in reading Latin). If this my conjecture be found to have foundation, then old Abraham Mendes has made a luckier hit perhaps than usual, in translating *gopher* as "sedro." Gopher is in the Arabic version before me,—published, in quarto, at Newcastle-upon-Tyne, in 1811, and which edition was, I understand, superintended by the late Arabic Professor the Rev. J. D. Carlyle,—شمشار, or *shemshar*.

Father Philip, the aforesaid modern Spaniard, seems again in this passage tacitly to confess his utter ignorance of the matter, by turning *gopher* into "*maderas labradas*"!

11. *Rhamnus Lotus*, Linn. This shrub is, according to Mr. Schousboe, common in the whole of Marocco, growing in the plains; but I have not met with it in this northern province *Al Gharb*. I found it in great abundance on the plains of Sragna and near Marocco, in the vast plain watered by the Tensift; and yet more frequently, I think, in the stony arid plain on the west of the lesser range of hills called جبيلاة

Djebeelat (or *the Little Mountains*), in contradistinction to the lofty range of Atlas, of which one vast Sierra runs nearly parallel to the Djebeelat.

It is the true *Lotos lotophagorum* of the ancients, according to Mr. Schousboe.

The branch I send you I picked near *Ras el ain*, the source of the Tensift, where I saw several of the *Rhamnus Lotus* (which is generally no larger than a large shrub), about twelve feet high or more, having trunks about three feet in circumference; and which, at a little distance, resembled in their general appearance large hawthorn trees.

The shrub itself is called *Sidra* سدرة, and the fruit of it *Nebăk* نبق.—See the Lexicon of Golius, p. 1156 and 2296.

The fruit is much eaten in this country, and I have found it very refreshing when just plucked off the tree. It is brought from a considerable distance to this market; and I have today bought a small quantity of it, which I add to my present little selection. It is very harmless food, and much used by the Moors' children. When fresh the fruit has a flavour somewhat like gingerbread.

12. *Elæodendron*,—the *Argan* أرگن of the Moors. The specimen sent is from a flourishing tree in the garden of the Danish consulate at this place, of which the seed came from Mogodor. It produces flowers and fruit annually. I send with it a parcel of the seed. The Argan tree is not indigenous in the northern provinces of Marocco. The oil obtained from its fruit is preferred for its sweetness by the Moors to that of the olive, and is much used by the natives in cookery. It sells therefore for rather a higher price than olive-oil.

13. Two bulbs of the *Scilla undulata*, that one of my party took up, on the 1st of January 1830, a little to the north of the river *Oom Errbĕk* ام الربّع, "the mother of herbage," a poetical name that a botanist may particularly admire (the *Morbeya* of the maps), about the middle of its course.—See the "*Flora Atlantica*." I understand that it was first discovered by Desfontaines, who gives a drawing of it.

14. Three bulbs of the *Lachmolia serotina*,—the *Hyacinthus serotinus*: from the district of the Shtooka tribe before mentioned.

British Consulate, Tangier, April 6, 1831.

LXVIII. *On Iodic Æther.* By James F. W. Johnston, *M.A. F.R.S. E. &c. &c.**

When a saturated solution of iodine in alcohol is poured into hot nitric acid in a large flask, a violent action takes place with evolution of nitric æther, acetic acid and deutoxide of azote; and the colour of the iodic solution disappears. If the heat be kept up, and iodine in a solid state be gradually added as long as the action takes place, and the colour disappears, there is deposited on cooling a transparent yellowish oily-looking fluid, heavier than water, and possessing the following properties:—

1. It has a strong penetrating odour, very different from that of the hydriodic æther of Gay-Lussac, and a sharp burning taste, the effect of which remains upon the tongue for a considerable time.

2. When free from excess of iodine it is of a very pale yellow colour; a slight heat, however, discolours it by causing partial decomposition.

3. It is not easily inflammable. It cannot be volatilized without decomposition. The heat and light of the sun decompose it in close vessels; it becomes coloured and deposits iodine in regular crystals. Kept in contact with the acid liquid in which it was originally formed, it remains colourless for a great length of time. Left to spontaneous evaporation in the open air it thickens, becomes discoloured, and disappears very slowly. On the hand it volatilizes rapidly, and leaves a stain like iodine.

4. Its specific gravity at 60° Fahr. is about 1·34.

5. The boiling point of the compound is as high as 230° Fahr. When gradually heated in a small retort, a colourless fluid, having an æthereal odour, begins to distill over as low as 160°; while the æther in the retort gradually thickens and becomes dark coloured. At 380° this coloured liquid comes over very slowly in brownish red fumes, which condense in the beak of the retort into a dark brown solid, consisting chiefly of iodine. Over a spirit-lamp the distillation and decomposition are much more rapid; iodine is given off in copious violet-coloured vapours, and there remains a light shining charcoal, which in the flame of a candle burns away very slowly. The clear liquid which distills over by a gentle heat reddens litmus, but gives no æther by admixture with water.

In preparing this æther, if we continue the heat after the iodine has disappeared without adding more, the æther held in solution by the acid liquid is again decomposed, the solu-

* Communicated by the Author.

tion becomes coloured, iodine is deposited and volatilized, and olefiant gas is given off. If the experiment be made in a tubulated retort, the iodine condensed in the beak and in the receiver is gradually converted, by the absorption of the olefiant gas which comes over, into Faraday's iodide of carbo-hydrogen, which crystallizes in long white prisms of one or two inches, or forms an entire massive coating in the interior of the long beak.

6. It dissolves largely in alcohol, either cold or hot, giving a colourless solution, from which water precipitates a large quantity of it, but of a brown colour. The alcoholic solution when distilled gives a colourless neutral liquid not troubled by water, but which, mixed with caustic potash and placed in the light, becomes brown, showing that it contains iodine. In the retort there remains the brown opake fluid. Æther mixes with it in all proportions, and by agitation separates it from the acid liquid in which it is formed. It might therefore be employed with advantage in the preparation of the iodic æther, were it not difficult again to separate the whole of it by water without decomposition. Water dissolves it in small quantity. When the yellow æther is washed with water it becomes less in quantity, less fluid, and of a brown colour, which by further washing gradually deepens to a dark brownish red. The aqueous solution is colourless, and slightly acid, due, as appears from its reactions, to the presence of a small quantity both of iodic and of hydriodic acid.

7. Sulphuric acid in the cold decomposes it, rendering it dark brown; when heated it becomes dirty black, and vapours of iodine are given off. A few minute prisms of a yellowish colour also condense in the upper part of the tube, which are probably iodide of carbo-hydrogen (iodide of ætherine). On muriatic acid it floats unchanged, but as the lighter parts evaporate or are dissolved it becomes brown and dense, and sinks to the bottom; the acid at the same time becomes yellow. Nitric acid in the cold does not act upon it. The acid solution in which it is formed retains it in solution only till it cools. When once separated by cooling, it cannot be redissolved by the application of heat.

8. When chlorine is passed over it, muriatic acid is formed, and the æther becomes red. This gas, however, does not seem to be capable of decomposing it entirely; for when gently heated after long exposure to an atmosphere of chlorine, it gives off chlorine and muriatic acid vapour, and sinks apparently unchanged, except in colour, when put into water.

9. When obtained by decantation from the acid liquid in which it is formed, the æther reddens litmus; and from the

ease with which water and the caustic and carbonated alkalies discolour and partially decompose it, and the impossibility of distilling it, I have not hitherto obtained it in a state, in which, in the air at least, it does not possess this property in a slight degree. A weak solution of caustic potash or soda acts upon it like water, discolouring it and diminishing its volume; but after washing again with water to remove the alkali a slight action upon litmus is still observable. This is to be ascribed solely, I believe, to partial decomposition. A concentrated solution of a caustic fixed alkali acts upon it, with the evolution of heat and some gas; and when allowed to subside after agitation the alkaline solution is of a red colour, and the æther, much diminished in quantity, is colourless, or nearly so. Agitated with pure water the æther again becomes coloured and tinges litmus. With a sufficient excess of caustic alkali it appears, like muriatic æther, to be resolvable into a colourless oil containing only carbon and hydrogen.

The alkaline solution evaporated to dryness, and the dry salt redissolved gives no trace of iodic acid. It precipitates lead of the well-known yellow colour, but it does not precipitate muriate of barytes. Nitric acid separates iodine from the solution.

10. After being treated with caustic potash in a concentrated solution, potassium has a very slight action upon it, becoming tarnished, evolving minute bubbles of gas, and making the liquid slightly brownish. If potassium be dropped into the æther as first obtained, much action and evolution of heat takes place, æther and an iodide are formed, and charcoal remains behind.

11. When dry phosphorus is thrown upon it considerable action takes place, with evolution of heat, and an iodide of phosphorus is formed. The same takes place under water, and the supernatant liquid contains hydriodic acid, from which nitric acid precipitates the iodine. On sulphur it has no action.

12. Mercury does not act upon it in the cold, unless the æther have become discoloured by partial decomposition, when the mercury removes the free iodine to which the colour is due. When slightly heated a greenish pellicle is formed on the mercury, and the colour developed in the æther by heat disappears. This greenish pellicle dried and heated becomes red, showing that some iodide had been formed. The decomposition, however, is due to the heat and not to the action of the mercury.

This æther may also be prepared by the substitution of sulphuric æther for alcohol, in which case, after the violence of the action has ceased, the bottle may be placed in the sun for several days, and a little more iodine added as the colour slowly disappears. The addition of a little sulphuric æther will at any time by agitation give a solution of the iodic in common æther, which floating on the top may easily be separated.

The supernatant acid liquid in the first process contains a large quantity of iodine in solution, partly in the state of æther, partly, probably, as Faraday's iodide of ætherine, and partly as Serullas's periodide of carbon. The æther is not wholly separated by subsidence on cooling; a further portion is thrown down by the addition of water, and a second portion by saturation with an alkali, though in both cases slightly coloured. Agitation with sulphuric æther separates it most completely. Saturated with soda the supernatant liquid becomes dark coloured, and by evaporation may be brought to a treacly consistence, but does not crystallize. The dark colour is not due alone to free iodine, for it does not disappear by long exposure to the air, nor by heating, but to carbon, which exists either in a peculiar state of combination with iodine, or as ulmic or azulmic acid. By evaporation to a syrup, and subsequent dilution with cold water, a carbonaceous matter is separated, which is soluble in hot water, and in solution throws down a yellow iodide from the salts of lead. Alcohol does not separate the iodine from this carbonaceous matter, but it may be driven off by the heat of a spirit-lamp, leaving a spongy charcoal.

When the acid liquid is diluted and supersaturated with ammonia a yellow precipitate falls, which is chiefly Serullas's periodide of carbon. This compound is sometimes obtained also on decomposing the æther by dry caustic potash. Nitric acid throws down iodine from the filtered ammoniacal solution, and by evaporation it becomes dark coloured as above stated. The saturated supernatant liquid does not precipitate chloride of barium.

What is the true constitution of this interesting compound, it is difficult to decide. From the mode in which it is formed by the aid of nitric acid we should be led to infer the presence of oxygen; while, on the other hand, its properties and the absence of iodic acid, both in the caustic alkaline solution by which it has been decomposed, and in the supernatant liquid, which even when saturated gives no precipitate with muriate of barytes, lead to a contrary conclusion. When iodic acid is heated in alcohol decomposition takes place, and by distillation the whole of the iodine passes over, the odour of hydriodic æther, which is perceptible, showing that a small quantity of that compound has also been formed. The presence of alcohol, therefore, in a hot solution, seems incompatible with an oxide of iodine; and if such be the case it will easily account for the absence of oxygen in the iodic æther, though formed by the agency of nitric acid. It is probable that nitric æther is formed first, and that from its decomposition the new compound containing iodine results. It seems to me, therefore, most likely that the æther described in this paper is a compound of iodine and ætherine ($4\,C + 4\,H$), belonging probably to the same class of compounds as the solid iodide of Mr. Faraday. Indeed in one experiment, instead of the æther subsiding as I expected, I obtained a group of large crystals of the solid iodide of carbo-hydrogen*.

Portobello, April 22, 1833.

LXIX. *Remarks on* Sir David Brewster's *Paper "On the Absorption of Specific Rays, &c."* *By* G. B. Airy, *Esq. M.A. Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge. In a Letter to* Sir David Brewster, *K.H. LL.D. F.R.S. &c. &c.*

My dear Sir David,

IN commenting upon your paper in the last Number of the Philosophical Magazine, I cannot but feel that I am undertaking an invidious task. That you will misinterpret my motives or feelings I am not afraid; but to others it may appear presumptuous in me to criticize the remarks of one whom I revere as the author of nearly all our experimental knowledge in the most important parts of optics. But science is public property: it is the right of all, and may be the duty of some, to expose what they conceive to be erroneous; and the obligation is at least not lessened when such seeming error is backed by the highest scientific character.

I commence with your remarks on the test of theory. "The power of a theory to explain and predict facts is by no means a test of its truth; and in support of this observation we have only to appeal to the Newtonian Theory of Fits, and to Biot's beautiful and profound Theory of the Oscillation of Luminous Molecules." I must surely have misinterpreted this sentence. That theories essentially and fundamentally different can apply equally to the explanation of phænomena embracing so many classes as the phænomena of optics, is, I conceive, quite impossible. What test, then, can there be for the truth of a theory but the power which it gives us of calculating old observations

* The colourless transparent prismatic crystals described in this paper as Faraday's iodide, differ from that compound, as generally described, in being slightly soluble in water, from which they may be again volatilized in beautiful prisms by a very gentle heat.

and predicting new phænomena? This principle has been recognised by every philosopher, and is tacitly acted upon in every investigation which is going on in every other branch of science. Is optics to be excepted? Or am I to understand you to say that Newton's and Biot's molecular theories will apply to the explanation of phænomena of various classes equally well with the undulatory theory? If this is your meaning, the *onus probandi* is upon you. It is certain that observations have been calculated upon the undulatory theory, and have been found to agree with the calculations, which have not been calculated on any other theory: it is certain that phænomena have been predicted from the undulatory theory, and have corresponded exactly to the prediction, which have not been predicted from any other theory*. If you intend that the sentence above quoted should be received literally, you are bound to point out some steps at least of the calculation on other theories.

Nothing appears to me more prejudicial to the progress of science than vague statements of such a kind as that to which I allude. I am desirous of avoiding this error, and I will therefore point out several instances in which the two theories that you have mentioned fail.

Newton's theory of alternate states of easy reflexion and easy transmission will *not* explain the jetty blackness of the central spot in Newton's rings. It will *not* explain the dilatation of the rings on increasing the angle of incidence, without another principle (the lengthening of the fits), which is negatived by every use made of light which has passed obliquely through glass. It will *not* plausibly explain Grimaldi's fringes, and *fails* totally for the fringes produced by narrow openings. It will *not* in the slightest degree explain the fringes, &c. in the shadows of bodies of different forms. It will *not* explain the interference-bars produced by two mirrors†. It will *not* explain the spectra formed in telescopes by Fraunhofer's gratings.

Biot's theory of moveable polarization will *not* explain the ordinary polarized rings of Iceland spar, in different positions

* As a simple instance of calculation, I may point out the polarized rings of Iceland spar, in different positions of the analysing plate; and as a simple instance of prediction, the change in the character of Newton's rings at a certain angle of incidence, when the lower plate is metallic and polarized light is used. Perhaps the most remarkable prediction that has ever been made, is that lately made by Professor Hamilton. [See present vol. p. 112 and 207.—EDIT.]

† In some place, to which I am at present unable to refer, I have seen a hypothesis to account for the destructive interference of light on a theory of emission. I envy the imagination of any one who can form such a con-

of the analysing plate. It will *not* explain the rings produced with Iceland spar with circularly polarized light. It is *unable* even to express the nature of circularly and elliptically polarized light. It will *not* explain the phænomena of quartz. It will *not* explain the rings of biaxal crystals with plane or circularly polarized light, in different positions of the analysing plate.

The phænomena which I have mentioned have all been calculated on the undulatory theory, and they agree perfectly with the deductions from that theory: these calculations moreover are all to be found in print. They are also phænomena which ought to be explained by the theories above mentioned if those theories possess any value. They are, therefore, as appears to me, a fair subject of examination to any person who wishes to decide in the choice of a theory.

I think it unnecessary to remark further upon your sentence, " Twenty theories may all enjoy the merit of accounting for a certain class of facts, &c." because my opinion is sufficiently expressed above. In whatever degree twenty theories may enjoy this merit in conceivable cases, there are not two that enjoy it in optics. And the ground upon which the supporters of the undulatory theory receive that theory, is, not that it explains phænomena as well as any other theory, but that it explains phænomena which no other theory can explain.

To the authority of Newton (supposing authority to deserve the least weight when our collection and variety of facts, and our powers of calculation, have been so immensely increased,) I attribute no importance, for the following reason. I think that Dr. Young has fully made out (Phil. Trans. 1802,) that Newton was a believer in the theory of undulations. In his Optics he most cautiously urges the reader to connect no physical conception with the theory of Fits. Nor are reasons wanting for his adoption of the molecular theory *for calculation*. It was impossible in Newton's time to make any extended calculation on the undulatory theory; for even the principle of the coexistence of small vibrations, which occurs in every part of such calculations, was then unknown. With all the methods necessary for the calculation of a molecular theory he was, on the contrary, perfectly familiar.

ception; and if he could extend the hypothesis so as to include polarized light (plane, circular, and elliptical), I should have a sincere respect for his inventive powers. But if he seriously applied it to the explanation of phænomena, which are a necessary consequence of the simple fundamental assumptions of the undulatory theory, I should have little esteem for his judgement as a philosopher. The man who, in order to subtract 2 from 3, should wait for the completion of Mr. Babbage's engine, would not, in my opinion, be acting more absurdly.

You mention, and justly, that the undulatory theory is defective as a *physical* representation of the phænomena of light. I imagine that any theory must be defective in this point. But is the undulatory theory more or less defective than the molecular theory? To assist in forming an answer to this question, I will point out two or three facts. The theory of undulations explains well the reflexion at the surfaces of transparent media; which Newton's theory can hardly be strained to explain. With certain assumptions, it gives laws for the intensity of the reflected light, which your subsequent experiments on the position of the plane of polarization have confirmed; no one has even conceived how such a calculation could be commenced on the molecular theory. It explains the relation (discovered by you) between the polarizing angle and the refractive index; an explanation perfectly inconceivable on the molecular theory. It explains with less certainty the elliptic polarization at total internal reflexion, and it does not at all explain the elliptic polarization at metallic reflexion; but the molecular theory is unable even to give a notion of these kinds of light. Lastly, it explains well the connexion between double refraction and polarization; an explanation which has been hailed by every philosopher who has examined it as the greatest addition made to our physics since the days of Newton; and one which it will be useless to attempt on a hypothesis of emission.

The dispersion is doubtless a formidable objection; though it has been shown that the explanation may be completed by the introduction of causes analogous to those which act in other cases. But is not the dispersion a formidable objection to the molecular theory? I confess that I have no distinct conception of the supposition which must be made in order to explain dispersion on Newton's system. It must be remembered that the cause must explain the connexion between the refractive index and the length of the waves or fits; as it is now certain that the smallest change in the latter is accompanied in every instance by a change in the former.

I now come to the ostensible subject of the paper,—absorption. I avow, as fully as any opposer of theory can desire, that no explanation of absorption has been given upon the undulatory system. I assert as fully, that no explanation has been given, or seems likely to be given, on the theory of emission. If we are at present called on to decide between two theories, this subject appears to me to be unimportant. If we are to decide whether there shall exist any theory of light at all, the resolution of the question will depend upon our determining whether absorption must necessarily enter into a theory of light.

I do not think that absorption is to be considered a necessary part of the theory. It is a sort of extraneous interruption which either leaves the ordinary laws in full vigour, or wholly destroys, not the laws, but that which is the subject of the laws. Reflexion, refraction, interference, double refraction, polarization, go on with absorption just as if there were no such thing in nature. The supposition of undulation by transversal vibration, the principle of superposition of small vibrations, the assumption that the velocity is different in different media, are necessary in every investigation; the suppositions (whatever they are,) that are to account for absorption are necessary only now and then. The former suppositions, in the vast majority of instances, do not require the latter; the latter when wanted must be combined with the former. These considerations seem to point out clearly that absorption requires a *supplementary* theory; and our only care with optical theories at present must be, that our present assumptions may admit of such a supplement at some future time. As far as I can judge, either theory (emission or undulation) seems likely to admit of such a supplement, and I do not see that one will admit of it more easily than the other.

A remarkable instance of the same kind has already occurred in the history of optics. When Fresnel's theory and measures of diffraction had given a very high probability to the undulatory theory, there still remained, to be accounted for, the laws of polarization and the connexion between polarization and double refraction. The undulatory theory was therefore generally adopted, leaving the kind of vibration to be determined by the consideration of accounting in the best manner for these remaining phænomena. The success with which this was afterwards done, by the assumption of transversal vibrations, exceeds anything that has been gained in philosophy since the establishment of the theory of universal gravitation. Had Fresnel proceeded as you (apparently) would wish us to proceed, the undulatory theory would not now have existed.

Every other branch of philosophy presents instances similar to the last. If, for instance, at the time of inquiring into the mutual action of bodies on each other, Newton had insisted on including in his general theory (whatever it might be,) the effects of what we now call magnetism and capillary attraction, the theory of gravitation would never have been formed. By leaving these as subjects for future investigators, and by reducing to law the preponderating set of phænomena, he was able to form the most complete cosmical theory that has ever appeared. Many years passed before those supplementary laws were reduced to a simple form; yet by the consent of the world, the theory of gravitation, though imperfect as a theory of attraction, though sometimes completely disguised by the forces which Newton left unexplained, was adopted as a true system. That the existing theory of undulations stands in the same relation to the complete theory of light as Newton's universal gravitation to the complete theory of attractions, I have not the slightest doubt.

With regard to the importance, as a difficulty, of the *number* of interruptions in the spectrum produced by nitrous gas, I do not entirely agree with you. If a plausible reason can be found, on either theory, for a single interruption, I have no doubt that good conjectural reasons will very soon be found for a thousand interruptions. And with regard to the attention which, in shaping an optical theory, these interruptions at present deserve, I may perhaps not quite agree with you. They are not yet disciplined under laws: they stand a mere "mob of facts;" and no one can tell what they seem to indicate. But every attention ought to be given to reduce them to rules; and the apparent uniformity of the lines of nitrous gas, at least as compared with the solar lines or the interruptions by different kinds of glass, seem to make it probable that this beautiful and important discovery (setting apart its practical uses,) may assist us in discovering the laws which govern the most obscure and most difficult part of optics. The theory of crystalline absorption cannot then, I think, remain long without explanation. With sincere respect, I remain,

My dear Sir David, your faithful Servant,

Observatory, Cambridge, May 7, 1833. G. B. AIRY.

LXX. *Remarks on* Mr. Barton's *Paper "On the Inflexion of Light," in the London and Edinburgh Journal of Science, &c.* No. X. *By the Rev.* B. POWELL, *M.A. F.R.S. Savilian Professor of Geometry, Oxford**.

THE question respecting the truth of the undulatory theory of light is at the present time exciting more discussion than we might have anticipated so abstract a topic would be likely to call forth: this circumstance alone is a favourable indication of the increasing interest taken in matters of pure science; and whether we consider the importance of the subject, the beautiful and refined nature of the inquiry, or the value of fair and well-urged objections and dispassionate controversy for eliciting truth, and promoting the real advance

* Communicated by the Author.

of science, I trust the pages of this Journal will not be considered as improperly occupied by a few remarks, which have suggested themselves to me on reading Mr. Barton's paper above referred to.

I hope also that Mr. Barton will have no ground to complain of me as a prejudiced "undulationist," determined at all hazards to support a favourite theory. I am desirous that every theory should be examined with fairness and impartiality. But it appears to me, in the present stage of this inquiry, that in preference to pulling down or constructing theories *in toto*, it would be far the more philosophical mode of proceeding, to examine carefully the extent of the actually demonstrated laws to which the varied phænomena of light are referrible, and thence to pursue the inquiry as to what sort of hypothetical action will *best account* for the *greatest number* of them. Of such action some of the characteristics of *periods* or *intervals* appear so indisputably essential to the explanation of the results, that no one, capable of appreciating the accumulated and *cumulative* evidence on which the assumption of them rests, can doubt the legitimacy of that assumption. And these characteristic actions are precisely those which would necessarily result from the vibrations of an æther. In the present state of our knowledge if we should allow that this sort of theoretical action may fail to account for several facts, yet we must contend that it unquestionably accounts for a vast number more than any other principle which has been, or probably can be, alleged.

(1.) With regard to the author's first objection I shall merely observe, that on any theory we must admit the reality of the *intervals*, which are called lengths of undulations. And in interference-experiments, let us suppose two rays arrive at the centre of the screen in a conspiring state, and give a bright point; the effect at other points on the screen will depend upon the *successive differences* in the length of route of the two rays arriving together at those points, *compared with that of the first pair of rays*. It is not the absolute, but the relative differences of route with which we are concerned, and these are measured from the aperture as the origin.

But on the undulatory view of the matter, it is a point belonging to the most unequivocal and elementary part of the theory, that any small portion of a large wave may be taken, separately from the rest, as the origin of a new small wave diverging from it in a spherical form. Such an origin of a small wave (in Mr. Barton's diagram) occurs at A, where, *if the aperture be very small*, it is shown by theory that the new wave diverging from it, which is produced by the sum of all the small waves belonging to the original waves from RR′ &c., will be equally strong in all directions, and A in this case is the real centre and origin from which the new undulations commence.

But for all accurate experiments, the luminous point made use of is the focus of a lens of short focal length; in which case the theory is equally clear; and the waves after converging to this point diverge from it in the same manner as if that point were a centre of excitation, or source of light. (See Professor Airy's Tract, p. 289.) Indeed (unless I mistake Mr. Barton's meaning,) it is difficult to conceive how such a point could have occasioned any embarrassment or ground of objection.

(2.) The second observation is of a more important character, and brings us to an objection of a more tangible kind.

The author admits at the outset that "the theory of Fresnel agrees pretty well with the results of his own experiments." Now this admission appears to me to involve very nearly *a concession of the whole question.* Fresnel's researches were conducted with a delicacy and a precision far exceeding those of any previous experimenters, and keeping pace with the increased and elaborate refinements of the theoretical analysis. These coincidences of observation and theory are, in fact, such as no one can examine without an irresistible conviction of the truth of the laws which the formulæ express. These considerations alone might almost suffice to render superfluous any further argument; since all that Mr. Barton alleges is grounded on a comparison of the theory with *older*, and therefore probably far less accurate experiments. The observations of an older date, however valuable for the period at which they were made, are not, in researches of this kind, to be put in comparison with those obtained by the philosopher of the present day with all "the appliances and means to boot," which the improvements of recent science place at his disposal: if a discordance should be found, the more precise results of the recent investigation would be fairly entitled to the preference.

The observations of Newton on what has been called inflexion of light, were indeed remarkable instances of accuracy and skill; and when we consider that their illustrious author had not only to discover the facts, but almost to invent the *art* of experimenting, it would be no disparagement to his unparalleled preeminence, if modern research, enjoying superior facilities of instrumental precision, should have produced experimental determinations of such a character, as to supersede, or even to invalidate, his earlier results.

Newton has given us in his Optics, Book iii. the detail of these measurements, and their exactness is amply sufficient for the nature of his inquiry. Here is another point which should always be borne in mind in criticizing experimental researches. Newton does not apply these results for computing the *lengths* of the "*fits*;" their accuracy might be quite sufficient for the general establishment of the facts, though it should not be so for such a very delicate computation as this. Yet Mr. Barton takes Newton's measurements, in these experiments, as data from which to calculate, by Fresnel's theorems, the value of the *length of an undulation.*

I think it must be admitted that such a mode of comparison is hardly fair either to Newton or Fresnel. The data assumed to reason upon are inches and parts of an inch by measurement; the results turn upon differences in calculated millionths; the distance of the aperture or knife-edges from the origin of light, and from the screen, and the width of the aperture, are obviously determinations open to small errors; and no consideration seems to be made of the amount to which a small uncertainty here might influence the almost infinitesimal values which are to be deduced. Mr. Barton, however, calculating by Fresnel's formula on these data, finds the resulting lengths of an undulation to differ among themselves in the different experiments, and the greatest of them he finds to be less than Fresnel's value by about one fifth.

Now it appears to me that a fairer mode of comparison would be, to assume the length of an undulation according to Fresnel, and see what degree of inaccuracy, in the confessedly looser data, will suffice to give an accordance with that result.

If we adopt (for convenience of calculation) $\lambda = \cdot 000025$ inch, which is somewhat less than Fresnel's value for red rays, the distance (a) from the origin to the slit being 101 inches, the formula used by Mr. Barton on substituting the above values and transposing will give us very nearly,

$$\cdot 1875 = c\sqrt{\frac{202+2b}{b}}.$$

From which we may obtain the calculated values either of (b) the distance from the slit to the screen, or of (c) the width of the aperture, the other being assumed, in each of Newton's experiments for the supposed value of (λ); and we may compare them with the measured values. It will suffice for our purpose to do this with respect to the values of c; thus we shall have as follows:—

Newton's value of b.	Value of c calculated nearly.	Newton's Measurement of c.	Difference.
1·5	·013	·012	·001 inch.
3·3	·023	·020	·003
8·6	·036	·034	·002
32·	·058	·057	·001
96·	·091	·081	·010
131·	·098	·087	·011

This calculation is not carried to any great accuracy; but it will suffice to show that even without affecting the value of (b) (which is open to much uncertainty), the differences are fairly within the limits of error. We might proceed to calculate the errors in (b), or to estimate their joint effect; but what is here given, is, I believe, quite enough for our purpose.

Mr. Barton enters on a calculation in some measure the counterpart of this to find from the assumed length of an undulation, and Newton's values of (c), what ought to be the values of (b). This is done by taking the value of (λ) deduced as before from the 1st of Newton's experiments; but since that value has been found to be different in each of the experiments, it would have been more fair to take the mean value from all the experiments: and still more to the purpose of the inquiry, to take Fresnel's value of (λ) (as I have before done), and ascertain the amount of error which might account for the discrepancy; and even if this should turn out considerable, it would be no more than accords with the admission that Fresnel's measures were more precise than those of Newton.

The author then refers to certain experiments of M. Biot, in which, with a given aperture, he measures the distance at which the centre of the screen first becomes a dark point in homogeneous red light. Upon Biot's data for (b) and (c), Mr. Barton computes what must have been the values of (a) by the same formula as before; which in this case, upon transposition and squaring, will give,

$$a = \frac{c^2\, b}{7{\cdot}03\,\lambda\, b - c^2}.$$

The circumstance that the denominator may become negative indicates of course that there are certain limits of distance, consistent with the other conditions, within which the formula cannot be applied to any real case. Mr. Barton finds in the first three of Biot's experiments that the formula thus applied gives rise to the absurd result of a negative value of (a), and thence concludes that the values of (λ) are incorrect. The source of error may surely just as probably lie in the values of (b) or (c). At least it would be satisfactory to see what degree of inaccuracy in these measures might suffice to bring the result within the limit before indicated. We may easily make a sufficiently near estimate of this.

Taking the millimetre = ·03937 inch, we shall have the value before assumed for $\lambda = \cdot 00063$ millimetre nearly. We shall thus find for the limit, which makes the denominator vanish, $c^2 = \cdot 0044\ b$.

If we assume Biot's values of (b), and calculate those of (c) which are the *least* compatible with them, as just explained, we shall find, nearly,

Values of b.	Limit of c.	Biot's Value of c.	Difference.	
12	·23	·25	·02 m =	·0007 inch.
46	·45	·50	·05	·0019
120	·73	·75	·02	·0007

These values of (c) were probably obtained by estimation; and errors even to the insensibly minute amounts here stated would bring them to the limit, even supposing the values of (b) unaltered. These however are of a nature open to considerable uncertainty; (as indeed Biot confesses in the higher numbers, *Traité de Phys.* iv. 764.) In experiments of this kind it is almost impossible to determine, precisely, at what distance the central point of the screen is at its maximum of darkness. It is not necessary here to enter into further formal computations of the amount of error necessary to reconcile the alleged discrepancies; since it is obvious that results of this kind are not of a nature to be depended on for such minute calculations as those of the values of the lengths of undulations. Those of Biot were tried with a view to a different computation. In my own trials of such experiments, I have seldom been able to feel certain of the distance at which the dark band first appeared, or of that at which it attained its maximum, and have often shifted the eye-glass through a considerable space before any marked difference appeared.

(3.) Mr. Barton's third objection is certainly more formidable in appearance than the preceding; but some of the same remarks will apply here as in the former cases: we shall also find one or two other considerations, which will furnish a complete explanation.

With respect to the experiment of Newton, to which our author here refers as at variance with Fresnel's theory, we might recur to the general and obvious unreasonableness of citing experiments made when the art of observing was in its infancy, as tests of the refined theories of modern research; but the present is a peculiarly strong case: this particular experiment of Newton's was confessedly of a rougher description than most of his others; and the passage in which it is described appears to me a little obscure throughout. (See Optics, Book iii. p. 300 et seq. Ed. 1721.) But there is one circumstance attending the experiment, which alone suffices to place it out of all fair comparison with Fresnel's formulæ; viz. that the origin of rays, instead of being a single point (the essential assumption in the theory), was a hole a quarter of an inch in diameter.

But we may view it in some of its other relations: Newton shortly after describes very precisely his well-known experiment with two straight edges meeting at a small angle, and gives a representation of the fringes. In this case it is evident that the part of the image corresponding to the very narrowest part of the opening is by no means dark at the centre; but just beyond, between the diverging hyperbolic branches, there is the commencement of a dark space represented. Now if we suppose two such constructions placed with the points towards each other, there would result an appearance on the whole similar to that in Mr. Barton's diagram; and the arrangement of the edges would resemble his two curved edges, except that it is implied they were not actually in contact at their point of nearest approach; but if they were so near as to allow no sensible portion of light to pass, even at one point, the case would be precisely that just described.

But it is not necessary to suppose the edges actually in contact to produce this effect; for even when they are at a sensible distance, it results from the well-known fact of the *enlargement of the shadows* of the two edges beyond their geometrical boundaries, that these shadows (projecting as it were before the edges to which they belong,) will *coalesce*, before the edges meet; and this is a result of the undulatory theory.

The same thing will be true with parallel rectilinear edges, and may be seen even without the necessity of a single luminous point as the origin; and this seems very likely to have been the real result observed by Newton, in the experiment at first referred to.

I have repeated the experiment with slightly curved, as well as with rectilinear edges, in several forms and at various distances; and I have invariably observed, when the curved edges approached very near, an appearance similar to that represented in Mr. Barton's diagram, *but with this important difference*, viz. that the central dark portion, which (if I understand his diagram rightly,) is represented as absolutely dark and *isolated*, and having a continuation of the bright fringes on each side of it, in my experiments always appears *to join continuously* with the dark shadow on each side; the bright fringes always bending off in an hyperbolic form, and *not approaching* each other so as to form any junction at the sides: the moment the aperture was sufficiently widened to allow of their thus joining into continuous curves (as in Mr. Barton's diagram), the dark space entirely disappeared, and the progress of its disappearance was always that a faint light appeared *first at the centre*, and thence extended to the edges.

In all my experiments I invariably find that with *very narrow* apertures, up to the degree of approach which gives the coalescing of shadows, the *centre* is always a point of *relative brightness:* just before the formation of the shadow the whole becomes dull, but the centre the least so; the actual formation of the shadow commences by the *approach* of the two shadows *from* the edges, which at length unite in the centre.

Now in comparing these results with theory, we must confine ourselves to the case of rectilinear edges; for the slightest consideration will show, that, with curved edges, the portion of light at the wider parts, may, according to theory, conspire to the production of the effect, at the narrower part, in a degree dependent on the increase of breadth or degree of inclination or curvature of the edges; a case in which the analytical investigation would become immensely complicated. Taking then the formula as given in Prof. Airy's Tract (p. 317), and supposing an aperture with parallel rectilinear sides, it will be readily seen from the expressions for the values of (s), between which the integrals are to be taken (assuming such distances, &c. as will afford convenience of calculation), we shall easily find that commencing from the central part of the screen, and reckoning thence to lateral distances (which it will be convenient to measure in terms of the breadth which the geometrical shadow would have at the screen), we shall have for the limits of integration, successively, at the centre, at the edge of the shadow, and at 1, 2, &c. whole breadths of the shadow beyond the edge successively, when the aperture is *very narrow*, the values of (s) as in the following table; and with these, by means of the table of integrals, we shall find the quantities which are proportional to the brightness of the corresponding points of the screen.

Values of s.		Brightness.
$\begin{pmatrix} +\cdot05 \\ -\cdot05 \end{pmatrix}$	....	$(\cdot1)^2 \quad +(\cdot0002)^2$
$\begin{pmatrix} \cdot1 \\ \cdot0 \end{pmatrix}$	....	$(\cdot0999)^2+(\cdot0006)^2$
$\begin{pmatrix} \cdot2 \\ \cdot1 \end{pmatrix}$	....	$(\cdot0999)^2+(\cdot0036)^2$

Values of s.		Brightness.
$\binom{\cdot 3}{\cdot 2}$	....	$(\cdot 0994)^2+(\cdot 0098)^2$
$\binom{\cdot 4}{\cdot 3}$	....	$(\cdot 0981)^2+(\cdot 0192)^2$
$\binom{2\cdot 1}{2\cdot}$	....	$(\cdot 0933)^2+(\cdot 0307)^2$
$\binom{5\cdot 5}{5\cdot 4}$	....	$(\cdot 0788)^2+(\cdot 0397)^2$

Let us next suppose *the aperture wider*, such, for example, that, agreeably to the above remarks, we may have for the centre $s = \pm\ 2\cdot$; and proceeding thence only to $\frac{1}{4}$ of the breadth beyond the geometrical shadow, we shall have (under the same heads,)

$\binom{+2\cdot}{-2\cdot}$	....	$(\cdot 9772)^2+(\cdot 6864)^2$
$\binom{5\cdot}{1\cdot}$	....	$(\cdot 2165)^2+(\cdot 0611)^2$

Thus it will be apparent, even without taking the trouble of calculating further, that these results of theory give an exact representation of the effects actually observed. The centre is in either case a maximum, or point of *relative* brightness compared with other parts of the screen. When the aperture is extremely narrow the whole intensity is very small, or the image extremely dull, or even almost totally dark, which is precisely the character of the coalescing shadows: when slightly wider, the increase of light is considerable, the centre being the brightest, but the variation towards the sides but small: when of considerable width (within a certain limit), the centre is still a maximum, the increase in the absolute intensity great, and the variation from the centre to the edge more rapid.

The difference in the representation given in Mr. Barton's diagram, from my observations, may possibly be no more than arises from the imperfect nature of the sketch, or of the engraving; and it may not have been intended to convey the idea of *bright fringes continued at the sides*, or of the centre as absolutely dark; but however this may be, it is obviously an essential point; and Mr. Barton will, perhaps, in a future communication, be able to state it more explicitly.

Upon the whole I cannot but consider the theory of optics as under obligation to Mr. Barton for having brought forward several objections to the undulatory system; which are certainly of a nature deserving the most attentive examination, and supported by good computations of the theoretical results, altogether evincing much research and skill. If I have been compelled to differ from him as to the comparison instituted with earlier experiments, and in the actual results observed in the last case, I hope that he will regard these remarks in no other light than as conspiring with his own researches in the common cause of scientific truth.

P.S.—In immediate connexion with the foregoing remarks, I have now to add, that I have received from Prof. Airy the following statement relative to these experiments; and I gladly avail myself of his permission to lay it before the readers of this Journal.

"I have repeated several times Newton's experiment on knife-edges, placed at a very small distance apart. As my object was to verify Newton's observations, and to verify Fresnel's theory, as far as calculations can be applied, I carefully avoided the use of curved edges, which introduce insurmountable difficulties into the mathematical investigation. My aperture was made by two rectilinear cheeks, adjustible by a screw. The edges of the cheeks are very truly worked. I have tried holes for admitting the sun's light into the room, of various breadths, from about $\frac{1}{4}$ inch (which was the breadth of Newton's hole in this experiment,) downwards. The aperture between the cheeks has in general been varied through all the values between $\frac{1}{10}$ inch and $\frac{1}{1000}$ inch or less (the latter measure is by estimation). My distances from the hole to the aperture, and from the aperture to the screen, have always been 30 inches; some of the observations have been made by myself alone, and others in the presence of another person. In every instance the centre of the image thrown on the screen has been the brightest part. There is one circumstance which would easily account for a careless observer supposing that the centre was dark: when the aperture is large ($\frac{1}{50}$ inch for instance), the centre is very bright. If now the aperture is suddenly contracted, and the central light consequently much diminished, the centre seems for a short time black. But this is merely the nervous effect of surprise on the eye; for on allowing the eye to rest for a few seconds, it becomes evident that the centre is brightest. If instead of contracting the aperture from a wide opening to a narrow one, it be gradually opened from a very narrow interval to a wide one, it is evident to the eye through the whole change that the centre is bright.

Newton's measures of the distances at which the first black bar is formed appear to be bad. According to theory, when the breadth of the aperture is very small, the distance should vary as the square of the breadth. This proportion is very closely preserved in Biot's experiments with very narrow apertures, but is not at all maintained in Newton's experiments. Newton's measures are therefore inconsistent with Biot's. Now Biot's methods were in every respect superior to Newton's. Among the principal causes of superiority I may mention the use of a semitransparent screen, which could be observed behind, and the use of light almost strictly homogeneous. It is also worthy of notice that Newton's experiments were not intended for publication; that Newton avers in his preface, 'that he had not repeated some until he had satisfied himself about all their circumstances;' and that he informs us that they 'were put together out of scattered papers.'

Very pressing occupations have prevented me, for the present, from repeating these experiments with accurate measures; the only observation with measures which I have obtained is the following: The distances from the image of the sun, formed by a lens, to the aperture, and from the aperture to the eye-piece, being each 60 inches, and the breadth of the aperture 0·07 inch, the centre was a bar of diluted blue inclosed by two bars of red-brown. This agrees well with Fresnel's theory and numbers; for it appears on calculation that the extreme blue is rather brighter at the centre than on each side, and that the bright yellow is much less bright at the centre than on each side.

LXXI. *Narrative of Experiments made with the Seconds Pendulum, principally in order to determine the hitherto unassigned Amount of the Influence of certain minute Forces on its Rate of Motion. By Mr.* James Scrymgeour.

[Concluded from p. 350.]

THE following experiments were made in order to determine the nature of the effect of the proximity of any body to the clock pendulum, and also to ascertain whether the current of air generated by the motion of the pendulum could be reduced, or prevented from being formed. The detached pendulum was mounted with two brass balls, weighing in all about 3 pounds; the larger being 2 inches in diameter, and the other somewhat less. Two pieces of deal were placed parallel to the motion of the pendulum, one on each side of the balls, at the distance of about $\frac{1}{4}$ of an inch; but the space at each end of the boards was left open. The detached pendulum being previously adjusted to the clock pendulum, at an extent of vibration of 2°, the pendulum was put in motion, $\frac{1}{10}$th of a degree beyond 2°; and though the extent of vibration decreased by 1° in 15 minutes, no difference in the times of vibration was perceivable. When the boards were placed at the distance of $\frac{1}{2}$ an inch from the balls, the extent of vibration decreased by $\frac{8}{10}$ths of a degree in the same time; and when they were removed altogether, it decreased by $\frac{7}{10}$ths of a degree.

The experiment with the boards at the distance of $\frac{1}{2}$ an inch from the balls was repeated, with this difference, that boards were placed at the ends of the former within $\frac{1}{2}$ an inch of the extent of vibration on each side of the vertical. Though the space in which the vibrations were performed was thus inclosed, no perceptible difference was observed in the times of vibration, owing to their rapid decrease; if there were however any difference, it must have been very small.

The preceding experiments prove that the vibrations of a clock pendulum are not altered in their time, though they may be altered in their extent by the approach of any body, provided the impulse be given in the middle; that is, as much in descent as in ascent, with the exception of the alteration which results from the diminished extent of vibration. If, however, the impulse be given in ascent, the times of vibration will be slower, and if given in descent, they will be quicker than if given in the middle; and these different conditions will alter the times only in proportion to the amount of friction or of resistance.

In the next set of experiments with the clock pendulum, the pallets were adjusted so as to give three fourths of the impulse in descent. The suspending spring was also adjusted so as to cause the long and short vibrations to be performed in the same time, when in a detached state, and when furnished with a leaden oval bob of 7 pounds weight. The clock was fitted with pendulum and other apparatus in the vessel in which the exhaustion was to be made. On the top of this vessel there was a glass receiver about a foot deep and 7 inches wide. In the receiver was placed the clock, furnished with a small temporary dial, to show minutes and seconds.

By this apparatus, the time and the proper adjustment of its different parts, such as the pendulum, suspension, &c. could be observed.

The glass vessel above mentioned is the same that was employed in all the experiments made with the pendulum vibrating *in vacuo*, or in a state of considerable exhaustion.

Set, No. 7.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	+12·3s	2·23°
In exhaustion, 6¼ in.	+11·6	2·36
Ditto 13¼	+ 9·2	2·36
Ditto 15½	+ 7·6	
In air	+ 7·0	2·23
Ditto	+ 5·6	2·23
Ditto	+ 5·5	2·23
Ditto	+ 5·1	2·23
Ditto	+ 4·2	2·23
Ditto	+ 4·7	2·23
In exhaustion, 8 in.	+ 7·4	2·35
Ditto 16½	+ 7·3	2·35

This set of experiments is not very satisfactory, owing to the change of rate occasioned by the weakening of the suspending spring. The difference between the results of the experiments in air and in exhaustion is small, being only about 2·5 seconds for nearly the same extent of vibration; whereas the results of those in air ought to have been about 6·5 seconds slower.

This discrepancy may be accounted for, by attributing a small portion of it to the effect of the scapement, and the rest to the strength of the suspending spring, which caused the pendulum to return more quickly, when at the limit of the vibration, than if it had been weaker. The difference between the extents of vibration in air and in exhaustion is only ·14 of a degree, which would make no difference in the times of the vibration, as the spring was adjusted.

In the above experiments the maintaining power was 5 pounds; now, with the same pendulum vibrating on a knife-edge, it would require only 2½ pounds to cause the pendulum to vibrate to the same extent; consequently, in the former case, the power employed was double what was necessary to produce the effect. This power being applied to the pendulum, had the effect therefore of bending up the suspending spring, which would thus have more power in its return in the first increments of descent, and would be less retarded by the current, than when vibrating on a knife-edge.

As the spring would cause the pendulum to return more quickly from the highest point in the arc of vibration, it might be supposed that the vibrations would be performed in a less time, or that they would be the same as those of a shorter pendulum. This is so far the case, that the pendulum must be made about half an inch longer, when vibrating by a strong suspending spring, than when vibrating on a knife-edge, in order that the vibrations may be performed in the same time.

In the next set of experiments, a suspending spring was employed, which was thinned so much as to be just sufficient to bear the weight of the pendulum. This was done as a preparatory step, previous to determining an extent of vibration, such that the amount of the increase of vibration *in vacuo* would be equivalent to the amount of the loss occasioned by the current generated in air. A cylindrical bob, of 8 pounds weight and 2 inches diameter, was first employed, which, it was supposed, would have nearly the same momentum or specific gravity, as the common mercurial pendulum with its glass vessel. A piece of thin tin-plate was fixed at the lower end, across the centre of the bob, and projecting downward $1\frac{1}{4}$ inch; and the pendulum was so constructed, that the bob could be turned by a swivel, without altering its length or removing it from the vessel. By this means the position of the piece of tin-plate could be altered, so as to be either edgewise or broadside to the path of the pendulum. The piece of tin-plate would thus produce a greater or less resistance, as well as a greater or less counter current to the motion of the pendulum, with the same weight, accordingly as it was placed broadside or edgewise.

Set, No. 8.—With the tin-plate placed broadside to the path of the pendulum, the impulse being given equally in ascent and descent.

Experiments.	Loss in 24 Hours.	Extent of Vibration.
In air	-6^m 19^s	2·0°
Ditto	−6 19·6	2·0
Ditto	−6 18·6	2·0
In exhaustion, 6 in. . .	−6 15·8	2·8
Ditto $13\frac{1}{4}$ . . .	−6 16·2	
In air	−6 19·2	2·0
Ditto	−6 19·5	2·0

Set, No. 9.—With the tin-plate turned edgewise to the path of the pendulum.

Experiments.	Loss in 24 Hours.	Extent of Vibration.
In air	-6^m $18·5^s$	2·2°
Ditto	−6 19·0	2·2
In exhaustion, 6 in. . .	−6 18·5	2·85
Ditto $19\frac{1}{2}$ . .	−6 19·2	2·6
In air	−6 19·2	2·2

Set, No. 10.—With an oval leaden bob of 8 pounds weight.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	+10·6^s	2·7°
Ditto	+10·1	2·7
In exhaustion, 6 in.	+ 9·1	3·15
Ditto 13½	+ 9·5	2·9
In air	+ 9·0	2·7
Ditto	+ 9·5	

Set, No. 11.—With the pallets set so as to give the impulse all, or very nearly all, in the ascent.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	+ 6·6^s	2·63°
Ditto	+ 6·2	2·63
Ditto	+ 6·4	2·63

Set, No. 12.—With the pallets set so as to give three fourths of the impulse in descent.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	+ 7·3^s	2·8°
Ditto	+ 7·6	2·8

For the experiments in Nos. 11 and 12, it was necessary that the pallets should be of a peculiar shape. It was formerly stated that they were ground hollow in the flanches for the purpose of giving the impulse in descent. The pallets were sufficiently broad to admit of two separate actions of the wheel; one half of the thickness of each, on the same side, was ground flat in the way usually done for a dead scapement. It was also stated that the pallets were jointed concentrically on the axis. The central brass collet or socket on which they were jointed, was fixed to the arbor by a pinching screw. By this means, the pallets could be moved along the arbor, and thus the action of the wheel could be shifted from the one to the other at pleasure, and the wheel could also be put out of action altogether, if required. In order to give the whole impulse in ascent, the pallets required to be pitched deeper in the wheel than usual; and in order to clear the teeth properly, they required to be made thinner or shorter in the flanches; this caused a corresponding loss of power.

The portion of the pallets which was employed to give the impulse in descent, was a little deeper or thicker than the portion used to give the impulse in ascent. By this means, a greater impulse was given in the former case than in the latter; and thus the greater extent of vibration in the experiments of

No. 12, as compared with those of No. 11, is accounted for. If 1·5 second be allowed for the difference in the extents of vibration, and if to this the 1 second actually shown by experiment be added, the amount 2·5 seconds will be the difference between the two methods,—the one of giving the whole of the impulse in ascent, and the other of giving three fourths of it in descent.

When the pendulum was suspended by a strong adjusted spring, there was no less than 5 seconds of difference between the results of the two methods.

In consequence of this alteration on the pallets, the pendulum had been lengthened so as to produce a difference of 4 or 5 seconds in 24 hours, which accounts for so great a difference in time as that shown between the experiments of Nos. 10 & 11.

Set, No. 13.—With the same pendulum bob as before, the impulse being given equally in ascent and descent.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	$+12{\cdot}7^s$	$1{\cdot}8^\circ$
Ditto	+12·6	1·8
In exhaustion, 6 in.	+18·0	2·06
Ditto 14	+16·3	1·9
In air	+13·0	1·8
Ditto	+12·5	1·8

Set, No. 14. — With a round bob of 8 pounds 2 ounces in weight, the impulse being given in the middle.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	$+\ 4{\cdot}6^s$	$1{\cdot}43^\circ$
Ditto	+ 4·8	1·43
In exhaustion, 6 in.	+14·6	1·8
Ditto 13·5	+13·5	1·6
In air	+ 4·8	1·43

Set, No. 15.—With the pendulum vibrating on a knife-edge, and the same maintaining power as above.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	$+\ 4{\cdot}1^s$	$1{\cdot}58^\circ$
Ditto	+ 4·7	1·58

The comparison of these experiments with the preceding, shows the increase in the extent of vibration arising from the substitution of the knife-edge for the suspending spring; or in other words, the loss in the extent of vibration arising from the use of such a spring, though very slender, instead of a knife-edge.

Set, No. 16.—With knife-edge and round bob of 8 pounds, the impulse being given in the middle.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	+ 0·5s	2·26°
Ditto	+ 0·1	2·26
In exhaustion, 6½ in.	+ 0·1	2·84
Ditto 16¼ . . .	+ 0·7	2·50
In air	+ 0·4	2·26
Ditto	+ 0·4	

Set, No. 17.—With knife-edge, and a greater maintaining power.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	— 4·2s	2·65°
In exhaustion, 6½ in.	— 4·4	3·35
Ditto 15½ . . .	— 3·6	3·00
In air	— 4·0	2·65
Ditto	— 4·0	2·65

Set, No. 18.—With an oval bob of 8 pounds weight.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	+ 3·2s	2·55°
Ditto	+ 3·6	2·55
Ditto	+ 3·5	2·55
In exhaustion, 6¾ in.	+ 3·7	3·00
Ditto 16½ . . .	+ 4·3	2·80
In air	+ 3·8	2·60

Set, No. 19.—With an oval pendulum 12½ pounds in weight, and the same maintaining power as above:—

Extent of vibration, 2°·1.

Set, No. 20.—With an increased maintaining power.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	+ 2·8s	2·75°
Ditto	+ 2·1	2·75
In exhaustion, 6½ in.	+ 2·4	3·10
Ditto 16½ . .	+ 2·7	3·00
In air	+ 2·8	2·75
Ditto	+ 2·2	2·75

Set, No. 21.—With a diminished maintaining power.

Experiments.	Gain in 24 Hours.	Extent of Vibration.
In air	+ 5·9s	2·3°
Ditto	+ 6·3	2·3
In exhaustion, 6¾ in.	+ 7·4	2·7
Ditto 18 . . .	+ 7·0	2·6
In air	+ 6·2	2·3
Ditto	+ 6·0	2·3

In these experiments, the exhaustions were carried as far as the pump was effective; the float-gauge generally gave indications within two or three tenths of the barometer, but the exhaustions were probably rather greater than what were indicated, as the surfaces of the mercury could not be seen, iron pipes being used for the gauge. The mean exhaustions detailed in the various experiments were taken at the end of 12 hours, and extended to 24, in the tables. The number of pieces of which the vessel was composed, and the necessity of frequently opening it, rendered it a matter of great difficulty to keep it air-tight. At the place where the pendulum bob traversed, a horizontal section of the vessel was 13 inches long and 9 inches broad.

From these experiments, it appears that if a pendulum with a mercurial or a cylindrical bob be made to vibrate at the extent of 2°·2 or 2°·3 from the point of rest, the time of its vibrations will not be altered by changes in the density of the atmosphere; and the same observation holds good with a common leaden bob, vibrating to an extent of 2°·6 or 2°·7 The reason of this is, that the gain resulting from the weakened current is compensated by the loss arising from the increase of vibration. The following conditions are, however, necessary for the production of this result: the impulse must be given in the middle (that is, one half in descent, and one half in ascent); and the pendulum must be suspended by a very thin spring, or on a knife-edge.

Though an adjusted spring would compensate for the loss of vibration arising from increase of friction or decrease of maintaining power, and would be but little affected by changes in the atmosphere, yet it is liable to changes in its rate arising from the weakening of the spring by continued action; at least this is generally the case for several years after it is first set going.

From these considerations, it appears that a weak suspending spring or knife-edge, adjusted so as to admit of an increase in the extent of vibration as the density of the air decreases, and conversely, is to be preferred to others in its application to the pendulum; for, by thus allowing a proportional increase in the extent of vibration, due compensation for changes in the density of the air is obtained; besides, by the use of the thin spring or knife-edge, instead of a thick adjusted spring, the liability to changes in the rate of the pendulum from that cause is removed. If good workmanship be not spared, and a dead-beat scapement with jewelled pallets be employed; and if the impulse be given in the middle, which can easily be done by slightly hollowing the impulse flanches; and if the pendulum, which must at least be 8 or 10 pounds in weight, be firmly fixed,—the maintaining power may be transmitted without any material diminution for a period of several years. The changes which arise from increase of friction, and cause a decrease of vibration, are but small as well as slow in their progress, and they can be easily calculated upon; but the changes which arise from the state of the atmosphere are frequent, and to estimate them properly is a much more difficult task.

Glasgow, 1833.

LXXII. *On separating the Phosphates of Lime and Magnesia. By Mr.* G. O. Rees.*

WHEN the phosphate of lime occurs in urinary calculi mixed with the phosphate of ammonia and magnesia, it is rather difficult to discriminate between them. I have, however, found the following process to answer this purpose perfectly.

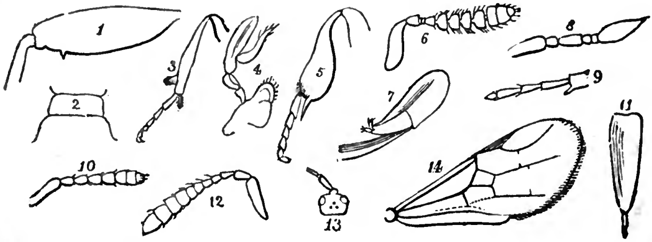
Heat two or three grains of the calculus to be examined to redness, so as to expel the ammonia present, which if allowed to remain would interfere with the future steps of the process by forming a triple salt. The residue is to be dissolved in dilute hydrochloric acid, and a solution of bicarbonate of potassa added in excess; part of the base of the phosphate, whether of lime or magnesia, is now held in solution as bicarbonate, and may be procured as carbonate by filtration and boiling. The carbonate so precipitated must be well washed, in order to free it from the phosphate of potassa, and may then be dissolved in dilute hydrochloric acid; by these means the phosphoric acid is entirely expelled from the earths, and their usual tests now act characteristically. Thus if magnesia be present, ammonia produces a precipitate soluble in a solution of muriate of ammonia; if lime be in the solution, oxalate of ammonia precipitates it; and if both earths be present, both these indications are fulfilled.

* Communicated by the Author.

This property which the bicarbonate of potassa possesses of decomposing the phosphates, is only available in qualitative examinations; as the conversion into carbonate is only partial. The use of the salts of lead in freeing the earths from phosphoric acid requires much time and attention; whereas the process here recommended, occupies but a few minutes, and though not so complete in its effects, answers completely to distinguish the earths when combined with the phosphoric acid. G. O. Rees.

LXXIII. *Descriptions of several new British Forms amongst the Parasitic Hymenopterous Insects.* By J. O. Westwood, F.L.S. &c.*

[Concluded from vol. i. p. 129.]



17. *Monodontomerus*, Westw. *Torymus*† B. a. Dalm. *Torymus?* Walk.

Callimomi Spin. affinis. Differt præcipuè collari majori transverso (fig. 2.) femoribusque posticis crassioribus, nec serratis, subtùs dente unico paullò ante apicem armatis (fig. 1.). Clava antennarum quàm articulis duobus præcedentibus vix brevior. Ramus stigmaticalis ut in *Callimome*. Mesoscutum suturis distinctis.—*Monod. obscurus*, Westw. Viridi-æneus, abdomine suprà chalybeo cupreoque nitenti, subtùs saturatè fulvescenti, segmento basali viridi; femoribus piceis, in medio æneis, tarsis tibiisque fulvis, his in medio obscurioribus; alæ sub stigmate obscuriores stigmate fusco. Antennæ nigræ, scapo piceo-fulvescenti, oviductus abdominis longitudine. Long. Corp. $1\frac{3}{4}$ lin. Variat paullò major, colore fulvescenti subtùs magis diffuso. Ensham, August 1826. Warwick, August 1827.

18. *Mesopolobus*, Westw.

Pachylarthro Westw. affinis. Caput thorace latius, antennæ sensim clavatæ 13-articulatæ, articulo 3tio annuliformi, 4to majori. Mandibulæ 3—4-dentatæ. Palpi maxillares furcati (fig. 4.). Tibiæ intermediæ ferè ad apicem externum lobo parvo triangulari ciliato. Thorax elongato-ovatus. Abdomen parvum angustum depressum. ♀ ignota.—*Mes. fasciiventris*, Westw. Lætè viridis, abdomen nigrum, chalybeo cupreo viridique nitens, fasciâ fulvâ ante medium; antennis fulvis, pedibus flavis, tarsis apice fuscis, tibiarum lobo

* Communicated by the Author.

† Obs. Nomen "Torymus" omninò respuendum.

nigro. Alæ hyalinæ apice areolæ costalis ramoque stigmaticali fuscis. Long. Corp. $\frac{3}{4}$ lin. Coombe, May 1827. Birmingham, August 1827. Windsor, July 1830.

19. *Platymesopus*, Westw.

Mesopolobo Westw. affinis. Differt præcipuè palpis maxillaribus non furcatis articulo 2do magno dilatato 4toque longissimo. Tibiæ intermediæ sensim dilatatæ ferè ad apicem, angulo externo apicali in fasciculum parvum terminato (fig. 5.). Tibiæ anticæ etiam paullò dilatatæ. Abdomen ovatum depressum thorace multò minus; antennarum clava magna. ♀ ignota.—*Plat. tibialis*, Westw. Viridis, abdomen nigrum subcupreo nitens; antennæ fulvæ, basi flavæ, apice fuscæ; pedibus flavis, tarsorum apice fusco, femoribus tibiisque intermediis lineâ fuscâ, his etiam lineâ rubrâ, fasciculo apicali nigro; alarum nervi pallidè fuscescentes. Long. Corp. 1 lin. Coombe, April, May 1827—1828.—Obs. Speciem? majorem è Dom. G. T. Rudd accepi.

20. *Gastrancistrus*, Westw.

Caput transversum thorace latius. Antennæ mediocres apice crassiores 12-articulatæ, articulis 3 et 4 annuliformibus 5—9 cyathiformibus (fig. 6.). Abdomen elongato-ovatum, depressum, apice corniculis 2us recurvis; oviductu exserto, abdominis dimidio longitudine ferè æquante (fig. 7.). Alæ ramo stigmaticali longo clavato. Tarsi pentameri, omnes simplices, pulvillis magnis.—*Gastr. vagans*, Westw. Thorax purpureus, abdomine æneo, basi viride; capite æneo-nigro; pedibus piceis, genubus pallidioribus, antennis nigris. Long. Corp. $\frac{1}{2}$ lin. Coombe, May 1827.—Obs. *Eupelmo* et *Callimomi*, oviductu exserto affinis. Ex illo tarsis simplicibus, ex hoc ramo stigmaticali elongato antennisque differt.

21. *Trichogramma*, Westw.

Agonioneuro Westw. affinis. Caput breve, thoracis latitudine et illo arctè applicatum. Antennæ breves, 6-articulatæ, articulo 1mo longo, 2do brevi gracili, 3tio quàm 2do majori crassiori; 4 et 5 brevibus, 6toque maximo oblongo-ovato apice acuminato (fig. 8.). Thorax ferè quadratus, posticè rotundatus, abdomine longior, scutello magno; abdomen breve, transversum, sessile, thoracis latitudine ferè ad apicem. Alæ anticæ magnæ pilosæ, ramo stigmaticali elongato, pilisque in lineis circiter 12 longitudinalibus positis. Pedes simplices. Tarsi ut mihi videtur 3-articulati, pulvillis magnis (fig. 9.).—*Trich. evanescens*, Westw. Fulvo-fuscescens, abdomine obscuriori, pedibus pallidioribus. Long. Corp. $\frac{1}{7}$ lin. Chelsea, June 11, 1828.—Obs. Omnium *Chalcididarum* minutissimus.

22. *Aprostocetus*, Westw.

Eulopho affinis. Caput thoraxque mediocres. Antennæ 8-articulatæ, articulis 2, 3, 4, et 5 longitudine æqualibus, at sensim paullò crassioribus, articulis 3 ultimis clavam crassiorem formantibus (fig. 10.). Abdomen elongatum, sessile, thoracis latitudine et illo duplò longius, ad apicem sensim acuminatum; oviductu exserto (parte exsertâ tertiam partem longitudinis abdominis æquante (fig. 11.). Tarsi tetrameri.—*Aprost. caudatus*, Westw. Nigro-æneus, abdomine æneo nitido, antennis pedibusque piceis, tarsis genubusque pallidioribus. Long. corp. ovid. incl. $\frac{5}{6}$ lin. Coombe, May 1827.

23. *Embolemus*, Westw.

Caput suprà transverso-quadratum cum tuberculo antico (fig. 13.), in quo insident antennæ, quæ sunt 10-articulatæ, corpore longiores, filiformes, nudæ, articulo 1mo crassiori, 2do brevissimo, reliquis elongatis. Palpi maxillares longi, penduli. Thorax elongato-ovatus. Alæ superiores cellulâ 1 marginali unâque discoideâ rhomboideâ, cellulæ aliæ quædam etiam indicantur (fig. 14.). Abdomen ovatum, convexum, posticè acuminatum. Pedes longi, graciles, femoribus crassioribus.—*Emb. Ruddii*, Westw. Niger, abdomine nitido, pedibus piceis, femoribus tibiisque in medio obscurioribus; alis subfuscescentibus. Long. Corp. $1\frac{1}{3}$ lin. Exp. alar. $3\frac{1}{2}$ lin. Yorkshire, Rev. G. T. Rudd.—Obs. Alarum nervi secundum typum *Alysiidarum* disponuntur, at antennæ caputque tuberculatum affinitatem cum *Proctotrupidibus* quibusdam demonstrant.

24. *Hemisius*, Westw.

Telenomo Hal. affinis. Caput thoracis ferè magnitudine. Antennæ in tuberculum parvum anticum positæ, longæ, ad apicem clavatæ, articulis 11-discretis, 3tio, 2do minori, clavâ 4-articulatâ (fig. 12.). Thorax convexus, rotundatus; alæ thorace toto vix longiores, ramo stigmaticali elongato, clavato, in alæ discum obliquè descendenti. Abdomen ovatum, subdepressum, segmento 2do maximo.—*Hem. minutus*, West. Niger, abdomine piceo-nigro, pedibus flavescentibus, antennis piceis basi pallidis. Long. Corp. $\frac{1}{4}$ lin.

The Grove, Hammersmith, April 24, 1833.

LXXIV. *On the Modulus of Elasticity of Gold. By* B. Bevan, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IT is something remarkable that while the modulus of elasticity and stiffness of a number of the common metals have been investigated and determined, that of gold, which is considered the most valuable metal, should have been neglected, or overlooked. To supply this defect I have lately obtained a piece of pure gold, and have ascertained the measure of its elastic force to be about 11,690,000 pounds to the square inch, or 1,390,000 feet when recently drawn into wire, or about 1,000,000 feet lower than the modulus of platinum, and 5,000,000 feet less than that of plate-glass. I suspect, however, that the modulus of gold as alloyed for coinage, is something higher than that of pure gold; but at present I have not been able to procure a piece of suitable dimensions to demonstrate it.

Those who are in the daily habit of taking gold coin soon acquire a knowledge of the proper sound or note given upon striking a piece of money upon a table or hard substance: this well-known though undefined note or sound depends upon the modulus of elasticity of the metal, as well as upon the diameter and thickness. A piece of coin, of the same dimensions, both as to diameter and thickness, of silver, will give a note about a major fifth higher than one of gold, when a similar coin of copper will give a note an octave above that of gold; and if made of steel would give a note a minor third above that of copper.

To convince any person of the influence of the modulus of elasticity of metals on the musical sound produced by them, let them have a tuning-fork made of bell-metal, of the same length and thickness as a fork made of steel: they will find the note given by the bell-metal fork a fifth lower than the note from steel.

The stiffness of gold, against taking a permanent set or flexure, I find about $\frac{2}{5}$ths of that of brass; $\frac{1}{4}$th of the stiffness of wrought iron, and $\frac{1}{8}$th of that of untempered steel.

I am, Gentlemen, yours truly,

B. Bevan.

LXXV. *On the Theory of Magnetic Electricity.* By Mr. Wm. Sturgeon, *Member of the British Association for the Promotion of Science; Lecturer at the Hon. East India Company's Military Academy, Addiscombe, &c. &c.*

[Concluded from p. 371.]

In the positions which I have advanced for exhibiting the *proximate* laws of magnetic electricity, I have carefully avoided every consideration that could possibly embarrass the mind, or prevent them from being understood. They would *virtually*, however, have been but very little affected by taking into account the magnetism of the metal as an intermediate agent in the process of excitation; but they are much simplified by omitting those *remote* laws, which would be better exhibited separately, and as a distinct class, which may be admitted, or rejected, at pleasure, without affecting the calculations of the experimenter.

Position 7, with its illustrations, will explain the *apparent* anomalies in the direction of the electric current in wires, when excited at various parts of the surface of the magnet; and will show that, with respect to the exciting *polar magnetic lines*, the direction of the current is constantly the same.

The electrical vortices also, both simple and compound, as I have discovered them to be exhibited by plates and discs, whether rotating on an axis, or moving in right lines, may very easily be explained by the same position.

The simple vortex represented in fig. 24. (Plate I. vol. i.) may be regarded as the ring with its exciting *polar lines*, in fig. 7 and 8. Pl. I. of the present volume, in an inverted order; having the marked ends of the *exciting lines* downwards instead of upwards, which is the case in all the figures of the former plate.

In fig. 23. (Plate I. vol. i.) the ring may be supposed to be advancing with its *external* surface against the exciting *polar magnetic lines.* Hence the direction of the current in the ring will appear to be reversed; though, with regard to the exciting *lines* which called it forth and gave it motion, the direction remains constantly the same.

The compound vortices in fig. 16, 17, 21, and 22. (Plate I. vol. i.) are easily explained in the same manner, by considering each vortex as a simple ring. In fig. 16 and 21. the *interior* surface of the supposed ring strikes the *magnetic lines* in the vortex on the right-hand side of each figure. But the *exterior* surface of the ring receives the exciting impressions in the vortex represented on the left side of each figure. The compound vortices represented in fig. 17 and 22. are explained in the same way, by considering them to be receiving the exciting impressions in the contrary order.

By taking advantage of this beautiful law, I have been enabled to exalt the force on the edge of a revolving disc to a considerable extent, as will be shown by the following experiment.

Experiment 22.—Let fig. 10. (Plate I.) represent a disc of copper, revolving in a vertical plane between the poles of two horse-shoe magnets, situated as in the figure, having the *north pole* of one magnet and the *south pole* of the other on the same side of the disc.

With this arrangement the electric forces will be distributed as indicated by the small arrows in the interior of the circular plate; when it is rotated in the direction of the large exterior arrow. By this distribution the resulting forces in the upper and lower edges of the plate have the same general direction. In the lower edge the aggregate force or current is in the same direction as that in which the plate revolves; but in the upper edge the aggregate current is in the opposite direction to that of the revolving plate. By reversing the rotatory motion, the whole systems of currents become reversed also.

There is a very great advantage by this disposition of the magnets and the copper disc; for not only is the force in the upper and lower edges very much exalted, but by the arrangement of the magnetic poles they very nearly neutralize each other's effects on the needle. To accomplish this point the most decidedly, which is an important consideration in the experiment, the exciting magnets ought, as nearly as they can possibly be procured, to be of the same power.

If, instead of a single disc, the compound disc, described in *Experiment* 21. in my last communication, be employed, the excited forces are still more powerful. A large straight needle placed on a pivot either above or below, with a slight directive tendency in the plane of the plate, will, with a very moderate uniform velocity of the latter, become steadily deflected at right angles to the edge or plane of the revolving disc. Indeed, the needle, although at nearly two inches distant from the edge, is very frequently thrown several times round on its pivot by a sudden motion of the disc.

The line of greatest energy in the area of the disc by the arrangement in fig. 10, is in that diameter which joins the magnetic poles; and its general tendency is in the direction of the straight arrow, but becomes inverted by inverting the motion of the plate. When one magnet only is employed, as in fig. 16 and 17 (Plate I. vol. i.), the line of greatest energy in the area of the disc is always a curve, unless the plate be very small.

By looking over Mr. Faraday's paper, I find that amongst other ingenious arrangements, he has also employed a disc of copper in some of his very interesting experiments; but the arrangements with that gentleman's apparatus are very different to those of mine, already described.

Mr. Faraday has given to one of his revolving discs the title of "a new electrical machine;" and as the deflections which he obtained by this apparatus were by the employment of a delicate multiplying galvanometer, and those which I have described were obtained by a heavy needle, without any multiplying apparatus whatever, it may perhaps be interesting to some readers, if we were to bring into one view the results obtained by Mr. Faraday's "new electrical machine," and those which I have shown to be produced by my comparatively old one.

Besides the delicate multiplying galvanometer which Mr. Faraday has described, he also states that he employed, what I believe to be the largest artificial magnet in the world,—the compound magnet belonging to the Royal Society of London; "composed of about 450 bar magnets, each fifteen inches long, one inch wide, and half an inch thick, arranged in a box so as to present at one of its extremities two external poles. These poles projected horizontally six inches from the box, and were each twelve inches high and three inches wide. They were nine inches apart; and when a soft iron cylinder, three quarters of an inch in diameter, and twelve inches long, was put across from one to the other, it required a force of nearly one hundred pounds to break the contact*. (Phil. Trans. of the Royal Society of London, for the year 1832. Part i. page 135.)

* There is a material difference in the proportions of magnitude and power of this magnet, and of that which I described in the Phil. Mag. and Annals for March 1832. Here are 450 bars, which collectively weigh at

With this magnetic force, and the assistance of a galvanometer which multiplied the electric force more than fifty times, "a permanent deflection of the needle of nearly 45° could be sustained."

With my simple electrical machine, excited by a magnet of about three pounds weight only, and a needle, supported on a pivot, either above or below the edge of the revolving disc, a permanent deflection of more than 40° can be exhibited. And when two such magnets are employed, as in fig. 10, the needle may be kept steadily deflected at right angles to the plane of the disc.

From this simple statement of facts, we readily perceive that the apparatus of Mr. Faraday exhibits but a very small portion indeed of the excited force in the disc, and leaves in complete obscurity the finest application of that force ever exhibited on the magnetic needle.

The electric force which may be led or conducted by a wire from a revolving disc may be very much exalted by taking advantage of the distribution accomplished by the arrangement of magnets exhibited in the following experiment.

Experiment 23.—Let the disc revolve between the poles of two horse-shoe magnets, having both the *north* poles on one side, and consequently both the *south* poles on the other side of the disc, as in fig. 11. (Plate I.) In this case the four systems of forces which flow over the surface of the disc give two resultants in the same diameter. When the disc revolves in the direction of the exterior arrow, those resultant forces will run from between the poles of both magnets towards the centre or axis of motion, where they meet. From the axis of the disc, a portion of those forces may be led off by one or more wires at pleasure. The resultant forces will be reversed by reversing the direction of the revolving disc.

When four or more magnets are similarly arranged on diameters of the revolving disc, several resultants are driven to or from the axis or centre. By this means the force led off is very much increased. No application of magnets to revolving

least 7 cwt. The power of this gigantic magnet on the iron rod is only about one hundred pounds, or not quite $\frac{1}{7}$th of its own weight. This force, however, must necessarily be much less than the magnet is capable of exerting on a proper cross piece or lifter; but it is not likely from this fact, that it is capable of supporting its own weight. The horse-shoe magnet which I described weighs between nine and ten pounds; and its lifting power equals one hundred and twenty pounds, or about twelve times its own weight.

discs, however, can drive off through wires the whole force excited.

Cylinders properly mounted, with respect to the exciting *magnetic lines*, offer a much more efficient apparatus than discs for driving a continuous current through conducting wires. I have made some apparatus upon this principle, but must defer the description till another opportunity.

When a sudden and momentary current is to be exhibited, no mode of excitation hitherto discovered can be employed with greater advantage than that of suddenly making and annihilating a temporary magnet of soft iron, inclosed in a spiral of copper wire,—a mode which I believe was first introduced by Mr. Faraday in some of his experiments for deflecting the magnetic needle; and which, in the experiments of M. Nobili, and afterwards, in this country, in those of Mr. Saxton and Mr. Forbes, has been so successfully employed in exhibiting the electric spark.

By this mode of excitation the whole of the exciting *polar magnetic lines* are called forth simultaneously, and with a velocity not easily accomplished any other way; and in directions the most suitable to produce the greatest effect.

I have only to add in this place, that whatever claims may have been made by others to the first discoveries of this branch of science, I apprehend that the experiments and explanations hitherto produced in this series of communications can leave very little difficulty in placing those discoveries in the proper quarter. My vibrating disc (Phil. Mag. and Annals, N.S. vol. xi. Plate III. fig. 3.) has, I perceive, already been recognised as the first instrument which exhibited phænomena which could not be reconciled to the hypothesis advanced upon the experiments of Arago. And my rotating disc is not only the first "machine" of this class that was ever made, but is at this time the most efficient of its kind. The deflections of the needle exhibited by the former apparatus led to the construction and employment of the latter. And although I did not, in my first communication, advance a direct assertion that the excited force in the discs was the *electric*; my statements, to say the least of them, were favourable to the supposition,—perhaps as much so, as the nature and results of my experiments, and a due regard to propriety, would permit. My drawings, however, amply testify that my real views of the character of the force were perfectly correct. It is however due to other experimenters that I should state, that I never employed *wires* in my experiments in magnetic electricity until I heard of them being employed by Mr. Faraday. And the first time that I witnessed the electric spark by magnetic excitation, it was shown to me by Mr. Watkins, in his shop at Charing-cross, some considerable time after it had been shown in London by Mr. Saxton, with a similar apparatus.

LXXVI. *Results of the Repetition of* Mr. Potter's *Experiment of interposing a Prism in the Path of Interfering Light. By Professor* Airy*.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I HAVE lately had several opportunities of repeating, under favourable circumstances, Mr. Potter's experiment of interposing a prism in the path of interfering light, and am able to assert positively, as an experimental fact, that the gradual displacement and ultimate disappearance of the centre of the fringes take place in the manner which I stated as a consequence of theory; namely, that on receding from the prism, the *fringes* remain stationary; while their character changes, in such a manner, that the *centre of fringes* passes gradually and rapidly from the centre of the mixture of lights to its border.

The apparatus which I have used consists of an eye-piece, with a wire fixed in its focus, attached to a support which slides on a bar that is placed in a position parallel to the path of the light after refraction at the prism. By proper adjustment of this bar, the wire may be kept steady upon one of the fringes while the eye-piece is drawn from contact with the prism to the greatest distance at which the fringe is visible. In this manner I have kept one fringe under the wire, with the certainty that, though its colour has altered, it has not deviated half the breadth of a fringe; while the centre of fringes has gradually moved through the space occupied by twelve double fringes.

I have made the experiment with light of various degrees of heterogeneity, and in all cases, as far as I could judge, the displacement of the centre of fringes was the same at the same distance; which is also a result of theory. It is necessary to observe that, when the light is nearly homogeneous, the number of visible fringes is so much increased that it is difficult to fix precisely on the centre of fringes. The light was coloured by the use of five different red glasses (one of which made the light nearly homogeneous) and of one green glass.

I am, Gentlemen, your obedient Servant,

Observatory, Cambridge, May, 16 1833. G. B. Airy.

* See our Numbers for February, March, April and May.—Edit.

LXXVII. *Remarks upon an Optical Phænomenon, seen in Switzerland.* By H. F. Talbot, *Esq. M.P. F.R.S.**

In the Number of the Philosophical Magazine for November (page 332) is an account, by Professor Necker, of a pretty optical phænomenon, seen in Switzerland, when the sun rises from behind distant trees standing on the summit of a mountain. The Professor adds, that he is surprised it has never been noticed before.

I am happy to be able to bear testimony to the accuracy of his description of this phænomenon, having observed it myself with great attention in the summer of 1823, with the view of discovering its cause. The place of my observation was in the Val Levantine, at the foot of a cliff about a thousand feet high, whose summit was fringed with pine trees:—it was about the middle of the day, and the sun was very elevated. By approaching the cliff, or retiring from it, I could make its summit conceal the sun's disk or not, at pleasure, so that I could observe the appearances which took place with great facility. I observed with a telescope, of which Mr. Necker makes no mention; but I think it is absolutely necessary, in order to see the full beauty of the phænomenon.

When the sun is about to emerge from behind the crest of pine trees on the cliff's summit, every branch and leaf is lighted up with a silvery lustre of indescribable beauty. But it will be seen, by observing the trunks of the trees and the larger branches, that this silvery light forms only a *margin* to every object: it is only their *outline* which is luminous. Of course this cannot be discerned with respect to the smaller and more complicated objects (such as the foliage), which therefore appear altogether luminous. The birds, as Mr. Necker very truly describes, appear like flying brilliant sparks: others are seen occasionally, smaller than these, which may in all probability be insects or butterflies.

With regard to the cause of this appearance, I have no hesitation in ascribing it to diffraction. It may be seen not only in Switzerland, but to a certain extent in any country, by observing with a telescope a distant building from behind which the moon is going to emerge. Just before the emersion the outline of the building acquires a bright silvery appearance. But the Swiss phænomenon, which is a very striking one, cannot be well observed except in a mountainous country.

* Communicated by the Author.

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

[Continued from p. 283.]

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

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

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
52	247	...	January	Corvus	Chinese Records.	Seen during 156 days.
53	248	...	April	Near the Pleia.	Chinese Records.	Seen 42 days.
54	—	C?	August	Crater, Corvus	Chinese Records	
55	251	...	December	Pegasus	Chinese Records.	Seen 90 days.
56	252	...	March	Orion	Chinese Records.	
57	253	...	December	Near η Virginis	Chinese Records.	Seen 190 days.
58	255	...	January	......	Chinese Records.	
59	257	...	Nov. Decemb.	Virgo	Chinese Records.	
60	262	...	December	Feet of Virgo.	Chinese Records.	Seen 45 days.
61	265	...	June	Cassiopeia	Chinese Records	Seen 12 days.
62	268	...	February	Corvus	Chinese Records.	Of a pale blue colour.
63	269	...	October	Near the North Pole	Chinese Records.	
64	275	...	January	Corvus	Chinese Records.	
65	276	...	June	Libra	Chinese Records.	
66	—	...	July	Near Arcturus	Chinese Records.	
67	—	...	August	Crater, Hydra to Urs. Maj.	Chinese Records.	
68	277	...	January	......	Chinese Recor.	It is possible that these five may be reduced to a smaller number, three of them falling within a month of each other.
69	—	...	April	Musca	Chinese Recor.	
70	—	...	May	Near π Leonis	Chinese Recor.	
71	—	...	June	....	Chinese Recor.	
72	—	...	August	....	Chinese Recor.	
73	278	...	June	Gemini	Chinese Records.	
74	279	...	March	Hydra	Chinese Records.	
75	—	...	April	Near π Leonis	Chinese Records.	
76	281	...	September	Hydra	Chinese Records.	
77	—	...	December	Leo	Chinese Records.	
78	283	...	April	....	Chinese Records.	
79	287	...	......	Sagittarius ..	Chinese Records.	Seen 10 days.
80	290	...	April	Orion	Chinese Records.	
81	295	...	May	Between Andromeda and Pisces	Chinese Records.	
82	300	...	December	Capricornus ..	Chinese Records.	
83	301	...	April	Near ω Capric. α Rami	Chinese Records.	
84	302	...	May	.....	Chinese Records.	

Number.	Year of Appearance A. C.	Same as that of	Month or Season when it appeared.	Place or Direction in which it appeared.	By whom mentioned.	Remarks.
85	303	...	April	Paws of Ursa Major	Chinese Records.	
86	305	...	September	Near the Pole	Chinese Records.	
87	—	...	November	Ursa Major	Chinese Records.	
88	324±	C?	....	....	Lycosthenes, &c.	
89	329	...	August	Hercules	Chinese Records.	Seen 23 days.
90	336	...	....	Andromeda	Eutro. & Chi. Re.	
91	340	...	February	Bootes, Virgo, Leo	Chinese Records.	
92	343	...	December	Feet of Virgo	Chinese Records.	
93	350	...	January	Feet of Virgo	Chinese Records.	
94	358	...	July	....	Chinese Records.	
95	363	...	August	Virgo	Ammian. Marc., Chin. Records.	
96	373 } ?	...	March	Capricor., Leo, Virgo, Corvus, Hydra	Chinese Records.	
97	— }	...	April	Libra	Chinese Records.	
98	—	...	October	Pegasus	Chinese Records.	
99	374	...	January	Scorpio, Sagit.	Chinese Records.	
100	375	...	November	....	Amm. Marcellin.	
101	389	...	August	Ursa Major	Mar. Phil. Nicep.	Seen 28 days.
102	390	...	Aug. Sept.	Gem., Ur. Maj.	Prosp. Tyr. Marc. Chin. Records.	Seen 30 days.
103	392	...	....	....	Chinese Records.	
104	395	...	August	Sagit., Aquar. Equuleus	Chinese Records.	
105	400	...	March	Androm., Pisc.	Socrat. Niceph. Sozo., Ch. Rec.	
106	401	...	January	Near δ Cygni	Chinese Records.	
107	402±	...	....	Ceph., Cassio., Ursa Major	Claudian.	
108	415 or 416	...	June	Hercules	Chinese Records.	
109	—	...	June	Hercu., Scorpi.	Chinese Records.	
110	418	...	June	Ursa Major	Chi. Rec., Marc.	
111	—	...	July, Sept.	....	Phil., Chi. Rec.	
112	419	...	February	....	Chinese Records.	
113	420 or 421	...	Spring	......	Chi.Re., Pro.Tyr.	
114	422	...	March	Equuleus	Chr.Pas., Ch.Re.	Seen 10 days.
115	—	...	December	Pegasus	Chinese Records.	
116	423	...	February	Pegas., Andro.	Chinese Records.	
117	—	...	December	Libra	Marc., Chi. Rec.	
118	432	...	....	Leo	Chinese Records.	
119	436	...	June	Scorpio	Chinese Records.	
120	442	...	December	Ursa Major, Aurig., Taurus, Eridanus	Marcel. Idatius, Chi. Records.	Seen during some months.

When it was stated, at page 194 of the present volume, that the best Catalogue of Comets was probably that contained in Delambre's Astronomy, the writer had not seen the one published by Olbers, translated and republished by the late Dr. T. Young, in the Quarterly Journal of Science for 1823, from the first Number of the *Astronomische Abhandlungen* of Professor Schumacher, and to which his attention has been called by that gentleman. This Catalogue, comprising all the comets of which the elements had been computed at that time, is by far the best extant, and, as such, with the warmest acknowledgements to the original editor, will be incorporated in the present compilation.

[To be continued.]

Errata in the preceding parts of the Catalogue.

Page 195,	line 7,	*for*	Year and	*read*	Year of
—— 282,	11,	—	1758	—	1759
—— 282,	12,	—	Year and	—	Year of

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

Report of the First and Second Meetings of the British Association for the Advancement of Science; at York in 1831, *and at Oxford in* 1832: *including its Proceedings, Recommendations, and Transactions.* London, 1833, 8vo, pp. 624; with an engraved Geological Section through Europe.

MR. WHEWELL, when discussing, in his "Report on the recent Progress and present State of Mineralogy," which constitutes a distinguished feature in the volume now before us, the various systems of classification which have of late been proposed in mineralogy, and which have for the most part originated with the mineralogists and chemists of the Continent, remarks, that the "prosecution of details, and apathy or contempt with respect to methods, appears to be a part of the intellectual character of this country. Men here appear to feel no interest with regard to rules and systems till they are so complete, so clearly developed as to principle, their apparent difficulties so far explained, that the general rule will bear a strict application in each particular instance. They are disposed to despise the dim glimmerings of dawning principles, in cases where, though *a* connexion may be probable or certain, *the* asserted connexion is clearly not exact. Our countrymen," he continues, "thus often lose much of the pleasure and honour which belong to those who labour to unfold an obscure and imperfect truth: but yet, on this very account, their discoveries, when made, have a more positive character and a more original tone than they might otherwise possess." Concurring entirely with Mr. Whewell in these representations (though we are far from regarding the peculiarity of character in question as altogether a beneficial quality), we think that they are strikingly applicable, *mutatis mutandis*, to the history of the British Association and the continental meetings which were its precursors, and which, in fact, suggested its establishment. The remarks we have quoted have resulted from comparing the small progress which has been made in this country in systematic mineralogy, with its steady advancement on the Continent; while, on the other hand, the exertions of our countrymen in *mineralogical observation*,—in the examination of details,—have far surpassed those of our foreign brethren in science. That those remarks are equally applicable to other departments of science, the present volume bears ample testimony; and we conceive also, that they may be applied with equal truth to the comparative progress of improvement in every branch of human affairs, in this country and on the Continent; and in particular, as we have observed, to the history of the British Association. The first example of a national periodical assembly of the cultivators of science, in order to promote its advancement, was shown by the philosophers of Germany. The meetings successively holden at Berlin, Hamburg, Heidelberg, and Vienna, clearly indicated the advantages which would accrue to the pursuit of natural knowledge universally, by the adoption of similar measures in this country. The British Association was accordingly established, and we have in this volume the results of its first year's existence. As in so many other instances, the example has been set and the commencement has been made by foreigners, but our own countrymen in adopting the plan, have greatly improved it, and have, almost at once, made it eminently effectual in the promotion of science. That the continental associations have proved, in themselves, highly advantageous to the interests of science, we are happy to testify; but we believe that the benefits which have as yet accrued from them are, take them altogether—except indeed the establishment of the British Association—greatly inferior to those which will arise from the production of the Reports now before us. We believe that such a volume as the present has not emanated from any of the meetings on the Continent, and that no contribution to the welfare of the pursuits of science, which has originated in a direct manner from them, has equalled it in importance. While the philosophers of Britain have been assiduously engaged in the prosecution of the details of science, they have certainly, until within these very few years, shown great apathy or contempt with respect to combined exertion and the methods of promoting the investigation of nature; but the efforts of which the results are before us, have in consequence assumed " a more positive character and a more original tone" than they would otherwise have possessed, or than those which have been made by our scientific brethren abroad.

This work commences with a reprint of the First Report of the Association for 1831, from which we gave ample extracts in the Phil. Mag. and Annals, N.S. vol. xi. p. 225: this is succeeded by the Second Report, for 1832, occupying no fewer than 533 pages, and consisting, for the most part, of Reports on the progress and present state of various branches of science, together with an account of the proceedings of the general meetings of the Association, and of the transactions of its different sections. Of a volume whose contents are so multifarious, it will be impossible to give an adequate account within the compass of a review; we shall therefore confine ourselves to an enumeration of them,—brief characters of such of the Reports as the late period of its publication has allowed us to peruse,—and a few extracts on points of peculiar or present importance.

The Second Report commences with a sketch of the proceedings of the General Meeting of the Association at Oxford in 1832, which is followed, in succession, by the proceedings of the General Committee at that time (including a list of the Officers of the Association,) the Recommendations of the several Committees, and the "Transactions." Appended to the Recommendations of the Committee for Chemistry, &c. is a list of Isomorphous Substances drawn up by Professor Miller, forming a very valuable contribution, at the present æra of the discussion on isomorphism, which seems to assume a more important aspect every day, in proportion to the attention bestowed upon it, and the increasing collision of opposite opinions.

The Transactions of the Association commence with a "Report," by Professor Airy, "on the progress of Astronomy during the present century," which we cannot but regard as forming, in every point of view, one of the most valuable parts of the volume. It is observed in the Preface, by the Officers of the Association who have taken the laborious duty of Editors, that "the want of better information respecting the recent advances and actual state of our knowledge has long been felt in every department of inquiry; and the influence which the Association has been able to exercise, in procuring the supply of this *desideratum*, may be judged of from the declaration of the Professor of Astronomy at Cambridge, who stated at the late Meeting that no inducement but that of such a solicitation as he had received could have impelled him to undertake the task which, in the following pages, he has fulfilled. The ability and industry which have thus been enlisted in rendering a laborious and responsible service to science, prove the efficacy of a system of public invitation in giving incitement and direction to the energies of individuals, and show the existence of a public spirit entirely in accordance with the designs of the Institution."

Prof. Airy's Report extends through sixty-five closely printed pages; and is arranged under the following principal heads:

"I. A short general history of institutions and periodical publications. II. An account of some of the instruments principally in use. III. A statement of the improvements in the catalogues of fundamental stars, including the discussions of the various corrections. IV. An account of the more extended star-catalogues, with the tables for facilitating the corrections. V. Notices upon the measures of double stars, the observations of nebulæ, &c. VI. An account of the principal observations, tables, &c. of the Sun and Moon, the old planets and their satellites. VII. History of the new planets and periodical comets: and of comets generally. VIII. Account of measures whose object is to determine the figure of the earth. IX. General history of physical theories. X. Comparison of the progress of Astronomy in England with that in other countries. XI. Suggestion of points to which it seems desirable that the attention of Astronomers should be directed."

To give any account of so detailed a history of the recent progress of Astronomy as that contained in this Report, is impracticable; but we shall extract a few passages which possess peculiar interest. The Cambridge Observatory, it appears, is devoted especially to the observation of the planets: on this subject Prof. Airy informs us, that

"A vast number of observations of planets is to be found in the Transactions, the Ephemerides, and the astronomical periodicals. Their object however is generally rather confined. The inferior planets are little observed: the superior, little except at opposition. At the regular observatories they have been much neglected. In the *Berliner Jahrbuch* 1816, it is remarked that in two years there were only six observations of planets at Greenwich. The foreign observations are sometimes given without any comparison: sometimes however (especially in the *Milan Ephemeris,*) they are compared with the Tables, and even the equations of condition for correcting the elements are formed (as in *Milan Eph.* 1822). In reflecting on these circumstances, it appeared to me desirable that one set of good instruments should be devoted to the observation of planets: and when the Cambridge Observatory was put under my care, I determined on making the planets my principal object. I hope in a few years to collect a mass of observations directed to this point that will possess great value. I have already obtained and compared with Tables about 1100 right ascensions of planets, besides numerous observations of the sun and moon."

Of the Trigonometrical Survey of Ireland now in progress, we have the following notice:

"The survey of Ireland that has lately been and is now going forward, is, I suppose, in accuracy and in excellence of arrangement, (I am not speaking of the minutiæ of the map, but of the principal triangles, by which the great distances north and south or east and west are to be measured,) superior to every preceding survey. Little is now wanting for the measure of an arc of meridian but the observation of zenith-distances of stars at its extremities. The country is also favourable for the measuring an arc of parallel of considerable extent: and a new method of producing intense light, introduced into practice by one of the gentlemen employed on the survey, will probably give the means of determining the differences of longitude on a long arc without the errors produced by intermediate stations. It is also understood that our Government have long contemplated the repetition or extension of Lacaille's measure at the Cape of Good Hope: and several circumstances lead me to hope that this undertaking, which would perhaps contribute more than any other to our knowledge of the earth's figure, will ere long be seriously taken up. The extension of Struve's arc is in contemplation."

A considerable part of many volumes in the first series of the Philosophical Magazine, and also of many in the second series, is occupied by the records and discussions of the pendulum experiments, which, since Capt. Kater's beautiful application of the convertible pendulum, have been so assiduously and so extensively prosecuted. On this subject, which has thus so often occupied our pages, we have the following remarks by Professor Airy:

"Of pendulum experiments, the most valuable series is that made by Captain Sabine in almost every practicable latitude. Invariable pendulums which had been observed in London (to ascertain the number of vibrations made per day,) were observed in the same manner at all the stations, and again in the same manner on returning to London. In this manner, without ascertaining the absolute force of gravity at any one place, the proportion at different places is found probably with greater accuracy than by any other method. This is the method commonly adopted by the English experimenters. Experiments were previously made at several places in Britain by Captain Kater; and others have been made in different parts of the world by Captain Hall, Sir Thomas Brisbane, Mr. Goldingham, &c. A vast number of most careful observations by Captain Foster, in his last voyage, has been received in England, and is now (I believe) preparing for the press. Advantage has also been taken of our repeated expeditions to the North Seas to observe pendulums at high latitudes. The method commonly used by the French philosophers was, to observe the absolute length of the seconds pendulum at each station: thus they experimented at several stations in France and Italy, in the Mediterranean, and in Britain. An extensive series, however, made in Freycinet's voyage, and a few in Duperrey's, were made with invariable pendulums. In the course of experiments for ascertaining the absolute length of the seconds pendulum by a new method, Bessel found that the correction applied in all former experiments for the buoyancy of the air was defective. This has been fully confirmed by Captain Sabine's experiments in a vacuum; and Mr. Baily has been actively employed in determining, with superior accuracy, the correction that ought to be adopted. This error, however, produces very little effect on the determinations of the proportion of the force of gravity at different places.

"A series of pendulum experiments was made by Carlini, at the Hospice of Mont Cenis, to ascertain the diminution of gravity at the height of a thousand toises. The account of these is given in the *Milan Ephemeris* for 1824. The result obtained for the mean density of the earth agrees pretty well with that generally received; but the changes which experiment has shown to be necessary in the elements of reduction, throw a little doubt upon its value. The mountain Schehallien (on which Maskelyne's observations of attraction were made,) has been surveyed, and some alteration made in the numerical results: the calculations of Cavendish's experiments have also been corrected. See various volumes of the *Phil. Trans.*

"In the theory, no improvement has been made, I believe, since the time of Clairaut. No satisfactory rule has been given for taking into account the elevation of the station: perhaps the considerations suggested by Dr. Young in the *Phil. Trans.* for 1819, may be regarded as the most useful.

"It is generally thought that the measures of arcs give an ellipticity of nearly $\frac{1}{300}$ to the earth; some persons considering it a little greater, and others a little smaller. The pendulum experiments, with Clairaut's theorem, give an ellipticity rather greater, though not without remarkable anomalies."

The interest and importance, as well in a national as in a scientific point of view, of the tenth section of this Report, are so considerable, that we feel we should omit a duty to our readers were we not to transfer it entire to our pages.

"X. In the preceding sections I have endeavoured to give materials for estimating the steps which Astronomy has made in this century, and for understanding its present state, at least in all the important parts. But I cannot forget that the Association which I have the honour to address, while it is a Philosophical Association, is also a British Association, and that while it is anxious to promote science abstractedly, it is also jealous of our national scientific character. I feel therefore that my Report would be incomplete if I did not, in some degree, give means for answering the questions, What has England contributed to the progress of Astronomy? and, How have the knowledge and practice of Astronomy advanced generally in England?

"I fear that the answer to the first of these questions will not be very satisfactory. While I allow that in some important parts of Astronomy we have done much, I cannot conceal that in other parts, especially those which cast a lustre on the conclusion of the last century, and those which are peculiarly distinctive of the present century, we have done nothing.

"A subject so complicated as Astronomy, may be divided in several different ways, and thus different comparisons may be made as to the progress of its various parts. I shall here view the subject in two different manners, and I will assert:—

"First, That in those parts which depend principally on the assistance of Governments or powerful bodies, requiring only method and judgement, with very little science, in the persons employed, we have done much; while in those which depend exclusively on individuals, we have done little.

"Secondly, That our principal progress has been made in the instrumental and mechanical parts, and in the lowest parts of Astronomy; while to the higher branches of the science we have not added anything.

"I must of course refer generally to what has gone before for materials to justify these assertions; but I may here point out a few of the leading facts which have induced me to bring forward these opinions.

"With regard to the first, I can assert that we have contributed more than all the rest of the world to furnish materials for ascertaining the figure of the earth. This praise is to be divided, I suppose, between our Government and the East India Company. Be that as it may, I conceive that nothing which has been done by other nations can be put in competition with the arcs of meridian and parallel in England, the great arc of meridian in India, and the pendulum expeditions of Kater, Foster, Sabine, &c. To some of the latter, objections have been made which are in my opinion groundless; but if they were ever so well founded, they would detract nothing from the merit of originating these expeditions. But these expeditions, though they require care and prudence in the persons who conduct them, demand very little science. The vast improvement of chronometers is entirely due to the encouragement offered by our Government. I may also assert that the observatories depending on our Government are maintained with an extent of establishment which few Governments would be willing to allow. And in speaking of this, I cannot forbear alluding to one Institution, which I hope some future reporter on Astronomy will be able to describe as having been beneficial to the science. The Observatory at Cambridge was built, not from any fund bequeathed of old for the purpose, nor with the assistance of any other body, but partly by grant of the University as a corporate body, when its funds were ill able to support such an expense, and partly by the private subscription of its members. It was built and is to be furnished on a plan which will enable it to stand in competition with any other at home or abroad. Whatever may be its success, none is more creditable to the body which founded it.—Now if we examine what has been done by individual attempts, we shall find it small. We have discussed theories of refraction and aberration, perhaps quite as much as our share in the science requires; but we have done nothing in examining the past state of the heavens, or making it subservient to a knowledge of their future state: the reduction of Bradley's observations was left to a foreigner; the formation of Tables of the Sun and Moon, from British observations, even when the theory was put in a distinct shape, was left to foreigners; and, as if we had determined to leave the present state of the heavens also in obscurity, our own observations have too generally been cast on the world unreduced, with a hope, I suppose, that others would have the zeal to reduce them. The observations that require only moderate instruments, with patience and zeal on the part of the observer, as the discovery and observation of comets, and the observation of the small planets, (which on the Continent have generally been made with unmounted telescopes,) have been little attended to. Of the latter, some observations by Mr. Groombridge, some at Greenwich, and a few by myself, constitute, I believe, the whole amount.

"I will not deny that there are some exceptions to my general assertion, and in one of these my hearers will anticipate me. I think that I can fix on only two discoveries, the results of combined theory and observation, which are original in the present century, and one of these belongs to an Englishman. New planets and periodical comets had been discovered in the last century; abstract theory of every kind and observations of almost every kind had been produced: but the existence of a resisting medium was established in this century by Encke, and the practical prediction of the phases of double stars is due to Sir John Herschel. Nor can I omit to mention Sir Thomas Brisbane and Mr. Baily, and (for several investigations connected with the physics of Astronomy,) Mr. Ivory, and lately Mr. Lubbock. But after every credit has been given to their labours, it will, I believe, be allowed that the part in which England has contributed most to Astronomy, and which is likely to be mentioned with greatest gratitude by future historians of the science, is that in which she has contributed as a nation.

"In proof of the justice of my second assertion, the following remarks may be sufficient. Our instruments I conceive (though a German would not allow it,) to be superior to those of any other nation. The observations at our observatories are conducted, I imagine, with greater regularity and greater steadiness of plan than those of foreign observatories. This, indeed, is the character which gave (in some respects) preeminent value to the Greenwich observations of last century, and which makes those of the present century highly valuable. In the reduction of these observations we begin to fall off. Though Dr. Brinkley has investigated from observations a new Table of refractions, and applied it to his own observations, yet Bradley's Table, known twenty years since to be sensibly erroneous, is still the standing Table of refractions at Greenwich. The discussion of the reduced observations has been, I think, confined absolutely to the proper motion of stars. On one or two occasions a number of observations of the moon have (by order of the Board of Longitude,) been compared with the then existing Tables, but not with a view of improving the Tables. I have had occasion to mention the correction of the elements of the earth's orbit made by myself (from Greenwich observations), and the discovery, in consequence, of a new equation in the perturbations of the Earth and Venus. As far as I have been able to ascertain, this was the first improvement in the solar Tables made by an Englishman since the time of Halley, and the first addition to the solar theory since the time of Newton. From English observations of planets it has been impossible to extract a result, because scarcely any have been made. To show the extent of this deficiency, I will mention a mortifying circumstance that has occurred to myself. In order to verify completely the equation above alluded to, I was desirous of collecting observations of Venus near her inferior conjunction. In examining the Greenwich observations I found that no opportunity of making this observation was omitted by Bradley or his immediate successor Bliss; soon after the accession of Maskelyne it was wholly neglected; and from that time till several years after his death scarcely an observation is to be found: several conjunctions have been passed over by the present Astronomer Royal; five however have been completely observed. Under these circumstances, (though the deficiency for the latter part of the time only might be supplied from scattered foreign observations,) considering how desirable it is, in a research of some delicacy, to use observations made at the same place, I believe that I shall be compelled to abandon it entirely. The superior planets have been more frequently observed, and those but very little. And generally as to the comparison of theory with observation, and its immediate consequences, the reducing of complicated phænomena to simple laws, or the showing that new supplementary laws are necessary, forming altogether the most glorious employment for the intellect of man, I may state, in one word, to the best of my knowledge *nothing* has been done in England. In the lunar and planetary theories we have done nothing, not even in the way of numerical application. In the theory of the new planets and the periodical comets, we not only have done nothing, but we have scarcely known what others have done. With regard to the latter points, the distinguishing discoveries of the present century, our humiliation is great. Some of the new planets are very faint, and all are subject to excessive perturbation. If Astronomy had been confined to England, we never should have rediscovered them, even if we had once made out their orbits. If Astronomy had been confined to England, the paths of the comets would never have been traced, and the consequences deduced from the appearances of Encke's comet, the brightest discovery of the age, would have been lost. While Germans, Italians, and Frenchmen, have emulously pushed on the theory and the observation of these bodies, Englishmen alone, of all the nations professing to support a high scientific character, have stood still.—I am glad to turn from this dispiriting subject.

"There are other points to which I can scarcely allude without introducing a degree of personality which cannot be admitted in a public Report. They can be understood perhaps only by those who know the state of observation here, and who have seen the interior of foreign observatories. Of the latter, I can only profess personally to be slightly acquainted with those of France and those of the North of Italy. The characteristic difference between the spirit of the proceedings in England and on the Continent may be stated thus.—In England, an observer* conceives that he has done everything when he has made an observation. He thinks that the merely noting the passage of a star over one wire and its bisection by another, is all that can be expected from him; and that the use of a Table of logarithms, or anything beyond the very first stage of reduction, ought to be left to others. In the foreign observatories, on the contrary, an observation is considered as a lump of ore, requiring for its production, when the proper machinery is provided, nothing more than the commonest labour, and without value till it has been smelted. In them, the exhibition of results and the comparison of results with theory, are considered as deserving much more of an astronomer's attention, and demanding greater exercise of his intellect, than the mere observation of a body on the wire of a telescope. As an instance of the extent to which the reductions are carried there, I may mention that in one Italian observatory where the planets were considered the principal object, not only were the observations freed from instrumental errors and astronomical corrections, but the tabular places were computed by direct use of the Tables, (the ephemeris attached to Schumacher's lunar distances not

* "I am far from asserting that this is the character of every English observer, and I am equally unwilling to point out any individual to whom it is applicable. My object is merely to explain what I conceive to be the kind of difference which exists between English observers generally and foreign observers generally."

having reached that country,) and the equations of condition were regularly prepared for the correction of the elements. I suppose such a thing has never been done in England. This system must however contribute powerfully to produce that strong connexion between physical theory and practical observation, which is general on the Continent, but which does not exist in England.

"I believe that in the actual state of our institutions, reasons might be found which would seem to render it improbable that there ever can be so strong a connexion; and I can only hope that my view may be incorrect. There is one point with regard to the foreign astronomers to which I cannot help alluding, without however intending to draw any distinct inference. It is, that they have first obtained distinction while in the lower departments of the observatories. Encke's reputation was first acquired, not when he became Astronomer at Berlin, but when he was assistant at Seeberg: and Bessel became known in every part of Europe, not as Astronomer at Königsberg, but as assistant at Lilienthal. Walbeck and Argelander, in similar situations, have arrived at considerable eminence.

"I now proceed, and with great pleasure, to consider the second question. And this leads me to explain my opinion on a point respecting which I am anxious that I may not be misunderstood. I am not one of those who have joined in the cry of 'the decline of science in England,' nor do I believe that in this science there is any foundation for that cry. On the contrary, I assert without hesitation, that it is now and has been for some years rapidly advancing in this country. That there has been a decline, thirty or forty years ago, or rather that we have not kept up with the advances made by foreigners at that time, I am willing to admit. Perhaps this arose from political separation; perhaps in some degree from our pertinaciously retaining a system of mathematics which was insufficient for the deep investigations of Physical Astronomy, (for it was in this principally that we were behind our neighbours). And I have not disguised my opinion that in all the important branches of science we are still behind them. But in all with which I am acquainted a rapid progress has lately been made. In Physical Astronomy more has been done in England within the last five years than in the preceding century; and this not only with regard to the additions actually made by Englishmen to the stock of results drawn from that science, but also with respect to the number of persons who understand its principles, and who at some future time may be expected to contribute to its progress. In the University with which I am best acquainted, the study of this subject has made great advances. Of the amount and excellence of our geodetic measures and pendulum experiments, and of our discussions of refraction and aberration, I have already spoken. In accuracy of examination and correction of instrumental errors, perhaps something has been gained. In the extension of our star catalogues, much more has been done within a few years than in the whole previous time which followed Bradley's death. In the observation of planets, and the regular comparison of observations with Tables, (the first essential step to the improvement of the latter,) it is hoped that a great advance has been made. The observation of occultations and eclipses has extended; the exhibition of the results also, both for terrestrial and celestial determinations, has increased; and the regular publication of them in the *Memoirs of the Astronomical Society*, saves from oblivion the past and insures more completely the observation of the future. In the observation of double stars very much has been done. In all this I see grounds for exultation at 'the advance of science in England.' And when I remark the growing intermixture of physical with observing science, I indulge in the hope that the character as well as the extent of our Astronomy is improving, and that the time is approaching when a person will not in England be considered a great astronomer because he can observe a transit or measure a zenith-distance correctly."

[To be continued.*]

Scientific Works in the Press, and shortly to be published.

The Internal Structure of Fossil Vegetables described and illustrated; containing minute Descriptions and numerous Figures of all the Fossil Plants, retaining traces of organic structure, hitherto found in the various sedimentary deposits from the old red sandstone to the chalks. With Remarks on the Nature and Origin of Coal. By HENRY THORNTON MAIRE WITHAM, Esq., F.R.S.E., F.G.S., &c.

A new and improved edition, being the thirteenth, of the CHEMICAL CATECHISM. By the late SAMUEL PARKES, F.L. & G.S., &c. &c. Revised, and adapted to the present state of Chemical Science, by E. W. Brayley, jun., A.L.S.: of the London Institution.

Remarks on the Mineralogy and Geology of the Peninsula of Nova Scotia, accompanied by a coloured Map illustrative of the Structure of the Country, and by several Views of its Scenery. By CHARLES T. JACKSON and FRANCIS ALGER. Cambridge, United States. 4to, 1832.—This work is now on sale in London.

LXXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

1832. Dec. 13.—A PAPER was read, entitled, "On the extensive atmosphere of Mars." In a Letter to His Royal Highness the President. By Sir James South, Knt. F.R.S.

A paper was also read, entitled, "On the Law which connects the various Magneto-electric Phenomena lately discovered by Dr. Fara-

* Our limits rendering it necessary to reserve the continuation of the above review for our next Number, we think it right again to remind our readers and the cultivators of science generally, that the time fixed for the Association to assemble at Cambridge, is Monday the 24th of the present month, (June). For this Meeting, we learn from the Preface already quoted, the following Reports, among others, have been promised: *On the principal Questions debated in the Philosophy of Botany*, by Prof. Lindley; *On the Question of the Permanence of the relative Level of the Sea and Land*, by Mr. Stevenson; *On the State of our Knowledge respecting the Magnetism of the Earth*, by Mr. Christie; *On the State of our Knowledge of Hydraulics, considered as a Branch of Engineering*, by Mr. George Rennie; *On the State of our Knowledge of the Strength of Materials*, by Mr. Barlow; *On the State of our Knowledge respecting Mineral Veins*, by Mr. John Taylor; and *On the State of Zoological Knowledge*, by Mr. Vigors. From the contents of this volume and the announcements given in it, as well as from what we have heard from other sources, we are convinced that the interest of the ensuing proceedings at Cambridge, will not only indicate a still further advance in the attainment of all the objects of the Association, but also, if possible, will render it still more delightful to every lover of knowledge and the uses of knowledge, than even those which took place at Oxford last year.

day." By the Rev. William Ritchie, LL.D. F.R.S. Professor of Natural and Experimental Philosophy in the Royal Institution of Great Britain, and Professor of Natural Philosophy and Astronomy in the University of London.

A paper was then read, entitled, "An Account of an extraordinary Meteor seen at Malvern, November 12, 1832." By W. Addison, Esq. F.L.S. Communicated by W. G. Maton, M.D. V.P.R.S.

Dec. 20.—A paper was read, entitled, "On certain properties of Vapour." By the Rev. Dionysius Lardner, LL.D. F.R.S.

A paper was also read, entitled, "On the Secretion and Uses of the Bile." By B. Phillips, Esq. Communicated by W. G. Maton, M.D. V.P.R.S.

A paper was communicated to the Society, entitled, "Experimental Researches on Electricity, Third Series," by Michael Faraday, Esq. D.C.L. F.R.S. M.R.I., the reading of which was deferred to the next Meeting.

1833.—Jan. 10.—The reading of Mr. Faraday's paper, communicated at the last Meeting, and entitled, "Experimental Researches on Electricity, Third Series," was commenced.

Jan. 17.—The reading of Mr. Faraday's paper was resumed and concluded.

Jan. 24.—A paper was read, entitled, "Magnetical Experiments, made principally in the South of Europe and Asia Minor, during the years 1827 and 1832." By the Rev. George Fisher, M.A. F.R.S.

Jan. 31.—A paper was read, entitled, "An experimental Inquiry into the Treatment of Tic Douloureux." By W. R. Whatton, Esq. F.S.A. M.R.C.S. Communicated by P. M. Roget, M.D. Sec. R.S.

Feb. 7.—A paper was read, entitled, "On the relation which subsists between the Nervous and Muscular Systems in the more perfect Animals, and the nature of the Influence by which it is maintained." By A. P. W. Philip, M.D., F.R.S. L. & E.

Feb. 14.—A paper was read, entitled, "On the Existence of four distinct Hearts, having regular Pulsations, connected with the Lymphatic System, in certain Amphibious Animals." By John Müller, M.D., Professor of Physiology in the University of Bonn. Communicated by Leonard Horner, Esq., F.R.S.

Feb. 21.—A paper was read, entitled, "On the Influence of the Sun's Rays on the Oscillations of the Magnetic Needle." By William Snow Harris, Esq. F.R.S. In a letter addressed to Samuel Hunter Christie, Esq. M.A. F.R.S.

An Appendix to the preceding paper was also read, entitled, "Remarks on Mr. Snow Harris's Communication." By S. H. Christie, Esq. M.A. F.R.S.

Feb. 28.—A paper was read, entitled, "A Relation of the case of Thomas Hardy Kirman, with remarks on Corpulence." By Thomas Joseph Pettigrew, Esq. F.R.S.

The reading of a paper, entitled, "Experimental Determination of the Laws of Magneto-electric Induction in different masses of the same Metal, and of its Intensity in different Metals," by Samuel Hunter Christie, Esq. M.A. F.R.S., was commenced.

March 7.—The reading of Mr. Christie's paper was resumed and concluded.

A paper was then read, entitled, "Note on the Tides." By John William Lubbock, Esq. V.P. and Treasurer of the Royal Society.

A paper was also read, entitled, "On the Nature of Sleep." By A. P. W. Philip, M.D. F.R.S. L. & E.

March 14.—A paper was read, entitled, "On the Figures obtained by strewing Sand on Vibrating Surfaces, commonly called Acoustic Figures." By Charles Wheatstone, Esq. Communicated by Michael Faraday, Esq. D.C.L. F.R.S.

March 21.—A paper was read, entitled, "An Account of two cases of inflammatory Tumour produced by a deposit of the Larva of a large Fly (*Œstrus humanus*) beneath the Cutis in the Human Subject; accompanied with Drawings of the Larva." By John Howship, Esq. Communicated by Charles Hatchett, Esq. F.R.S.

The reading of a paper, entitled, "Experimental Researches in Electro-magnetism," by the Rev. William Ritchie, LL.D. F.R.S., was commenced.

March 28.—The reading of Dr. Ritchie's paper was resumed and concluded.

A paper was then read, entitled, "Notice of the Remains of the recent Volcano in the Mediterranean." By John Davy, M.D. F.R.S. Assistant Inspector of Army Hospitals.

The Society then adjourned over the Easter Vacation, to meet again on the 18th of April.

April 18.—A paper was read, entitled, "On Improvements in the Instruments and Methods employed in determining the Direction and Intensity of Terrestrial Magnetism." By Samuel Hunter Christie, Esq. M.A. F.R.S.

April 25.—A paper was read, entitled, "An Account of an extraordinary luminous appearance in the Heavens, seen at Athboy in Ireland, on the 21st of March, 1833." By the Right Honourable the Earl of Darnley. Communicated by John George Children, Esq. Sec. R.S.

A paper was also read, entitled, "On the Magnetic Power of Soft Iron." By Mr. Francis Watkins. Communicated by Michael Faraday, Esq. D.C.L. F.R.S.

May 2.—A paper was read, entitled, "Essay towards a first approximation to a Map of Cotidal Lines." By the Rev. William Whewell, M.A. F.R.S. Fellow of Trinity College Cambridge.

We shall in future Numbers give abstracts of some of the papers, the reading of which is here announced.

GEOLOGICAL SOCIETY.

At the Anniversary Meeting on Feb. 15th, 1833, the following Noblemen and Gentlemen were elected the Officers and Council for the ensuing year.

Officers:—*President*, George Bellas Greenough, Esq. F.R.S., &c.: *Vice-Presidents*, William John Broderip, Esq. B.A. F.R.S. & L.S.; Henry Thomas De la Beche, Esq. F.R.S. & L.S.; William

Henry Fitton, M.D. F.R.S. & L.S.; Rev. Adam Sedgwick, M.A. F.R.S. Woodwardian Professor in the University of Cambridge: *Secretaries,* Edward Turner, M.D. F.R.S. L. & E. Professor of Chemistry in the University of London; William John Hamilton, Esq.: *Foreign Secretary,* Charles Lyell, Esq. F.R.S. & L.S. Professor of Geology in King's College, London: *Treasurer,* John Taylor, Esq. F.R.S.

Council:—George William Aylmer, Esq.; Rev. William Buckland, D.D. F.R.S. & L.S. Professor of Geology and Mineralogy in the University of Oxford; Francis Chantrey, Esq. D.C.L. F.R.S.; Rev. William Daniel Conybeare, M.A. F.R.S.; Viscount Cole, M.P. F.R.S.; Charles Daubeny, M.D. F.R.S.; Sir Philip Egerton, Bart. F.R.S.; Right Hon. Earl Fitzwilliam, F.R.S.; Davies Gilbert, Esq. M.A. F.R.S.; R. I. Murchison, Esq. F.R.S.; J. W. Pringle, Esq. Capt. R.E.; W. Somerville, M.D. F.R.S.; Henry Warburton, Esq. M.P. F.R.S.; Rev. James Yates, M.A. F.L.S.

In the evening the following Address was delivered by Roderick Impey Murchison, Esq. F.R.S., L.S. &c., on retiring from the President's Chair.

Gentlemen,

Twenty-five years only have elapsed since this Society was first formed under the auspices of Mr. Greenough and a few zealous naturalists.—In the year 1826, when your Charter was obtained, the number of Members had already reached 476, and since that period a still more rapid increase has taken place, which has now swelled our list to 694. This remarkable yet steady augmentation of our forces is the best proof of the estimation in which your labours are held; and it further shows, that the pursuits of the geologist are no longer viewed as purely speculative, but are at length considered as essentially connected with the development of the national resources.

The past Session has been fatally marked by the decease of three distinguished geologists.

The Rev. Benjamin Richardson, of Farley near Bath, one of the earliest Members of this Society, was a man of great singleness of character and generosity of disposition, and, as a cultivator of science, he was distinguished by the extent of his knowledge,—not drawn from books, but from an examination of Nature in her own domains. In the pursuit of geology he was well instructed from his own researches; but he was ever delighted to tell that he owed his first clear ideas of the subject to William Smith; and his latter days were gladdened by knowing that the merits of his friend had been acknowledged by this Society. To his generosity of disposition our museum, and those of many local institutions, are deeply indebted. He collected only that he might give away; and, regardless of all personal fame, he never failed, when a discovery was made, to call around him those who could profit by it. Thus, though he was never seen among us, and though his name was rarely heard, he was steadily labouring in our cause, and silently, but effectually, urging it on.

I have next the painful duty to record the death of the venerable Sir James Hall, one of that bright constellation of philosophers which arose in Scotland towards the end of the last century.

The intimate friend of Hutton and of Playfair, he eagerly imbibed the opinions of these celebrated men, and satisfied himself of the leading truths in the Huttonian theory by extended and patient examinations of geological phænomena,—not merely amongst the British Isles, but in the Alps, in Italy, and in Sicily. The result of these observations was communicated in a series of Memoirs read before the Royal Society of Edinburgh, of which distinguished body he was for many years the President. In alluding to these Memoirs, I at once remind you how materially he assisted in demonstrating that a certain class of granitic veins had been injected into the overlying deposits posterior to their consolidation. He endeavoured to explain experimentally the contortions of certain strata, and the manner in which the phænomena had been effected by upheaving forces acting under compression. He subjected various rocks of igneous origin to chemical analysis, and succeeded in establishing their relative degrees of fusibility. He gave an original and perspicuous account of the true mode of formation of volcanic cones; and whilst he pointed out that Monte Somma was simply the segment of a vast volcano, from the flank of which the present Vesuvius had arisen, he showed the intimate analogy between the dykes of lava of the former and the ancient trap-dykes of our continents. If, in tracing the revolutions of the surface of the earth, he was led to attribute too much to the influence of one great diluvial current, we must recollect that in this, his only dereliction from the principles of Hutton, his conclusions were founded on a striking class of phænomena first observed by himself; and that the diluvial theory (though in a modified sense) has still the support of many of our most eminent geologists. To a mind so accustomed to speculate upon the intense energy of volcanic phænomena, it was a natural inference that the fractures and dislocations of mountain-masses have been produced by paroxysmal efforts of nature,—in short, by mighty earthquakes, and their accompanying elevations, depressions, and eruptions.

Much, however, as we owe to him for his many accurate observations of nature, our debt of gratitude must specially be acknowledged for his successful application of chemistry to geology, without which, one essential condition of the theory of Hutton would not have been established, as it now is, upon an immovable basis. The important discovery of carbonic acid by Black, which was destined to lead to the solution of many occult terrestrial phænomena, was at first cited by the Wernerians as destructive of the very basis of the theory of the igneous consolidation of the strata of the earth, it appearing impossible to explain the formation of crystalline marble from earthy carbonate of lime, by the very agent which drives off the gaseous constituent in every lime-kiln. To obviate this difficulty, the founder of the new theory propounded, that the heat by which rocks had been solidified was applied under enormous pressure; that in consequence effects had taken place entirely differing from those which manifest themselves under the mere pressure of our atmosphere; and that under such circumstances carbonate of lime might have been reduced to a state of fusion without calcination. Though the genius of Hutton had thus divined the true cause of the phænomena in dispute, that great man shrunk from the prosecution of experiments which might prove the truth of his hypothesis, being persuaded that the immensity of natural objects was far beyond the reach of man's imitation. It was reserved for Hall to have the glory of demonstrating the truth of the doctrine of his friend;—"the conjectures of genius," as he tells us, "at length ceased to appear extravagant; the mist which obscured the objects being dissipated by degrees, they appeared in their true colours, and a distant prospect opened to his view of scenes before unsuspected." To his ardent mind the realization, upon the surface of the earth, of that which had occurred below the deep abyss of the ocean, was not a hopeless effort, and he commenced a series of experiments which occupied a long period of his life,—were conducted with undaunted perseverance, and with a surprising fertility of invention, until he completely triumphed in fusing earthy carbonate of lime under vast pressure, producing from it a pure and crystalline marble. In establishing this fact, he turned the weapons of his opponents against themselves, and paved the way for the reception, among all the philosophers of Europe, of the leading doctrines which he advocated.

The gradual decay attendant upon advanced age, had prepared us in some measure for the other losses we have sustained; but Cuvier has been snatched from us when his comprehensive intellect was in its fullest vigour, and without any of those warnings by which both body and mind, are wont to announce that their mortal race is nearly run.

The death of such a man has called forth deep lamentations from every land upon whose children the rays of science have shed their light, and the eulogies poured forth in his honour are heard in almost every language of the civilized globe. How are we to limit our praise of one whose ample mind was matched only by the benevolence of his heart, and whose whole life was passed in unremitting exertions to enlarge the domain of science by blending it with civil polity, and by infusing it into the principles of education? With an almost incredible knowledge of the structure and functions of every part of organic nature, he possessed a power above that of every other man of emancipating himself from mere details, and of ascending to lofty generalizations, which were ever recommended by him with all the charms of eloquence; so that in his hands natural history became adorned, for the first time, with the highest attributes of pure philosophy. To him we owe the most important of the laws which have regulated the distribution of the animal kingdom, and by the application of which we have been made to comprehend many of the mutations of the surface of our planet. He it was who, removing from geology the incumbrance of errors and conceits heaped on it by cosmogonists, contributed more than any individual of this century to raise it to the place which it is assuming amongst the exacter sciences. Unlike our precursors, we no longer have to wade through the doubts and perplexities which retarded their acquaintance with the lost types of creation; to his skill we are indebted for a knowledge of their analogies with existing races; and he it was who, from their scattered bones, remodelled the skeletons of those wondrous originals which have successively passed away from the surface of our planet.

Those among us who have enjoyed the honour and delight of social intercourse with this great man will ever remember his suavity of manner,—his lucid power of exposition,—in short, that intellectual bearing which served to impress all listeners with the feeling, that every province of natural truth was within the grasp of his mighty thought.

The extent to which English geologists have profited by his instructions is recorded in the volumes of your Transactions, and a mere recapitulation of such of his writings as illustrate our subject is uncalled for on this occasion; but I cannot avoid remarking, that a Memoir on Zootomy, lately read before us, has proved a posthumous tribute to his fame. Of all the comparisons which he had instituted in his *Ossemens Fossiles* between the lost and living species, no one showed more ingenuity, and deep acquaintance with the laws of animal œconomy, than that in which he pointed out the close analogy subsisting between the gigantic Megatherium of South America, and the existing tribe of Sloths.

Well, therefore, may English geologists rejoice, that the discovery of another individual of this species has enabled one of our Fellows, eminent for his skill in comparative anatomy, to confirm the views of our great zoological master.

Thus, Gentlemen, the name of Cuvier, associated, as it has been, with discoveries forming the true basis of geology, is also interwoven with the most recent advances of this Society; and, as an appeal is now made to the naturalists of all nations to unite in a tribute to his memory, may those who have reaped such fruits of his genius, and are so justly proud of having sympathized in his living fame, hasten to record their obligations on the pedestal of that monument which is to be erected on the field of his greatest glory.

I now proceed to lay before you a sketch of the progress of geology in our own country during the past year. Deviating from the chronological order in which the different memoirs were considered at the last anniversary, I shall on this occasion, for the sake of greater perspicuity, class them under scientific heads: in so doing, I shall endeavour to connect our advances with the general progress of geology upon the continent, by passing allusions to such works of foreigners as the active nature of my own employment has permitted me to consult.

Recent Deposits.—In the class of historic alluvia, the Rev. J. Yates has described a partially submerged and ancient forest near the mouth of the river Dovey, chiefly composed of the *Pinus sylvestris*, and supposed to have been destroyed by the accidental demolition of a sea-dyke. A similar case of a submerged wood had previously been traced on the shores of Hampshire by Mr. C. Harris, who in communicating the discovery to Mr. Lyell, has proposed a most ingenious and probable explanation of the cause of these appearances*.

* Principles of Geology, vol. ii. p. 274, Second edition.

In attempting to account for the existence of large and shady forests on spots where the coasts are now entirely shorn of vegetation, we must embrace in our consideration the similar phænomena which are so numerous, as almost to form a submarine fringe around our island; and from these we may conclude, that when the whole country was densely clothed with wood, the forests might have extended their limits in full vigour to marine tracts, where single trees will no longer flourish.

You were last year made acquainted with the existence, at various places, of accumulations of sand, gravel, and clay, containing existing species of marine shells, placed at different heights above the sea; and a subsequent Memoir of Mr. Trimmer on a part of the estuary of the Mersey describes the presence of fragments of shells of existing species, in a stratum of sandy clay, containing numerous erratic pebbles, and a few boulders.

Having myself traced beds with recent sea-shells at considerable and various heights above the sea, both on our eastern and western coasts, I am disposed to think that there is already sufficient evidence of our shores having undergone elevation at periods comparatively recent, however difficult it may be to explain all such superficial accumulations upon a similar hypothesis.

If the coasts exhibit testimonies of such elevations, the evidence is corroborated when we follow the course of those indentations which penetrate far within the interior of the island. In most of these we perceive accumulations of shingle and sand on the sides of valleys, some of which, by the fine lamination of their beds, indicate long-continued and tranquil formation; others, by the shivered and fragmentary condition of their contents, bespeak a more tumultuous mode of aggregation: the latter, therefore, were probably coincident with periods of elevation of the land, which throwing up the shores of the island, have converted former estuaries into existing plains, bounded by ancient shores of gravel, leaving the rivers to meander between their widely separated banks.

If such phænomena be still traceable within this island, where the subterranean energies of nature are now, and have been for so long a period quiescent, what amount of valuable instruction may we not hereafter derive from the presence of good observers in those countries where volcanos and earthquakes, with their accompanying elevations and depressions, are in frequent activity? You are already aware of the important services of Mr. Lyell, and how effectually he has attracted attention to this branch of inquiry. I would further remind you of the discoveries of M. de Boblaye, who has placed the successive elevations of land in a remarkably clear light, by showing the existence in the Morea of four or five distinct ranges of ancient sea cliffs, marked at different levels in the limestone escarpments by lithodomous perforations, lines of littoral and sea-worn caverns, and other striking proofs of former tidal action.

The description of a large granitic boulder, by Mr. Maxwell, resting on the slaty shores of Appin, in Argyleshire, leads me to observe, that the numerous detached masses of rock, foreign to the districts in which they are scattered through Northern Germany and Westphalia, have met with an additional expositor of their origin in Professor Hausmann, of Göttingen, who, coinciding in the views of M. Brongniart and others, is of opinion that these fragments have been derived from the mountains of Sweden.

M. A. De Luc has again come before the public, with a Memoir on the gravel and other transported materials of the basin of Geneva; being a second part of his former essay on the same subject. He indicates the localities in which the fragments of different rocks have originated, showing that although some have been drifted from the east, and others from the west, many of them are probably remnants of those calcareous mountains which were shattered on the spot, at that period of dislocation, when by the expulsion of their debris, that great cavity was formed, which is now occupied by the lake. The superficial sediments of the basin are said to vary much in their composition; whilst their beds are inclined in all directions, thereby indicating the effects of numerous and conflicting currents of water, which in some cases have hurled down large boulders of primary rock from the higher Alps, and in others have heaped up the finer alluvia derived from the adjacent secondary formations. All these phænomena are supposed by the author to have been caused by debacles incident to lengthened periods in which the surrounding mountains were forcibly and violently elevated.

From these and other writings of the present day, we perceive that correct observations have now established, that the diluvial and transported detritus of each great geographical division of Europe, when viewed on a great scale, can for the most part, be traced to an axis of elevation within that region; so that as each great mountain-chain has been the source of the detritus covering the adjacent low country, we can no longer attribute such drifts of sedimentary matter to one particular diluvial current, which has acted in any given direction.

However indisposed, therefore, the diluvialists may be to adopt as a full and satisfactory explanation of these appearances the modified view of the theory of diurnal action of Hutton, as put forth by Mr. Lyell, the dispassionate reasoner must admit, that the question between the diluvialist and the advocate of existing causes is fast resolving itself into one of amount or intensity of forces. Each party has now recourse to modern analogies in referring changes between the levels of sea and land to eruptions from beneath; and he who is unwilling to quit a path of induction pointed out, as he believes, by nature, invokes only *repeated* shocks of earthquakes, elevations, and depressions, in preference to a *limited* number of stupendous catastrophes insisted upon by his antagonist.

Tertiary Deposits.—In the illustration of tertiary geology, I may announce to you, that the last pages of the Third Volume of the Principles of Geology, by Professor Lyell, are in the press. In this volume, which I have perused, the author successfully applies to the tertiary formations the principles laid down in the two first volumes. He subdivides these younger deposits into four natural epochs, founded upon a mass of zoological evidence infinitely more comprehensive, and yet more precise than any which has ever been brought before us. In treating chronologically of alluvial, fresh-water, marine, and volcanic phænomena, a wide range is afforded for the development of his extensive knowledge and observation; enabling him to ground his reasonings on countries visited by himself, and to interpret the handwriting which Nature has left upon the walls of her geological monuments, in such a manner as not only to expound her ancient records, but to connect them with the history of our present races.

Although this volume is devoted chiefly to the description of the younger formations, as more intimately connected with the main object of the author, the secondary and primary rocks are reviewed so far as was necessary to show their connexion with his theory, and to indicate how well their structure can be accounted for by causes, which he supposes to be still in full and undiminished operation. The powerful effects produced upon the public mind by the first and second volumes of this work will, I may venture to say, be highly augmented by a perusal of this concluding part; and even those geologists who may differ from the author on a few theoretical points, will gladly eulogize the efforts of one who has so greatly advanced their knowledge.

Fossil Zoology.—The Session has been fertile in communications upon fossil zoology. The splendid specimens of Megatherium, &c. brought to this country by Mr. Woodbine Parish have, in the hands of Mr. Clift, afforded us much curious instruction. The tribute which these remains enabled Mr. Clift to pay to his great master in comparative anatomy, has already been adverted to; but we must not forget that they also elicited brilliant lectures from Dr. Buckland, both within these walls, and on the occasion of the late scientific festival at Oxford.

The Rev. Mr. Stanley has given a lively description of the caves of Cefn, in Flintshire, one of which, like that of Kirkdale, is supposed to have been the inhabited den of hyænas; whilst another and larger cavern, situated at a lower level on the side of the same mountain, contained only the remains of recent animals. From the distinct nature of the upper and lower layers of alluvia collected within the inhabited cave, and arranged above and below the floor of fossil bones, the author speculates on layers as evidences of different periods of aqueous debacle.

Mr. Mantell, whose energies seem to expand in each succeeding year, notwithstanding the limited field to which his researches are necessarily confined, has presented us with an account of an undescribed and singular species of Saurian, to which he assigns the name of *Hylæosaurus*. This fortunate exhumation has, I am happy to say, encouraged the enterprising ranger of Tilgate Forest to make it the nucleus of a new and comprehensive work, in which he will not only describe all the vertebrated animals in his rich domain, the Wealds of Sussex, but will embrace in it a geological description of his own, and of the adjoining counties.

The bright example of Mr. Mantell is meeting with worthy imitators in other parts of England, in the persons of other zealous young members of the same profession, among whom may be mentioned Mr. Chaning Pearce of Bradford, and Mr. T. Hawkins of Glastonbury;—the first of whom has collected and arranged a vast number of new species of the organic remains in his neighbourhood; the latter, within the short space of two years, has disinterred numerous fine Saurians from the lias: among these we recognise a Plesiosaurus, so perfect, that it serves to commemorate the skill of Mr. Conybeare, whose elaborate restoration of the skeleton from one imperfect specimen is now amply confirmed.

A recent discovery of Miss Mary Anning, that indefatigable purveyor to the store-houses of our science, has furnished Mr. T. Hawkins with the disjointed fragments of an animal, which upon being reintegrated, proves to be the largest individual of the *Ichthyosaurus platyodon* ever yet found entire upon our shores.

Two Members of your Council, Viscount Cole and Sir Philip Egerton, have for some years entered zealously into the pursuit of fossil zoology, and have reaped a rich harvest, both on the continent and at home, having with their own hands brought to light some osseous relics unknown even to Cuvier.

If these are among the latest fruits of fossil zoology in England, our coadjutors on the continent have not relaxed their efforts. I had formerly occasion to direct your attention to that invaluable work, the Conchological Classifications of M. Deshayes; and I ought at the same time to have noticed a most useful and clear production of the same author, entitled *Coquilles Caractéristiques des Terreins.*

The "Mémoires Palæontologiques" of M. Boué, which embraces memoirs from all countries, may, it is to be hoped, in great measure supply the loss which must have been deeply felt by every practical geologist, in the cessation of that most useful periodical the *Bulletin Universel des Sciences.*

M. Pentland, from the examination of a collection of fossil bones which had been consigned to his deceased friend, Baron Cuvier, has enlarged our acquaintance with the Fauna of Australia, by the addition of several new and undescribed species of animals, principally marsupial.

The "Palæologica" of M. Hermann Von Meyer, of Frankfort, brings together, in a synoptical form, all our present stock of knowledge of extinct vertebrated animals; and being a compendious index to all the works written upon this subject, must be considered a necessary portion of every geological library.

Our distinguished Foreign Associate Von Buch has just produced a work upon Ammonites, which is intended to simplify the natural arrangement of this obscure class of fossil bodies.

A blank in fossil zoology is about to be filled up by Dr. Agassiz, of Neufchatel, whose work on "Fossil Fishes" will furnish us with materials which we looked for from the pen of the lamented Cuvier. Precise anatomical distinctions, even to the minutest forms of the scales, will be so considered in this work, that the learned Professor hopes to realize the application of the system of his great instructor, and from the forms of parts to enable us to decide upon the specific character of the entire fish to which they belonged. The short sketch* by this author of the fishes of Œningen and of the lias, may lead us to a favourable anticipation of the success of his forthcoming volumes,—and to hope that fossil ichthyology may hereafter serve our cause as efficiently as other branches of zoological evidence.

Fossil Plants.—The early experiments of Hall and Hatchett, amplified and illustrated by MacCulloch, had nearly produced conviction that all the varieties of carbonaceous matter, from the ill-consolidated surturbrand, through every stage of brown coal to pure jet; and in our older strata from anthracite to bituminous coal, were the products of vegetables. Botanists have since corroborated the soundness of these views, by developing the Flora of the associated strata; and one of our body has enabled us to refer many of these plants to their natural families in living nature, by an ingenious method of exhibiting polished sections of their stems: but it has been reserved to Mr. W. Hutton in pursuing this line of inquiry, to complete the solution of the problem by demonstrating the vegetable structure in coal itself. The Memoir of Mr. W. Hutton is further of high and practical utility in describing the source of those enormous volumes of imprisoned gases, which upon admixture with our atmosphere become explosive, and occasion such disastrous results to our miners.

As a slight contribution towards a knowledge of the condition of the surface of the earth during one of the periods in the formation of the oolitic series, which is marked by its vegetation, I offered to you a few remarks on the vertical position of the stems of Equiseti, in a sandstone of the eastern Moorlands of Yorkshire. This phænomenon extending over a large area is analogous to that observed in the Isle of Portland by Dr. Buckland and Mr. De la Beche; from which however it differs, as it appeared to me, in requiring for its explanation the desiccation of submarine sediments, so as to leave a stagnant marsh for the place of growth of these plants; which, after this marsh had been gradually silted up, were submerged by a fresh irruption of the sea, accumulating above them the deposits of the middle and upper oolite. [To be continued.]

ROYAL ASTRONOMICAL SOCIETY.

March 8.—The following communications were read.

On Prof. Bessel's improved method of deducing the Longitude from a Lunar Distance. By Lieut. Stratford, R.N.

Transits of the Moon with Moon-culminating Stars, observed at Cambridge Observatory, in the month of February 1833.

On the Mass of Jupiter. By Professor Airy; the reading of which was not finished.

April 12.—The following communications were read.

Prof. Airy's paper "On the Mass of *Jupiter*" was resumed and concluded.

A paper was also read "On a method of determining the Longitude

* Jahr. Buch, 1832, Dritter Jahrgang, Zweites Quartal-Heft.

with considerable accuracy by means of Lunar Eclipses." By Capt. Henry Kater, F.R.S.

Planetary Observations made at the Observatory of Wilna. By M. Slavinski; consisting of right ascensions and declinations of *Uranus, Mars, Jupiter*, and *Vesta*.

A letter from the Rev. W. R. Dawes to Mr. Dollond, on an improvement in the Micrometer by the latter.

Observations, &c. made at Padua, from the year 1829 to 1832, inclusive; comprising occultations of *Aldebaran*, &c.; the transit of Mercury, occultation of *Saturn*, and eclipse of the sun of 1832; and observations of Gambart's comet of July 19, 1832, and of Biela's comet.

Observations made at Saville Row, by Mr. Snow; consisting of right ascensions of Mars, and of stars observed with Mars, in Nov. and Dec. 1832; mean right ascensions of 60 unknown stars for 1832; and observations of the egress of *Jupiter's* satellites on Dec. 26, 1832, made for the purpose of ascertaining whether the phænomena could be observed with sufficient accuracy, agreeably to the recommendation of the superintendant of the *Nautical Almanac*.

Of several of these papers we purpose to give a further account in future Numbers.

ZOOLOGICAL SOCIETY.

Proceedings of the Committee of Science and Correspondence.

October 23, 1832.—The exhibition was resumed of the collection of Shells formed by Mr. Cuming on the western coast of South America, and among the islands of the Southern Pacific Ocean. The new species were accompanied, as on the previous occasions, by descriptions from the pens of Mr. Broderip and Mr. G. B. Sowerby; they belonged to the genera *Cancellaria, Ovulum, Murex, Typhis*, and *Ranella*.

Mr. Owen exhibited a preparation of the mammary gland of *Echidna Hystrix*, Cuv.; and read his Notes respecting it.

Nov. 13.—A numerous collection of Fishes was exhibited, which had been formed in Ceylon by Dr. Sibbald, Corr. Memb. Z.S., and had been presented by him to the Society.

The new species of *Cowries* contained in the collection formed by Mr. Cuming were exhibited and characterized by Mr. Gray.

A skull of the *Capybara, Hydrochærus Capybara*, Erxl., was exhibited, and Mr. Owen read some Notes thereon.

Nov. 27.—A letter was read, addressed to the Secretary of the Society, by W. Smith, Esq., Secretary of the Hudson's Bay Company, referring to an *Arctic Fox, Canis lagopus*, Linn., recently presented by that Company to the Society, together with a living *Pekan*, or *Fisher Martin, Mustela Canadensis*, Schreb.

A specimen was exhibited of the *Falco rufipes*, Bechst., a bird of exceedingly rare occurrence in Britain.

At the request of the Chairman, Mr. Gould exhibited a very extensive collection of *Bird-Skins*, from the Orkneys, and pointed out particularly those which he regarded as most interesting, either on account of their rarity or the state of their plumage. They included beautiful specimens of the *Ivory Gull, Larus eburneus*, Temm., and of the *King Duck, Somateria spectabilis*, Steph., as well as of other rare species.

A paper was read, containing "a brief account of a particular function of the nervous system," in which Dr. Marshall Hall detailed a series of experiments tending to prove the existence of a source of muscular action distinct from all those hitherto noticed by physiologists: viz. volition, the irritation of the motor nerves in some part of their origin or course, or that of the muscles themselves. The peculiarity of this motion he stated to consist in its being excited "by irritation of the extreme portion of the sentient nerves, whence the impression is conveyed through the corresponding portion of brain and spinal marrow as a centre, to the extremities of the motor nerves."

Dec. 11.—A specimen was exhibited of a Hedgehog from the interior of South Africa, which formed part of a rich collection of preserved animals, recently brought from that country by Mr. A. Steedman. It was characterized by Mr. Bennett as *Erinaceus frontalis*.

A specimen was exhibited of the *Phasianus lineatus*, Lath., obtained from the Tennasserim coast by G. Swinton, Esq., Corr. Memb. Z. S., by whom it was presented to the Society. The species was characterized by Mr. Vigors in the First Part of the 'Proceedings,' page 24, or Phil. Mag. and Annals, N. S. vol. ix. p. 147.

The exhibition of Mr. Cuming's *Shells* being resumed, new species of the following genera were characterized by Mr. Broderip and Mr. G. B. Sowerby; viz. *Murex, Ranella, Cardita, Pectunculus, Capsa, Solenella, Nucula, Amphidesma, Neritina*, and *Ancylus*.

The stomach, *cæca, cranium, &c.* of *Hyrax Capensis* were exhibited, the former constituting part of the collection of Mr. Thomas Bell. Mr. Owen, who had anatomically examined the individual from which they were obtained, read an account of its structure.

It was announced that the Meetings of the Committee were now concluded.

Proceedings of the General Meetings of the Society for Scientific Business.

Jan. 8.—This was the first of the General Meetings for the transaction of Scientific Business.

The Vice-Secretary (Mr. E. T. Bennett) called the attention of the Meeting to a stuffed specimen of the *M'horr Antelope*, which was exhibited on the table; and characterized, in addition to the *M'horr*, two other species of the same form of *Antelope*.

Mr. Spooner read his Notes of the *post mortem* examination of the *M'horr*.

A stuffed specimen was exhibited of a female of the *harnessed Antelope, Antilope scripta*, Pall., which had lived for some months in the collection of the Zoological Society of Dublin, by whom it was presented to the Society.

Preparations were exhibited of the *tracheæ* of the *Penelope Guan* of M. Temminck, and of the *Anas Magellanica*, Auct., and Mr. Yarrell read short descriptions of them.

Specimens were exhibited of the following *Mollusca* and *Conchifera*, hitherto undescribed, forming part of Mr. H. Cuming's collection: they were accompanied by characters by Mr. Broderip. SPONDYLUS *Princeps*, *dubius*, *Leucacantha*, and *aculeatus*; TRITON *lignarius*, *constrictus*, *tigrinus* (bearing some distant resemblance to *Trit. femoralis*), *rudis*, *lineatus*, *gibbosus*, *scalariformis*, and *convolutus*; TURBINELLA *tuberculata*, (approaching in its general appearance some of the *Pleurotomata*, which have a short canal,) *armata*, and *Cæstus* (approaching nearest to *Turb. pugillaris*); and *Purpura Xanthostoma*.

A paper was read by Dr. Grant, "On the Nervous System of *Beroë Pileus*, Lam., and on the Structure of its *cilia*."

Mr. Yarrell detailed some observations on the changes of plumage in Birds; which he illustrated by Notes on several species in the Society's Gardens made by James Hunt, one of the Keepers; a Note also by whom, on the breeding of the *Passenger Pigeon*, *Ectopistes migratorius*, Swains., in the Society's Menagerie, was also read.

Jan. 22.—A letter was read, addressed to Charles Telfair, Esq., Corr. Memb. Z.S., as President of the Mauritius Natural History Society, by M. Goudot of Madagascar. It contained an account of a remarkable phænomenon exhibited by the larvæ of a species of *Aphrophora* (*Cercopis*), which attach themselves to a tree of the genus *Morus*, not uncommon in the vicinity of Tamatave, in the island last named.

Mr. Bennett called the attention of the Society to a stuffed specimen of an Antelope, from the southern part of the peninsula of India, which had been presented to the Society several months since by Mr. Telfair. He was disposed to regard it as the young of the *Indian Antelope*, *Antilope Cervicapra*, Pall.

Specimens were exhibited of the adult male of the *lineated Pheasant*, *Phasianus lineatus*, Lath., and of two immature birds of the same species: for the whole of these the Society is indebted to George Swinton, Esq., Corr. Memb. Z.S. Mr. Gould made some observations upon them.

Dr. Grant exhibited numerous specimens from Whitsand Bay, Cornwall, of *Ianthina vulgaris*, Lam., and of *Velella limbosa*, Lam., both animals of rare occurrence on the English coast, and chiefly met with floating in tropical or warmer seas.

Feb. 12.—A letter from M. Geoffroy-Saint-Hilaire, For. Memb. Z.S., was read, consisting of reflections on the communication respecting the *Ornithorhynchus*, made by Dr. Weatherhead to the Committee of Science and Correspondence, on September 11, 1832, and published in the Proceedings, Part II. p. 145; or present vol. of Phil. Mag. p. 71.

EVENING MEETINGS AT THE UNIVERSITY OF LONDON.

The Professors have for some time past held Monthly Evening Meetings in the Anatomical Museum of the University, and have now made the addition, on the plan of the Royal Institution, of having a lecture on some literary or scientific subject. The first lecture was given by Dr. Ritchie, On the communication of scientific knowledge to Youth, an outline of which will be found in the April Number of this Journal, page 312. The second lecture, On the Chemistry of Geology, was delivered by Dr. Turner on the 7th of May. We hope, in a future Number, to give an outline of this lecture.

LXXXII. *Intelligence and Miscellaneous Articles.*

ON KINIC ACID AND SOME KINATES. BY M. BAUP.

M. BAUP states that, according to MM. Henry and Plisson, kinic acid consists of

Carbon	34·4320	or 2 atoms.
Hydrogen	5·5602	4 atoms.
Oxygen	60·0078	3 atoms.
	100·0000	

M. Baup observes that this atomic constitution does not at all agree with its saturating power. M. Liebig has given as the result of his analysis:

Carbon	46·193	or 15 atoms.
Hydrogen.............	6·101	24 atoms.
Oxygen	47·706	12 atoms.
	100·000	

According to M. Baup its analysis and atomic constitution are:

Carbon	50·000	or 15 atoms	= 90
Hydrogen......	5·556	10 atoms	= 10
Oxygen........	44·444	10 atoms	= 80
	100·000	Atomic weight	=180

These numbers are equivalent to 3 atoms carbon, 2 hydrogen, and 2 oxygen; but they do not accord with its saturating power. M. Baup remarks, kinic acid is equivalent to a compound of equal weights of carbon and water, agreeing exactly with Dr. Prout's analysis of lignin. M. Baup therefore considers kinic acid and lignin as isomeric bodies, though their properties are singularly different.

Crystallized kinic acid is not anhydrous; it contains water, which does not enter into the composition of the dried kinates. Crystallized kinic acid contains 1 atom of water.

Kinate of Soda is composed of an atom of acid 180, 1 of soda 32, and 4 of water 36 = 248. It is very easily prepared by saturating bi-carbonate of soda with kinic acid, and exposing the concentrated solution to spontaneous evaporation. This salt is not bitter when pure; it dissolves in half its weight of water at 60°.

M. Baup could not obtain crystallized kinate of potash or ammonia, either neutral or acid.

Kinate of Lime is composed of an atom of acid 180, 1 of lime 28, and 10 of water 90 = 298. This salt exists naturally in some kinds of cinchona in considerable quantity. It is unalterable by exposure to the air. It crystallizes in rhombic laminæ of about 78° and 112°. These sometimes become hexagonal by the replacement of the two acute angles. These crystals are easily divisible into brilliant leaves. Kinate of lime dissolves in 6 parts of water at 62°; its solubility is greatly dependent upon temperature.

Kinate of Strontia.—This salt is composed of an atom of acid 180, 1 of strontia 52, and 10 of water 90 = 322. It effloresces quickly by exposure to the air. It is soluble in 2 parts of water at 54°.

Kinate of Barytes.—This salt may be prepared by adding carbonate of barytes to the acid. It is composed of an atom of acid 180, 1 of barytes 76, and 6 of water 54 = 310. It crystallizes in acute triangular dodecahedrons. It does not effloresce by exposure to the air.

Bi-kinate of Copper.—Hitherto two distinct salts have been confounded under the name of Kinate of Copper. One is a bi-kinate, and the other a sub-kinate; the former is prepared by putting carbonate or oxide of copper into kinic acid, taking care that the acid is sensibly in excess; if during evaporation a greenish salt should deposit, it ought to be immediately separated. By cooling, or by the spontaneous evaporation of the solution, the bi-kinate crystallizes. It is to be redissolved in water containing a little kinic acid, and recrystallized.

A solution of this kinate, made with cold water, soon decomposes; this effect is accelerated by heat; to avoid it, it is requisite to have a slight excess of acid, which however increases the difficulty of having a pure salt. Bi-kinate of copper is of a pale blue colour, and in acicular crystals; it effloresces in the air, and loses 2-5ths of its water of crystallization. It dissolves in about 3 parts of water at ordinary temperatures. It is composed of 2 atoms of acid 360, 1 of peroxide 80, and 10 of water 90.

Sub-kinate of Copper.—This may be prepared by heating a weak solution of kinic acid with excess of carbonate or peroxide of copper; or by the double decomposition of a kinate, with the acetate, but not with the sulphate or nitrate of copper.

This salt has the form of very small brilliant crystals; its colour is a fine green, which does not alter in the air. It is soluble in from 1150 to 1200 parts of water at 60°. Boiling water dissolves a larger quantity, which crystallizes on and after cooling. It is composed of acid 57·931; oxide 27·586; water 14·483. It is difficultly reducible to any probable atomic constitution.

Kinate of Lead.—This salt crystallizes only when the solution is so concentrated that it is difficult to detach the acicular crystals which occur in the paste. A portion of the mass being dried and pulverized, was exposed to the air in a warm chamber, until it ceased to lose weight. In this state kinate of lead consists of an atom of acid 180, 1 of oxide 112, and 2 of water 18 = 310.

Sub-kinate of Lead may be prepared by mixing kinate of soda, or preferably of ammonia, with subacetate of lead. As it is soluble in excess of subacetate of lead, it is better to stop before precipitation entirely ceases. This salt has a great tendency to combine with carbonic acid by mere exposure to the air; it ought to be kept from it, pressed between folds of blotting-paper, and dried under the receiver of the air-pump over lime or sulphuric acid, and afterwards heated, if required for analysis. Its composition when dry is, acid 180, oxide 460; if it were a tetra-kinate the oxide would be 448. It is not easy to reduce this to an atomic constitution.

Kinate of Silver.—A solution of kinate of silver, evaporated in the dark with a very gentle heat, or at common temperatures, under the receiver of the air-pump, gave in both cases a very white anhydrous salt, of a spherical or mammillated form. When heated, it soon fuses, swells, and leaves (after giving abundant white vapours) silver in the metallic state. It is composed of acid 180, and oxide 116; undoubtedly an atom of each.

Kinate of Cinchonia is soluble in half its weight of water at about 60°. It contains 4 atoms of water of crystallization; it is partially decomposed by alcohol. If it be dissolved with heat in a quantity of alcohol which is insufficient to hold it in solution when cold, a salt is deposited in colourless brilliant crystals, which are short compressed prisms, with four or six facets, and obliquely truncated. They appear to be unalterable in the air, or by a slight degree of heat. After a long time these crystals become perfectly opake.

Kinate of Quina.—This salt also contains 4 atoms of water of crystallization. It is soluble in $3\frac{1}{2}$ parts of water at 50°, and in 8·88 parts of alcohol at the same temperature.—*Ann. de Chim. et de Phys.* tom. li. p. 56.

ANALYSIS OF ASPARAGIN, AND ASPARTIC ACID.

MM. Boutron and Chalard have analysed the above substances by means of combustion with oxide of copper: a gaseous mixture was obtained, in which the azote and carbonic acid were to each other as 1 to 4. The ultimate result gives, as the composition of asparagin,

Carbon......	8	atoms	611·504	....	39·060
Hydrogen....	16	do.	98·836	....	6·377
Azote.......	4	do.	354·072	....	22·610
Oxygen.....	5	do.	500·000	....	31·953
					100·000

When crystallized, it contains 12·58 per cent. of water, equivalent to two atoms.

The aspartic acid analysed was obtained by boiling barytes water in excess with asparagin, until the evolution of ammonia had for some time ceased, and precipitating the barytes, accurately, by sulphuric acid, while the liquor was hot. The acid being but very slightly soluble while cold, was precipitated almost entirely in the form of pearly and shelly crystals. The taste was acidulous, much resembling that of mucic acid. This process is more convenient and expeditious than that proposed by M. Plisson, which consists in boiling asparagin with litharge, and decomposing the aspartate of lead by sulphuretted hydrogen. The insolubility of the oxide and aspartate of lead increases the length of the operation. Aspartic acid appears to consist of 8 atoms carbon, 12 hydrogen, 2 azote, and 6 oxygen ; or

Carbon	41·78
Hydrogen	5·11
Azote	12·09
Oxygen	41·02
	100·00

This is the composition as it exists in the aspartates; but in its crystallized state it contains water, (which however it does not lose by exposure to a temperature considerably above the boiling point of water,) and then consists of

Carbon	38·
Hydrogen	5·54
Azote	11·23
Oxygen	44·43

Aspartate of lead was found to be composed of 66·9 acid, and 63·8 oxide. Aspartate of silver yielded acid 43·0, oxide 43·0.—*Journal de Pharmacie*, April 1833.

COVENT-GARDEN MEASURES.

To the Editors of the Phil. Mag. and Journal of Science.

Gentlemen,

There is a deficiency in the specification of the relative capacities of the measures used at Covent Garden, (page 406 of your last Number,) which I will thank you to correct ;—thus,

2 sieves	=	1 bushel.
4 half sieves	=	1 bushel.
8 quarter sieves	=	1 bushel.
12 large punnets	=	1 bushel.
16 second punnets	=	1 bushel.
32 third punnets	=	1 bushel.
48 least punnets	=	1 bushel.

In other words, the Sieve may be considered equal to half a bushel.

Half sieve	=	1 peck.
Quarter sieve	=	1 gallon.
Second punnet	=	1 pottle.
Third punnet	=	1 quart.
Least punnet	=	$1\frac{1}{3}$ pint.

Yours, &c.

May 9, 1833. B. Bevan.

CORRECTION IN MR. ENYS'S PAPER ON THE GRANITE OF PENRYN.

To the Editors of the Phil. Mag. and Journal of Science.

Gentlemen,

A mistake has been committed in the amount of power required in "cleaving," in my paper on the Granite of Penryn, page 324 of your last Number.

The account should stand as follows:

	Inches.		Inches.		Sq. In.		Power.	
Capping ..	24	by	26	=	624	....	3	3 Wedges.
Quartering	24	by	15	=	360	....	5	2 Wedges. 1 Ripper.

I may add that some cleavers said that the relative power was as 2, 3, and 10. But one of the best informed, who had the most general acquaintance with the different rocks, though he allowed such might be the fact in some tough rocks, thought that 2, 3, 5 was the average power required, as stated in page 323.

I am, Gentlemen, your obedient Servant,

Enys, May 3, 1833. JOHN S. ENYS.

TEMPERATURE AND HUMIDITY IN FEBRUARY AND MARCH.

On looking over my former Journals, it appears that the February of this year was the mildest, and also that in which we have had the greatest quantity of wet, of any February these seven years, at least; and that the succeeding March was the coldest we have had during the same period. SAMUEL VEALL.

Boston, April 8, 1833.

LUNAR OCCULTATIONS FOR JULY AND AUGUST.

Occultations of fixed Stars by the Moon, visible at Greenwich in the Year 1833. *Computed by* THOMAS MACLEAR, *Esq.; and circulated by the Astronomical Society.*

1833.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions. Sidereal time.	Immersions. Mean time.	Immersions. Angle from North Point.	Immersions. Angle from Vertex.	Emersions. Sidereal time.	Emersions. Mean time.	Emersions. Angle from North Point.	Emersions. Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
July 27	52 Ophiuc.	7	2011	15 15	6 54	89	77	16 34	8 13	245	243
30	4 Capricor.	6	2384	17 30	8 57	117	93	18 43	10 10	253	237
Aug. 9	61 δ^1 Tauri	4	488	0 48	15 34	121	83	1 52	16 39	280	249
	64 δ^2 Tauri	4·5	492	1 21	16 8	82	47	2 19	17 5	319	291
	68 δ^3 Tauri	5	499	2 31	17 17	158	132	3 17	18 3	240	224
19	88 Virginis	7	1571	17 18	7 26	51	81	18 20	8 28	261	296
26	[1324] Sagit^t	7	2345	16 54	6 34			a near	approa.		
	(369) Sagitt.	6·7	2356	19 4	8 45	38	27	19 47	9 28	336	334
29	56 *f* Aquar.	6	2686	18 29	7 58	59	31	19 19	8 48	339	319
31	30 *r* Pisciu.	4·5	2870	19 19	8 40	111	75	20 25	9 46	308	276
	33 *s* Pisciu.	5	2877	21 32	10 53	87	63	22 41	12 1	326	312

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

Days of Month, 1833.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Penzance. Max.	Barometer. Penzance. Min.	Barometer. Boston $8\frac{1}{2}$ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Penzance. Max.	Thermometer. Penzance. Min.	Thermometer. Bost. $8\frac{1}{2}$ A.M.	Wind. Lond.	Wind. Penz.	Wind. Bost.	Rain. Lond.	Rain. Penz.	Rain. Bost.
April 1	29·145	28·910	28·984	28·872	28·90	57	43	54	45	42	s.	sw.	E.	0·08	0·410	0·10
2	29·123	29·045	29·172	29·034	28·55	57	45	54	45	47	s.	w.	calm	·23	·135	·13
3	29·665	29·489	29·481	29·422	29·07	61	43	56	45	45	w.	sw.	calm	...	·340	...
○ 4	29·548	29·501	29·572	29·298	29·12	60	42	56	47	45	s.	sw.	SE.	·10	·120	...
5	29·905	29·814	29·872	29·745	29·25	58	42	54	42	46·5	w.	w.	NW.	·02	·130	·16
6	30·024	29·980	29·884	29·825	29·50	57	32	56	40	47·5	s.	s.	w.	...	...	...
7	29·881	29·845	29·922	29·831	29·46	57	35	51	43	48	E.	N.	calm	...	·610	...
8	30·079	29·981	30·072	29·978	29·42	58	32	53	43	44	NE.	NW.	E.	...	...	·16
9	30·121	29·978	30·084	30·078	29·57	59	42	55	44	43·5	sw.	NW.	calm	·05	...	...
10	29·883	29·476	29·934	29·672	29·22	56	40	55	47	51	s.	w.	calm	·26	...	·15
11	29·438	29·304	29·584	29·484	28·85	53	36	51	43	47·5	sw.	w.	w.	·18	·080	...
☾ 12	29·632	29·357	29·584	29·440	28·90	51	37	50	42	46	NW.	N.	NW.	·08	·070	·20
13	29·771	29·402	29·830	29·672	29·30	56	40	50	42	46·5	w.	NW.	NW.	·06	·100	·20
14	29·634	29·259	29·778	29·696	28·94	50	31	50	43	45	NW.	w.	N.	·02	...	...
15	29·570	29·242	29·640	29·478	29·08	53	33	51	43	42	sw.	NW.	calm	·47	·370	·14
16	29·574	29·462	29·528	29·490	29·02	50	30	48	38	44	N.	NW.	NE.	...	·060	·63
17	29·613	29·541	29·681	29·596	29·10	50	33	50	38	43	N.	NW.	NW.	·21	...	...
18	29·832	29·705	29·878	29·696	29·16	51	29	49	39	41	N.	NW.	calm	...	...	·12
19	30·059	30·000	30·002	29·996	29·44	58	39	54	38	42	sw.	w.	calm	·06	·155	...
● 20	30·166	30·111	30·081	30·020	29·54	58	31	55	43	49·5	w.	w.	calm	·02	...	...
21	30·213	30·210	30·122	30·090	29·74	57	32	55	43	50	SE.	SE.	calm	...	...	...
22	30·241	30·171	30·154	30·148	29·64	67	37	57	42	54	sw.	SE.	NW.	...	...	...
23	30·214	30·104	30·151	30·122	29·62	61	44	57	43	54	NW.	SE.	calm	·02	...	...
24	30·117	30·008	30·178	30·034	29·44	57	45	52	40	51	SE.	NE.	calm	·16	·200	...
25	30·270	30·213	30·284	30·278	29·64	55	34	54	42	49	N.	N.	calm	·02	...	...
26	30·285	30·118	30·284	30·178	29·70	63	47	58	44	50·5	w.	w.	calm	...	...	...
☽ 27	30·087	29·897	30·031	29·908	29·43	62	48	57	48	56	s.	sw.	calm	·12	·385	...
28	29·769	29·674	29·914	29·768	29·16	59	32	54	44	54	sw.	NW.	NW.	...	0·150	·21
29	29·639	29·581	29·687	29·684	29·05	59	30	53	43	53	NW.	NW.	calm	·24	...	...
30	29·666	29·186	29·687	29·534	29·17	62	37	54	45	52	NW.	NW.	calm	·31	...	·10
	30·285	28·910	30·284	28·872	29·26	67	29	58	38	47·6				2·71	3·315	2·30

Remarks.

London.—April 1, 2. Heavy rain. 3. Cloudy: fine. 4. Rain. 5, 6. Fine. 7. Foggy: fine. 8. Fine, but cold. 9. Foggy: rain. 10. Overcast: stormy and wet. 11. Cold and windy: stormy showers, with hail: thunder at night. 12. Showery: thunder at night. 13. Fine: heavy rain. 14. Stormy showers, with hail: clear and frosty at night. 15. Fine, with showers. 16 Cold and cloudy. 17. Rain. 18. Showery. 19. Fine. 20. Heavy rain: clear and frosty at night. 21—23. Fine. 24. Hazy: rain, with thunder. 25. Drizzly. 26, 27. Foggy mornings: very fine. 28. Fine. 29. Fine: thunder 3 P.M., heavy storm of hail for nearly an hour. 30. Foggy: very fine: heavy rain in the evening.—General character of the month: unsettled, cold, and wet.

Penzance.—April 1. Fair: rain. 2. Misty: rain. 3. Fair: rain. 4. Fair: showers. 5. Clear: showers. 6. Clear: fair. 7. Rain: fair. 8, 9. Misty: fair. 10—13. Fair: showers. 14. Fair: rain at night. 15, 16. Fair: showers, hail and rain. 17. Clear: fair. 18. Fair. 19. Fair: rain at night. 20. Fair. 21. Foggy: fair. 22. Clear. 23. Clear: fair. 24. Rain: fair. 25. Clear. 26. Fair. 27. Fair: rain. 28. Clear: rain. 29, 30. Fair.

Boston.—April 1. Rain. 2, 3. Cloudy. 4. Cloudy: rain P.M. 5. Cloudy. 6, 7. Fine. 8. Rain. 9. Cloudy. 10. Cloudy: rain early A.M. 11. Cloudy: hail and rain A.M. 12. Cloudy: hail and rain, with thunder and lightning P.M. 13. Fine. 14, 15. Cloudy: rain A.M. and P.M. 16. Cloudy. 17. Cloudy: rain P.M. 18—20. Cloudy. 21—23. Fine. 24. Cloudy: shower early A.M. 25—27. Cloudy. 28. Fine: rain early A.M. 29. Fine: rain, with thunder and lightning P.M. 30. Fine: rain P.M.

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END OF THE SECOND VOLUME.

LONDON:
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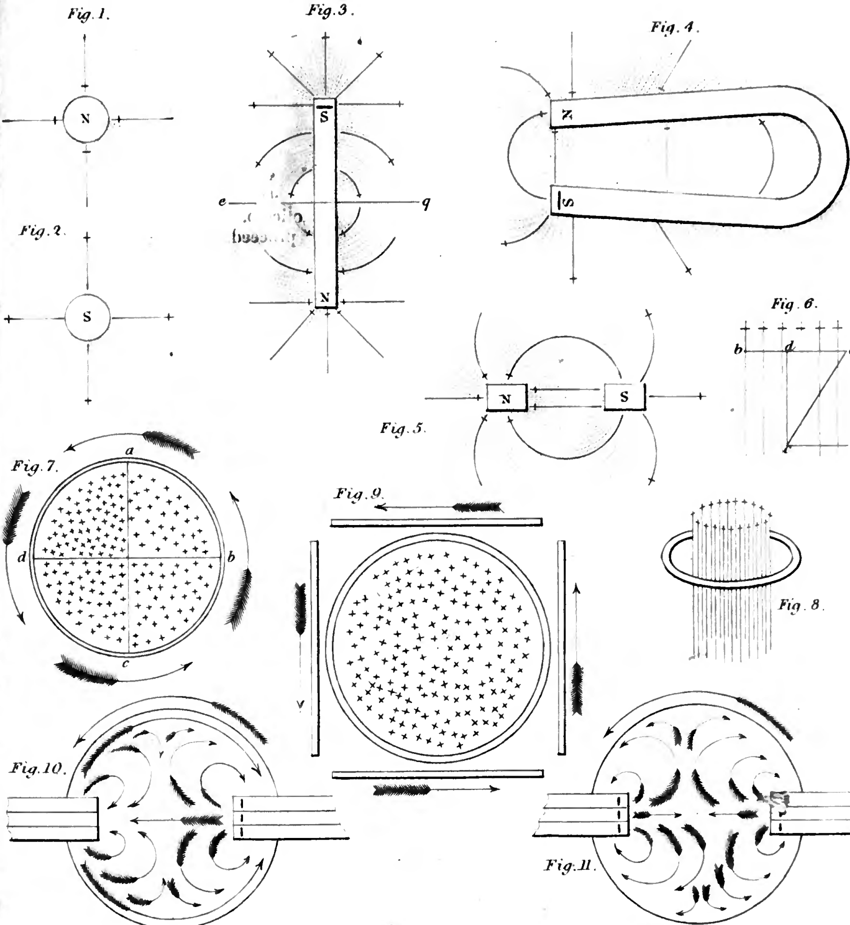
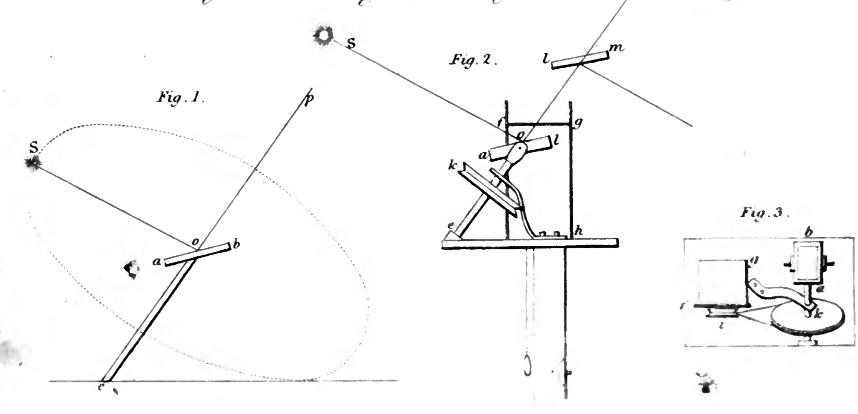
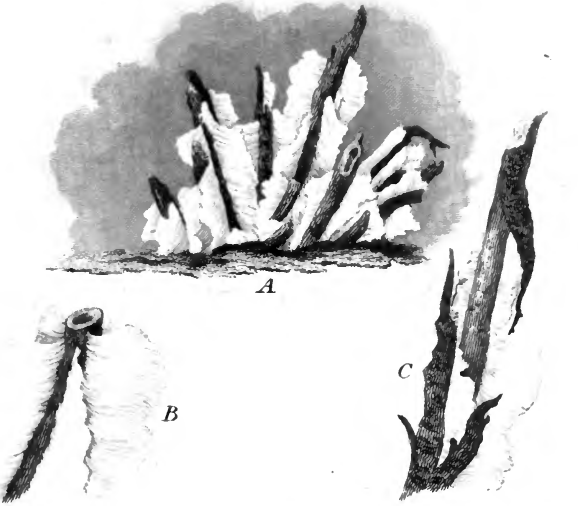
1833.



Mr. Sturgeon's Theory of Magnetic Electricity



Mr. Potter's new Heliostat.



Depositions of Ice observed by Sir John F. W. Herschel.

Fig. 3.

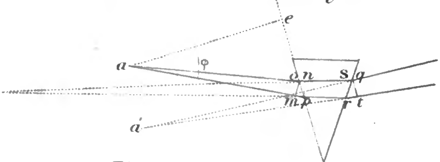


Fig. 2.

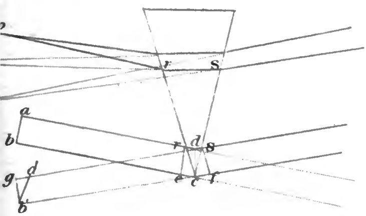


Fig. 1.

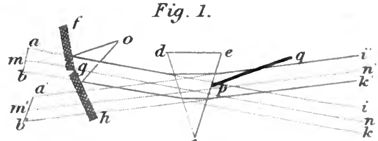


Fig. 5.

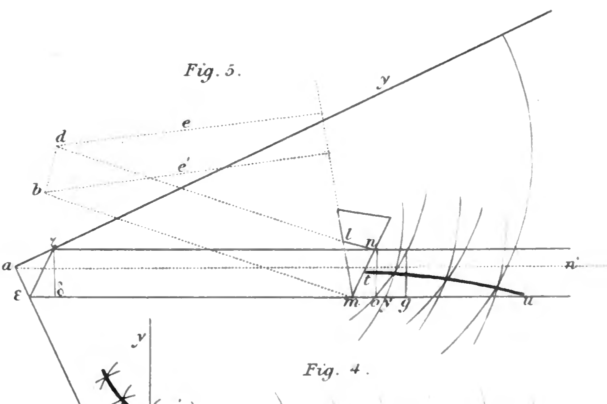
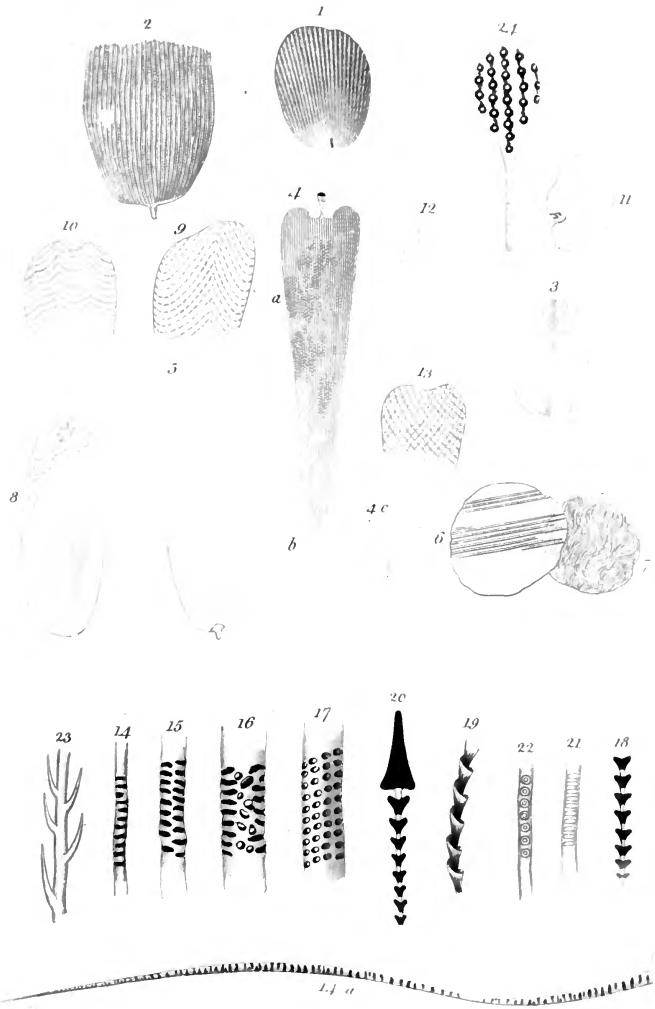


Fig. 4.

Pritchard's Microscopic Cabinet.



Drawn by A. Pritchard

Mr. R. W. Fox's Map of the Granite District, near Penryn.

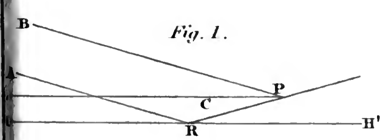


Fig. 1.

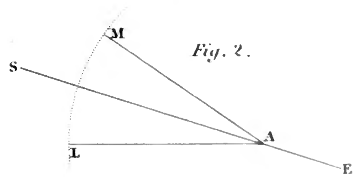


Fig. 2.

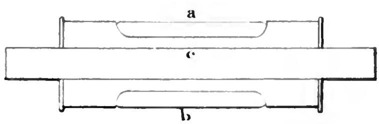


Fig. 3.

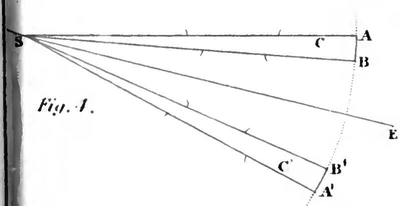


Fig. 4.

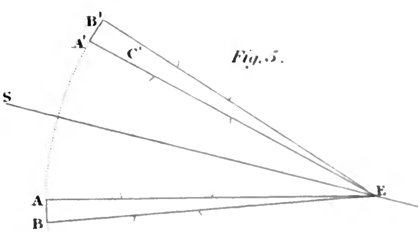


Fig. 5.

Mr. Nixon on his Horizon Sector.